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## Trafficking Networks and the Mexican Drug War<sup>†</sup>

By MELISSA DELL\*

*Drug trade-related violence has escalated dramatically in Mexico since 2007, and recent years have also witnessed large-scale efforts to combat trafficking, spearheaded by Mexico's conservative PAN party. This study examines the direct and spillover effects of Mexican policy toward the drug trade. Regression discontinuity estimates show that drug-related violence increases substantially after close elections of PAN mayors. Empirical evidence suggests that the violence reflects rival traffickers' attempts to usurp territories after crackdowns have weakened incumbent criminals. Moreover, the study uses a network model of trafficking routes to show that PAN victories divert drug traffic, increasing violence along alternative drug routes. (JEL D72, D85, K42, O17, Z13)*

Violence related to the drug trade has escalated dramatically in Mexico since 2007, claiming over 60,000 lives and raising concerns about the capacity of the state to monopolize violence. Recent years have also witnessed large-scale efforts to combat drug trafficking, spearheaded by Mexico's conservative National Action Party (PAN). These efforts have cost around 9 billion USD per annum, nearly as much as the government expends on social development.<sup>1</sup> Yet there is limited causal evidence about the impacts of crackdowns. This study uses plausibly exogenous variation from close Mexican mayoral elections, a network model of drug trafficking, and confidential data on the drug trade to identify how crackdowns have affected violence and trafficking. It examines both the direct effects of crackdowns in the places experiencing them and the spillover effects they exert by diverting drug traffic elsewhere.

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<sup>1</sup>See Estados Unidos de Mexico, Gobierno Federal (2010) and Keefer and Loayza (2010). Social development encompasses a variety of anti-poverty programs, the most prominent of which is the extensively evaluated Progresa/Oportunidades conditional cash transfer program.

Mexico is the largest supplier to the US illicit drug market, with Mexican traffickers earning approximately 25 billion USD each year in wholesale US drug markets (United Nations Office on Drugs and Crime (UNODC) 2011). Official data described later in this study document that in 2008, drug trafficking organizations maintained operations in two-thirds of Mexico's municipalities, and illicit drugs were cultivated in 14 percent of municipalities.

While Mexico is a major player in the drug trade, its high levels of drug violence and drug enforcement expenditures are not unique. Global annual drug enforcement spending exceeds 100 billion USD, and traffickers in Central America, West Africa, and elsewhere use violent tactics and often belong to the same transnational trafficking organizations that operate in Mexico (Count the Costs 2013). Because law enforcement does not randomly decide where to crack down, the existing evidence on drug enforcement impacts consists primarily of correlations. While often the best evidence available, these can be nontrivial to interpret. For example, a positive cross-sectional correlation between violence and drug enforcement could result because areas with higher violence attribute it to drug consumption and thus expend more fighting the drug trade, and a positive correlation in a panel could occur because governments crack down in places where they expect violence to later increase.

This study isolates plausibly exogenous variation in drug enforcement policy by exploiting the outcomes of 2007–2010 close mayoral elections involving the PAN party.<sup>2</sup> The PAN federal government's role in spearheading the war on drug trafficking, as well as qualitative evidence that PAN mayors have contributed to these efforts, motivate this empirical strategy. While municipalities where PAN candidates win and lose by wide margins are likely to be different, when focusing on close elections it becomes plausible that election outcomes are driven by idiosyncratic factors. The study shows that the outcomes of close elections are in fact uncorrelated with baseline municipal characteristics and violence trends.

Regression discontinuity estimates document that there are 27 to 33 more drug trade-related homicides per 100,000 municipal inhabitants per annum after a PAN mayor takes office in a municipality than after a non-PAN mayor takes office, and these effects persist throughout the mayor's term and plausibly beyond. Relative to the pre-period, drug trade-related violence increases by a factor of 5.5 in municipalities with a close PAN victory, as compared to doubling in municipalities with a PAN loss. These results are robust to alternative specifications, samples, and lengths of the analysis period and to using the overall homicide rate, which is balanced throughout the 17 years of available pre-period data. Most of these homicides were not prosecuted, as just under 20 percent of homicides in Mexico result in an arrest (México Evalúa 2012). Homicide is typically in the jurisdiction of Mexico's state-level criminal justice systems, and in several of the most violent states the clearance rate for homicide is only 3 to 7 percent. RD evidence also documents that police-criminal confrontations increase substantially following PAN inaugurations.

<sup>2</sup>See Lee, Moretti, and Butler (2004) for a pioneering example of a regression discontinuity (RD) design exploiting close elections. A number of studies have used discontinuous changes in policies, in the cross section or over time, to examine illicit behavior. See Zitzewitz (2012) for a detailed review.

Over 85 percent of the total drug-related violence consists of drug traffickers killing each other, and the increases in violence are concentrated in municipalities that are plausibly the most valuable for drug trafficking organizations to control. Analysis using information on the industrial organization of trafficking suggests that the violence reflects rival traffickers' attempts to wrest control of territories after crackdowns initiated by PAN mayors have weakened the incumbent traffickers. The violence does not appear to be explained by differences in corruption between the PAN and other parties or by several other alternative hypotheses that are explored.

These results provide novel evidence that crackdowns caused large and sustained increases in the homicide rate. They contribute to a literature—consisting primarily of cross-sectional and panel correlations across countries or cities, particularly within the United States—which on balance documents a positive relationship between drug enforcement and violence. Miron (forthcoming); Werb et al. (2011); and Keefer and Loayza (2010) offer detailed reviews.<sup>3</sup>

The study's results also compliment qualitative and descriptive studies arguing that Mexican government policy has been the primary cause of the large spike in violence in recent years (Guerrero 2011; Escalante 2011; Merino 2011), as well as recent work by Durante and Gutierrez (2013), who use close Mexican elections to argue that coordination across municipalities can reduce drug violence.<sup>4</sup> The findings contrast with studies arguing that the increase in Mexican violence can primarily be explained by the diversification of drug trafficking organizations into new criminal activities, by increased arms availability, by increased US deportation of immigrants with a criminal record, by job loss, or by cultural shifts in morality (see Williams 2012; Escalante et al. 2011; Rios 2013 for a discussion).

Finally, the results on mechanisms relate to work by Angrist and Kugler (2008) documenting that exogenous increases in coca prices spurred violence in rural districts in Colombia because combatant groups fought over the additional rents. In Mexico, crackdowns likely reduce rents from criminal activities while in effect, but by weakening the incumbent criminals they also reduce the costs of taking control of a municipality. Controlling territory plausibly offers substantial rents from trafficking and a variety of other criminal activities once the crackdown and its violent aftermath subside. While the study shows that the violent effects of crackdowns are sustained in the medium run, trafficking organizations may have a longer run horizon or might have underestimated how long gang wars would last.

When policy leads one location to become less conducive to illicit activity, organized crime may relocate elsewhere, affecting violence in other municipalities.<sup>5</sup> Examining these spillovers is central to understanding the broader impacts of crackdowns. A number of studies have examined the drug trade and organized crime, but

<sup>3</sup>Examples of cross-country studies include Miron (2001) and Fajnzylber, Lederman, and Loayza (1998). Examples of studies within the United States include Brumm and Cloninger (1995); Miron (1999); and Rasmussen, Benson, and Sollars (1993).

<sup>4</sup>Guerrero (2011) compiles extensive qualitative and descriptive evidence suggesting that government policies have ignited violent conflicts between traffickers. Escalante (2011) documents a state-level correlation between homicides and the deployment of the Mexican military and federal police, and Merino (2011) expands Escalante's analysis by using a matching strategy to argue that Mexican homicides in 2008–2009 would have totaled 14,000 rather than 19,000 in the absence of intervention.

<sup>5</sup>For example, in a non-drug-related context, work by Di Tella and Schargrodsky (2004) documents that the allocation of police officers to Jewish institutions in Buenos Aires substantially reduced auto theft in the immediate vicinity but may also have diverted some theft to as close as two blocks away.

to the best of my knowledge this study is the first to empirically identify spillover patterns in drug trade activity.<sup>6</sup> To do so, the study develops a network model of trafficking routes that predicts where drug traffic—and potentially drug violence—is diverted following crackdowns.

A trafficker's objective is to minimize the costs of transporting drugs from producing municipalities in Mexico across the road network to US entry points. In the simplest version of the model, traffickers take the most direct route to the United States that avoids municipalities with a closely elected PAN mayor. PAN victories increase the costs of trafficking drugs through a municipality, diverting least-cost routes elsewhere. Because elections occur at different times during the sample period, this generates plausibly exogenous within-municipality variation in predicted routes throughout Mexico. This variation from close elections is essential to identifying spillovers because it breaks the well-known reflection problem, allowing spillovers to be isolated from correlations (Manski 1993). Traffickers may care about the routes other traffickers take, and thus the study also estimates a richer model that imposes congestion costs when routes coincide. The approach is illustrated in Figure 1, and predicted routes for the beginning of the sample period are shown in Figure 2.

When a municipality acquires a predicted route, the monthly probability that a drug trade-related homicide occurs increases from a baseline probability of 4.8 percent to 6.2 percent, and for each additional route the drug trade-related homicide rate increases by 0.54 per 100,000. There is some evidence that violence spillovers are concentrated in municipalities where multiple routes coincide. While the magnitudes of violence spillovers are typically small compared to the direct effects of crackdowns, they are nevertheless important given the gravity of homicides.

The study also examines how within-municipality variation in predicted routes compares to within-municipality variation in actual illicit drug seizures. This exercise can validate whether the trafficking model is predictive, as changes in drug confiscations are not used to estimate the routes. The value of drug seizures increases by 18 percent when a municipality acquires a predicted trafficking route, and this estimate is significant at the 1 percent level. Robustness and placebo checks also support the validity of the network approach.

In addition to validating the model, the drug seizures results can shed some light on the "diversion hypothesis," which argues that when the government cracks down in one place, drug activity is partially diverted elsewhere without being substantially reduced. The diversion hypothesis plays a major role in debates over the War on Drugs and is a leading explanation popularly given for why—despite a massive increase in drug enforcement expenditures globally over the past 40 years—drug markets have continued to expand and drug use has not declined (UNODC 2010; Reuter and Trautmann 2009). For example, descriptive evidence suggests that coca eradication policies in Bolivia and Peru during the 1990s led cultivation to shift to

<sup>6</sup>Studies of the economics of organized crime include Levitt and Venkatesh's (2000) analysis of the finances of a US drug gang; Gambetta's (1993) economic analysis of the Sicilian mafia; Pinotti's (2012) study of the economic costs of organized crime in southern Italy; Bandiera (2003); Dimico, Isopi, and Olsson's (2012); and Buonanno et al.'s (2012) investigations of the historical origins of the Sicilian mafia; Varese's (2005) and Frye and Zhuravskaya's (2000) analyses of the rise of the Russian mafia; and Mastrobuoni and Patacchini's (2012) study of the network structure of crime syndicates.

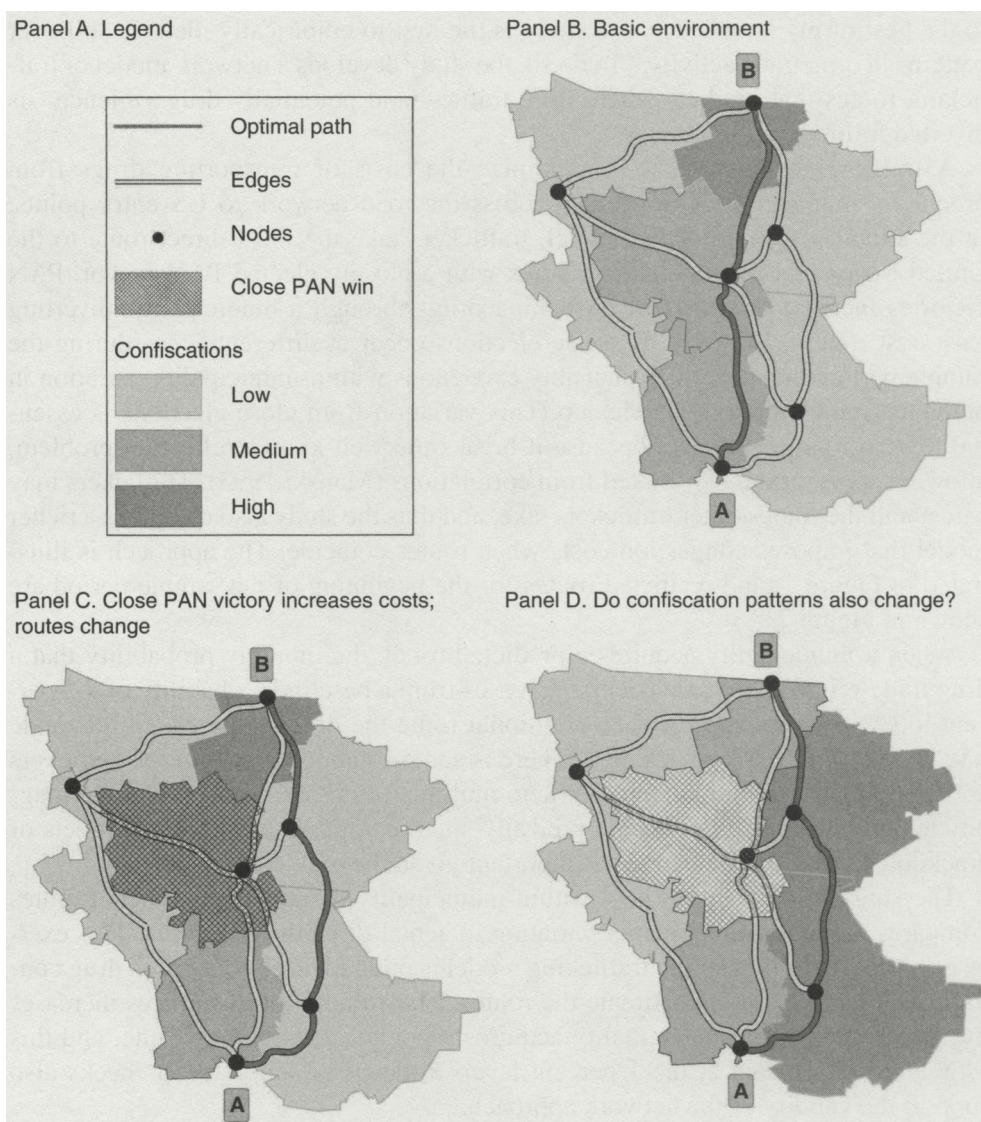


FIGURE 1. ILLUSTRATION OF SPILLOVERS METHODOLOGY

Colombia, and large-scale coca eradication in Colombia in the 2000s has since led cultivation to re-expand in Peru and Bolivia, with total coca cultivation unchanged (Isacson 2010; Leech 2000).<sup>7</sup>

The spillover results and the descriptive patterns of drug consumption suggest that it is unlikely that the Mexican drug war led to large, sustained reductions in the consumption of illicit drugs. Estimating the share of drugs diverted and the share no longer trafficked using the spillovers model would require making potentially

<sup>7</sup>The diversion hypothesis may be particularly applicable to drug trafficking—to the extent that trafficking can be substituted across different locations without significantly impeding traffickers' objectives—and will be less applicable to crimes that can only be conducted in a specific location.

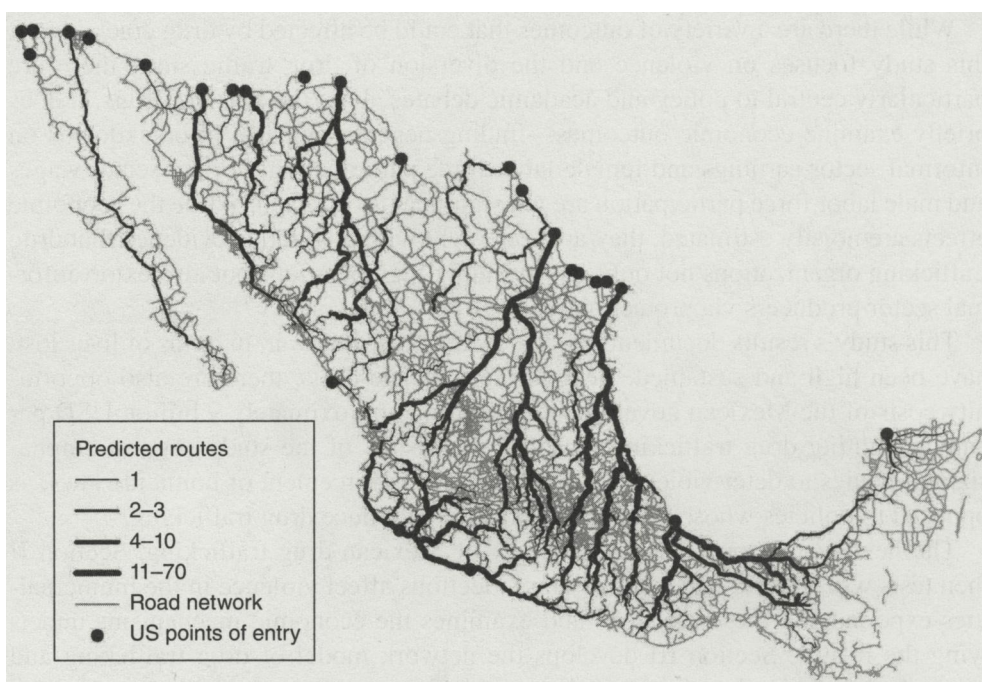


FIGURE 2. ROAD NETWORK AND PREDICTED TRAFFICKING ROUTES

*Note:* The least-cost routes plotted in this figure are predicted using the network model with congestion costs.

untenable assumptions about drug cultivation and seizure rates. Nevertheless, the diversion of drug traffic following crackdowns was large enough to substantially increase illicit drug seizures along predicted alternative routes. Additionally, many law enforcement resources used for drug seizures and eradication were redirected during the Calderón administration to focus on the unexpected escalation in violence, potentially reducing the crackdown's impact on drug prices to the extent that seizures and eradication reduce supply more than efforts to quell gang wars (National Drug Intelligence Center 2010). Moreover, the share and overall quantity of Mexican heroin in the US heroin market actually increased during the Calderón crackdown, and the availability of other drugs is unlikely to have declined substantially (Díaz-Briseño 2010).<sup>8</sup> While the counterfactual is not clear, diversion could contribute to this descriptive pattern. Existing evidence supports the conjecture that the price impacts of large-scale crackdowns tend to be modest. For example, Kuziemko and Levitt (2004) find that the massive 15-fold increase in incarceration for drug-related offenses in the United States between 1980 and 2000 led to a 5 to 15 percent increase in cocaine prices and conclude that the increase in incarceration is unlikely to have been cost effective.<sup>9</sup>

<sup>8</sup> Mexican wholesale cocaine already nearly completely dominated the market, and the Mexican share remains high (US Drug Enforcement Agency 2011). The share of Mexican marijuana in the market is difficult to measure since marijuana can be home-produced.

<sup>9</sup> The cocaine elasticity of demand is around  $-0.5$  (Reuter and Trautmann 2009), implying that effects on demand are unlikely to have exceeded 2.5 to 7.5 percent.

While there are a variety of outcomes that could be affected by drug crackdowns, this study focuses on violence and the diversion of drug traffic since these are particularly central to policy and academic debates about the War on Drugs. It does briefly examine economic outcomes—finding negative impacts of crackdowns on informal sector earnings and female labor force participation. Formal sector wages and male labor force participation are not significantly affected. While the economic effects are noisily estimated, they are consistent with qualitative evidence that drug trafficking organizations not only set up smuggling operations but also extort informal sector producers via protection rackets.

This study's results document that the costs of the drug war, in terms of lives lost, have been high and sustained. Beyond the violence costs, there are also opportunity costs of the Mexican government expending approximately 9 billion USD per annum fighting drug trafficking. Overall, the results of the study suggest emphasizing policies to deter violence and improve the enforcement of homicide laws, as opposed to policies whose primary objective is to reduce drug trafficking.

The next section provides an overview of Mexican drug trafficking. Section II then tests whether the outcomes of close elections affect violence in the municipalities experiencing these elections and examines the economic mechanisms underlying the results. Section III develops the network model of drug trafficking and tests whether PAN victories exert drug trafficking and violence spillovers. Finally, Section IV concludes.

## I. Drugs and Violence in Mexico

### A. *The Drug Trafficking Industry*

Mexican drug traffickers dominate the wholesale illicit drug market in the United States, earning between 14 and 48 billion USD annually.<sup>10</sup> According to the UN World Drug Report, Mexico is the largest supplier of heroin to US markets and the largest foreign supplier of marijuana and methamphetamine. Official Mexican government data, obtained from confidential sources, document that 14 percent of Mexico's municipalities regularly produce opium poppy seed (heroin) or cannabis. Moreover, 90 percent of cocaine consumed in the United States transits through Mexico (US Drug Enforcement Agency 2011). The US market provides substantially more revenue than Mexico's domestic drug market, which is worth an estimated 560 million USD annually (Secretaría de Seguridad Pública 2010).<sup>11</sup>

At the beginning of this study's sample period, in December 2006, there were six major drug trafficking organizations (DTOs) in Mexico. Official data obtained from confidential sources document that 68 percent of Mexico's 2,456 municipalities were known to have a major DTO or local drug gang operating within their limits in early 2008. These data also estimate that 49 percent of drug producing

<sup>10</sup>Estimates are from the US State Department (2009). Estimates by US Immigration and Customs Enforcement, the US Drug Enforcement Agency, and the Mexican Secretaría de Seguridad Pública are broadly similar and also contain a large margin of error.

<sup>11</sup>According to the US National Survey on Drug Use and Health, 14.2 percent of Americans (35.5 million people) have used illicit drugs during the past year, as contrasted to 1.4 percent of the Mexican population (1.1 million people) (CONADIC 2008).



municipalities were controlled by a major trafficking organization, with the remaining controlled by local gangs.

The term “cartel,” used colloquially to refer to DTOs, is a misnomer, as these organizations do not collude to reduce illicit drug production or to set prices. Alliances between DTOs are highly unstable, and there is considerable decentralization and conflict within DTOs (Williams 2012; Guerrero 2011, p. 10, 106–108). Decisions about day-to-day operations are typically made by local cells, as this ensures that no single trafficker will be able to reveal extensive information if captured by authorities. Moreover, the number of major DTOs increased from 6 in 2007 to 16 by 2011, with groups splitting over leadership disputes.

In addition to trafficking drugs, Mexican DTOs engage in a host of illicit activities that range from protection rackets, kidnapping, human smuggling, and prostitution to oil and fuel theft, money laundering, weapons trafficking, arson, and auto theft (Guerrero 2011, p. 10). Notably, protection rackets involving the general population have increased substantially in recent years (Rios 2011; Secretariado Ejecutivo del Sistema Nacional de Seguridad Pública 2011). The poor, who have limited recourse to state protection, are particularly likely to be extorted (Díaz-Cayeros et al. 2011).

The second half of the 2000s witnessed large, rapid increases in drug trade-related violence. Over 50,000 people were killed by drug trade-related violence between 2007 and 2012, and homicides increased by at least 30 percent per year during most of this period (Rios 2011). By 2010, violent civilian deaths per capita had reached levels higher than in Iraq and Afghanistan during the same period, higher than in Russia during the 1990s, and higher than in Sicily following the Second World War (Williams 2012). Over 85 percent of the drug-related violence consisted of people involved in the drug trade killing each other, 95 percent of the victims were male, and 45 percent were under age 30. The violence has been public and brutal, with bodies hung from busy overpasses and severed heads placed in public spaces (Williams 2012). Public displays of brutality and activities such as kidnapping and extortion affect the general public, and 2011 public opinion surveys found that security was more likely than the economy to be chosen as the largest problem facing the country.

### *B. Mexico's War on Drug Trafficking*

Combating drug trafficking has been a major priority of the Mexican federal government in recent years. Notably, President Felipe Calderón (December 2006–2012) of the conservative National Action Party (PAN) made fighting organized crime the centerpiece of his administration. During his second week in office, Calderón deployed 6,500 federal troops to combat trafficking, and by the close of his presidency approximately 45,000 troops were involved.

For most of the twentieth century, a single party, the Institutionalized Revolutionary Party (PRI), dominated Mexican politics. The PRI historically took a passive stance toward the drug trade, and widespread drug-related corruption is well-documented (Shannon 1988; Chabat 2010). Mexicans elected their first opposition president in 2000, and today Mexico has three major parties: the PAN on the right, the PRI, and the Party of the Democratic Revolution (PRD) on the left. While presidents Ernesto Zedillo (1994–2000, PRI) and Vicente Fox (2000–2006, PAN) did implement

security reforms and crackdowns (Chabat 2010), these were on a lesser scale than Calderón's. Calderón's crackdown appears to have been unanticipated, as the 2006 presidential election—decided by an extremely narrow margin—made limited mention of security issues (Castañeda and Aguilar 2012).

Mexico's crackdown on trafficking has focused on arrests and enforcement against traffickers, whereas illicit crop eradication has declined somewhat as resources have been diverted to respond to violence (National Drug Intelligence Center 2010). Drug seizures and high-level arrests are typically made by the federal police and military, who have the requisite training and weaponry to fight heavily armed traffickers. Municipal police are relevant because they can provide local information for federal interventions, which often target specific actors about whom reliable information is available (Chabat 2010). Municipal police also serve as valuable allies for traffickers, collecting information on who is passing through a town. This information is essential for protecting criminal operations and anticipating attacks by federal authorities and rivals.<sup>12</sup> Municipal police form the largest group of public servants killed by drug-related violence (Guerrero 2011).

Mayors name the municipal police chief and set policies regarding police conduct. Thus, PAN mayors could assist Calderón's drug war by appointing amenable law enforcement authorities and by encouraging them to share information with the PAN federal government. Qualitative evidence indicates that PAN mayors under Calderón were more likely to request law enforcement assistance from the PAN federal government than their non-PAN counterparts and also suggests that operations involving the federal police and military have been most effective when local authorities are politically aligned with the federal government (Guerrero 2011, p. 70).<sup>13</sup>

PAN mayors may have been more likely to aid Calderón's war on drugs because authorities from the same party cooperate more effectively, because of differences in corruption, or because of ideology. For example, Jones (2013) argues based on fieldwork conducted in Baja California that PAN politicians not only received more military assistance than their non-PAN counterparts but also were provided with newer, more sophisticated military hardware. Corruption will be discussed quantitatively later in this study. Moreover, PAN mayors plausibly had strong career incentives to cooperate. Mexican mayors are barred from consecutive reelection, and securing a subsequent political post typically requires support from party leaders.<sup>14</sup> For example, the mayorship is a common stepping stone to national politics, and a substantial part of the Mexican Congress is elected from closed party lists.<sup>15</sup> PAN party leaders choose these lists, and if a candidate is placed high enough, she will enter the legislature (Langston and Aparicio 2008; Wuhs 2006). Parties likewise distribute

<sup>12</sup> See for example *Tribuna de Los Cabos*, Sección *El País*. "Zetas Ejecutaron a los 72 migrantes" August 26, 2010.

<sup>13</sup> For example, while drug trade-related violence initially increased in Baja California in response to a large federal intervention, the violence has since declined, and the state is frequently showcased as a federal intervention success story (Guerrero 2011). The governor of Baja California belonged to the PAN, and the federal intervention began under the auspices of a PAN mayor in Tijuana. On the other hand, in Ciudad Juárez both the mayor and governor belonged to the opposing PRI party, and conflicts and mistrust between municipal and federal police have been rampant.

<sup>14</sup> The PAN controlled the mayorship in around a third of municipalities at the beginning of the sample.

<sup>15</sup> In a survey of 1,400 representatives who had served in Mexico's lower house of Congress, 77 percent of the PAN legislators had previously been involved in municipal politics (Langston and Aparicio 2008). Two hundred of the 500 seats in the lower house of Congress and 32 of the 128 Senate seats are filled from the party lists.

millions of pesos in public campaign resources, and the federal government controls thousands of appointed posts (Langston and Aparicio 2008).

The criminal justice system is also relevant for understanding the effects of crack-downs. Like the United States, Mexico has state and federal criminal justice systems, and homicides are typically in the jurisdiction of the states. The clearance rate for homicides in Mexico is low, with just under 20 percent of homicides resulting in an arrest (México Evalúa 2012), and the national average masks substantial regional variation. In the state of Chihuahua, only 3.6 percent of homicides in 2010 resulted in an arrest by the end of the following year, with Durango and Sinaloa close behind at 4.6 percent and 7 percent, respectively. In contrast, Yucatan had a clearance rate of 100 percent for the 34 homicides that it experienced in 2010 and Baja California Sur had a clearance rate of 90 percent. States with the lowest clearance rates tend to be the most violent. For comparison, the clearance rate for homicides in the United States is around 67 percent, ranging from a low of 52 percent in Michigan to a high of 89 percent in Wyoming (Investigation 2013). Low clearance rates in Mexico result from difficulties in both investigating homicides and apprehending suspects. Only 36 percent of warrants result in an arrest, with the arrest rate ranging from 14 percent to 76 percent across Mexican states (México Evalúa 2012, 2010). The federal government passed major judicial reforms in 2008, but the system remains weak.

These statistics, however, should not be interpreted as evidence that the Mexican federal government is incapable of arresting wanted criminals in a targeted manner. Drug-related crimes and organized criminal activity are often federal offenses, and nearly half of all federal inmates are charged with drug-related crimes (Lecuona 2013; México Evalúa 2013). Having the cooperation of local authorities can be central in making drug trade-related arrests, and confidential data on high-level drug arrests shows that—while rare—they are about twice as common following close PAN victories as compared to close PAN losses.

These facts can plausibly help explain why violence has increased dramatically in Mexico in recent years. With the help of municipal authorities, the federal government has used a targeted strategy to arrest drug traffickers on federal drug-related or organized crime charges, plausibly destabilizing drug gangs. Rival gangs knew they could violently exploit this weakness without much risk of being prosecuted by state criminal justice systems for homicide, at least in Mexico's most violent states.

## II. Direct Effects of Close PAN Victories on Violence

This section uses a regression discontinuity approach to test whether the outcomes of close mayoral elections affect violence in the municipalities experiencing these elections. It first describes the data and identification strategy. Then it examines the relationship between PAN victories and violence, as well as the economic mechanisms underlying this relationship.

### A. Data

The analysis uses official government data on drug trade-related outcomes, obtained from confidential sources unless otherwise noted. Data on drug trade-related

homicides occurring between December of 2006 and October of 2011 were compiled by a committee with representatives from all ministries that are members of the National Council of Public Security.<sup>16</sup> This committee met each week to classify which homicides from the past week were drug trade related.<sup>17</sup> Drug trade-related homicides are defined as any instance in which a civilian kills another civilian, with at least one of the parties involved in the drug trade. The classification is made using information in the police reports and validated whenever possible using newspapers. The committee also maintains a database of those killed in armed clashes between police and organized criminals. Month  $\times$  municipality data on all homicides occurring between 1990 and 2012 were obtained from the National Institute of Statistics and Geography (INEGI) and allow examination of a longer pre- and post-period than the drug trade-related homicide data. The overall homicide data are drawn from vital statistics compiled by state government authorities in each of Mexico's 31 states and federal district. Month  $\times$  municipality confidential data on high-level drug arrests are also employed.<sup>18</sup>

Moreover, this section uses official government data on which of Mexico's 2,456 municipalities had a DTO or local drug gang operating within their limits in early 2008. This offers the closest available approximation to pre-period DTO presence, as systematic municipal-level data about DTOs were not collected before this time.

Finally, electoral data for elections occurring during 2007–2010 were obtained from the electoral authorities in each of Mexico's states. The sources for a number of other variables, used to examine whether the RD sample is balanced, are listed in the notes to Table 1.

### B. *Econometric Framework*

This study uses a regression discontinuity (RD) approach to estimate the impact of PAN victories on violence. The RD strategy exploits the fact that the party affiliation of a municipality's mayor changes discontinuously at the threshold between a PAN victory and loss. Municipalities where the PAN wins by a large margin are likely to be different from municipalities where the PAN loses by a wide margin. However, when we narrow our focus to the set of municipalities with close elections, it becomes more plausible that election outcomes are determined by idiosyncratic factors and not by systematic municipal characteristics that could also affect violence. Thus, under certain conditions municipalities where the PAN barely lost can serve as a reasonable counterfactual for municipalities where they barely won. This section examines the plausibility of the RD identifying assumptions in detail, but first it is helpful to specify the regression form. The baseline analysis estimates the following regression model within a narrow window around the PAN win-loss threshold:

$$(1) \quad y_m = \alpha_0 + \alpha_1 PANwin_m + \alpha_2 PANwin_m \times f(spread_m) \\ + \alpha_3(1 - PANwin_m) \times f(spread_m) + \epsilon_m,$$

<sup>16</sup>The Mexican government stopped compiling these data after October of 2011.

<sup>17</sup>Previous homicides are also considered for reclassification if new information becomes available.

<sup>18</sup>High-level traffickers include DTO kingpins, regional lieutenants, assassins, and financiers.

TABLE 1—BASELINE CHARACTERISTICS

	Own municipality				Neighboring municipalities			
	5 percent vote spread		<i>t</i> -stat on means difference	RD estimate	<i>t</i> -stat on RD estimate	RD estimate	<i>t</i> -stat on RD estimate	
	PAN won	PAN lost						(1)
<i>Political characteristics</i>								
Mun. taxes per capita (2005)	59.84	56.75	(0.23)	40.96	(0.86)	10.80	(0.42)	
Turnout	0.61	0.59	(0.99)	0.05	(0.77)	0.02	(0.42)	
PAN incumbent	0.27	0.32	(-0.61)	-0.05	(-0.22)	0.14	(1.52)	
PRD incumbent	0.17	0.13	(0.63)	-0.02	(-0.12)	-0.16	(-1.42)	
Percent alternations (1976–2006)	0.31	0.31	(-0.01)	0.06	(0.70)	0.01	(0.25)	
PRI never lost (1976–2006)	0.07	0.07	(-0.04)	-0.21	(-1.64)	-0.15	(-1.61)	
<i>Demographic characteristics</i>								
Population (2005)	6.03	5.10	(0.35)	5.08	(0.60)	-1.37	(-0.62)	
Population density (2005)	220.23	191.05	(0.42)	-374.00	(-1.42)	-186.70	(-1.19)	
Migrants per capita (2005)	0.02	0.02	(-0.69)	0.00	(-0.06)	0.00	(-0.77)	
<i>Economic characteristics</i>								
Income per capita (2005)	4.29	4.48	(-0.53)	-0.69	(-0.56)	0.10	(0.15)	
Malnutrition (2005)	32.76	31.20	(0.53)	3.90	(0.50)	-5.23	(-0.89)	
Mean years schooling (2005)	6.26	6.19	(0.32)	-0.55	(-0.77)	-0.10	(-0.22)	
Infant mortality (2005)	22.54	22.26	(0.22)	0.80	(0.22)	0.39	(0.17)	
HH w/o access to sewage (2005)	8.51	8.44	(0.05)	0.80	(0.26)	-1.53	(-0.52)	
HH w/o access to water (2005)	16.14	18.22	(-0.62)	-8.35	(-1.06)	-6.45	(-0.97)	
Marginality index (2005)	-0.15	-0.12	(-0.23)	-0.05	(-0.13)	-0.23	(-0.73)	
<i>Road network characteristics</i>								
Detour length (km)	26.15	22.48	(0.19)	-53.47	(-1.29)	-14.63	(-0.90)	
Road density	0.15	0.13	(0.98)	-0.06	(-1.08)	-0.04	(-0.86)	
Distance US (km)	708.27	735.49	(-0.55)	-126.82	(-0.86)	-135.29	(-0.92)	
<i>Geographic characteristics</i>								
Elevation (m)	1,403.42	1,367.95	(0.26)	547.41	(1.29)	487.19	(1.24)	
Slope (degrees)	3.62	3.19	(1.02)	0.26	(0.22)	-0.17	(-0.18)	
Surface area (km <sup>2</sup> )	1,787.36	725.37	(1.36)	1,808.90	(1.61)	544.10	(0.65)	
Average min. temperature, C	7.17	7.59	(-0.46)	-4.56	(-1.60)	-4.04	(-1.51)	
Average max. temperature, C	22.54	22.90	(-0.53)	-3.44	(-1.58)	-3.24*	(-1.66)	
Average precipitation, cm	1,164.40	1,084.63	(0.65)	-93.64	(-0.32)	-90.74	(-0.32)	
Observations	70	82		152		152		

Notes: Columns 1–5 examine these variables for municipalities with close elections. Column 6 and 7 examine these characteristics for municipalities that border a municipality with a close election. Column 3 reports the *t*-statistic on the difference in means between municipalities where the PAN barely won and where they barely lost. Columns 4 and 6 report the coefficient on PAN win from a standard RD specification where the respective characteristic is used as the dependent variable, and columns 5 and 7 report the respective *t*-statistic.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

Sources: Data on population, population density, mean years of schooling, and migrants per capita are from *II Censo de Poblacion y Vivienda*, INEGI (National Institute of Statistics and Geography 2005). Data on municipal tax collection are from *Sistema de Cuentas Municipales*, INEGI. Data on household access to sewage and water are from CONAPO (National Population Council) (2005). Data on malnutrition are from CONEVAL (National Council for Evaluating Social Development Policy), *Indice de Reazgo Social* (2005). Data on infant mortality are from PNUD Mexico (UN Development Program 2005). The marginality index is from CONAPO (2005). Data on distance to the United States and other road network characteristics are from the author's own calculations. Electoral data are from Mexico Electoral-Banamex and electoral results published by the Electoral Tribunals of each state. For 11 states, data on the total number of eligible voters, required to calculate turnout, are not reported. The geographic characteristics are from Acemoglu and Dell (2009).

where  $y_m$  is the outcome of interest in municipality  $m$ .  $PANwin_m$  is an indicator equal to 1 if the PAN candidate won the election, and  $spread_m$  is the margin of PAN victory. The study examines robustness to different functional forms,  $f(\cdot)$ , for the RD polynomial, which is estimated separately on either side of the PAN win-loss threshold. The baseline specifications use linear or quadratic RD polynomials, as recommended in a recent paper by Gelman and Imbens (2014). For completeness, results using higher-order polynomials are reported in the online Appendix. Some robustness specifications also include region fixed effects and baseline controls.

Elections in Mexico occur at different times throughout the sample period. The study considers two samples: close elections occurring in 2007–2008 and close elections occurring in 2007–2010. The former provides a longer post-inauguration period but fewer municipalities. In order to be able to check for violence pre-trends, both samples are limited to municipalities with at least half a year of drug trade-related homicide data prior to the elections. The samples are also limited to municipalities with at least a year of post-inauguration data, so that violence effects can be examined in the medium-term.

Identification requires that all relevant factors besides treatment vary smoothly at the threshold between a PAN victory and loss. Formally, letting  $y_1$  and  $y_0$  denote potential outcomes under a PAN victory and loss, identification requires that  $E[y_1|spread]$  and  $E[y_0|spread]$  are continuous at the win-loss threshold. This assumption is needed for municipalities where the PAN barely lost to be an appropriate counterfactual for those where they barely won. This assumption would be violated if the outcomes of close elections were determined not by idiosyncratic factors but by a systematic advantage of winners.<sup>19</sup>

To assess the plausibility of the identifying assumptions, Table 1 examines whether 25 political, economic, demographic, road network, and geographic pre-characteristics are balanced across the PAN win-loss threshold. The next section considers violence trends prior to the relevant elections. The body of the study focuses on the baseline 2007–2008 elections sample for municipalities with a vote spread of 5 percentage points or less, and the online Appendix provides analogous results for the 2007–2010 elections and for alternative bandwidths.<sup>20</sup>

Column 1 reports the mean value of each characteristic in municipalities where the PAN won, column 2 does the same for municipalities where they lost, and column 3 reports the  $t$ -statistic on the difference. In no case are there statistically significant differences, including for political characteristics such as turnout and the party of the incumbent.<sup>21</sup> I also estimate the RD specification given by equation (1) using each of the baseline characteristics as the dependent variable. The coefficients on PAN win are reported in column 4 and  $t$ -statistics in column 5. While some of the coefficients are noisily estimated, they are statistically identical to zero. Online Appendix Figures A-2 through A-5 show analogous RD plots for each characteristic. Regional characteristics could also differ across the PAN win-loss threshold, and thus I calculate the average of each baseline characteristic in the municipalities that

<sup>19</sup>For example, Caughey and Sekhon (2011) show that in US House elections between 1942 and 2008, close winners have financial and incumbency advantages.

<sup>20</sup>Online Appendix Figure A-1 shows a map of the close election municipalities, which are located throughout Mexico.

<sup>21</sup>The economically large difference in surface area is driven by a single large municipality, Ensenada.

border each municipality in the RD sample. Columns 6 and 7 repeat the RD analysis using these characteristics as the dependent variable. Again, while some coefficients are noisy, nearly all are statistically insignificant.

To provide a more complete view of how pre-characteristics vary around the threshold, online Appendix Tables A-1 through A-4 repeat this analysis limiting the sample to vote spread bandwidths of 4 percent, 3 percent, 2 percent, and 13.3 percent (the Imbens-Kalyanaraman bandwidth (Imbens and Kalyanaraman 2012)), respectively, and online Appendix Tables A-5 through A-9 do the same for the 2007–2010 close election sample. These tables document qualitatively similar patterns.<sup>22</sup>

When all pre-characteristics are combined into a single measure based on their ability to predict post-period violence—providing a higher-powered test of whether pre-characteristics are balanced—there likewise is not a discontinuity at the PAN win-loss threshold. Specifically, I regress average violence during the post-period—which lasts for the duration of a mayor's term—on all the characteristics from Table 1, as well as on average violence during the pre-period. Both the average monthly probability that at least one homicide occurs and the average homicide rate are considered. I then use the coefficients from this regression to predict post-period violence, and online Appendix Figure A-6 plots the predicted violence measures against the PAN margin of victory. Panel A examines the predicted drug-related homicide probability, panel B the predicted drug-related homicide rate, panel C the predicted overall homicide probability, and panel D the predicted overall homicide rate. In no case is there a statistically significant discontinuity, and the magnitude of the discontinuity is modest.

Identification also requires the absence of selective sorting around the PAN win-loss threshold. This assumption would be violated, for example, if elections were rigged in favor of the PAN in municipalities that would later experience an increase in violence. To formally test for sorting, I implement the McCrary test (McCrary 2008) by collapsing the election data to 1 percentage point vote spread bins and using the observation count in each bin as the dependent variable in equation (1). Online Appendix Figures A-7 and A-8 show that there is not a discontinuous change at the PAN win-loss threshold for either the 2007–2008 or 2007–2010 close election samples. In other words, neither the PAN nor its opponents systematically win close elections. For manipulation of the threshold to be consistent with these figures and Table 1, there would need to be an equal number of elections rigged in favor of and against the PAN, and the pre-characteristics would need to be the same on average for these municipalities and their neighbors, a scenario that is unlikely.

The absence of selective sorting is also institutionally plausible. Elections in Mexico are coordinated by a multi-partisan state commission, and genuine recourse exists in the case of suspected fraud. While traffickers may have incentives to intimidate voters and candidates, recall that mayors prior to the Calderón administration had limited capacity to challenge heavily armed traffickers given the absence of a widespread federal crackdown. The elections, particularly for the 2007–2008 sample,

<sup>22</sup>Regressions using the Imbens-Kalyanaraman bandwidth use a triangular kernel such that the weight given to each observation decays with distance from the threshold. This is the standard approach for implementing the Imbens-Kalyanaraman bandwidth—which is estimated assuming that kernel weighting will be used. The other bandwidths produce very similar estimates when combined with kernel weighting.

are at the beginning of Calderón's crackdown, when traffickers were unlikely to have anticipated how sustained it would be and the role mayors would play. Thus, they may not have found it worthwhile to influence local elections. Consistent with this conjecture, systematic killings of mayors by traffickers began only after the federal crackdown had been sustained for several years.

### C. Graphical Analysis of Violence Patterns

This section graphically analyzes the relationship between close election outcomes and violence, documenting that violence is similar prior to elections but diverges significantly thereafter. The main body of the paper focuses on the 2007–2008 close election sample, in which the post-inauguration period encompasses the mayor's three year term.

Figure 3 examines the probability that at least one drug trade-related homicide occurs in a given month and the homicide rate, both averaged across the pre-election, lame duck, and post-election periods. Examining these two outcomes allows the study to distinguish whether crackdowns led to sustained increases in violence in places that would not have otherwise experienced drug violence or mostly intensified conflict in places that would have experienced some drug violence regardless. The figure plots the violence measures against the PAN margin of victory, with a negative margin indicating a PAN loss. Each point represents average violence in 0.5 percentage point vote spread bins. The solid line plots predicted values from a regression of violence on a quadratic polynomial in the vote spread, estimated separately on either side of the PAN win-loss threshold, and the dashed lines show 95 percent confidence intervals.

While close PAN victories do not significantly increase the average probability that drug trade-related violence occurs during the mayor's subsequent term (panel A), the drug-related homicide rate increases by around 40 per 100,000 following a close PAN victory, as compared to a close PAN loss (panel B). At the threshold, violence is around three times higher in PAN municipalities. In contrast, panel C (panel D) shows that the drug trade-related homicide probability (rate) during the two to five month period following the election but prior to the inauguration of new authorities is similar regardless of whether the PAN barely won or lost. Finally, panels E and F document the absence of a violence discontinuity at the PAN win-loss threshold during the six months prior to the elections, providing additional evidence that the RD sample is balanced. Online Appendix Figure A-9 presents an analogous figure for the 2007–2010 elections, documenting similar patterns.<sup>23</sup> Violence is somewhat higher nearer the threshold, on both sides, in the pre- as well as post-periods.<sup>24</sup> This is plausibly explained by the fact that drug trade-related violence is concentrated in larger, more urban municipalities, which are more likely to have very close elections (see online Appendix Figure A-2).

<sup>23</sup> Online Appendix Figure A-11 shows that both the 2007–2008 and 2007–2010 estimates are similar in magnitude when a negative binomial model is used to account for the count nature of the homicide rate.

<sup>24</sup> See online Appendix Table A-48 for an examination of the relationship between the absolute value of the vote spread and violence.



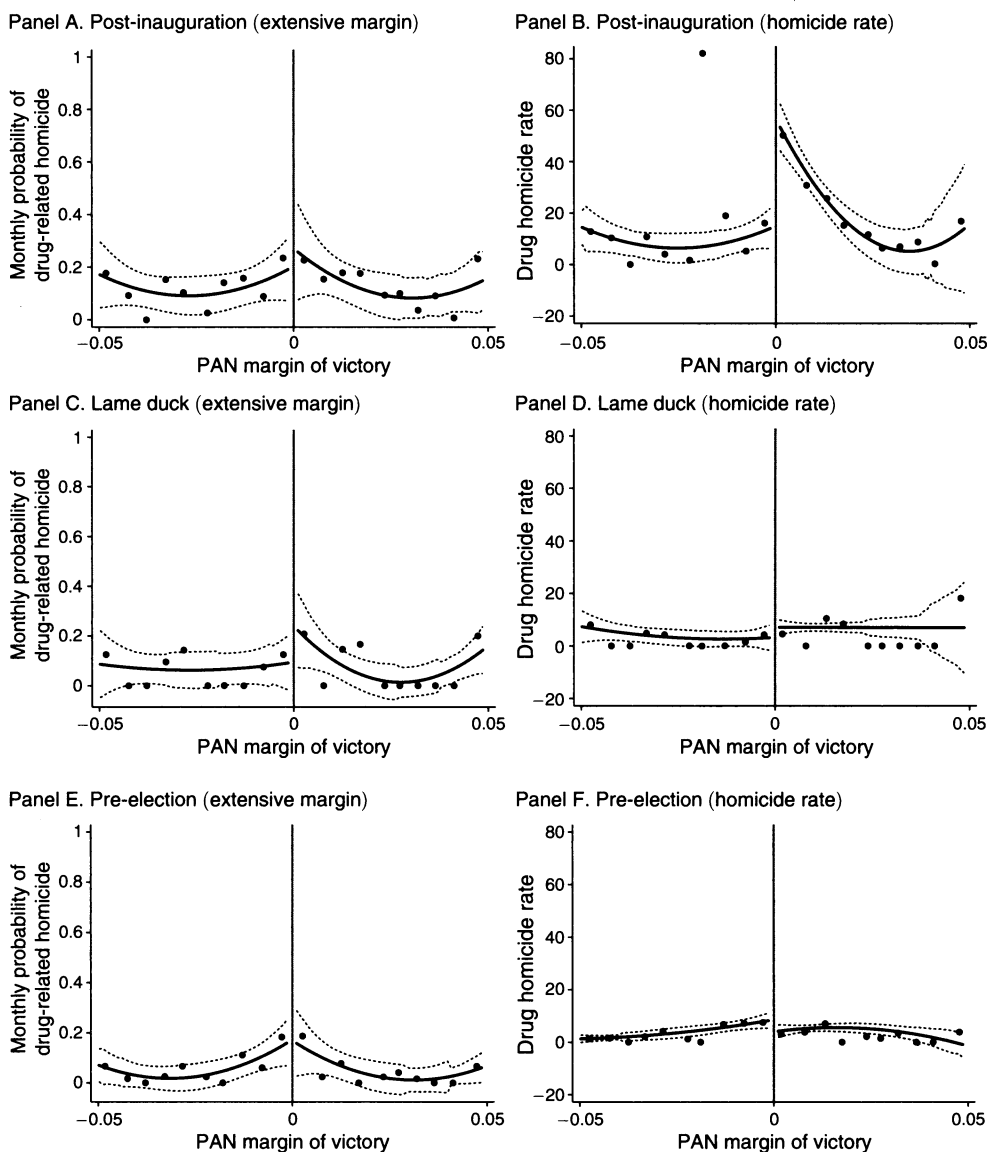


FIGURE 3. DRUG TRADE-RELATED HOMICIDES RD FIGURES

Notes: This figure plots violence measures against the PAN margin of victory, with a negative margin indicating a PAN loss. Each point represents the average value of the outcome in vote spread bins of width one-half of a percentage point. The solid line plots predicted values, with separate quadratic vote spread trends estimated on either side of the PAN win-loss threshold. The dashed lines show 95 percent confidence intervals.

Figure 4 provides an analogous exercise for overall homicides, in order to address two potential concerns with the drug trade-related homicide data. First, while homicides are classified as drug trade-related by a national committee, it is possible that the police reports used to make this classification systematically differ across municipalities. Moreover, the pre-period in the drug trade-related homicide data is relatively short since the data were not collected prior to President Calderón’s inauguration in December of 2006. The overall homicide data are available for 1990–2012 and hence offer a much longer pre-period.

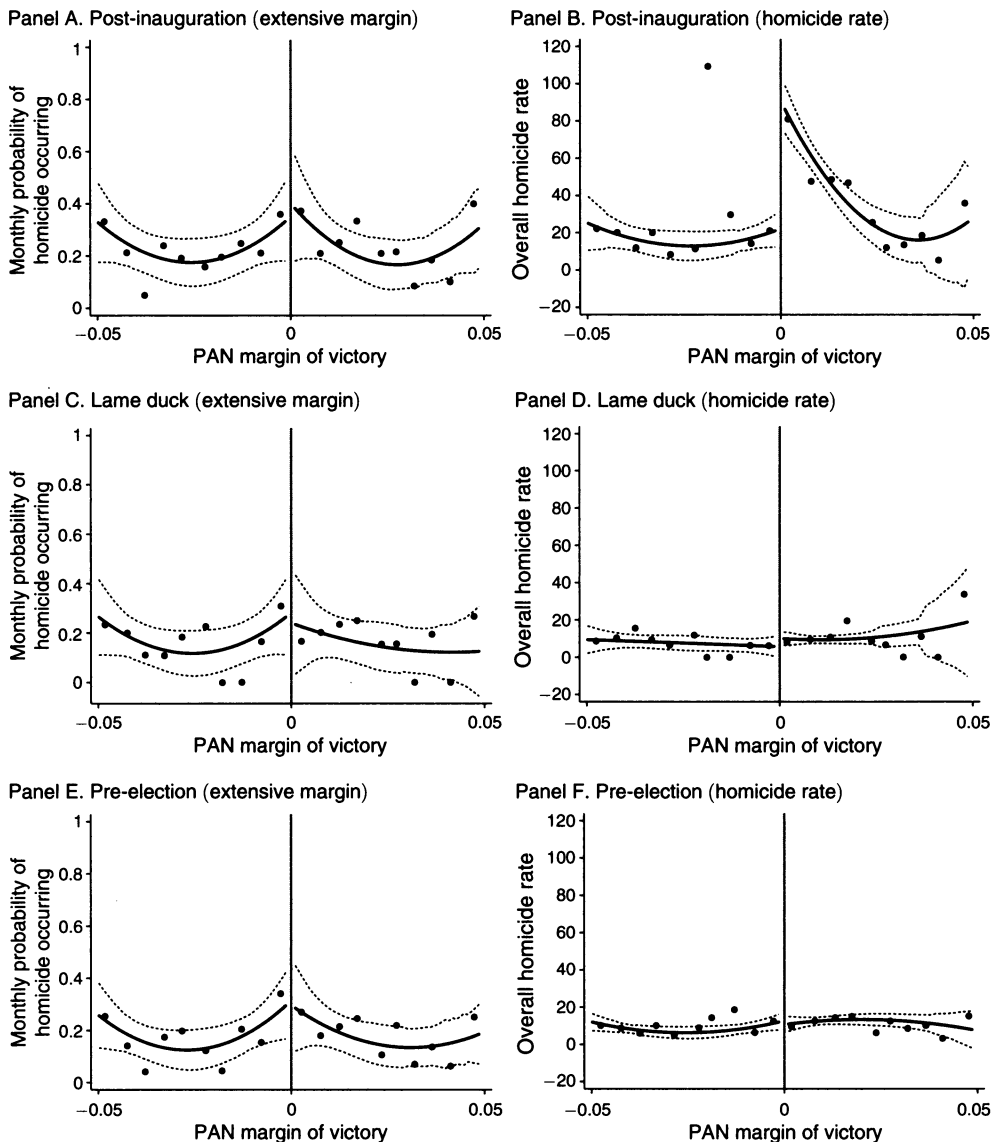


FIGURE 4. ALL HOMICIDES RD FIGURES

*Notes:* This figure plots violence measures against the PAN margin of victory, with a negative margin indicating a PAN loss. Each point represents the average value of the outcome in vote spread bins of width one-half of a percentage point. The solid line plots predicted values, with separate quadratic vote spread trends estimated on either side of the PAN win-loss threshold. The dashed lines show 95 percent confidence intervals.

The patterns documented in Figure 4 are similar to those in Figure 3. There is a significant increase in the overall homicide rate following the inauguration of PAN mayors (panel B), whereas violence during the pre-period—which in this case extends over 17 years—is balanced.<sup>25</sup> Online Appendix Figure A-10 documents

<sup>25</sup>Results (available upon request) are also similar when overall homicides are considered using the same period as for the drug trade-related homicides.

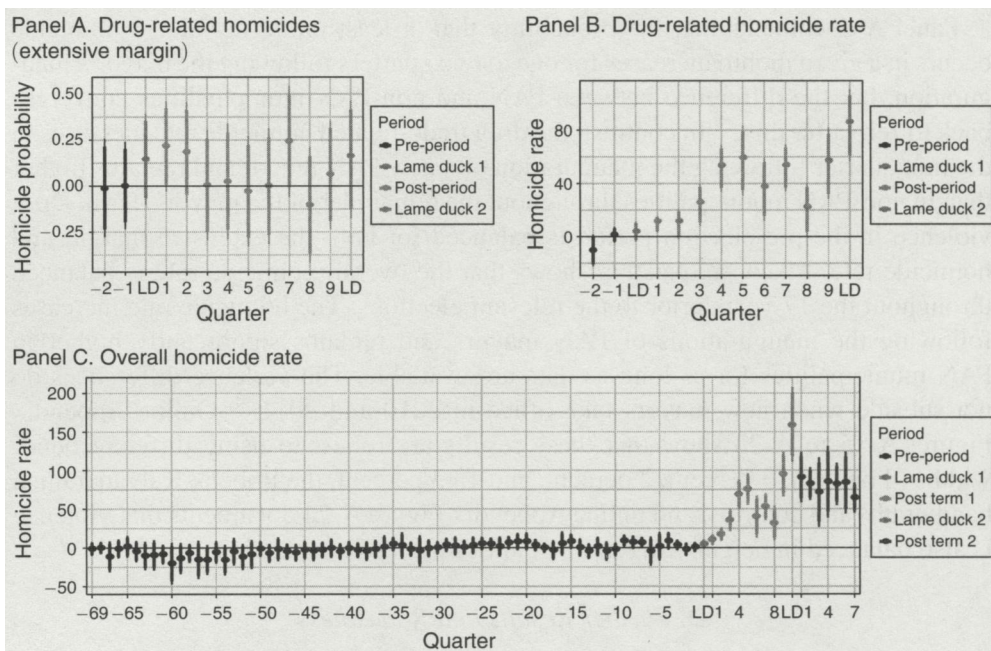


FIGURE 5. HOMICIDE RD FIGURES

Notes: In panel A, each point plots a separate RD estimate of the impact of close PAN victories on the average probability that a drug trade-related homicide occurred. In panel B, each point plots a separate RD estimate of the impact of close PAN victories on the drug-related homicide rate in a given quarter. In panel C, each point plots a separate RD estimate of the impact of close PAN victories on the overall homicide rate in a given quarter. All regressions include a quadratic RD polynomial, estimated separately on either side of the PAN win-loss threshold. The thin lines plot 95 percent confidence intervals, and the thick lines plot 90 percent confidence intervals.

analogous overall homicide patterns in the 2007–2010 close election sample.<sup>26</sup> Since it is quite unlikely that homicide differences of this magnitude would be driven by differences in reporting, these results provide strong evidence that there is a genuine increase in violence following PAN victories. The overall homicide estimates tend to be somewhat larger in magnitude than the drug trade-related homicide estimates, and the positive correlation between changes from month-to-month in drug-related homicides and overall homicides net drug-related homicides suggests that some homicides related to drug gang wars may not have been classified as such.

To uncover more detail about the relationship between violence and PAN victories, I estimate equation (1) separately for each quarter prior to the election and following the inauguration of new authorities. Figure 5 plots the PAN win coefficients against time, which is defined relative to each municipality’s election and inauguration. The thin lines plot 95 percent confidence intervals, and the thick lines plot 90 percent confidence intervals. Quarterly data are used to improve the legibility of the plots, and similar patterns are documented using monthly data in online Appendix Figure A-13.

<sup>26</sup>Figure A-12 documents similar results for the homicide rate using a negative binomial model.

Panel A documents that the probability that at least one drug-related homicide occurs in a given month increases for one to two quarters following the mayor's inauguration, but the difference between PAN and non-PAN municipalities converges back to zero after this.<sup>27</sup> In contrast, the drug trade-related homicide rate increases in the first quarter following the inauguration of a new PAN mayor and remains higher than in non-PAN municipalities throughout the remainder of the mayor's term. Drug violence in the pre-election period is balanced for both the extensive margin and homicide rate. Likewise, panel C shows that the overall homicide rate is balanced throughout the 17 years prior to the relevant elections. The homicide rate increases following the inaugurations of PAN mayors and remains significantly higher in PAN municipalities for as long as data are available. The violence differences do not subside when new mayors take office in 2010 and 2011.<sup>28</sup> Online Appendix Figures A-15 to A-23 show that these results are robust to using different bandwidths (4 percent, 3 percent, 2 percent, and 13.3 percent, the Imbens-Kalyanaraman bandwidth) and controls, and online Appendix Figure A-24 documents that violence is also balanced in neighboring municipalities prior to the elections.

#### *D. Further Results and Robustness*

Tables 2 and 3 estimate the RD specification given in equation (1) for the 2007–2008 and 2007–2010 close election samples.<sup>29</sup> Panel A considers drug trade-related homicides and panel B overall homicides.

Table 2 documents that close PAN victories increase the drug trade-related homicide rate by around 33 (SE = 9) homicides per 100,000 per annum during the mayor's three-year term. Table 3 considers close elections occurring between 2007 and 2010, estimating that PAN victories increase the drug-related homicide rate by 27 (SE = 9) homicides per 100,000 during the one year following the mayor's inauguration. The difference between these estimates is explained by the post-period lengths. When the analysis examining 2007–2008 elections uses a one-year post-inauguration period, the estimated impact of close PAN victories is 22 per 100,000. The estimates for the overall homicide rate show a similar pattern but are somewhat larger in magnitude, as discussed above. Columns 1 through 3 of both tables document that close PAN victories do not have a sustained impact on the violence extensive margin.

Back of the envelope calculations suggest that crackdowns are responsible for at least half of the increase in homicides in Mexico in recent years. When using 2007–2008 elections to compare the drug trade-related homicide rate in the pre- and post-periods, the homicide rate approximately doubles in municipalities where the PAN barely loses and increases by a factor of 5.5 in places where the PAN barely wins. Similarly, the overall homicide rate in municipalities with close PAN elections in 2007–2008 nearly doubles in municipalities with a PAN loss and increases

<sup>27</sup> Whether the impact is statistically significant in the second quarter varies with the specification used, though coefficient magnitudes are similar across specifications.

<sup>28</sup> The probability of at least one homicide occurring in a given month is also balanced across the full 17 year period prior to the elections, see online Appendix Figure A-14.

<sup>29</sup> The homicide rate is more noisily measured in smaller municipalities. Thus, following the standard approach in the crime literature, regressions use inverse variance weighting.

TABLE 2—CLOSE PAN ELECTIONS AND HOMICIDES: 2007–2008 ELECTIONS

	Homicide probability			Homicide rate		
	Post inaug. (1)	Lame duck (2)	Pre- election (3)	Post inaug. (4)	Lame duck (5)	Pre- election (6)
<i>Panel A. Drug trade-related homicides</i>						
PAN win	0.053 (0.090)	0.051 (0.082)	−0.003 (0.076)	32.981*** (9.346)	4.967 (4.122)	−2.038 (3.776)
$R^2$	0.01	0.01	0.04	0.33	0.04	0.11
Observations	152	152	152	152	152	152
<i>Panel B. All homicides</i>						
PAN win	0.037 (0.101)	0.031 (0.101)	0.020 (0.089)	56.630*** (12.768)	2.457 (2.922)	3.088 (4.361)
$R^2$	0.01	0.01	0.01	0.40	0.03	0.04
Observations	152	152	152	152	152	152

*Notes:* Panel A examines drug-related homicides, and panel B examines all homicides. In column 1 the dependent variable is the average monthly homicide probability during the mayor's term, and in column 4 it is the average homicide rate. Columns 2 and 5 similarly examine homicides during the lame duck period, and columns 3 and 6 examine homicides during the pre-election period. PAN win is an indicator equal to one if a PAN candidate won the election, and the sample includes elections in 2007–2008 where the PAN was first or second by a 5 percentage point or less vote spread margin. All columns include a linear RD polynomial estimated separately on either side of the threshold.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

TABLE 3—CLOSE PAN ELECTIONS AND HOMICIDES: 2007–2008 ELECTIONS

	Homicide probability			Homicide rate		
	Post inaug. (1)	Lame duck (2)	Pre- election (3)	Post inaug. (4)	Lame duck (5)	Pre- election (6)
<i>Panel A. Drug trade-related homicides</i>						
PAN win	0.042 (0.054)	0.030 (0.054)	0.027 (0.050)	26.735*** (8.560)	6.331 (5.212)	3.830 (4.688)
$R^2$	0.02	0.01	0.01	0.10	0.01	0.01
Clusters	307	307	307	307	307	307
Observations	310	310	310	310	310	310
<i>Panel B. All homicides</i>						
PAN win	0.053 (0.064)	0.001 (0.071)	0.039 (0.054)	44.820*** (12.289)	6.622 (6.665)	4.740 (3.498)
$R^2$	0.02	0.01	0.03	0.24	0.01	0.03
Clusters	307	307	307	307	307	307
Observations	310	310	310	310	310	310

*Notes:* Panel A examines drug-related homicides, and panel B examines all homicides. In column 1 the dependent variable is the average monthly homicide probability during the one year following the mayor's inauguration, and in column 4 it is the average homicide rate. Columns 2 and 5 similarly examine homicides during the lame duck period, and columns 3 and 6 examine homicides during the pre-election period. PAN win is an indicator equal to one if a PAN candidate won the election, and the sample includes elections in 2007–2008 where the PAN was first or second by a 5 percentage point or less vote spread margin. All columns include a linear RD polynomial estimated separately on either side of the threshold.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

by a factor of four in municipalities with a PAN victory. These estimates are likely a lower bound since the party of the mayor is not the sole determinant of federal security assistance.

The online Appendix conducts a number of additional robustness checks. First, online Appendix Tables A-10 and A-11 examine the robustness of the drug trade-related homicide rate estimates to using alternative bandwidths (5 percent, 4 percent, 3 percent, 2 percent, and 13.3 percent—the Imbens-Kalyanaraman bandwidth), alternative RD polynomials (linear, quadratic, cubic, and quartic, estimated separately on either side of the PAN win-loss threshold), and alternative inclusions of controls, for the 2007–2008 and 2007–2010 samples respectively. Online Appendix Tables A-12 and A-13 perform an analogous exercise for the overall homicide rate. The coefficients reported in the main text fall toward the middle of the distribution of the 132 coefficients reported in each of the analogous robustness tables and tend to be statistically similar. Moreover, online Appendix Figures A-25 and A-26 document that PAN victories robustly increase the homicide rate, regardless of the length of the analysis period.<sup>30</sup> I focus on close elections because they allow for causal identification. For completeness, online Appendix Table A-14 reports that when all PAN elections are included in the sample, effects on violence are positive but only statistically significant for the overall homicide rate.

If violence before the elections is balanced, a differences-in-differences specification will estimate PAN win effects that are similar to the RD coefficients. Online Appendix Tables A-15 to A-24—which each explore a different vote spread bandwidth (5 percent, 4 percent, 3 percent, 2 percent, and 13.3 percent) and dependent variable (the drug trade-related or overall homicide rate)—document that this approach does indeed produce estimates similar to those from the RD, for both the 2007–2008 and 2007–2010 close election samples. This remains true even when the differences-in-differences specifications control for municipality-specific time trends.

### E. Interpretation

This section examines the mechanisms through which PAN victories affect drug trade-related violence. The evidence suggests that PAN victories lead to crackdowns that weaken incumbent criminals, spurring violence between traffickers.

First, consider whether PAN victories lead to crackdowns. Municipality-level data on military and federal police deployments cannot be released to individuals outside these organizations, but other available evidence suggests that PAN mayors are more likely to crack down on the drug trade. Online Appendix Table A-25 examines police-criminal confrontation causalities. Using the RD specification in equation (1), it documents that casualties are significantly higher in PAN than in non-PAN municipalities, for both the 2007–2008 and 2007–2010 election samples.

<sup>30</sup>Online Appendix Figure A-25 shows that results using the drug trade-related homicide rate are robust to varying the pre-period from 1 to 6 months, the lame duck period from 1 to 5 months, and the post-period from 2 to 35 months. Online Appendix Figure A-26 shows analogously that results for the overall homicide rate are robust to varying the pre-period from 1 to 205 months ( $\approx 17$  years), the lame duck period from 1 to 5 months, and the post-period from 2 to over 50 months.

TABLE 4—HETEROGENEITY

	Dependent variable: Drug-related homicide rate			
	2007–2008 elections			
	(1)	(2)	(3)	(4)
PAN win	32.981*** (9.346)	37.699*** (9.000)	37.333*** (9.752)	-2.344 (4.020)
PAN win × far from United States		-49.364*** (12.674)		
PAN win × low violence			-51.267*** (11.524)	
PAN win × local gang				0.331 (14.867)
PAN win × rival				33.747*** (10.827)
PAN win × ally				11.522 (10.992)
$R^2$	0.326	0.433	0.443	0.504
Clusters	152	152	152	152
Observations	152	152	152	152
PAN win effect (far from United States)		-11.670 (8.924)		
PAN win effect (low violence)			-13.930** (6.140)	
PAN win effect (local gang)				-2.013 (14.310)
PAN win effect (rival)				31.400*** (10.050)
PAN win effect (ally)				9.178 (10.230)

*Notes:* PAN win is an indicator equal to one if a PAN candidate won the election, far from United States is an indicator equal to 1 if the municipality is above median distance from the United States, low violence is an indicator equal to 1 if the municipality had a below median homicide rate during 2004–2006, local gang is an indicator equal to one if the municipality contains only a local gang, rival is an indicator equal to one if it contains a major DTO and borders territory controlled by a rival DTO, and ally is an indicator equal to one if it contains a major DTO and does not border territory controlled by a rival DTO. All columns limit the sample to municipalities where a PAN candidate was the winner or runner-up by less than a 5 percentage point vote spread margin and include a linear RD polynomial estimated separately on either side of the PAN win-loss threshold. In addition to the interactions, main effects are also included. Standard errors, clustered by municipality, are in parentheses.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

In contrast, casualties are balanced in the pre-period. Arrests of high-level drug traffickers, while rare, likewise occur more frequently following PAN victories.<sup>31</sup>

Table 4 examines whether the patterns of heterogeneity in the data are consistent with the hypothesis that crackdowns spur conflicts between traffickers. Column 1 reproduces the baseline post-inauguration results for the 2007–2008 close election sample. Column 2 examines whether effects are larger in municipalities that are

<sup>31</sup>The federal government does not maintain a database of all drug-related arrests. During the post-inauguration period, high-level arrests happened twice as often in places where the PAN barely won.

closer to the United States, where most illicit drugs are consumed. These municipalities are plausibly more valuable for traffickers to control and potentially more worth fighting over. I interact PAN win, spread, and PAN win  $\times$  spread with an indicator equal to one if the municipality is greater than median distance from the United States. The effect of PAN victories on the post-inauguration homicide rate is large and positive in municipalities closer to the United States, and smaller and statistically insignificant in municipalities further from the United States. Online Appendix Tables A-26 to A-35 document robustness across different bandwidths and measures of the homicide rate (drug trade-related and overall homicides), for both the 2007–2008 and 2007–2010 election samples.

Column 3 performs a similar exercise, interacting the PAN win indicator and RD polynomial with an indicator equal to 1 if the municipality had a below average homicide rate prior to Calderón's presidency. It is plausible that municipalities more valuable to the drug trade were already experiencing baseline violent conflict prior to the Mexican Drug War, and the extensive and intensive margin results in Tables 2 and 3 suggest that the sustained violence effects are concentrated in places that would have experienced some violence in any case. The heterogeneity result in Table 4 documents that sustained increases in violence during the post-inauguration period are in fact concentrated in municipalities with an above average pre-period homicide rate. Results are similar regardless of the bandwidth, election period, or homicide measure used (online Appendix Tables A-26 to A35).

Crackdowns could plausibly ignite conflicts within DTOs if members fight to be promoted to the positions of higher-level traffickers who have been arrested or killed. Crackdowns could also weaken the incumbent DTO, creating incentives for rival DTOs to violently wrest control of a territory while the incumbent is vulnerable. Incentives to usurp territory are plausibly greatest when the territory is nearby, as controlling an entire region allows traffickers to monopolize the many criminal activities in which they engage.<sup>32</sup> To shed light on these hypotheses, I categorize municipalities into four groups using confidential data on DTOs: (i) municipalities controlled by a major DTO that border territory controlled by a rival, (ii) municipalities controlled by a major DTO that do not border territory controlled by a rival DTO, (iii) municipalities controlled by a local drug gang, and (iv) no known drug trade presence.<sup>33</sup> Municipalities with no known drug trade presence had not experienced drug trade-related homicides or illicit drug confiscations at the time the DTO data were compiled, and local authorities had not reported the presence of a drug trade-related group.

Column 4 of Table 4 interacts these indicators with the RD terms, providing robust evidence that violence effects are concentrated where the control of territory is fragmented. PAN win effects are large, positive, and statistically significant in municipalities with a major DTO that border territory controlled by a rival, and online Appendix Tables A-26 to A-35 show that these effects are highly robust across RD samples. There is also some evidence that PAN victories increase the homicide

<sup>32</sup>For example, a DTO can charge higher prices for prostitution if it controls brothels throughout a region than if it has to compete with a rival group.

<sup>33</sup>The major DTOs during the sample period are Beltran, Familia Michoacana, Golfo, Juarez, Sinaloa, Tijuana, and Zetas.



rate in municipalities controlled by a major DTO that border territory controlled by allies, although estimates are not statistically significant across all samples. With the 5 percent bandwidth, which is used in the main text, impacts are not statistically significant, but the coefficients are broadly similar to those estimated using the bandwidths examined in the online Appendix, which do tend to be statistically significant. These estimates suggest that PAN victories increase the drug-related homicide rate by between 5 and 16 per 100,000 in municipalities that border an ally, as compared to effects of around 35 per 100,000 in municipalities that border a rival. The effects for municipalities with a local drug gang and with no known drug trade presence are small and statistically insignificant.

Why would a rival DTO want to wrest control of territory experiencing a crack-down, as crackdowns and their violent aftermath could make the territory less profitable? Crackdowns are unlikely to persist in the long run, and their violent aftermath plausibly also fades eventually. While the study finds that the violent effects of crackdowns are sustained in the medium term, DTOs are multibillion-dollar businesses and may make long-run investment decisions that are expected to pay out only over a longer period than has elapsed since the start of the Mexican Drug War. Moreover, the level of drug violence in Mexico in recent years, as well as the widespread recruitment of ex-military special forces personnel by drug gangs, is unprecedented (Guerrero 2011). Given recent changes in the weaponry and military strategies used to wage gang wars, DTOs may have underestimated the time and costs required to monopolize a territory after the incumbent DTO was weakened.

While this evidence suggests that crackdowns spur violence between traffickers, there are other potential explanations of the PAN win effects that will be considered in turn. First, a PAN victory is more likely to signal a change in the party controlling the mayorship, which could spur violence by disrupting the status quo. The PAN is the incumbent party in around 30 percent of the 2007–2008 election sample and 37 percent of the 2007–2010 election sample, whereas the PRI is the incumbent party in around half of municipalities. This hypothesis is examined in Table 5. The dependent variable is the drug trade-related homicide rate in the post-inauguration period.

Column 1 reports the baseline RD estimates, whereas column 2 distinguishes whether the PAN was the incumbent party. This specification includes the same terms as the baseline and also interacts PAN win, spread, and PAN win  $\times$  spread with the incumbency dummy. Column 2 estimates that in municipalities with a non-PAN incumbent, close PAN victories increase the drug homicide rate during the mayor's subsequent term by 30 homicides per 100,000 (SE = 8), whereas the estimated impact in municipalities with a PAN mayor previously is  $-3$  (SE = 5).<sup>34</sup> Online Appendix Tables A-36 to A-45 document that these patterns are robust to bandwidth selection, to the election period used, and to different homicide measures (drug trade-related and overall homicides).

Column 3 interacts the RD terms with an indicator equal to 1 if the party controlling the mayorship changed.<sup>35</sup> The impact of an alternation on violence is smaller and statistically insignificant. While violence increases when the party switches to

<sup>34</sup> The coefficient in column 1 is not a simple weighted average of the coefficients in column 2 because the latter estimates the RD polynomial separately for municipalities with a PAN and non-PAN incumbent.

<sup>35</sup> Vote spread is negative when the party stays the same and positive when it changes.

TABLE 5—LOCAL POLITICS AND VIOLENCE

	Dependent variable: Drug-related homicide rate					
	2007–2008 elections					
	(1)	(2)	(3)	(4)	(5)	(6)
PAN win	32.981*** (9.346)	30.134*** (8.082)				34.038*** (11.173)
PAN win × PAN incumb.		−32.965*** (9.704)				
Alter (PAN)			8.147 (6.313)			
PRI win				11.523 (10.550)		
Alter (PRI/PRD)					4.419 (3.728)	
PAN win × PAN gov.						3.617 (15.494)
Clusters	152	152	152	142	142	152
Observations	152	152	152	142	142	152
R <sup>2</sup>	0.326	0.470	0.104	0.038	0.039	0.342
PAN win effect (PAN incumb.)		−2.831 (5.370)				
PAN win effect (PAN gov.)						37.660*** (10.730)

*Notes:* PAN win is an indicator equal to one if a PAN candidate won the election, PAN incumbent is an indicator equal to 1 if the PAN held the mayorship during the previous term, PAN governor is an indicator equal to 1 if the state has a PAN governor, PRI win is an indicator equal to 1 if the PRI won the election, and alter is a dummy equal to 1 if the party controlling the mayorship changed. Columns 1–3 and 6 limit the sample to municipalities where a PAN candidate was the winner or runner-up by less than a 5 percentage point vote spread margin; and columns 4 and 5 limit the sample to municipalities with a close election between PRI and PRD candidates. All columns include a linear RD polynomial estimated separately on either side of the threshold. In columns 2 and 6, main effects are also included. Standard errors, clustered by municipality, are in parentheses.

\*\*\*Significant at the 1 percent level.

\*\*Significant at the 5 percent level.

\*Significant at the 10 percent level.

the PAN, it does not increase when the party switches to the PRI or PRD, so the average effect of an alternation is small.

As a further check, I examine close elections between the PRI and PRD. If alternations between parties spur violence, we would expect to see an impact of alternations in the PRI-PRD sample, whereas if the violence effects are driven by the PAN, alternations should not influence violence in this sample. Results are consistent with the latter scenario. For comparison purposes, column 4 runs the standard RD specification comparing municipalities where the PRI or PRD barely won or lost, and the PRI win coefficient is positive but not statistically significant. Column 5 examines whether alternations in this sample affect violence. The violence effects are small, equaling 4.4 (SE = 4.1). These results are robust across the bandwidths, election samples, and homicide measures examined in online Appendix Tables A-36 to A-45.

Governors control the deployment of state police and disbursement of state funds. Another alternative explanation is that security assistance from PAN governors—and

not the PAN federal government—drives the results. This explanation is unlikely since less than 10 percent of the sample is in a state with a PAN governor, and column 6 of Table 5 shows that the impact of PAN victories on violence is similar regardless of the governor's party.<sup>36</sup>

Beyond security assistance, PAN mayors could also in theory receive more economic assistance from the PAN federal government, which traffickers might fight to siphon off through extortion. While 90 percent of Mexican state and local spending are financed by federal transfers, economic resources are allocated transparently using formulas (Haggard and Webb 2004). Since the RD sample is balanced on the characteristics used in these formulas, economic transfers do not differ between PAN and non-PAN municipalities.<sup>37</sup>

A final alternative hypothesis is that the violence effects result from differences in corruption between PAN and non-PAN mayors. While this hypothesis cannot be completely ruled out due to difficulties in observing corruption and noise in the available corruption estimates, the available evidence is more consistent with violence effects being driven by PAN mayors receiving federal security assistance. Official government data on mayoral corruption in 2008 offer unique insight into corruption at the local level, documenting around 25 percent of mayors in the RD sample engaging in corruption. The data are available for mayors who had taken office by 2008, providing 102 observations for the 5 percent vote spread bandwidth. Online Appendix Table A-46 examines the relationship between PAN victories and an indicator equal to 1 if the mayor was corrupt, with panel A reporting a simple means comparison and panel B including a linear RD polynomial. Estimates for five different bandwidths are reported. The coefficient on PAN win is typically small, relative to high mean levels of corruption, although the estimates become noisier once the vote spread trends are included. The most precise estimates use the 13.3 percent (Imbens-Kalyanaraman) bandwidth, and equal 0.007 (SE = 0.055) and -0.005 (SE = 0.091) for the means comparison and RD.

Elections involving the PRI and PRD can provide an additional test of this hypothesis, and also suggest that corruption differences are unlikely to generate the violence results. The outcomes of PRI-PRD elections should matter for violence if the corruption hypothesis is true and there are significant differences in these parties' propensities to engage in corruption. The corruption data suggest that the PRI is more corrupt than the PRD, but columns 4 and 8 of Table 5 document that if anything violence is higher in places that barely elect a PRI mayor, although the differences are not statistically significant.<sup>38</sup>

<sup>36</sup> Because few states have a PAN governor, caution must be used in interpreting these results when narrow bandwidths are used in the online Appendix.

<sup>37</sup> Municipalities receive federal resources through two main funds: the Fondo para la Infraestructura Social Municipal, which is distributed proportionally to the number of households living in extreme poverty, and the Fondo de Aportaciones para el Fortalecimiento de los Municipios, which is distributed proportionally to population. Resource transfers from state to local governments are less transparent, but recall that I do not find differences by the party of the state governor.

<sup>38</sup> RD estimates of the impact of PRI victories on corruption (available upon request) are positive and statistically significant regardless of the bandwidth used. When using the five standard bandwidths employed throughout this study, two of the five means comparison estimates are statistically significant.

### III. A Network Analysis of Spillover Effects

Thus far the analysis has focused on how crackdowns in a given municipality affect that location, but crackdowns may also impact other municipalities by motivating traffickers to relocate their operations. Whether spillovers occur is an empirical question, and one can construct various scenarios in which crackdowns do not divert drug traffic away from existing routes: for example if they actually make it cheaper to use existing routes by weakening incumbent traffickers. This section utilizes a network model of drug trafficking to provide empirical evidence about whether and where spillovers are likely to occur. This section first specifies the baseline model and uses data on drug confiscations to test whether the model predicts the diversion of drug traffic following close PAN victories. It then examines whether close PAN victories increase violence along alternative trafficking routes and finally develops several extensions of the model.

#### A. A Network Model of Drug Trafficking

In order to test whether crackdowns affect violence and drug trafficking elsewhere, it is necessary to specify a model of where spillovers are likely to occur. DTOs are profit-maximizing entities who face economic constraints, and the trafficking model captures this in a simple and transparent way. In the model, traffickers minimize the costs of transporting drugs from producing municipalities in Mexico across the road network to US points of entry. They incur costs from the physical distance traversed and from crackdowns and thus take the most direct route to the United States that avoids municipalities with crackdowns. This framework provides a starting point for examining patterns in the data without having to first develop extensive theoretical or empirical machinery. Section IIIC will specify and estimate richer versions of the model that include congestion and other costs.

The model setup is as follows: let  $N = (\mathcal{V}, \mathcal{E})$  be an undirected graph representing the Mexican road network, which consists of sets  $\mathcal{V}$  of vertices and  $\mathcal{E}$  of edges. Traffickers transport drugs across the network from a set of origins to a set of destinations. The routes are calculated using Dijkstra's algorithm (Dijkstra 1959), an application of Bellman's principal of optimality. The online Appendix provides a formal statement of the problem.

Destinations consist of Mexico–US border crossings and major Mexican ports. While drugs may also enter the United States between terrestrial border crossings, the large amount of legitimate commerce between Mexico and the United States offers ample opportunities for drug traffickers to smuggle large quantities of drugs through border crossings and ports (US Drug Enforcement Agency 2011).<sup>39</sup> All destinations pay the same international price for a unit of smuggled drugs.

Each origin  $i$  produces drugs and has a trafficker whose objective is to minimize the cost of trafficking these drugs to US entry points. Producing municipalities are

<sup>39</sup>There are 370 million entries into the United States through terrestrial border crossings each year, and 116 million vehicles cross the land borders with Canada and Mexico (US Drug Enforcement Agency 2011). Each year more than 90,000 merchant and passenger ships dock at US ports, and these ships carry more than 9 million shipping containers. Commerce between the United States and Mexico exceeds a billion dollars a day.

identified from confidential Mexican government data on drug cultivation (heroin and marijuana) and major drug labs (methamphetamine). In practice we know little about the quantity of drugs cultivated, and hence I make the simplifying assumption that each origin produces a single unit of drugs. Opium poppy seed and marijuana have a long history of production in specific regions with particularly suitable conditions, and thus the origins for domestically produced drugs are relatively stable throughout the sample period. In contrast, cocaine—which can only be produced in the Andean region—typically enters Mexico along the Pacific coast via small vessels at locations that are flexible and less well-known (US Drug Enforcement Agency 2011). Thus, I focus on trafficking routes for domestically produced drugs.

In the baseline spillovers analysis, I assume that close PAN victories increase the costs of trafficking drugs through a municipality to infinity when the PAN mayor takes office. Because mayors take office at different times throughout the sample period, close elections generate plausibly exogenous within-municipality variation in predicted routes across Mexico. If the aim of the exercise were purely predictive, routes in the baseline specification could also vary with landslide elections and other time varying characteristics. However, such an approach would not identify spillovers, due to the well-known reflection problem (Manski 1993). For example, support for the PAN and drug trafficking activity could be growing in tandem in a region because of economic factors, generating correlations between politics in one municipality and violence nearby. In contrast, Table 1 and online Appendix Figure A-24 show that the outcomes of close elections are uncorrelated with neighbors' characteristics and pre-period violence trends. For completeness, Section IIIC nevertheless examines robustness to imposing costs to pass through all PAN municipalities.

There is a large and growing literature examining conflict and crime on networks. These studies tend to emphasize local bilateral interactions. For example, Ballester, Calvo-Armengol, and Zenou (2006, 2010) model bilateral interactions between criminals on a network, identifying which player(s) should be arrested in order to reduce crime the most, and König et al. (2014) apply a similar key player analysis of bilateral interactions to insurgent groups in the Congo War. Baccara and Bar-Isaac (2008) use network tools to model how the information flow within criminal organizations might change in response to law enforcement strategies that target specific parts of the network, and Kovenock and Roberson (2012) employ a network to model the relationships between conflicts on multiple battlefields. Brown et al. (2006) examine how networks can be made less vulnerable to attack by terrorists, and Goyal and Vigier (2010) develop a related model where a designer chooses a network and an attacker chooses an attack strategy. Allocating defense budgets to a node of the network can prevent the attack from spreading locally to other nodes.

Core to the trafficking model is a global optimization decision—choosing a path across a congested network—whereas the networks literature in economics has focused largely on local bilateral interactions, for example between a farmer and his agricultural contacts or between criminal associates in the mafia. It would be appropriate to apply such a model to the trafficking problem if traffickers sold drugs to their associates in the next municipality over based on what was locally optimal, and these associates in turn sold the drugs to their locally optimal associates and so forth until the drugs were sold to a consumer in the United States. Instead, a single

trafficker makes a global decision about how to transport drugs across a network, which is the problem that the routing model was designed to solve.

### B. Baseline Results

To shed light on whether this simple model provides reasonable predictions of where spillovers are likely to occur, this section examines the relationship between model predicted routes and actual illicit drug confiscations using the following empirical specification:

$$(2) \quad \text{conf}_{mst} = \beta_0 + \beta_1 \text{Routes}_{mst} + \psi_{st} + \delta_m + \epsilon_{mst},$$

where  $\text{conf}_{mst}$  is confiscations of domestically produced drugs (marijuana, heroin, and methamphetamine) in municipality  $m$ , state  $s$ , month  $t$ . Official government data on confiscations of different types of drugs were obtained from confidential sources. Both an indicator and a continuous measure are examined.  $\text{Routes}_{mst}$  is a measure of predicted trafficking routes,  $\psi_{st}$  is a month  $\times$  state fixed effect, and  $\delta_m$  is a municipality fixed effect. The error term is clustered simultaneously by municipality and state-month to address spatial correlation (Cameron, Gelbach, and Miller 2011), and the sample excludes municipalities that themselves experience a close election.<sup>40</sup> The main text focuses on the 2007–2008 close election sample, and the baseline sample period extends through 2009, when mayors from all the 2007–2008 elections have taken office. To examine robustness to extending the length of the sample period, the online Appendix predicts trafficking routes using close elections between 2007 and 2010. The empirical approach is summarized in Figure 1.

The municipality fixed effect ensures that  $\beta_1$  is identified from within municipality variation. Thus, if enforcement is constant within municipalities over time, confiscations will provide a proxy for actual drug traffic. Typically, local politics does not change when a municipality acquires a predicted route, and thus relatively constant enforcement appears plausible. This assumption will be further examined in the empirical analysis.

Panel A of Table 6 reports estimates from equation (2). In column 1, the dependent variable is an indicator equal to 1 if domestically produced drugs (marijuana, heroin, or methamphetamine) were confiscated in the municipality-month and the routes measure is an indicator equal to 1 if the municipality had at least one predicted route in that month. When a municipality acquires a predicted route, the probability of confiscating drugs during a given month increases from a baseline of 5.4 percent to 7 percent, and the effect is significant at the 1 percent level. In column 2, the dependent variable equals the log value (in US dollars) of drug seizures if they are positive and equals 0 otherwise. This measure is always nonnegative, as even the smallest confiscations are worth hundreds of dollars.<sup>41</sup> Acquiring a predicted trafficking route is associated with an 18 percent increase in the total value of

<sup>40</sup>It also excludes producing municipalities, since much of the analysis focuses on the extensive margin of trafficking routes and producing municipalities mechanically contain a predicted trafficking route. Results (available upon request) are robust to including these municipalities.

<sup>41</sup>Working in logs is attractive because drug confiscations are highly right-skewed, with several drug busts confiscating tens of millions of dollars of drugs.

TABLE 6—THE DIVERSION OF DRUG TRAFFIC

	Dependent variable: Domestic illicit drug confiscations					
	Full sample		Limited sample		Full sample	
	Dummy (1)	Value (2)	Dummy (3)	Value (4)	Dummy (5)	Value (6)
<i>Panel A. Shortest paths</i>						
Predicted routes dummy	0.016*** (0.005)	0.170*** (0.050)	0.016** (0.007)	0.169*** (0.065)	0.004 (0.004)	0.028 (0.020)
<i>Panel B. Model with congestion costs</i>						
Predicted routes dummy	0.013** (0.005)	0.149*** (0.057)	0.011* (0.006)	0.129** (0.065)	0.002 (0.004)	0.009 (0.025)
Municipalities	1,869	1,869	1,562	1,562	1,869	1,869
Observations	69,153	69,153	57,794	57,794	69,153	69,153

*Notes:* The dependent variable in columns 1 and 3 is an indicator equal to 1 if domestic illicit drug confiscations are made in a given municipality-month; the dependent variable in columns 2 and 4 is the log value of domestic illicit drug confiscations (or 0 if no confiscations are made); the dependent variable in column 5 is an indicator equal to 1 if cocaine confiscations are made in a given municipality-month; and the dependent variable in column 6 is the log value of confiscated cocaine (or 0 if no confiscations are made). Columns 3 and 4 limit the sample to municipalities that do not border a municipality that has experienced a close PAN victory. Panel A predicts trafficking routes using the shortest paths model, and panel B uses the model with congestion costs. All columns include month  $\times$  state and municipality fixed effects. Standard errors clustered by municipality and month  $\times$  state are reported in parentheses.

\*\*\* Significant at the 1 percent level.

\*\* Significant at the 5 percent level.

\* Significant at the 10 percent level.

confiscated domestic drugs, and again the correlation is significant at the 1 percent level. Online Appendix Table A-49 examines robustness to using the 2007–2010 election sample and period. Results are broadly similar, although the relationship between confiscation value and the predicted routes dummy is no longer statistically significant.

These results suggest that the model can predict the diversion of drug traffic following PAN victories. However, if alternative routes traverse nearby municipalities and if the military or federal police become active throughout a region when deployed to PAN municipalities, this could generate a correlation between changes in predicted routes and confiscations that is unrelated to the diversion of drug traffic. In contrast, it is difficult to tell a plausible story in which PAN victories directly affect confiscations along alternative routes located further away. Columns 3 and 4 examine whether the model is predictive when I exclude municipalities bordering those with close PAN victories. The estimated coefficients are similar to those in columns 1 and 2 and remain statistically significant.<sup>42</sup>

<sup>42</sup>This contrasts to the 2007–2010 results, which are not significant for the limited sample. The limited sample offers little power for the 2007–2010 exercise since many more municipalities border a 2007–2010 close election municipality than border a 2007–2008 close election municipality. Moreover, some of the extensions in the following section will improve the fit for this sample.

Another concern is that authorities along alternative routes may have increased enforcement efforts in response to a small increase in drug traffic. In this case the model would correctly identify the locations of spillovers, which is its central aim, but the confiscations data would exaggerate the magnitudes. To assess this possibility, I examine whether predicted domestic drug routes are correlated with cocaine confiscations. While cocaine and domestic routes ultimately intersect before reaching the United States, in general they are different since cocaine entry points and drug producing municipalities are in distinct locations. When domestic drug traffic changes in a town that also contains a cocaine route, its cocaine route often will be unaffected by the change in local politics that diverted domestic drug traffic. Thus, if enforcement is constant, cocaine confiscations will on average change little when a municipality acquires or loses a domestic route. In contrast, if changes in enforcement drive most of the large increases in domestic drug confiscations that occur when a municipality acquires a predicted route, cocaine confiscations should also increase. Columns 5 and 6 document that within-municipality variation in predicted domestic routes is in fact uncorrelated with variation in the presence and value of cocaine confiscations.<sup>43</sup>

As a final check on the model, I show that the strong correlation between predicted routes and actual confiscations that Table 6 documents is unlikely to have arisen by chance. I randomly assign placebo close PAN victories such that the number of randomly selected municipalities that are infinitely costly to traverse increases each month by the number of close PAN inaugurations that actually occurred that month. I calculate predicted routes and regress confiscations on an indicator for the presence of a predicted route (along with municipality and state  $\times$  month fixed effects), repeating this exercise 1,000 times and plotting the coefficients in online Appendix Figure A-27. Only six of the coefficients are statistically different from zero at the 5 percent level, and the coefficient from column 2 of Table 6 is in the far right tail of the coefficient distribution, more than three standard deviations above the mean.

Modeling the diversion of drug trafficking activity can shed some light on the “diversion hypothesis,” which argues that when the government cracks down in one place, drug activity is partially diverted elsewhere without being substantially reduced. The diversion hypothesis plays a major role in debates over the War on Drugs and is a leading explanation popularly given for why—despite a massive increase in drug enforcement expenditure globally over the past 40 years—drug markets have continued to expand and drug use has not declined (UNODC 2010; Reuter and Trautmann 2009). In the context of the Mexican Drug War, the shares of Mexican heroin and possibly other Mexican drugs in the US market increased during the Calderón crackdown (Díaz-Briseño 2010). In this case, as with the other mostly qualitative and anecdotal evidence about the diversion hypothesis, the correlations are consistent with the diversion hypothesis but it is difficult to know the counterfactual.

The current study addresses this concern by providing causal evidence about the diversion of drug traffic. While it would ideally quantify what percentage of drugs

<sup>43</sup> Results are similar when I limit the sample to municipalities with cocaine confiscations during the beginning of the sample period. Because of the municipality fixed effects, municipalities without cocaine confiscations only affect the routes coefficient through their influence on the fixed effects estimation.



are diverted by crackdowns, what percentage continue to be trafficked along the original routes, and what percentage are no longer grown in Mexico, this would require making potentially untenable assumptions about drug cultivation and seizure rates. I am not aware of data that could be used to calculate how seizure rates vary across space before and after crackdowns—they plausibly change in the midst of a crackdown, which is why the study does not examine the direct effects of crackdowns on confiscations—nor are there data that quantify how many drugs were initially trafficked along each route. Nevertheless, the diversion of drug traffic following PAN victories is substantial enough to increase the value of illicit drug seizures by around 18 percent along each predicted alternative route. Combined with the fact that law enforcement resources used for drug seizures and eradication were ultimately redirected during the Calderón administration to focus on steeply increasing rates of violence (National Drug Intelligence Center 2010), these results suggest that it is unlikely that the Mexican Drug War led to large sustained reductions in the supply or consumption of illicit drugs.

Given that the trafficking model predicts the diversion of drug traffic following crackdowns, it can be used as an empirical tool to assess whether crackdowns affect violence by diverting illicit activity elsewhere. Specifically, Table 7 employs the panel specification from equation (2) to test whether homicides change when a municipality acquires a predicted trafficking route. Panel A, column 1 estimates that the presence of a predicted route increases the monthly drug trade-related homicide probability from a baseline of 4.8 percent to 6.2 percent, and the effect is statistically significant at the 1 percent level. Column 2 does not find a statistically significant relationship between the homicide rate and the predicted routes indicator, although the coefficient is positive.<sup>44</sup> Columns 3 and 4 distinguish whether the municipality contains none, one, or more than one predicted route. The coefficients on the one and more than one route indicators are positive and statistically identical for the homicide probability in column 3, but when examining the homicide rate in column 4, violence increases are concentrated in municipalities with more than one route.<sup>45</sup> The coefficient on the more than one route indicator is large and statistically significant at the 5 percent level. This is consistent with the view that crackdowns spur conflicts between traffickers by diverting drug traffic to routes that are already in use.

Next, columns 5 through 8 examine the limited sample that excludes municipalities bordering a close PAN victory. While the effects are no longer statistically significant, the routes coefficients in the full and limited samples are not statistically different. The extensions developed in the next section will improve the predictive power of the limited sample. Finally, online Appendix Table A-50 documents broadly similar results for the 2007–2010 close election sample, particularly for the homicide rate measure.<sup>46</sup>

The straightforward model of drug trafficking offers additional traction over a reduced form approach. Online Appendix Tables A-53 and A-54 document that the

<sup>44</sup> When a count measure of predicted routes is used, each predicted route increases the homicide rate by 0.54 per 100,000 (SE = 0.3).

<sup>45</sup> Results for both confiscations and violence are similar when I control for having a PAN mayor (online Appendix Tables A-51 and A-52).

<sup>46</sup> Impacts on the homicide probability are not statistically significant.

TABLE 7—VIOLENCE SPILLOVERS

	Dependent variable: Drug trade-related homicide							
	Full sample				Limited sample			
	Dummy (1)	Rate (2)	Dummy (3)	Rate (4)	Dummy (5)	Rate (6)	Dummy (7)	Rate (8)
<i>Panel A. Shortest paths</i>								
Predicted routes dummy	0.014*** (0.005)	1.163 (1.251)			0.006 (0.006)	-0.702 (1.199)		
One route			0.017** (0.007)	-6.180 (3.779)			0.014 (0.010)	-5.530 (3.449)
More than one route			0.011 (0.008)	10.314** (4.746)			-0.000 (0.010)	6.164 (4.511)
<i>Panel B. Model with congestion costs</i>								
Predicted routes dummy	0.017*** (0.005)	1.870** (0.811)			0.019*** (0.006)	1.831** (0.930)		
One route			0.010 (0.006)	2.188 (1.631)			0.010 (0.007)	1.399 (0.943)
More than one route			0.020*** (0.006)	1.746* (1.058)			0.023*** (0.007)	2.019* (1.034)
Municipalities	1,869	1,869	1,869	1,869	1,562	1,562	1,562	1,562
Observations	69,153	69,153	69,153	69,153	57,794	57,794	57,794	57,794

*Notes:* The dependent variable in columns 1, 3, 5, and 7 is an indicator equal to 1 if a drug trade-related homicide occurred in a given municipality-month, and the dependent variable in columns 2, 4, 6, and 8 is the drug trade-related homicide rate per 100,000 municipal inhabitants. Columns 5–8 limit the sample to municipalities that do not border a municipality that has experienced a close PAN victory. All columns include month  $\times$  state and municipality fixed effects. Standard errors clustered by municipality and month  $\times$  state are reported in parentheses. \*\*\* Significant at the 1 percent level.

\*\* Significant at the 5 percent level.

\* Significant at the 10 percent level.

trafficking model outperforms a reduced form model that counts a municipality as treated if it borders a municipality that has inaugurated a closely elected PAN mayor during the sample period. Neither confiscations nor violence is statistically significantly correlated with the reduced form treatment, and the coefficients are smaller than when the trafficking model is used to predict drug traffic.

While the violence spillovers are relatively small in magnitude, even a modest increase in a city's homicide rate due to other municipalities' crackdowns is likely to be of relevance due to the seriousness of the offense. The overall effects of spillovers are relatively small because the violence impacts of acquiring an additional trafficking route are substantially less than the direct effects of crackdowns, and the number of municipalities who acquire an additional route as a result of crackdowns—net of those who lose a route—is also modest. Back of the envelope calculations suggest that the direct effects of crackdowns are about 30 times larger than the spillover effects, though this is plausibly a lower bound since the network model is an imperfect predictor of where spillovers occur.

### C. Extensions

This section develops several extensions of the spillovers analysis. First, it introduces more realism into the model by incorporating congestion costs and costs for

transporting drugs through rival DTOs' territories. It also estimates the cost and duration of PAN crackdowns and examines additional outcomes. Finally, while the main objective of the trafficking model is to locate the spillover effects of crackdowns, the model has more general applications to problems where understanding the economics of trafficking is important. One particularly interesting application, to the allocation of law enforcement resources, is briefly developed.

*Congestion Costs.*—This section introduces more realism into the shortest paths model by incorporating congestion costs when trafficking routes coincide. There are several reasons why edge latencies may depend on drug flows: as drug traffic increases, the probability of conflict with other traffickers may change; the quality of hiding places may decline, particularly at US entry points; and law enforcement may direct more or less attention per unit of traffic.

The setup for the model with congestion costs is as follows: as in the shortest paths model, every origin produces a unit of drugs and has a trafficker who decides how to transport those drugs to US entry points, which have a size given by the number of commercial lanes for terrestrial border crossings and the container capacity for ports. All US entry points pay the same international price for a unit of drugs. Each edge  $e$  has a cost function  $c_e(l_e, x_e)$ , where  $l_e$  is the length of the edge and  $x_e$  is the total drug flow on edge  $e$ . A trafficker's objective is to minimize the costs of transporting his municipality's drugs, taking aggregate flows as given. Recall that most trafficking decisions are made within local cells, so the assumption that traffickers are small is reasonable and simplifies the analysis. This assumption is relaxed later as a robustness check.

In equilibrium, the costs of all routes used to transport drugs from a producing municipality to the United States are equal and less than the cost that would be experienced by reallocating traffic to an unused route. These conditions were first formalized by Wardrop (1952) and characterize the Nash equilibrium of the game. Formally, an equilibrium satisfies:

- (i) For all  $p, p' \in \mathcal{P}_i$  with  $x_p, x_{p'} > 0$ ,  $\sum_{e \in p'} c_e(x_e, l_e) = \sum_{e \in p} c_e(x_e, l_e)$ .
- (ii) For all  $p, p' \in \mathcal{P}_i$  with  $x_p > 0$   $x_{p'} = 0$ ,  $\sum_{e \in p'} c_e(x_e, l_e) \geq \sum_{e \in p} c_e(x_e, l_e)$ ,

where  $\mathcal{P}_i$  denotes the set of all possible paths between producing municipality  $i$  and US entry points and  $x_p$  denotes the flow on path  $p$ . An equilibrium routing pattern always exists, and if each  $c_e$  is strictly increasing, it is unique. The equilibrium is not typically socially optimal, since traffickers do not internalize the congestion externalities. Note that the shortest paths model is a special case of the more general model where congestion costs are assumed to be zero.

Beckmann, McGuire, and Winsten (1956) proved that the equilibrium can be characterized by a straightforward optimization problem, which is stated in the estimation Appendix. For a given network, set of supplies, and specification of the congestion costs  $c_e(\cdot)$ , the problem can be solved using numerical methods, also detailed in the online Appendix.

Edge costs in this more general model are not directly observed. To make progress, the study assumes that congestion costs take a Cobb-Douglas form. In the most parsimonious specification, traffickers incur costs to enter the United States that depend on the amount of drug traffic using the entry point, normalized by the entry point's size. Formally, edges connecting Mexico to the United States (which by definition are of length zero) impose costs equal to  $\phi_t(\text{flow}_e/\text{lanes})^\delta$  for terrestrial border crossings and  $\phi_p(\text{flow}_e/\text{containers})^\delta$  for ports, where  $\{\phi_t, \phi_p, \delta\}$  are parameters that will be estimated, *lanes* is the number of commercial crossing lanes, and *containers* is the port container capacity.  $\delta$  captures the shape of congestion costs, and  $\{\phi_t, \phi_p\}$  scale these costs to the same units as physical distance. Interior edges are not congested:  $c_e^{\text{int}}(l_e, x_e) = l_e$ . The online Appendix also estimates a more flexible specification with six  $\phi$  parameters for different sizes of terrestrial crossings and ports, as well as a specification with congestion costs on crossing and interior edges:  $c_e^{\text{int}} = l_e(1 + \phi_{\text{int}}\text{flow}_e^\gamma)$ , where  $\phi_{\text{int}}$  and  $\gamma$  are parameters whose interpretations are analogous to  $\phi_t/\phi_p$  and  $\delta$ .<sup>47</sup> Congestion costs, particularly in the most flexible models, should be interpreted broadly. For example, if traffickers prefer large crossings because they are closer to US population centers, this will appear as lower estimated congestion costs for large crossings.

The above parameters, as well as a scaling parameter  $\kappa$  that maps model-predicted flows to model-predicted confiscations, are estimated using the simulated method of moments (SMM).<sup>48</sup> Every choice of the model's parameters generates a set of moments that summarize model-predicted confiscations, and the study estimates the parameters by matching these moments to their counterparts calculated from cross-sectional data on actual illicit drug confiscations during the beginning of the sample period.<sup>49</sup> The estimation Appendix specifies the SMM objective, lists the moments, and discusses inference.<sup>50</sup>

Online Appendix Table A-55 reports the parameter estimates.<sup>51</sup> All three specifications estimate convex congestion costs on US entry points ( $\delta > 1$ ), and interior congestion appears modest, with total congestion costs at US entry points 39 times larger than total interior congestion costs. This appears plausible, as US entry points are bottlenecks with a large law enforcement presence. All specifications estimate that total congestion costs are nearly as large as total distance costs. Predicted pre-period routes are shown in Figure 2.

As expected given the SMM approach, model predicted and actual pre-period confiscations are highly correlated. The more challenging test is whether the model, fitted using pre-period data, can predict changes in confiscations during later periods. I examine this by using the panel specification given in equation (2) to compare within-municipality variation in predicted routes and actual confiscations. The routes are calculated using the three congestion parameters in column 1 of online

<sup>47</sup> Results are robust to specifying interior costs as  $l_e + \phi_{\text{int}}\text{flow}_e^\gamma$ .

<sup>48</sup>  $\kappa$  likely varies with the local environment, but it is not possible to estimate this dependence.

<sup>49</sup> This lasts from December 2006, when the data become available, until the first authorities elected during the sample period took office in July 2007.

<sup>50</sup> As is often the case with choice problems, the SMM objective is not globally convex. Thus I minimize the objective using simulated annealing. It is not possible to guarantee that an estimation procedure will find the global minimum of a non-convex objective, but Monte Carlo type simulations suggest that the trafficking problem is well-behaved.

<sup>51</sup> Conley standard errors are in brackets, and robust standard errors are in parentheses.

Appendix Table A-55, and online Appendix Tables A-56 and A-57 document robustness to using alternative parameterizations of congestion.

Panel B of Table 6 shows that the estimates using the congestion model are similar to those using the shortest path model. Columns 1 and 2 estimate that when a municipality acquires a predicted route, the probability of confiscations increases by 1.3 percentage points ( $SE = 0.005$ ), and the value of confiscations increases by around 15 percent ( $SE = 0.057$ ). Columns 3 and 4 document robustness to excluding municipalities bordering a municipality that has experienced a close PAN victory, and columns 5 and 6 show that the correlations between predicted domestic routes and the presence/value of cocaine confiscations are low. Results are also robust to assuming that traffickers are non-atomic.<sup>52</sup> The similarity between the shortest paths and congestion results is consistent with a world in which the risk of confiscations by authorities and other criminals increases with time on the road, and hence traffickers prefer to use direct routes.

The shortest path and congested routing models also estimate similar violence spillovers. Panel B of Table 7 reports that the probability of drug trade-related violence increases by 1.7 percentage points ( $SE = 0.005$ ) when a municipality acquires a predicted route, and the homicide rate increases by around 2 per 100,000 ( $SE = 0.8$ ). Columns 5 through 8 document that estimates are similar in the limited sample, which drops municipalities that border a close PAN victory.<sup>53</sup>

Thus far, the model has not imposed costs for transporting drugs through territory controlled by a rival DTO. For several reasons, I focus on this as a robustness check and not as the main specification. With ideal data, one could estimate these costs by matching predicted routes for each DTO to confiscations made from that DTO. However, DTO  $\times$  municipality confiscations data are not available. Additionally, 51 percent of producing municipalities are controlled by local gangs, and there is not information on which larger organizations, if any, these groups coordinate with to transport drugs. Finally, DTO costs impose player-specific edge latencies, and a trafficking equilibrium may not exist (Gairing, Monien, and Tiemann 2011). In any case, least-cost routes between producing municipalities controlled by a major DTO and US entry points tend to pass primarily through territory controlled by that DTO, so territorial costs may not influence the trafficking equilibrium much.

In the territorial costs robustness check, decision-makers—consisting of a single representative for each DTO and drug producing gang—minimize the costs of transporting their group's drugs to the United States. Costs are incurred from passing through another DTO's territory, as well as from distance or distance and congestion.<sup>54</sup> Online Appendix Table A-58 shows that the coefficients from regressing actual confiscations or drug-related homicides on the predicted routes dummy are

<sup>52</sup>When I assume that non-atomic DTOs and local gangs make trafficking decisions, with a single decision-maker for each DTO, predicted routes are still statistically significantly correlated with actual confiscations, but these predicted routes lose a horse race to the atomic decision-maker predicted routes. This is consistent with the qualitative evidence that most decisions within DTOs are decentralized.

<sup>53</sup>The violence results are weaker for the 2007–2010 sample (online Appendix Table A-50), but the next section will show that they become stronger when the cost and duration of PAN crackdowns are estimated.

<sup>54</sup>To solve the routing game for a given set of parameters, the best response functions are iterated to convergence. Although an equilibrium is not guaranteed to exist, simulations show that the best responses always converge to a unique equilibrium regardless of the starting point. Parameters are estimated using SMM, and the moments are listed in the online Appendix.

positive and often—though not always—significant, regardless of whether congestion costs are estimated or assumed to be zero. When I regress actual confiscations on both the routes dummy from this model and the routes dummy from the model without territorial costs, only the coefficient on the latter is large and statistically significant (regardless of whether congestion costs are included), suggesting that territorial costs are noisily inferred from available data.

*Estimating the Costs and Duration of PAN Crackdowns.*—The baseline model assumes that crackdowns impose infinite costs on trafficking drugs through a municipality and persist through the end of the sample period. In order to examine robustness to these assumptions, I use the simulated method of moments to estimate the cost and duration of PAN crackdowns. Specifically, I match confiscations to predicted routes using data from all available periods, choosing the crackdown cost and duration parameters such that the evolution of confiscations matches the evolution of predicted trafficking routes as closely as possible. Unlike in the baseline model—where the congestion parameters are estimated using a single cross-section—the PAN cost and duration parameters are identified from changes in confiscation patterns when the PAN takes office. I cannot validate the richer model by comparing changes in predicted routes to changes in confiscations because they match by construction, and for this reason I examine this model as a robustness check and not as a main specification. The estimates of the violence spillovers of crackdowns are similar to the baseline model, as the PAN cost parameter is sufficiently high that most drug traffic circumvents PAN municipalities, and the crackdown duration parameter is not binding for most municipalities. Since the direct effects of PAN crackdowns on violence also persist for as long as data are available, the direct and spillover results appear quite consistent.

If the effects of PAN victories on trafficking are similar regardless of the margin of victory, the model's predictions could plausibly be improved by imposing a cost to pass through all municipalities with a PAN mayor. Using the approach outlined above, I estimate the cost and duration of PAN victories using the close elections and then plug these parameters in for all municipalities that elect a PAN mayor, regardless of the margin of victory. Estimates using routes predicted by the richer model are presented in online Appendix Table A-59. Estimates are similar to the baseline model when 2007–2008 elections are used, whereas the fit improves substantially when 2007–2010 elections are examined.

*Additional Outcomes.*—In order to assess the full effects of the Mexican drug war, one would ideally examine an extensive range of outcomes, including drug consumption, drug prices, and economic variables. Much of these data are not available at a sufficiently disaggregated level, but labor force data can be used to shed light on the economic effects of crackdowns. The National Occupation and Employment Survey surveys municipalities on a quarterly basis. While it does not offer enough coverage of the RD sample to elucidate the direct economic effects of crackdowns, the sample is larger when considering spillovers.<sup>55</sup>

<sup>55</sup> Moreover, it is possible that PAN mayors might influence economic outcomes in their municipalities through channels besides security policy.

While the results are noisy, there is little compelling evidence for positive average wage impacts that would be substantial enough to outweigh the costs of the Mexican drug war. Online Appendix Table A-60 shows that—when the congested routing model is used—the presence of a predicted trafficking route lowers female labor force participation by 1.3 percentage points ( $SE = 0.57$ ), relative to an average female participation rate of 51 percent, but does not affect male labor force participation. It also documents that predicted routes are uncorrelated with prime-aged male formal sector wages, whereas informal sector earnings fall by around 2.3 percent ( $SE = 1.3$ ) when a municipality acquires a predicted route.<sup>56</sup> When the shortest path routes are used, estimates are statistically identical to the estimates using the congested routes but are not statistically different from zero. Informal sector workers—and women more generally—are often self-employed, and the survey calculates their earnings as monthly profits divided by hours worked. If informal sector workers report their earnings as net of rents extracted by traffickers, then extortion by drug gangs—which appears to be widespread—could explain the decline (Díaz-Cayeros et al. 2011).

*Endogenizing Crackdowns.*—The routing model’s insights into the economics of trafficking can be applied to other important questions faced by law enforcement and national security officials. While the study’s focus is on evaluating the impacts of crackdowns, this section provides an illustrative example of how the routing model can be applied to a different question: how to make crackdowns more efficient, conditional on pursuing them. This question is highly relevant in the current policy environment, but it is treated as a secondary result since the study’s empirical estimates raise questions about the desirability of cracking down in the first place.

The setup for this application is as follows. The trafficking model is embedded in a Stackelberg network game (Başar and Srikant 2002), with the government deciding in the first stage how to allocate law enforcement resources to edges in the road network, subject to a budget constraint. Traffickers’ costs of traversing an edge increase when law enforcement resources are placed on it. In the second stage, traffickers simultaneously select least-cost routes to the United States. The government’s objective is to maximize the total costs that traffickers incur, and each trafficker minimizes his own costs. This framework can accommodate multiple types of resources with deployment costs that vary by edge. The most closely related work methodologically is a study by Israeli and Wood (2002), which develops an efficient algorithm to solve for which  $k$  edges to remove in the context of a noncongested shortest path problem on a directed graph with a single origin and destination. This study’s problem examines an enriched setting with congestion and multiple origins and destinations.<sup>57</sup>

There are several things to note about how the network structure conditions the equilibrium allocation of law enforcement resources. First, while a naive policy might allocate law enforcement to edges with the most drug traffic, the network model highlights that the extent to which law enforcement affects trafficking costs

<sup>56</sup> Formal sector workers are those that contribute to the federal social security system.

<sup>57</sup> While there are other studies that focus on removing key nodes or edges from a network—notably Ballester, Calvo-Armengol, and Zenou (2006, 2010)—since the network interactions modeled by these studies are different the techniques for solving them are likewise quite distinct.

depends on available detours. In fact, increasing an edge's latency can decrease total trafficking costs if there are externalities from congestion. This result, known as Braess's (1968) paradox, occurs for 15 percent of the edges in the congested trafficking equilibrium. Moreover, the effects of law enforcement in different locations are interconnected through the network structure, implying that resource allocation decisions should be made jointly rather than on a location-by-location basis.

The online Appendix provides an illustrative example of how this framework can be used to inform the allocation of scarce law enforcement resources. It first shows that the government's allocation problem is NP hard, which implies that the time required to solve for the optimum increases quickly as the size of the problem grows. Intuitively, the problem is challenging because allocating resources to two edges at the same time might increase the government's objective function more than the summation of changes in the objective when resources are allocated to each edge separately. Hence the order in which a solution algorithm proceeds may matter. The estimation Appendix develops an algorithm for solving the game, and online Appendix Table A-61 documents robustness to changing the algorithm's details.

If the government has enough resources to triple the latencies on 25 edges (this is the factor by which PAN victories are estimated to increase edge costs), this will increase traffickers' total predicted costs by 17 percent. This exercise illustrates how the network model can contribute unique information to interdiction efforts, as the 25 edges chosen differ from the 25 most trafficked edges.

This exercise is a simplified illustrative example, but with appropriate data it would be straightforward to extend the model to make it more realistic. Multiple types of law enforcement resources could be incorporated, as well as deployment costs that vary by edge. Future work could also examine mixed strategies by law enforcement, which may be desirable but are beyond the scope of the current study.

#### IV. Conclusion

This study examines the direct and spillover effects of Mexican crackdowns on the drug trade, documenting the following results. First, regression discontinuity estimates show that violence in a municipality increases substantially after the close election of a PAN mayor and remains higher throughout the mayor's term and beyond. The empirical evidence suggests that the violence largely reflects rival traffickers' attempts to wrest control of territories after crackdowns initiated by PAN mayors have challenged the incumbent criminals. Moreover, an economic model of equilibrium trafficking routes predicts the diversion of drug traffic following close PAN victories. This diversion is large enough to make a substantial difference in illicit drug seizures along alternative routes, and when drug traffic is diverted to other municipalities, violence in these municipalities increases.

These results demonstrate how traffickers' economic objectives and constraints imposed by the routes network have conditioned the policy outcomes of the Mexican drug war. While further research is needed to identify the causal effects of crackdowns on additional outcomes and in alternative contexts, overall the results of the study suggest emphasizing policies to deter violence and improve the enforcement of homicide laws, as opposed to policies whose primary objective is to reduce drug trafficking.



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