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

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Abstract

When crafting law enforcement policy, drug-producing —or "source"— countries must adjudicate between domestic security priorities and international pressure to curb drug supply. What are the political consequences of prioritizing supply reduction? I analyze the case of illicit crop eradication in Mexico, where the army destroys thousands of fields yearly. While fundamental for ensuring conditional US aid, residents of crop-growing communities understand eradication as an unjust federal policy. I argue that residents negatively update on the trustworthiness of law enforcement after eradication and are discouraged from attempting to change federal policy through electoral means, decreasing turnout. To test, I construct a novel eradication measure using the universe of satellite-detected illicit fields. Using exogenous variation in location and timing, I show eradication depresses turnout in federal elections and trust in the army. Supply reduction might come at the cost of eroding trust in law enforcement and undermining domestic accountability in source countries.

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1 Introduction

For over six decades, the United States has funded and promoted the destruction and interdiction of illicit 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 decreasing the supply of imported drugs should 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 on an annual certification extended only to major drug-producing or drug transit countries that "cooperated fully with the United States in drug control efforts" (Storrs, 2002).

Extant research has conceptualized law enforcement policy as endogenous to domestic politics, responding to electoral incentives and reinforcing preexisting social inequalities (Magaloni, Franco-Vivanco and Melo, 2020; González and Mayka, 2022; Visconti, 2020; Huber and Gordon, 2004; Holland, 2013). In contrast, the case of source country, or supply-reducing operations, emphasizes how foreign law enforcement priorities can shape the use of domestic coercive resources. While source country operations, like crop eradication, are generally ineffective in 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.[1] Consequently, US support for source country operations engenders numerous contentious interactions between citizens and law enforcement agents in drug-producing countries.

In this paper, I analyze the domestic political consequences of a paradigmatic example of US-sponsored source country operations: illicit-crop eradication in Mexico. Drug production and trafficking in Latin America are closely monitored by the US, with particular emphasis on Mexico (DEA, 2021). Besides Mexico, the US government identified 21 other countries as major drug transit or major illicit drug-producing countries in 2022, 16 of which are in Latin America.[2] Mexico grows

[1] For example, the Presidential Determination on Major Drug Transit or Major Illicit Drug Producing Countries for Fiscal Year 2020 explicitly states: "We need the Mexican government to intensify its efforts to increase poppy eradication, illicit drug interdiction, prosecutions, and asset seizures, and to develop a comprehensive drug control strategy."

[2] See: "Presidential Determination on Major Drug Transit or Major Illicit Drug Producing Countries for Fiscal Year 2021." Available at: `https://www.federalregister.gov/documents/2020/09/25/2020-21390/presidential-determination-on-major-drug-transit-or-major-illicit-drug-producing-countries-for`.

substantial quantities of two illicit crops: poppy and marijuana (Johnson, Mendelson Forman and Bliss, 2012; DEA, 2021), and the United States has forcefully supported eradication efforts in the country since the Nixon administration (Cedillo, 2021; Teague, 2019).

I contend that crop eradication is an informative signal of federal law enforcement priorities and tactics for civilians. Drawing from ethnographic and journalistic work, I argue that people living in crop-growing communities understand the destruction of their crops as the federal government behaving punitively and unjustly by interfering with the already precarious well-being of marginalized citizens (Le Cour Grandmaison, 2021; Álvarez Rodriguez, 2021*b*). Conceptualizing institutional trust as the belief that a representative government agent will tend to act in one's best interest (Slough and Torreblanca, 2023; Bhattacharya, Devinney and Pillutla, 1998; Hardin, 2003), I hypothesize that eradication operations reduce citizens' trust in the army, the bureaucracy in charge of eradicating all illicit crops.

I further hypothesize that illicit crop eradication discourages citizens from voting in federal elections. Crop eradication is a federal law enforcement policy carried out by the Mexican army, a federal bureaucracy. Research shows that security concerns and law enforcement policy often motivate citizens to participate in politics (Bateson, 2012; Walker, 2019), making the grievances inflicted by crop eradication a powerful potential catalyst for turnout. However, conditional US aid constrains the political opportunity structure by removing crop eradication reform from the set of feasible policies politicians can propose or enact. Consequently, aggrieved citizens looking to change the federal policy of crop eradication have few opportunities and little incentive to do so through their ballots.

To test these hypotheses, I use novel satellite data on the more than fifty thousand illicit fields detected by the Mexican army between 2013 and 2020. Because most destroyed fields are incinerated, I can combine the universe of satellite-detected illicit fields with NASA's historical satellite data on fires to identify the electoral precinct and the date of each eradication. I use variation in the army's decision to eradicate a field arising from exogenous time and capacity constraints to estimate the effect of eradication on electoral participation. For army commanders, the choice of what specific field to incinerate out of all detected illicit fields in their area of operation depends on stochastic factors like detection timing, personnel availability, ongoing military operations, and other similar considerations.[3] This randomness in field selection allows me to leverage the *ad hoc* geographic organization of army operations to compare participation in electoral precincts where the army detected but *did not* eradicate illicit fields to nearby, demographically similar precincts where it did eradicate.

The results show that destroying an illicit field before a federal election decreases turnout in an electoral precinct by almost two percentage points on average, or 10% of a standard deviation, compared to similar crop-growing precincts in the same military zone where fields were not eradicated. Additionally, using the timing of survey collection for eight waves of an annual national-representative survey, I show that rural dwellers of municipalities eradicated before survey collection show less trust in the army than individuals living in comparable areas that were eradicated *after* survey collection. However, eradication did not seem to affect trust in law enforcement institutions unrelated to the federal government, nor trust in the army among *urban* inhabitants who do not personally observe crop eradication operations. I show that neither migration nor population changes can account for the results. I also show that eradication does not measurably affect participation through changes in the level of lethal violence, and, using the collapse of poppy prices in 2017, I rule out the possibility that the negative economic shock of field destruction can mechanically account for all the demobilizing effects of eradication.

To validate the identifying assumptions underpinning the empirical strategy, I show that field and precinct-level geographic characteristics that could be endogenously related to turnout are not predictive of the army's decision to eradicate a specific field. Further, I show that the results are robust to using official municipal-level eradication data published by the Mexican army. I test for the possibility that bias in the precinct-level measurement of eradication drives the result, retrieving little supporting evidence. First, I repeat the precinct-level analysis but define treatment as instances of fires unrelated to crop eradication. I find a *positive* and statistically insignificant relationship between these unrelated fires and electoral turnout, suggesting that random measurement error might understate the results. Last, I use a shorter intra-election panel of official geolocated data on eradication and compare it to the fire-based measure of predicted eradication to benchmark its accuracy. With a simulation, I find that the results hold even with a higher-than-expected proportion of misclassified units.

[3] Corroborated in an interview with a high-ranking armed forces commander.

This paper contributes to the literature on the political economy of law enforcement by emphasizing the domestic accountability implications of foreign security aid. Governments tailor security policy to service their domestic constituencies (González, 2020; Soss and Weaver, 2017; Holland, 2013). However, the case of US-sponsored source country operations highlights how aid conditionality encourages governments to service foreign security preferences, even at the cost of adopting ineffective or domestically unpopular policies, potentially undermining domestic accountability.

Additionally, this paper contributes to the growing literature on the behavioral and attitudinal consequences of law enforcement in developing countries. Recent studies have examined if trust-building policing practices improve security outcomes or citizens' evaluation of the police and government, finding mixed results (Blair et al., 2021; Magaloni, Franco-Vivanco and Melo, 2020). In parallel, work exploring the political consequences of crime in similar contexts has found that personal victimization and insecurity undermine the government's legitimacy, engender in victims a preference for punitive law enforcement, and may depress electoral participation (Visconti, 2020; Ley, 2017; Marshall, 2022). Despite the empirical political relevance of crime and law enforcement, the *consequences* of interactions between citizens and security agents charged have not been comprehensively analyzed in developing contexts.

Finally, this paper contributes to research on the negative consequences of attempting to curb drug production and consumption using coercion. Partly as a response to the decades of US support for source country operations, producing countries have increasingly approached drug control from a security perspective (Loveman, 2006; Flores-Macías and Zarkin, 2021). Researchers have argued that this "securitization" of the "War on Drugs" may increase state-perpetrated violence and often fails to improve security outcomes (Dell, 2015; Castillo and Kronick, 2020; Magaloni and Rodriguez, 2020; Blair and Weintraub, 2023). However, the *political* results of counternarcotics law enforcement have only been explored as mediated by violence (Trejo and Ley, 2020) or in the mid and long-term (Flores-Macías, 2018; Osorio, Schubiger and Weintraub, 2021). In contrast, the short-term direct political implications have been overlooked.

2 Source Country Operations

2.1 The US and Source Country Operations

When President Nixon declared a "worldwide offensive dealing with the problems of sources of supply" in 1971,[4] a new, well-funded, counternarcotic US foreign policy emerged: source country operations. By destroying drugs at their source or stopping them from crossing the border, authorities believed drug use in the US could be prevented or significantly reduced (Tokatlian, 2015). This US foreign policy has been well funded. Over the last six decades, the US has devoted billions of dollars in security aid to support drug interdiction, marijuana defoliation, and illicit field destruction operations abroad (Vorobyeva, 2015; Teague, 2019).

In 1986, Congress doubled down on source country operations as a cornerstone of US counternarcotics policy by requiring the President to identify all major illicit drug-producing and drug transit countries each year, certify that these countries were cooperating with the US in counternarcotics efforts, and withhold 50% of non-counternarcotics US security assistance to countries that failed certification (Storrs, 2003). While most trade sanctions after decertification were often waived for national interest reasons, certification was a momentous event, and the threat of decertification was powerful leverage for the US when negotiating policy with recipient countries.[5]

The certification process became somewhat laxer in 2002 by allowing the President to bypass Congressional veto to certification (Storrs, 2003). However, it remains a yearly hurdle for drug-producing countries to navigate, and drug interdiction and destruction activities are still cited as evidence for or against certification. For instance, the 2018 Presidential Determination on Major Drug Transit or Major Illicit Drug Producing Countries states: "Colombia as a country that has failed demonstrably to adhere to its obligations under international counternarcotics agreements due to the extraordinary growth of coca cultivation [...] including record cultivation during the last 12 months.",[6] while the 2019 Determination states: "I am deeply concerned that illicit drug crops have expanded over successive years in Colombia, Mexico, and Afghanistan, and are now at record

[4] See "Remarks About an Intensified Program for Drug Abuse Prevention and Control", June 17, 1971. Available at: `https://www.presidency.ucsb.edu/documents/remarks-about-intensified-program-for-drug-abuse-prevention-and-control`

[5] See "U.S. IS CERTIFYING MEXICO AS AN ALLY IN FIGHTING DRUGS", March 1, 1997. Available at: `https://www.nytimes.com/1997/03/01/world/us-is-certifying-mexico-as-an-ally-in-fighting-drugs.html`

[6] Available at: https://www.federalregister.gov/documents/2017/09/28/2017-21028/presidential-determination-on-major-drug-transit-or-major-illicit-drug-producing-countries-for

levels."[7]

2.2 Source Control in Mexico

Mexico has been a stage for source country operations since their inception. From 1969 until 1988, and since 2007, the US has funded continuous operations in Mexico to destroy illicit crop cultivation and disrupt drug trafficking (Cedillo, 2021). The authoritarian PRI regime had good reason to embrace US-funded counternarcotic source country operations. The US provided arms, training, and resources for the Mexican security apparatus, also in charge of destroying the illicit crops, while the operations gave the PRI regime political cover to use those resources to crack down on political opponents and antagonistic social movements (Cedillo, 2021; Aviña, 2018; Teague, 2019).

Mexican authorities continued to eradicate illicit crops routinely after the transition to democracy, as US authorities pressured the Mexican government to sustain efforts towards reducing drug supply. However, a significant change to Mexican eradication policy occurred in 2007. The Mexican army had always assisted in eradication operations (Cedillo, 2021), however the then-newly-elected president Calderón officially transferred all eradication duties from the Federal Attorney's (PGR) to the Mexican army that year (Carvente Contreras, 2014; SEDENA, 2012). Since then, only the armed forces,[8] whom citizens unambiguously identify as a federal bureaucracy, eradicate illicit crops.

The army uses two techniques to eradicate illicit crops: aerial aspersion and manual incineration. The former involves fumigation from the air, while the latter requires soldiers securing the identified field and cutting and incinerating the plants. Contrary to eradication efforts in the Andean region (Dion and Russler, 2008), the Mexican army overwhelmingly uses the manual technique. Between 2011 and 2020, it reports having destroyed 46,663 hectares of marijuana manually, 81% of all destroyed marijuana hectares. As for poppy eradication, the army reports having manually destroyed and incinerated 172,947 hectares, or 86.4% of the total. Specifically, in 2015 and 2018, the army destroyed 95% and 98% of all fields manually.[9]

While being a central part of counternarcotics law enforcement policy, illicit crop eradication

[7] Available at: https://www.federalregister.gov/documents/2018/10/04/2018-21806/presidential-determination-on-major-drug-transit-or-major-illicit-drug-producing-countries-for

[8] The navy sporadically eradicates illicit crops. Between 2013 and 2020, it was responsible for 2% of all destroyed poppy or marijuana hectares.

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

does not significantly affect drug-trading organizations' (DTOs) profits. As schematized in Figure A2, illicit crop growers, often poor individuals in marginalized communities, own the crops and sell only the raw material directly to intermediaries, not drug trading organizations. Conversely, DTOs increasingly profit from synthetic instead of crop-based drugs like fentanyl (DEA, 2021).

Recent qualitative research describes just how noteworthy crop eradication operations are for locals. Le Cour Grandmaison, Morris and Smith (2019*b*) narrate how illicit-crop fields were very visible and even close to the main street in town. At the same time, 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'." (Álvarez Rodriguez, 2021*b*).

This research further argues that illicit crop growers interpret the destruction of their fields as the government behaving punitively towards them, the weakest link in the drug-trafficking chain, instead of pursuing criminals that generate violence (Le Cour Grandmaison, 2021; Álvarez Rodriguez, 2021*b*). In her ethnography of a crop-growing community in Guerrero, Álvarez Rodriguez (2021*b*) succinctly captures the dynamic: "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."

2.3 Theoretical expectations

Extant research conceives of law enforcement as endogenous to domestic politics; observing crime and its punishment informs citizens of security policy and implementation, shapes their law enforcement preferences, and can affect their participation decisions. (Ley, 2017; Kronick, 2014; Visconti, 2020; Bateson, 2012). Citizens' political choices, 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).

Conversely, the case of source country or supply-reducing operations emphasizes how foreign law enforcement priorities, operating through channels other than electoral accountability, can mold the use of coercive resources. The Mexican government has electoral incentives to respond to the security preferences of voters. However, the US is vested in Mexican law enforcement efforts and uses its conditional financial resources to pressure the Mexican government towards adopting its favored policy: supply reduction. Thus, the Mexican government has one blunt tool, security policy, to respond to the needs and preferences of two competing "principals": voters and their most important trade partner, the US.

Simultaneous political incentives to adapt policy to the taste of different principals generates a multiple-principal or common-agency akin dynamic (Voorn, van Genugten and van Thiel, 2019; Dixit, Grossman and Helpman, 1997). In multiple principal dynamics, service provision by the agent is complicated when principals have divergent interests. In such cases, the agent must balance the competing interests of all of their principals when crafting and implementing policy. However, if principals differ in their relative bargaining power, the agent has powerful incentives to be more responsive to the preferences of the stronger principal (Voorn, van Genugten and van Thiel, 2019).

In this paper, I study the reaction of the weaker principal -voters in crop-growing precincts- to actions the Mexican government took in response to US policy preferences. I contend that source country operations inform civilians of domestic law enforcement priorities and tactics. Consequently, I expect them to affect citizens' beliefs about the government and how it enforces the law. Research characterizes source country operations as perceived to be violent and unfair (Dion and Russler, 2008; Anria, 2013; Álvarez Rodriguez, 2021*b*). Thus, source country operations indicate to voters in illicit crop-growing districts that the government is not responsive to their policy preferences. I hypothesize that experiencing source country operations engenders distrust in the law enforcement agents carrying them out. In contrast, I argue that the behavioral consequences of source country operations hinge on the opportunity structure within which individuals can *react* politically.

People's ability and motivation to participate in politics depends on their available resources and the institutional incentive structure (Verba, Nie and Kim, 1978; Franklin, 2004; Cantú and Ley, 2017). Suppose political mobilization could lead to a meaningful change in the law enforcement policy of eradicating the supply of drugs. In that case, people aggrieved by source country operations might be motivated to participate more in politics to effect policy change. An example of this is the "Movimiento al Socialismo", or MAS movement in Bolivia, where the large communities of indigenous cocaleros used preexisting local organizations to mount a resistance to the eradication policy that, over the years, coalesced in a formal political movement that took power through electoral means (Anria, 2013).

Conversely, suppose affected citizens cannot mount political opposition potent enough to countervail US pressure, as is the case for Mexico's small and marginalized illicit crop-growing communities. In that case, there is little incentive for politicians to propose or credibly commit to policy change that might endanger US financial suppot. Thus, aggrieved people have little motivation to participate in politics as a result of opposing such operations.

Conditionality in US aid deeply molds the political opportunity structure by constraining aid-recipient governments' ability to abandon the policy of source country operations. Aid conditionality disincentivizes politicians from proposing reforms to such policies; sitting governments benefit economically and politically from their continuation despite the potential loss in political support from small crop-growing communities. Thus, I hypothesize voters are unlikely to have the opportunity or the willingness to attempt to effect change to the policy through their electoral participation. Consequently, I expect source country operations to depress turnout in the short term when the political opportunity structure is fixed.

3 Data

I draw on official data from the National Electoral Authority (INE), the National Institute of Geography and Statistics (INEGI), and data I obtained from the Mexican army. Municipal-level data on illicit-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 illicit-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.

3.1 Outcomes of Interest

I collected data on the four federal elections for deputies after the army took over eradication duties for the analysis. These data are available at the municipality and the electoral precinct levels. Federal elections in Mexico happen every three years for federal deputies and every six years for presidents and senators. Municipal elections might or might not be concurrent with federal elections since local elections follow their own calendar. I use the data published by Magar

(2018) to identify municipalities with concurrent local elections to account for the increased levels of turnout they may produce.

To measure trust in the government, I look at responses from eight waves[10] of the yearly National Survey of Crime Victimization and Public Safety, ENVIPE, which asks respondents to rate their trust for several institutions related to law enforcement.

3.2 Crop eradication

For information on the share and location of destroyed crops, I rely on two data sources based on official information from the Mexican army. The first consists of the type, number, and size of all illicit fields manually destroyed by the army, per 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 illicit-crop-growing communities are often small and in rural areas. Using a freedom of information request to get around the data limitations, I obtained a novel data set that contains the latitude and longitude of all poppy and marijuana fields detected by the army using satellite images between 2013 and June 2021. Besides the coordinates and the date of detection, these data report whether the army validated the detected field as a true positive or a false positive.

Figure 1 shows the date and number of illicit-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 destroyed fields. Since the data includes the latitude and longitude, I can match each satellite image to an electoral precinct[11] and compute the number of satellite-detected fields in each electoral precinct each month.

The army does not report whether it later eradicated the fields it detected or not. 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

[10] From 2013 to 2021, excluding 2020 because of COVID-19.

[11] 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).

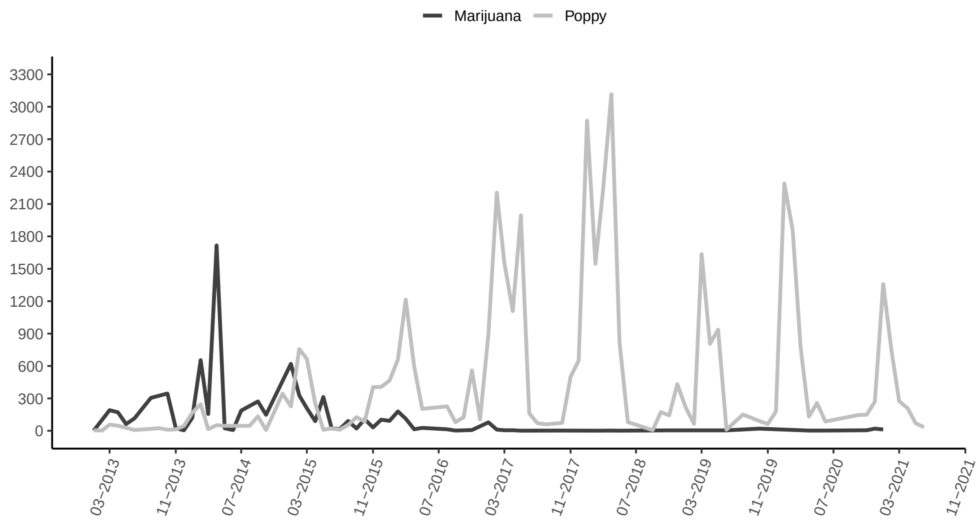

Figure 1: The figure shows the monthly count of illicit fields detected via satellite, as reported by the Mexican army, according to crop type. Data spans from February 2013 until June 2021. The count excludes fields that the army determined to be false-positives.

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 buffer of 2km around the coordinates of the illicit field. The size of the buffer accounts for measurement error in the satellite fire data, which reports the center of a 1km pixel, and potential differences between the coordinates of the illicit fields and the place where eradication was carried out (for instance, because soldiers gather the plants to the side of a field instead of to the center.)

2. Keep all high-quality fires recorded within the 2km buffer for the three months after the illicit field was detected. Three months is the most stringent specification since, at best, fields can be harvested three times per year (Le Cour Grandmaison, Morris and Smith, 2019*b*) or every four months.

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

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

4. If there was only one fire inside the 2km buffer within the three months, then assign the date of the eradication as the date of the fire. If more than one fire meets the criteria, assign eradication as taking place on the date of the fire geographically closest to the original coordinates of the reported field.

Out of the 53,509 illicit 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 six-to-four 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 illicit-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. However, in the next section, I discuss this measure's validity in-depth and provide several placebo tests to corroborate that it is indeed capturing eradication by incineration.

Constructing a measure of illicit field eradication using automated satellite images instead of 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 illicit-crop fields, like ground patrols or intelligence from other institutions. Consequently, the universe of municipalities where the army reports eradicating illicit-crop fields is larger than the universe of municipalities where it detected illicit fields via satellite. Figure 2 shows the municipalities where the army reports having eradicated illicit crops manually, as well as municipalities where it detected illicit fields via satellite during 2015 or 2018. Table A1 reports basic summary statistics. While the army reports at least one eradication operation in 433 municipalities, 91.6% of all hectares eradicated in 2015 or 2018 were destroyed in one of the municipalities with satellite-detected illicit fields, making the overlap between the two measures of eradication large.

4 Empirical Strategy

The following section presents two empirical strategies to measure the effect of eradication on turnout: a fixed-effects model at the electoral-precinct level and a two-way fixed-effects model at

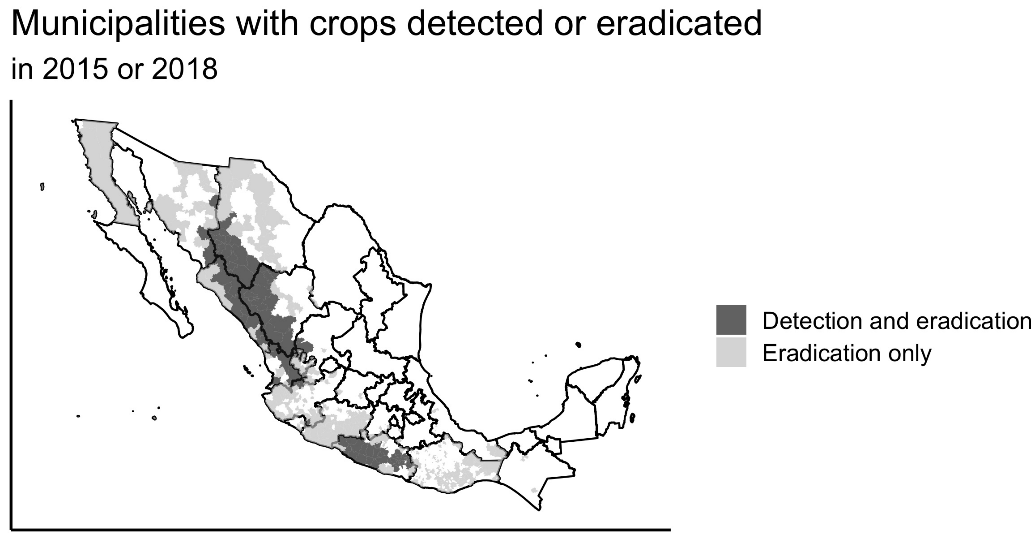

Figure 2: Map marks municipalities where the Mexican army reports having conducted eradication operations in either 2015 or 2018, as well as municipalities that had at least one positive satellite-detection of illicit-crop fields.

the municipal level. Additionally, I detail the empirical strategy used to measure the effect of eradication on institutional trust.

4.1 Crop eradication: Electoral precinct

The first empirical strategy is motivated by the observation, corroborated in an interview with a high-ranking armed forces commander, that conditional on detection, the army's decision to target a given field depends on stochastic factors like other ongoing operations, personnel availability, or similar organizational considerations unrelated to political participation. Intuitively, this empirical strategy compares participation in eradicated electoral precincts to participation in similar precincts that could have been eradicated because they had illicit fields growing but were not.

I leverage the army's *ad hoc* geographic organization to further refine the causal contrast. Mexico organizes its army around military regions, each encompassing several military zones. These military regions and zones imperfectly follow the country's political geography: a region can encompass two to five states, while a military zone can straddle municipalities belonging to one, two, or three distinct states.[13] Using a series of freedom of information requests, I assigned each

[13]Conversely, a single state can be composed of one, two, three, four, and even five military zones, like the state of Chiapas.

municipality to a military region and zone.[14] Military zone commanders, who can be assigned and reassigned discretionally by the president, are responsible for all operations, including eradication (SEDENA, 2012). I include $year \times zone$ fixed-effects in all precinct-level specifications to guarantee that the comparisons are between electoral precincts in the same military zone, the same year, and overseen by the same military zone commander.

Electoral precincts' boundaries are not politically salient. However, one worry is that political considerations could impact the army's decision to eradicate certain *municipalities* more or less intensively. Specifically, since the army is a federal bureaucracy, we could worry that municipalities headed by copartisan mayors would be spared eradication more often. To account for this potential dynamic, I control for whether the electoral precinct is in a municipality where the mayor is the president's copartisan. Additionally, I control for concurrent municipal elections.

I fit the following fixed-effects regression:

$$Y_{pt[z]} = \gamma Predicted_{pt} + \beta \boldsymbol{X}_{pt} + \mu_{t \times z} + \varepsilon_{pt[z]} \tag{1}$$

where $Y_{pt[z]}$ is the turnout rate of electoral precinct p, in military zone z, during election year t, $Predicted_{pt}$ is either a dummy variable that takes the value of 1 if there is any pre-election predicted eradication in precinct p and 0 if illicit fields were only *detected*, or the $(log + 1)$ count of fields or hectares predicted to have been manually eradicated in that electoral precinct that year. $\mu_{t \times z}$ are year $\times$ military zone fixed-effects. X_{pt} is a vector of pre-treatment covariates, including an indicator of whether or not there were concurrent municipal elections and whether the mayor was the president's copartisan. Additionally, all models with "full" covariates include the $(log + 1)$ number of illicit fields detected in adjacent precincts in year t and an Inverse Covariance Weighted index (ICWa) (Anderson, 2008), constructed from a battery of demographic characteristics taken from the 2010 census. Instead of including all covariates separately, the index summarizes orthogonal variation from all variables as efficiently as possible. In this setting, the decrease of statistical power from including all the covariates would not be compensated by the extra information since most demographic characteristics are highly correlated. Section A4.3 explains the index construction in detail. However, results hold when including sociodemographic characteristics separately. Finally,

[14] Figure A1 in the Appendix shows the result.

robust standard errors are clustered at the electoral-precinct level.

4.2 Crop eradication: Municipality

My second empirical strategy tests the same mechanism with official municipal-level data on eradication, published monthly by the Mexican army. I look at the turnout for four federal elections after the army took over eradication responsibilities: 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 from the sample municipalities that select their authorities via indigenous self-governance. The final sample includes data from 596 municipalities, where 96% of the hectares destroyed by the army were located.

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 places eradicated *after* the election took place, but within the same year. The identifying assumption embedded in the analysis is that, net of time-invariant characteristics, whether eradication happens before or after the election is orthogonal to time-variant confounders.

I estimate the following two-way fixed-effects model:

$$Y_{mt} = \gamma EradicationBefore_{mt} + \mu_t + \theta_m + \varepsilon_{mt} \tag{2}$$

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 illicit 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.

4.3 Trust

I test the effects of illicit-crop eradication on institutional trust by looking at responses from eight waves[15] of a yearly national representative survey, ENVIPE. I leverage the timing of the

[15] From 2013 to 2021, excluding 2020 because of COVID-19.

survey to compare self-reported attitudes about institutions in municipalities where the government eradicated illicit fields before survey collection began to the attitudes of respondents living in municipalities where the government eradicated fields *after* the survey was collected.

All eight waves of the ENVIPE were collected by the National Institute of Statistics (INEGI) between March and April. I pool the eight waves of the survey and keep respondents who live in a municipality where the army reports having eradicated at least one illicit field during the year the survey was collected. Since illicit crops are grown in rural and remote areas, I look at respondents that live in rural communities inside these eradicated municipalities for the main analysis. Later, I use urban respondents, who are unlikely to have witnessed or experienced any eradication, as a placebo. After pooling, I have data from 25,287 unique respondents from rural communities in 323 different municipalities from 25 different states in Mexico.

I fit a series of models with the following specification:

$$Y_{it[m]} = \gamma EradicationBefore_{tm} + \beta \boldsymbol{X_{it}} + \mu_t + \theta_m + \varepsilon_{it[m]} \tag{3}$$

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 between January and April of year t and zero if it was eradicated at a later month. 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.

5 Results

5.1 Crop eradication: Electoral precinct

First, I present the precinct-level results from specification (1). As I discussed in the previous section, the contrast is between electoral precincts where the army only detected illicit fields and precincts with predicted eradication, conditional on them being demographically similar, and in the same military zone the same year.

Columns 1 and 2 in Table 1 show the estimated difference-in-means in turnout between electoral precincts with at least one predicted eradication before federal elections and precincts with only illicit-field detection but no eradication. On average, eradication decreases turnout between 1.7 (the preferred specification) and 2 percentage points, or between 10% and 12% of a standard deviation. Additionally, columns 3 and 4 show that a one-unit increase in the log number of eradicated hectares decreases turnout by .87 percentage points.

To contextualize the magnitude of the effect: going from no eradication in a precinct to 23.5 destroyed hectares, the median amount for the treated group, is estimated to reduce turnout by $[ln(23.5 + 1) - ln(1)] \times -.87 \approx -2.75$ percentage points. Similarly, columns 4 and 5 show that a one-unit increase in the log number of eradicated fields before an election decreases turnout by around 1.5 percentage points. Alternatively, going from no eradication in a precinct to 13 destroyed fields, the median amount for the treated group, is estimated to reduce turnout by $[ln(13 + 1) - ln(1)] \times 1.55 \approx -4.1$ percentage points. That is equivalent to a decrease of almost 25% of a standard deviation.

	Turnout					
	(1)	(2)	(3)	(4)	(5)	(6)
Any eradication (dummy)	−1.974*	−1.696+				
	(0.936)	(0.979)				
Destroyed hectares (log)			−0.866*	−0.868*		
			(0.336)	(0.389)		
Destroyed fields (log)					−1.579**	−1.550**
					(0.518)	(0.546)
Controls:	Basic	Full	Basic	Full	Basic	Full
Fixed-effects: Year × Military Zone	Yes	Yes	Yes	Yes	Yes	Yes
Num.Obs.	1039	1039	1039	1039	1039	1039
R2 Adj.	0.397	0.397	0.399	0.399	0.401	0.401

Two-tailed p-values: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 1: Illicit-crop eradication and turnout in federal elections for deputies: precinct-level results. Dependent variable measures turnout as the share of all registered voters in the electoral precinct. Robust standard errors clustered at the electoral precinct level.

5.1.1 Measurement concerns

One worry when interpreting the results in Table 1 is that that the effects capture something unrelated to eradication because treatment is predicted instead of observed. I conduct several ancillary analyses to validate the measure. First, I use the 8.1% of the satellite detection data observations that the army labeled 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 of conceptualizing the effect of "false field eradication" on turnout is as the effect of wildfires or controlled fires in places *without* illicit fields. Table 2 reports the results of this placebo test. Suppose predicted eradication in true illicit 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 2. However, the estimated difference-in-means effect of eradicating any "false" field on turnout is *positive* and not significant, while the estimated effect of destroying an additional log field is much smaller in magnitude and statistically insignificant. The placebo test is particularly stringent if false-positives are more common in places suitable for illicit-crop-growing. Although the magnitude of the standard errors in column 2 alerts us that the analysis is underpowered, back-of-the-envelope calculations using the estimated standard error of the simple difference-in-means estimator show that this specification is precise enough to detect effects of only $0.047 \times 2.8 = 0.13$ percentage points.[16]

	Turnout	
	(1)	(2)
Any false-positive "eradication" (dummy)	0.031	
	(0.047)	
False-positive fields "destroyed" (log)		-0.365
		(1.504)
Controls:	Full	Full
Fixed-effects: Year × Military Zone	Yes	Yes
Num.Obs.	1039	1039
R2 Adj.	0.397	0.397

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

Table 2: False-positive eradication and turnout at the precinct level. Dependent variable is turnout on federal elections for deputies, measured as share of all registered voters. Independent variable is the (log+1) number of illicit fields, later determined to be false-positives, that the algorithm predicts were eradicated in each electoral precinct. Robust standard errors clustered at the electoral precinct level.

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 for municipalities where the algorithm predicts eradication during months when the army does not report any. Predicted destroyed fields

[16] Assuming 80% power for a 95% confidence interval.

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 on the prior or subsequent month from the one predicted by the algorithm.

Last, I simulate the sampling distribution of the most imprecisely estimated outcome, the difference-in-means estimator, under different assumptions of the "true" proportion of misclassified observations. Section A6.1 in the Appendix explains the simulation in detail. Results show that more than 45% of treatment units or more than 40% of control units would need to be misclassified for misclassification to explain away the weakest effect. To test whether this level of misclassification is plausible, I obtained official geolocated data from the Mexican army on illicit field destruction data for 2019 and 2020 to benchmark the predicted eradication measure. I compare the measure of predicted eradication with geolocated reported eradication for this period and estimate the proportion of false positives and negatives included in the data. Comparing my measure of predicted eradication with the army's official reports, I find that 9.45% of control units were possibly misclassified as treatment, and 22.8% of treated units were possibly misclassified as control, not enough to overturn the results according to the simulations.[17]

5.1.2 Selection concerns

The identifying assumption for the precinct-level results is that precincts where the army detected, but did not destroy illicit fields, are a suitable counterfactual to precincts where the army destroyed illicit fields, conditional on them being demographically similar and in the same military zone, during the same year. The results would be biased if electoral precincts where the army eradicated fields were systematically different in ways that covaried with political participation. Specifically, one might worry that poorer, less well-connected precincts within military zones are more likely to get eradicated and that these precincts, in turn, are less likely to participate politically.

However, it is difficult to think of the army adjusting its behavior as a function of the arbitrary geographic delineation of electoral precincts. First, the national electoral authority draws all the federal electoral precincts. Once drawn, the only two adjustments the federal electoral authority

[17] I cannot distinguish fields that were detected via satellite and later eradicated from fields that were detected with other methods with the geolocated army data on eradication. Thus, the proportion of false negatives is likely overstated.

(INE) makes are to remove electoral precincts or join them with adjacent precincts when population sizes change too drastically. Consequently, precincts often were drawn decades before and straddle multiple communities, making them independent to political dynamics.

Alternatively, we could worry that when deciding between eradicating similar fields, army soldiers could systematically choose to eradicate more accessible fields that imply less work for them. If political participation covaries in the geographic characteristics, then field-level selection could explain part of the results. To discount this possibility, I test how well geographic characteristics predict eradication. 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 A4 in the Appendix shows this model's confusion matrix. Geographic characteristics do a very poor job of predicting eradication: only 0.13% of all eradicated fields are correctly predicted to be eradicated, lending credence to the identifying assumption.

5.2 Crop eradication: Municipality

Next, I present the results of specification (2). Recall that all municipal-level analyses use official monthly data on crop eradication reported by the army. Thus, this specification should help assuage concerns that the results are driven by the construction of the precinct-level eradication measure.

Column 1 in Table 3 shows the estimated effect of the army doing *any* manual eradication on turnout relative to no eradication before the elections. On average, eradication before the election, relative to eradication after, is estimated to decrease turnout by 1.8 percentage points,

	Turnout (1)	Turnout (2)	Turnout (3)
Any eradication (dummy)	−1.763+		
	(0.927)		
Manually er. fields (log)		−0.494*	
		(0.233)	
Manually er. hects. (log)			−0.981**
			(0.344)
Fixed-effects: Municipality	Yes	Yes	Yes
Fixed-effects: Year	Yes	Yes	Yes
Num. Obs	1253	1253	1253
R2 Adj.	0.660	0.661	0.663

Two-tailed p-values: + $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

Table 3: Illicit-crop eradication and turnout in federal elections for deputies: municipal-level results. Dependent variable measures turnout as the share of all registered voters in the municipality. Robust standard errors clustered at the municipality level.

or 12.4% of a standard deviation (p-value 0.057). Columns 2 and 3 show the estimated marginal effect of a one-log unit increase in the number of eradicated fields and hectares respectively. The effects are estimated more precisely as expected from the added variation of continuous measures. A one-unit increase in the log number of eradicated fields is estimated to decrease turnout by around .5 percentage points, while a similar increase in the log number of eradicated hectares decreases turnout by almost one percentage point on average. To contextualize the magnitude of the effects: going from no fields destroyed prior to the election to the median number of destroyed fields and hectares in the treated group, 18 and 2.6, respectively, is expected to decrease turnout by $[ln(18+1)-ln(1)] \times -.494 \approx -1.45$ percentage points and $[ln(2.6+1)-ln(1)] \times -.981 \approx -1.26$ percentage points.

Including year and municipal fixed-effects guard against time-invariant unit-specific confounders or year-specific confounders common to all municipalities. Additionally, by comparing municipalities eradicated before an election to those eradicated after, the design plausibly accounts for time-variant unobserved confounders common to all *eradicated* crop-growing municipalities. However, for columns 2 and 3 in Table 3 to recover the average effect of a marginal increase in the intensity of eradication given the continuous nature of the treatment, effects must be constant across groups, periods, and dosages (Callaway, Goodman Bacon and Sant'Anna, 2021). I fit the same two models with a flexible ten-knot cubic regression spline and plot the results in Figure A3. This exercise provides evidence that the effects 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. While $\hat{\gamma}$ will still recover a causal quantity in the absence of time-varying confounders, precincts in military zones with less cross-sectional homogeneity treatment assignment will contribute more variation to the estimation.

5.3 Trust

Next, I look at how eradication affects people's self-reported level of trust in several government institutions by drawing on the information collected in the yearly survey, ENVIPE. The survey does not explicitly ask about trust in the federal government. However, it does ask about trust in all other institutions related to the provision of security.

The results for rural respondents, reported in dark gray in Figure 3, suggest that eradication in rural areas dampened people's trust in federal law enforcement corporations generally. Reported trust in the army, the navy, and the federal police is around .05 standard deviations lower when respondents lived in a municipality eradicated before survey collection. However, only in the case of the army the difference is statistically significant at conventional levels. For the rest of the non-federal or non-policing agencies there is no difference in trust.

Next, I use respondents from *urban* localities as a placebo. Since illicit 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, only the coefficient for trust in judges is statistically significant in the case of urban respondents- and positive in magnitude- despite the sample being more than twice as large as those conducted with rural respondents. Further, the estimates' magnitude for differences in trust in the army or other federal policing agencies is precisely zero or very close to zero.

While survey evidence indicates that people exposed to eradication trust the army less, identification depends on the timing of eradication at the municipal level being orthogonal to potential political participation. 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 check for this possibility by comparing their responses to self-reported trust in family and neighbors and report the results in Figure A4. I find no significant difference in these measures, and the point estimates are very close to zero.

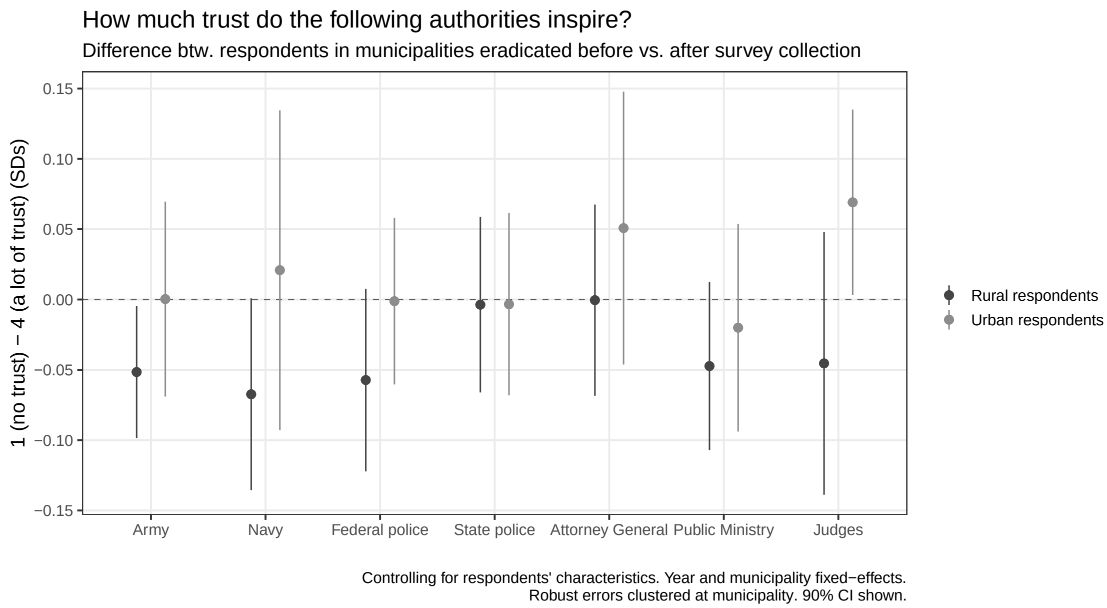

Figure 3: Figure plots the 90% confidence intervals of the difference-in-means ($EradicationBefore_{t,m}$ in specification 3) in self-reported trust in each authority for respondents living in rural areas of municipalities eradicated before vs. after the survey was collected. All specifications include year and municipality fixed-effects and controls for respondent-level characteristics (age, sex, and educational attainment). Robust errors are clustered at the municipality level.

6 Alternative Explanations

6.1 Income

Thus far, results show that eradication decreases turnout and trust in the army. While the hypothesized mechanism hinges on citizens' changes in beliefs and electoral incentives, a reasonable concern is that the loss of income could mechanically depress participation. While extant research on the correlates of income and voting in Latin America finds a null or weak association between the two (Carreras and Castañeda-Angarita, 2014), the economic interdependence of illicit crop-growing towns[18] makes the income channel essential to test.

To explore the importance of the economic channel, I leverage the 2018 collapse of the price of poppy due to the increased demand 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*). Thus, the lost income from an eradicated field in 2015 was significantly higher than the

[18]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 resources obtained through illicit harvesting.

lost income from one eradicated just three years later. Section A7.1 in the Appendix describes the analysis in detail. The estimated effects of eradication on turnout are of comparable magnitue both in 2015 and 2021. 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.

6.2 Compositional changes

Next, I consider the possibility that the identified effects are a function of population changes. I first consider whether arrests could mechanically decrease turnout by reducing the population in an electoral precinct. Using a freedom of information request, I obtained data on the number of people arrested for illicit-crop farming each year. Although growing illicit crops is a felony that warrants jail time, the data show that the penalty is not routinely enforced. Specifically, while the mean yearly number of sentences for the crime of illicit-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.

Alternatively, 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 (Le Cour Grandmaison, Morris and Smith, 2019*b*). 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 the number of people who changed their voting address and the electoral precinct they moved from and to. 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 INE ID card (Finan, Seira and Simpser, 2021). If there is a net deficit of people moving from precincts where the army eradicated fields to precincts where it merely detected fields, it will alert us that compositional changes could explain part of the effect mechanically. On average, 3.2 and 5.8 people moved from each precinct with eradicated crops to a precinct with crops and no eradication in 2015 and 2018, respectively. However, during those same years, 3.3 and 5.9 people moved from a precinct with crops and no eradication to a precinct with crops and predicted eradication. Consequently, the average net difference is small and *positive*. Overall, data on arrests and address changes do not suggest that population changes are mechanically driving the effects.

6.3 Violence

Lastly, I consider the possibility that eradication operations affect participation through increased violence. This alternative explanation holds that eradication affects cartels' relative strength, which fuels drug-related violence, resulting in lower turnout. It is improbable that eradication operations significantly affect drug cartels' income because often growers, not cartels, own the crops and absorb the economic costs (Álvarez Rodriguez, 2021*a*; Farfán-Mendez, 2021). Further, cartels have diversified to synthetic drugs like fentanyl (DEA, 2021), unaffected by drug eradication operations. Lastly, even if field destruction negatively impacted drug-trading organizations' finances, illicit crop growing is the step with the least value added in the drug-trafficking chain and, thus, the least likely to result in violent cartel readjustment.

However, as a descriptive exercise, I look at whether official municipal-level data on eradication predict changes in lethal violence one month and two months after. As Figure A7 shows, eradication is a poor predictor of homicide down the line. Using a two-way fixed-effects design, I find no relationship between eradication and the change in lethal violence the following month and a close-to-zero relationship between eradication and lethal violence two months later.

7 Conclusion and Discussion

In this paper, I emphasized how the US focus on source country operations has deeply influenced counternarcotics policy in drug-producing countries, incentivizing their governments to focus on supply reduction. I examined the case of illicit crop eradication operations in Mexico to explore the political consequences of adopting supply-reducing counternarcotics tactics for producing countries. Attitudinally, I show that such operations decreased trust in the army, consistent with ethnographic research showing that these army-led operations are understood as unjust and aggressive by crop-growing communities. Behaviorally, I show that army-led crop eradication operations depressed electoral participation in Mexican federal elections. I interpret the latter result as evidence that aggrieved individuals correctly understand eradication policy as unresponsive to their preferences. With a battery of ancillary analyses, I explored the possibility that measurement error or selection bias could spuriously drive the results, finding little evidence to support such claims.

The results have important implications for policing and security in Latin America, where 17 of the 22 countries identified as major drug transit or drug-producing centers are. Latin America is the most violent region in the world (Vilalta, 2020). Governments have responded to domestic security concerns by investing in their coercive capabilities, militarizing their police, and increasing law enforcement efforts (Lehman, 2006; Brinks, 2007; Flores-Macías and Zarkin, 2021). However, these Latin American governments have simultaneously invested significant resources and effort in supply reducing operations. This paper suggests that efforts to reduce supply, when understood as unfair and aggressive by grower communities, can depress trust and potentially undermine electoral accountability. If trust facilitates cooperation and makes policing more effective, as some evidence suggests (Peyton, Sierra-Arévalo and Rand, 2019; Skogan, 2006), then by alienating people who live in the peripheries of crime, source country operations might be especially effective at undermining efforts to improve domestic security.

Last, by emphasizing how participation is a strategic decision taken in a political context constrained by the preferences of other political actors, the results have important implications for the study of voter behavior. Governments are politically motivated to tailor policy to fit voters' preferences *and* the preferences of other organized interests, like lobbies, corporations, and foreign countries. The relative strength of these distinct actors will produce policy that is "good for some people and bad for others, depending on who has the power to impose their will." (Moe, 2005) By the same token, we should expect heterogenous political responses that follow from differences in the political context from within which voters react. Specifically, this paper stresses how voters, relatively weakened by pressure exerted through foreign aid, can opt to withdraw from electoral politics instead of mobilizing. Such a reaction on the part of voters can dilute the connection with their elected representatives and further weaken voters relative to other organized interests, increasingly aggravating the dynamic.

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

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Contents

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Appendix A Military Geography

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

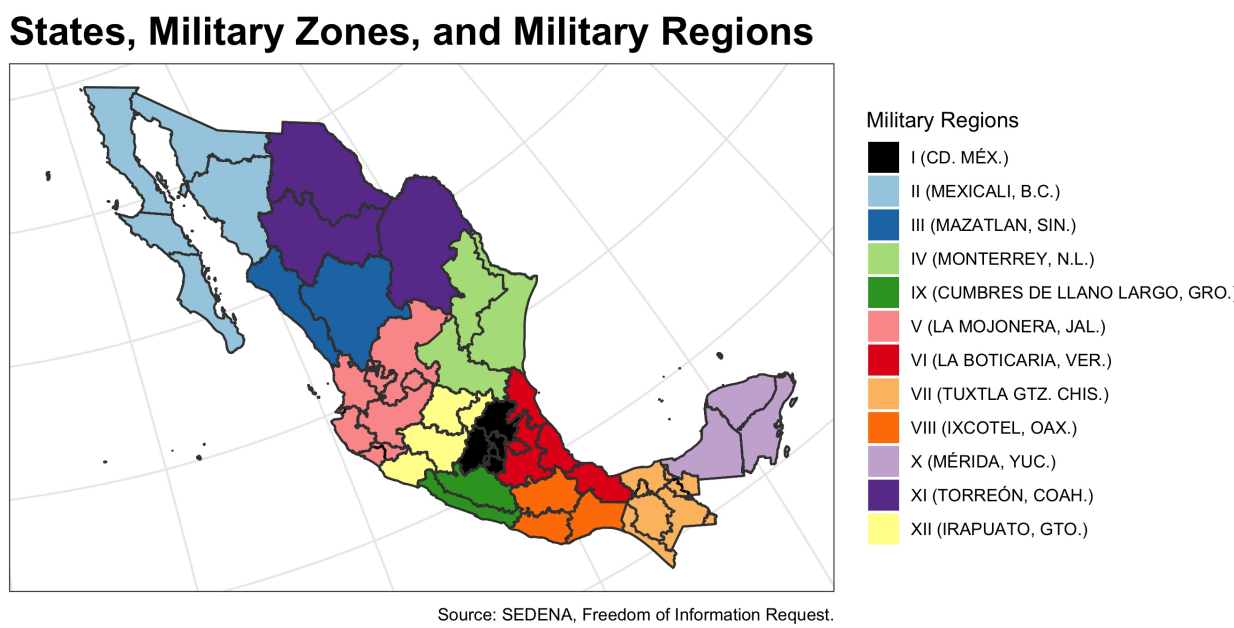


Figure A1: The map colors municipalities by the military region to which they belong, and shows the borders of military zones in black.

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.

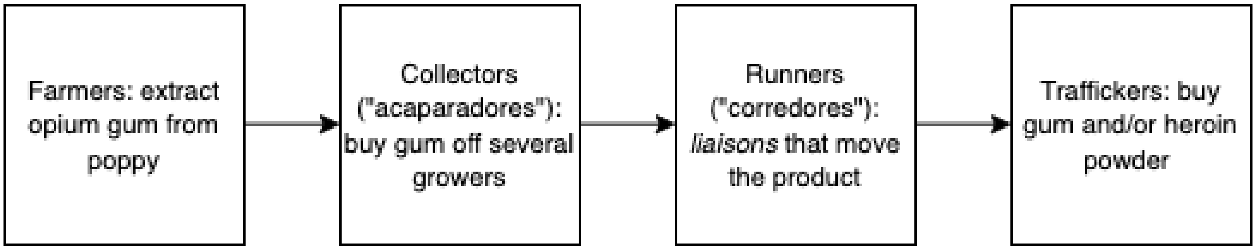


Figure A2: The figure describes the drug-trafficking process, starting from the selling of poppy farming in a town of the Sierra de Guerrero, as described in Álvarez Rodriguez (2021a).

Appendix C 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 the effect estimated from the continuous mesure 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.

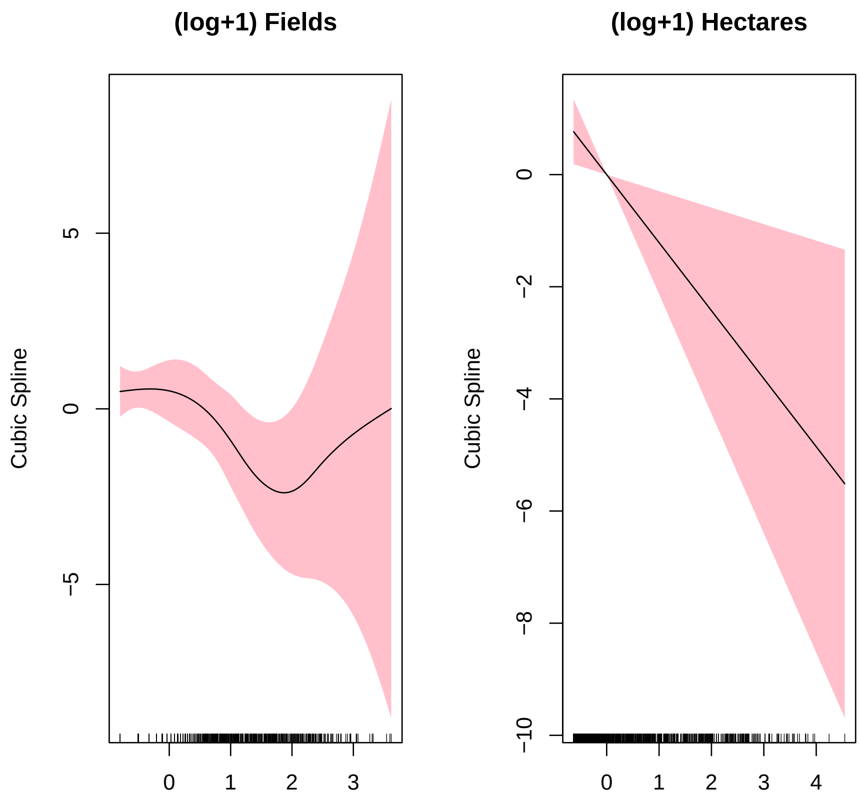


Figure A3: Figure shows the result of fitting smoothing cubic splines with 10 knots on a model with hectares or fields as the independent variable, and turnout as the dependent variable.

Appendix D Eradication: Precinct level results

A4.1 Sample

The sample I use for the precinct-level results consists of all precincts where the army detected an illicit field during a federal election year. To measure eradication, I combine data on all fires detected by NASA satellites with the geolocated information on detected fields. With these data, I construct a measure of precinct-level eradication as described in section 4.3 of the main paper. Table A1 provides basic summary statistics of the resulting precinct-level sample.

Predicted eradication?			No					Yes		
Variable	N	Mean	Min	Max	Sd	N	Mean	Min	Max	Sd
Turnout (%)	701	52.29	5.49	97.73	16.51	336	56.73	5.9	99.4	15.25
Detected fields (count)	701	8.46	1	206	17.15	336	29.24	1	417	46.27
Destroyed fields (count)	701	-	-	-	-	336	7.73	1	97	13.01

Table A1: Table shows basic summary statistics according to presence or absence of predicted eradication at the precinct level.

A4.2 Results with municipal fixed effects

Table A2 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 A2: Illicit-crop eradication and turnout in federal elections for deputies: precinct-level results. Dependent variable measures turnout as share of all registered voters in the precinct. Robust standard errors clustered at the precinct level.

A4.3 ICWa construction

I construct the following inverse covarance weighted average that synthesize a battery of sociodemigraphic characteristics. For each electoral precinct i, the ICW willl equal

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

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

Appendix E Trust

A5.1 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 A4 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.

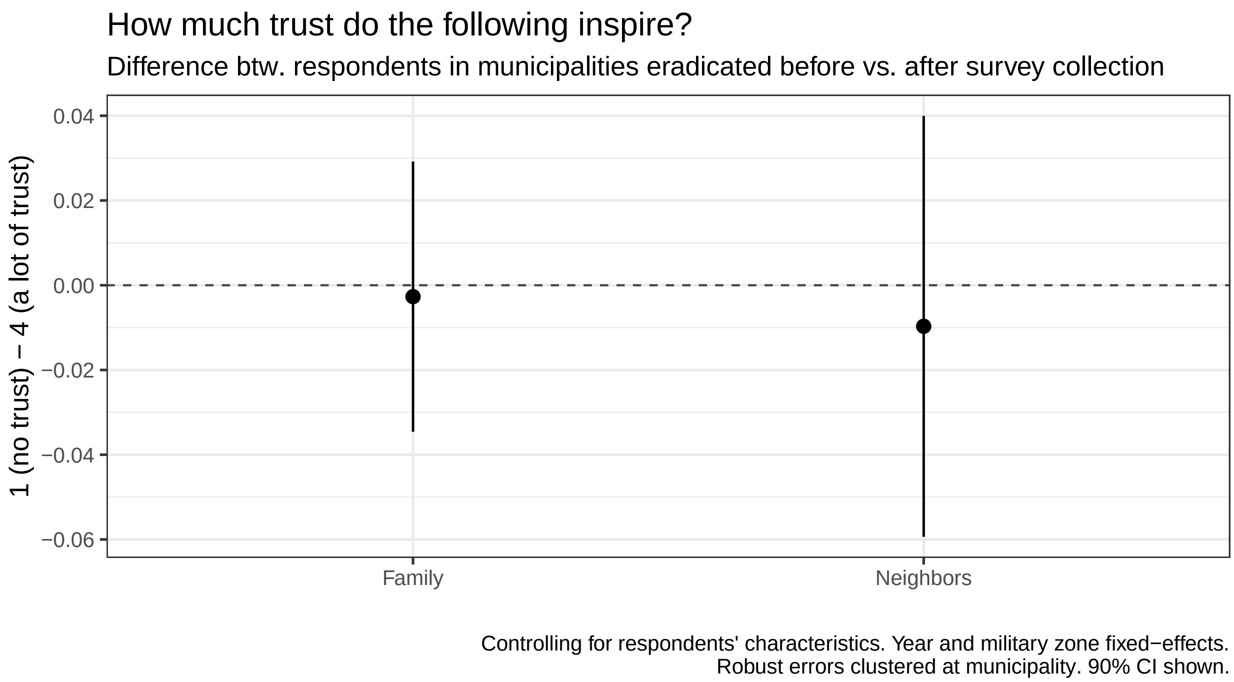


Figure A4: Difference-in-means in two measures of trust reported by ENVIPE respondents living in municipalities eradicated before vs after survey was collected.

A5.2 Figure 3: Figure 3: Full Results

Table A3 shows the full results of the difference-in-means in self-reported trust in law enforcement institutions, plotted in Figure 3.

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 = 1277	-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

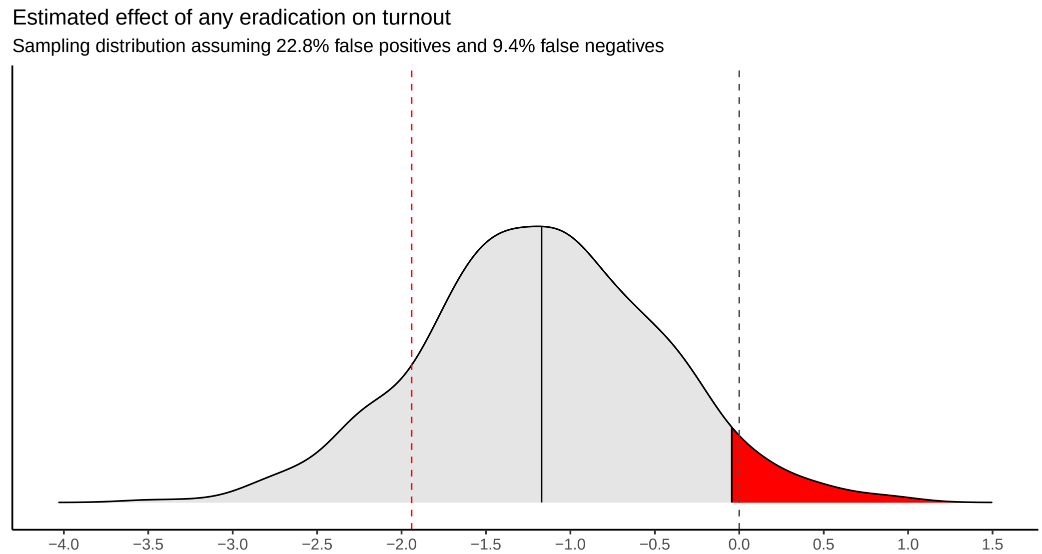
Table A3: Table corresponds to Figure 3 in the main paper. Robust standard errors clustered at the municipality are shown in brackets. The dependent variable is the standardized response to the question "How much trust do the following authorities inspire?" measured on 1-4 scale. Columns show the estimated 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.

Appendix F Measurment and Selection Issues

A6.1 Missclasification

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 it detected with 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 units. 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.



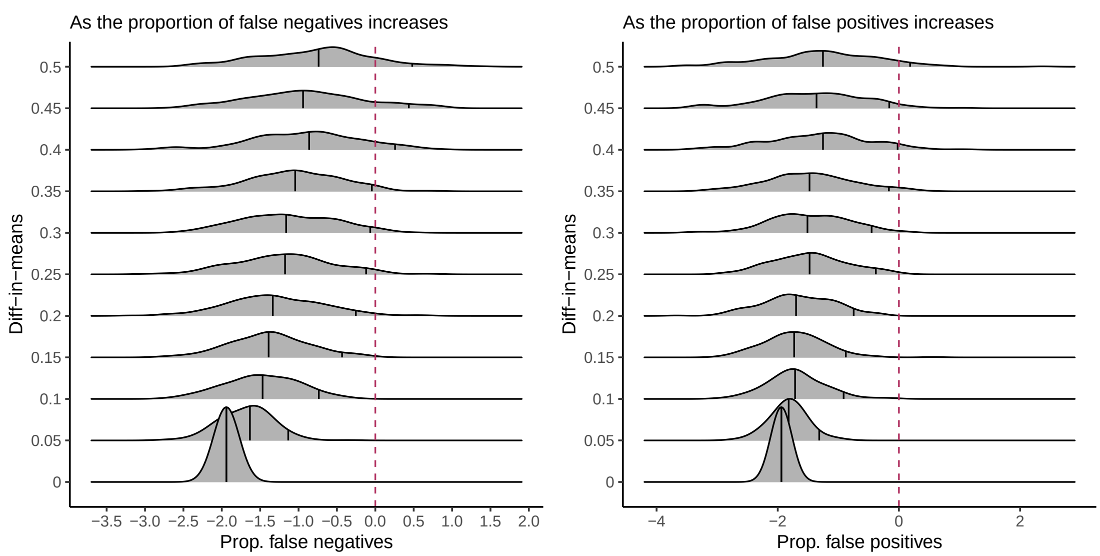


Figure A5: The top panel plots the estimated sampling distribution of the effect of eradication on turnout, assuming 22.8% of the observations classified as 'eradicated' are false positives and 9.4% of the observations classified as 'not eradicated' are false negatives. The bottom left panel show the 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.

A6.2 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 A4 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 A4: Do geographic characteristics predict field eradication? Confusion table from predicting eradication using the geographic characteristics of detected fields.

Appendix G Alternative Explanations

A7.1 Income

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

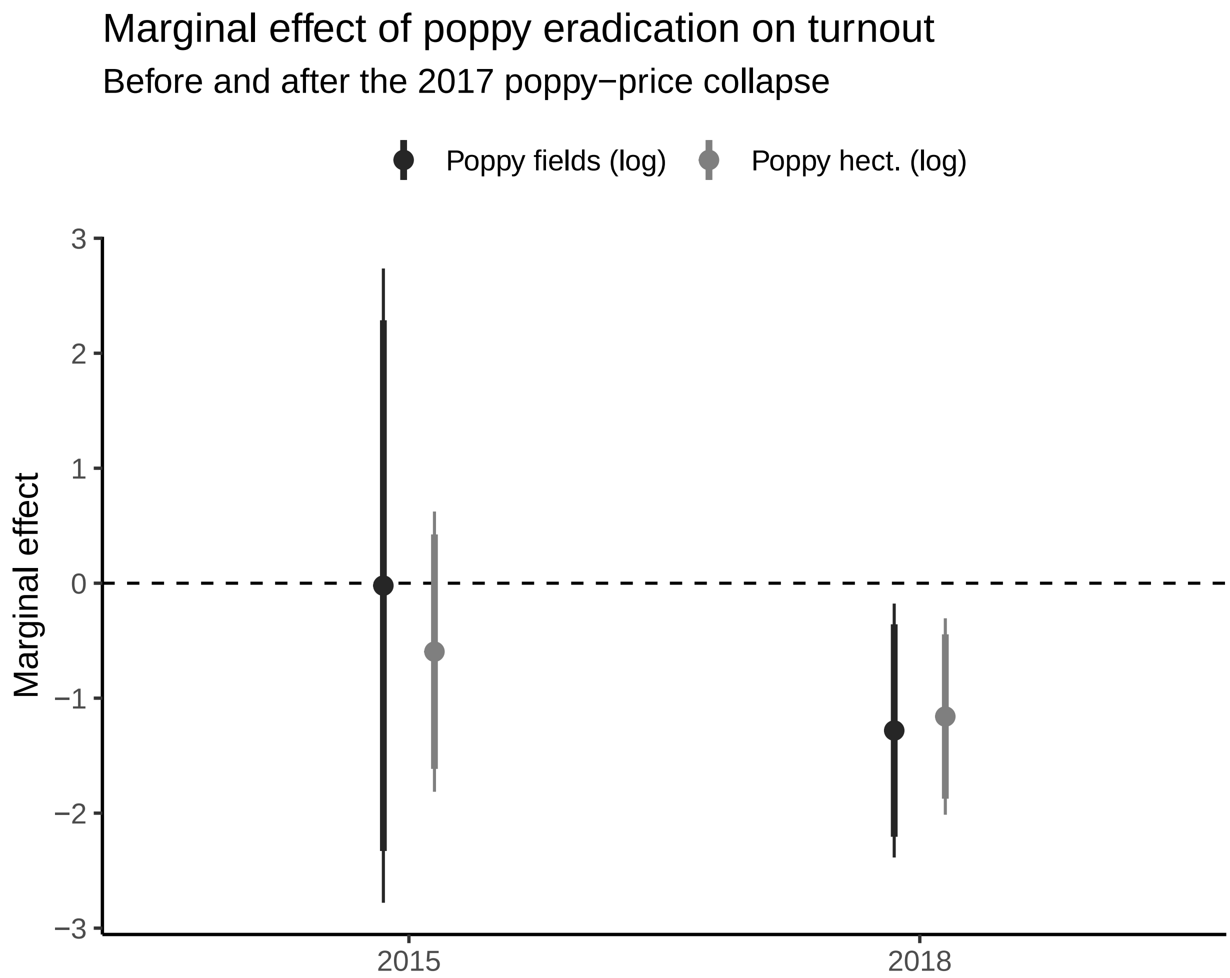


Figure A6: Figure plots the marginal effect per year of a model with turnout as a dependent variable, and the log+1 number of eradicated poppy fields/hectares as the main independent variable. All controls and fixed-effects are included. Only electoral precincts with detected poppy fields are included in the control group.

A7.2 Lethal violence

In this subsection, I consider the possibility that eradication operates on participation by changing the intensity of violence perpetrated by drug trading organizations (DTOs). If, for instance, DTOs respond to eradication by trying to capture new territory, or if eradication weakens DTOs and incentivizes competitors to attack, then eradication might increase violence, which then might depresses electoral participation. To test descriptively, I analyze whether eradication operations predict downstream violence.

Table A5: Homicide rate and eradication

	Change (t-t+1)	Homicide rate t+1 (log)	Homicide rate t+2 (log)
Eradicated hectares (log)	-0.016	0.017**	0.015**
	(0.028)	(0.005)	(0.005)
Num.Obs.	17008	187421	186259
R2 Adj.	0.097	0.201	0.202
Fixed effects: Year	Yes	Yes	Yes
Fixed effects: Municipality	Yes	Yes	Yes

Robust errors clustered at the municipality
+ $p < 0.1$, * $p < 0.05$, ** $p < 0.01$

I estimate the following two-way fixed-effects model:

$$Violence_{mt} = \gamma Eradication_{mt} + \mu_t + \theta_m + \varepsilon_{mt} \tag{1}$$

where $Violence_{mt}$ is a measure of lethal violence in municipality m during month t and $Eradication_{mt}$ is 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. Results show a weak, but statistically significant positive association between eradication and the (log) municipal homicide rate one and two months after eradication. However, Figure A7 shows how eradication is a very poor predictor of lethal violence.

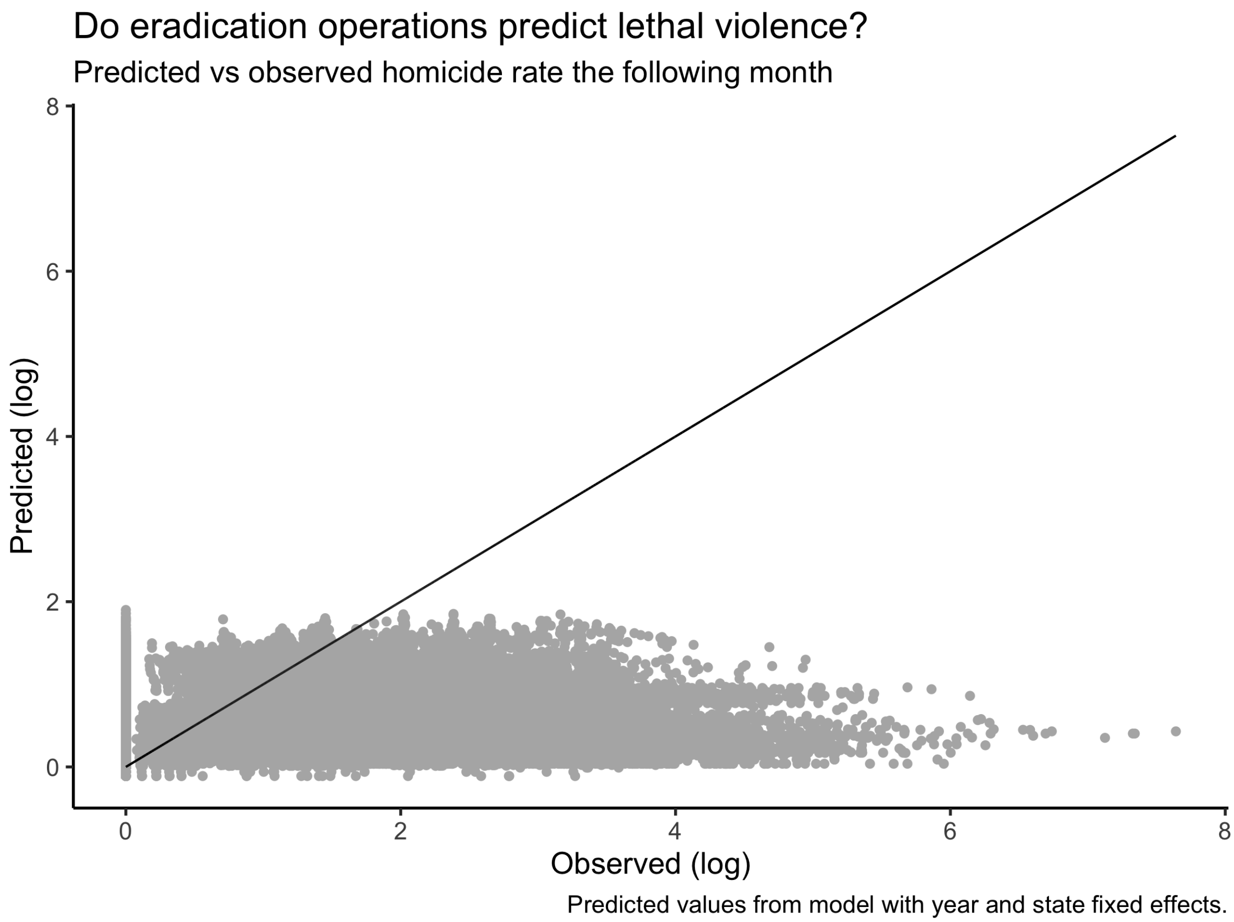


Figure A7: Figure plots the observed homicide rate per municipality (x axis) vs the predicted (log+1) number of eradicated hectares in the municipality (y axis). As can be seen, lethal violence is a poor predictor of eradication.