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Appendix A: Sources and coding for variables

Cocaine Production:
Our first independent variable is a dummy variable that captures the extent to which a district has significant coca production intended for the illegal cocaine trade – in other words, we code as '0' both districts without significant coca production, and districts where coca was produced for use in its unprocessed form within Peru's borders. Districts coded as '1' are those in which the coca produced was refined into cocaine and exported into the global illegal narcotics trade. Because sources like satellite data are not available for the time period under investigation in this paper,[i] coding this variable involves a degree of imprecision. This can be seen in Alvarez (1992), which walks through the assumptions made in various methods for estimating coca production in the UHV and the country as a whole, showing how widely estimates vary. Additionally, even when more detailed measures of coca acreage are available (as in Colombia since the late 1990s), prominent studies of the relation between coca and conflict still use dichotomous classifications of coca presence, for example Angrist and Kuegler (2008).

To address this uncertainty in measurement, we use three methods for our coding. First, because of the broad consensus that the Upper Huallaga valley was the central location of the cocaine trade in Peru (INCB 1982, Felbab-Brown 2005, Weinstein 2007), we code as '1' all districts within that valley for all the years in our study, generating a variable called **uhv**.[ii]

We also use reports of the UNODC, Devida, and the International Narcotics Control Board to identify other provinces that had coca production linked to trafficking (there is no information at the district level). Here we create two alternate codings. One (our **coca** variable) includes districts in those provinces only after 1992, when a fungus affected coca production in the Upper Huallaga and started to displace crops towards regions such as the Lower and Central Huallaga and the Apurimac River Valley[iii] (now known as the VRAEM region) (INCB 1993, Lee and Clawson1993). Since some reports mention coca production in those provinces for the 1980s as well (INCB 1993), our third coding (**coca2**) overestimates coca production by coding districts in those provinces as 1 before 1992, although such production was probably not intended for cocaine processing. 75 of our 963 total districts have significant illegal coca production for at least some of the years in the dataset, and thus are coded one on this measure.

One might be concerned that these measures of cocaine production might have a post-treatment component, since whether a district produces coca might be a function of whether the conflict has affected local political, economic, and social conditions. Detailed history of the spread of the armed conflict casts some doubt on this line of argument, since major production of coca for the drug trade began in the Huallaga in the mid-1970s, well before insurgents arrived in the early 1980s. Though some sources claim that "Sendero began its political work in the UHV in 1980" (Gonzales 1992, 124), significant Sendero presence only began to emerge several years later. Even as early as 1980, it is clear that insurgents discovered a "large-scale drug economy flourishing" and "a peasantry that was pre-mobilized and in many cases organized in opposition to the government." (Felbab-Brown 2010, 41) Yet it is possible, as discussed elsewhere in the paper, that Sendero's presence affected the flourishing of the drug trade in regions where it held sway.

Control variables:
Our analyses include a set of control variables. Here we provide detail on those variables and, where relevant, on how they are measured or the sources used.

Altitude of district capital: Following scholars like Fearon & Laitin (2003), we expect that the terrain of a district might affect the ease of access for state forces, the information available to them, and the ability of rebels to establish and exercise control. We therefore include the log of the *altitude* of the district capital, assuming that districts at higher altitude are more favorable sites for insurgents.

Seat of government (Capital): Because state forces and institutions might be concentrated in regional centers of power, we include a dummy variable indicating the districts in which departmental or provincial capitals are located as a proxy for the political centrality of a district to the state administrative grid.

Socio-economic indicators: We might also expect the behavior of state actors to be shaped by socio-economic conditions. Here, we face significant data limitations: to our knowledge there are no available time series of socioeconomic indicators at the district level for Peru in the 1980s and 1990s.[iv] As our proxy, we use district-level *electrification*[v] rates, drawn from Peru's 1981 and 1993 censuses. We use the levels reported in the 1981 census for the years 1980-1992, and 1993 levels for the remaining years.

District population: We use data from two sets of district level population projections from Peru's national statistical agency (INEI 2002) for all the years between 1980 and 2000. We include a logged value of population in the analyses.

Territorial Control: Scholars see territorial control as central in civil wars, and Kalyvas (2006) showed that it predicts patterns of violence by armed actors. We therefore include it as a control in all analyses. We draw on the work of de la Calle (2017, 432-3), who uses the ability of Sendero to enforce boycotts of elections between 1980 and 1995 as an indicator of rebel control in Peru's armed conflict. Using data from Peru's electoral commission, he codes districts with annulled elections as under rebel *control*, and those where spoilage rates were higher than 50% of all votes cast as districts with *contested* territory. These are mutually exclusive categories; the third (omitted) category are districts without election irregularities, which are considered under state control. We lag this coding of control by one year so that we can use it as a predictor of violence.

Presidencies: Because they saw rapid swings in ideology, rhetoric, and policy about drug trade and counter-insurgency, we include dummy variables for each presidency.[vi] While these variables for presidencies are intended to capture broad policy differences, we acknowledge that other features of presidencies might also be relevant. We therefore caution the reader against substantive interpretations of the coefficient on this variable.

Previous Sendero and MRTA Violence: We might see greater state violence in coca regions because the state is responding to greater insurgent violence in coca regions. If the drug trade increases rebel capacity via both increased revenue and increased civilian support in response to state eradication efforts, this leads to increased state violence in the course of counter-insurgency (Peceny and Durán, 2006). To account for this effect we include in all models one-year lags of the count of victims of killings committed by Sendero and the MRTA in each district-year.

Previous state violence: Some locations may see more state violence because they have a prior history of state abuse. This is particularly important considering the history of tensions between populations and police forces throughout Peru. (Heilman 2018) We therefore include in all models a one-year lag of the count of victims of killings perpetrated by state forces in each district-year.

Though the cross-national literature (e.g. Stanton 2016) has found regime type to be an important predictor of repression, we do not include controls for subnational regime type, for two reasons. First, levels of democratic competition can be endogenous to conflict dynamics, as seen in Sendero's influence on the conduct and competitiveness of local elections. Second, because military commanders controlled counter-insurgency throughout the period with little oversight from civilian political institutions, local political conditions in Peru had a limited effect on state coercive behavior.

References

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Table A1 – descriptive statistics

Variable	# obs	Mean	Std. Dev.	Min	Max	Source
Total State Acts	20061	0.426	4.541	0	212	CVR (2003)
Dummy State Acts	20063	0.042	0.201	0	1	
State Killings	20061	0.159	2.162	0	159	
State Disappearances	20061	0.101	1.015	0	49	
State Massacre (victims)	20061	0.031	0.872	0	90	
State Massacre (events)	20063	0.004	0.083	0	5	
State Recruitment (victims)	20061	0.014	0.522	0	38	
State Recruitment (events)	20063	0.005	0.17	0	13	
State Rape (victims)	20061	0.090	2.847	0	212	
State Rape (events)	20063	0.017	0.41	0	26	
State Kidnapping (victims)	20061	0.052	1.426	0	136	
State Kidnapping (events)	20063	0.012	0.26	0	15	
Army Acts of Violence	20061	0.351	4.179	0	212	
Police Acts of Violence	20061	0.105	1.793	0	121	
State extrajudicial killings (victims)	20061	0.082	1.614	0	161	
State torture (victims)	20061	0.093	1.203	0	58	
State torture (events)	20063	0.036	0.29	0	11	
Lag State Killings	19100	0.167	2.215	0	159	
Lag MRTA Killings	19099	0.008	0.130	0	6	
Lag Sendero Killings	20060	0.338	2.539	0	103	
Sendero Total Killing Leaders	20061	0.041	0.341	0	17	
Sendero Total Killings Members Org.	20061	0.022	0.203	0	7	
uhv	20061	0.020	0.140	0	1	Authors' coding
coca	20061	0.040	0.196	0	1	Authors' coding
coca2	20061	0.077	0.267	0	1	Authors' coding
drug_policy	472	0.201	0.401	0	1	Authors' coding
capital	20061	0.099	0.299	0	1	INEI (2002)
logaltitude	19998	7.520	1.232	1.386	8.450	INEI (2002)
logpop	19796	8.359	1.255	4.575	13.504	INEI (2002)
electrification	18719	20.759	28.114	0	99.54	1981 and 1993 Census
garcia	15235	0.311	0.463	0	1	Authors' coding
fujimori	20045	0.479	0.500	0	1	Authors' coding
lcontested	14181	0.155	0.362	0	1	De La Calle (2017)
lfull	14181	0.132	0.339	0	1	De La Calle (2017)
milshare	20061	0.247	3.735	-105	122	Calculated from CVR
milrate	830	0.697	0.415	0	1	Calculated from CVR
staterate	19796	0.00003	0.0004	0	0.039	Acts of state violence, per capita, calculated from CVR
Syr	20063	8.17	6.08	0	20	Time count of years since last event of state violence, calculated from CVR

Table A2. Baseline model without controls and with socioeconomic controls only

	state	state
coca	1.557***	1.609***
	(3.41)	(3.27)
logaltitude		1.224***
		(3.39)
capital		1.482***
		(3.16)
electricityperc		0.983***
		(-7.35)
logpop		2.125***
		(13.80)
ln_r	0.457***	0.509***
	(-11.50)	(-9.48)
ln_s	0.185***	0.317***
	(-16.48)	(-8.96)
N	20061	18592

Exponentiated coefficients; *t* statistics in parentheses
+ p<0.10, * p<0.05, *** p<0.01
Negative binomial models

Appendix B: Data generation and addressing potential sample bias

We might be concerned about both over-reporting and under-reporting of violent events in our dataset because it is a product of reports made to the CVR, Peru's Truth and Reconciliation Commission. Indeed, the truth commission itself, rather than taking the sum of deaths reported to represent the total figure for the conflict as a whole, engaged in a multiple systems estimation exercise (CVR, Anexo 3) to arrive at a figure much larger than the number of incidents the commission collected. Yet this multiple systems estimation exercise could only be used at the national level: as Landman & Gohdes (2013, 84) write "despite the wealth of data collected, its distribution across time and geographic location made it impossible to estimate those numbers at this level of disaggregation. The data were thus adequate for the estimation of total deaths and disappearances but insufficiently dense for any analysis looking at specific temporal and spatial patterns at the same time."

We therefore acknowledge that our data represent a convenience sample of the full set of human rights violations, and consider possible sources of bias that might be entailed in its use. It is useful to begin by outlining how the data we use was generated to get a sense of where biased reporting might enter. As Theidon (2013, 7) writes, the commission's data collection exercise was "a two year process that involved focus groups, in-depth interviews, 14 public audiences, ethnographic research, the review of archives including those compiled by the US State Department, and the collection of almost 17,000 testimonies from people throughout the country, many given to the commission's mobile teams that worked in rural areas." These testimonies were recorded and transcribed, and turned into accounts 2-3 pages long that were used as the basis for coding the dataset on which we draw. The centerpiece of the CVR's data collection efforts were public audiences, in which communities recounted the violence they suffered during the conflict. León (2012, 999 fn 11) writes that "Public audiences were widely advertised in the locality where the audience was going to be held, as well as in neighboring localities. The main location where the audiences were held were determined based on previous reports of the incidence of violence from human rights organizations, the ombudsman, or the press. Additionally, communities could ask for an audience to be held in their town. There were no complaints at the time that the CVR emphasized politically active or unstable areas."

This description of the process suggests a few sources of bias: we might be concerned about the under-reporting of violence that occurred in remote areas that were hard for the commission's mobile teams to reach, and where residents faced high costs of travel to sites where public audiences were held.[vii] Since the commission prioritized places where violence was already known to have occurred, we might see under-reporting in areas less affected by violence. Additionally, since the collection of testimony took place after the conflict ended, and thus more than 20 years after its first incidents occurred, we might imagine that for a variety of reasons incidents that took place earlier in the conflict might be more subject to under-reporting. Finally, since public audiences were a central element of data collection, we might expect under-reporting of types of violence that were harder to discuss in public, such as sexual violence.

Additional concerns are raised by Theidon's ethnographic immersion in communities that were embroiled in the truth and reconciliation process. Because she was situated in communities before the mobile teams arrived to conduct the public audiences, she was able to observe that (109) "In every community, there were assemblies held to discuss what would be said to the TRC's mobile teams when they arrived to take testimonies. There was an effort to close narrative ranks, prompted by the many secrets people keep about a lengthy, fratricidal conflict, as well as the expectations a commission generates. I attended numerous assemblies in which authorities reminded everyone what they had decided to talk about..." Thus, in the case of a community that had been a Sendero support base, "in the assemblies held in this community prior to the arrival of the TRC, it was decided that people should only talk about those who had died at the hands of the soldiers." One reason was that some violence had been committed by men who were still present in the community (whether Sendero cadre or non-affiliated violent actors), and "we knew we couldn't talk about it like that or everyone would be killing each other again." And local authorities "were also concerned that if people began talking about killings within the community, it would be taken as proof of Sendero's presence and their sympathies during the war." "Thus the local authorities decided that only certain deaths – those that occurred at the hands of the armed forces – would be talked about with the TRC." Moreover, (ibid., 115) while the CVR defined women as non-combatants – bystanders and victims – and made explicit efforts to gather evidence about their wartime experiences, some 40% of Sendero militants were women. The result was that the commission's all-female focus groups designed to talk about victimization could include perpetrators, and therefore silence other women.

Theidon's analysis suggests that there are important reasons to be concerned about reporting bias in the CVR, but that modeling the determinants of this bias is more complicated than controlling for a community's size or distance, or the period in the conflict in which incidents occurred: one might also need to account for community alignment during the conflict, the extent to which active Senderistas were present in the community in the post-conflict period, and the extent of intra-community violence that took place during the conflict. This information is hard to collect systematically.

The preceding discussion suggests two reasons for concern in the use of a selection model for our analysis. First, some of the factors that predict whether an act of violence is included in the dataset (such as community location and year) are also predictors of violence. This violates the exclusion restriction necessary for a Heckman selection model to work. Second, some of the other factors that predict inclusion (such as those identified by Theidon) cannot be measured in any systematic way. Results of any attempt to directly model the effects of selection are therefore not reliable.

We therefore take two other strategies to address these selection issues in this Appendix. First, following existing practice (León 2012; Schubiger n.d.; García-Ponce n.d.) we recode our dependent variables as a dummy for presence/absence of violence because this measure is less subject to over-reporting and under-reporting biases than an ordinal measure of the intensity of violence. We show results from this analysis in Appendix Table B1a-B1c. Second, in Appendix Tables B2a-B2c, we provide the results from three other modeling strategies as additional robustness checks for the core analysis in Table 1: Table B2a shows the results from a zero-inflated model, Table B2b from a tobit model that looks for censoring on the lower bound of the dependent variable, and

Table B2c a rare events model to see if excess zeros affect findings on the dependent variable.[viii]

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Appendix B1 – analysis with dummy variables rather than counts
As León (2012) recommended and others have implemented, we use a dummy variable for the presence of state violence rather than the count variables used in the main analyses. To do so, we generate a new variable *stateactsdummy*, which has a mean of 0.0427 and a standard deviation of 0.2022. Using this variable, we run models repeating the core analyses in Tables 1 and 2 of the paper, as shown in Appendix Tables B1a-b1b, below.

Table B1a: Replication of Table 1, Column 1 with dummy variable for state acts

	Dummy State Acts
coca	1.163***
	(6.56)
logaltitude	0.304***
	(5.87)
capital	0.496***
	(4.40)
electrification	-0.00336
	(-1.58)
logpop	0.733***
	(15.18)
Lag (SL killings)	0.139***
	(9.01)
Lag (state killings)	0.186***
	(6.45)
Lag (MRTA killings)	0.308+
	(1.96)
lfull	1.132***
	(9.12)
lcontested	0.785***
	(6.73)
fujimori	-0.170
	(-1.43)
garcia	0.379***
	(3.47)
_cons	-12.34***
	(-18.54)
N	12505
P>chi2	0.0000
Pseudo R2	0.2111
Log likelihood	-2063.9585

Logit model +<0.1 *<0.05 **<0.01 ***<0.001

If we compare these results to those in Table 1 column 1, we see that our main result holds – the coca variable remains positive, statistically significant, and substantively large. All but one of our control variables retain the same sign and most retain their significance levels.

Table B1b: Replication of Table 2 with dummy variable for state acts

	Dummy state acts
Drug policy	-1.323+
	(-1.84)
Log Altitude	-0.143
	(-0.49)
Capital	0.479
	(0.73)
Electrification	-0.0227
	(-1.31)
Log Pop	1.444***
	(4.32)
Lagged SL killings	-0.0104
	(-0.25)
Lag MRTA killings	-0.0782
	(-0.14)
Lag state killings	0.912***
	(4.00)
lfull	1.128*
	(2.38)
lcontested	0.659
	(1.22)
Fujimori	-0.457
	(-0.65)
Garcia	1.279+
	(1.67)
_cons	-14.00***
	(-4.25)
N	328

Logit model +<0.1 *<0.05 **<0.01 ***<0.001

If we compare the results obtained here to those in Table 2, we see the same general finding: the sign on the drug policy variable (which Hypothesis 2 predicted to be positive) is negative. In this model, that variable is significant at the 0.1 level, while in our original analysis it was negative but insignificant, but the two sets of results coincide in pointing against the hypothesis that eradication-focused drug policy is associated with greater state violence.

Appendix B2 – alternative modeling strategies.

Based on the discussion above, we believe that our convenience sample may be over-representing violent acts as a function of factors including population size and year, and that violent acts may also be excluded from the dataset for reasons that cannot be modeled in a systematic way. Under these circumstances, a selection model is not an option because we cannot identify any variable that could systematically predict selection (inclusion in the CVR dataset) but not violence. We therefore turn to three alternative modeling strategies that take a more conservative approach that assumes some error in our zeroes. The tables below repeat the regression in Table 1 column 1 and show that our results hold. Thus, while we cannot completely discard the possibility of selection issues in our data, we have reasons to believe that our findings are not the result of those selection issues.

Table B2a – results of zero-inflated model of analysis in Table 1, Column 1

A zero-inflated model echoes our main analysis in finding that the presence of the drug trade predicts a significant and sizable increase in violence committed by state actors, just as in Table 1. Thus, even if some of our zeroes are a function of non-inclusion in the CVR dataset, our results seem to hold.

The right column below provides results of the first stage logistic model predicting whether a district-year actually did not see violence (the certain zeroes). As seen in the negative and statistically significant coefficients for nearly every variable, many of our variables (though not coca) are statistically significant predictors of not having violence. This provides further justification of the decision not to use a Heckman selection model.

	Count model State Acts		Inflated Logit State Acts
coca	0.952***	coca	-0.284
	(3.97)		(-0.83)
logaltitude	0.236*	logaltitude	-0.239**
	(2.07)		(-2.72)
capital	0.369+	capital	-0.259
	(1.76)		(-0.98)
electrification	0.000565	electrification	0.00132
	(0.10)		(0.33)
logpop	-0.000317	logpop	-0.764***
	(-0.00)		(-8.59)
Lag SL killings	0.0294+	Lag SL killings	-1.048***
	(1.93)		(-4.23)
Lag MRTA killings	0.0463	Lag MRTA killings	-1.020
	(0.17)		(-0.96)
Lag State killings	0.0609*	Lag State killings	-1.453***
	(2.23)		(-4.12)
lfull	-0.172	lfull	-1.051***
	(-0.77)		(-4.43)
lcontested	-0.0104	lcontested	-0.702***
	(-0.05)		(-3.60)
Fujimori	-1.495***	Fujimori	-0.286
	(-6.61)		(-1.48)
Garcia	-0.707***	Garcia	-0.600***
	(-3.61)		(-3.92)
_cons	-0.0256	_cons	11.73***
	(-0.02)		(10.02)
N 12505			
Non zero observations 672 Zero observations 11833			
P>chi2 0.0000			
Log Pseudo likelihood -3947.256			

Table B2b – results of tobit model of analysis in Table 1, Column 1

The table below presents the results of two tobit models, with the lower bound set at 0 and at 1, respectively. The results of both are very similar to those presented in Table 1, Column 1. A comparison of the model statistics for the two models (AIC and BIC) does suggest that some of our zeroes might be a function of left-censoring, but the fact that both of these analyses produce results very similar to our core analysis gives us

	Lower limit=0 logState Acts	Lower limit=1 Logstate Acts
coca	2.045***	1.344***
	(7.22)	(6.92)
logaltitude	0.485***	0.336***
	(6.25)	(5.95)
capital	0.771***	0.467***
	(4.37)	(3.82)
electrification	-0.00734*	-0.00662**
	(-2.32)	(-2.91)
logpop	1.158***	0.773***
	(14.70)	(13.57)
Lag SL killings	0.142***	0.0917***
	(9.30)	(9.05)
Lag MRTA killings	0.675*	0.476**
	(2.56)	(2.72)
Lag State killings	0.0700***	0.0418***
	(4.30)	(3.84)
lfull	1.978***	1.270***
	(10.42)	(9.59)
lcontested	1.273***	0.827***
	(7.34)	(6.84)
Fujimori	-0.356*	-0.390**
	(-2.10)	(-3.27)
Garcia	0.500**	0.212+
	(3.17)	(1.95)
_cons	-19.81***	-12.32***
	(-17.05)	(-14.56)
/		
var(e.logstate)	10.15***	4.236***
	(14.68)	(13.58)
N	12505	12505
AIC	6303.585	4769.123
BIC	6407.659	4873.197

Logit model +<0.1 *<0.05 **<0.01 ***<0.001

Table B2c – results of rare events model of analysis in Table 1, Column 1

In our final robustness check, we use a rare events model to assess the possibility that excess zeros in the dataset affect the results. To control for temporal dependence in the data, we include the time polynomial of years since the state last committed violence (syr variables in the model). As seen below, the results are very similar to those reported in Table 1 of the main paper, with coca showing a strong, positive, and statistically significant effect on the likelihood of observing any state violence. As noted above, this model does not include MRTA violence in the present version.

	Dummy State Acts
coca	2.549***
	(5.85)
logaltitude	1.286***
	(5.01)
capital	1.487**
	(2.75)
electricityperc	0.997
	(-1.37)
logpop	1.808***
	(10.83)
senderomuertoslag	1.096***
	(4.09)
statedeathslag	1.081
	(1.28)
lfull	2.588***
	(6.97)
lcontested	1.870***
	(5.12)
fujimori	1.274
	(1.59)
garcia	1.933***
	(4.67)
syr	0.420***
	(-8.27)
syr2	1.126***
	(6.22)
syr3	0.995***
	(-5.58)
N	12505

Exponentiated coefficients; *t* statistics in parentheses
+<0.1 *<0.05 **<0.01 ***<0.001

Appendix B3 – Matching model

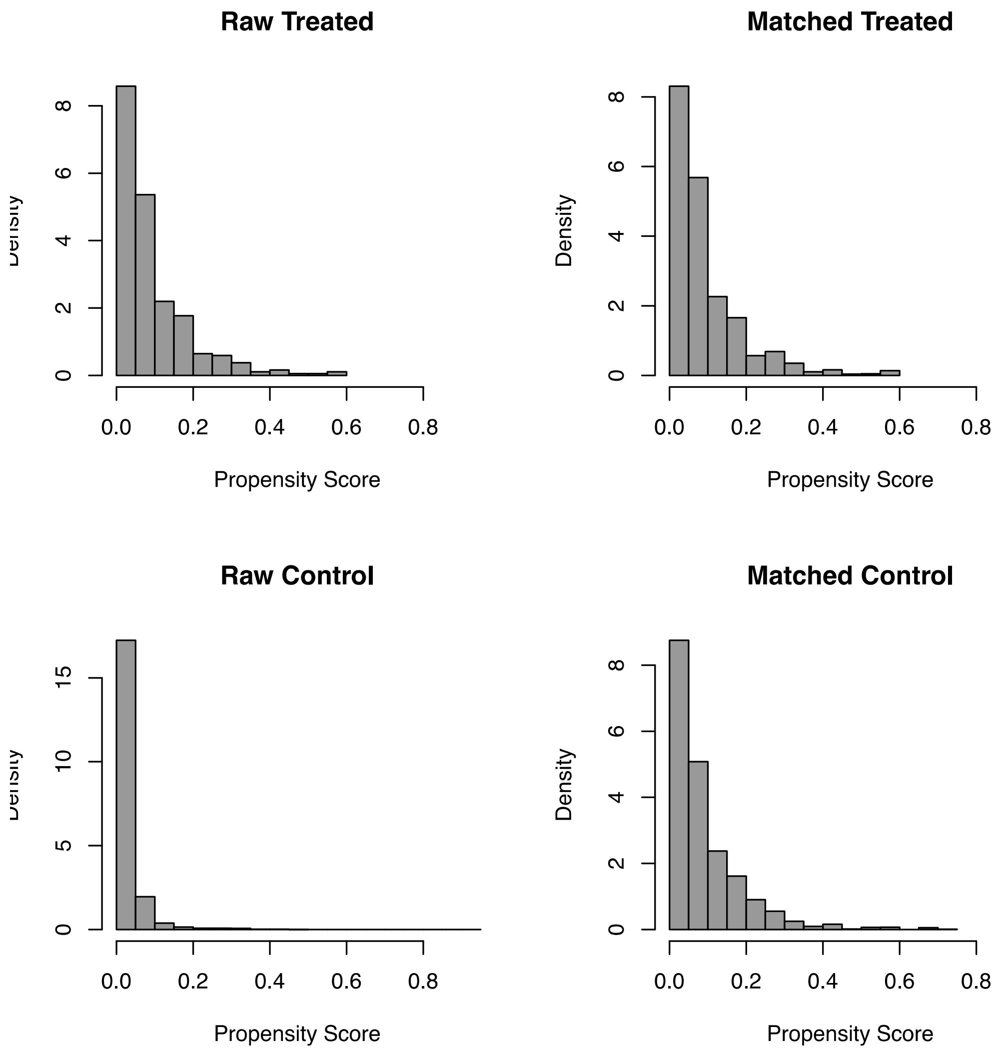
In Part 3 of the paper, we provide a narrative discussion of the evolution of drug production and the internal conflict in Peru, arguing that the former was not endogenous to the latter. One might nevertheless be concerned that the systematic determinants of drug production might also shape the treatment of civilians by Peruvian state actors during the country's armed conflict. Here, we use a matching model to address this concern and reduce bias in the estimation of the treatment effect of drug production.

Following the guidance of Stuart (2010), we use full matching, including as observed covariates all of the independent variables in the regression models in Table 1.[ix] As Table B3a below shows, we achieve good balance across control and treated samples for all of our variables. The histograms in Figure B3a show that the propensity scores for the control and treated samples are similar for the matched data.

Table B3a – matching balance

	All Data			Matched Data		
	Means (treated)	**Means (control)**	**Mean Diff**	**Means (treated)**	**Means (control)**	**Mean Diff**
Distance	0.0928	0.0279	0.0649	0.0928	0.0932	-0.0004
Log(pop)	8.8300	8.3536	0.4764	8.8300	8.6126	0.2174
Log(altitude)	6.5465	7.5458	-0.9993	6.5465	5.9342	0.6123
Capital	0.1340	0.1051	0.0290	0.1340	0.1286	0.0055
Electrification	16.6764	19.5623	-2.8859	16.6764	21.3434	-4.6670
SL killings lag	1.8901	0.3512	1.5389	1.8901	1.2000	0.6900
MRTA killings lag	0.0429	0.0094	0.0335	0.0429	0.0533	-0.0104
State killings lag	0.7051	0.1691	0.5360	0.7051	0.4603	0.2448
Lfull	0.1903	0.1258	0.0646	0.1903	0.1174	0.0730
Lcontested	0.1287	0.1685	-0.0398	0.1287	0.0694	0.0593
Fujimori	0.6059	0.3217	0.2842	0.6059	0.5161	0.0898
Garcia	0.1930	0.3422	-0.1491	0.1930	0.2183	-0.0253

Figure B3a – propensity score histograms



With the matched samples, we then repeat the analysis in Table 1, Column 1. As Column 1 in Table B3b below shows, the main finding holds up – the effect of coca production for the drug trade on state violence against civilians is positive, statistically significant, and substantively important. As Stuart (2010, 16) recommends, we also provide results with sub-class fixed effects. While a negative binomial model did not converge, an OLS model with this specification is shown in Table B3b column 2, with similarly strong results for our first hypothesis.

Table B3b: post-matching regressions – compare to Table 1 Column 1

	State acts (neg binomial)	State acts (OLS with subclass fixed effects)
Constant	-17.7619***	-11.9174***
	(0.7107)	1.6004
Coca	1.4828***	1.1877***
	(0.2538)	(0.3141)
Log(altitude)	0.7495***	6.2704***
	(0.0574)	(0.6473)
Capital	1.4347***	-2.8919***
	(0.1432)	(0.3654)
Electrification	0.0029	0.3444***
	(0.0037)	(0.0367)
Population (log)	1.0623***	-2.6445***
	(0.0670)	(0.3450)
SL deaths lag	0.1164***	-0.0365
	(0.0078)	(0.0552)
MRTA deaths lag	0.1510	-0.7004**
	(0.1052)	(0.2784)
State deaths lag	0.1527***	-0.1481***
	(0.0095)	(0.0223)
Lfull	1.6244***	-1.5283**
	(0.1681)	(0.5172)
Lcontested	0.7907***	1.9233***
	(0.1972)	(0.3959)
Fujimori	-0.1778	-13.5057***
	(0.1720)	(1.1438)
Garcia	0.4526**	-1.2300***
	(0.1954)	(0.2982)
AIC	7560.3	
# obs	12505	12505
Theta	0.06094	
R-squared		0.5072
Adj R-squared		0.4916

Works cited:

Stuart, E. A. (2010). Matching methods for causal inference: A review and a look forward. *Statistical science: a review journal of the Institute of Mathematical Statistics* vol.25 #1.

Appendix B4. Difference in difference estimations

Prominent studies assessing the impact of resources on conflict use difference in difference estimations to provide a robust identification strategy of the impact of a given resource on violence. While we considered using this modeling strategy, we believe that it is not the best strategy in our case due to both substantive concerns and data quality issues. However, we present below the results of what we consider the most plausible difference in difference estimations, which largely align with our findings.

Difference in difference estimations rely on a) identifying a clear treatment, such as a radical policy change, or, b) in the studies relevant for us, a clear exogenous price shock or exogenous production boom (Dube and Vargas, 2013). To assess hypothesis 1 in our paper, a difference in difference approach would identify a clear price shock in coca, or an external shock in coca availability, to then compare coca and non-coca regions and assess changes in state violence after the shock. The problem is that there is no such clear shock in Peru. One imperfect possibility is to use 1984 (the year most studies identify as the beginning of coca boom for the cocaine market in the Upper Huallaga Valley). Though in 1984 there was already some production both inside and outside the UHV, this approach would be similar to the one taken by Angrist and Kugler (2008), who analyze the impact of coca on violence and employment in Colombia, using 1994 as the year of a clear change in production due to the decline of coca production in Peru, though they acknowledge that some production already existed in Colombia. Below, we report difference in difference estimations using 1984 as the beginning of the time trend for treatment. Though the results are consistent with our main findings, we remain skeptical about using these estimations as our main models because we remain unsure that the year actually represents a clear break that could work as a treatment. Additionally, despite achieving a balanced panel, we doubt that the differences between coca and non-coca regions would have been stable over time without the coca boom, or that the coca boom was independent of other characteristics that affected conflict dynamics in the UHV.

For hypotheses 2 and 3 we use the policy as the treatment: movement away from manual eradication in 1989, and increased militarization in 1992. The problem with this analysis is that we lack fine grained measures of eradication or of militarization that we can use to assess the policy, and to the best of our knowledge those measures are not available. And as with H1 we believe that the key assumptions for difference in difference estimations are not tenable in this case. Trends of state violence in coca and non-coca regions were already different before the policy treatment was introduced, and the logic of the militarization and drug policy was likely correlated with the trends in coca production. The assumptions of exchangeability and positivity thus are unlikely to be satisfied. We still report difference in difference estimations for coca regions using 1989 and 1992 as the beginning of the treatment years.

The table below presents the results for these models, reporting the difference in difference for each interaction of time and treatment. Model 1 reports difference in difference using 1984 as the treatment year and uhv as the treatment considering that the coca boom mainly affected the uhv. The interaction between time and coca production shows that in the uhv violence increased significantly in the years following the coca boom, compared to the non-uhv. Model 2 repeats the same estimation but using our coca measure as the treatment, and the coefficient of the difference is also positive and significant. In other words, these estimations are consistent with our findings that coca production was associated with increases in state violence. In models 3 and 4 we turn to difference in difference estimations for our drug policy variable. Model 4 uses a triple difference for drug policy using coca and 1989 as the initiation of the treatment time trend but the difference is not significant, aligning with our main results which show no clear impact of eradication-based policy. In model 4 we assess the militarization trend; here the coefficients of the time trend reflect the overall decline in violence in the years after 1992, but the difference in difference is not significant. In other words, the coca producing regions did not experience the same significant decline in state violence as the non-coca regions. Overall, we believe these results align with our findings, but for the reasons stated above we don't consider these tests as the best evidence in support of our hypotheses.

State violence using difference in difference estimations (DV: acts of state violence)

	Treatment after 1984 (using uhv) Model 1 (uhv)	Treatment after 1984 coca boom (using coca) Model 2 (coca)	Triple difference drug policy and coca Model 3	Treatment militarization after 1992 Model 4
Time trend	-0.3031**	-0.2979**	-1.5075	-0.5050***
	0.1064	0.1068	2.0625	0.0989
drug policy			-1.9143	
			2.1853	
coca	-0.7581	-0.7743	0	1.3539***
	0.6985	0.6997	(.)	0.3743
			0	
			(.)	
			8.9034**	
			2.9880	
			0	
			(.)	
Diff. in Difference	3.6381***	2.0122**	0	-0.3555
	0.7829	0.7464	(.)	0.4972
logaltitude	0.0881*	0.0960*	-0.6013	0.1016**
	0.0386	0.0391	0.5980	0.0392
capital	0.3998**	0.3752**	-0.6818	0.3667*
	0.1449	0.1451	1.7114	0.1452
electrification	-0.0014	-0.0018	-0.0030	-0.0004
	0.0018	0.0018	0.0291	0.0018
logpop	0.2195***	0.2393***	1.2703*	0.2310***
	0.04	0.04	0.6245	0.04
senderomuertoslag	0.3863***	0.3908***	0.1270	0.3901***
	0.0189	0.019	0.1110	0.0189
mrtadeathslag	0.2716	0.2111	-0.9387	0.2483
	0.2737	0.2742	1.5299	0.2742
statedeathslag	0.1570***	0.1599***	1.9219***	0.1588***
	0.0218	0.0219	0.1638	0.0218
lfull	0.6180***	0.6385***	1.2714	0.6232***
	0.1327	0.1329	1.1956	0.1315
lcontested	0.2907*	0.2877*	0.4731	0.3640**
	0.1159	0.1161	1.2847	0.1171
_cons	-2.0847***	-2.3003***	-5.4946	-2.4183***
	0.4881	0.4905	6.6764	0.4862
N	3796	12505	328	12505
r2	0.0788	0.0757	0.467	0.0769

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Appendix C. The effect of coca on Sendero Violence

This appendix looks at whether our hypothesis that violence is higher in drug producing regions also holds for Sendero Luminoso. Each column represents a distinct dependent variable (type of violence) and we see that the coefficient on our coca variable is positive and significant across all ten models.[x] This is consistent with the finding for state violence in Table 1, and with the findings of Weinstein (2007), which were based on a more temporally restricted and less systematic set of data on human rights violations in the Peruvian conflict.

	(1) **Acts**	(2) **Killings**	(3) **Disappearance**	(4) **Killings members org.**	(5) **Killings leaders**
coca	0.956***	1.054***	1.238***	0.689*	0.655*
	(5.89)	(7.92)	(5.40)	(2.21)	(2.38)
logaltitude	0.257***	0.249***	0.0234	0.418***	0.455***
	(3.41)	(4.25)	(0.26)	(3.67)	(4.55)
capital	0.637***	0.422**	0.744**	0.113	0.00401
	(3.88)	(3.16)	(3.25)	(0.43)	(0.02)
electrification	-0.0203***	-0.0174***	-0.0285***	-0.0242***	-0.0247***
	(-6.81)	(-7.24)	(-5.97)	(-5.16)	(-6.13)
logpop	0.609***	0.583***	0.686***	0.918***	0.804***
	(10.00)	(11.91)	(7.57)	(9.38)	(9.96)
Lag SL killings	0.0184***	0.0109**	0.0220***	0.0237**	0.0154*
	(4.68)	(3.20)	(3.86)	(3.19)	(2.56)
Lag MRTA kill	0.278+	0.397***	0.562***	0.340+	0.530***
	(1.78)	(4.36)	(3.54)	(1.86)	(3.55)
Lag State kill	0.00588	0.00836*	0.00317	-0.00151	-0.00216
	(1.33)	(2.26)	(0.43)	(-0.17)	(-0.30)
lfull	0.661***	0.800***	1.027***	0.880***	0.809***
	(6.63)	(10.06)	(7.31)	(5.11)	(5.80)
lcontested	0.364***	0.425***	0.452**	0.645***	0.630***
	(3.67)	(5.35)	(2.97)	(3.92)	(4.76)
fujimori	-0.115	0.0178	-0.603***	0.241	-0.0961
	(-1.14)	(0.22)	(-3.95)	(1.33)	(-0.66)
garcia	0.550***	0.657***	0.349**	0.816***	0.675***
	(6.40)	(9.40)	(2.93)	(5.11)	(5.53)
_cons	-9.856***	-8.988***	-8.388***	-13.20***	-12.39***
	(-11.01)	(-12.80)	(-7.09)	(-9.32)	(-10.32)
ln_r	-0.394***	0.114	0.525***	1.580***	1.118***
	(-5.22)	(1.58)	(4.25)	(7.85)	(8.07)
ln_s	-1.077***	-0.545***	-1.050***	-0.718***	-0.620***
	(-8.66)	(-5.35)	(-6.18)	(-3.70)	(-3.98)
N	12505	12505	12505	12505	12505

	Police Killings	Massacre	Forced Recruitment	Rape	Kidnapping
coca	2.350***	1.072***	2.659***	1.799***	1.813***
	(5.34)	(3.68)	(6.63)	(4.03)	(7.12)
logaltitude	0.254	0.403**	0.216	0.246	0.159
	(1.60)	(3.05)	(1.00)	(1.16)	(1.42)
capital	0.876*	-0.144	0.276	0.317	0.875***
	(2.49)	(-0.58)	(0.59)	(0.79)	(3.92)
electrification	-0.00167	-0.0304***	-0.0189+	-0.0251*	-0.0220***
	(-0.21)	(-5.38)	(-1.80)	(-2.46)	(-4.04)
logpop	0.919***	0.810***	0.409*	1.129***	0.491***
	(5.29)	(8.78)	(2.32)	(5.58)	(5.08)
Lag SL killings	0.0435*	0.0287***	0.0397**	0.0390***	0.0301***
	(2.23)	(4.70)	(2.80)	(3.40)	(3.75)
Lag MRTA kill	-0.0146	0.479*	-25.81	0.354	-0.349
	(-0.03)	(2.09)	(-0.00)	(0.78)	(-0.63)
Lag State Kill	0.00956	0.0163**	0.00708	-0.00745	0.0273***
	(0.34)	(2.58)	(0.39)	(-0.39)	(3.31)
lfull	-0.190	1.327***	0.574	1.609***	0.932***
	(-0.36)	(7.03)	(1.44)	(4.02)	(4.41)
lcontested	-0.242	0.621**	0.661+	0.799+	0.517*
	(-0.48)	(3.09)	(1.76)	(1.80)	(2.37)
Fujimori	0.839	-0.327	-0.951*	-1.052*	-0.0379
	(1.58)	(-1.49)	(-2.21)	(-2.42)	(-0.16)
Garcia	1.611**	0.454*	0.168	-0.216	0.527**
	(3.28)	(2.47)	(0.50)	(-0.61)	(2.59)
_cons	-16.54***	-15.55***	-12.06***	-18.96***	-11.06***
	(-7.21)	(-11.00)	(-5.03)	(-7.05)	(-8.69)
ln_r	1.643**	2.567	11.58	1.318	1.178
	(2.85)	(1.20)	(0.02)	(0.48)	(1.39)
ln_s	0.926	5.212*	14.65	4.891	2.950*
	(1.08)	(2.20)	(0.02)	(1.30)	(2.33)
N	12505	12505	12505	12505	12505

Table D1a. **Hypothesis 1 – effects of coca production on violence by state forces, using victim counts rather than event counts**

	Massacres	Rapes	Kidnappings	Forcible Recruitment	Extrajudicial killings	Torture
coca	1.134**	1.7497***	1.5986***	2.6895***	1.259***	1.221***
	2.6	0.3979	0.3638	0.5133	4.60	5.08
logaltitude	0.371+	0.4051*	0.0478	0.323	0.462***	0.357***
	1.83	0.1765	0.148	0.2737	3.99	4.07
capital	0.0765	0.3391	0.9705**	0.3192	0.418+	0.574**
	0.21	0.3344	0.3067	0.535	1.85	3.13
Electrification	-0.0295***	-0.0099	-0.0167*	-0.0134	-0.00945*	-0.0087*
	-3.36	0.007	0.0075	0.0121	-2.18	-2.42
logpop	0.992***	0.8627***	0.6180***	0.6968**	0.7928***	0.8407**
	6.40	0.152	0.1454	0.2364	8.62	10.51
Lag (SL killings)	0.0262**	0.0359***	0.0340***	0.0443**	0.0216***	0.0315***
	2.75	0.0103	0.0095	0.0153	3.55	4.64
Lag (Mrta Killings)	0.0521				0.207	0.351**
	0.07				0.76	2.68
Lag (state killings)	0.00319	0.0029	0.0229*	0.0095	-0.0031	0.0033
	0.28	0.0139	0.0091	0.0194	-0.0048	0.0056
lfull	1.849***	1.4204***	1.4620***	0.4747	1.377***	0.951***
	6.05	0.3373	0.3179	0.5519	8.05	6.29
lcontested	0.724*	0.8848**	0.9006**	0.6483	0.762***	0.582***
	2.10	0.3435	0.3353	0.5078	4.20	3.93
fujimori	-1.301***	-0.7605*	-0.3105	-0.7504	-0.530**	-0.295*
	-3.82	0.3395	0.3619	0.5579	-3.20	-2.18
garcia	-0.701*	-0.1916	0.5657+	0.2371	-0.108	0.0497
	-2.49	0.3007	0.3092	0.455	-0.73	0.40
_cons	-17.57***	-17.768***	-13.005***	-16.292***	-14.24***	-13.54***
	-7.74	2.1537	1.8254	3.2452	-10.23	-12.13
ln_r	2.912	12.4344	12.6079	13.4393	-0.158	-0.0981
	0.63	415.791	364.116	647.808	0.1113	-0.95
ln_s	5.984	16.9816	16.1057	16.8777	-0.819***	-0.6554***
	1.20	415.793	364.117	647.809	-3.66	-3.33
N	12505	12505	12505	12505	12505	12505

Table D2a. Hypothesis 1 evaluated using per capita rates as measures of state violence (compare to Table 1, Column 1)

	Per capita victims of state violence
coca	0.1640**
	0.0545
logaltitude	0.0918***
	0.0167
capital	-0.0729+
	0.0435
electrification	-0.0063***
	0.0012
logpop	0.0627***
	0.0181
Lag (Sendero killings)	0.0096*
	0.0038
Lag (MRTA killings)	0.1036*
	0.0486
Lag (state killings)	0.0055
	0.0044
lfull	0.4404***
	0.0657
lcontested	0.1050*
	0.0445
_cons	-5.3379***
	0.1807
N	12505
r2	0.07

Table D2b. Hypothesis 2 evaluated using per capita rates of state violence (compare to Table 2)

	Per capita victims of state violence
drug_policy	-0.1999+
	0.1090
logaltitude	0.0093
	0.0391
capital	-0.0981
	0.1241
Electrification	-0.0039
	0.0028
logpop	-0.0017
	0.0516
Lag (Sendero killings)	0.0134*
	0.005
Lag (MRTA killings)	0.0556
	0.070
Lag (state killings)	0.0152+
	0.0091
lfull	0.3010**
	0.0966
lcontested	-0.2725*
	0.1374
fujimori	0.1207
	0.1324
garcia	0.1777
	0.134
_cons	-4.0557***
	0.4854
N	328
r2	0.07

Table D2c. Hypothesis 3 evaluated using per capita rates of state violence (Compare to Table 3, left columns)

	Per capita victims of state violence Pre 1992	**Per capita victims of state violence Post 1992**
coca	0.0941	0.4085***
	0.0780	0.0761
logaltitude	0.1188***	0.0459+
	0.0241	0.0269
capital	-0.0739	0.0024
	0.0494	0.082
electrification	-0.0088***	0.0011
	0.0015	0.0017
logpop	0.0778***	-0.0231
	0.0220	0.0218
Lag (SL killings)	0.0087*	0.0095+
	0.0039	0.0049
Lag (MRTA killings)	0.1390*	0.1137**
	0.0615	0.0433
Lag (state killings)	0.0048	0.0188*
	0.0043	0.0078
lfull	0.4781***	0.3013***
	0.0728	0.0833
lcontested	0.1572**	0.0463
	0.0496	0.0796
_cons	-5.6321***	-4.5818***
	0.2301	0.2481
N	9191	3314
r2	0.07	0.05

Appendix D3: alternative measures of drug production

Table D3a: Analyses from Table 1 with alternative (coca2) measure of drug production

	State Acts	Killings	Disappearances	Massacre	Rape	Kidnapping	Torture	Forced Recruitment	Extrajudicial
coca2	0.808***	0.694***	0.662**	1.604**	1.415**	1.333**	1.198***	2.544***	1.104***
	(5.07)	(4.19)	(3.27)	(2.98)	(3.05)	(3.16)	(3.99)	(4.29)	(4.39)
logaltitude	0.310***	0.179*	0.193*	0.783*	0.602*	0.101	0.495***	0.570+	0.498***
	(4.76)	(2.54)	(2.36)	(2.57)	(2.57)	(0.56)	(4.44)	(1.79)	(4.20)
capital	0.423**	0.541**	0.487**	-0.0194	0.00533	0.795*	0.621**	0.459	0.445+
	(3.08)	(3.26)	(2.71)	(-0.04)	(0.01)	(2.15)	(2.76)	(0.75)	(1.96)
electrification	-0.00581*	-0.00983**	-0.0218***	-0.0271**	-0.0104	-0.0170*	-0.0111**	-0.00779	-0.00815+
	(-2.16)	(-3.29)	(-5.75)	(-2.69)	(-1.29)	(-2.04)	(-2.70)	(-0.62)	(-1.87)
logpop	0.731***	0.539***	0.879***	1.010***	0.886***	0.590***	0.892***	0.624*	0.787***
	(12.49)	(8.83)	(11.39)	(5.42)	(4.96)	(3.63)	(9.80)	(2.50)	(8.61)
Lag SL killings	0.0221***	0.0153***	0.0158**	0.0114	0.0469**	0.0328**	0.0379***	0.0448*	0.0156**
	(5.18)	(3.58)	(3.16)	(0.99)	(2.76)	(2.64)	(4.72)	(2.34)	(2.62)
Lag MRTA killin	0.266*	0.421***	0.438***	0.108	-0.344	0.107	0.309*	-15.43	0.233
	(2.36)	(3.73)	(3.76)	(0.14)	(-0.36)	(0.20)	(2.25)	(-0.00)	(0.87)
Lag State killings	0.00642	0.00370	0.00370	-0.00236	-0.0148	0.0306*	-0.00000608	-0.00216	-0.00194
	(1.46)	(0.81)	(0.68)	(-0.18)	(-1.04)	(2.44)	(-0.00)	(-0.10)	(-0.29)
lfull	0.889***	1.002***	1.029***	1.587***	1.194***	1.301***	0.904***	0.634	1.420***
	(7.71)	(8.56)	(7.67)	(5.01)	(3.51)	(3.98)	(6.14)	(1.17)	(8.27)
lcontested	0.489***	0.352**	0.638***	0.460	0.601	0.857*	0.436**	0.433	0.713***
	(4.28)	(2.88)	(4.73)	(1.27)	(1.64)	(2.45)	(2.91)	(0.81)	(3.93)
fujimori	0.0299	-0.0686	-0.240+	-1.167***	-0.643+	-0.186	-0.202	-0.538	-0.479**
	(0.28)	(-0.62)	(-1.87)	(-3.44)	(-1.90)	(-0.52)	(-1.54)	(-0.99)	(-2.90)
garcia	0.328***	0.181+	0.236*	-0.695*	-0.247	0.497	0.0410	0.0641	-0.116
	(3.37)	(1.80)	(2.14)	(-2.52)	(-0.82)	(1.62)	(0.35)	(0.14)	(-0.80)
_cons	-12.32***	-8.748***	-11.86***	-15.64***	-17.33***	-11.18***	-13.28***	-16.74***	-14.52***
	(-14.95)	(-9.99)	(-11.58)	(-4.95)	(-6.25)	(-5.25)	(-9.83)	(-4.73)	(-10.37)
ln_r	-0.583***	0.0593	0.132	3.331**	-0.289	0.133	1.301***	-0.736**	-0.175
	(-7.58)	(0.68)	(1.32)	(3.07)	(-1.51)	(0.64)	(8.00)	(-2.69)	(-1.58)
ln_s	-0.727***	-0.792***	-0.808***	-0.902+	-1.207**	-0.797+	-0.860***	-1.000	-0.832***
	(-4.43)	(-5.71)	(-5.23)	(-1.94)	(-2.69)	(-1.77)	(-5.43)	(-1.11)	(-3.76)
N	12505	12505	12505	12505	12505	12505	12505	12505	12505

Negative binomial models with random effects. +<0.1 *<0.05 **<0.01 ***<0.001

Table D3b: Hypothesis 3 – Analyses from Table 3 with alternative (coca2) measure of drug production

	State 1980-1991	**State 1992-2000**	**Military 1980-1991**	**Military 1992-2000**
Coca2	0.622**	1.651***	0.817***	1.8249***
	3.23	5.34	3.65	0.3366
Log(altitude)	0.426***	0.307**	0.483***	0.1618
	5.16	2.97	4.55	0.1371
Capital	0.353*	0.6489**	0.380+	0.8592**
	2.18	0.2423	1.96	0.3154
Electrification	-0.0097**	0.0056	-0.0135**	-0.0027
	-2.820	1.25	-3.25	0.0061
Log(population)	0.783***	0.792***	0.728***	0.6512***
	11.21	7.75	8.95	0.1319
Lag SL killings	0.0229***	0.0453*	0.0213***	0.0412+
	5.06	2.42	4.42	0.0205
Lag MRTA killings	0.263+	0.301	0.372*	
	1.84	0.96	2.53	
Lag state killings	0.00023	0.0565***	0.0009	0.0610***
	0.007	4.11	0.0019	0.0175
Lfull	0.958***	0.771*	1.150***	1.1681**
	7.47	2.45	8.29	0.3638
Lcontested	0.730***	0.0741	0.719***	0.3255
	5.75	0.24	4.91	0.3688
Constant	-13.42***	-14.18***	-13.355***	-12.2787***
	-13.44	-9.89	-11.15	1.796
N	9191	2442	9191	2442
Wald chi2	354.99	327.11	298.68	246.64
P>chi2	0.0000	0.0000	0.0000	0.0000
Log likelihood	-3248.5977	-494.85613	-2622.7282	-351.2544

Negative binomial models with random effects. +<0.1 *<0.05 **<0.01 ***<0.001

Table D3c: Hypothesis 3 – Analyses from Table 4 with alternative (coca2) measure of drug production

	Milshare	Milrate
Coca2	0.294**	2.670
	2.04	(0.58)
Log(altitude)	0.0130	-1.695
	0.039	(-0.86)
Capital	0.0901	5.081
	0.75	(1.35)
Electrification	-0.0016	-0.403***
	-1.05	(-4.67)
Log(population)	0.0737*	-6.037***
	2.23	(-3.36)
Lag SL killings	0.297***	0.279
	18.90	1.27
Lag MRTA killings	0.196	6.129
	0.86	(1.09)
Lag state killings	0.133***	0.413+
	7.34	(1.85)
Lfull	0.6550***	13.9372***
	0.1105	(3.9833)
Lcontested	0.2772**	2.3592
	0.0972	(3.8609)
Fujimori	-0.2572**	26.5276***
	0.0870	(4.0484)
Garcia	-0.0419	22.0477***
	0.0844	(3.6109)
Constant	0.5881	123.1054***
	0.4139	(26.0497)
N	12505	672
Prob>f	0.0000	0.0000
R sq	0.0627	0.2595

OLS regression. +<0.1 *<0.05 **<0.01 ***<0.001

APPENDIX D4:

In **Tables D4a-D4d** we repeat the analyses in Tables 1-4 with an alternative poverty measure to the electrification measure used in the body of the paper. There are no available time series of socioeconomic indicators at the district level for Peru in the 1980s and 1990s. Nor do the 1981 and 1993 censuses have a common question we can use to build a consistent time series, which is why we opted for electrification as our main indicator. Here, for 1981, we use data on household income to calculate an indicator of the poverty rate – percentage of households with income under 50 soles per month, on a 0 to 1 scale.[xi] The national average for this variable is 0.62. From the 1993 census, we use employment; we draw on answers to a question that asks people their employment situation, measuring the percentage in each district that answer that they are currently working. The national average is 36.7%. This measure likely understates the employment rate in Peru especially when informal labor is taken into account. Yet because the extent of this understatement is not likely to vary across districts, we believe it is an adequate measure of variation in socio-economic conditions.[xii]

District level economic indicators are not available for other years. We therefore use the 1981 poverty rates as indicators of economic conditions for the years 1980-1992, and the 1993 employment rates as indicators for the years 1993-2000. To create a variable that covers the entire time period, we calculate a dummy variable for each district-year that reflects whether poverty is above (1) or below (0) the national average.[xiii] The online database for the 1981 census is missing data from three entire departments, including two (Apurímac and San Martín) in our dataset. To calculate poverty rates for districts in those departments, we decided to use our poverty indicator for 1993 for all years in those districts.

Table D4a: Hypothesis 1 – Repeating Table 1 using alternative poverty measure

	Acts (victims)	Killings (victims)	Disappearances (victims)	Massacres (events)	Rapes (events)	Kidnapping (events)	Extrajudicial Killings (victims)	Torture (victims)
coca	0.888***	0.936***	0.690***	1.825**	1.964***	1.834***	1.475***	1.352***
	(5.37)	(5.30)	(3.33)	(3.28)	(4.09)	(4.44)	(5.63)	(5.81)
logaltitude	0.268***	0.145*	0.151+	0.557*	0.608**	0.116	0.493***	0.390***
	(4.35)	(2.17)	(1.96)	(2.29)	(2.66)	(0.74)	(4.38)	(4.56)
capital	0.415**	0.517**	0.329+	-0.156	-0.0692	0.789*	0.436*	0.536**
	(3.09)	(3.24)	(1.82)	(-0.37)	(-0.17)	(2.26)	(2.03)	(2.96)
Poor district	0.509***	0.666***	0.644***	2.105***	0.544	0.194	0.673***	0.309*
	(5.03)	(5.62)	(4.77)	(4.32)	(1.52)	(0.63)	(3.76)	(2.16)
logpop	0.704***	0.505***	0.679***	0.993***	0.831***	0.380**	0.784***	0.792***
	(13.60)	(9.17)	(10.17)	(5.69)	(5.02)	(2.80)	(9.41)	(11.11)
Lag SL killing	0.0255***	0.0187***	0.0195***	0.0154	0.0513**	0.0411***	0.0218***	0.0322***
	(5.79)	(4.28)	(3.87)	(1.33)	(3.05)	(3.41)	(3.60)	(4.75)
Lag MRTA kill	0.280*	0.431***	0.438***	-0.0228	-0.437	0.0716	0.252	0.360**
	(2.48)	(3.79)	(3.86)	(-0.03)	(-0.44)	(0.13)	(0.97)	(2.79)
Lag State kill	0.00441	0.00314	0.00224	-0.00386	-0.0140	0.0326*	-0.00321	0.00304
	(1.01)	(0.69)	(0.41)	(-0.30)	(-1.01)	(2.39)	(-0.49)	(0.47)
lfull	0.873***	0.973***	1.086***	1.732***	1.105**	1.160***	1.380***	0.958***
	(7.78)	(8.37)	(8.19)	(5.51)	(3.27)	(3.62)	(8.11)	(6.42)
lcontested	0.562***	0.455***	0.754***	0.819*	0.744*	1.023**	0.816***	0.636***
	(5.16)	(3.79)	(5.72)	(2.32)	(2.07)	(3.12)	(4.56)	(4.37)
fujimori		0.00280	-0.249+	-0.998**	-0.645+	-0.419	-0.426*	-0.301*
		(0.03)	(-1.96)	(-2.99)	(-1.88)	(-1.18)	(-2.58)	(-2.22)
garcia		0.189+	0.250*	-0.717**	-0.195	0.468	-0.171	0.00907
		(1.90)	(2.31)	(-2.64)	(-0.65)	(1.60)	(-1.18)	(0.07)
_cons	-12.02***	-8.810***	-10.42***	-15.76***	-17.53***	-9.889***	-15.13***	-13.70***
	(-14.84)	(-10.25)	(-10.71)	(-5.17)	(-6.19)	(-5.00)	(-10.82)	(-12.27)
ln_r	-0.598***	0.0563	0.0969	3.264**	-0.261	0.148	-0.201+	-0.0942
	(-7.91)	(0.66)	(1.01)	(3.28)	(-1.34)	(0.69)	(-1.83)	(-0.93)
ln_s	-0.789***	-0.812***	-0.980***	-0.981*	-1.063*	-0.564	-0.788***	-0.665***
	(-5.02)	(-6.01)	(-6.59)	(-2.31)	(-2.09)	(-1.08)	(-3.52)	(-3.51)
N	12825	12825	12825	12825	12825	12825	12825	12825

t statistics in parentheses
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table D4b: Hypothesis 2 – Repeating Table 2 using alternative poverty measure

	Victims of state acts
drug_policy	-0.5178
	0.3801
logaltitude	0.1146
	0.2757
capital	-0.2783
	0.5117
Poor district	-0.3306
	0.3020
logpop	1.1688***
	0.2667
Lag (SL killings)	0.0063
	0.0268
Lag (MRTA killings)	0.2560
	0.3783
Lag (State killings)	0.0225
	0.0207
lfull	0.6150*
	0.2996
lcontested	0.4091
	0.4422
fujimori	0.8301
	0.5601
garcia	1.0525+
	0.5575
Constant	-14.0105***
	2.58
ln_r	-0.2908
	0.3607
ln_s	-0.0433
	0.7756
N	353

Table D4c: repeating Table 3 with alternative poverty measure

	Victims of acts 1980-1991	**Victims of Acts After 1992**	**Armed Forces 1980-1991**	**Armed Forces After 1992**
coca	1.093***	1.461***	1.220***	1.718***
	(4.59)	(5.96)	(4.58)	(6.08)
logaltitude	0.463***	0.180*	0.503***	0.168
	(5.84)	(2.24)	(4.92)	(1.59)
capital	0.311*	0.784***	0.294	1.035***
	(1.97)	(3.96)	(1.58)	(4.00)
Poor district	0.144	0.190	0.353*	0.342
	(0.98)	(1.03)	(2.10)	(1.57)
logpop	0.687***	0.764***	0.628***	0.554***
	(11.06)	(10.07)	(8.54)	(5.80)
senderomuertoslag	0.0283***	0.0299+	0.0271***	0.0287
	(6.22)	(1.84)	(5.53)	(1.44)
mrtadeathslag	0.277*	0.290	0.389**	0.472*
	(2.01)	(1.32)	(2.76)	(2.21)
statedeathslag	-0.000128	0.0512***	0.000369	0.0575**
	(-0.03)	(3.49)	(0.07)	(2.96)
lfull	0.919***	0.734**	1.138***	0.961***
	(7.25)	(3.08)	(8.16)	(3.49)
lcontested	0.813***	0.110	0.808***	0.414
	(6.54)	(0.47)	(5.60)	(1.53)
_cons	-13.09***	-12.48***	-13.25***	-11.00***
	(-13.36)	(-10.75)	(-11.10)	(-7.57)
ln_r	-0.665***	0.800*	-0.732***	0.320
	(-8.28)	(2.08)	(-8.57)	(0.79)
ln_s	-0.872***	2.107**	-1.195***	1.250
	(-4.96)	(3.00)	(-6.51)	(1.19)
N	9424	3401	9424	3401

Table D4d: repeating Table 4 with alternative poverty measure

	Milshare	Milrate
coca	1.1182***	18.3860***
	0.2056	5.0009
logaltitude	0.0248	0.2829
	0.0322	1.8825
capital	0.0809	2.6373
	0.1221	3.6266
Poor district	0.0737	6.9125*
	0.0823	3.4269
logpop	0.0633+	-9.8043***
	0.0328	1.5504
Lag (SL killings)	0.2956***	0.3966+
	0.0161	0.2162
Lag (MRTA killings)	0.1731	8.5179
	(0.2314)	(5.5570)
Lag (state killings)	0.1344***	0.4344+
	0.0186	0.2224
lfull	0.6938***	14.5030***
	0.1106	3.9368
lcontested	0.2632**	5.6502
	0.0985	3.7447
fujimori	-0.2725**	22.0305***
	0.0916	3.9425
garcia	-0.0209	21.5231***
	0.0857	3.5547
Constant	-0.6811+	127.9692***
	0.4076	25.300
N	12825	690
r2	0.0613	0.2530

APPENDIX D5:

In Tables D5a-D5d, we drop presidential election years (1980, 1985, 1990, 1995) from the analysis. Because presidential inauguration in Peru takes place on July 28, and can be accompanied by policy shifts of the kind relevant to many of our hypotheses, we might be concerned that including them in the analysis might be a source of error.

Table D5a: Hypothesis 1 – Repeating Table 1 excluding presidential election years.

	Victims of Acts	Killings	Disappearances	Massacres	Victims Torture	Massacres	Rape
coca	0.705***	0.656***	0.248	1.569***	1.042***	0.9712+	1.6429***
	(3.77)	(3.42)	(1.07)	(3.43)	(4.13)	0.5262	0.4334
logaltitude	0.276***	0.136+	0.0821	0.0200	0.335***	0.4880*	0.4322*
	(4.15)	(1.94)	(1.00)	(0.11)	(3.78)	0.2444	0.1942
capital	0.419**	0.426*	0.446*	0.904*	0.636***	-0.2087	0.276
	(2.82)	(2.40)	(2.28)	(2.28)	(3.39)	0.4235	0.3645
electrification	-0.00859**	-0.00995**	-0.0258***	-0.0222*	-0.00926*	-0.0257**	-0.006
	(-3.05)	(-3.29)	(-6.30)	(-2.37)	(-2.52)	0.0095	0.0074
logpop	0.767***	0.528***	0.917***	0.628***	0.832***	0.9970***	0.8418***
	(12.31)	(8.28)	(10.85)	(3.49)	(10.10)	0.1756	0.1672
Lag SL killings	0.0500***	0.0507***	0.0620***	0.0448*	0.0495***	0.0800***	0.0836***
	(7.83)	(8.00)	(8.06)	(2.15)	(5.63)	0.0137	0.0125
Lag MRTA killings	0.360*	0.560***	0.280	0.0176	0.457*		
	(1.99)	(3.70)	(1.43)	(0.03)	(2.20)		
Lag State killings	0.0224***	0.0213***	0.0239***	0.0389+	0.0240**	0.0363**	0.0409**
	(4.16)	(4.18)	(3.74)	(1.84)	(3.12)	0.0133	0.0127
lfull	0.938***	1.078***	0.959***	1.040**	1.108***	1.8275***	1.3975***
	(7.35)	(8.44)	(6.33)	(2.67)	(6.66)	0.3243	0.3671
lcontested	0.484***	0.338*	0.455**	0.764*	0.624***	0.5713	0.7382+
	(3.87)	(2.55)	(3.01)	(2.00)	(3.85)	0.374	0.3802
fujimori	-0.125	-0.265*	-0.378**	0.0618	-0.474**	-1.6921***	-1.1496**
	(-1.07)	(-2.21)	(-2.79)	(0.14)	(-3.25)	0.3526	0.3538
garcia	0.147	-0.0443	-0.0680	0.937*	-0.153	-0.8392**	-0.6534+
	(1.30)	(-0.38)	(-0.54)	(2.41)	(-1.08)	0.2951	0.3428
_cons	-12.29***	-8.263***	-11.18***	-11.25***	-13.33***	-13.7579***	-16.9434***
	(-14.23)	(-9.19)	(-10.52)	(-4.94)	(-11.67)	2.91	2.4143
ln_r	-0.535***	0.103	0.130	0.182	0.0149	4.0107***	13.2771
	(-6.34)	(1.09)	(1.19)	(0.65)	(0.13)	1.1218	416.0616
ln_s	-0.533**	-0.672***	-0.718***	-0.448	-0.342	0.5267	15.448
	(-2.69)	(-4.10)	(-3.90)	(-0.58)	(-1.41)	1.0141	416.0625
N	10867	10867	10867	10867	10867	10867	10867

Table D5b: table 2 without presidential election years

	Victims of acts
Drug policy	0.4842
	0.7566
logaltitude	-0.1653
	0.3142
capital	0.1774
	0.5811
electrification	-0.0230
	0.0146
logpop	1.3003***
	0.3082
Lag (SL killings)	0.0058
	0.0293
Lag (MRTA killings)	0.2053
	0.3620
Lag (state killings)	0.0389
	0.0241
lfull	0.7431*
	0.3609
lcontested	0.4331
	0.4812
fujimori	0.9613
	0.7636
_cons	-13.5359***
	2.88
ln_r	-0.2547
	0.345
ln_s	0.0968
	0.8314
N	301

Table D5c: Table 3 excluding presidential election years

	Victims of acts Pre 1992	Victims of Acts Post 1992	Armed Forces Pre 1992	Armed Forces Post 1992
coca	0.653*	1.485***	0.861**	1.607***
	(2.27)	(5.75)	(2.77)	(5.26)
logaltitude	0.413***	0.175*	0.419***	0.108
	(4.66)	(2.09)	(3.86)	(0.96)
capital	0.282	0.798***	0.325	1.154***
	(1.55)	(3.95)	(1.54)	(4.18)
electrification	-0.0114**	0.000643	-0.0131**	-0.00862+
	(-3.04)	(0.17)	(-3.01)	(-1.72)
logpop	0.805***	0.765***	0.705***	0.644***
	(10.45)	(8.90)	(8.20)	(5.73)
Lag SL killings	0.0668***	0.0310+	0.0851***	0.0312
	(7.62)	(1.88)	(7.90)	(1.50)
Lag MRTA killings	-0.182	0.299	-0.419	0.450*
	(-0.47)	(1.37)	(-0.82)	(2.04)
Lag State Killings	0.0166*	0.0475**	0.0182**	0.0523*
	(2.49)	(3.22)	(2.61)	(2.51)
lfull	1.038***	0.819***	1.329***	1.053***
	(7.15)	(3.40)	(8.43)	(3.70)
lcontested	0.741***	0.144	0.762***	0.380
	(5.18)	(0.59)	(4.59)	(1.32)
_cons	-13.61***	-12.46***	-13.16***	-10.98***
	(-12.41)	(-10.73)	(-10.36)	(-7.28)
ln_r	-0.619***	0.767+	-0.597***	0.167
	(-6.67)	(1.95)	(-5.68)	(0.43)
ln_s	-0.603*	2.064**	-0.542+	0.816
	(-2.53)	(2.82)	(-1.78)	(0.70)
N	7553	3314	7553	3314

t statistics in parentheses
+ p<0.10, * p<0.05, ** p<0.01, *** p<0.001

Table D5d: Table 4 excluding presidential election years

	Milshare	Milrate
coca	0.7811***	10.1178+
	0.2038	5.79
logaltitude	0.0103	-1.0860
	0.0326	2.08
capital	0.0437	5.24
	0.1215	4.06
electrification	0.0001	-0.3498***
	0.0015	0.0919
logpop	0.0237	-6.6459***
	0.0334	1.975
Lag (SL killings)	0.5047***	0.9362**
	0.0187	0.3147
Lag (state killings)	0.2184***	0.3890
	0.0212	0.2953
Lag (MRTA killings)	-0.1200	2.1309
	0.2617	6.2561
lfull	0.5879***	13.9348**
	0.1169	4.4827
lcontested	0.1273	1.6539
	0.0985	4.2211
fujimori	-0.3788***	27.0194***
	0.0876	4.4539
garcia	-0.1777*	23.8899***
	0.0882	4.1125
_cons	-0.1404	117.7307***
	0.4084	28.3498
N	10867	558
r2	0.104	0.2825

Table D6: Militarization of drug policy and state violence against civilians for different types of violence (Compare to left two columns of Table 3)

	Killings (victims) Pre 1992	After 1992	Disappearances (victims) Pre 1992	After 1992
coca	1.134***	1.503***	0.840**	1.277***
	(3.76)	(5.00)	(2.92)	(3.36)
logaltitude	0.353***	-0.0353	0.193*	0.0434
	(3.92)	(-0.37)	(2.21)	(0.33)
capital	0.468*	0.798**	0.522**	0.662*
	(2.44)	(2.87)	(2.64)	(2.04)
electricityperc	-0.0130***	-0.00744+	-0.0218***	-0.0250***
	(-3.43)	(-1.69)	(-5.02)	(-3.47)
logpop	0.635***	0.483***	0.822***	1.031***
	(8.60)	(5.01)	(9.69)	(6.36)
senderomuertoslag	0.0173***	0.0479*	0.0206***	0.0321
	(3.74)	(2.45)	(3.88)	(1.35)
mrtadeathslag	0.394*	0.620**	0.455**	0.468+
	(2.21)	(3.05)	(3.14)	(1.75)
statedeathslag	-0.000637	0.00678	-0.00308	0.0520*
	(-0.13)	(0.39)	(-0.49)	(2.47)
lfull	1.024***	0.647*	1.088***	0.586
	(7.89)	(2.54)	(7.56)	(1.61)
lcontested	0.669***	-0.348	0.874***	-0.0298
	(4.92)	(-1.22)	(5.99)	(-0.09)
_cons	-10.77***	-6.615***	-11.24***	-12.44***
	(-9.86)	(-5.08)	(-10.20)	(-6.55)
ln_r	-0.0324	0.591**	0.125	0.693**
	(-0.35)	(3.19)	(1.18)	(2.78)
ln_s	-0.925***	-0.360	-0.783***	-0.145
	(-6.12)	(-0.92)	(-4.55)	(-0.24)
N	9191	3314	9191	3314

	Kidnapping (events) Pre 1992	After 1992	Extrajudicial killings (victims) Pre 1992	After 1992	Torture (Victims) Pre 1992	After 1992
coca	1.518**	2.367*	1.578***	2.305***	1.339***	1.752***
	(2.86)	(2.22)	(3.62)	(4.92)	(3.70)	(4.27)
logaltitude	-0.0461	0.713	0.653***	0.290	0.456***	0.230+
	(-0.27)	(1.20)	(4.30)	(1.56)	(4.10)	(1.70)
capital	0.890*	0.840	0.320	0.642	0.527*	0.982**
	(2.30)	(0.94)	(1.27)	(1.47)	(2.50)	(3.15)
electricityperc	-0.0217*	-0.00543	-0.0109*	0.00158	-0.0113*	-0.00513
	(-2.27)	(-0.34)	(-2.06)	(0.21)	(-2.56)	(-0.87)
logpop	0.571**	0.747+	0.853***	0.693***	0.796***	0.953***
	(3.24)	(1.71)	(8.02)	(4.09)	(8.52)	(6.45)
senderomuertoslag	0.0407**	0.0506	0.0211***	0.0227	0.0342***	0.0201
	(3.06)	(0.73)	(3.30)	(0.64)	(4.72)	(0.74)
mrtadeathslag	-17.61	0.862	0.186	0.0932	0.338+	0.337
	(-0.00)	(1.23)	(0.51)	(0.21)	(1.94)	(1.22)
statedeathslag	0.0204	0.313*	-0.00607	0.0294	-0.00417	0.0250
	(1.58)	(2.45)	(-0.85)	(0.99)	(-0.55)	(1.21)
lfull	1.353***	0.872	1.271***	1.307**	0.974***	0.670+
	(3.69)	(0.83)	(6.81)	(3.15)	(5.80)	(1.79)
lcontested	1.018**	0.816	0.900***	0.0367	0.806***	0.111
	(2.70)	(0.83)	(4.64)	(0.07)	(4.94)	(0.32)
_cons	-9.850***	-17.76**	-16.23***	-13.27***	-13.90***	-13.61***
	(-4.49)	(-2.69)	(-9.35)	(-5.39)	(-10.33)	(-7.03)
ln_r	0.361	-0.282	-0.276*	0.539	-0.174	0.573*
	(1.23)	(-0.67)	(-2.38)	(1.56)	(-1.57)	(2.54)
ln_s	-0.00541	-2.110***	-0.990***	0.119	-0.733**	-0.502
	(-0.01)	(-3.38)	(-4.23)	(0.14)	(-3.29)	(-1.09)
N	9191	3314	9191	3314	9191	3314

Appendix D7. Main Model with year fixed effects

To address concerns about the crudeness of our temporal dummy variables for state drug and counter-insurgency policy, we re-run the analyses for Hypothesis 3 with dummy variables for all years.[xiv] The armed forces model for the full period does not converge, but in all of the other models (shown below) we find that our core result – that the coca variable is associated with state violence, and that the association is especially strong in the post-1992 period – holds even with the inclusion of these dummy variables. This suggests that something about the entire period, rather than a specific year, is at work.

	state 1980-1995	state 1992-1995	state 1980-1995	armed_forces 1992-1995
coca	2.6151***	5.3399***	3.0680***	5.9020***
	(0.4684)	(1.3776)	(0.8389)	(1.8923)
logaltitude	1.3356***	1.2295*	1.5139***	1.1355
	(0.0866)	(0.1039)	(0.1249)	(0.1316)
capital	1.5338**	2.0799***	1.4438*	3.1408***
	(0.2160)	(0.4227)	(0.2404)	(0.8773)
electricityperc	0.9950+	1.0043	0.9895**	0.9955
	(0.0028)	(0.0038)	(0.0034)	(0.0051)
logpop	2.0482***	2.1070***	2.1731***	1.8664***
	(0.1227)	(0.1793)	(0.1565)	(0.2119)
senderomuerto slag	1.0253***	1.0308+	1.0256***	1.0323
	(0.0048)	(0.0165)	(0.0050)	(0.0236)
statedeathslag	1.0104*	1.0473**	1.0059	1.0483*
	(0.0044)	(0.0162)	(0.0048)	(0.0240)
lfull	1.8731***	1.7131*	1.9103***	2.0835*
	(0.2298)	(0.4355)	(0.2656)	(0.6440)
lcontested	1.6373***	1.1357	1.9572***	1.5438
	(0.1933)	(0.2794)	(0.2589)	(0.4593)
fujimori	1.6917	1.0000	6.6210***	
	(0.7075)	(.)	(2.3759)	
garcia	7.2158***	1.0000	6.9284***	
	(2.5628)	(.)	(2.4732)	
1981.ano	1.0000		1.0000	
	(.)		(.)	
1982.ano	2.1235+		2.1312+	
	(0.8665)		(0.8698)	
1983.ano	6.8410***		7.0770***	
	(2.4664)		(2.5516)	
1984.ano	8.0870***		8.2899***	
	(2.8948)		(2.9698)	
1985.ano	3.5199***		3.6761***	
	(1.3326)		(1.3904)	
1986.ano	0.6022**		0.6153*	

	(0.1158)		(0.1194)	
1987.ano	0.4755***		0.5001**	
	(0.1005)		(0.1068)	
1988.ano	0.5890**		0.6143*	
	(0.1149)		(0.1212)	
1989.ano	1.6076**		1.6694**	
	(0.2515)		(0.2644)	
1990.ano	1.0000		1.0000	
	(.)		(.)	
1991.ano	4.0237***		1.0000	
	(1.1392)		(.)	
1992.ano	2.9854***	1.0000		1.0000
	(0.8496)	(.)		(.)
1993.ano	2.4570**	0.6010*		0.5779*
	(0.7124)	(0.1248)		(0.1443)
1994.ano	1.7396+	0.4636**		0.3502***
	(0.5467)	(0.1104)		(0.1111)
1995.ano	1.0000	0.2640***		0.2494***
	(.)	(0.0757)		(0.0896)
ln_r	0.5570***	2.0958+	0.5137***	1.0408
	(0.0423)	(0.8150)	(0.0413)	(0.2914)
ln_s	0.4307***	7.0193**	0.3785***	1.2612
	(0.0674)	(5.2235)	(0.0634)	(1.1962)
N	12505.0000	3314.0000	9191.0000	3314.0000

Exponentiated coefficients; *t* statistics in parentheses
+ p<0.10, * p<0.05, *** p<0.01

Appendix E: Qualitative evidence on corruption by Peru's armed actors

In Section 6 of the paper, we tentatively explored the effects of corruption on state violence in Peru's drug conflict. Yet we lacked systematic quantitative data on how corruption varied across time and space that we could use in a direct test of its effects. On the state side, while Jaskoski (2013) points to generalized and centralized corruption in the military after 1992, information about earlier periods of the conflict is more fragmented and anecdotal. On the insurgent side, the relationship of Sendero and the MRTA to the drug trade varied quite a bit across time and space, undermining simple and dichotomous codings such as that found in Weinstein (2005).

Drawing on the case studies in Volume 5 of the CVR report as well as other sources, this Appendix provides qualitative information about the engagement of the armed actors in Peru's conflict in the drug trade. Highlighting the diverse forms and unstable nature of corruption, it provides further evidence of the difficulty to draw inferences in this case. It thus confirms that our discussion of the corruption channel should be seen primarily as a theory building exercise, and supports our call for future scholarship to explore this issue in more detail.

Sendero's Corruption

Sendero's relation to the drug trade was complex. Its rhetoric emphasized the protection of growers from state eradication, crop substitution, and the depredations of traffickers such as the manipulation of weights and measures in their transactions with growers. Thus, for example, it supported a coca growers' strike in Aucayacu as early as 1981 (vol.5 p.189) and subsequently visited villages in the area to advocate for coca. (ibid; 190) It also repeatedly attacked eradication teams in the field (vol.5 p.487), as well as their offices in urban areas.

But Sendero also allied with traffickers to oust the police from drug producing regions and thus protect not only coca growing but higher stages in production and trafficking. In the town of Tocache in the UHV, where Sendero held open control from 1986-1992, its entry came with a proposal to traffickers to support its efforts to remove the police, protect against crime, and protect drug flights from the town airport. Traffickers seem to have accepted this offer, providing SL with arms from Colombia to beef up its force. (vol. 5 p.193) Traffickers sometimes used SL for protection against rivals as well, showing the fragmentation present in drug production even at the height of the boom, as in the case of Uchiza in 1989. (vol. 5 pp.195-6)

This shared interest in removing the police and keeping drug flights active generated initial alliances, though these became frayed when traffickers did not wish to dissolve their private security as SL demanded, and the result was significant bloodshed between the two. (vol.5 pp.191-2) This conflict sometimes led traffickers to call on the army to protect them from SL, as in the case of the town of Paraíso in October 1987. (Vol. 1 p.100)

Sendero also taxed traffickers for the use of rural airstrips that it controlled (vol. 5 p.190), and imposed transaction taxes on coca leaves and coca paste (*pasta básica*). Especially as coca prices fell, Sendero's impositions on the drug market increased. It began to tax growers, as much as 50% of their production, and to take over coca harvesting on land abandoned by those who refused to pay. At one stage late in the conflict (vol.5 p.198) Sendero even attempted to set itself up as a producer, operating *fundos* of coca fields and sending drugs to Colombia on its own behalf. At times, Sendero sought to control the market for coca leaves and pasta básica, setting prices, requiring its approval for transactions, and even trying to ban export as a way to drive up prices. (vol.5 p.196) It sought to enforce these interventions with violence. (vol. 5 p.196) This increasing attempt to regulate drug markets led to conflict with traffickers. In the Huallaga, the most notable collapse of relations between SL and traffickers was the case of Vaticano, who controlled the drug trade in the central valley. After relations with SL turned sour, he built up his own army including former MRTA members, and was able to establish sufficient control over the town of Campanilla to keep SL out, relying in part on the assistance of the army. (vol.5 pp.224-5)

These diverse dynamics make it hard to generalize about patterns of corruption. We can see the extent of complexity by drawing on Volume 5, Chapter 11 of the CVR, which provides a case study of violence and drug trafficking in two provinces of Ucayali department, which can be used to see how these relations unfolded over time in a single location. Drug traffickers entered this region at the end of the 1980s as they were displaced from the UHV (231)[xv], and the armed presence of both Sendero and the MRTA followed.[xvi] As coca production spread into Padre Abad, the state responded with manual eradication (232, fn 467) and attempts to promote crop substitution. The former prompted immediate resentment among coca growers, while the latter foundered with the 1988 onset of hyperinflation, which wiped out the value of credit issued to growers to take up non-coca products. A February 1989 protest by coca growers saw massive police repression with 8 killed and many disappearances (233) and this led both Sendero and the MRTA to increase their armed actions in the region, and to the declaration of a state of emergency by President García in June 1989.

Between 1988 and 1992, Sendero was heavily involved in the drug trade in Padre Abad. It attacked eradictors and crop substitution agents (237), initially attracting the support of small-scale growers who believed Sendero’s promises that it would protect them against both the abusive practices of traffickers (256) and the state. But Sendero began regulating the drug trade, acting as a mediator between growers and traffickers and regulating the prices of commodity exchange in the production chain. (245) The group also established a clandestine airstrip and charged traffickers fees for its use. As Sendero’s fees rose, tensions with traffickers did as well, and similarly when Sendero began to collect fees from growers as well, its popular support was undermined. Yet the group exercised “nearly absolute control” of Padre Abad for a two year period before being forced to retreat due to a combination of military operations, deaths of some of its cadre due to what the CVR (246) calls “direct problems with traffickers” and the corruption of others, who fled with the proceeds from fees collected rather than turning them in to the organization.

As it spread into Coronel Portillo province starting in 1989, Sendero emphasized establishing a presence in remote regions where coca growing had started. The insurgents demanded half of the proceeds from coca growers, which met significant resistance from the local population. But revenue the group gained was (245) used to sustain its armed campaign at a national level. Sendero’s deep involvement in the drug trade also generated significant corruption within the group, including the skimming of revenue by individual commanders. (245) State operations in Ucayali in this period were split between the army and the navy (247), but both engaged in a "scorched earth" (247) strategy against insurgents that emphasized the bombing and burning of villages. Thus, as elsewhere in the conflict, there was little to no collusion between state forces and insurgents over the drug trade.

In all, this case fits with the broader pattern sketched above. Sendero's relation to the drug trade took many forms. There is a common element of an effort to extract revenue from the drug trade; Sendero never turned to eradication or to efforts to forbid drug production, as the MRTA initially did.[xvii] But relations seem to have been tense, unstable, and fairly fluid, even as Sendero consistently emphasized extraction at the expense of its political project of popular struggle. Yet few of the interactions described in the previous paragraphs are the sort of corruption that might be expected to generate stable bargains and reduced violence.

Corruption of state forces

In some times and places, military and state authorities were fundamentally corrupted. One example was the police charged with counter-narcotics operations in the UHV in the early 1980s, which was described as being characterized by "generalized and systematic corruption" (Vol.5 p.188; vol.5 pp.495-7) A second instance can be found in Uchiza in early 1992. Here, traffickers paid two thousand dollars per flight from the counter-insurgency base airstrip, with money divided among those serving at the base, in addition to payments to the mayor and the governor. (Vol. 2, p.242)

More broadly, military corruption in the UHV during the 1990s was sufficiently widespread and stable that officers paid to be stationed there. (ibid; see also Jaskoski 2013, 26-7) Eighteen airstrips were located near counter-subversive bases (Vol. 5 pp.500-1), and one of these alone was used for some 280 drug flights. As the CVR shows (vol.5 Chapter 23 pp.501ff) and Jaskoski (2013) confirms, the corruption of the drug trade was highly centralized; the trafficker Vaticano denounced National Intelligence chief Vladimiro Montesinos as being on his monthly payroll for fifty thousand dollars. (ibid., 502) and other notable examples of corruption came to light in a series of scandals. Yet even these varied in form (502-3) from units attacking traffickers to seize their products to army protection of clandestine airstrips, assistance in prison escapes for a jailed trafficker, and the loan of troops and weapons to provide security. Thus, as in the case of Sendero described above, the corruption of the armed forces contained a great deal of diversity in relations with traffickers.

Yet even in the late 1980s as military presence in the UHV rose sharply, there was variation in corruption. In 1989, under General Arciniega, the Army was willing to reach agreement with traffickers that did not work with SL, so that it could focus on those that did. (Vol. 2 p.202) On the other hand, in Ucayali during the same time period, the Navy "combatió por igual toda la actividad cocalera" (ibid; 205) based on the belief that the extent of subversive activity was correlated with the size of the drug economy. In Uchiza, meanwhile, the high fees charged by police to protect drug flights prompted traffickers to support Sendero's 1989 attack on the city police headquarters. (vol.5 p.498)

Setting these two local dynamics side by side shows that even at the same time, state drug policy had a patchwork quality in terms of the nature of corruption which complicates our ability to accurately analyze the relationship between corruption and violence in the Peruvian case and tends to support our claim that despite prominent cases of top-controlled corruption like those involving Vladimiro Montesinos, it can still be characterized in general as decentralized. Scholars interested in investigating these relationships in more detail will have to identify deeper case studies of specific local dynamics, or track down evidence of systematic variation in order to build on our preliminary efforts in this vein.

Table E1. State violence as a function of Sendero lagged violence using per capita rates

	Per capita victims of state violence Non coca	Per capita victims of state violence Coca
logaltitude	0.1211***	0.0066
	0.0218	0.0321
capital	-0.0714	-0.1112
	0.0449	0.1228
electrification	-0.0054***	-0.0038
	0.0012	0.0028
logpop	0.0679***	0.0051
	0.0191	0.0509
Lag (SL killings)	0.0076+	0.0125*
	0.004	0.0058
Lag (MRTA killings)	0.1571*	0.0552
	0.0493	0.0701
Lag (State killings)	0.0044	0.0154+
	0.0047	0.0091
lfull	0.5348***	0.3079***
	0.0863	0.0902
lcontested	0.1635***	-0.2567+
	0.0467	0.1348
fujimori	-0.4117***	0.4642***
	0.0815	0.129
garcia	-0.1924*	0.3817**
	0.0786	0.1271
_cons	-5.4885***	-4.4443***
	0.2068	0.4739
N	12132	373
r2	0.08	0.07

Table E2: State violence as a function of Sendero lagged violence with alternative (coca2) measure of drug production

	Coca2=0	Coca2=1
Log(altitude)	0.3292***	0.0558
	0.0724	0.1945
Capital	0.4143**	0.2100
	0.1526	0.3606
Electrification	-0.0047	-0.0111
	0.0028	0.0097
Log(population)	0.7097***	1.2496***
	0.0639	0.2048
Lag Sendero killings	0.0282***	0.0201**
	0.0058	0.0067
Lag MRTA killings	0.2554*	0.1019
	(0.1256)	(0.2689)
Lag state killings	0.0003	0.0195**
	0.0053	0.0076
Lfull	0.9328***	0.8675***
	0.1319	0.2548
Lcontested	0.5753***	0.1822
	0.1251	0.3025
Fujimori	-0.2012	1.0643***
	0.1175	0.2951
Garcia	0.2554**	0.8492**
	0.1050	0.2920
Constant	-12.1965***	-15.1662***
	0.9262	1.9982
Wald Chi2	341.79	115.45
P>Chi2	0.0000	0.0000
Log likelihood	-3502.2033	-547.23139
N	11640	865

Table E3: State violence as a function of Sendero lagged violence with alternative poverty measure

	no coca	coca
	state violence	**state violence**
logaltitude	0.2907***	0.108
	0.0655	0.2683
capital	0.4580***	-0.3267
	0.1421	0.4947
Poor district	0.5098***	-0.2836
	0.1163	0.3012
logpop	0.6980***	1.1715***
	0.054	0.2608
Lag (SL killings)	0.0235***	0.0032
	0.0046	0.0264
Lag (MRTA killings)	0.2591	0.2626
	0.1190	0.3826
Lag (state killings)	0.0037	0.0264
	0.0047	0.0205
lfull	0.9123***	0.6183*
	0.1269	0.2829
lcontested	0.6043***	0.4015
	0.1171	0.4363
fujimori	-0.1323	2.2355***
	0.1144	0.5600
garcia	0.1720+	2.0672***
	0.0983	0.5402
Constant	-12.1781***	-15.4206***
	0.0985	0.5404
ln_r	-0.6029***	-0.2799
	0.0776	0.3438
ln_s	-0.8341***	0.0396
	0.1596	0.8285
N	12427	398

Table E4: State violence as a function of Sendero lagged violence excluding presidential election years

	no coca	coca
	Victims of acts	Victims of acts
logaltitude	0.3123***	-0.1289
	0.0702	0.3125
capital	0.4108**	0.2368
	0.158	0.5734
Electrification	-0.0080**	-0.0247+
	0.0029	0.0147
logpop	0.7728***	1.2626***
	0.0658	0.3031
Lag (SL killings)	0.0481***	0.0075
	0.0070	0.0293
Lag (MRTA killings)	0.3702+	0.2158
	0.2107	0.3612
Lag (state killings)	0.0200***	0.0420+
	0.006	0.0241
lfull	1.0232***	0.6315+
	0.1438	0.3509
lcontested	0.5023***	0.3867
	0.1316	0.4761
fujimori	-0.3269*	2.7231***
	0.1247	0.7583
garcia	0.056	2.2321**
	0.1248	0.7575
_cons	-12.5158***	-15.1299***
	0.9192	2.9077
ln_r	-0.5510***	-0.2725
	0.0859	0.3380
ln_s	-0.6347**	0.0456
	0.1968	0.8021
N	10521	346

[i] Estimations of coca cultivation based on satellite data do not start until the 2000 in Peru under UNODC's Global Monitoring of Illicit Crops.

[ii] These districts include: Daniel Alomías Robles, Luyando, Monzón, Jircán, Rupa-Rupa, Cochabamba, Cholón, Hermilio Valdizán, Mariano Damaso Beraun, José Crespo y Castillo, Churubamba, Chinchao and Marias in the department of Huanuco, Huánuco; Pataz in La Libertad; and Tocache, Uchiza, Campanilla, Nuevo Progreso, Shunte y Pólvora in the department of San Martín.

[iii] Central and Lower Huallaga includes the districts of Alto Biavo, Bajo Biavo, Bellavista, Huallaga, San Pablo, San Rafael, Agua Blanca, San José de Sisa, San Martín, Santa Rosa, Shatoja, Alto Saposoa, El Eslabón, Piscoyacu, Sacanche, Saposoa, Tingo de Saposoa, Huicungo, Juanjuí, Pachiza, Pajarillo, Buenos Aires, Caspisapa, Picota, Pilluana, Pucacaca, San Cristóbal, San Hilarión, Shamboyacu, Tingo de Ponasa, Tres Unidos in the department of San Martin. Apurimac River Valley (now VRAEM) includes Masamari, Pangoa, Río Tambo in Junin; Ayahuanco, Llochegua, Sivia, Ayna, Santa Rosa, San Miguel, Anco, Chungui in Ayacucho; Pichari, Quimbiri, Vilcabamba in Cusco. La Convención includes the districts of Quellouno, Ocobamba, Huayopata, Maranura, Santa Teresa (Norte), Echerate (Sur), Vilcabamba (Noreste) in the department of Cusco.

[iv] Measures of poverty in the Peruvian census have several limitations. We discuss these in Appendix D4, and show there that our results hold when using them despite these concerns.

[v] Electricity provision is a public good normally provided by the state, so one might wonder it is an indicator of poverty rather than state capacity. Yet the fact that many districts have small but non-zero levels of electricity provision shows that the state's electric grid reaches the district, and thus that low electrification is not a function of state absence. Moreover, even if electricity provision reflects state capacity, its presence allows for economic development, and thus it is reasonable to use electrification as a proxy for local economic conditions.

[vi] The omitted category is the Belaúnde presidency (1980-1985). The fact that presidential terms began in July might raise concerns about the coding of the presidential term dummy variables for these transition years. Results of all the models below are robust to excluding presidential election years from the analyses (Appendix D5).

[vii] Some of the items on this list appear in León (2012).

[viii]Due to a Stata version problem, this rare events analysis does not include the MRTA variable. We hope to resolve this issue as we continue to work on the paper.

[ix] This analysis was conducted using the MatchIt package in R. Thanks to [REDACTED] for consultation about this portion of our analysis.

[x] Here we include two additional dependent variables that capture victim characteristics as coded by the CVR (2003): members of organizations and leaders. Descriptions of how those variables are defined and coded can be found in the CVR statistical appendix.

[xi] To be precise, the census question does not ask people their income for a specific time period. In a separate question, the time period is asked and the most common period cited is monthly. Higher income thresholds for this proxy for the poverty rate generate insufficient variation across cases to be of use.

[xii] The census has ten response categories for this question: (1) other; (2) working; (3) had a job but currently unemployed; (4) unpaid assistance ("ayuda sin pago"); (5) seeking work, having worked previously; (6) seeking work for the first time; (7) taking care of the house; (8) studying; (9) living from a pension and not working; (10) living from income

and not working. We use the percentage who choose option (2) as our measure of employment.

[xiii] Note that since the 1993 measure uses employment data, values for 'employed' falling below the national mean receive a score of 1.

[xiv] While our full dataset goes up to 2000, the lfull and lcontested variables are only coded for the period 1980-1995. We exclude the MRTA deaths variable as models including it only converge in two models (the models after 1992) and the results are the same.

[xv] Unless noted, all page numbers in this section come from Volume 5 of the CVR report.

[xvi] Both Sendero and the MRTA had clandestine organizers present in the region much earlier, but their armed presence did not begin until the late 1980s.

[xvii] Unlike Sendero, the MRTA apparently did not impose fees on traffickers or taxes on airstrips it controlled before 1990. (vol. 5 p.222) Nor did it initially emphasize the protection of coca growers, focusing instead on building relations with other peasant organizations and with organizations demanding regional autonomy, drawing on the long roots of MIR organization in the Huallaga. (225) Indeed, on its initial entry, the MRTA threatened those involved in the sale and production of coca. (ibid)