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RURAL WINDFALL OR A NEW RESOURCE CURSE? COCA, INCOME, AND CIVIL CONFLICT IN COLOMBIA

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Abstract—We study the consequences of an exogenous upsurge in coca prices and cultivation in Colombia, where most coca leaf is now harvested. This shift generated only modest economic gains in rural areas, primarily in the form of increased self-employment earnings and increased labor supply by teenage boys. The rural areas that saw accelerated coca production subsequently became considerably more violent, while urban areas were affected little. These findings are consistent with the view that the Colombian civil conflict is fueled by the financial opportunities that coca provides and that rent-seeking by combatants limits the economic gains from coca.

> If it weren't for the armed groups, I think we could reach a consensus on what the region needs to progress. But all the armed groups want is to control the economic question, and all are willing to massacre or murder or force people from their homes to win.
> —Gloria Cuartas, mayor of Apartadó (quoted in Kirk, 2003)

I. Introduction

NOWHERE is the interest in regional economic conditions more acute than in war-torn nations or regions embroiled in civil conflict. Perhaps not coincidentally, many such areas appear to have local economies that depend to a large extent on natural resources, especially those related to illegal economic activities or products for which there is a black market. Examples include the drug trade in Latin America and Afghanistan and so-called blood diamonds in Africa. Even legal extraction activities, such as timber harvesting and oil mining, have been associated with social breakdown (Ross, 2001). The concentration of extraction activities in conflict zones raises the question of whether this association is causal. Although an increase in resource income may reduce poverty, thereby moderating combatants' desire to fight, natural resources also give the parties to a conflict something to fight over. Moreover, the income from resources provides financing for continued conflict.

The idea that resource wealth can be bad for development is sometimes known as the "resource curse" (for example, Sachs & Warner, 2000). Economic analyses of the resource curse typically focus on the possibilities of an export-induced Dutch disease and effects on government corruption or rent-seeking (for example, Sala-i-Martin & Subramanian, 2003; Hausmann & Rigobon, 2003). The effect of natural resources on the incidence and duration of civil wars provides a less-explored channel by which natural resources may have perverse effects. This channel features in a burgeoning political science literature, which includes empirical contributions by Collier, Hoeffler, & Soderbom (2004), Fearon (2004), and Ross (2004a). An antecedent in economics is the theoretical analysis by Grossman (1991). There is also some circumstantial evidence suggesting that illegal resources such as drugs increase the duration of civil conflicts (Ross, 2004b), but economists and political scientists have yet to produce evidence on this question from a compelling natural experiment.

In this paper we use a quasi-experimental research design to study the impact of demand shocks for illicit resources on rural economic conditions and civil conflict. The setting for our study is Colombia, a good laboratory since almost all of the cocaine consumed in North America and Europe comes from the Andean nations of Bolivia, Colombia, and Peru (United Nations, 2001). Moreover, we exploit a sharp change in the structure of the Andean drug industry: before 1994, most of the cocaine exported from Colombia was refined from coca leaf grown in Bolivia and Peru. Beginning in 1994, however, in response to increasingly effective air interdiction by American and local militaries, the so-called air bridge that ferried coca paste from growers to Colombian refiners was disrupted. In response, coca cultivation and paste production shifted to Colombia's countryside, where it eventually surpassed preinterdiction levels. We use this shift in an attempt to assess the consequences of the coca economy for Colombia's rural population.

The first question considered here is whether increased demand for coca affected economic conditions for the rural population in ways we can measure using survey data. In particular, the end of the air bridge is used to look at the claim that drug interdiction has substantial economic costs for rural producers (see, for example, Leons, 1997, and Chauvin, 1999). If interdiction is costly, then the post-air-bridge Colombian coca boom of the early 1990s should have had substantial economic benefits. We therefore look at effects on earnings, labor supply, and income, as well as

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child labor and school enrollment. Of course, coca cultivation per se may do little to enrich the cultivators, since—as with the relationship between the farmgate price of coffee and the beans we buy at Starbucks—the price of raw coca leaf makes up a small fraction of the price of cocaine (Alvarez, 1995). On the other hand, most estimates suggest cocaine plays a large enough role in the Colombian economy for changes in the demand for coca to have a perceptible economic effect.[1]

The widely observed association between illicit crops and civil strife raises the question of whether an increase in coca cultivation has an impact on violence by increasing the resources available to insurgent groups. The link with violence is especially relevant in Colombia, which has experienced some of the highest homicide rates in the world. This is in spite of substantial economic growth through most of the twentieth century and Colombia's status as one of the oldest democracies in Latin America. The effect of the drug trade on violence has been widely debated in Colombian policy circles (see, for example, Cardenas, 2001). While a link at first seems obvious, it bears emphasizing that the historical record is ambiguous. Marijuana became an important crop only in the 1960s and the cocaine trade began in the 1970s, with significant coca plantings appearing only in the 1990s (see, for example, Bagley, 1988). Yet violence and civil conflict, especially outside the major cities, have been major factors in Colombian political life since independence. During the period known as La Violencia (1948–1957), as many as 200,000 Colombians were killed (Winn, 1999). Clearly, cocaine cannot be blamed for starting this conflict, though it may play a role in perpetuating it.

Weighing in favor of a link between the Colombian drug trade and violence is the fact that some of the more recent violence is the work of drug cartels or individuals operating on their behalf. Thus, homicide rates peaked in the late 1980s and early 1990s, when the cartel leadership rebelled against extradition efforts. Probably more importantly, the major Colombian guerrilla groups, especially the Colombian Revolutionary Armed Forces (FARC) and the National Liberation Army (ELN), are widely believed to derive substantial income by taxing drug proceeds, as do illegal self-defense groups or paramilitaries (Rangel, 2000; Rabasa & Chalk, 2001; Villalon, 2004).[2]

Although the evidence is not seamless, two broad features of our findings tend to support the view that coca fuels Colombia's seemingly interminable civil conflict, while generating few economic benefits for local residents. First, coca does not boost earnings in an entire growing region, though it is associated with increased self-employment income for those already active in this sector. This is consistent with anecdotal evidence that the economic benefits of coca growing are largely taxed away by combatants or otherwise dissipated through nonproductive activities. Second, in spite of the fact that income and hours worked increased for some groups, violence also increased in regions where coca cultivation increased. This runs counter to the findings in Miguel, Satyanath, and Sergenti (2004), who link improvements in economic conditions generated by rainfall to decreased civil conflict in Africa, but appears consistent with economic theories of rent-seeking behavior by combatants (for example, Grossman, 1991; Collier & Hoeffler, 2004).[3]

The paper is organized as follows. The next section provides additional background. Section III outlines the approach we used to divide Colombia into coca-growing and nongrowing regions for the purposes of our within-country analysis. Section IV discusses estimates of the effect of coca growing on rural economic conditions and section V presents the mortality estimates. Section VI summarizes and interprets the results.

II. Institutional Background and Economic Framework

Until the early 1990s, coca was mainly harvested in Bolivia and Peru, after which most cultivation moved to Colombia.[4] Whether in Bolivia, Peru, or Colombia, coca is typically grown in thousands of small peasant holdings. Harvested coca leaves are dried by farmers and sold to entrepreneurs who make them into coca paste, a simple chemical process that takes a few days. Coca paste has about one-hundredth the volume of coca leaves, and the transition from leaf to paste is where most of the weight reduction in cocaine production occurs. The next step in coca processing is to make coca base, a somewhat more complicated chemical process. Finally, cocaine hydrochloride is refined from coca base, a chemical process that often occurs in towns or cities. Street cocaine is made by diluting cocaine hydrochloride with sugar and baking soda, usually in the consuming country.

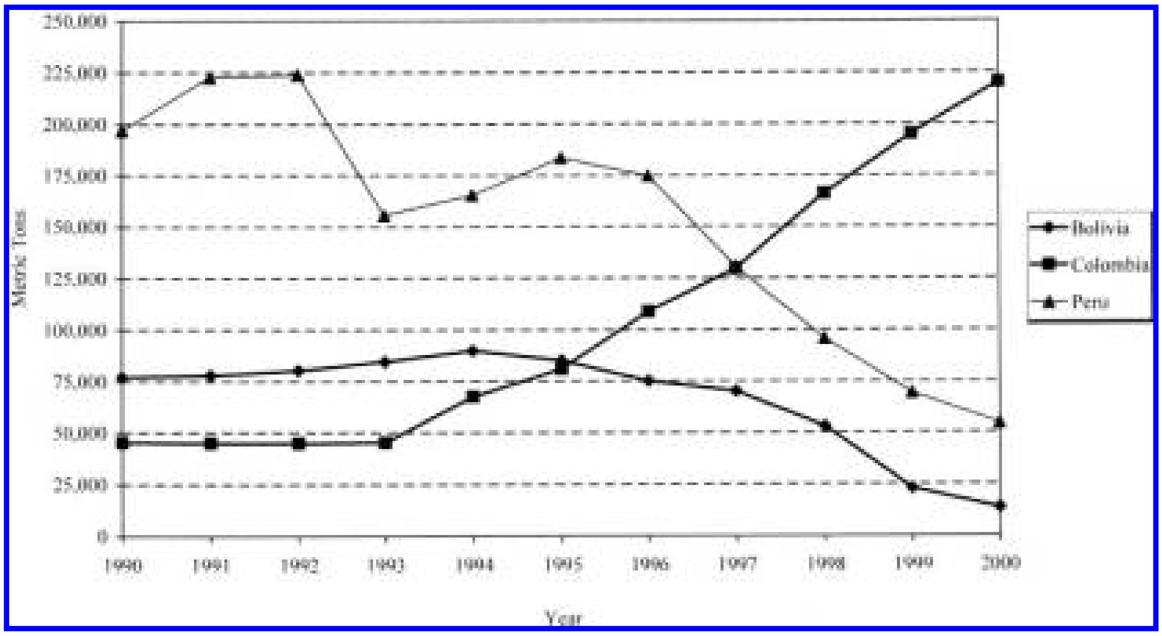
[1] For example, Steiner (1998) estimates total Colombian income from illegal drugs at 4%–6% of GDP in the first half of the 1990s. See also Thoumi (2002).

[2] Guerrilla and paramilitary groups do not refer to control of the drug trade as a primary goal of the conflict. Rather, the rhetoric on both sides concerns security for various constituencies and social justice. For instance, the FARC was initially a farmers' defense coalition that formed in the 1950s to resist the minority conservative government. Later, the FARC established ties with the Colombian Communist Party. The ELN was created by university students and inspired by Che Guevara and the Cuban revolution. The AUC (a paramilitary group) ostensibly protects the interests of ranchers, farmers, and other landowners. The income from taxing drug proceeds appears to fund political action, maintenance of a precarious social security system for members and their families, occasional work on infrastructure, and combat activity including weapons purchases (Rangel, 2000).

[3] Díaz and Sánchez (2004) offer a recent exploration of the coca-conflict nexus in Colombia, arguing that conflict causes coca and not vice versa. But their spatial-correlations research design does not exploit exogenous shifts. In related studies, Guidolin and La Ferrara (2005) and Pshisva and Suarez (2004) put conflict and kidnappings on the right-hand side of a firm-level investment equation. A number of studies have also used cross-country data to address the association between social conflict, institutions, and growth (for example, Rodrik, 1999), and natural resources, institutions, and growth (Mehlum, Moene, & Torvik, 2006).

[4] This section draws on Whynes (1992) and Thoumi (1995).

FIGURE 1.—PRODUCTION OF COCA LEAF IN COLOMBIA, PERU, AND BOLIVIA, 1990–2000



Note: Data from United Nations (2001).

While Colombia has almost always been the principal exporter of refined cocaine, until fairly recently little coca was grown there. Colombian middlemen and exporters operated by importing coca paste (or coca base) from Bolivia and Peru, specializing in refining and distributing cocaine hydrochloride (that is, cocaine). In the early 1990s, the drug industry changed in response to a change in emphasis in U.S. and producer-country enforcement policies. In April 1992, after Peruvian president Fujimori's so-called self-coup, the Peruvian military began aggressively targeting jungle airstrips and small planes suspected of carrying coca paste as part of a general process of militarization of the drug war (Zirnite, 1998). Colombia followed suit in 1994 with a similar shoot-down policy for planes ferrying paste from both Peru and Bolivia. U.S. policy moved in tandem with Presidential Decision Directive 14 in November 1993, which shifted U.S. interdiction away from Caribbean transit zones like Bermuda toward an attempt to stop cocaine production in the Andes. The disruption of the air bridge ferrying coca paste into Colombia was a key part of this effort.[5]

The militarization of the drug war and disruption of the air bridge does not appear to have reduced the supply of cocaine (see, for example, Rabasa & Chalk, 2001). It did, however, lead to a marked shift in the organization of the drug industry among producer countries. This can be seen in figure 1, which uses data from a United Nations (2001) drug report to show the change in the locus of production of dry coca leaf from Peru and Bolivia to Colombia. While Bolivian production was flat in the early 1990s, it started to decline in 1994. Peruvian production fell sharply from 1992 to 1993, followed by a sharp and steady increase in Colombian production from 1993 to 1994 and continuing thereafter. Part of this increase appears to have come from increased cultivation and part from improved yields. Colombian production continued to grow thereafter, as did the Colombian share of total production. Other figures in United Nations (2001) show that by 1997, potential coca production in Colombia (in other words, before crop eradication) exceeded that in Peru.

A. *Economic Framework*

We see the end of the air bridge as initiating an exogenous fall in the price of coca (leaf, paste, or base) in the traditional producer nations of Bolivia and Peru, while causing a price increase in Colombia. The price in traditional growing countries fell when coca could no longer be shipped to Colombian refineries and distributors. Peruvian and Bolivian growers have no competitive export channels of their own since they have no Caribbean ports and because their foreign distribution networks are not well developed. At the same time, the price of coca grown in Colombia increased when drug middlemen and entrepreneurs tried to elicit new and more accessible supplies. Farmers and potential farmers responded to the increase in the price of coca by growing more of it, a response that very likely accounts for the

[5] The Peruvian and Colombian shoot-down policies can be seen as a response to U.S. pressure. Militarization of the drug war began as part of first President Bush's "Andean Strategy" in 1990, with a program of military, economic, and law-enforcement assistance for Andean nations in FY 1990–1994. Initially, however, this effort met with little sympathy in the region (Washington Office on Latin America, 1991). Late 1992 and 1993 marked the beginning of a period of independent interdiction efforts and sharply increased cooperation by producer nations.

pattern in figure 1, though noneconomic factors may have been at work as well.

Did the end of the air bridge really change coca prices in the manner described above? Although we do not have a reliable time series of coca prices by producer country, anecdotal evidence supports this description of the coca market in the mid-1990s. For example, Zirnite (1998, p. 171) quotes the regional U.S. military commander testifying to Congress in 1996 that "the so-called air bridge between Peru and Colombia saw a greater than 50% temporary reduction of flights," and that consequently, ". . . there was a glut of coca base on the market and the price of the product being shipped fell 50 percent overall and by as much as 80 percent in some areas." On the Colombian side, data reported in Uribe (1997, p. 62) for the department of Guaviare show that the price of base more than doubled from 1992 to 1994. Journalistic accounts similarly point to an increase in prices in Colombia (for example, Villalon, 2004).[6]

This description suggests a number of channels through which increased coca cultivation might affect economic conditions and the level of rural violence in coca-growing regions. The increase in coca prices presumably made coca farmers better off, with possible aggregate regional effects of the sort documented by Black, McKinnish, and Sanders (2005) in the Appalachian coal-mining region and by Carrington (1996) in Alaska. Increased prices for coca production generated new sources of revenue for taxation. Because the central government is weak in the Colombian countryside, these opportunities most likely benefited guerrillas and paramilitaries. Of course, if coca taxes are too high, then there is no incentive to produce. Taxes were imposed not only at the point of sale, however, but also through kidnapping, extortion, and based on the guerrilla's "economic census," a sort of partisan's tax return (Rangel, 2000, p. 588).[7] This tax and extortion system may have transferred a large fraction of the economic benefits of coca production to combatants, while still leaving coca production more attractive than alternative activities. In addition, to the extent that coca finances a disruptive civil conflict, increased coca production may have reduced the overall level of economic activity.[8]

III. Classification of Regions

Our research design exploits the fact that changes in the drug industry in the early 1990s probably had a disproportionate effect on Colombian departments which, by virtue of climate and soil conditions, politics, or infrastructure, were hospitable to the cultivation of coca plants and the production of coca paste. This naturally raises the question of how to classify departments or regions as potential coca growers and paste producers. The best candidates for future coca production seem likely to be departments with a preexisting coca presence. We identified baseline coca-growing departments using estimates for 1994 reported in Uribe (1997, p. 67). This source collects a number of international observers' estimates of hectares of coca bush under cultivation in Colombian departments. The reports summarized in the table are dated October 1994, so the data were presumably collected somewhat earlier. The nine departments that had at least 1,000 hectares under cultivation are Bolívar, Caquetá, Cauca, Guaviare, Meta, Nariño, Putumayo, Vaupés, and Vichada.[9]

In a second coding scheme, we expanded the definition of the growing region to include the five additional departments identified as growing on a satellite map in Perafán (1999, p. 11). This map is also dated 1994. The Perafán map adds the three northern departments of Cesar, Magdalena, and La Guajira, and the departments of Norte de Santander and Guainia. These five are also listed as growing regions in Uribe (1997), while all in the group of nine are identified as growing on Perafán's (1999) map. We refer to the expanded coding scheme as defining a "fourteen department growing region" and the five additional departments added to the nine growing departments to construct this region as "medium producers."

Our map, reproduced in the appendix, shows the nine-department growing region to be concentrated in the southern and eastern part of the country. Note, however, that not all southern or eastern departments grow large amounts of coca. For example, Amazonas, in the southeast corner, and Arauca, in the east, are not coded as growing departments in either scheme. The group of nine growing departments includes two, Meta and Caquetá, that were ceded to FARC control from 1998 to 2001 as part of an abortive peace effort. We refer to these two as the demilitarized zone (DMZ) and allow for separate DMZ effects in some of the empirical work. The five departments coded as medium producers are mostly in the northern part of the country, though one, Guainía, is in the far eastern region. As a final check on the results, we also distinguish all departments on the basis of previous guerrilla activity.

[6] A related question is why coca was not previously grown in large quantities in Colombia. The answer appears to be that Colombian coca farms were less productive; see page. 71 in Uribe (1997). Consistent with the increase in coca production, the production of Colombian coffee, which like coca is grown mainly in small plots, turned sharply downwards in the mid- to late 1990s, after increasing over most of the previous two decades (see http://www.dane.gov.co/inf_est/ena.html).

[7] About 85% and 65% of the FARC's and ELN's revenues, respectively, are estimated to come from drugs and extortion (Rangel, 2000, p. 585). While estimates of revenue sources for paramilitary groups do not exist, these groups are also widely believed to benefit from the drug trade. Grossman and Mejia (2005) develop a theoretical model of guerrilla involvement in drug production.

[8] A caveat here, relevant for our empirical strategy, is the possibility of general equilibrium effects in nongrowing regions. Examples include price effects and migration of labor. Although we cannot look at regional price variation, we found no evidence of coca-related migration patterns at the departmental level. Moreover, in the case of coca, extensive linkages with other consumer prices and farm input prices seem unlikely, given that cocaine is primarily for export and coca production requires few inputs other than labor.

[9] Black, McKinnish, and Sanders (2005) similarly identify countries affected by the coal boom and bust using preexisting production data.

TABLE 1.—FIRST STAGE FOR COCA CULTIVATION GROWTH

Treatment Group	Parameter	1994 to 1999 (1)	(2)	(3)	(4)	1994 to 2000 (5)	(6)	(7)	(8)
A. With DMZ Departments									
9-dept. growing	Intercept	735	207			506	292		
		(1,749)	(2,112)			(2,024)	(2,458)		
	Growing	7,554	8,082			8,748	8,961		
		(3,445)	(3,724)			(3,876)	(4,358)		
14-dept. growing	Intercept			207				292	
				(2,053)				(2,348)	
	Growing			6,100				6,127	
				(3,152)				(3,604)	
Linear	Intercept				1,658				2,147
					(1,708)				(1,989)
	Hectares				0.553				0.362
					(0.321)				(0.373)
Includes medium producers?		control	no	treated	yes	control	no	treated	yes
R^2		0.134	0.121	0.108	0.087	0.141	0.107	0.085	0.029
B. Without DMZ Departments									
9-dept. growing	Intercept	735	207			506	292		
		(1,845)	(4,198)			(2,074)	(4,911)		
	Growing	8,434	8,966			9,533	9,746		
		(3,883)	(4,198)			(4,364)	(4,911)		
14-dept. growing	Intercept			207				292	
				(2,115)				(2,414)	
	Growing			6,287				6,112	
				(3,400)				(3,879)	
Linear	Intercept				1,699				2,201
					(1,759)				(2,057)
	Hectares				0.697				0.328
					(0.398)				(0.466)
Includes medium producers?		control	no	treated	yes	control	no	treated	yes
R^2		0.140	0.125	0.106	0.091	0.141	0.105	0.079	0.017

Notes: The table reports estimates of the change in cocaine cultivation on 1994 levels for the 33 Colombia departments (states). The 1994 variable is the average of four measures from Uribe (1997, p. 67, cuadro II). The 1999 and 2000 data are police estimates, reported in Government of Colombia (2002).

To establish a "first-stage" relation for our division of Colombian departments into growing and nongrowing regions, we report the results of a regression of the growth in coca cultivation from 1994 to 1999 or 1994 to 2000 on an indicator for growing status in 1994. Growth is measured from a 1994 base since this is the year used to classify growing regions (as noted earlier, the 1994 data were probably collected earlier). The endpoint years of 1999 and 2000 are used because these are the years for which departmental cultivation figures are available. In any case, the change from 1994 to the end of the decade seems likely to provide a good summary of coca penetration in the relevant period.[10]

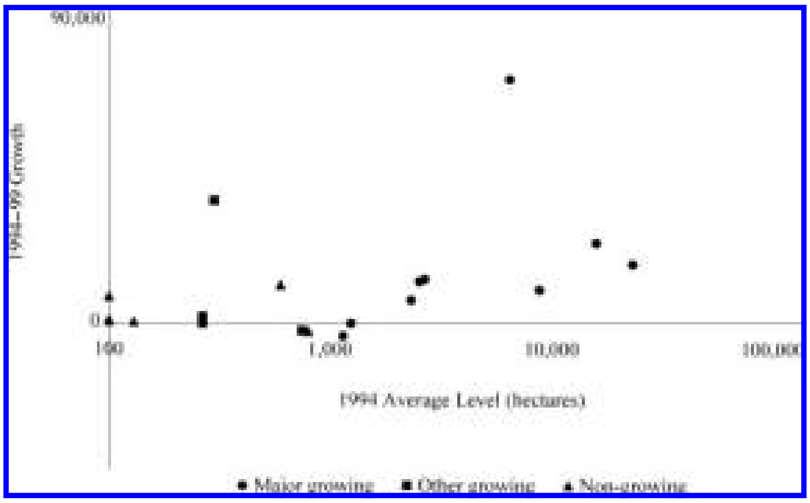
The first-stage results, summarized in table 1, show a strong correlation between coca growth and base-period growing status.[11] The estimates in column 1, panel A, indicate that cultivation grew by about 7,500 more hectares in the nine-department growing region than elsewhere, while the omission of medium producers leads to a slightly larger effect. Omission of the two DMZ departments leads to an even larger effect of almost 9,000 hectares, shown in column 2 of panel B. With or without DMZ departments, the estimates are significantly different from 0. The estimates in columns 5–8 show mostly larger effects when growth is measured through 2000 instead of 1999, with growing regions gaining 8,961 (s.e. = 4,358) hectares over the period in the sample without medium producers.[12] None of the intercept estimates are significantly different from 0, indicating essentially no growth in the departments with no initial production in 1994. Finally, estimates with growing status defined using the fourteen-department scheme, that is, moving the medium producers to the treated group, also

[10] The 1999–2000 data are from Colombia's antidrug agency, Direccion National de Estupefacientes (DANE, 2002), collected through the Illicit Crop Monitoring System (SIMCI, Sistema Integrado de Monitoreo de Cultivos Ilícitos). This system was implemented by the United Nations Office on Drugs and Crime with the logistical support of the Colombian antinarcotics Police (DIRAN) and in coordination with the DNE. The data are from satellite images and verification flights. Data for 2000 appear to be more complete than the 1999 data.

[11] Our use of the term first-stage in this context is motivated by the fact that, given consistent departmental time series data on coca production, we could use interactions between initial growing conditions and a post-air-bridge dummy as an instrumental variable for the effects of endogenous coca production on economic conditions and violence. In the absence of reliable data on the relevant endogenous variable, we focus below on the reduced-form regressions of economic and mortality outcomes on initial conditions/time interactions.

[12] Mean growth was about 2,800 hectares through 1999 and 2,900 through 2000. The 1994 mean for hectares under cultivation is about 2,100. The base mean was 7,155 in the nine-department growing region and 4,732 in the fourteen-department growing region. We estimate that in 1994, roughly 15%–19% of cultivated hectares were devoted to coca in the fourteen- and nine-department growing regions.

FIGURE 2.—COCA CULTIVATION: 1994–99 GROWTH AS A FUNCTION OF 1994 LEVELS



Note: Scales are logarithmic. The 100 hectare base group includes 100 or less.

show substantial growth in cultivation, but less than in the nine-department subset omitting medium producers (see columns 3 and 7). The fourteen-department scheme also generates a smaller intercept.

An interesting finding in this context, relevant for our choice of estimation strategy, is that dummies for the two growing regions do a better job of predicting coca growth than a linear predictor using base-period levels. Results from the linear parameterization can be seen in the last two rows of each panel of table 1. A visual representation of alternative parameterizations is presented in figure 2, which plots coca growth against base-period levels, using different symbols for the nongrowing region, the nine-department growing region, and the remaining growing region on a log scale. The two growing regions have much higher coca growth, but the relationship between base-period levels and growth rates is not especially linear. Although the best single predictor of coca growth is a dummy for the nine-department region, the empirical work below also uses the fourteen-region scheme since this turns out to balance pretreatment homicide rates better than the nine-department scheme and because the rural household survey is missing some growing departments.[13]

It is also worth emphasizing that the empirical first-stage is not meant to provide precise measure of the link between base-period levels and the growth in coca cultivation. If information on coca cultivation is subject to transitory classical measurement error, then the first-stage estimates reported here underestimate the impact of levels on growth. Moreover, producing regions may do a better job of hiding cultivated areas, leading to errors in satellite data that are negatively correlated with levels, further exacerbating attenuation bias. This suggests the first-stage estimates should be viewed as an underestimate, and may explain why categorical variables do a better job than linear terms at predicting cultivation growth. The division into growing categories is based on an average of rough estimates for 1994 from three different observers and sources. These data seem likely to capture the distinction between areas with substantial coca cultivation and areas with little or none even if the 1999–2000 satellite data are noisy.

A. *Descriptive Statistics by Region Type*

Not surprisingly, the growing departments are more rural and somewhat poorer than the rest of the country. This is apparent in the descriptive statistics in table 2, which compares growing and nongrowing regions along a number of dimensions. The comparison between growing and nongrowing regions is affected by the fact that the nongrowing region includes the three departments with Colombia's largest cities: the Bogotá capital district; Antioquia, which contains Medellín, an especially violent city; and Valle del Cauca, where Cali is located. To improve comparability with growing regions when comparing homicide rates, we tabulated statistics without these three departments. The Bogotá capital district, Antioquia, and Valle del Cauca are also dropped from the mortality analyses in order to avoid confounding with the secular decline in violence in big cities in the early 1990s. Only the Bogotá capital district is dropped from the analysis of rural labor markets and rural income.

[13] The ratio of coca hectares to noncoca hectares under cultivation grew by 0.33 from 1994 to 1999 and by 0.99 from 1994 to 2000 in the nine-department growing region. The corresponding statistics for the fourteen-department region are 0.18 and 0.62. Like the figures in levels, these are rough estimates, but they serve to identify regions with a strong and increasing coca presence (proportional coca cultivation declined slightly in the nongrowing region, by about 0.06). We also attempted to define growing regions based on climate and soil conditions using geographic information from Sánchez and Núñez (2000). In practice, this does not produce as strong a first-stage as a classification scheme based on 1994 levels, probably because coca grows under a broad range of conditions (Thoumi, 2002, p. 105).

TABLE 2.—DESCRIPTIVE STATISTICS BY REGION TYPE

Region Type	Department	Population 1993 (1)	% Urban 1993 (2)	Enrollment % Primary (3)	Enrollment % Secondary (4)	Homicide Rate (5)	Per Capita GSP 1993 (6)
Nongrowing	Santafé de Bogotá, DC	4,945,448	99.7	60.8	75.2	178.8	3,090
	Amazonas	37,764	50.4	56.3	33.2	35.3	1,744
	Antioquia	4,342,347	72.0	75.1	59.1	718.7	2,358
	Arauca	137,193	63.6	90.0	38.8	226.3	6,703
	Atlántico	1,667,500	93.7	68.2	67.9	65.2	1,633
	Boyacá	1,174,031	42.5	65.8	47.4	124.1	1,746
	Caldas	925,358	64.7	66.2	56.3	251.6	1,552
	Casanare	158,149	54.7	77.9	34.5	153.1	4,311
	Choco	338,160	38.5	66.6	32.0	65.6	845
	Córdoba	1,088,087	48.2	89.5	50.5	66.7	1,129
	Cundinamarca	1,658,698	54.7	71.7	55.3	126.4	2,112
	Huila	758,013	60.0	73.9	47.7	132.6	1,663
	Quindío	435,018	83.8	64.7	65.0	173.0	1,556
	Risaralda	744,974	81.3	66.1	59.1	303.4	1,633
	San Andres y Providencia	50,094	70.4	69.7	84.2	44.1	3,585
	Santander	1,598,688	68.9	68.9	53.3	192.7	2,111
	Sucre	624,463	67.1	96.6	51.3	38.5	892
	Tolima	1,150,080	60.7	71.9	55.5	140.9	1,626
	Valle del Cauca	3,333,150	85.3	71.9	62.6	311.5	2,382
	All 19 nongrowing	25,167,215	75.5	70.9	59.8	271.6	2,205
	Nongrowing (w/o Bogota, Antioquia, and Valle del Cauca)	12,546,270	64.5	72.7	54.0	140.7	1,756
9-dept. growing	Bolívar	1,439,291	68.6	75.4	50.4	41.1	1,628
	Cauca	979,231	36.7	73.8	36.3	170.0	1,065
	Guaviare	57,884	36.9	59.3	17.4	130.5	2,955
	Nariño	1,274,708	42.9	62.4	33.7	58.7	895
	Putumayo	204,309	34.6	75.4	28.0	170.7	1,042
	Vaupés	18,235	24.8	74.9	21.4	2.8	1,971
	Vichada	36,336	24.1	54.2	16.6	39.3	1,640
	All 9-dept. w/o DMZ	4,009,994	49.8	70.6	39.4	86.5	1,249
9-dept. DMZ	Caquetá	311,464	46.0	76.4	31.7	205.2	1,301
	Meta	561,121	64.0	72.9	52.2	204.7	2,190
	All DMZ	872,585	57.5	74.3	44.3	204.9	1,872
Medium producers	Cesar	729,634	62.9	82.1	50.7	146.2	1,354
	Guainía	13,491	30.4	43.3	17.1	45.3	1,871
	La Guajira	387,773	64.3	72.5	58.5	135.8	2,020
	Magdalena	882,571	64.0	67.7	41.0	70.8	1,272
	Norte de Santander	1,046,577	70.8	67.8	42.4	241.2	1,169
	All medium producers	3,060,046	66.0	71.7	45.6	150.6	1,354
	All departments	33,109,840	71.0	71.1	55.2	235.8	2,002

Notes: The table compares growing and nongrowing regions along several dimensions. The data are from *Colombia Estadistica, 1993–1997* and tabulations of vital statistics. Columns 1 and 2: from table 2.1.2. Column 2: % of pop. living in *cabecera municipal* (county seat). Columns 3 and 4: from tables 10.2.1 and 10.3.1. Column 3: primary enrollment divided by pop. aged 5–9 plus 60% of the pop. aged 10–14. Column 4: secondary enrollment divided by 40% of the pop. aged 10–14 plus pop. aged 15–19. Column 5: Homicide rates are for men aged 15–59, per 100,000. Column 6: 1993 per capita GSP in U.S. dollars.

Omitting the three big-city departments, the nongrowing population is 65% urban, in comparison with 50% urban in the nine department region minus the DMZ, 58% in the DMZ, and 66% urban in the five additional growing departments (medium producers). Although growing and nongrowing departments differ along the urban/rural dimension, they had similar primary school enrollment rates. Secondary school enrollment was somewhat lower in the growing regions, consistent with the fact that these regions are more rural and have lower per capita GSP. On the other hand, without the three big-city departments, the contrast in income levels between the growing and nongrowing regions is considerably reduced.

Homicide rates in the early 1990s were unusually high, even by Colombian standards. For example, the homicide rate reached a remarkable 719 per 100,000 (men aged 15–59) in Antioquia, mostly because of violence in Medellín, and was 272 overall in the nongrowing region. Without the big-city departments, homicide rates in the nongrowing region averaged 141 per 100,000. This can be compared with the rates of 87 in the nine-department growing region without the DMZ, 151 in the medium producers, and 205 in the DMZ. Thus, omission of big-city departments makes homicide rates somewhat more comparable across regions. Our working paper (Angrist & Kugler, 2005) discusses these homicide rates in a broader Latin American context.

B. *Potential Confounding Factors*

A potential complication for our analysis is the fact that many growing departments were previously centers of guerrilla activity. We would therefore like to distinguish growth in insurgent activity due to coca from a secular expansion in areas where central government control was already weak. In an attempt to distinguish coca-induced effects from the direct effect of a strong guerrilla presence, we estimated some models allowing for separate *trends* in regions (whether growing or not) with substantial preexisting guerrilla activity. On the other hand, *treatment effects* on violence might also be expected to be larger in areas where guerrillas already had a foothold, since a well-established guerrilla movement may be especially likely to benefit from resources from illegal activities. This possibility is therefore explored in a subset of the analyses as well.

A second consideration in the Colombian context is the large number of economic migrants who move to rural areas in search of work and especially the flow of refugees out of the countryside as a consequence of the civil conflict ("*poblacion desplazada*"). Both types of migration may induce selection bias in an analysis of economic circumstances in rural areas. As a partial check on this, we report results from samples with and without migrants. It is also worth noting that much war-related displacement occurs within departments, and that, according to the United Nations High Commission for Refugees (United Nations, 2002), the largest senders and receivers of displaced populations include both growing and nongrowing departments under our classification scheme. In addition, the phenomenon of internal displacement long predates the rise in coca production. In fact, a specification check that looks for growing-region/year interactions of the sort that might confound our analysis shows no growing/post-1995 effect on the probability of being a migrant.

Finally, a series of economic reforms since 1990 may be relevant. In 1991, the government of President Gaviria introduced a sharp reduction in tariffs and undertook steps toward deregulation of labor and financial markets (these are discussed in Kugler, 1999, and Eslava et al., 2004). In 1993, Gaviria's government also introduced a social security reform (Kugler & Kugler, forthcoming; Kugler, 2005). Around 1997, President Samper's government introduced a minor tax reform and privatized the energy sector. However, these structural reforms were adopted at the national level and should have affected growing and nongrowing regions alike. In principle, a decentralization effort in 1991 may have affected different types of regions differently. In practice, however, this change was very limited, with tax collection and most spending remaining under central control (Alesina, 2005). Finally, Plan Colombia, an important American-sponsored aid and antidrug initiative, came on the scene after our period of study.

IV. The Economic Consequences of a Coca Economy

A. *Data and Descriptive Statistics*

This section uses differences-in-differences type regressions to assess the economic consequences of the shift in coca production to Colombian growing regions. The data come from the rural component of Colombia's annual household survey and are described in the appendix. The rural survey provides large repeated cross sections, with information on households and individual household members, including children. We limit the analysis to data from 1992 (because of earlier changes in survey design) through 2000 (after which the survey was replaced by a new panel data set). The survey was conducted in 23 of Colombia's 33 departments. Using the fourteen-department definition of the growing region, the rural survey includes households from seven growing departments plus the two DMZ departments. Because only three non-DMZ departments from the nine-department growing region were included in the rural survey, we focus initially on the fourteen-department classification scheme.[14]

Our analysis looks separately at samples of adults, school-age children, and teenage boys who might be in the labor market. The sample of adults includes men and women aged 21–59, and is described in the first two columns of table 3 using data for 1992 and 1997. Roughly 30% of respondents in this sample were migrants, where migrants are defined as individuals who do not currently live in the county where they were born. Most were married and about half are male. The growing region contributed from 24% of the sample in 1992 to 30% of the sample in 1997. The number of respondents from the DMZ also increased, from 1.4% to 3.9%.

About two-thirds of adults in the survey were employed in 1992 and 1997, though only about 36% had positive wage and salary earnings. Employment rates for men were 93%–95%, as can be seen in columns 3 and 4, and 55% of men had positive wage and salary earnings. Between 25% and 26% of adult men and women had positive income from self-employment, while between 35% and 37% of adult men had positive income from self-employment. Self-employment income includes income from individual short-term contracts, from the sale of domestically produced goods, and from commercial or family-based agricultural production. Wage and salary earnings and self-employment income are reported in real terms and were constructed using the consumer price index provided by the Department of National Statistics (DANE). These variables are given in 1998 pesos, worth about 1,400 to the U.S. dollar. Thus, mean wages range from $52 to $58 per month, and mean self-employment income from $241 to $252 per year, in the sample of adults.

Descriptive statistics for the sample of children, reported in columns 5–8, show that most were enrolled, and enrollment rates increased somewhat between 1992 and 1997. Fewer children than adults were migrants, but the regional distribution of children was broadly similar to that for adults. Employment statistics for children are collected only for those over ten years of age. About a third of boys aged 10–16 and 10% of girls aged 10–16 were working, indicating the importance of child labor. The statistics in columns

[14] The included growing departments are Bolívar, Cauca, Nariño, Cesar, La Guajira, Magdalena, and Norte de Santander, plus Caqueta and Meta in the DMZ. In contrast with the mortality analysis, discussed below, Antioquia and Valle del Cauca are included in the household analysis because the survey is limited to rural households.

TABLE 3.—DESCRIPTIVE STATISTICS FOR RURAL SURVEY

	Adult Workers (men and women)		Adult Workers (men)		Boys		Girls		Teenage Workers (boys)	
Variable	1992 (1)	1997 (2)	1992 (3)	1997 (4)	1992 (5)	1997 (6)	1992 (7)	1997 (8)	1992 (9)	1997 (10)
Employed	0.658	0.647	0.950	0.931	0.360	0.283	0.095	0.077	0.600	0.506
	(0.474)	(0.478)	(0.219)	(0.253)	(0.436)	(0.413)	(0.264)	(0.250)	(0.450)	(0.458)
Hours worked per month	142.3	131.8	219.3	200.3	65.6	44.9	15.4	11.2	121.2	94.1
	(117.3)	(115.0)	(76.8)	(85.2)	(86.9)	(74.3)	(47.8)	(39.7)	(103.0)	(96.6)
Monthly wages	74,098	81,461	115,439	123,636					61,038	58,921
	(115,512)	(127,287)	(126,002)	(137,069)					(78,522)	(84,989)
Positive wages	0.362	0.369	0.551	0.555					0.418	0.350
SE income (5% top-code)	337,712	352,969	557,381	551,260					61,196	65,253
	(815,718)	(865,459)	(1,026,941)	(1,048,327)					(311,898)	(291,899)
Positive SE income	0.247	0.259	0.348	0.371					0.066	0.080
Enrolled	0.017	0.028	0.014	0.024	0.694	0.779	0.757	0.815	0.397	0.486
Age	36.43	37.02	36.60	37.12	11.91	11.94	11.87	11.90	16.16	16.22
	(10.65)	(10.62)	(10.68)	(10.63)	(2.10)	(2.18)	(2.15)	(2.19)	(1.95)	(1.90)
HH size	5.60	5.32	5.56	5.31	6.60	6.35	6.66	6.41	6.57	6.35
	(2.56)	(2.49)	(2.60)	(2.51)	(2.36)	(2.41)	(2.36)	(2.43)	(2.48)	(2.57)
Migrant	0.282	0.316	0.280	0.312	0.133	0.161	0.163	0.160	0.161	0.182
Single	0.229	0.221	0.284	0.277	1.00	0.999	0.990	0.981	0.981	0.970
Male	0.496	0.597								
Growing (14-dept.)	0.235	0.303	0.234	0.309	0.262	0.332	0.250	0.336	0.238	0.332
Growing (9-dept.)	0.137	0.181	0.133	0.180	0.147	0.191	0.138	0.198	0.138	0.197
DMZ	0.014	0.039	0.014	0.039	0.013	0.040	0.014	0.044	0.011	0.034
Max *N*	13,550	19,184	6,641	9,801	2,602	3,513	2,477	3,253	2,040	2,881

Notes: Adult workers include men aged 21–59. Boys and girls are aged 8–16. Labor market outcomes for boys and girls are reported only for those over 10 years of age. Teenage workers are boys aged 13–20. Wages and self-employment income include zeros and are in real (1998) pesos, about 1,400 to the U.S. dollar.

9 and 10 show that over half of boys aged 13–20 were working. Hours per month for boys were substantial, though lower than for adults. Boys also had lower earnings. The wage and salary income of boys ranges from $42 to $44 per month, and boys' self-employment income ranges from $44 to $47 per year. Less than half were still in school and few were married.

B. *Results for Adults*

The basic empirical framework looks for growing-region/post-air-bridge interactions while controlling for department and year effects. In particular, we estimated year-region interaction terms using the following model for respondent i in department j in year t:

$$y_{ijt} = X_i'\mu + \beta_j + \delta_t + \sum_s \alpha_{0s} g_{jst} + \sum_s \alpha_{1s} d_{jst} + \varepsilon_{ijt} \qquad (1)$$

where β_j is a department effect, δ_t is a year effect, g_{jst} indicates non-DMZ growing departments when $t = s$, and d_{jst} indicates DMZ departments when $t = s$ ($s = 1994, \ldots, 2000$). The parameters α_{0s} and α_{1s} are the corresponding region-type/year interaction terms. Some models also include linear trends for each department type as a control for omitted variables and serial correlation. This amounts to replacing β_j with $\beta_{0j} + \beta_{1j}t$, where β_{1j} takes on three values (nongrowing, growing, and DMZ). The estimating equations also control for a vector of individual covariates, X_i, which includes sex, age dummies, household size, marital status, and migrant status. For binary dependent variables, the linear model was replaced with the analogous logit and results are reported as marginal effects. Standard errors here and elsewhere are clustered at the department level to allow for correlation across individuals within a state and within states over time.[15]

The analysis of rural outcomes begins with estimates of effects on the probability of having self-employment income and on the log of self-employment income for those who have some. Because coca production is an agricultural activity, self-employment status (either as farmer, employer, landowner, or contractor) is of special interest. The interpretation of results for log self-employment income is potentially complicated by selection bias from conditioning on having earnings in this sector. As in a wage equation, however, we can make an educated guess as to the likely sign of any selection bias. Since the presumptive effect of being in the growing region after 1994 is to increase the likelihood of self-employment, the conditional-on-positive estimates of effects on log wages will typically be biased downwards by the fact that, on the margin, those induced to enter self-employment have lower self-employment earnings potential in the absence of treatment (see, for example, Angrist, 2001).

The first two columns of table 4A report marginal effects from the logit version of equation (1), with a dummy for self-employment status on the left-hand side. The sample includes women as well as men because women have a reasonably high probability of having self-employment income. The estimates in column 1 are small, with positive but insignificant effects in 1995–1997 and 1999–2000. We also report results without migrants as a partial control for

[15] Standard errors estimated with department-year clustering are similar.

TABLE 4A.—ADULT LABOR MARKET OUTCOMES: YEARLY INTERACTIONS, POOLED GROWING AND DMZ

	Male and Female Workers				Men Only					
	Positive SE Income		Log SE Income		Employed		Log Hours (All Jobs)		Log Monthly Wage and Salary Earnings	
Interaction Terms	All (1)	w/o Migrants (2)	All (3)	w/o Migrants (4)	All (5)	w/o Migrants (6)	All (7)	w/o Migrants (8)	All (9)	w/o Migrants (10)
1994	−0.034	−0.045	−0.025	0.080	−0.014	−0.019	−0.026	−0.031	0.014	0.027
	(0.025)	(0.029)	(0.161)	(0.137)	(0.013)	(0.016)	(0.024)	(0.026)	(0.029)	(0.036)
1995	0.009	0.002	0.159	0.190	−0.002	−0.008	0.004	0.006	−0.011	−0.008
	(0.017)	(0.020)	(0.146)	(0.145)	(0.013)	(0.016)	(0.021)	(0.026)	(0.025)	(0.030)
1996	0.042	0.048	0.362	0.414	0.003	−0.010	0.034	0.048	−0.008	0.002
	(0.039)	(0.035)	(0.155)	(0.171)	(0.017)	(0.020)	(0.031)	(0.037)	(0.055)	(0.068)
1997	0.025	0.034	0.270	0.292	−0.005	−0.013	−0.001	0.002	0.010	0.028
	(0.027)	(0.026)	(0.124)	(0.115)	(0.015)	(0.146)	(0.035)	(0.042)	(0.053)	(0.059)
1998	−0.020	−0.009	0.302	0.291	0.0004	−0.012	0.053	0.035	0.103	0.107
	(0.033)	(0.029)	(0.154)	(0.189)	(0.016)	(0.018)	(0.023)	(0.026)	(0.067)	(0.070)
1999	0.007	0.016	0.205	0.150	0.016	0.007	0.006	0.005	0.069	0.067
	(0.023)	(0.017)	(0.122)	(0.132)	(0.014)	(0.017)	(0.023)	(0.023)	(0.048)	(0.046)
2000	0.043	0.051	0.255	0.230	−0.003	−0.012	0.065	0.085		
	(0.033)	(0.029)	(0.158)	(0.174)	(0.014)	(0.015)	(0.024)	(0.025)		
N	148,306	100,638	40,338	28,912	74,781	50,914	69,144	46,770	34,451	22,257

Notes: The table reports growing-region/year interactions estimated using equation (1) in the text. For columns 1, 2, 5, and 6 the results reported are marginal effects from a logit regression. Regressions include controls for sex, age dummies, household size, marital status, and migrant status. Estimates for monthly wages omit data for 2000. Standard errors adjusted for department clustering are reported in parentheses.

potential selection biases from migration into and out of growing regions. Results omitting migrants, reported in column 2, are somewhat larger, peaking in 1996 at 0.048, with a similar marginally significant effect of 0.051 in 2000 (s.e. = 0.029).

In contrast with the small-to-zero estimates for self-employment probabilities, the estimates in columns 3 and 4 show a substantial increase in (log) self-employment income. In particular, there are large, statistically significant effects on the order of 0.3–0.4 in 1996–1998, a period when coca is likely to have had a major impact. For example, the effect in 1996 in the sample including migrants is 0.362 (s.e. = 0.155). There are somewhat smaller positive effects in 1995 and 1999–2000.

In an effort to improve precision, we also estimated models with pooled region-year interaction terms. These models can be written

$$y_{ijt} = X_i'\mu + \beta_j + \delta_t + \alpha_{0,95\text{–}97}g_{jt,95\text{–}97} + \alpha_{0,98\text{–}00}g_{jt,98\text{–}00} + \alpha_{1,95\text{–}97}d_{jt,95\text{–}97} + \alpha_{1,98\text{–}00}d_{jt,98\text{–}00} + \varepsilon_{ijt}, \quad (2)$$

where X_i is the vector of individual characteristics referred to above, with coefficient vector μ. The interaction dummies $g_{jt,95\text{–}97}$ and $g_{jt,98\text{–}00}$ indicate the non-DMZ growing region for t = 1995–1997 and t = 1998–2000, with corresponding interaction terms $\alpha_{0,95\text{–}97}$ and $\alpha_{0,98\text{–}00}$. Likewise, the interaction dummies $d_{jt,95\text{–}97}$ and $d_{jt,98\text{–}00}$ indicate the DMZ in 1995–1997 and 1998–2000, with corresponding interaction terms $\alpha_{1,95\text{–}97}$ and $\alpha_{1,98\text{–}00}$. As before, with binary dependent variables the reported results are logit marginal effects. Also, as with equation (1), we estimated versions of equation (2) replacing β_j with $\beta_{0j} + \beta_{1j}t$, where β_{0j} is a department fixed effect and β_{1j} is a trend taking on three values, one for each department type.

Self-employment results from models with pooled interaction terms and omitting trends are reported in columns 1 and 3 of table 4B. These models generate statistically significant estimates of effects on the probability of self-employment and on the log of self-employment income in the non-DMZ growing region. The former effects are small, on the order of 3–4 percentage points, but the latter are large (see, for example, the column 3 estimate of 0.25 in 1995–1997 with a standard error of 0.121). Moreover, the absence of substantial effects on the probability of having self-employment income suggests selection bias from changes in labor force participation is not much of a concern in this context.

Estimates of interaction terms for the DMZ show no effect on the probability of having self-employment income, but even larger (though imprecisely estimated) effects on log self-employment income than in the non-DMZ region. Again, these results may be subject to selection bias as a result of migration, especially in the DMZ, though we include a migrant dummy as a partial control. At the same time, as pointed out above, we found no evidence of selection bias due to migration in a regression of the probability of being a migrant on a growing/post-1995 interaction.

The evidence for an effect of the coca boom on the probability of self-employment is weakened considerably by the inclusion of region-specific trends. For example, the estimates reported in column 2 of table 4B are either 0 or negative. On the other hand, the 1995–1997 effect on the log of self-employment income, estimated in a model with region-specific trends, is about the same as when estimated without trends (compare 0.288 and 0.251) and marginally significant (t = 1.78). Moreover, the trend itself is not significantly different from 0.

TABLE 4B.—ADULT LABOR MARKET OUTCOMES WITH POOLED INTERACTION TERMS

	Male and Female Workers				Men Only					
	Positive SE Income		Log SE Income		Employed		Log Hours (All Jobs)		Log Monthly Wage and Salary Earnings	
Interaction Terms	No Trends (1)	w/Trends (2)	No Trends (3)	w/Trends (4)	No Trends (5)	w/Trends (6)	No Trends (7)	w/Trends (8)	No Trends (9)	w/Trends (10)
Panel A: Growing Effects (non-DMZ)										
1995–1997	0.038	0.005	0.251	0.288	0.004	0.008	0.020	0.013	−0.005	−0.035
	(0.020)	(0.020)	(0.121)	(0.162)	(0.010)	(0.014)	(0.018)	(0.031)	(0.037)	(0.058)
1998–2000	0.030	−0.035	0.260	0.333	0.013	0.020	0.044	0.030	0.079	0.026
	(0.021)	(0.035)	(0.130)	(0.286)	(0.012)	(0.025)	(0.017)	(0.057)	(0.044)	(0.122)
Trends		0.011		−0.012		−0.001		0.002		0.010
		(0.008)		(0.048)		(0.003)		(0.010)		(0.022)
Panel B: DMZ Effects										
1995–1997	0.002	−0.027	0.625	0.383	−0.020	0.058	0.050	0.057	−0.047	−0.067
	(0.025)	(0.045)	(0.125)	(0.150)	(0.040)	(0.044)	(0.032)	(0.084)	(0.017)	(0.105)
1998–2000	−0.097	−0.152	0.349	−0.129	−0.039	0.109	0.126	0.138	0.058	0.024
	(0.069)	(0.108)	(0.117)	(0.306)	(0.027)	(0.035)	(0.025)	(0.087)	(0.067)	(0.225)
Trends		0.009		0.077		−0.025		−0.002		0.006
		(0.007)		(0.057)		(0.004)		(0.018)		(0.031)
N	148,306		40,338		74,781		69,144		34,451	

Notes: The table reports pooled growing-region/year interaction terms estimated using equation (2) in the text. For columns 1, 2, 5, and 6 the results reported are marginal effects from a logit regression. Regressions include controls for sex, age dummies, household size, marital status, and migrant status. Estimates for monthly wages omit data for 2000. Standard errors adjusted for department clustering are reported in parentheses.

The remaining estimates in tables 4A and 4B are for effects on labor supply measures and the log of monthly wages in a sample of men. We focus on men because male participation rates are considerably higher than female participation rates, especially in the wage sector. The estimated employment effects for men show little evidence of a change in participation in the growing region. Most of the estimated yearly interaction terms are small and none are significantly different from 0. There is some evidence of an increase in log hours, though it is not very robust. For example, in the hours equation, the 1996 interaction without migrants is 0.048 (s.e. = 0.037) and the 1998 interaction with migrants is 0.053 (s.e. = 0.023). In models with pooled interactions, there is stronger evidence for a significant effect in 1998–2000 than in 1995–1997, though again the estimates are muddied by inclusion of region-specific trends. The pattern of results for log wages similarly offers no robust evidence of an effect.

C. *Results for Children and Youth*

We might expect the increase in coca production to have reduced school enrollment and to have generated an increase in child labor.[16] Columns 1 and 2 in table 5A indeed show statistically significant reductions of 0.065 and 0.073 in boys' school enrollment in 1997, but estimates for other years are smaller, and none of the corresponding estimates in pooled models, with or without trends, are significant (see table 5B). Moreover, while the estimated interaction terms without trends are all negative, inclusion of trends causes the signs to flip for boys. Estimates for girls are mainly positive, though on the whole not significant. An exception is the DMZ, where effects are negative and marginally significant without trends.[17]

While there appears to have been little impact on school enrollment, the pattern of estimates for teen boys' labor supply is more complex. The one-year employment effect in 1997 and the pooled later-period employment effect in the non-DMZ growing region are positive. On the other hand, the pooled interaction term for the later period is negative and significant for the DMZ, though imprecise and implausibly large in models with trends. The pooled non-DMZ growing effects are also negative when estimated in models with trends. It therefore seems fair to say that there is no robust evidence of an increase in boys' employment rates.

Results for log hours are more clear-cut. In models without trends, log hours appear to have increased in both the non-DMZ growing region and the DMZ. For example, the pooled estimate for the earlier period for the non-DMZ area is 0.112 (s.e. = 0.039) and many of the yearly interactions in table 5A are significant. Inclusion of trends wipes out the DMZ effect but leaves the non-DMZ effects essentially unchanged, though no longer significant. Again, however, the trend in the non-DMZ growing region is insignificant as well. On balance, therefore, table 5A provides support for the notion that coca production increased teen boys' labor supply, at least in the growing departments outside of the DMZ.

[16] Edmunds and Pavcnik (2004) recently explore the link between trade flows and child labor. Following their taxonomy, coca can be seen as an unskilled-labor-intensive good that is a candidate for production with child labor.

[17] To adjust inference for within-household clustering, estimates for children and youth were averaged up to the household level. For details, see the appendix.

TABLE 5A.—OUTCOMES FOR CHILDREN: YEARLY INTERACTIONS, POOLED GROWING AND DMZ

	Enrollment				Labor Market (Teenage Boys)			
	Boys		Girls		Employment		Log Hours (All Jobs)	
Interaction Terms	All (1)	w/o Migrants (2)	All (3)	w/o Migrants (4)	All (5)	w/o Migrants (6)	All (7)	w/o Migrants (8)
1994	−0.004	−0.042	0.054	0.044	0.021	0.025	−0.028	−0.040
	(0.042)	(0.050)	(0.027)	(0.038)	(0.048)	(0.055)	(0.082)	(0.107)
1995	−0.011	−0.012	0.061	0.049	−0.037	−0.048	0.069	0.074
	(0.031)	(0.037)	(0.032)	(0.031)	(0.038)	(0.039)	(0.085)	(0.099)
1996	−0.006	0.001	0.025	0.023	0.033	0.034	0.187	0.173
	(0.035)	(0.033)	(0.031)	(0.035)	(0.052)	(0.058)	(0.082)	(0.071)
1997	−0.065	−0.073	0.031	0.025	0.078	0.092	0.081	0.041
	(0.037)	(0.035)	(0.043)	(0.047)	(0.040)	(0.046)	(0.069)	(0.076)
1998	−0.020	−0.042	0.036	0.042	0.010	0.001	0.228	0.243
	(0.046)	(0.051)	(0.062)	(0.074)	(0.061)	(0.069)	(0.062)	(0.062)
1999	−0.055	−0.052	0.017	0.009	0.099	0.123	0.180	0.160
	(0.035)	(0.039)	(0.046)	(0.054)	(0.052)	(0.055)	(0.047)	(0.051)
2000	−0.054	−0.059	0.034	0.017	0.078	0.072	0.272	0.253
	(0.047)	(0.053)	(0.044)	(0.048)	(0.066)	(0.067)	(0.045)	(0.057)
N	27,382	22,695	25,771	21,259	22,365	18,319	12,528	10,104

Notes: The table reports growing-region/year interactions estimated using equation (1) from the text. For regressions with a binary dependent variable, the results reported are marginal effects. Standard errors adjusted for department clustering are reported in parentheses.

D. *Estimates Using Urban Controls and without Medium Producers*

Although estimates of equations (1) and (2) point to effects on self-employment income for adults and effects on hours worked by teenage boys, these results are made less precise by the inclusion of region-specific trends. In an effort to increase precision and further check the robustness of these findings, we tried a pooled analysis that stacks urban with rural data for the subset of departments included in both surveys.[18] The idea here is to check whether growing-region/post-air-bridge interaction effects are indeed larger in rural than urban parts of growing departments, since we expect income shocks generated by coca to be larger in the countryside. An urban-rural stack also facilitates control for region-specific trends, assuming these trends have similar effects in urban and rural areas. A second modification we explored in an effort to sharpen the growing/nongrowing contrast is to drop the five medium producer departments from the list of fourteen growing regions. This is in the spirit of Black, McKinnish, and Sanders's (2005) analysis of coal-producing counties, which also excludes middle producers according to the level of baseline production.

The estimating equation for the stacked sample allows for urban main effects and urban interactions with both region-

[18] The urban household survey is distinct from the rural survey and has somewhat different geographic coverage and variable definitions. For details, see the appendix.

TABLE 5B.—OUTCOMES FOR CHILDREN WITH POOLED INTERACTION TERMS

	Enrollment				Labor Market (Teenage Boys)			
	Boys		Girls		Employment		Log Hours (All Jobs)	
Interaction Terms	No Trends (1)	w/Trends (2)	No Trends (3)	w/Trends (4)	No Trends (5)	w/Trends (6)	No Trends (7)	w/Trends (8)
	Panel A: Growing Effects (non-DMZ)							
1995–1997	−0.010	0.040	0.035	0.024	0.016	−0.087	0.112	0.117
	(0.023)	(0.038)	(0.016)	(0.042)	(0.032)	(0.046)	(0.039)	(0.058)
1998–2000	−0.028	0.069	0.027	0.006	0.073	−0.132	0.236	0.246
	(0.039)	(0.067)	(0.042)	(0.083)	(0.057)	(0.066)	(0.046)	(0.136)
Trends		−0.016		0.004		0.034		−0.002
		(0.011)		(0.015)		(0.010)		(0.020)
N	27,382	27,382	25,771	25,771	22,365	22,365	12,528	12,528
	Panel B: DMZ Effects							
1995–1997	−0.103	0.174	−0.115	−0.197	−0.032	−0.248	0.238	−0.004
	(0.024)	(0.030)	(0.069)	(0.076)	(0.030)	(0.171)	(0.039)	(0.116)
1998–2000	−0.079	0.436	−0.138	−0.293	−0.213	−0.613	0.232	−0.216
	(0.031)	(0.046)	(0.056)	(0.099)	(0.069)	(0.394)	(0.042)	(0.239)
Trends		−0.087		0.025		0.066		0.071
		(0.008)		(0.013)		(0.054)		(0.040)
N	27,382		25,771		22,365		12,528	

Notes: The table reports pooled growing-region/year interaction terms estimated using equation (2) in the text. For regressions with a binary dependent variable, the results reported are marginal effects. Estimates for monthly wages omit data for 2000. Standard errors adjusted for department clustering are reported in parentheses.

TABLE 6.—ADULT AND TEENAGE LABOR MARKET OUTCOMES: RURAL AND URBAN EFFECTS

	Male and Female Workers				Men Only				Teenage Boys	
	Positive SE Income		Log SE Income		Log Hours (All Jobs)		Log Monthly Wage and Salary Earnings		Log Hours (All Jobs)	
Interaction Terms	14 growing (1)	no med. prod. (2)	14 growing (3)	no med. prod. (4)	14 growing (5)	no med. prod. (6)	14 growing (7)	no med. prod. (8)	14 growing (9)	no med. prod. (10)
Panel A: Rural Effects										
1995–1997	0.005	0.015	0.286	0.322	0.037	0.058	−0.019	−0.042	0.119	0.117
	(0.016)	(0.023)	(0.129)	(0.138)	(0.026)	(0.028)	(0.036)	(0.042)	(0.052)	(0.059)
1998–2000	−0.025	−0.022	0.295	0.298	0.079	0.122	0.045	−0.011	0.216	0.198
	(0.021)	(0.031)	(0.183)	(0.213)	(0.042)	(0.036)	(0.071)	(0.070)	(0.105)	(0.128)
Panel B: Urban Effects										
1995–1997	0.011	−0.01	0.155	0.198	0.025	0.041	−0.038	−0.060	−0.004	−0.014
	(0.018)	(0.022)	(0.113)	(0.104)	(0.021)	(0.019)	(0.043)	(0.044)	(0.068)	(0.076)
1998–2000	−0.015	−0.052	0.174	0.197	0.040	0.074	−0.087	−0.122	−0.047	−0.017
	(0.030)	(0.035)	(0.184)	(0.189)	(0.041)	(0.040)	(0.071)	(0.077)	(0.091)	(0.109)
Trend	0.008	0.011	−0.004	−0.019	−0.005	−0.013	0.011	0.021	0.001	−0.004
	(0.005)	(0.006)	(0.028)	(0.032)	(0.007)	(0.006)	(0.012)	(0.014)	(0.015)	(0.019)
N	482,053	457,855	116,896	109,693	192,840	181,882	101,779	96,797	22,141	20,544

Notes: The table reports results from a stacked urban and rural sample. Estimates for monthly wages omit data for 2000. Standard errors adjusted for department clustering are reported in parentheses. Columns labeled "14 growing" use the same growing region as in tables 4 and 5. Columns labeled "no medium producers" drop the five medium-producing departments from the analysis.

type and period dummies in a pooled model similar to the one used to construct the estimates reported in table 4B. The coefficients of interest are growing-region/posttreatment interaction terms, which are allowed to differ by urban-rural status. This analysis pools growing and DMZ because one DMZ department is missing from the urban survey. Finally, these models control for region-specific trends, which are assumed to be the same in both the rural and urban areas of a given department type. The addition of urban data potentially allows us to estimate these trends more precisely. The stacked analysis is limited to the subset of adult and children outcomes of primary interest and/or for which there is some evidence of an effect in tables 4 and 5.

Estimates for adult self-employment outcomes in the rural sector, constructed from the urban/rural stack and reported in columns 1–4 of table 6, are similar to those generated using rural data only. Again, there is no evidence of an increased likelihood of self-employment in cities or in the countryside (in fact, there is a negative effect for 1998–2000 in urban areas). At the same time, however, the stacked results show a sharp increase in log self-employment earnings for rural workers; in column 3, for example, the effect is 0.29 (s.e. = 0.13) in 1995–1997. In contrast, the corresponding urban effect is an insignificant 0.16 (s.e. = 0.11). The interaction terms in column 3 for the 1998–2000 period similarly show larger effects in rural than urban areas. Estimates of effects on log self-employment income using a sample without medium producers are slightly larger than in the full sample, but otherwise similar.

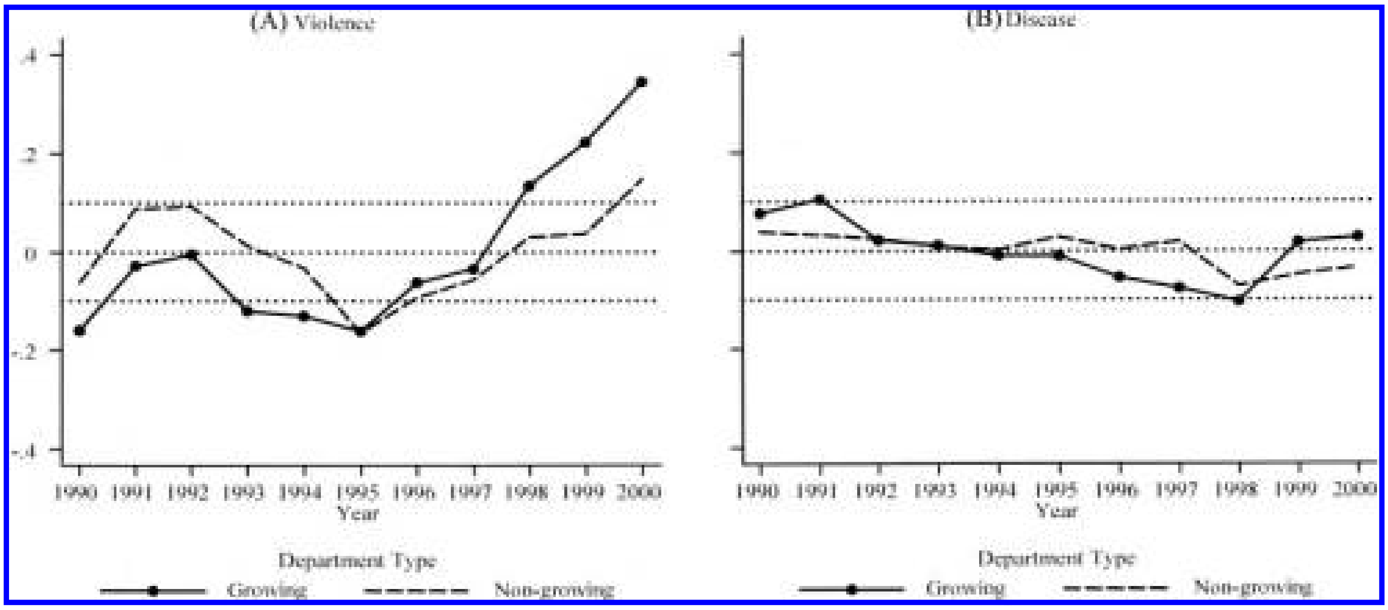
In contrast with the self-employment results, the estimated effects on hours worked by adult men are more mixed. Estimates with medium producers included, reported in column 5, show no significant rural or urban effects. On the other hand, dropping the medium producers leads to significant rural effects and a smaller but significant urban effect in 1995–1997. The rural effect is larger but not significantly different from that in urban areas. Therefore, taking the urban effect as a control for confounding factors, this suggests there is no effect of coca cultivation on adult hours in rural areas. At the same time, columns 7–8 show no effect on wage and salary earnings in either urban or rural locations.

A clearer picture emerges from the analysis of hours worked by teen boys in rural areas. These results, reported in columns 9–10 of table 6, show mostly significant effects with or without medium producers. The results for teen boys also generate a significant contrast by urban-rural status. In particular, there are substantial increases in hours worked by rural teen boys, with no corresponding effect on teen boys in urban areas. For example, the effect on hours worked in 1995–1997 is 0.12 (s.e. = 0.052) in rural areas, but −0.004 (s.e. = 0.068) in urban areas. Finally, we note that estimates for employment status using the urban/rural stack and corresponding to those in table 6 (but not reported in this table) show no effects on either adult men or teen boys.

E. *Robustness Checks and Discussion of Magnitudes*

The results in table 6 provide consistent evidence of an increase in self-employment income and hours worked by teen boys in the rural parts of coca-growing departments. As a check on these findings, we estimated a number of variations on the basic setup using data from the rural sample. These results, reported in appendix table A1, are for models with pooled yearly interaction terms and pooled growing and DMZ effects as in table 6. The first variation adds department rainfall as a time-varying control variable to the model without department trends; the second includes rainfall and department trends. Neither of these variations has a marked effect on the estimated effects, though as

FIGURE 3.—DEATH RATES FOR MEN AGED 15–59



Note: The figures plot log death rates, relative to the average rate by department type (growing, nongrowing). The nongrowing region omits Antioquia, Valle, and Bogotá, DC.

before control for linear trends reduces precision. The remaining two variations allow for different sorts of trends; first, an interaction between per capita gross state (department) product in 1993; and second, a linear trend specific to departments with a substantial FARC presence in 1994. The results here also change little, showing similar increases in self-employment income and teen boys' log hours in the growing regions.[19]

To give a sense of whether the magnitude of self-employment earnings effects in columns 3–4 of table 6 can plausibly be attributed to a coca boom, we take 0.2 as a benchmark, a number between the rural effect of 0.29 from column 3 and the corresponding difference in urban and rural effects, $0.29 - 0.16 = 0.13$. To calibrate, we use Uribe's (1997) description of a typical family coca farm, consisting of a half-hectare plot that generates about 110,000 pesos/month in revenues from the sale of coca leaf. Our estimates imply an increase in monthly self-employment earnings of about 22,800 pesos at the mean positive self-employment income for self-employed workers (roughly 114,000 pesos per month). We do not know how many self-employed workers were actually growing coca. But assuming a quarter of self-employed adults in rural growing regions had small coca plots, an aggregate increase of 20% in self-employment earnings could have been generated by a 40% increase in output among existing producers jointly with a 40% increase in prices. This is obviously just a rough guess. The point is that the magnitude of the price increase and the earnings from coca at baseline are very likely large enough to sustain the kind of impact suggested by our estimates.

V. Coca and Violence

The estimates in the previous section point to some localized benefits from the coca boom. The benefits are largely those that might be expected to accrue to farmers or others directly involved in the coca industry, including a marginal labor force of teenage boys. On the other hand, there is little evidence of wider gains or spillovers in the form of a regionwide increase in earnings. In this section, we turn to an analysis of coca effects on violence, as captured by changing homicide rates.

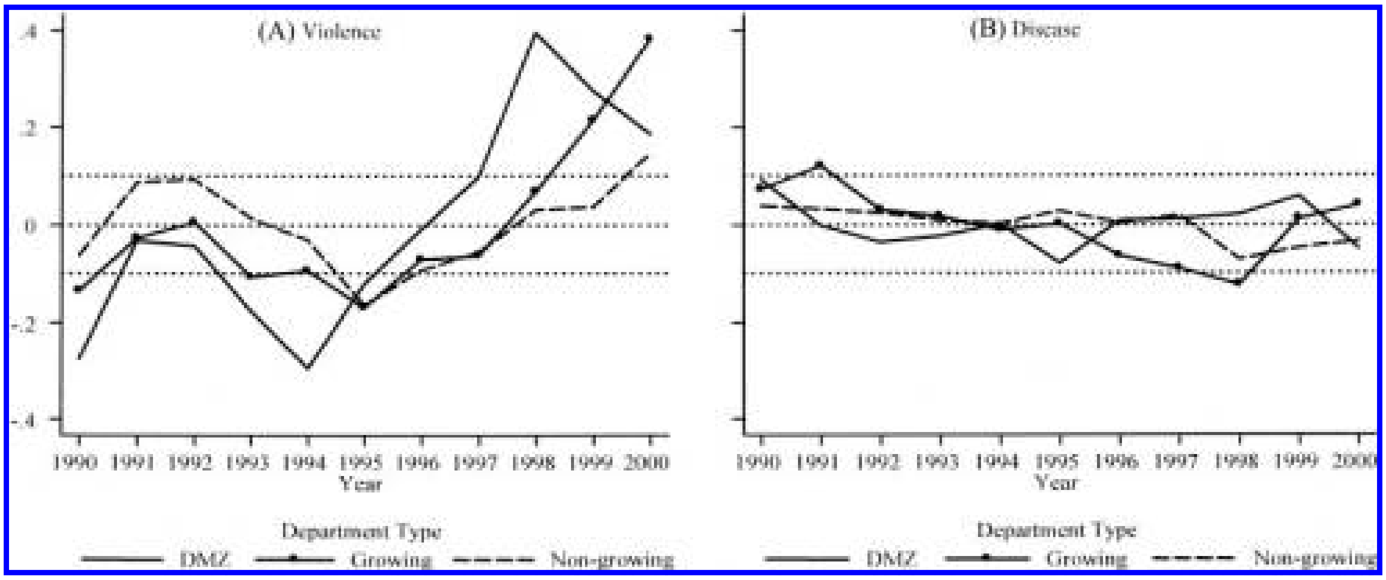
A. *Graphical Analysis*

The evolution of violent death rates in the 1990s is described in figure 3A, which plots death rates per 100,000 for men aged 15–59 by region type, after removing group means. This figure pools the DMZ with other growing departments defined using the fourteen-department scheme.[20] The resulting plot shows a remarkably parallel evolution of violent death rates in the growing and nongrowing areas through 1993. In particular, the growing and nongrowing regions both exhibit a similar up-then-down pattern. But death rates in the growing region flattened in 1994 while the

[19] The additional data used for robustness checks are documented at the end of the data appendix. The FARC data used to construct the estimates in table A1 were also used to construct some of the estimates in table A2 and the estimates in panel A of table 10, discussed below.

[20] The nongrowing region omits Antioquia, Valle del Cauca, and Bogotá, the departments with Colombia's three largest cities. Death rates were coded from vital statistics microdata obtained from the Colombian statistical agency, DANE. Violent deaths are defined here as homicides, suicides, deaths from military and insurgent activity (not a distinct category in all years), and a small number of nonaccident deaths by external causes not elsewhere classified. Over 90% of violent deaths are homicides. For additional details see the data appendix.

FIGURE 4.—DEATH RATES FOR MEN AGED 15–59



Note: The figures plot log death rates, relative to the average rate by department type (DMZ, growing, nongrowing). The nongrowing region omits Antioquia, Valle, and Bogotá, DC.

decrease in the nongrowing region accelerated in 1995. Violent death rates increased in both regions after 1995, but the average rate of increase in the growing region became much steeper than in the nongrowing region. In contrast with this parallel-then-divergent pattern in violent death rates, death rates from disease fell somewhat more steeply in the growing than in the nongrowing region from 1990 through 1998, when there was a sharp upturn in the growing region. The evolution of death rates from disease can be seen in figure 3B.[21]

Figures 4A and 4B show a similar picture in the context of a three-region analysis that separates the DMZ from other growing departments. Here too, violent death rates in the non-DMZ growing region flatten in 1994 while death rates in the nongrowing region turn more sharply downwards in 1995, so that by 1995, the gap in death rates between the two region types had closed. The main difference between this picture and that in figure 3A is that violent death rates in the DMZ fell through 1994, after which they increased sharply until a 1998 peak. In contrast with the relative increase in violent death rates in the growing region, death rates from disease improved steadily relative to the nongrowing region beginning in 1992. Although somewhat more volatile, death rates from disease in the DMZ also roughly tracked those in the nongrowing region until 1998 when the nongrowing region saw a relative decline.

A possible complication in the analysis of death rates is the quality of the population statistics used for the denominator. We used census-based five-year estimates and population projections published by DANE (1998) for 1990, 1995, and 2000, linearly interpolating statistics for in-between years. As noted above, however, the 1990s were marked by considerable population movement, so the population denominator may be inaccurate.[22] An alternative strategy that avoids this problem is to look at violent death rates relative to death rates from other causes. After transformation to log odds, this approach can be motivated by a multinomial logit model for the risk of death by cause.

To describe the logit strategy more formally, let v_{jt} denote the number of violent deaths in department j and year t and let n_{jt} denote the number of deaths from all other causes. Let p_{jt} denote the corresponding population statistics. Write the probability of violent and nonviolent death as

$$v_{jt}/p_{jt} \equiv \exp(\alpha_{jt}(v))/[1 + \exp(\alpha_{jt}(v)) + \exp(\alpha_{jt}(n))],$$

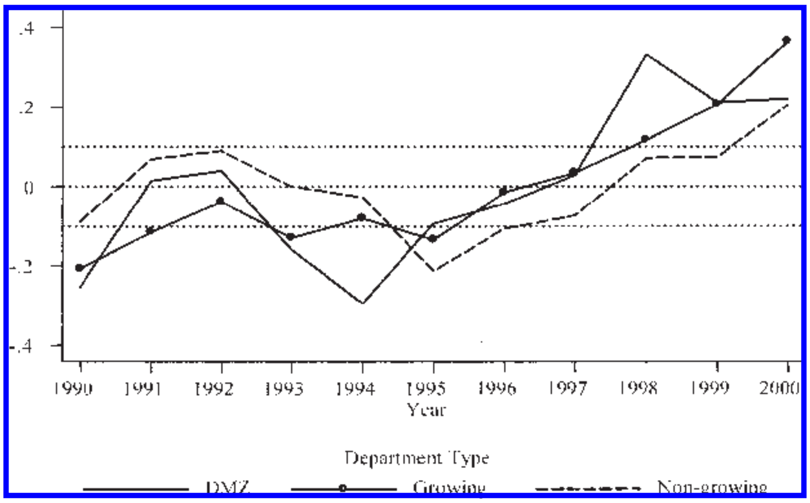
$$n_{jt}/p_{jt} \equiv \exp(\alpha_{jt}(n))/[1 + \exp(\alpha_{jt}(v)) + \exp(\alpha_{jt}(n))],$$

where $\alpha_{jt}(v)$ and $\alpha_{jt}(n)$ are functions of region and year. We assume further that $\alpha_{jt}(v)$ can be modeled as an additive function of region (B_j) and year (Δ_t) main effects plus growing-region/year interaction terms induced by the shift of coca production to Colombia, while $\alpha_{jt}(n)$ has additive region and year main effects only. In this framework,

[21] Competing risks complicate the interpretation of the decline in disease death rates since some of those who die by violence may have otherwise died of disease. Still, it seems likely that an environment of deteriorating public health would turn up in higher disease death rates (a pattern observed in the DMZ after the government ceded control). The competing-risks problem is likely mitigated by the fact that those most likely to die from disease (the very old and very young) are least likely to die by violence. As a check on this problem, we looked at infant and child mortality by region type. These data also show relative improvement in the growing region until 1998.

[22] These statistics differ from the population figures in table 2, which are based on the 1993 census.

FIGURE 5.—DEATH RATES, LOGIT(VIOLENT/NONVIOLENT), FOR MEN AGED 15–59



Note: The figure plots the residual from a regression of $\ln(v_{jt}/n_{jt})$ on region effects (that is, deviations from group means) using the 14-department classification scheme, separating the DMZ from other growing departments as in figure 4. The nongrowing region omits Antioquia, Valle, and Bogota, DC.

nonzero estimates of the interaction terms B_{0s} and B_{1s} in the grouped-logit equation

$$\ln(v_{jt}/n_{jt}) \equiv \alpha_{jt}(v) - \alpha_{jt}(n) = B_j + \Delta_t + \sum_s B_{0s}g_{jst} + \sum_s B_{1s}d_{jst} + v_{jt}$$

provide evidence of region-specific shocks that increased the risk of violent death.

This strategy is illustrated in figure 5, which plots the residual from a regression of $\ln(v_{jt}/n_{jt})$ on region effects (that is, deviations from group means) using the fourteen-department classification scheme, separating the DMZ from other growing departments as in figure 4.[23] The logit plot shows the same initial pattern of up-then-down log-odds of violent death in both growing and nongrowing regions, with a more stable then increasing pattern of log-odds of violent death in the growing region after 1993 and especially after 1995, while the odds of death by violence were decreasing in the nongrowing region from 1993 to 1995. This is broadly similar to the pattern exhibited by log death rates in figure 4A. The log-odds of violent death in the DMZ also turned sharply upwards in 1995.

On balance, the figures suggest that beginning sometime between 1994 and 1996, violent death rates in the non-DMZ growing region became markedly higher than what should have been expected based on pre-1994 trends. The pattern was generally similar in the DMZ, where the FARC presence was stronger. Moreover, the increase in violent death rates contrasts with a gradually improving disease environment in this period, both nationwide, and in the growing relative to the nongrowing region. The contrast in trends for violence and disease mortality weighs against the notion of a secular deterioration in infrastructure or social systems that caused the increase in violence.

B. Regression Estimates

To quantify the relative increase in violent death rates in the growing region, growing-region/year interaction terms were estimated using the following equation:

$$\ln(v_{ajt}/p_{ajt}) = \mu_a + \beta_j + \delta_t + \textstyle\sum_s \alpha_{0s}g_{jst} + \sum_s \alpha_{1s}d_{jst} + \varepsilon_{ajt}. \quad (3)$$

The dependent variable, $\ln(v_{ajt}/p_{ajt})$, is the log death rate in cells defined by ten-year age groups (indexed by a), department (indexed by j), and year (indexed by t). The term μ_a is an age effect, while other parameters are defined as in equation (1). Also, as with the analysis in the previous section, some models include trends for each department type.[24]

Unweighted estimates of α_{0s} and α_{1s} are reported in panel A of table 7, while population-weighted estimates are shown in panel B. The table reports results with and without medium producers. The unweighted estimates of α_{0s} in column 1 show an insignificant 12.3% (log point) increase in mortality in 1994 in the non-DMZ growing region, with no effect in 1993 (a specification check). The unweighted growing-region effect increases through 1997, while the DMZ effect increases through 1998. Few of the individual yearly effects are significant, but, as we show in later tables,

[23] Year effects are left in the plotted series so the common trend is visible.

[24] For purposes of estimation, the sample was expanded slightly to include ages 15–64 to accommodate the ten-year age groups. Data are analyzed for age-specific cells to control for changes in the age distribution due to migration and because mortality trends tend to be age-specific. Standard errors were adjusted for department clustering.

TABLE 7.—MORTALITY ESTIMATES

	14 Growing Departments				No Medium Producers			
	No trends		With trends		No trends		With trends	
	Growing (1)	DMZ (2)	Growing (3)	DMZ (4)	Growing (5)	DMZ (6)	Growing (7)	DMZ (8)
	A. Unweighted							
1993	0.026	−0.059			0.012	−0.059		
	(0.131)	(0.079)			(0.217)	(0.080)		
1994	0.123	−0.078	0.133	−0.145	0.209	−0.077	0.255	−0.145
	(0.144)	(0.127)	(0.144)	(0.087)	(0.226)	(0.128)	(0.168)	(0.087)
1995	0.102	0.198	0.118	0.098	0.236	0.198	0.302	0.097
	(0.131)	(0.279)	(0.203)	(0.175)	(0.190)	(0.280)	(0.284)	(0.176)
1996	0.154	0.266	0.176	0.134	0.289	0.266	0.374	0.132
	(0.163)	(0.222)	(0.298)	(0.154)	(0.202)	(0.223)	(0.459)	(0.155)
1997	0.230	0.402	0.259	0.237	0.346	0.403	0.451	0.236
	(0.178)	(0.293)	(0.344)	(0.162)	(0.225)	(0.294)	(0.501)	(0.163)
1998	0.200	0.514	0.235	0.315	0.506	0.514	0.631	0.314
	(0.200)	(0.288)	(0.452)	(0.171)	(0.243)	(0.290)	(0.727)	(0.173)
1999	0.197	0.340	0.238	0.109	0.259	0.340	0.403	0.107
	(0.119)	(0.243)	(0.482)	(0.203)	(0.155)	(0.244)	(0.763)	(0.204)
2000	0.289	0.142	0.337	−0.122	0.358	0.142	0.522	−0.124
	(0.120)	(0.251)	(0.550)	(0.246)	(0.144)	(0.252)	(0.874)	(0.248)
Dept. trend			−0.006	0.033			−0.020	0.033
			(0.064)	(0.040)			(0.103)	(0.041)
	B. Weighted							
1993	−0.080	−0.100			−0.202	−0.101		
	(0.085)	(0.055)			(0.071)	(0.055)		
1994	0.045	−0.137	0.097	−0.105	0.062	−0.137	0.283	−0.105
	(0.111)	(0.131)	(0.131)	(0.097)	(0.134)	(0.132)	(0.106)	(0.097)
1995	0.141	0.126	0.205	0.159	0.183	0.125	0.473	0.159
	(0.120)	(0.242)	(0.157)	(0.211)	(0.161)	(0.243)	(0.120)	(0.212)
1996	0.163	0.178	0.241	0.214	0.180	0.178	0.538	0.214
	(0.204)	(0.230)	(0.221)	(0.199)	(0.241)	(0.232)	(0.205)	(0.200)
1997	0.179	0.260	0.269	0.298	0.220	0.259	0.647	0.298
	(0.184)	(0.249)	(0.226)	(0.205)	(0.217)	(0.250)	(0.194)	(0.207)
1998	0.120	0.434	0.222	0.474	0.169	0.434	0.665	0.475
	(0.151)	(0.255)	(0.261)	(0.235)	(0.156)	(0.256)	(0.246)	(0.236)
1999	0.313	0.296	0.428	0.339	0.369	0.296	0.934	0.339
	(0.149)	(0.205)	(0.324)	(0.223)	(0.185)	(0.206)	(0.268)	(0.224)
2000	0.388	0.119	0.516	0.163	0.401	0.118	1.034	0.164
	(0.127)	(0.197)	(0.344)	(0.246)	(0.150)	(0.198)	(0.288)	(0.247)
Dept. trend			−0.013	−0.002			−0.069	−0.002
			(0.039)	(0.024)			(0.034)	(0.024)

Notes: The table reports results of regressions with log violent death rates on left-hand side, controlling for year and age effects. The model is estimated using statistics aggregated by department, year, and ten-year age groups, for men aged 15–64. Standard errors adjusted for department clustering are in parentheses.

the consistent pattern across regions and over times generates significant pooled estimates. Weighting also tends to increase precision, though it leads to somewhat smaller effects in models without trends. Omission of medium producers leads to larger and more significant effects.[25]

Inclusion of region-specific trends leads to less precise estimates in columns 3 and 4, though the weighted estimates are markedly larger when estimated with trends than without. None of the estimates using the fourteen-department classification are significant when estimated with trends. On the other hand, weighted models with trends generate significant effects as early as 1994 when estimated without medium producers. These estimates are reported in column 7. Although some of the estimates are quite large in percentage terms, it should be noted that the number of deaths involved is often small, particularly in sparsely populated rural departments.

Appendix table A2 reports the results of robustness checks for the basic mortality estimates similar to the robustness checks reported in panels A–C of table A1 for the rural results. Again we focus on models combining growing and DMZ departments, retaining the yearly-interaction format of table 7. The variations reported here include a benchmark with no modifications in columns 1 and 5 (other than the pooling of both department types), the addition of time-varying department-average rainfall, and the addition of GSP trends. These modifications leave the results unchanged. A more detailed exploration of results involving differential trends and treatment effects for departments with a previous guerrilla presence is discussed at the end of this section.

[25] DMZ effects are (by construction) identical with and without medium producers since the medium producers are a subset of the non-DMZ growing region.

TABLE 8.—MORTALITY ESTIMATES BY URBAN/RURAL RESIDENCE

	14 Growing Departments				Without Medium Producers			
	No trends		With trends		No trends		With trends	
	Growing (1)	DMZ (2)	Growing (3)	DMZ (4)	Growing (5)	DMZ (6)	Growing (7)	DMZ (8)
				A. Rural				
1994	0.038	−0.087	0.644	0.322	0.155	−0.086	0.645	0.321
	(0.173)	(0.094)	(0.418)	(0.129)	(0.186)	(0.094)	(0.379)	(0.130)
1995	0.310	0.378	1.305	1.057	0.301	0.378	1.111	1.055
	(0.148)	(0.152)	(0.567)	(0.169)	(0.189)	(0.153)	(0.583)	(0.170)
1996	0.356	0.580	1.740	1.529	0.317	0.580	1.446	1.527
	(0.185)	(0.080)	(0.801)	(0.364)	(0.190)	(0.081)	(0.866)	(0.366)
1997	0.397	0.283	2.170	1.503	0.301	0.284	1.751	1.501
	(0.221)	(0.292)	(1.06)	(0.316)	(0.223)	(0.294)	(1.26)	(0.317)
1998	0.266	0.491	2.427	1.981	0.324	0.491	2.094	1.978
	(0.244)	(0.344)	(1.25)	(0.364)	(0.222)	(0.346)	(1.45)	(0.366)
1999	0.415	0.055	2.965	1.816	0.329	0.056	2.420	1.813
	(0.245)	(0.290)	(1.52)	(0.452)	(0.261)	(0.291)	(1.78)	(0.454)
2000	0.506	−0.019	3.444	2.011	0.299	−0.019	2.709	2.008
	(0.280)	(0.223)	(1.76)	(0.606)	(0.263)	(0.224)	(2.00)	(0.610)
Dept. trend			−0.388	−0.270			−0.320	−0.270
			(0.217)	(0.094)			(0.245)	(0.094)
				B. Urban				
1994	0.196	−0.048	0.147	0.183	0.279	−0.048	0.246	0.183
	(0.129)	(0.092)	(0.295)	(0.226)	(0.134)	(0.093)	(0.384)	(0.227)
1995	0.052	0.100	−0.030	0.484	0.200	0.099	0.146	0.483
	(0.130)	(0.237)	(0.428)	(0.371)	(0.172)	(0.274)	(0.586)	(0.374)
1996	0.165	−0.040	0.051	0.498	0.345	−0.041	0.269	0.496
	(0.155)	(0.333)	(0.531)	(0.474)	(0.196)	(0.335)	(0.727)	(0.477)
1997	0.109	0.146	−0.038	−0.838	0.356	0.146	0.258	0.837
	(0.181)	(0.292)	(0.687)	(0.573)	(0.188)	(0.294)	(0.887)	(0.576)
1998	0.034	0.332	−0.145	1.177	0.333	0.331	0.213	1.176
	(0.192)	(0.266)	(0.825)	(0.655)	(0.230)	(0.268)	(1.09)	(0.659)
1999	0.053	0.329	−0.158	1.328	0.217	0.328	0.076	1.326
	(0.165)	(0.259)	(0.999)	(0.744)	(0.203)	(0.260)	(1.37)	(0.749)
2000	0.080	0.115	−0.164	1.268	0.246	0.115	0.083	1.266
	(0.141)	(0.306)	(1.16)	(0.888)	(0.170)	(0.306)	(1.59)	(0.894)
Dept. trend			0.033	−0.154			0.022	−0.154
			(0.152)	(0.119)			(0.204)	(0.120)

Notes: The table reports results of regressions with log violent death rates on left-hand side, controlling for year and age effects. The model is estimated using statistics aggregated by department, year, and ten-year age groups, for men aged 15–64. Standard errors adjusted for department clustering are in parentheses.

C. *Stacked Urban and Rural Mortality Estimates*

The link between coca production and increased death rates may run through a number of channels. Like other illegal industries, cocaine is associated with violence and intimidation. Moreover, as we noted at the outset, coca is widely believed to provide revenue for guerrilla and paramilitary groups in rural areas, either through taxation, protection rackets, or direct control of production. The resulting revenue makes it easier for violent groups to obtain weapons and recruits, and generally step up their activities, which are largely in rural areas. In contrast, most of the violence associated with the cocaine *trade* occurred in large cities (most dramatically, in Medellín). Violence that arises through increased coca *cultivation* should therefore be a bigger problem in the countryside than in cities. To substantiate this, we estimated growing-region effects on violent death rates separately for urban and rural victims.[26]

For the most part, estimates by urban/rural status indeed point to a stronger link between coca penetration and violent death rates in rural than urban areas. This is documented in table 8, which reports estimates of equation (3) for rural residents in panel A and for urban residents in panel B.[27] For example, the rural estimates of α_{0s} in column 1 are 0.31 (s.e. = 0.15) for 1995, 0.36 (s.e. = 0.19) for 1996, and 0.4 (s.e. = 0.221) for 1997. The corresponding estimates for urban deaths, reported in column 1, panel B, are 0.05, 0.17, and 0.11, none of which are significant. Overall, the contrast between columns 1 and 2 in panels A and B shows much

[26] DANE mortality files identify the type of area in which the deceased lived and the location of death. We defined urban/rural status by type of residence since hospitals where victims may die are mostly found in cities. For the purposes of our analysis, the deceased was identified as urban when residence was coded as "cabecera municipal." The urban residence variable is available only from 1992.

[27] The urban/rural distinction is used for the numerator but ignored in the population denominator. Since the model is in logs, this probably provides a reasonable approximation to an analysis of true death rates by urban/rural status. Estimates in table 8 are unweighted since we do not have departmental population estimates by urban/rural status for intercensal years. An unweighted analysis may be more appropriate in any case, since the regressor of primary interest varies at the department-year level.

TABLE 9.—MORTALITY ESTIMATES BY URBAN/RURAL RESIDENCE AND AGE

	14 Growing Departments				No Medium Producers			
	No trends		With trends		No trends		With trends	
	Age < 35 (1)	Age ≥ 35 (2)	Age < 35 (3)	Age ≥ 35 (4)	Age < 35 (5)	Age ≥ 35 (6)	Age < 35 (7)	Age ≥ 35 (8)
A. Rural								
1994	0.072	−0.029	0.067	−0.034	0.299	−0.064	0.411	0.048
	(0.222)	(0.157)	(0.263)	(0.151)	(0.200)	(0.169)	(0.228)	(0.176)
1995–1997	0.386	0.349	0.374	0.338	0.401	0.294	0.661	0.554
	(0.156)	(0.146)	(0.245)	(0.180)	(0.181)	(0.129)	(0.294)	(0.197)
1998–2000	0.414	0.313	0.392	0.292	0.435	0.173	0.913	0.651
	(0.202)	(0.244)	(0.309)	(0.249)	(0.206)	(0.229)	(0.323)	(0.262)
Trend			0.003				−0.073	
			(0.050)				(0.050)	
B. Urban								
1994	0.165	0.140	0.146	0.121	0.282	0.112	0.289	0.119
	(0.136)	(0.155)	(0.146)	(0.179)	(0.156)	(0.192)	(0.156)	(0.221)
1995–1997	0.071	0.122	0.028	0.079	0.263	0.215	0.278	0.230
	(0.149)	(0.131)	(0.190)	(0.199)	(0.163)	(0.171)	(0.209)	(0.255)
1998–2000	0.123	0.070	0.043	−0.010	0.341	0.206	0.369	0.234
	(0.153)	(0.141)	(0.280)	(0.282)	(0.154)	(0.169)	(0.348)	(0.368)
Trend			0.012				−0.004	
			(0.039)				(0.049)	

Notes: The table reports results of regressions with log violent death rates on left-hand side, controlling for year and age effects. The model is estimated using statistics aggregated by department, year, and ten-year age groups, for men aged 15–64. Standard errors adjusted for department clustering are in parentheses.

larger effects in rural than urban areas. This pattern also persists in models that include trends (results reported in columns 3 and 4), with some of the effects on rural areas large and significant (and the corresponding trends large and negative). The contrast in results by urban/rural status using a sample that omits medium producers, reported in columns 5–8, generally also shows marked larger effects in rural areas, with the exception of some entries in column 5.

D. *Estimates by Age and Previous Guerrilla Presence*

The possibility of differential effects by demographic group and across departments motivates two further analyses, one by age and one allowing for a distinction between departments with and without a strong preexisting guerrilla presence. Violence caused by the criminal activities of those involved in the cocaine industry may be concentrated among young men. Consistent with this, we found no evidence of an effect of coca expansion on violent deaths among women. Civilian victims of the Colombian civil conflict, on the other hand, seem to come from all age groups, though they are also mostly male (see, for example, the accounts in Kirk, 2003). We therefore estimated a version of the models used to construct the estimates in table 8, pooling interaction terms across growing regions and years as in table 6, but allowing effects to differ for men under and over 35. These estimates, reported in table 9, show essentially similar impacts on younger and older men. This supports the view that the accelerating violence in growing regions is probably more conflict-related than criminal in nature.

Models allowing for separate trends and separate treatment effects in areas with a preexisting guerrilla presence were estimated both as a robustness check and to explore the variability in treatment effects across departments. This is motivated by the fact that a strong preexisting guerrilla presence is prima facie evidence of an environment hospitable to insurgents. The baseline strength of the guerrilla presence is measured in two ways, first using the number of FARC attacks in 1994, and second, summing attacks by three guerrilla groups plus the AUC (a paramilitary group) in 1995. In particular, we coded two dummies for departments with guerrilla activity above the median for these measures.[28] The estimates come from a pooled specification similar to that used to construct the estimates in table 6. In this case, however, in addition to the treatment-effect interactions, the models allow for 1994, 1995–1997, and 1998–2000 interactions with guerrilla activity dummies to control for trends specific to (both growing and nongrowing) departments with a strong preexisting guerrilla presence.

The relationship between coca growth and accelerating violence is stronger for departments with substantial early guerrilla activity. This is documented in table 10, which reports estimates from models using both measures of guerrilla activity. For example, when growing regions are defined using the fourteen-department definition, estimates using the first measure show no effect of coca growth on violence in departments without much early activity. Models estimated without medium producers generate effects in both types of departments, but the effects are consistently

[28] In principle, an earlier measure of guerrilla activity might be preferable, but data for the earlier period are spotty. We note, however, that there appears to be considerable overlap between the 1994 and 1995 measures used here and the departments listed as guerrilla strongholds in an earlier study by the Federal Research Division (1988) of the Library of Congress.

TABLE 10.—MORTALITY ESTIMATES BY PREVIOUS GUERRILLA PRESENCE

	14 Growing Departments				No Medium Producers			
	No trends		With trends		No trends		With trends	
	Low (1)	High (2)	Low (3)	High (4)	Low (5)	High (6)	Low (7)	High (8)
A. By 1994 FARC								
1994	−.083	.116	−.088	.112	.269	.071	.380	.182
	(.288)	(.156)	(.317)	(.157)	(.361)	(.096)	(.391)	(.095)
1995–1997	.073	.593	.064	.583	.277	.458	.537	.717
	(.191)	(.173)	(.281)	(.184)	(.138)	(.163)	(.256)	(.233)
1998–2000	.017	.550	−.001	.532	.106	.351	.582	.829
	(.190)	(.338)	(.362)	(.279)	(.108)	(.331)	(.340)	(.310)
Trend			.003				−.073	
			(.049)				(.050)	
B. By 1995 FARC/ELN								
1994	−.296	.208	−.303	.201	−.176	.268	−.068	.375
	(.158)	(.176)	(.175)	(.202)	(.116)	(.190)	(.128)	(.210)
1995–1997	.271	.477	.254	.460	.256	.441	.508	.689
	(.093)	(.189)	(.211)	(.230)	(.098)	(.185)	(.218)	(.266)
1998–2000	.073	.633	.043	.603	.087	.514	.547	.974
	(.272)	(.242)	(.394)	(.283)	(.304)	(.219)	(.433)	(.282)
Trend			.005				−.071	
			(.049)				(.049)	

Notes: The table reports results of regressions with log violent death rates on left-hand side, controlling for year and age effects. The model is estimated using statistics aggregated by department, year, and ten-year age groups, for men aged 15–64. Columns labeled "low" denote departments with below-median measures of guerrilla presence, while columns labeled "high" denote departments with above-median measures of guerrilla presence. Standard errors adjusted for department clustering are in parentheses.

larger in departments that had substantial earlier guerrilla activity. These findings are therefore consistent with the view that an initial guerrilla presence facilitated the increase in insurgent activities due to coca.

VI. Summary and Conclusions

The disruption of the Andean air bridge provides an opportunity to assess the impact of coca production on Colombia's rural population and to study the link between a resource boom and violence. On the economic side, we find some evidence of increases in self-employment income, though not in the likelihood of having income from this source, in the probability of working more generally, or in wage and salary earnings. The increase in self-employment income is estimated to be on the order of 13–29 log points, a substantial gain. There is also some evidence of an increase in boys' labor supply.

Because the gains to increased coca cultivation appear to be fairly concentrated, it seems unlikely that increased coca production raised overall standards of living in growing areas. The absence of wider gains may be due to the fact that coca has few links with other sectors, or to extortion on the part of insurgents and paramilitary forces and the fact that coca finances a conflict that reduces economic activity.[29] Consistent with this latter story, our results show increased violent death rates in growing areas after the increase in coca cultivation, though it should be noted that these results are weaker in models that include department trends. On the other hand, the findings are reasonably consistent in models with time-varying controls for rainfall and various other trend controls.

We cannot conclusively identify the channels through which coca cultivation might abet violence, but differences in effects by urban/rural status are consistent with the notion that coca supports rural insurgents and paramilitaries. The fact that older and younger men are similarly affected also points to the civil conflict, as opposed to criminal activity, as a primary cause of increased rural violence, as does the finding that violence increased more in the rural parts of growing departments with substantial previous conflict. Remarkably, the increase in rural violence occurred against a backdrop of generally improving public health as measured by death rates from disease.

Our results provide an interesting case study of a situation where increases in income did not lead to a reduction in civil conflict, but rather fueled the fires of unrest. This contrasts with the more optimistic picture in Miguel, Satyanath, and Sergenti (2004), but is in line with journalistic accounts of the role played by blood diamonds in Africa's civil wars and economic theories of insurrection as extraction or extortion (for example, Grossman, 1991; Collier & Hoeffler, 2004). Coca may indeed be emblematic of a resource curse associated with goods for which there is a well-developed black market.

[29] Pshisva and Suárez (2004) suggest that the risk of kidnapping reduces investment by Colombian firms.

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APPENDIX A

TABLE A1.—ROBUSTNESS CHECKS FOR LABOR MARKET OUTCOMES

	Male and Female Workers				Men Only				Teenage Boys	
	Positive SE Income		Log SE Income		Log Hours		Log Monthly Wage		Log Hours	
Interaction Terms	14 Growing (1)	No Med. Prod. (2)	14 Growing (3)	No Med. Prod. (4)	14 Growing (5)	No Med. Prod. (6)	14 Growing (7)	No Med. Prod. (8)	14 Growing (9)	No Med. Prod. (10)
Panel A: Rainfall Control, No Trends										
1995–1997	0.037	0.060	0.234	0.249	0.019	0.017	−0.001	0.017	0.107	0.112
	(0.021)	(0.026)	(0.119)	(0.162)	(0.017)	(0.024)	(0.036)	(0.055)	(0.039)	(0.037)
1998–2000	0.033	0.058	0.256	0.177	0.039	0.034	0.082	0.086	0.232	0.179
	(0.021)	(0.021)	(0.126)	(0.115)	(0.017)	(0.018)	(0.041)	(0.057)	(0.045)	(0.033)
Panel B: Rainfall Control and Linear Department Type Trend										
1995–1997	0.004	−0.004	0.284	0.287	0.009	0.015	−0.026	−0.054	0.123	0.121
	(0.018)	(0.014)	(0.158)	(0.155)	(0.030)	(0.025)	(0.059)	(0.062)	(0.059)	(0.079)
1998–2000	−0.032	0.068	0.357	0.253	0.018	0.03	0.037	−0.043	0.265	0.197
	(0.035)	(0.022)	(0.289)	(0.307)	(0.056)	(0.032)	(0.131)	(0.149)	(0.140)	(0.176)
Trend	0.011	0.021	−0.017	−0.013	0.003	0.001	0.008	0.024	−0.006	−0.003
	(0.007)	(0.006)	(0.049)	(0.048)	(0.010)	(0.006)	(0.024)	(0.031)	(0.021)	(0.027)
Panel C: Initial GSP Trend										
1995–1997	0.031	0.054	0.246	0.260	0.029	0.028	−0.017	0.001	0.108	0.110
	(0.023)	(0.030)	(0.137)	(0.177)	(0.023)	(0.031)	(0.039)	(0.053)	(0.044)	(0.045)
1998–2000	0.022	0.049	0.279	0.193	0.057	0.055	0.050	0.051	0.234	0.169
	(0.025)	(0.029)	(0.171)	(0.179)	(0.013)	(0.036)	(0.059)	(0.076)	(0.062)	(0.060)
Trend	−0.003	−0.002	0.007	0.004	0.005	0.005	−0.007	−0.007	0.001	−0.002
	(0.004)	(0.004)	(0.028)	(0.029)	(0.005)	(0.005)	(0.010)	(0.010)	(0.009)	(0.009)
Panel D: Initial FARC Presence Trend										
1995–1997	0.034	0.059	0.283	0.268	0.016	0.015	−0.003	0.018	0.133	0.117
	(0.022)	(0.026)	(0.125)	(0.154)	(0.020)	(0.026)	(0.037)	(0.053)	(0.045)	(0.040)
1998–2000	0.034	0.059	0.256	0.171	0.040	0.035	0.075	0.079	0.233	0.177
	(0.021)	(0.021)	(0.130)	(0.117)	(0.018)	(0.019)	(0.043)	(0.062)	(0.047)	(0.033)
Trend	−0.002	−0.001	0.023	0.026	−0.002	−0.003	0.000	0.001	0.010	0.004
	(0.001)	(0.001)	(0.007)	(0.005)	(0.002)	(0.002)	(0.001)	(0.001)	(0.004)	(0.003)
N	148,306	126,358	40,338	34,111	69,144	58,314	34,451	29,275	12,528	10.685

Notes: The table reports estimates from models and samples similar to those used to construct the estimates in tables 4B and 5B, with the addition of trend and control variables as indicated.

Table A2.—Additional Mortality Estimates

	14 Growing Departments				No Medium Producers			
	No trends		With trends		No trends		With trends	
	No Ctls. (1)	Rainfall (2)	No Ctls. (3)	Rainfall (4)	No Ctls. (5)	Rainfall (6)	No Ctls. (7)	Rainfall (8)
	A. Unweighted							
1993	−.025	−.025	−.024	−.023	−.017	−.022	−.014	−.018
	(.158)	(.146)	(.157)	(.145)	(.162)	(.147)	(.161)	(.146)
1994	.129	.111	.131	.113	.136	.115	.141	.120
	(.174)	(.148)	(.175)	(.149)	(.180)	(.152)	(.182)	(.155)
1995	.248	.227	.251	.230	.237	.216	.243	.222
	(.157)	(.145)	(.158)	(.146)	(.164)	(.152)	(.164)	(.153)
1996	.283	.275	.286	.278	.281	.268	.289	.276
	(.163)	(.158)	(.165)	(.160)	(.175)	(.169)	(.178)	(.172)
1997	.305	.316	.309	.320	.315	.325	.325	.335
	(.182)	(.194)	(.182)	(.194)	(.192)	(.202)	(.191)	(.202)
1998	.536	.535	.540	.540	.481	.479	.492	.491
	(.201)	(.204)	(.200)	(.203)	(.203)	(.206)	(.202)	(.204)
1999	.251	.254	.256	.260	.281	.282	.293	.296
	(.135)	(.135)	(.137)	(.138)	(.142)	(.141)	(.146)	(.145)
2000	.240	.234	.245	.241	.289	.277	.303	.291
	(.134)	(.138)	(.139)	(.144)	(.141)	(.144)	(.150)	(.154)
Trend			.002	.002			.003	.003
			(.005)	(.005)			(.005)	(.005)
	B. Weighted							
1993	−.199	−.198	−.205	−.203	−.184	−.184	−.188	−.187
	(.060)	(.064)	(.062)	(.066)	(.068)	(.071)	(.069)	(.072)
1994	.022	.021	.014	.013	.026	.026	.021	.020
	(.115)	(.113)	(.117)	(.116)	(.127)	(.125)	(.130)	(.128)
1995	.157	.156	.146	.145	.173	.173	.166	.166
	(.138)	(.138)	(.138)	(.138)	(.147)	(.147)	(.148)	(.147)
1996	.150	.151	.136	.137	.178	.178	.169	.169
	(.199)	(.200)	(.200)	(.201)	(.222)	(.222)	(.223)	(.224)
1997	.199	.199	.182	.183	.222	.223	.211	.212
	(.186)	(.187)	(.191)	(.192)	(.195)	(.196)	(.199)	(.200)
1998	.203	.201	.183	.182	.213	.212	.200	.199
	(.146)	(.146)	(.154)	(.153)	(.150)	(.149)	(.154)	(.154)
1999	.309	.311	.287	.289	.356	.357	.341	.342
	(.159)	(.160)	(.164)	(.165)	(.167)	(.168)	(.172)	(.172)
2000	.276	.276	.251	.251	.351	.350	.334	.334
	(.147)	(.146)	(.153)	(.152)	(.144)	(.144)	(.153)	(.153)
Trend			−.009	−.009			−.005	−.005
			(.010)	(.010)			(.009)	(.009)

Notes: The table reports estimates from models and samples similar to those used to construct the estimates in table 7, with the addition of trend and control variables as indicated.

APPENDIX B

DATA APPENDIX

1. *Colombian Rural Household Surveys*

The analysis here uses the "Encuesta Rural de Hogares," the rural component of the "Encuesta Nacional de Hogares," which became "Encuesta Continua de Hogares" (a panel) in 2001. The rural household survey was first conducted as a pilot in 1988. The survey was conducted again in December 1991 after the sampling methodology was updated and then used on a consistent basis every September starting in 1992 until 2000. The survey collects data on a representative sample from 23 departments in four rural regions: Atlantic Region (which includes the departments of Atlántico, Córdoba, Magdalena, Sucre, Cesár, La Guajira, and Bolívar); Pacific Region (which includes the departments of Chocó, Nariño, Cauca, and Valle del Cauca); the Central Region (which includes the departments of Antioquia, Caldas, Huila, Tolima, Quindío, Risaralda, and Caquetá); and the Eastern Region (which includes the departments of Norte de Santander, Santander, Boyacá, Cundinamarca, and Meta).

Rural definition. The survey uses the following criteria to identify the rural population:

(i) The population of the city where the county's government is located if the city has less than 10,000 inhabitants.
(ii) The population of the city where the county's government is located if the city has more than 10,000 inhabitants and it meets one of the following characteristics:
  (a) the percentage of residents in the city does not exceed 50% of the population in the entire county,
  (b) the percentage of the active population engaged in agricultural activities exceeds 50%, or
  (c) the percentage of housing units without basic services (water, electricity, etc.) exceeds 20%.
(iii) Everyone living in towns with less than 10,000 inhabitants.
(iv) Everyone not living in either cities or towns.

Sampling methodology. The sample for the survey is taken from the universe of the census population living in private households. The sampling methodology consists of first generating strata according to geographical location and socioeconomic level; then, randomly drawing *municipios* (the equivalent of counties in the United States) from these strata; next, randomly drawing neighborhoods from these *municipios;* and, finally, randomly drawing blocks and then households from these neighborhoods. To facilitate the collection of information, households are grouped into segments of ten households on average. The typical year includes approximately 8,500 households, but the sample has increased over time. In particular, the sample size increased in 1996. The survey collected data from 148 *municipios* in 1992–1995, but it collected data from 197 municipios in 1996–2000.

In addition, the survey methodology changed as follows in 1996. First, between 1992 and 1995 the sample was drawn from the 1985 census, while starting in 1996 and until 2000 the sample was drawn from the 1993 census. Second, starting in 1996, interviewers were required to revisit households, which generated an increase in response rates.

Sample weights. The survey weights include factors of adjustment to account for changes in subsampling and for nonresponse. So, we use the weighted data in our analysis to take account of the 1996 changes. In particular, the weights are estimated as

$$W = (I/P) \times S \times (I_S/N_S),$$

where P is the probability of an individual being sampled and S is a weight given to segments. S equals 1 unless the number of households within the segment exceeds ten. The last term is the ratio of the number of households actually interviewed within a segment, I_S, and the number of households selected for interviewing within a segment, N_S, so it captures the response rate within a segment.

Since the average number of children per household is around three, we generate within-household averages for the children's data in order to avoid multiple observations per household. Likewise, since the weights are individual weights, we construct household weights by summing up the individual weights for all children within the household.

Top-coding and imputation. Labor market information is collected from individuals aged 10 and up. We impute zeros for the employment and hours of 8- and 9-year-olds in the descriptive statistics in table 5. Hours are collected from all employed workers, including salaried and wage workers as well as self-employed workers. Wage and salary earnings were collected for all jobs in 1992–1999. In 2000, wage and salary earnings were collected separately for the main job and for secondary jobs, so we exclude 2000 from the wage and salary regressions. Yearly self-employment income is collected separately as earnings from business and commercial activities and family-based agricultural production. In the original data, earnings and self-employed income were top-coded only between 1992 and 1995. We impose uniform top-coding by applying a cap at the 95th percentile (including zeros) for each year. In addition, we imputed the mean earnings and self-employment income by department and year for all those individuals who reported having earnings or self-employed income but did not report an amount.

2. *Colombian Urban Household Surveys*

We also use the urban component of the "Encuesta Nacional de Hogares," from 1992 to 2000. Coverage for the urban component of the survey is more limited than for the rural survey. In particular, the urban survey excluded the departments of Magdalena, Caquetá, and Chocó in 1992–1995. For the urban/rural analysis we used the twenty departments that are covered in both surveys in 1992–2000. Also, because Caquetá, one of the two DMZ departments, is not in the urban survey during the initial years, the urban/rural analysis pools Meta, the other DMZ department, with the non-DMZ growing departments, so that these specifications include only non-DMZ growing interaction terms.

The sampling methodology and sample weights used in the urban component are similar to those used in the rural component. The urban population includes all people not included under the rural definition.

As in the rural component of the survey, wage and salary earnings were collected for all jobs in 1992–1999, but in 2000 wage and salary earnings were collected separately for the main job and for secondary jobs, so the 2000 data are excluded from the wage and salary regressions. Yearly self-employment income is collected separately as earnings from business and commercial activities and the sale of domestically produced goods. In the urban component of the survey, earnings and self-employed income were top-coded only between 1992 and 1996, so we apply the same uniform top-coding as used in the rural survey by applying a cap at the 95th percentile (including zeros) for each year. We also imputed mean earnings and self-employment income by department and year for all those individuals who reported having earnings or self-employed income but did not report an amount.

3. *Mortality Detail Files*

We obtained mortality detail files from the Colombian national statistical agency, DANE, for 1990–2001. These files, the source of published vital statistics (such as http://www.dane.gov.co/inf_est/vitales.htm), show individual death records, with basic demographic information on the deceased and cause of death. The 1990 and 1991 files did not include reliable urban/rural codes and are therefore omitted from the sample used to construct table 8. The 2001 file also had some inconsistencies (the file was provisional) and was therefore dropped.

Cause of death. We aggregate detailed causes of death on a consistent basis from year to year into the following larger groups: homicide and suicide, accident and other nonviolent trauma, disease, other causes, and other violent deaths. The violent death rate used here is the sum of homicide and suicide plus other violent deaths. Data after 1997 show separate categories for general external causes not identified as accidents and deaths due to actions by state and guerrilla forces. These two categories appear to correspond to the "other violence category" from previous years.

Location information. Our construction of death rates by department and year is for department of death and not residence. Urban/rural status, however, is by area of residence. This is coded somewhat differently from year to year. We established a consistent urban/rural status by coding as urban those listed as living in "*cabecera municipal*" and coding the institutionalized as nonurban. Those with missing urban/rural status (about 1/16 of deaths) are omitted from the analysis used to produce table 8.

Match to population information. As noted in the text, population statistics for each department-year-age (five year)-sex category were obtained from DANE (1998) for 1990, 1995, and 2000. The Colombian census used for these data was conducted in 1993, so for other years we use intercensal estimates and projections. We interpolated using five-year growths for each cell.

Finally, we aggregated mortality counts to match five-year age bands, and then matched to the relevant population denominators.

4. *Miscellaneous*

Farm hectares used to calculate percentage of cultivated land devoted coca in the text discussion come from various published and Web sources distributed by the Colombian statistical agency (DANE) and the Ministry of Agriculture. Details are available on request.

Data on GSP are from the DANE Web site. We divided aggregate GSP by the 1993 population figures in table 2 to get per capita GSP.

Data on 1994 FARC and 1995 aggregate guerrilla activity come from police records and were kindly provided to us by Gustavo Suárez. These data measure the number of attacks/actions taken by each of these groups, including terrorist activities, extortion, attacks on rural and urban populations, harassment, attacks on infrastructure, attacks on airplanes, arms smuggling, and confrontations with other groups. The 1995 data sum actions by the FARC, ELN, and EPL guerrilla groups, plus the paramilitary AUC. We coded dummies for departments above the median of each (8 for FARC in 1994, 22 for total in 1995). Half of the fourteen growing departments had substantial previous guerrilla activity using the 1994 measure; eight of fourteen growing departments had substantial previous guerrilla activity using the 1995 measure.

Data on rainfall were purchased from the Colombian Institute of Hydrology, Meteorology, and Environmental Studies (IDEAM). These data measure yearly precipitation measured in centiliters as well as the total number of days of precipitation during the year. We have information for all departments for the station corresponding to the main airport in each department. The data include complete yearly data for the period from 1986 to 2005, thus covering the entire period for which we have household and homicide data.

APPENDIX C

FIGURE C1.—GROWING, NONGROWING, AND DMZ REGIONS IN COLOMBIA