Do Illegal Drug Markets Breed Violence? Evidence for Colombia*

Daniel Mejía† Pascual Restrepo‡

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Abstract

This paper proposes a novel identification strategy in order to disentangle the causal impact of illicit drug markets on violence using data for Colombian municipalities. We take advantage of two sources of exogenous variation. First, due to technical reasons related to soil quality, temperature and climate conditions, coca cultivation is more productive at low altitudes. Second, if it is indeed true that illegal drug markets breed violence, external demand shocks for Colombian cocaine should be reflected in higher levels of violence. Importantly, this effect should be stronger in municipalities located at low altitudes. Using these two sources of exogenous variation, we estimate the causal impact of the size of illegal drug markets on violence in Colombia. We partially test the exclusion restriction using data for 1990-1993, showing that the altitude of municipalities does not have a direct effect on violence prior to 1994 (when the level of coca cultivation in Colombia was relatively low and unimportant). Our estimations indicate that, on average, a 10% increase in the value of coca cultivation increases the homicide rate between 1.2% and 2%, and forced displacement between 6% and 10%.

Keywords: Violence, Illegal Markets, Cocaine, Colombia.

JEL Classification Numbers: D74, K42.

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†Universidad de los Andes, e-mail: dmejia@uniandes.edu.co

‡Corresponding author. Universidad de los Andes, e-mail: p.restrepo23@uniandes.edu.co.

1 Introduction

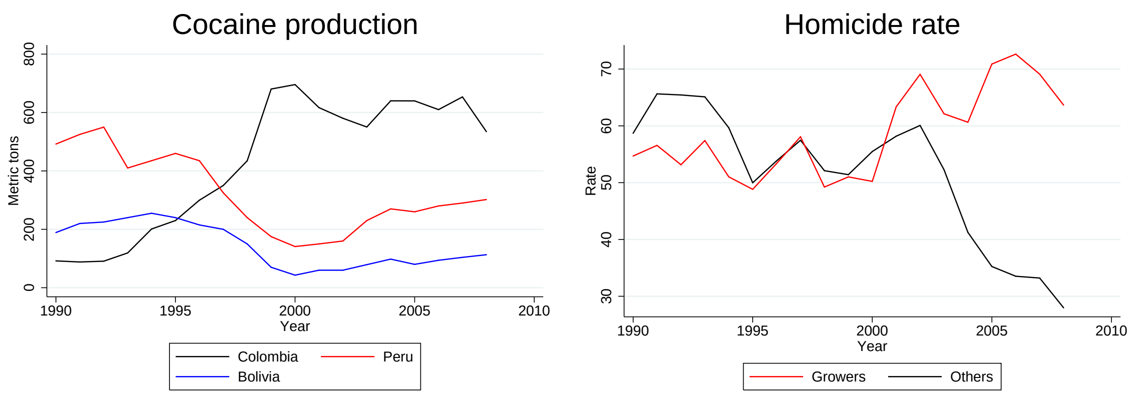
It is widely held by economists and other social scientists that illegal markets tend to be more violent, not necessarily because of the type of goods being transacted, but because of their illegal status. Despite the well documented correlation between illegal markets and violence, the direction of causality remains a puzzle: do illegal markets cause violence or does violence lead the expansion of these markets? Another possibility is that a third variable is responsible for the positive correlation between illegal markets and violence. Despite these difficulties, uncovering the direction of causality is crucial for designing policies aimed at reducing violence and controlling illegal markets. On the one hand, if higher levels of violence lead to the creation of illegal markets, enforcement against these markets by itself might not be enough to reduce supply; correspondingly, targeting armed groups may become a more effective policy. On the other hand, if illegal markets lead to more violence, enforcement against armed groups might not be enough to reduce the latter. In this case, unless the size of black markets is reduced, violence will prevail and armed groups will continue to be replaced so long as illegal market rents are present.

The existing empirical evidence concerning the relationship between illegal drug markets and violence is mixed. Cross-country evidence suggests that homicide rates are positively related to the intensity of drug enforcement (see Miron (2001)). On the one hand, evidence from Afghanistan indicates that the direction of causality runs from illegal drugs to violence; that is, drug production increases in places controlled by illegal armed groups; these destroy the infrastructure required for legal production, thus fostering illegal activities such as opium cultivation and heroin production (see Lind, Moene and Willumsen (2008)). Using spacial econometric techniques and data for Colombia's municipalities between 1994 and 2000, Díaz and Sanchez (2004) obtain a similar result; they show that the territorial expansion of armed groups in Colombia has led to increasing levels of coca cultivation. On the other hand, using a similar data set and period of analysis as that used by Díaz and Sanchez (2004), Angrist and Kugler (2008) show that the direction of causality runs in the opposite direction. More precisely, they show that violence increased more sharply between 1994 and 2000 in departments that had more coca cultivation in 1994. Angrist and Kugler (2008) show that the closure of the air bridge connecting coca cultivation areas in Bolivia and Peru with cocaine processing facilities in Colombia in 1994 led to significant spikes in violence, especially in those Colombian departments prone to coca cultivation (e.g., those departments that had coca cultivation in 1994). However, Angrist and Kugler (2008) require the equal trends assumption between grower and non-grower departments in 1994. This assumption seems strong, as departments already cultivating coca in 1994 might have had very different non-observable characteristics from those that did not have coca cultivation that year, and these characteristics may have triggered different dynamics of violence over time.

Yet the relationship between illegal markets and violence is not necessarily intrinsic to the nature of illegal drugs. In fact, a recent paper shows that violence increased after the extraction and trade of mahogany (a tropical wood grown in the Brazilian Amazon) was declared illegal by the Brazilian government (see Chimeli and Soares (2010)). More precisely, the authors show how violence disproportionately increased following prohibition in Brazilian states that had higher shares of mahogany extraction prior to prohibition; in states that reported higher extractions of "other tropical timber species" following prohibition[1]; and in municipalities where mahogany extraction was a natural phenomenon.

The case of Colombia is particularly relevant in terms of disentangling the relationship between illegal drug markets and violence, because the country has gone through long and pronounced cycles of violence and illicit drug production, with a significant degree of heterogeneity between coca growers' municipalities and non-grower municipalities (see Figures 1A and B). In short, the main goal of this paper is to contribute to this open debate by using sources of exogenous variation in the value of coca cultivation in Colombia in order to disentangle the relationship between illegal drug markets and violence.

Figure 1: Cocaine production and homicide rates



(A) (B)

Coca, the raw material used for producing cocaine, is typically grown between 0 and 1700 meters above sea level. At low altitudes, where the soil and climate conditions are more favorable, and productivity for the cultivation of coca is greatest.[2] Not surprisingly, 93.5% of the coca crops in Colombia are located in municipalities below the mean altitude[3] (see Mejía

[1]Chimeli and Soares (2010) provide convincing evidence that mahogany extraction continued after prohibition, only that it was reported as the extraction of "other tropical timber species".

[2]Nevertheless, coca can be grown at higher altitudes, but at a lower productivity (e.g., fewer kilograms of pure cocaine are produced per hectare of coca crops per year).

[3]About 1,165 meters above sea level.

and Rico (2010) for a thorough description of the process of coca cultivation and cocaine production in Colombia). This observation, combined with different measures of external demand shocks for Colombian cocaine that vary over time, allows us to construct an exogenous instrument for the value of coca cultivation, which varies both across municipalities and over time.

Our identification strategy relies heavily on the observation that coca cultivation is more profitable in municipalities located at low altitudes. Thus, if it is indeed true that illegal drug production leads to higher levels of violence, shifts in the value of cocaine induced by external factors should have a higher impact on the level of violence in municipalities located at low altitudes. This is the basic intuition behind our identification strategy. In order to carry it out, we use different measures of exogenous demand shocks for Colombian cocaine together with municipalities' altitudes. More specifically, we use exogenous shifts in the demand for Colombian cocaine arising from both upstream and downstream markets. First, following Angrist and Kugler (2008), we characterize the closure in 1994 of the air bridge connecting coca cultivation centers in Peru and Bolivia and cocaine processing facilities in Colombia as an exogenous demand shift for Colombian coca. Importantly, the closure of the air bridge was carried out by Peruvian authorities, and had nothing to do with the levels of violence in Colombian municipalities. Second, we use cocaine interdiction rates in other producer countries - that is, we use the ratio of cocaine seizures to potential cocaine production in Peru and Bolivia as a proxy for exogenous shifts in the demand for Colombian cocaine. Third, we use interdiction rates in upstream markets (transit and consumer countries). These constitute yet another exogenous demand shock for Colombian cocaine.

Our results indicate that a larger size of illegal drug markets leads to higher levels of violence. In particular, we show that places with greater drug cultivation - due to exogenous technical reasons (like soil and temperature conditions such as are implied by altitude) - become more violent in the presence of positive demand shocks for Colombian cocaine.[4] Importantly, we can partially test the exclusion restriction using data for 1990-1993, when coca cultivation in Colombia was relatively low and unimportant. In particular, we show that municipalities' altitudes did not have an impact on violence prior to 1994.

Our evidence is consistent with the view that illegal drug markets increase violence in producer countries such as Colombia via the increased conflict over the control of land for cultivating coca and over the control of strategic corridors for transporting illegal drug shipments abroad (Mejía and Restrepo (2008), and Restrepo (2011)). In particular, we show that a larger size of illegal drug markets not only leads to higher homicide rates, but also to

[4] This relationship cannot be explained by claiming that drug consumption generates violence, since most of the cocaine produced in Colombia is exported.

higher levels of forced displacement and violent attacks perpetrated by those groups actively involved in cocaine production and trafficking. One possible source of criticism for our results is that it is not illegal drug markets *per se* that lead to more violence, but rather enforcement against illegal activities. However, we control for different measures of enforcement against drug production and trafficking, such as the intensity of aerial eradication campaigns, drug-related captures and other broad measures of enforcement, and show that our results are not affected by the inclusion of these enforcement measures.[5]

The paper is divided into five sections, of which this introduction is the first. Section two describes in detail the data used in the empirical exercise; section three describes our identification strategy; and section four presents the results. Section five provides some concluding remarks.

2 Data description

We use a panel of Colombian municipalities for the years 1990 through 2008. The homicide rates are obtained from the Colombian Vice President's Office, and are available for the entire period. Coca cultivation figures from the SIMCI[6] are available at the municipality level from 1999 through 2008. We also have estimates for coca crops for 1994 at the municipality level, which we use in our second identification strategy. Our variable of interest is the value of coca production in a given municipality, calculated as the product of the area under cultivation (the number of hectares of land with coca crops per municipality), productivity per hectare (the number of kgs of cocaine per hectare per year), and the farm gate price of cocaine. We use national figures for productivities and prices from UNODC yearly reports, inasmuch as there is no good data at the municipality level. Although productivity and prices may vary by region or municipality, using the national average only introduces measurement error, which is corrected by our IV approach.

We obtain municipality level covariates from the CEDE's municipality panel, including one of our instruments - altitude. Our covariates include exogenous demographic controls, such as distance to the capital city, Bogotá, or main markets; soil aptitude and erosion; the presence of tropical diseases; dummies for the strategic or tactical location of the municipality; etc. These geographic controls are important because they are potentially correlated with altitude, and could, to some extent, determine violence. We also include time varying covariates, including population density, rurality and tax revenues. The latter works as a

[5]Using a panel of Colombian municipalities, a related paper finds that drug enforcement, measured by drug-related captures, is not related to violence (see Medina and Martínez (2003)).

[6]Sistema Integrado de Monitoreo de Cultivos Ilícitos - a United Nations Office for Drugs and Crime in Colombia.

proxy for income and socioeconomic conditions. In order to take into account long-term differences in the level of development, we also include variables such as the Gini coefficient; the efficiency of the judiciary; infrastructure and sewer coverage; the number of NGO's per capita; and the presence of several institutions and state-sponsored programs. Some of these measures are taken from OCHA Colombia, and are measured for the year 2000. We treat the latter as fixed for the years 1999 through 2008, since they are highly persistent.

In our robustness exercises, we use additional covariates. These include the total eradication at the municipality level for the years 1994 through 2000, and the number of drug-related captures (normalized by population) available at the state level from 1990 through 2008. Both measures were taken from the Colombian police. We also include additional proxies for enforcement, which do not vary during our period of analysis. These include the distance to a battalion, the presence of police stations, and per capita expenditures on security. These variables are our proxies for drug-related enforcement, and we use them as controls in some of our specifications. We also use dummies for the presence of armed groups (FARC, ELN and AUC) from the CEDE in order to control for the expansion of these groups that took place during the years of our analysis. Finally, we use alternative dependent variables in some specifications, including the rate of forced displacement and the robbery rate. Table 1 shows the summary statistics of the main variables used in our empirical analysis.

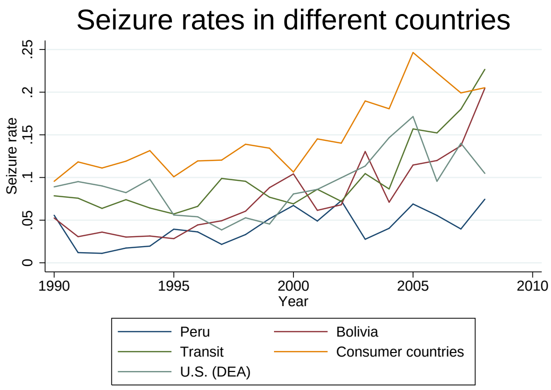
[INSERT TABLE 1 HERE]

For our second identification strategy, we require aggregate drug enforcement measures from other countries. We focus on the seizure rate (the ratio of drug seizures to drugs available in the market at each stage) in source countries other than Colombia (Peru and Bolivia), transit countries and consumer countries. All these rates are calculated using UNODC reported seizures for each year from 1990 through 2008. We focus on the rate and not the total quantities seized, because the former measures the intensity of enforcement, while the latter does not, and might be directly related to Colombian production, thus making it endogenous. We also use DEA figures for U.S. seizures and drug-related arrests in order to construct seizure and arrest rates for the U.S. during this period. Figure 2 shows the evolution of these measures between 1990 and 2008 (apart from the arrest rate in the U.S.).

3 Identification strategy

Many observers have pointed out that cocaine production in Colombia has led to the escalation of violence. Theoretically, since the rents from drug production accruing to the 'owners'

Figure 2: Evolution of enforcement in other countries different from Colombia



of land are proportional to the value of coca production in a municipality, one would expect more conflict in municipalities with greater production (as measured by its value). The illegality of cocaine production plays a central role in this argument, because land used for this illegal activity does not have well defined property rights. Thus, armed groups find it profitable to fight for control of land, instead of renting it in a formal land market under the state's radar. Consequently, illegal drug producers cannot request protection from the State for their property (cocaine processing facilities and laboratories, chemical precursors, vehicles, etc.) when others try to expropriate it (see Naranjo (2007), and Restrepo (2011)).

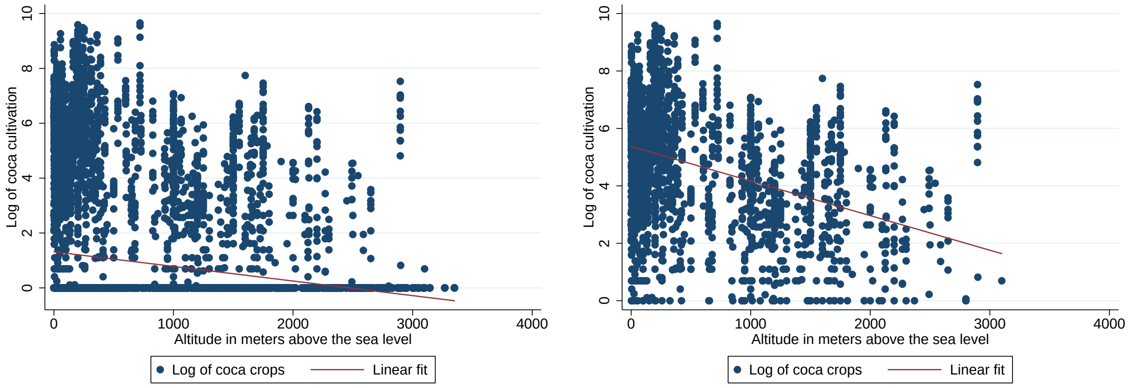
Additionally, prohibition makes the use of violence profitable relative to non-violent production. For instance, if drug production were legal, producing cocaine by renting land in places with a strong state presence would be more profitable than forcing farmers off their land. However, under prohibition, the first strategy becomes more costly relative to the second one. Consequently, more producers resort to violence and coercion to fulfill their objectives. Furthermore, the conflict over the control of arable land and strategic corridors does not only involve armed groups fighting between themselves or displacing farmers. It also involves a conflict against the state over the effective control of land and drug routes. Thus, yet again, cocaine production generates violence, as producers are willing to fight the state in order to maintain their rents. Moreover, the higher the value of the rents associated with illegal drug markets, the more they will fight to keep them. Illegality is crucial here, because armed groups would not be competitive if the state allowed legal production in places it already controls. In sum then, there are theoretical reasons for suspecting that an increase in the value of cocaine production in a municipality will cause violent outbursts.

However, testing this simple argument empirically entails many problems, and a positive correlation between violence and the value of cocaine production is not enough to conclude that illegal drug markets breed violence. First, we don't know if production is concentrated in places that were already violent prior to illegal production. Second, we have a reverse causality problem; that is, it could well be the case that violence causes cocaine production. For instance, if violence and conflict destroy the infrastructure required for legal production, it could force populations in isolated areas to shift to cocaine production. Third, we don't know if a third factor is causing both phenomena. For instance, a weak state presence can generate greater cocaine production and more violence at the same time, without it necessarily being the case that the two variables are directly related. In short, coca cultivation and cocaine production are endogenous. Thus, in order to estimate its causal impact on violence, we need an exogenous source of variation that, on the one hand, affects coca cultivation and its value, but, on the other hand, is not directly related to other determinants of violence.

One potential exogenous determinant of coca location and cultivation is a municipality's altitude. For technological reasons related to soil and climatic conditions, coca bushes are more productive at lower altitudes. Thus, one would expect that for any price level, a municipality located at a low altitude will have more land with coca crops, and that this land would be more productive. Consistent with this observation, Figure 3, panel A, shows that there is a strong first-stage negative relationship between coca cultivation and altitude, while panel B shows that this relationship holds in the intensive margin as well. To our knowledge, there is no direct relationship between altitude and other determinants of violence once we control for the most important observable variables at the municipality level (geographic controls, socioeconomic conditions, the level of development, demographic controls, size, etc.). Thus, the relationship between altitude and coca cultivation occurs due to exogenous technological reasons that make crops more productive at low altitudes, yet which are not directly related to other determinants of violence, such as state presence, culture or the rule of law. If altitude is exogenous to violence conditional on observables, an IV approach using altitude as an instrument for the value of cocaine production will consistently estimate the causal impact of the size of illegal markets on violence.

Our first approach is to use the exogenous variation in crops productivity and location implied by altitude in order to estimate the impact of the value of illegal cocaine production on violence. Since our instrument does not vary over time, the causal effect is locally identified from differences in violence across the municipalities that grow coca bushes. Thus, our local estimator calculates the impact of cocaine markets on violence by comparing the homicide rate between two similar municipalities (in observable controls), wherein one has a higher value of coca cultivation than the other due to being located at a lower altitude. This strategy correctly estimates the causal effect of the value of cocaine production on violence (as measured by the homicide rate) as long as altitude satisfies the exclusion restriction - that is, as long as altitude does not have a direct effect on violence and as long as altitude

Figure 3: Coca cultivation and altitude.



(A) (B)

is uncorrelated with other unobservable variables determining violence. Our first strategy consists of estimating the following model using 2SLS, instrumenting the value of cocaine production in municipality m and year t with its (fixed) altitude. We thus estimate the following model for the years 1999-2008:

$$\ln h_{mt} = \alpha_t + \beta_0 + \beta_1 \ln CV_{mt} + \theta X_{mt} + \lambda X_m + \varepsilon_{mt}, \tag{1}$$

where $\ln h_{mt}$ is the logarithm of the homicide rate in municipality m for year t[7]. α_t is a time effect controlling for national trends and β_0 is the common intercept. $\ln CV_{mt}$ is the log of the value of coca cultivation in municipality m at time t, calculated as the number of hectares of land with coca crops multiplied by their productivity that year (the number of kg of cocaine per hectare per year) and the farm gate price of cocaine. Note that our variable of interest is not the value of the coca leaves, but the value of cocaine production at the municipality level. We use the latter instead of the former because armed groups involved in cocaine production do not sell coca leaves, but rather use them to produce cocaine that they then sell to drug traffickers. We should stress that our productivity measures and drug prices vary by municipality, but as there is not enough data, and we are forced to use national averages. In any event, this would only generate measurement error, which is corrected by our IV approach. X_{mt} are time varying covariates at the municipality level, and include population, density, rurality and tax revenues. The last one is our proxy for economic development, since there are no GDP measures at the municipality level. X_m are municipality fixed characteristics, and include extension, soil quality, an index for soil erosion, the distance to markets and capital cities, the presence of main national roads, the presence of tropical diseases, the strategic location of the municipality, a dummy for

[7]In some specifications, we use the logarithm of the forced displacement rate or the illegal armed groups' attack rate as our dependent variable.

tactic corridors, and the number of ethnic groups. In some specifications, we also include municipality controls that do not vary over time, and which we only observe once, at the beginning of 2000. These include a measure of land inequality, the efficiency of the judiciary, infrastructure and sewer coverage, the number of NGOs per capita, and dummies for the presence of several institutions (churches, libraries, health posts, banks and so on) and government programs. These variables are included in order to capture long-term differences in the level of development and institutions across municipalities, that are likely to affect both coca cultivation and violence. Since we cannot include municipality-fixed effects (they are collinear with our instrument), it is essential to control for all these variables in order to guarantee that altitude is not capturing persistent differences between municipalities with respect to any relevant dimension. Otherwise, the exclusion restriction would not hold.

Our coefficient of interest, β_1, captures the causal effect of crop cultivation (measured by the value of cocaine production) on violence so long as altitude is exogenous in our first model (equation 1) - e.g., as long as the exclusion restriction is satisfied - and so long as it is a good predictor of crops location - e.g., as long as the instrument is not weak. The second condition can be tested, and we show that we do not have a weak instrument problem. As for the first condition, we can partially test it using data for the period 1990-1993, when coca cultivation was relatively low and unimportant in Colombia. During those years, most cultivation took place in Peru and Bolivia, and Colombia was only a marginal player in terms of coca cultivation. We are thus able to show that, conditional on our set of controls, altitude did not have a significant effect on the homicide rate during this period. This placebo test strongly suggests that altitude was exogenous to violence before 1994. Although this is not enough to guarantee its exogeneity (as structural determinants of violence may have changed over time), it is indicative of the fact that our instrument is not picking up unobservable heterogeneity (different from the value of coca cultivation) across municipalities that can consistently explain violence over time. The results from this placebo test, as well as its implications, are discussed in the next section. Additionally, we attempt to rule out other channels through which altitude might affect violence. These include the expansion of armed groups, especially paramilitaries, that occurred after 1998, especially in low altitude municipalities characterized by extensive cattle production.

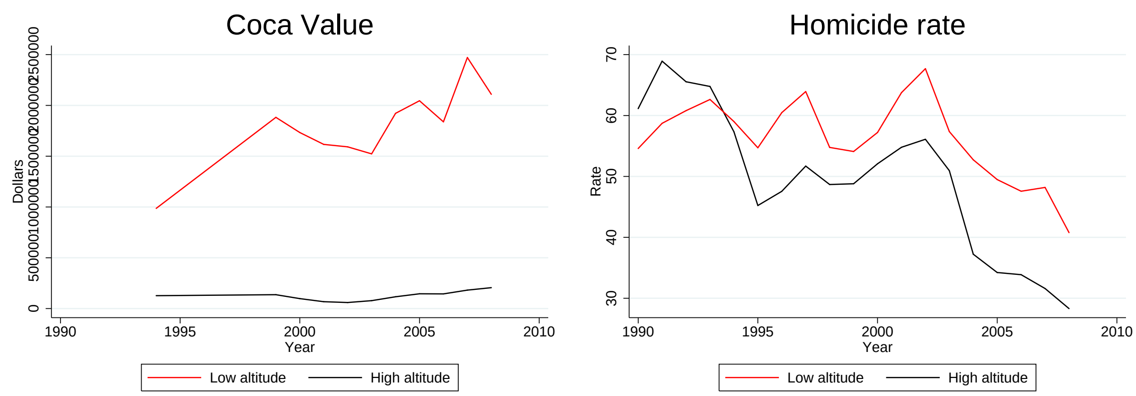
Although our first strategy consistently estimates our coefficient of interest provided that the exclusion restriction for altitude is satisfied, it has two weaknesses. First, it only identifies the local effect from differences across municipalities. Second, and related to the first point, we cannot include municipality-fixed effects, which would allow us to control in a more flexible manner for persistent differences across municipalities.

Our second approach strengthens our analysis by constructing a series of different time varying instruments, which allows us to include municipality fixed effects in our model and exploit variations over time within municipalities. In order to have more time variation, we also include cultivation figures for 1994, in addition to our data for 1999-2008.

Our time-varying instrument is the interaction of altitude and time-varying external demand shocks for Colombian cocaine. We capture the latter using the intensity of drug-related enforcement in other countries - drug seizures and drug-related arrest rates in downstream cocaine markets (other producer countries) and upstream cocaine markets (transit and consumer countries). The idea behind using enforcement in other countries as an exogenous shock to the demand for Colombian cocaine is that larger seizures and higher drug-related arrests rate in other source countries leads drug traffickers to substitute for Colombian cocaine, thus generating a positive and exogenous demand shock for it. In a similar fashion, enforcement in upstream markets (transit or consumer countries) leads traffickers to demand more drugs from source countries in order to compensate for their additional losses, as long as demand is inelastic (see Restrepo (2011) for a model rationalizing this ideas). Thus, increasing enforcement in upstream markets also generates positive and exogenous demand shocks for Colombian cocaine as long as the demand for cocaine is price inelastic. Importantly, these demand shocks are likely to disproportionately affect the value of cocaine production in municipalities located at low altitudes, which have soil and climatic conditions better suited for coca leaf cultivation. In a similar way, we also use an interaction of altitude and a dummy for the years after 1994 in order to capture the exogenous shift in demand from Peru and Bolivia to Colombia following the closure of the air bridge connecting coca cultivation centers in Peru and Bolivia and cocaine processing facilities in Colombia. As before, this positive demand shock for Colombian cocaine presumably affected more those municipalities located at lower altitudes, as they are more suited for coca cultivation. The idea of using the closure of the air bridge between Peru and Colombia is taken from Angrist and Kugler (2008), who use this variation in time to set up a DD analysis of the effect of coca cultivation on violence using variation across Colombian departments. Thus, our crucial assumption for identification is that, in the absence of coca cultivation, violence in similar municipalities with different altitudes would have follow similar trajectories in response to different external demand shocks.

The consequence of higher seizures of cocaine in other countries since 1994 has been an increase in the demand for Colombian cocaine and a reduction in the production of other source countries, as shown in Figure 1A. Moreover, the higher demand for Colombian cocaine has affected low altitude municipalities disproportionately, and has increased the value of cocaine production in these municipalities relative to those located at higher altitudes (see Figure 4A). Simultaneously, the homicide rate in high-altitude municipalities has decreased to a greater extent than in low-altitude ones (although it has decreased for the country as a whole), as shown in Figure 4B. In fact, prior to 1994, the homicide rates between the two groups were roughly equal; since 1994, low-altitude municipalities began to experience more violence relative to high-altitude ones. These figures suggest that the rise in coca cultivation and cocaine production since 1994 - as a consequence of explicit policies implemented by other governments - is the main factor behind the difference in the evolution of violence between low-altitude and high-altitude municipalities. In short, our second identification strategy exploits these patterns in a two step approach, in order to identify the causal effect of the rise of illegal drug markets on violence.[8]

Figure 4: Cocaine production in the Andes.



(A) (B)

Our second strategy consists of estimating the following model using 2SLS, instrumenting the value of cocaine production at municipality m in year t, with its altitude interacted with different measures of drug-related enforcement in other countries. We thus estimate the following model for the years 1994 and 1999-2008:

$$\ln h_{mt} = \alpha_t + \gamma_m + \beta_1 \ln CV_{mt} + \theta X_{mt} + \varepsilon_{mt}. \tag{2}$$

All variables have the same definition as in the first model (see equation 1), though this one also includes municipality-specific intercepts, γ_m, and we no longer require municipality covariates that do not vary over time, X_m.

For this strategy to consistently estimate the causal effect of the value of cocaine production on violence, our instrument must be exogenous. In this case, this is equivalent to saying

[8]We label municipalities as "low altitude" when they are below 1,165 meters (the average altitude of Colombian municipalities). Otherwise they are labeled "high altitude" municipalities. It is obvious that the use of this threshold allows us to assign 50% of Colombian municipalities to each group. Nevertheless, it is worth pointing out that the patterns described are robust to changes in this threshold.

that the exogenous demand shocks for Colombian cocaine do not have a direct effect on violence, besides its effect through cultivation, such that it differs systematically between low and high altitude municipalities. Also, there cannot be unobservable time varying variables correlated with altitude and demand shocks that also determine violence. Since we include municipality effects, there is no risk of a bias arising from long term differences between municipalities with different altitudes. In the next section, we discuss this assumption in detail and control for potentially confounding factors.

4 Results

We start with our first approach by estimating model 1 by OLS. Table 2 shows the first set of results. For all specifications, we include time and department effects; standard errors are clustered at the municipality level. In the first column, we report the results without any controls. In the second column, we add geographic controls, including extension, the distance to main cities and markets, soil quality and erosion, the presence of tropical diseases, the tactical and strategic location, and the number of ethnic groups. In the third column, we add demographic covariates that vary over time, including population, density and rurality. In column four, we add municipality tax revenues, which vary over time and capture a proxy for the level of income in a municipality. In the fifth column, we add time invariant municipality characteristics that try to capture differences in the level of development, as described in the previous section. Finally, the last column is the same as the fourth, but instead we use lagged values for all time varying covariates (their value prior to 1994), since these may also be affected by our variable of interest. The estimated impact of the value of cocaine markets on violence is very stable across specifications, and implies that a 10% increase in the value of coca cultivation in a municipality increases the homicide rate by about 0.4%, with the effect being significant at all traditional levels.

[INSERT TABLE 2 HERE]

However, we cannot give these estimates a causal interpretation because coca cultivation is endogenous, as explained above. However, the direction of the bias is not clear. On the one hand, there is reverse causality, along with omitted variables, that will bias our coefficient upwards, implying that the OLS strategy overestimates the true effect. On the other hand, given the illegal nature of coca cultivation and cocaine production, we know that the data is measured with error, and hence that the OLS coefficients may be attenuated. To conduct our IV approach, we begin by documenting the first stage relationship between illegal drug markets (measured by the value of the cocaine produced) and altitude. Table 3 shows the first-stage relationship between altitude and the value of cocaine across municipalities for the years 1999-2008. We estimate a negative and significant relationship, which implies that low-altitude municipalities have higher levels of cultivation, and hence cocaine markets with greater value. Importantly, the F statistic is always above 36, indicating that we are not confronted with a weak instrument problem. Thus, even after controlling for the geographic, demographic, economic and development characteristics of the municipalities, together with time and department fixed effects, we find a strong negative relationship between the value of cocaine production and altitude. All of these models have standard errors clustered at the municipality level, and F statistics adjusted accordingly.

[INSERT TABLE 3 HERE]

Using our first stage results, we are able to estimate model 1 using 2SLS. Table 4 shows the results for the same specifications presented in the previous table. Interestingly, the impact of illegal drug markets on violence is now much greater, suggesting that the OLS coefficients were attenuated by measurement error. Our estimates suggest that, on average, a 10% increase in the value of cocaine production in a municipality raises the homicide rate by about 1.3%. The estimates are very stable across specifications. We can interpret these results as causal as long as altitude satisfies the exclusion restriction - that is, as long as altitude does not have a direct effect on violence, and is uncorrelated with omitted municipality characteristics that determine it (besides cocaine production activities). This assumption seems plausible, since we know that altitude does not capture the effect of geographic conditions for which we are already controlling (soil quality and erosion, which affects other agricultural products more severely). Similarly, altitude does not capture persistent differences across municipalities, because we control for many proxies of economic development.[9] Finally, to the best of our knowledge, there is no theoretical channel through which altitude might affect violence directly.

[INSERT TABLE 4 HERE]

Nevertheless, in order to provide further evidence regarding the exogeneity of altitude, we conduct a placebo test. More precisely, we estimate a reduced form specification of model 1, in which we replace the value of cocaine markets for altitude for the years 1990-1993.[10]

[9] For instance, the indigenous population prior to colonization was highly concentrated in high places, and this may have affected institutions and subsequent development (see Garcia (2005)). However, we are already controlling for a large number of proxies for institutions and development.

[10] Unfortunately, there is no disagregated data on coca cultivation and cocaine production prior to 1994 in Colombia.

During this period, coca cultivation took place mostly in Peru and Bolivia, and Colombia mostly focused on cocaine processing[11] and trafficking (not coca cultivation). Unlike coca cultivation, cocaine processing and trafficking do not require special soil and climatic conditions related to altitude; hence, conditional on observables, one should not observe an effect of altitude on the homicide rate at the municipality level. Table 5 shows our results for this placebo test, with the reduced form model estimated using OLS. According to our estimates, altitude had no effect on violence prior to 1994, indicating that altitude is an exogenous variable that only affects violence through coca cultivation.

[INSERT TABLE 5 HERE]

Although the placebo test just described is not enough to guarantee that altitude is exogenous, it does provides two important pieces of information. First, altitude does not capture any persistent differences across municipalities other than coca cultivation. If it did, one would observe altitude having an effect on violence before 1994. Second, the only source of bias would be a time varying variable that significantly changed after 1994, and affected both violence and coca cultivation disproportionately in low altitude municipalities. Moreover, this variable must be irrelevant prior to 1994, or altitude would pick up its effect on violence. One potential candidate is the expansion of paramilitary groups, which took place in 1998, and was highly concentrated in low altitude municipalities featuring extensive cattle production. In order to address this concern (and similar concerns about the expansion of guerrilla groups during this period), we control directly for the presence of illegal armed groups by including dummies for the three main groups during the period of our analysis - FARC, AUC and ELN.

Table 6 presents estimates of model 1 controlling for the presence of illegal armed groups. Our results show that locations with armed groups are more violent, with all groups' dummies being positive and highly significant. More importantly for our purposes, the coefficient for the value of cocaine markets remains positive and significant, although smaller and less precisely estimated. This last fact is not surprising, since part of the effect generated by coca cultivation activities occurs through the presence of armed groups associated to this illegal activity. Thus, their inclusion leads us to underestimate the true effect and lose precision, since part of the effect of the value of cocaine markets on violence is via the presence of illegal armed groups. We obtain similar results (not presented here) if we use the lagged presence of armed groups, with our coefficient of interest becoming bigger, as the discussion above would suggest.

[11] Coca paste and coca base were brought from Peru and Bolivia (mostly via small aircrafts) to be processed into cocaine in laboratories located in Colombia's south.

[INSERT TABLE 6 HERE]

Given the previous results, we are confident that our estimate captures the causal impact of illegal drug markets on violence. We thus proceed to study potential channels through which this relationship takes place. We have in mind a framework wherein prohibition creates rents associated with the control of land outside the State's radar. More precisely, illegal armed groups actively involved in coca cultivation and cocaine production activities engage in a conflict against one another, the population and the state over the control of arable land suitable for the cultivation of illegal crops. Moreover, because low altitude municipalities have more land suitable for production - and hence more rents associated with illegal drug markets - they should become more violent in the presence of external demand shocks for Colombian cocaine. In order to show that this is indeed the case, we estimate model 1 using the rate of forced displacement, an outcome directly associated with the conflict over the control of land rents, as our dependent variable. Table 7 shows a strong and larger impact of the value of cocaine markets on displacement, indicating that part of the mechanism involves the conflict over the control of arable land necessary to cultivate illegal crops and operate the illegal drug trafficking business. Our estimates imply that, on average, a 10% increase in the value of the cocaine produced in a municipality raises forced displacement by about 5%.

[INSERT TABLE 7 HERE]

Another possibility is that it is not illegal markets *per se,* but enforcement against these activities, which causes violence. In order to test this alternative explanation, we include different measures of drug related enforcement as direct controls. For instance, Table 8 shows the effect of including eradicated areas at the municipality level, while Table 9 shows the effect of including drug-related captures at the state level. Before discussing the results, we should clarify that these controls are not included in order to test whether cocaine production causes violence (the exogeneity of altitude guarantees that this is the case), but rather to test whether violence occurs because coca cultivation causes enforcement, which in turn leads to more violence. Our results suggest that cocaine markets have an effect on violence that is independent of the level of enforcement against these markets. In both cases, we find a negative effect of enforcement, but this coefficient is not consistently estimated and is not significant. The partial conclusion from this exercise is that enforcement does not appear to have a direct effect on violence; it only has an effect via the value of cocaine produced (e.g., a market size effect). Thus, enforcement in one municipality increases violence in other producer municipalities because it raises the price of drugs, and hence the value of coca cultivation. Moreover, enforcement policies implemented at the national level, such as those implemented under Plan Colombia, increase the value of cocaine markets, as the demand for illegal drugs at the wholesale level is price inelastic (see Mejia and Restrepo, 2011).[12] We obtain similar results (not reported here) if we use alternative and broader measures of enforcement, such as the presence of police stations, the distance to battalions and expenditures on security. Independent of the level (or the proxy) of enforcement against illegal drug markets, in our estimations, we always find a direct causal effect for the value of cocaine markets on violence.

[INSERT TABLE 8 AND 9 HERE]

In order to asses whether illegal drug markets also lead to other types of crime (in particular, property crime), we use data on robberies, available at the municipality level. Table 10 shows that illegal drug markets have no effect on robberies. This suggests that our estimates are not capturing negative socioeconomic shocks induced by crime in low altitude municipalities during the period of our analysis. Moreover, this confirms that illegal drug markets cause violence through conflict, and not necessarily via regular property crime. This is not innocuous, since some observers have argued that coca cultivation and cocaine production create a 'culture of illegality' that triggers an increase in the level of violence. Another potential channel is the consumption of cocaine, which may generate crime via a negative income shock on consumers' families. However, this channel cannot explain our results, since we would most probably observe an effect on robberies. Additionally, most of the cocaine produced in Colombia is exported, and the small amounts left to cover internal demand are consumed in big cities where there is no coca cultivation or cocaine production.

Although not reported here, we also find a positive and significant effect of illegal drug markets on violent attacks perpetrated by armed groups actively involved in cocaine production and trafficking. These results (available from the authors upon request) provide further evidence that the causal effect of illegal drug markets on violence that we capture is generated by illegal armed groups who, acting as rent seekers in illegal drug markets, resort to violence to settle disputes over the control of arable land and strategic corridors.

[INSERT TABLE 10 HERE]

We next estimate model 2. We start by estimating the first stage, for which we use different combinations of the available instruments. Table 11 shows our results. All columns

[12] More precisely, if the demand for illegal drugs is price inelastic, enforcement increases the size of illegal drug markets, and hence the level of violence; the opposite happens if the demand for illegal drugs is price elastic.

include municipality fixed effects, time effects, demographic controls and our proxy for the level of economic activity (industry and commerce tax revenues). We also use data for 1994 in order to obtain greater time variation, which we exploit with time varying instruments. Importantly for our results, the F statistics is well above the rule of thumb value of 10.

[INSERT TABLE 11 HERE]

In the first column, we use the interaction of altitude with a dummy that captures the shift in production after 1994. The associated coefficient is negative and significant, suggesting that after 1994, the value of cocaine production disproportionately increased to a greater extent in municipalities located at low altitudes. The second column uses the interaction of altitude and seizure rates in Peru and Bolivia. Again, the coefficients are negative, suggesting that a higher interdiction rate in these two cocaine producing countries led to an exogenous demand shock for Colombian cocaine; also that this effect was stronger in Colombian municipalities located at low altitudes. Column 3 uses the combined seizure rate for Peru and Bolivia and provides similar results. Column 4 uses the interaction of altitude with seizure rates in transit countries. The negative coefficient implies that enforcement in upstream markets also causes positive demand shocks for Colombian cocaine, as traffickers and dealers demand more drugs in order to compensate for the extra losses incurred as a result of larger seizures in transit countries (this effect is backed up by a theoretical framework as long as demand is inelastic). Again, these demand shocks disproportionately affect low altitude municipalities, which are more suitable for cocaine production. Columns 6 and 7 use interactions with enforcement in consumer countries; the interpretation is similar to that for column 5. Columns 5 and 8 use combinations of the instruments. In all cases, the instruments capture positive demand shocks for Colombian cocaine that are especially strong in low altitude municipalities, except in the last column, where the interaction of altitude and seizures in transit countries is not significant.

Table 12 shows our 2SLS estimation of the effect of the value of cocaine markets on homicides. These estimates are all positive and significant. Notoriously, they are also larger than those obtained using the first identification strategy described above. Presumably, this is because under our second identification strategy, we use a significant degree of variation over time in order to estimate the effect. In particular, the estimates in Table 12 imply that, on average, a 10% increase in the value of cocaine production in a given municipality increases the homicide rate by about 2.2%. Similarly, Table 13 shows our estimates for forced displacement. Yet again, these estimates are considerably larger, as we capture the huge wave of forced displacement that took place during the period 1994-2000. Additionally, our time varying instrument allows us to use this time variation to identify the causal effect of illegal markets on forced displacement. These estimates suggest that a 10% increase in the value of cocaine production in a municipality increases the forced displacement rate by about 11.3%.

[INSERT TABLE 12 AND 13 HERE]

In this case, it is again important to control for the expansion of armed groups, especially paramilitary ones, which tended to locate in low altitude municipalities featuring extended cattle activities. In particular, if the location of these groups is not entirely driven by the possibility of illegal drug production, but by other variables correlated with altitude (for instance cattle production), this effect would be captured by our coefficient of interest, thus biasing it. Table 14 shows our results for model 2, where we control for armed group presence. Armed group expansion is correlated with more violence, although these variables are obviously endogenous. Again, our coefficient of interest becomes smaller, but is still highly significant. This may be because we are adding "bad controls", in the sense that they are also affected by our variable of interest. In other words, part of the effect that we were estimating before occurs through the expansion of illegal armed groups. In any case, we are confident that the real effect of illegal drug markets on violence lies somewhere between the estimates with and without controls. Also, our estimates are less precisely estimated in this case because of the high correlation between treatment and control variables.

[INSERT TABLE 14 HERE]

Finally, it is well known that during our period of analysis, Colombia underwent a process of increased enforcement, especially in the form of increased eradication campaigns (e.g., the aerial spraying of illicit crops and their manual eradication). If these efforts had an independent effect on violence, then our coefficient may be capturing the effect of enforcement. However, as long as eradication campaigns are concentrated in low altitude municipalities due to the fact that they have more coca crops, and not because of other exogenous reasons, we can feel secure that we are not overestimating our coefficient of interest. In this case, increased enforcement would simply be one of the channels through which coca cultivation and cocaine production affect violence. In order to partially isolate this channel, we include the eradicated area as a control and show that our estimate is not entirely driven by higher levels of enforcement. Table 15 presents our results. Our coefficient of interest does not change by much, but it is now less precisely estimated. It is only in the first column that we find no significant effect of illegal drug markets on violence, but this is due to the loss of precision caused by including controls that are also caused by our variable of interest (eradication itself is also not significant).

[INSERT TABLE 15 HERE]

5 Conclusions

This paper proposes a novel identification strategy in order to disentangle the relationship between the size of illicit drug markets and violence. Our identification strategy heavily relies on two observations. First, due to technical conditions related to soil quality, temperature and humidity, coca cultivation and cocaine production activities are more efficient in municipalities located at low altitudes. Second, and related to the first observation, external demand shocks for Colombian cocaine have a larger effect on the value of cocaine markets in municipalities located at low altitudes.

Importantly, we are able to partially test the exogeneity of one of our instruments using data for the period 1990-1993, when coca cultivation in Colombia was a rather isolated phenomenon. In particular, we show in a reduced form estimation that altitude only began having an effect on violence after 1994, when the Peruvian-government-driven closure of the air bridge connecting coca cultivation areas in Peru and Bolivia and cocaine processing facilities in Colombia led to a significant increase in coca cultivation in Colombia.

Regardless of the choice made between the two identification strategies proposed in this paper, our results indicate that larger illicit drug markets leads to higher levels of violence. On average, we estimate that a 10% increase in the value of cocaine markets leads to an increase in the homicide rate of between 1.2% and 2%. We are able to partially identify the channel through which illegal drug markets breed violence in producer countries such as Colombia: the intensification of the conflict over the control of arable land for cultivating coca crops and strategic corridors used in cocaine trafficking activities.

In order to get a sense of the economic significance of our estimates, we introduce the following back-of-the-envelope calculation. The value of Colombian cocaine cultivation has increased from 1990 to 2008 by about 230% (from $138 million dollars at the beginning of the 90s to about $1.3 billion during the late 2000s). These estimates suggest that homicide rates are between 30% and 50% higher today, and forced displacement rates 92-260% higher today, than what they would be if illegal drug production and trafficking activities hadn't risen to the extent they did during the late 1990s. Thus, the increase in coca cultivation and cocaine production accounts for between 1/4 and 2/5 of the homicide rates in Colombia today, and for between 2/3 and 9/10 of the forced displacement rates. According to our estimates, illegal drug production activities currently cost Colombia between 4,600 and 7,000 lives each year (about 17,700 individuals were killed in 2010) and between 180,000 and 277,000 displaced persons each year (about 307,000 persons were displaced in 2008).

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Table 1: Descriptive statistics

	Mean	Std. Dev.	Observations
	Measures of violence and conflict		
Homicide rate	49.03	65.15	12298
Forced displacement rate	1023.33	2872.58	12147
Attack rate	10.59	22.80	10580
Robbery rate	0.51	2.68	5471
	Cocaine measures		
Coca crops in hectares	92.73	614.54	12237
Cocaine production	527.05	3231.32	12237
Coca production value	944.54	5674.86	12237
	Armed group presence		
FARC presence	0.38	0.48	12276
AUC presence	0.16	0.37	12276
ELN presence	0.17	0.38	12276
	Enforcement measures		
Eradicated hectares	121.13	918.05	12309
Drug-related captures	1305.42	1792.39	12279
Distance to battalion	2.27	0.71	12012
Police station presence	0.85	0.36	12012
Expenditures in security	260333	738795	12034
	Geography and controls		
Altitude	1163.08	917.31	11803
Population	38593	229471	12239
Rurality	0.55	0.25	11690
Tax revenues	10642.18	33052.95	11968

Notes: The table shows descriptive statistics for the main variables and covariates used in our empirical exercise.

Table 2: OLS estimates of the impact of the value of coca crops on homicides

	(1)	(2)	(3)	(4)	(5)	(6)
Coca value	0.039***	0.041***	0.043***	0.043***	0.041***	0.042***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)	(0.006)
R-squared	0.281	0.284	0.288	0.289	0.307	0.297
Observations	10724	9230	9221	9215	9195	8990
Geography	N	Y	Y	Y	Y	Y
Demography	N	N	Y	Y	Y	Y
Economy	N	N	N	Y	Y	Y
Development	N	N	N	N	Y	N
Pretreatment controls	N	N	N	N	N	Y

NOTE: The table reports the coefficient of the value of coca crops from an OLS regression of the log of the homicide rate on the log of the value of coca crops in a municipality, controlling for the corresponding municipality-level covariates. All regressions include year and state-fixed effects. Robust standard errors with clustering by municipality are shown in parentheses. For the reported coefficients, those with *** are significant at the 1% level; those with ** are significant at the 5% level; and those with * are significant at the 10% level.

Table 3: First stage estimates, first strategy

	(1)	(2)	(3)	(4)	(5)	(6)
Altitude	-0.002***	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
R-squared	0.343	0.440	0.450	0.451	0.464	0.436
Observations	10724	9230	9221	9215	9195	8990
Geography	N	Y	Y	Y	Y	Y
Demography	N	N	Y	Y	Y	Y
Economy	N	N	N	Y	Y	Y
Development	N	N	N	N	Y	N
Pretreatment controls	N	N	N	N	N	Y

NOTE: The table reports the first stage coefficient of altitude on the the value of coca crops in a municipality, controlling for the corresponding municipality-level covariates. All regressions include year and state-fixed effects. Robust standard errors with clustering by municipality are shown in parentheses. For the reported coefficients, those with *** are significant at the 1% level; those with ** are significant at the 5% level; and those with * are significant at the 10% level.

Table 4: IV estimates of the impact of the value of coca crops on homicides

	(1)	(2)	(3)	(4)	(5)	(6)
Coca value	0.114***	0.145***	0.138***	0.136***	0.122***	0.130***
	(0.028)	(0.044)	(0.044)	(0.044)	(0.046)	(0.047)
R-squared	0.244	0.225	0.241	0.243	0.273	0.256
Observations	10724	9230	9221	9215	9195	8990
F (first stage)	82.77	46.75	47.69	48.12	42.77	36.95
Geography	N	Y	Y	Y	Y	Y
Demography	N	N	Y	Y	Y	Y
Economy	N	N	N	Y	Y	Y
Development	N	N	N	N	Y	N
Pretreatment controls	N	N	N	N	N	Y

NOTE: The table reports the coefficient of the value of coca crops from an IV regression of the log of the homicide rate on the log of the value of coca crops in a municipality, controlling for the corresponding municipality-level covariates. All regressions include year and state-fixed effects. Robust standard errors with clustering by municipality are shown in parentheses. F statistics are adjusted by clustering as well. The last column includes lagged controls prior to 1994. For the reported coefficients, those with *** are significant at the 1% level; those with ** are significant at the 5% level; and those with * are significant at the 10% level.

Table 5: OLS estimates of the impact of altitude on homicides prior to 1994

	(1)	(2)	(3)	(4)	(5)	(6)
Altitude	-0.000	-0.000	-0.000	-0.000	0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
R-squared	0.305	0.285	0.287	0.325	0.347	0.287
Observations	4292	3692	3595	898	898	3596
Geography	N	Y	Y	Y	Y	Y
Demography	N	N	Y	Y	Y	Y
Economy	N	N	N	Y	Y	Y
Development	N	N	N	N	Y	N
Pretreatment controls	N	N	N	N	N	Y

NOTE: The table reports reduced form coefficient of the effect of altitude on the log of the homicide rate in a municipality prior to 1994, controlling for the corresponding municipality-level covariates. All regressions include year and state-fixed effects. Robust standard errors with clustering by municipality are shown in parentheses. The last column includes lagged controls prior to 1994. For the reported coefficients, those with *** are significant at the 1% level; those with ** are significant at the 5% level; and those with * are significant at the 10% level.

Table 6: IV estimates of the impact of the value of coca crops on homicides

	(1)	(2)	(3)	(4)	(5)	(6)
Coca value	0.089***	0.120***	0.118***	0.117***	0.111**	0.109**
	(0.028)	(0.044)	(0.043)	(0.043)	(0.045)	(0.046)
AUC	0.465***	0.414***	0.421***	0.419***	0.385***	0.410***
	(0.055)	(0.066)	(0.064)	(0.064)	(0.064)	(0.069)
FARC	0.485***	0.468***	0.481***	0.482***	0.482***	0.480***
	(0.056)	(0.057)	(0.056)	(0.056)	(0.055)	(0.057)
ELN	0.350***	0.283***	0.292***	0.293***	0.275***	0.323***
	(0.066)	(0.087)	(0.085)	(0.085)	(0.087)	(0.086)
R-squared	0.301	0.281	0.290	0.292	0.311	0.306
Observations	10694	9230	9221	9215	9195	8990
F (first stage)	80.44	47.59	49.38	49.88	45.59	37.97
Geography	N	Y	Y	Y	Y	Y
Demography	N	N	Y	Y	Y	Y
Economy	N	N	N	Y	Y	Y
Development	N	N	N	N	Y	N
Pretreatment controls	N	N	N	N	N	Y

NOTE: The table reports the coefficient of the value of coca crops from an IV regression of the log of the homicide rate on the log of the value of coca crops in a municipality, controlling for the corresponding municipality-level covariates. All regressions include year and state-fixed effects. Robust standard errors with clustering by municipality are shown in parentheses. F statistics are adjusted by clustering as well. The last column includes lagged controls prior to 1994. For the reported coefficients, those with *** are significant at the 1% level; those with ** are significant at the 5% level; and those with * are significant at the 10% level.

Table 7: IV estimates of the impact of the value of coca crops on forced displacement

	(1)	(2)	(3)	(4)	(5)	(6)
Coca value	0.405***	0.483***	0.507***	0.506***	0.477***	0.553***
	(0.049)	(0.081)	(0.084)	(0.084)	(0.088)	(0.101)
R-squared	0.394	0.305	0.279	0.282	0.339	0.205
Observations	10703	9230	9221	9215	9195	8990
F (first stage)	82.79	46.75	47.69	48.12	42.77	36.95
Geography	N	Y	Y	Y	Y	Y
Demography	N	N	Y	Y	Y	Y
Economy	N	N	N	Y	Y	Y
Development	N	N	N	N	Y	N
Pretreatment controls	N	N	N	N	N	Y

NOTE: The table reports the coefficient of the value of coca crops from an IV regression of the log of the forced displacement rate on the log of the value of coca crops in a municipality, controlling for the corresponding municipality-level covariates. All regressions include year and state-fixed effects. Robust standard errors with clustering by municipality are shown in parentheses. F statistics are adjusted by clustering as well. The last column includes lagged controls prior to 1994. For the reported coefficients, those with *** are significant at the 1% level; those with ** are significant at the 5% level; and those with * are significant at the 10% level.

Table 8: IV estimates of the impact of the value of coca crops on homicides

	(1)	(2)	(3)	(4)	(5)	(6)
Coca value	0.150***	0.183**	0.169**	0.167**	0.140**	0.155**
	(0.054)	(0.074)	(0.073)	(0.073)	(0.071)	(0.077)
Eradication	-0.125	-0.136	-0.107	-0.104	-0.071	-0.094
	(0.089)	(0.103)	(0.102)	(0.100)	(0.096)	(0.110)
R-squared	0.212	0.187	0.212	0.215	0.260	0.235
Observations	10724	9230	9221	9215	9195	8990
F (first stage)	54.00	35.00	34.70	35.09	34.70	29.37
Geography	N	Y	Y	Y	Y	Y
Demography	N	N	Y	Y	Y	Y
Economy	N	N	N	Y	Y	Y
Development	N	N	N	N	Y	N
Pretreatment controls	N	N	N	N	N	Y

NOTE: The table reports the coefficient of the value of coca crops from an IV regression of the log of the homicide rate on the log of the value of coca crops in a municipality, controlling for the corresponding municipality-level covariates. All regressions include year and state-fixed effects. Robust standard errors with clustering by municipality are shown in parentheses. *F* statistics are adjusted by clustering as well. The last column includes lagged controls prior to 1994. For the reported coefficients, those with *** are significant at the 1% level; those with ** are significant at the 5% level; and those with * are significant at the 10% level.

Table 9: IV estimates of the impact of the value of coca crops on homicides

	(1)	(2)	(3)	(4)	(5)	(6)
Coca value	0.114***	0.145***	0.138***	0.136***	0.122***	0.130***
	(0.028)	(0.044)	(0.044)	(0.044)	(0.047)	(0.047)
Drug captures	-0.002	-0.024	-0.020	-0.018	-0.020	-0.027
	(0.026)	(0.028)	(0.028)	(0.028)	(0.028)	(0.028)
R-squared	0.241	0.226	0.241	0.244	0.274	0.257
Observations	10694	9220	9211	9205	9185	8980
F (first stage)	82.88	46.69	47.56	47.99	42.53	36.91
Geography	N	Y	Y	Y	Y	Y
Demography	N	N	Y	Y	Y	Y
Economy	N	N	N	Y	Y	Y
Development	N	N	N	N	Y	N
Pretreatment controls	N	N	N	N	N	Y

NOTE: The table reports the coefficient of the value of coca crops from an IV regression of the log of the homicide rate on the log of the value of coca crops in a municipality, controlling for the corresponding municipality-level covariates. All regressions include year and state-fixed effects. Robust standard errors with clustering by municipality are shown in parentheses. F statistics are adjusted by clustering as well. The last column includes lagged controls prior to 1994. For the reported coefficients, those with *** are significant at the 1% level; those with ** are significant at the 5% level; and those with * are significant at the 10% level.

Table 10: IV estimates of the impact of the value of coca crops on robberies

	(1)	(2)	(3)	(4)	(5)	(6)
Coca value	-0.006	-0.020	-0.023	-0.023	-0.029*	-0.028
	(0.009)	(0.015)	(0.016)	(0.015)	(0.017)	(0.018)
R-squared	0.036	0.025	0.022	0.022	0.018	0.014
Observations	4286	3692	3688	3688	3680	3596
F (first stage)	69.25	31.53	30.37	30.15	27.46	23.28
Geography	N	Y	Y	Y	Y	Y
Demography	N	N	Y	Y	Y	Y
Economy	N	N	N	Y	Y	Y
Development	N	N	N	N	Y	N
Pretreatment controls	N	N	N	N	N	Y

NOTE: The table reports the coefficient of the value of coca crops from an IV regression of the log of the robbery rate on the log of the value of coca crops in a municipality, controlling for the corresponding municipality-level covariates. All regressions include year and state-fixed effects. Robust standard errors with clustering by municipality are shown in parentheses. F statistics are adjusted by clustering as well. The last column includes lagged controls prior to 1994. For the reported coefficients, those with *** are significant at the 1% level; those with ** are significant at the 5% level; and those with * are significant at the 10% level.

Table 11: First stage estimates, second strategy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Shift of production in 1994	-0.001***							
	(0.000)							
Seizure rate in Peru		-0.000**						
		(0.000)						
Seizure rate in Bolivia		-0.000***						
		(0.000)						
Seizure rate in Peru and Bolivia			-0.001***		-0.001***			-0.001***
			(0.000)		(0.000)			(0.000)
Seizure rate in transit countries				-0.000***	-0.000*			0.000
				(0.000)	(0.000)			(0.000)
Seizure rate in consumer countries						-0.001***		-0.001***
						(0.000)		(0.000)
Seizure rate in the U.S. (DEA)							-0.001***	
							(0.000)	
Drug related arrests in the U.S. (DEA)							-0.001***	
							(0.000)	
R-squared	0.784	0.784	0.784	0.783	0.784	0.783	0.784	0.784
Observations	11526	11526	11526	11526	11526	11526	11526	11526

NOTE: The table reports the first stage coefficient of the interaction of altitude and different demand shocks on the the value of coca crops in a municipality. All regressions include year and municipality-fixed effects, as well as time varying covariates at the municipality level. Standard errors are shown in parentheses. For the reported coefficients, those with *** are significant at the 1% level; those with ** are significant at the 5% level; and those with * are significant at the 10% level.

Table 12: IV estimates of the impact of the value of coca crops on homicides

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coca value	0.132**	0.208***	0.193***	0.402***	0.235***	0.407***	0.220***	0.222***
	(0.056)	(0.058)	(0.061)	(0.092)	(0.061)	(0.107)	(0.066)	(0.058)
R-squared	0.476	0.433	0.443	0.222	0.413	0.214	0.425	0.423
Observations	11526	11526	11526	11526	11526	11526	11526	11526
F (first stage)	83.94	42.71	76.98	46.55	40.27	35.13	34.04	29.38

NOTE: The table reports the coefficient of the value of coca crops from an IV regression of the log of the homicide rate on the log of the value of coca crops in a municipality. All regressions include year and municipality-fixed effects, as well as time varying covariates at the municipality level. Standard errors are shown in parentheses. F statistics from the first stage are reported below each model. For the reported coefficients, those with *** are significant at the 1% level; those with ** are significant at the 5% level; and those with * are significant at the 10% level.

Table 13: IV estimates of the impact of the value of coca crops on forced displacement

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coca value	1.294***	1.047***	1.183***	0.892***	1.125***	0.872***	1.635***	1.138***
	(0.149)	(0.124)	(0.144)	(0.149)	(0.135)	(0.169)	(0.204)	(0.131)
R-squared	.	0.239	0.074	0.403	0.148	0.422	.	0.131
Observations	11526	11526	11526	11526	11526	11526	11526	11526
F (first stage)	83.94	42.71	76.98	46.55	40.27	35.13	34.04	29.38

NOTE: The table reports the coefficient of the value of coca crops from an IV regression of the log of the forced displacement rate on the log of the value of coca crops in a municipality. All regressions include year and municipality-fixed effects, as well as time varying covariates at the municipality level. Standard errors are shown in parentheses. F statistics from the first stage are reported below each model. For the reported coefficients, those with *** are significant at the 1% level; those with ** are significant at the 5% level; and those with * are significant at the 10% level.

Table 14: IV estimates of the impact of the value of coca crops on homicides

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coca value	0.102*	0.186***	0.180***	0.350***	0.215***	0.333***	0.167**	0.197***
	(0.056)	(0.057)	(0.060)	(0.087)	(0.060)	(0.100)	(0.065)	(0.057)
FARC	0.454***	0.448***	0.448***	0.435***	0.445***	0.436***	0.449***	0.447***
	(0.033)	(0.035)	(0.035)	(0.040)	(0.035)	(0.039)	(0.034)	(0.035)
AUC	0.330***	0.305***	0.307***	0.257***	0.297***	0.262***	0.311***	0.302***
	(0.041)	(0.043)	(0.043)	(0.052)	(0.044)	(0.053)	(0.043)	(0.043)
ELN	0.322***	0.306***	0.307***	0.275***	0.300***	0.278***	0.309***	0.304***
	(0.043)	(0.045)	(0.045)	(0.052)	(0.046)	(0.052)	(0.045)	(0.045)
R-squared	0.503	0.464	0.468	0.307	0.443	0.328	0.475	0.457
Observations	11526	11526	11526	11526	11526	11526	11526	11526
F (first stage)	80.05	42.21	76.21	46.93	39.99	34.46	32.02	28.88

NOTE: The table reports the coefficient of the value of coca crops from an IV regression of the log of the homicide rate on the log of the value of coca crops in a municipality. All regressions include year and municipality-fixed effects, as well as time varying covariates at the municipality level. Standard errors are shown in parentheses. F statistics from the first stage are reported below each model. For the reported coefficients, those with *** are significant at the 1% level; those with ** are significant at the 5% level; and those with * are significant at the 10% level.

Table 15: IV estimates of the impact of the value of coca crops on homicides

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Coca value	0.114	0.214***	0.193**	0.544***	0.205**	0.585**	0.215***	0.193**
	(0.073)	(0.080)	(0.083)	(0.175)	(0.083)	(0.229)	(0.081)	(0.080)
Eradication	0.043	-0.011	-0.000	-0.189**	-0.006	-0.212*	-0.012	0.000
	(0.041)	(0.045)	(0.046)	(0.096)	(0.046)	(0.125)	(0.045)	(0.045)
R-squared	0.483	0.429	0.443	.	0.436	.	0.429	0.444
Observations	11526	11526	11526	11526	11526	11526	11526	11526
F (first stage)	52.41	23.92	43.85	17.73	21.96	11.23	23.52	15.64

NOTE: The table reports the coefficient of the value of coca crops from an IV regression of the log of the homicide rate on the log of the value of coca crops in a municipality. All regressions include year and municipality-fixed effects, as well as time varying covariates at the municipality level. Standard errors are shown in parentheses. F statistics from the first stage are reported below each model. For the reported coefficients, those with *** are significant at the 1% level; those with ** are significant at the 5% level; and those with * are significant at the 10% level.