Essays in Economic Development and Political Economy

by

Melissa Dell

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Abstract

This thesis examines three topics. The first chapter, entitled "Persistent Effects of Peru's Mining *Mita*" utilizes regression discontinuity to examine the long-run impacts of the *mita*, an extensive forced mining labor system in effect in Peru and Bolivia between 1573 and 1812. Results indicate that a *mita* effect lowers household consumption by around 25% and increases the prevalence of stunted growth in children by around six percentage points in subjected districts today. Using data from the Spanish Empire and Peruvian Republic to trace channels of institutional persistence, I show that the *mita*'s influence has persisted through its impacts on land tenure and public goods provision. *Mita* districts historically had fewer large landowners and lower educational attainment. Today, they are less integrated into road networks, and their residents are substantially more likely to be subsistence farmers.

The second chapter, entitled "Trafficking Networks and the Mexican Drug War" examines how drug traffickers' economic objectives influence the direct and spillover effects of Mexican policy towards the drug trade. Drug trade-related violence has escalated dramatically in Mexico during the past five years, claiming over 40,000 lives. By exploiting variation from close mayoral elections and a network model of drug trafficking, the study develops three sets of results. First, regression discontinuity estimates show that drug trade-related violence in a municipality increases substantially after the close election of a mayor from the conservative National Action Party (PAN), which has spearheaded the war on drug trafficking. This violence consists primarily of individuals involved in the drug trade killing each other. The empirical evidence suggests that the violence reflects rival traffickers' attempts to wrest control of territories after crackdowns initiated by PAN mayors have challenged the incumbent criminals. Second, the study predicts the diversion of drug traffic following close PAN victories by estimating a model of equilibrium routes for trafficking drugs across the Mexican road network to the U.S. When drug traffic is diverted to other municipalities, drug trade-related violence in these municipalities increases. Moreover, female labor force participation and informal sector wages fall, corroborating qualitative evidence that traffickers extort informal sector producers. Finally, the study uses the trafficking model and estimated spillover effects to examine the allocation of law enforcement resources. Overall, the results demonstrate how traffickers' economic objectives and constraints imposed by the routes network affect the policy outcomes of the Mexican Drug War.

The third chapter, entitled "Insurgency and Long-Run Development: Lessons from the Mexican Revolution" exploits within-state variation in drought severity to identify how insurgency during the Mexican Revolution, a major early 20th century armed conflict, impacted subsequent government policies and long-run economic development. Using a novel municipal-level dataset on revolutionary insurgency, the study documents that municipalities experiencing severe drought just prior to the Revolution were substantially more likely to have insurgent activity than municipalities where drought was less severe. Many insurgents demanded land reform, and following the Revolution, Mexico redistributed over half of its surface area in the form of *ejidos*: farms comprised of individual and communal plots that were granted to a group of petitioners. Rights to *ejido* plots were non-transferable, renting plots was prohibited, and many decisions about the use of *ejido* lands had to be countersigned by politicians. Instrumental variables estimates show that municipalities with revolutionary insurgency had 22 percentage points more of their surface area redistributed as *ejidos*. Today, insurgent municipalities are 20 percentage points more agricultural and 6 percentage points less industrial. Incomes in insurgent municipalities are lower and alternations between political parties for the mayorship have been substantially less common. Overall, the results support the hypothesis that land reform, while successful at placating insurgent regions, stymied long-run economic development.

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Chapter 1

Persistent Effects of Peru's Mining *Mita*

1.1 Introduction

The role of historical institutions in explaining contemporary underdevelopment has generated significant debate in recent years.[1] Studies find quantitative support for an impact of history on current economic outcomes (Nunn, 2008; Glaeser and Shleifer, 2002; Acemoglu et al., 2001, 2002; Hall and Jones, 1999) but have not focused on channels of persistence. Existing empirical evidence offers little guidance in distinguishing a variety of potential mechanisms, such as property rights enforcement, inequality, ethnic fractionalization, barriers to entry, and public goods. This paper uses variation in the assignment of an historical institution in Peru to identify land tenure and public goods as channels through which its effects persist.

Specifically, I examine the long-run impacts of the mining *mita*, a forced labor

[1]See for example Coatsworth, 2005; Glaeser et al., 2004; Easterly and Levine, 2003; Acemoglu et al., 2001, 2002; Sachs, 2001; Engerman and Sokoloff, 1997.

system instituted by the Spanish government in Peru and Bolivia in 1573 and abolished in 1812. The *mita* required over 200 indigenous communities to send one seventh of their adult male population to work in the Potosí silver and Huancavelica mercury mines (Figure 1). The contribution of *mita* conscripts changed discretely at the boundary of the subjected region - on one side all communities sent the same percentage of their population, while on the other side all communities were exempt.

This discrete change suggests a regression discontinuity (RD) approach for evaluating the long-term effects of the *mita*, with the *mita* boundary forming a multi-dimensional discontinuity in longitude-latitude space. Because validity of the RD design requires all relevant factors besides treatment to vary smoothly at the *mita* boundary, I focus exclusively on the portion that transects the Andean range in southern Peru. Much of the boundary tightly follows the steep Andean precipice - and hence has elevation and the ethnic distribution of the population changing discretely at the boundary. In contrast, elevation, the ethnic distribution, and other observables are statistically identical across the segment of the boundary on which this study focuses. Moreover, specification checks using detailed census data on local tribute (tax) rates, the allocation of tribute revenue, and demography - collected just prior to the *mita*'s institution in 1573 - do not find differences across this segment. The multi-dimensional nature of the discontinuity raises interesting and important questions about how to specify the RD polynomial, which will be explored in detail.

Using the RD approach and household survey data, I estimate that a long-run *mita* effect lowers equivalent household consumption by around 25% in subjected districts today. Although the household survey provides little power for estimating relatively flexible models, the magnitude of the estimated *mita* effect is robust to a number of alternative specifications. Moreover, data from a national height census of school children provide robust evidence that the *mita*'s persistent impact increases childhood stunting by around six percentage points in subjected districts today. These baseline results support the well-known hypothesis that extractive historical institutions influence long-run economic prosperity (Acemoglu et al., 2002). More generally, they provide microeconomic evidence consistent with studies establishing a relationship between historical institutions and contemporary economic outcomes using aggregate data (Nunn, 2008; Banerjee and Iyer, 2005; Glaeser and Shleifer, 2002).

After examining contemporary living standards, I use data from the Spanish Empire and Peruvian Republic, combined with the RD approach, to investigate channels of persistence. Though a number of channels may be relevant, to provide a parsimonious yet informative picture I focus on three that the historical literature and fieldwork highlight as important. First, using district level data collected in 1689, I document that *haciendas* - rural estates with an attached labor force - developed primarily outside the *mita* catchment. At the time of the *mita*'s enactment, a landed elite had not yet formed. In order to minimize the competition the state faced in accessing scarce *mita* labor, colonial policy restricted the formation of *haciendas* in *mita* districts, promoting communal land tenure there instead (Garrett, 2005; Larson, 1988). The *mita*'s effect on *hacienda* concentration remained negative and significant in 1940. Second, econometric evidence indicates that a *mita* effect lowered education historically, and today *mita* districts remain less integrated into road networks. Finally, data from the most recent agricultural census provides evidence that a long-run *mita* impact increases the prevalence of subsistence farming.

Based on the quantitative and historical evidence, I hypothesize that the long-term presence of large landowners in non-*mita* districts provided a stable land tenure system that encouraged public goods provision. The property rights of large landowners remained secure from the 17th century onward. In contrast, the Peruvian government abolished the communal land tenure that had predominated in *mita* districts soon after the *mita* ended, but did not replace it with a system of enforceable peasant titling (Jacobsen, 1993; Dancuart and Rodriguez, 1902, vol. 2, p. 136). As a result, extensive confiscation of peasant lands, numerous responding peasant rebellions, as well as banditry and livestock rustling were concentrated in *mita* districts during the late 19th and 20th centuries (Jacobsen, 1993; Bustamante Otero, 1987, p. 126-130; Flores Galindo, 1987, p. 240; Ramos Zambrano, 1985, p. 29-34). Because established landowners in non-*mita* districts enjoyed more secure title to their property, it is probable that they received higher returns from investing in public goods. Moreover, historical evidence indicates that well-established landowners possessed the political connections required to secure public goods (Stein, 1980). For example, the *hacienda* elite lobbied successfully for roads, obtaining government funds for engineering expertise and equipment and organizing labor provided by local citizens and *hacienda* peons (Stein, 1980, p. 59). These roads remain and allow small-scale agricultural producers to access markets today, though *haciendas* were subdivided in the 1970s.

The positive association between historical *haciendas* and contemporary economic development contrasts with the well-known hypothesis that historically high land inequality is the fundamental cause of Latin America's poor long-run growth performance (Engerman and Sokoloff, 1997). Engerman and Sokoloff argue that high historical inequality *lowered* subsequent investments in public goods, leading to worse outcomes in areas of the Americas that developed high land inequality during the colonial period. This theory's implicit counterfactual to large landowners is secure, enfranchised smallholders, of the sort that predominated in some parts of North America. This is not an appropriate counterfactual for Peru, or many other places in Latin America, because institutional structures largely in place before the formation of the landed elite did not provide secure property rights, protection from exploitation, or a host of other guarantees to potential smallholders.[2] The evidence in this study indicates that large landowners - while they did not aim to promote economic prosperity for the masses - did shield individuals from exploitation by a highly extractive state and ensure public goods. Thus, it is unclear whether the Peruvian masses would have been better off if initial land inequality had been lower, and doubtful that initial land inequality is the most useful foundation for a theory of long-run growth. Rather, the Peruvian example suggests that exploring constraints on how the state can be used to shape economic interactions - for example, the extent to which elites can employ state machinery to coerce labor or citizens can use state guarantees to protect their property - could provide a particularly useful starting point for modeling Latin America's long-run growth trajectory.

In the next section, I provide an overview of the *mita*. Section 3 discusses identification and tests whether the *mita* affects contemporary living standards. Section 4 examines channels empirically. Finally, Section 5 offers concluding remarks.

1.2 The Mining *Mita*

1.2.1 Historical Introduction

The Potosí mines, discovered in 1545, contained the largest deposits of silver in the Spanish Empire, and the state-owned Huancavelica mines provided the mercury required to refine silver ore. Beginning in 1573, indigenous villages located within a contiguous region were required to provide one seventh of their adult male popula-

[2] This argument is consistent with evidence on long-run inequality from other Latin American countries, notably Acemoglu et al. (2008) on Cundinamarca, Colombia and Coatsworth (2005) on Mexico.

tion as rotating *mita* laborers to Potosí or Huancavelica, and the region subjected remained constant from 1578 onwards.[3] The *mita* assigned 14,181 conscripts from southern Peru and Bolivia to Potosí and 3,280 conscripts from central and southern Peru to Huancavelica (Bakewell, 1984, p. 83).[4] Using population estimates from the early 17th century (Cook, 1981), I calculate that around 3% of adult males living within the current boundaries of Peru were conscripted to the *mita* at a given point in time. The percentage of males who at some point participated was considerably higher, as men in subjected districts were supposed to serve once every seven years.[5]

Local native elites were responsible for collecting conscripts, delivering them to the mines, and ensuring that they reported for mine duties (Cole, 1985, p. 15; Bakewell, 1984). If community leaders were unable to provide their allotment of conscripts, they were required to pay in silver the sum needed to hire wage laborers instead. Historical evidence suggests that this rule was strictly enforced (Garrett, 2005, p. 126; Cole, 1985, p. 44; Zavala, 1980; Sanchez- Albornoz, 1978). Some communities did commonly meet *mita* obligations through payment in silver, particularly those in present-day Bolivia who had relatively easy access to coinage due to their proximity to Potosí (Cole, 1985). Detailed records of *mita* contributions from the 17th, 18th, and early 19th centuries indicate that communities in the region that this paper examines contributed primarily in people (Tandeter, 1993, p. 56, 66; Zavala,

[3]The term *mita* was first used by the Incas to describe the system of labor obligations, primarily in local agriculture, that supported the Inca state (D'Altoy, 2002, p. 266; Rowe, 1946, 267-269). While the Spanish co-opted this phrase, historical evidence strongly supports independent assignment. Centrally, the Inca *m'ita* required every married adult male in the Inca Empire (besides leaders of large communities), spanning an area far more extensive than the region I examine, to provide several months of labor services for the state each year (D'Altoy, 2002, p. 266; Cieza de León (1967 [1551])).

[4]Individuals could attempt to escape *mita* service by fleeing their communities, and a number pursued this strategy (Wightman, 1993). Yet fleeing had costs - giving up access to land, community, and family; facing severe punishment if caught; and either paying additional taxes in the destination location as a 'foreigner' (*forastero*) or attaching oneself to an *hacienda*.

[5]*Mita* districts contain 17% of the Peruvian population today (INEI, 1993).

1980, II, p. 67-70). This is corroborated by population data collected in a 1689 parish census (Villanueva Urteaga, 1982), described in the appendix, which shows that the male-female ratio was 22% lower in *mita* districts (a difference significant at the 1% level).[6]

With silver deposits depleted, the *mita* was abolished in 1812, after nearly 240 years of operation. Sections 3 and 4 will discuss historical and empirical evidence showing divergent histories of *mita* and non-*mita* districts.

1.2.2 The *Mita*'s Assignment

Why did Spanish authorities require only a portion of districts in Peru to contribute to the *mita*, and how did they determine which districts to subject? The aim of the Crown was to revive silver production to levels attained using free labor in the 1550s, before epidemic disease had substantially reduced labor supply and increased wages. Yet coercing labor imposed costs: administrative and enforcement costs, compensation to conscripts for traveling as much as 1,000 kilometers each way to and from the mines, and the risk of decimating Peru's indigenous population, as had occurred in earlier Spanish mining ventures in the Caribbean (Tandeter, 1993, p. 61; Cole, 1985, p. 3, 31; Cañete, 1973 [1794]; Levillier, 1921 [1572], 4, p. 108). To establish the minimum number of conscripts needed to revive production to 1550s levels, Viceroy Francisco Toledo commissioned a detailed inventory of mines and production processes in Potosí and elsewhere in 1571 (Bakewell, 1984, p. 76-78; Levillier, 1921 [1572], 4). These numbers were used, together with census data collected in the early 1570's, to enumerate the *mita* assignments. The limit that the

[6]While colonial observers highlighted the deleterious effects of the *mita* on demography and well-being in subjected communities, there are some features that could have promoted relatively better outcomes. For example, *mita* conscripts sold locally produced goods in Potosi, generating trade linkages.

mita subject no more than one seventh of a community's adult male population at a given time was already an established rule that regulated local labor drafts in Peru (Glave, 1989). Together with estimates of the required number of conscripts, this rule roughly determined what fraction of Andean Peru's districts would need to be subjected to the *mita*.

Historical documents and scholarship reveal two criteria used to assign the *mita*: distance to the mines at Potosí and Huancavelica and elevation. Important costs of administering the *mita*, such as travel wages and enforcement costs, were increasing in distance to the mines (Tandeter, 1993, p. 60; Cole, 1985, p. 31). Moreover, Spanish officials believed that only highland peoples could survive intensive physical labor in the mines, located at over 4000 meters (13,000 feet) (Golte, 1980). The geographic extent of the *mita* is consistent with the application of these two criteria, as can be seen in Figure 1.[7] This study focuses on the portion of the *mita* boundary that transects the Andean range, which this figure highlights in white, and the districts along this portion are termed the study region (see Appendix Figure 1 for a detailed view). Here, exempt districts were the ones located furthest from the mining centers given road networks at the time (Hyslop, 1984).[8] While historical documents do not

[7]An elevation constraint was binding along the eastern and western *mita* boundaries, which tightly follow the steep Andean precipice. The southern Potosí *mita* boundary was also constrained, by the border between Peru and the Viceroyalty of Rio de la Plata (Argentina) and by the geographic divide between agricultural lands and an uninhabitable salt flat.

[8]This discussion suggests that exempt districts were those located relatively far from both Potosí and Huancavelica. The correlation between distance to Potosí and distance to Huancavelica is -0.996, making it impossible to separately identify the effect of distance to each mine on the probability of receiving treatment. Thus, I divide the sample into two groups - municipalities to the east and those to the west of the dividing line between the Potosí and Huancavelica mita catchment areas. When considering districts to the west (Potosí side) of the dividing line, a flexible specification of mita treatment on a cubic in distance to Potosí, a cubic in elevation, and their linear interaction shows that being 100 additional kilometers from Potosí lowers the probability of treatment by 0.873, with a standard error of 0.244. Being 100 meters higher increases the probability of treatment by 0.061, with a standard error of 0.027. When looking at districts to the east (Huancavelica side) of the dividing line and using an analogous specification with a polynomial

mention additional criteria, concerns remain that other underlying characteristics may have influenced *mita* assignment. This will be examined further in Section 3.2.

1.3 The *Mita* and Long Run Development

1.3.1 Data

I examine the *mita*'s long run impact on economic development by testing whether it affects living standards today. A list of districts subjected to the *mita* is obtained from Saignes (1984) and Amat y Junient (1947) and matched to modern districts as detailed in the online appendix, Table A1. Peruvian districts are in most cases small political units that consist of a population center (the district capital) and its surrounding countryside. *Mita* assignment varies at the district level.

I measure living standards using two independent datasets, both geo-referenced to the district. Household consumption data are taken from the 2001 Peruvian National Household Survey (ENAHO) collected by the National Institute of Statistics (INEI). To construct a measure of household consumption that reflects productive capacity, I subtract the transfers received by the household from total household consumption, and normalize to Lima metropolitan prices using the deflation factor provided in ENAHO. I also utilize a micro census dataset, obtained from the Ministry of Education, that records the heights of all six to nine year old school children in the region. Following international standards, children whose heights are more than two standard deviations below their age-specific median are classified as stunted, with the medians and standard deviations calculated by the World Health Organization from an international reference population. Because stunting is related

in distance to Huancavelica, the marginal effect of distance to Huancavelica is negative but not statistically significant.

to malnutrition, to the extent that living standards are lower in *mita* districts, we would also expect stunting to be more common there. The height census has the advantage of providing substantially more observations from about four times more districts than the household consumption sample. While the height census includes only children enrolled in school, 2005 data on primary school enrollment and completion rates do not show statistically significant differences across the *mita* boundary, with primary school enrollment rates exceeding 95% throughout the region examined (MINEDU, 2005b). Finally, to obtain controls for exogenous geographic characteristics, I calculate the mean area weighted elevation of each district by overlaying a map of Peruvian districts on 30 arc second (one kilometer) resolution elevation data produced by NASA's Shuttle Radar Topography Mission (SRTM, 2000), and I employ a similar procedure to obtain each district's mean area weighted slope. The online appendix contains more detailed information about these data and the living standards data, as well as about the data examined in Section 4.

1.3.2 Estimation Framework

Mita treatment is a deterministic and discontinuous function of known covariates, longitude and latitude, which suggests estimating the *mita*'s impacts using a regression discontinuity approach. The *mita* boundary forms a multi-dimensional discontinuity in longitude-latitude space, which differs from the single-dimensional thresholds typically examined in RD applications. While the identifying assumptions are identical to those in a single-dimensional RD, the multi-dimensional discontinuity raises interesting and important methodological issues about how to specify the RD polynomial, as discussed below. Before considering this and other identification issues in detail, let us introduce the basic regression form:

$$c_{idb} = \alpha + \gamma \; mita_d + X'_{id}\beta + f(\text{geographic location}_d) + \phi_\text{b} + \epsilon_{idb} \qquad (1.1)$$

where c_{idb} is the outcome variable of interest for observation i in district d along segment b of the *mita* boundary, and $mita_d$ is an indicator equal to 1 if district d contributed to the *mita* and equal to zero otherwise. X_{id} is a vector of covariates that includes the mean area weighted elevation and slope for district d, and (in regressions with equivalent household consumption on the lefthand side) demographic variables giving the number of infants, children, and adults in the household. $f(\text{geographic location}_d)$ is the RD polynomial, which controls for smooth functions of geographic location. Various forms will be explored. Finally, ϕ_b is a set of boundary segment fixed effects that denote which of four equal length segments of the boundary is the closest to the observation's district capital.[9] To be conservative, all analysis excludes metropolitan Cusco. Metropolitan Cusco is composed of seven non-*mita* and two *mita* districts located along the *mita* boundary and was the capital of the Inca Empire (Cook, 1981, p. 212-214; Cieza de León, 1959, p. 144-148). I exclude Cusco because part of its relative prosperity today likely relates to its pre-*mita* heritage as the Inca capital. When Cusco is included, the impacts of the *mita* are estimated to be even larger.

The RD approach used in this paper requires two identifying assumptions. First, all relevant factors besides treatment must vary smoothly at the *mita* boundary. That is, letting c_1 and c_0 denote potential outcomes under treatment and control, x denote longitude, and y denote latitude, identification requires that $E[c_1|x, y]$ and $E[c_0|x, y]$

[9]Results (available upon request) are robust to allowing the running variable to have heterogeneous effects by including a full set of interactions between the boundary segment fixed effects and $f(\text{geographic location}_d)$. They are also robust to including soil type indicators, which I do not include in the main specification because they are highly collinear with the longitude-latitude polynomial used for one specification of $f(\text{geographic location}_d)$.

are continuous at the discontinuity threshold. This assumption is needed for individuals located just outside the *mita* catchment to be an appropriate counterfactual for those located just inside it. To assess the plausibility of this assumption, I examine the following potentially important characteristics: elevation, terrain ruggedness, soil fertility, rainfall, ethnicity, pre-existing settlement patterns, local 1572 tribute (tax) rates, and allocation of 1572 tribute revenues.

To examine elevation - the principal determinant of climate and crop choice in Peru - as well as terrain ruggedness, I divide the study region into twenty by twenty kilometer grid cells, approximately equal to the mean size of the districts in my sample, and calculate the mean elevation and slope within each grid cell using the SRTM data.[10] These geographic data are spatially correlated, and hence I report standard errors corrected for spatial correlation in square brackets. Following Conley (1999), I allow for spatial dependence of an unknown form. For comparison, I report robust standard errors in parentheses. The first set of columns of Table 1 restricts the sample to fall within 100 kilometers of the *mita* boundary and the second, third, and fourth set of columns restrict it to fall within 75, 50, and 25 kilometers, respectively. Row 1 shows that elevation is statistically identical across the *mita* boundary.[11] I next look at terrain ruggedness, using the SRTM data to calculate the mean uphill slope in each grid cell. In contrast to elevation, there are some statistically significant, but relatively small, differences in slope, with *mita* districts being *less* rugged.[12]

[10]All results are similar if the district is used as the unit of observation instead of using grid cells.

[11]Elevation remains identical across the mita boundary if I restrict the sample to inhabitable areas (<4800 m) or weight by population, rural population, or urban population data (SEDAC, 2007).

[12]I also examined data on district soil quality and rainfall (results available upon request; see data appendix for more details). Data from the Peruvian Institute for Natural Resources (INRENA, 1997) reveal *higher* soil quality in *mita* districts. I do not emphasize soil quality because it is endogenous to land usage. While climate is exogenous, high resolution data are not available and interpolated climate estimates are notoriously inaccurate for the mountainous region examined in this study (Hijmans et al., 2005). Temperature is primarily determined by altitude (Golte, 1980;

Row 3 examines ethnicity using data from the 2001 Peruvian National Household Survey. A household is defined as indigenous if the primary language spoken in the household is an indigenous language (usually Quechua). Results show no statistically significant differences in ethnic identification across the *mita* boundary.

Spanish authorities could have based *mita* assignment on settlement patterns, instituting the *mita* in densely populated areas and claiming land for themselves in sparsely inhabited regions where it was easier to usurp. A detailed review by Bauer and Covey (2002) of all archaeological surveys in the region surrounding the Cusco basin, covering much of the study region, indicates no large differences in settlement density at the date of Spanish Conquest. Moreover, there is not evidence suggesting differential rates of population decline in the forty years between conquest and the enactment of the *mita* (Cook, 1981, p. 108-114).

Spanish officials blamed demographic collapse on excessive, unregulated rates of tribute extraction by local Hispanic elites (*encomenderos*), who received the right to collect tribute from the indigenous population in return for their role in Peru's military conquests. Thus Viceroy Francisco Toledo coordinated an in depth inspection of Peru, Bolivia, and Ecuador in the early 1570's to evaluate the maximum tribute that could be demanded from local groups without threatening subsistence. Based on their assessment of ability to pay, authorities assigned varying tribute obligations at the level of the district - socioeconomic group, with each district containing one or two socioeconomic groups. (See the appendix for more details on the tribute as-

Pulgar-Vidal, 1950), and thus is unlikely to differ substantially across the *mita* boundary. To examine precipitation, I use station data from the Global Historical Climatology Network, Version 2 (Peterson and Vose, 1997). Using all available data (from stations in 50 districts located within 100 km of the *mita* boundary), *mita* districts appear to receive somewhat *higher* average annual precipitation, and these differences disappear when comparing districts closer to the *mita* boundary. When using only stations with at least twenty years of data (to ensure a long-run average), which provides observations from twenty different stations (eleven outside the *mita* catchment and nine inside), the difference declines somewhat in magnitude and is not statistically significant.

sessment.) These per capita contributions, preserved for all districts in the study region, provide a measure of Spanish authorities' best estimates of local prosperity. Row 4 of Table 1 shows average tribute contributions per adult male (women, children, and those over age fifty were not taxed). Simple means comparisons across the *mita* boundary do not find statistically significant differences. Rows (5) through (8) examine district level data on how Spanish authorities allocated these tribute revenues, divided between rents for Spanish nobility (*encomenderos*, row (5)), salaries for Spanish priests (row (6)), salaries for local Spanish administrators (*justicias*, row (7)), and salaries for indigenous mayors (*caciques*, row (8)). The data on tribute revenue allocation are informative about the financing of local government, about the extent to which Spain extracted local revenues, and about the relative power of competing local administrators to obtain tribute revenues. Table 1 reveals some modest differences: when the sample is limited to fall within 100 km or 75 km from the *mita* boundary, we see that Spanish nobility received a slightly lower share of tribute revenue inside the *mita* catchment than outside (60% versus 64%), whereas Spanish priests received a slightly higher share (21% versus 19%). All differences disappear as the sample is limited to fall closer to the *mita* boundary.

In the ideal RD setup, the treatment effect is identified using only the variation at the discontinuity. Non-parametric RD techniques can be applied to approximate this setup in contexts with a large number of observations very near the treatment threshold (Imbens and Lemieux, 2008). While non-parametric techniques have the advantage of not relying on functional form assumptions, the data requirements that they pose are particularly high in the geographic RD context, as a convincing non-parametric RD would probably require precise geo-referencing: for example, each observation's longitude-latitude coordinates or address.[13] This information is rarely

[13] A notable example of a multi-dimensional non-parametric RD is Sandra Black's (1999) study

made available due to confidentiality restrictions, and none of the available Peruvian micro datasets contain it. Moreover, many of the datasets required to investigate the *mita*'s potential long-run effects do not provide sufficiently large sample sizes to employ non-parametric techniques. Thus, I use a semi-parametric RD approach that limits the sample to districts within 50 kilometers of the *mita* boundary. This approach identifies causal effects by using a regression model to distinguish the treatment indicator, which is a nonlinear and discontinuous function of longitude (x) and latitude (y), from the smooth effects of geographic location. It is important for the regression model to approximate these effects well, so that a nonlinearity in the counterfactual conditional mean function $E[c_0|x, y]$ is not mistaken for a discontinuity, or vice versa (Angrist, 2009). To the best of my knowledge, this is the first study to utilize a multi-dimensional, semi-parametric RD approach.

Because approaches to specifying a multi-dimensional RD polynomial have not been widely explored, I report estimates from three baseline specifications of $f(\text{geographic location}_d)$. The first approach uses a cubic polynomial in latitude and longitude.[14] This parametrization is relatively flexible; it is analogous to the standard single-dimensional RD approach; and the RD plots, drawn in "x-y-outcome" space, allow a transparent visual assessment of the data. For these reasons, this approach appears preferable to projecting the running variable into a lower-dimensional space - as I do in the other two baseline specifications - when power permits its precise estimation. One drawback is that some of the necessary datasets do not provide

of the value that parents place on school quality. Black compares housing prices on either side of school attendance district boundaries in Massachusetts. Because she employs a large and precisely geo-referenced dataset, Black is able to include many boundary segment fixed effects and limit the sample to observations located within 0.15 miles of the boundary, ensuring comparison of observations in extremely close proximity.

[14]Letting x denote longitude and y latitude, this polynomial is $x + y + x^2 + y^2 + xy + x^3 + y^3 + x^2y + xy^2$.

enough power to precisely estimate this flexible specification. The multi-dimensional RD polynomial also increases concerns about overfitting at the discontinuity, as a given order of a multi-dimensional polynomial has more degrees of freedom than the same order one-dimensional polynomial. This point is discussed using a concrete example in Section 4.3. Finally, there is no a priori reason why a polynomial form will do a good job of modeling the interactions between longitude and latitude. I partially address this concern by examining robustness to different orders of RD polynomials.

Given these concerns, I also report two baseline specifications that project geographic location into a single dimension. These single-dimensional specifications can be precisely estimated across the paper's datasets and provide useful checks on the multi-dimensional RD. One controls for a cubic polynomial in Euclidean distance to Potosí, a dimension which historical evidence identifies as particularly important. During much of the colonial period, Potosí was the largest city in the Western Hemisphere and one of the largest in the world, with a population exceeding 200,000. Historical studies document distance to Potosí as an important determinant of local production and trading activities and access to coinage (Tandeter, 1993, p. 56; Glave, 1990; Cole, 1985).[15] Thus, a polynomial in distance to Potosí is likely to capture variation in relevant unobservables. However, this approach does not map well into the traditional RD setup, though it is similar in controlling for smooth variation and requiring all factors to change smoothly at the boundary. Thus I also examine a specification that controls for a cubic polynomial in distance to the *mita* boundary. I report this specification because it is similar to traditional one-dimensional RD

[15]Potosí traded extensively with the surrounding region, given that it was located in a desert 14,000 feet above sea level and that it supported one of the world's largest urban populations during the colonial period.

designs, but to the best of my knowledge neither historical nor qualitative evidence suggest that distance to the *mita* boundary is economically important. Thus, this specification is most informative when examined in conjunction with the other two.

In addition to the two identifying assumptions already discussed, an additional assumption often employed in RD is no selective sorting across the treatment threshold. This would be violated if a direct *mita* effect provoked substantial out-migration of relatively productive individuals, leading to a larger indirect effect. Because this assumption may not be fully reasonable, I do not emphasize it. Rather I explore the possibility of migration as an interesting channel of persistence, to the extent that the data permit. During the past 130 years, migration appears to have been low. Data from the 1876, 1940, and 1993 population censuses show a district level population correlation of 0.87 between 1940 and 1993 for both *mita* and non-*mita* districts.[16] Similarly, the population correlation between 1876 and 1940 is 0.80 in *mita* districts and 0.85 in non-*mita* districts. While a constant aggregate population distribution does not preclude extensive sorting, this is unlikely given the relatively closed nature of indigenous communities and the stable linkages between *haciendas* and their attached peasantry (Morner, 1978). Moreover, the 1993 Population Census does not show statistically significant differences in rates of out-migration between *mita* and non-*mita* districts, though the rate of in-migration is 4.8% higher outside the *mita* catchment. In considering why individuals do not arbitrage income differences between *mita* and non-*mita* districts, it is useful to note that over half of the population in the region I examine lives in formally recognized indigenous communities. It tends to be difficult to gain membership and land in a different indigenous community, making large cities - which have various disamenities - the primary feasible destination for most migrants (INEI, 1993).

[16]The 2005 Population Census was methodologically flawed, and thus I use 1993.

In contrast, out-migration from *mita* districts during the period that the *mita* was in force may have been substantial. Both Spanish authorities and indigenous leaders of *mita* communities had incentives to prevent migration, which made it harder for local leaders to meet *mita* quotas that were fixed in the medium run and threatened the *mita*'s feasibility in the longer run. Spanish authorities required individuals to reside in the communities to which the colonial state had assigned their ancestors soon after Peru's conquest to receive citizenship and access to agricultural land. Indigenous community leaders attempted to forcibly restrict migration. Despite these efforts, the state's capacity to restrict migration was limited, and 17th century population data - available for 15 *mita* and 14 non-*mita* districts - provide evidence consistent with the hypothesis that individuals migrated disproportionately from *mita* to non-*mita* districts.[17] To the extent that flight was selective and certain cognitive skills, physical strength, or other relevant characteristics are highly heritable, so that initial differences could persist over several hundred years, historical migration could contribute to the estimated *mita* effect. The paucity of data and complex patterns of heritability that would link historically selective migration to the present unfortunately place further investigation substantially beyond the scope of the current paper.

I begin by estimating the *mita*'s impact on living standards today. First, I test for a *mita* effect on household consumption, using the log of equivalent household consumption, net transfers, in 2001 as the dependent variable. Following Deaton (1997), I assume that children aged 0 to 4 are equal to 0.4 adults and children aged 5 to 14 are equal to 0.5 adults. Panel A reports the specification that includes a cubic

[17]According to data from the 1689 Cusco parish reports (see the appendix), in the 14 non-*mita* districts 52.5% of individuals had ancestors who had not been assigned to their current district of residence, as compared to 35% in the 15 *mita* districts.

polynomial in latitude and longitude, Panel B the one that uses a cubic polynomial in distance to Potosí, and Panel C the one that includes a cubic polynomial in distance to the *mita* boundary. The first column of Table 2 limits the sample to districts within 100 kilometers of the *mita* boundary, and columns (2) and (3) restrict it to fall within 75 and 50 kilometers, respectively.[18] Columns (4) through (7) repeat this exercise, using as the dependent variable a dummy equal to one if the child's growth is stunted and equal to zero otherwise. Column (4) limits the sample to districts within 100 kilometers of the *mita* boundary, and columns (5) and (6) restrict it to fall within 75 and 50 kilometers, respectively. Column (7) limits the sample to only those districts bordering the *mita* boundary. When combined with the inclusion of boundary segment fixed effects, this ensures that I am comparing observations in close geographic proximity.

1.3.3 Estimation Results

Columns (1) through (3) of Table 2 estimate that a long-run *mita* effect lowers household consumption in 2001 by around 25 percent in subjected districts. The point estimates remain fairly stable as the sample is restricted to fall within narrower bands of the *mita* boundary. Moreover, the *mita* coefficients are economically similar across the three specifications of the RD polynomial, and I am unable to reject that they are statistically identical. All of the *mita* coefficients in Panels B and C, which report the single-dimensional RD estimates, are statistically significant at the 1% or 5% level. In contrast, the point estimates using a cubic polynomial in latitude and longitude (Panel A) are not statistically significant. This imprecision likely results

[18]The single-dimensional specifications produce similar estimates when the sample is limited to fall within 25 kilometers of the *mita* boundary. The multi-dimensional specification produces a very large and imprecisely estimated *mita* coefficient because of the small sample size.

from the relative flexibility of the specification, the small number of observations and clusters (the household survey samples only around one quarter of districts), and measurement error in the dependent variable (Deaton, 1997).

Columns (4) through (7) of Table 2 examine census data on stunting in children, an alternative measure of living standards which offers a substantially larger sample. When using only observations in districts that border the *mita* boundary, point estimates of the *mita* effect on stunting range from 0.055 (s.e. = 0.030) to 0.114 (s.e. = 0.049) percentage points. This compares to a mean prevalence of stunting of 40% throughout the region examined.[19] 11 of the 12 point estimates reported in Table 2 are statistically significant, and I cannot reject at the 10% level that the estimates are the same across specifications.

The results can be seen graphically in Figure 2. Each sub-figure shows a district level-scatter plot for one of the paper's main outcome variables. These plots are the three-dimensional analogues to standard two-dimensional RD plots, with each district capital's longitude on the x-axis, its latitude on the y-axis, and the data value for that district shown using an evenly-spaced monochromatic color scale, as described in the legends. When the underlying data are at the micro level, I take district level averages, and the size of the dot indicates the number of observations in each district. Importantly, the scaling on these dots, which is specified in the legend, is nonlinear, as otherwise some would be microscopic and others too large to display. The background in each plot shows predicted values, for a finely spaced grid of longitude-latitude coordinates, from a regression of the outcome variable under consideration on a cubic polynomial in longitude-latitude and the *mita* dummy. In

[19] A similar picture emerges when I use height in centimeters as the dependent variable and include quarter x year of birth dummies, a gender dummy, and their interactions on the righthand side.

the typical RD context, the predicted value plot is a two-dimensional curve, whereas here it is a three-dimensional surface, with the third dimension indicated by the color gradient.[20] The shades of the data points can be compared to the shades of the predicted values behind them to judge whether the RD has done an adequate job of averaging the data across space. The majority of the population in the region is clustered along the upper segment of the *mita* boundary, giving these districts substantially more weight in figures showing predicted values from micro-level regressions.

Table 3 examines robustness to fourteen different specifications of the RD polynomial, documenting *mita* effects on household consumption and stunting that are generally similar across specifications. The first three rows report results from alternative specifications of the RD polynomial in longitude-latitude: linear, quadratic, and quartic. The next five rows report alternative specifications using distance to Potosí: linear, quadratic, quartic, and the *mita* dummy interacted with a linear or quadratic polynomial in distance to Potosí.[21] Next, rows (9) through (13) examine robustness to the same set of specifications, using distance to the *mita* boundary as the running variable. Finally, row (14) reports estimates from a specification using ordinary least squares. The *mita* effect on consumption is always statistically significant in the relatively parsimonious specifications: those that use non-interacted, single-dimensional RD polynomials and ordinary least squares. In the more flexible specifications - the longitude-latitude regressions and those that interact the RD polynomial with the *mita* dummy - the *mita* coefficients in the consumption regres-

[20]Three-dimensional surface plots of the predicted values are shown in Figure A2 of the appendix, and contour plots are available upon request.

[21]The *mita* effect is evaluated at the mean distance to Potosí for observations very near (<10 km from) the *mita* boundary. Results are broadly robust to evaluating the *mita* effect at different average distances to Potosí - i.e. for districts < 25 km from the boundary, for bordering districts, or for all districts.

sion tend to be imprecisely estimated. As in Table 2, the household survey does not provide enough power to precisely estimate relatively flexible specifications, but the coefficients are similar in magnitude to those estimated using a more parsimonious approach. Estimates of the *mita*'s impact on stunting are statistically significant across most specifications and samples.[22]

Given broad robustness to functional form assumptions, Table 4 reports a number of additional robustness checks using the three baseline specifications of the RD polynomial. To conserve space, I report estimates only from the sample that contains districts within 50 km of the *mita* boundary. Columns (1) through (7) examine the household consumption data and columns (8) through (12) the stunting data. For comparison purposes, columns (1) and (8) present the baseline estimates from Table 2. Column (2) adds a control for ethnicity, equal to one if an indigenous language is spoken in the household and zero otherwise. Next, columns (3) and (9) include metropolitan Cusco. In response to the potential endogeneity of the *mita* to Inca landholding patterns, columns (4) and (10) exclude districts that contained Inca royal estates, which served sacred as opposed to productive purposes (Niles, 1987, p. 13). Similarly, columns (5) and (11) exclude districts falling along portions of the *mita* boundary formed by rivers, to account for one way in which the boundary could be endogenous to geography. Column (6) estimates consumption equivalence flexibly, using log household consumption as the dependent variable and controlling for the ratio of children to adults and the log of household size. In all cases, point estimates and significance levels tend to be similar to those in Table 2. As expected, the point estimates are somewhat larger when metropolitan Cusco is included.

Table 4 investigates whether differential rates of migration today may be respon-

[22]Results (not shown) are also robust to including higher order polynomials in elevation and slope.

sible for living standards differences between *mita* and non-*mita* districts. Given that in-migration in non-*mita* districts is about 4.8% higher than in *mita* districts (whereas rates of out-migration are statistically and economically similar), I omit the 4.8% of the non-*mita* sample with the highest equivalent household consumption and least stunting, respectively. Estimates in columns (7) and (12) remain of similar magnitude and statistical significance, documenting that migration today is not the primary force responsible for the *mita* effect.

If the RD specification is estimating the *mita*'s long-run effect, as opposed to some other underlying difference, being inside the *mita* catchment should not affect economic prosperity, institutions, or demographics prior to the *mita*'s enactment. In a series of specification checks, I first regress the log of the mean district 1572 tribute contribution per adult male on the variables used in the stunting regressions in Table 2. I then examine the shares of 1572 tribute revenues allocated to rents for Spanish nobility, salaries for Spanish priests, salaries for local Spanish administrators, and salaries for indigenous mayors. Finally, also using data from the 1572 census, I investigate demographics, with the population shares of tribute paying males (those aged 18 to 50), boys, and women as the dependent variables. These regressions, reported in Table 5, do not show statistically significant differences across the *mita* boundary, and the estimated *mita* coefficients are small.

To achieve credible identification, I exploit variation across observations located near the *mita* boundary. If the boundary is an unusual place, these estimates may have little external validity. To examine this issue further, I use ordinary least squares to estimate the correlation between the *mita* and the main outcome variables (including those that will be examined in Section 4), limiting the sample to districts located *between* 25 km and 100 km from the *mita* boundary. The estimates are quite similar to those obtained from the RD specifications (results available upon request). Moreover, correlations between the *mita* and living standards (measured by both consumption and stunting) calculated along the entire *mita* boundary within Peru are consistent in magnitude with the effects documented above.[23] In summary, the RD evidence appears informative about the *mita*'s overall impacts.

Why would the *mita* affect economic prosperity nearly 200 years after its abolition? To open this black box, I turn to an investigation of channels of persistence.

1.4 Channels of Persistence

This section uses data from the Spanish Empire and Peruvian Republic to test channels of persistence. There exist many potential channels, but to provide a picture that is both parsimonious and informative, I focus on three that the historical literature and fieldwork suggest as important: land tenure, public goods, and market participation. The results document that the *mita* limited the establishment of large landowners inside the *mita* catchment and, combined with historical evidence, suggest that land tenure has in turn affected public goods provision and smallholder participation in agricultural markets.

The tables in the main text report three specifications, which use a cubic polynomial in latitude and longitude, a cubic polynomial in distance to Potosí, or a cubic polynomial in distance to the *mita* boundary. Appendix Table A3 reports results from the fourteen additional specifications examined in Table 3. In most cases, the point estimates across these specifications are similar. When not, I note it explicitly.[24]

[23] When considering observations in Peru within 50 kilometers of any point on the *mita* boundary, being inside the *mita* catchment is associated with 28.4 percent lower equivalent household consumption and an increase of 16.4 percentage points in the prevalence of stunting.

[24] As in Table 3, the more flexible specifications in Table A3 are less likely than the parsimonious

1.4.1 Land Tenure and Labor Systems

This section examines the impact of the *mita* on the formation of *haciendas* - rural estates with an attached labor force permanently settled on the estate (Keith, 1971, p. 437). Critically, when authorities instituted the *mita* in 1573 (forty years after the Spanish conquest of Peru), a landed elite had not yet formed. At the time, Peru was parceled into *encomiendas*, pieces of territory in which appointed Spaniards exercised the right to collect tribute and labor services from the indigenous population but did not hold title to land (Keith, 1971, p. 433). Rivalries between *encomenderos* provoked civil wars in the years following Peru's conquest, and thus the Crown began to dismantle the *encomienda* system during the 1570's. This opened the possibility for manipulating land tenure to promote other policy goals, in particular, the *mita.*[25]

Specifically, Spanish land tenure policy aimed to minimize the establishment of landed elites in *mita* districts, as large landowners - who unsurprisingly opposed yielding their attached labor for a year of *mita* service - formed the state's principal labor market competition (Larson, 1988; Sanchez-Albornoz, 1978).[26] Centrally, as Bolivian historian Brooke Larson concisely articulates: "*Haciendas* secluded peasants from the extractive institutions of colonial society" (1988, p. 171). Moreover, by protecting native access to agricultural lands, the state promoted the ability of the indigenous community to subsidize *mita* conscripts, who were paid substantially below subsistence wages (Garrett, 2005, p. 120; Tandeter, 1993, p. 58-60; Cole, 1985, p. 31). Similarly, authorities believed that protecting access to land could be

ones to estimate statistically significant effects.

[25]Throughout the colonial period, royal policy aimed to minimize the power of the (potentially revolutionary) landed class - landowners did not acquired the same political clout as mine owners, the most powerful colonial interest group (Tandeter, 1993; Cole, 1985).

[26]For example, land sales under Philip VI between 1634 and 1648 and by royal charter in 1654 played a central role in *hacienda* formation and were almost exclusively concentrated in non-*mita* districts (Brisseau, 1981, p. 146; Glave and Remy, 1978, p. 1).

an effective means of staving off demographic collapse (Larson, 1982, p. 11; Cook, 1981, p. 108-114, 250; Morner, 1978). Finally, in return for ensuring the delivery of conscripts, local authorities were permitted to extract surplus that would have otherwise been claimed by large landowners (Garrett, 2005, p. 115).

I now examine the concentration of *haciendas* in 1689, 1845, and 1940. 1689 data are contained in parish reports commissioned by Bishop Manuel de Mollinedo and submitted by all parishes in the bishopric of Cusco, which encompassed most of the study region. The reports list the number of *haciendas* and the population within each subdivision of the parish and were compiled by Horacio Villanueva Urteaga (1982). For *haciendas* in 1845, I employ data collected by the Cusco regional government, which had jurisdiction over a substantial fraction of the study region, on the percentage of the rural tributary population residing in *haciendas* (Peralta Ruiz, 1991). Data from 1845, 1846, and 1850 are combined to form the c. 1845 dataset.[27] Finally, data from the 1940 Peruvian Population Census are aggregated to the district level to calculate the percentage of the rural population residing in *haciendas.*

Table 6, column (1) (number of *haciendas* per district) and column (2) (number of *haciendas* per 1000 district residents) show a very large *mita* effect on the concentration of *haciendas* in the 17th century, of similar magnitude and highly significant across specifications.[28] The median coefficient from Column (1), contained in Panel C, estimates that the *mita* lowered the number of *haciendas* in subjected districts by 11.3 (s.e.= 2.1), a sizeable effect given that on average *mita* districts contained only one *hacienda.* Figure 2, Panel C clearly demonstrates the discontinuity. Moreover, Table 6 provides reasonably robust support for a persistent impact. Column (3)

[27]When data are available for more than one year, figures change little, and I use the earliest observation.

[28]Given the *mita*'s role in provoking population collapse (Wightman, 1990, p. 72), the latter measure is likely endogenous, but nevertheless provides a useful robustness check.

estimates that the *mita* lowered the percentage of the rural tributary population in *haciendas* in 1845 by around 20 percentage points (with estimates ranging from 0.13 to 0.21), an effect that is statistically significant across specifications. Column (4) suggests that disparities persisted into the twentieth century, with an estimated effect on the percentage of the rural labor force in *haciendas* that is somewhat smaller for 1940 than for 1845 - as can be seen by comparing Panels D and E of Figure 2 - and not quite as robust. The median point estimate is -0.12 (s.e.= 0.045) in Panel C, the point estimates are statistically significant at the 1% level in Panels B and C, but the longitude-latitude specification estimates an effect that is smaller, at -0.07, and imprecise.

Table 6 also documents that the percentage of the rural population in *haciendas* nearly doubled between 1845 and 1940, paralleling historical evidence for a rapid expansion of *haciendas* in the late 19th and early 20th centuries. This expansion was spurred by a large increase in land values due to globalization and seems to have been particularly coercive inside the *mita* catchment (Jacobsen, 1993, p. 226-237; Favre, 1967, p. 243; Nuñez, 1913, p. 11). No longer needing to ensure *mita* conscripts, Peru abolished the communal land tenure predominant in *mita* districts in 1821 but did not replace it with enforceable peasant titling (Jacobsen, 1993; Dancuart and Rodriguez, 1902, vol. 2, p. 136). This opened the door to tactics such as the *interdicto de adquirir*, a judicial procedure which allowed aspiring landowners to legally claim "abandoned" lands that in reality belonged to peasants. *Hacienda* expansion also occurred through violence, with cattle hustling, grazing estate cattle on peasant lands, looting, and physical abuse used as strategies to intimidate peasants into signing bills of sale (Avila, 1952, p. 22; Roca-Sanchez, 1935, p. 242-43). Numerous peasant rebellions engulfed *mita* districts during the 1910's and 1920's, and indiscriminate banditry and livestock rustling remained prevalent in some *mita* districts for decades (Jacobsen, 1993; Ramos Zambrano, 1984; Tamayo Herrera, 1982; Hazen, 1974, p. 170-78). In contrast, large landowners had been established since the early 17th century in non-*mita* districts, which remained relatively stable (Flores Galindo, 1987, p. 240).

In 1969, the Peruvian government enacted an agrarian reform bill mandating the complete dissolution of *haciendas*. As a result, the *hacienda* elite were deposed and lands formerly belonging to *haciendas* were divided into "Agricultural Societies of Social Interest" (SAIS) during the early 1970's (Flores Galindo, 1987). In SAIS, neighboring indigenous communities and the producers acted as collective owners. By the late 1970s, attempts to impose collective ownership through SAIS had failed, and many SAIS were divided and allocated to individuals (Mar and Mejia, 1980). The 1994 Agricultural Census documents that when considering districts within 50 km of the *mita* boundary, 20% of household heads outside the *mita* catchment received their land in the 1970s through the agrarian reform, versus only 9% inside the *mita* catchment. Column (5), using data from the 1994 Agricultural Census, documents somewhat lower land inequality in non-*mita* districts. This finding is consistent with those in columns (1) through (4), given that non-*mita* districts had more large properties that could be distributed to smallholders during the agrarian reform.[29]

1.4.2 Public Goods

Table 7 examines the *mita*'s impact on education in 1876, 1940, and 2001, providing two sets of interesting results.[30] First, there is some evidence that the *mita* lowered

[29]The 1994 Agricultural Census also documents that a similar percentage of households across the *mita* boundary held formal titles to their land.

[30]Education, roads, and irrigation are the three public goods traditionally provided in Peru (Portocarrero and Zimmerman, 1988). Irrigation has been almost exclusively concentrated along the coast.

access to education historically, though point estimates are imprecisely estimated by the longitude-latitude RD polynomial. In column (1), the dependent variable is the district's mean literacy rate, obtained from the 1876 Population Census. Individuals are defined as literate if they could read, write, or both. Panels B and C show a highly significant *mita* effect of around two percentage points, as compared to an average literacy rate of 3.6% in the region I examine. The estimated effect is smaller, at around one percentage point, and not statistically significant, when estimated using the more flexible longitude-latitude specification.[31] In column (2), the dependent variable is mean years of schooling by district, from the 1940 Population Census. The specifications reported in Panels A through C suggest a long-run negative *mita* effect of around 0.2 years, as compared to a mean schooling attainment of 0.47 years throughout the study region, which again is statistically significant in Panels B and C. While this provides support for a *mita* effect on education historically, the evidence for an effect today is weak. In column (3), the dependent variable is individual years of schooling, obtained from ENAHO 2001. The *mita* coefficient is negative in all panels, but is of substantial magnitude and marginally significant only in Panel A.[32] It is also statistically insignificant in most specifications in Table A3. This evidence is consistent with studies of the Peruvian educational sector, which emphasize near-universal access (Saavedra, 2002; Portocarrero, 1988).

What about roads, the other principal public good in Peru? I estimate the *mita*'s

[31]In some of the specifications in Appendix Table A3 that interact the RD polynomial with the *mita* dummy, the estimated *mita* effect is near 0. This discrepancy is explained by two *mita* districts with relatively high literacy located near the *mita* boundary, to which these specifications are sensitive. When these two observations are dropped, the magnitude of the effect is similar across specifications.

[32]Data from the 1981 Population Census likewise do not show a *mita* effect on years of schooling. Moreover, data collected by the Ministry of Education in 2005 reveal no systematic differences in primary or secondary school enrollment or completion rates. Examination of data from a 2006 census of schools likewise showed little evidence for a causal impact of the *mita* on school infrastructure or the student-to-teacher ratio.

impact using a GIS road map of Peru produced by the Ministry of Transportation (2006). The map classifies roads as paved, gravel, non-gravel, and *trocha carrozable*, which translates as "narrow path, often through wild vegetation ... that a vehicle can be driven on with great difficulty" (Real Academia Española, 2008). The total length (in m) of district roads is divided by the district surface area (in km^2) to obtain a road network density.

Column 1 of Table 8 suggests that the *mita* does not impact local road networks, which consist primarily of non-gravel and *trocha* roads. Care is required in interpreting this result, as the World Bank's Rural Roads program, operating since 1997, has worked to reduce disparities in local road networks in marginalized areas of Peru. In contrast, there are significant disparities in regional road networks, which connect population centers to each other. Column (2), Panel A estimates that a *mita* effect lowers the density of regional roads by a statistically significant -29.3 meters of roadway for every km^2 of district surface area (s.e. = 16.0). In Panels B and C, the coefficients are similar, at -32.6 and -35.8 respectively, and are significant at the 1% level. This large effect compares to an average road density in *mita* districts of 20. Column (3) breaks down the result by looking only at the two highest quality road types, paved and gravel, and a similar picture emerges.[33]

If substantial population and economic activity endogenously clustered along roads, the relative poverty of *mita* districts would not be that surprising. While many of Peru's roads were built or paved in the interlude between 1940 and 1990, aggregate population responses appear minimal. The correlation between 1940 district population density and the density of paved and gravel roads, measured in 2006, is 0.58; when looking at this correlation using 1993 population density, it remains at

[33] 18% of *mita* districts can be accessed by paved roads versus 40% of non-*mita* districts (INEI, 2004).

0.58.

In summary, while I find little evidence that a *mita* effect persists through access to schooling, there are pronounced disparities in road networks across the *mita* boundary. Consistent with this evidence, I hypothesize that the long-term presence of large landowners provided a stable land tenure system that encouraged public goods provision.[34] Because established landowners in non-*mita* districts controlled a large percentage of the productive factors and because their property rights were secure, it is probable that they received higher returns to investing in public goods than those inside the *mita* catchment. Moreover, historical evidence indicates that these landowners were better able to secure roads, through lobbying for government resources and organizing local labor, and these roads remain today (Stein, 1980, p. 59).[35]

1.4.3 Proximate Determinants of Household Consumption

This section examines the *mita*'s long-run effects on the proximate determinants of consumption. The limited available evidence does not suggest differences in investment, so I focus on the labor force and market participation.[36] Agriculture is an important economic activity, providing primary employment for around 70% of the population in the region examined. Thus, Table 9 begins by looking at the percentage of the district labor force whose primary occupation is agriculture, taken from

[34]The elasticity of equivalent consumption in 2001 with respect to *haciendas* per capita in 1689, in non-*mita* districts, is 0.036 (s.e. = 0.022).

[35]The first modern road building campaigns occurred in the 1920's and many of the region's roads were constructed in the 1950's (Stein, 1980, Capuñay, 1951, p. 197-199).

[36]Data from the 1994 Agricultural Census on utilization of fifteen types of capital goods and twelve types of infrastructure for agricultural production do not show differences across the *mita* boundary, nor is the length of fallowing different. I am not aware of data on private investment outside of agriculture.

the 1993 Population Census. The median point estimate on $mita_d$ is equal to 0.10 and marginally significant only in Panel C, providing some weak evidence for a *mita* effect on employment in agriculture. Further results (not shown) do not find an effect on male and female labor force participation and hours worked.

The dependent variable in column (2), from the 1994 Agricultural Census, is a dummy equal to one if the agricultural household sells at least part of its produce in market. The corpus of evidence suggests we can be confident that the *mita*'s effects persist in part through an economically meaningful impact on agricultural market participation, though the precise magnitude of this effect is difficult to convincingly establish given the properties of the data and the mechanics of RD. The cubic longitude-latitude regression estimates a long-run *mita* effect of -0.074 (s.e. = 0.036), which is significant at the 5% level and compares to a mean market participation rate in the study region of 0.17. The magnitude of this estimate differs substantially from estimates that use a cubic polynomial in distance to Potosí (Panel B, -0.208, s.e. = 0.030) and a cubic polynomial in distance to the *mita* boundary (Panel C, -0.225, s.e. = 0.032). It also contrasts to the estimate from ordinary least squares limiting the sample to districts bordering the boundary (-0.178, s.e.= 0.050).

The surface plots in Figure 3 shed some light on why the cubic longitude-latitude point estimate is smaller. They show predicted values in "longitude-latitude-market participation rate" space from regressing the market participation dummy on the *mita* dummy (upper left), the *mita* dummy and a linear polynomial in longitude-latitude (upper right), the *mita* dummy and a quadratic polynomial in longitude-latitude (lower left), or the *mita* dummy and a cubic polynomial in longitude-latitude (lower right).[37] The *mita* region is seen from the side, appearing as a "canyon" with

[37]I show three-dimensional surface plots, instead of shaded plots as in Figure 2, because the predicted values can be seen more clearly, and it is not necessary to plot the data points.

lower market participation values. In the surface plot with the cubic polynomial, which is analogous to the regression in Panel A, the function increases smoothly and steeply, by orders of magnitude, near the *mita* boundary. In contrast, the other plots model less of the steep variation near the boundary as smooth and thus estimate a larger discontinuity. The single-dimensional RDs likewise have fewer degrees of freedom to model the variation near the boundary as smooth. It is not obvious which specification produces the most accurate results, as a more flexible specification will not necessarily yield a more reliable estimate. For example, consider the stylized case of an equation that includes the *mita* dummy and a polynomial with as many terms as observations. This has a solution that perfectly fits the data with a discontinuity term of zero, regardless of how large the true *mita* effect is. On the other hand, flexibility is important if parsimonious specifications do not have enough degrees of freedom to accurately model smoothly changing unobservables. While there is not, for example, a large urban area at the peak of the cubic polynomial causing market participation to increase steeply in this region, it is difficult to conclusively argue that the variation is attributable to the discontinuity and not to unobservables, or vice versa.[38] The estimates in Tables 9 and A3 are most useful for determining a range of possible *mita* effects consistent with the data, and this range supports an economically meaningful *mita* effect on market participation.[39]

A *mita* effect on market participation is consistent with the findings on road networks, particularly given that recent studies on Andean Peru empirically connect poor road infrastructure to higher transaction costs, lower market participation,

[38]Note, however, that the relatively large (*mita*) urban area of Ayacucho, while outside the study region, is near the cluster of *mita* districts with high market participation in the upper left corner of the *mita* area.

[39]The specifications interacting the *mita* dummy with a linear or quadratic polynomial in distance to the *mita* boundary, reported in Table A3, do not estimate a significant *mita* effect. Graphical evidence suggests that these specifications are sensitive to outliers near the boundary.

and reduced household income (Escobal and Ponce, 2002; Escobal, 2001; Agreda and Escobal, 1998).[40] An alternative hypothesis is that agricultural producers in *mita* districts supplement their income by working as wage laborers rather than by producing for markets. In column (3), the dependent variable is an indicator equal to one if a member of the agricultural household participates in secondary employment outside the agricultural unit, also taken from the 1994 Agricultural Census. Estimates suggest that, if anything, the *mita* effect on participation in secondary employment is negative.

Could residents in *mita* districts have less desire to participate in the market economy, rather than being constrained by poor road infrastructure? While Shining Path, a Maoist guerilla movement, gained a strong foothold in the region during the 1980's, this hypothesis seems unlikely.[41] Shining Path's rise to power occurred against a backdrop of limited support for Maoist ideology, and the movement's attempts to reduce participation in markets were unpopular and unsuccessful where attempted

[40]In my sample, 33% of agricultural households in districts with paved road density above the median participate in markets, as compared to 13% in districts with paved road density below the median. Of course, there may also exist other channels through which a *mita* effect lowers market participation. Data from the 1994 Agricultural Census reveal that the median size of household landholdings is somewhat lower inside the *mita* catchment (at 1.2 hectares) than outside (at 1.4 hectares). If marketing agricultural produce involves fixed costs, a broader group of small farmers in non-*mita* districts may find it profitable.

[41]Many of the factors linked to the *mita* (poor infrastructure, limited access to markets, poorly defined property rights, and poverty) are heavily emphasized as the leading factors promoting Shining Path (CVR, 2003, vol. 1, p. 94, McClintock, 1998; Palmer, 1994). Thus, I tested whether there was a *mita* effect on Shining Path (results available upon request). To measure the intensity of Shining Path, I exploit a loophole in the Peruvian constitution that stipulates that when more than two thirds of votes cast are blank or null, authorities cannot be renewed (Pareja and Gatti, 1990). In an attempt to sabotage the 1989 municipal elections, Shining Path operatives encouraged citizens to cast blank or null (secret) ballots (McClintock, 1993, p. 79). I find that a *mita* effect increased blank/null votes by 10.7 percentage points (standard error = 0.031), suggesting greater support for and intimidation by Shining Path in *mita* districts. Moreover, estimates show that a *mita* effect increased the probability that authorities were not renewed by a highly significant 43.5 percentage points. I also look at blank/null votes in 2002, ten years after Shining Path's defeat, and there is no longer an effect.

(McClintock, 1998; Palmer 1994).

Recent qualitative evidence also underscores roads and market access. The citizens I spoke with, while visiting eight primarily *mita* and six primarily non-*mita* provinces, were acutely aware that some areas are more prosperous than others. When discussing the factors leading to the observed income differences, a common theme was that it is difficult to transport crops to markets. Thus, most residents in *mita* districts are engaged in subsistence farming. Agrarian scientist Edgar Gonzales Castro argues: "Some provinces have been favored, with the government - particularly during the large road building campaign in the early 1950's - choosing to construct roads in some provinces and completely ignore others" (Dec. 14th, 2006). At the forefront of the local government's mission in the (primarily *mita*) province of Espinar is "to advocate effectively for a system of modern roads to regional markets" (2008). Popular demands have also centered on roads and markets. In 2004, (the *mita* district) Ilave made international headlines when demonstrations involving over 10,000 protestors culminated with the lynching of Ilave's mayor, whom protestors accused of failing to deliver on promises to pave the town's access road and build a local market (Shifter, 2004).

1.5 Concluding Remarks

This paper documents and exploits plausible exogenous variation in the assignment of the *mita* to identify channels through which it influences contemporary economic development. I estimate that its long-run effects lower household consumption by around 25% and increase stunting in children by around six percentage points. I then document land tenure, public goods, and market participation as channels through which its impacts persist.

In existing theories about land inequality and long-run growth, the implicit counterfactual to large landowners in Latin America is secure, enfranchised smallholders (Engerman and Sokoloff, 1997). This is not an appropriate counterfactual for Peru, or many other places in Latin America, because institutional structures largely in place before the formation of the landed elite did not provide secure property rights, protection from exploitation, or a host of other guarantees to potential smallholders. Large landowners - while they did not aim to promote economic prosperity for the masses - did shield individuals from exploitation by a highly extractive state and ensure public goods. This evidence suggests that exploring constraints on how the state can be used to shape economic interactions - for example, the extent to which elites can employ state machinery to coerce labor or citizens can use state guarantees to protect their property - is a more useful starting point than land inequality for modeling Latin America's long-run growth trajectory. The development of general models of institutional evolution and empirical investigation of how these constraints are influenced by forces promoting change are particularly central areas for future research.

Table 1: Summary Statistics

	Sample falls within:											
	<100 km of *mita* boundary			<75 km of *mita* boundary			<50 km of *mita* boundary			<25 km of *mita* boundary		
	Inside	Outside	SE	Inside	Outside	SE	Inside	Outside	SE	Inside	Outside	SE
GIS Measures												
Elevation	4042	4018	[188.77] (85.54)	4085	4103	[166.92] (82.75)	4117	4096	[169.45] (89.61)	4135	4060	[146.16] (115.15)
Slope	5.54	7.21	[0.88]* (0.49)***	5.75	7.02	[0.86] (0.52)**	5.87	6.95	[0.95] (0.58)*	5.77	7.21	[0.90] (0.79)*
Observations	177	95		144	86		104	73		48	52	
% Indigenous	63.59	58.84	[11.19] (9.76)	71.00	64.55	[8.04] (8.14)	71.01	64.54	[8.42] (8.43)	74.47	63.35	[10.87] (10.52)
Observations	1112	366		831	330		683	330		329	251	
Log 1572 tribute rate	1.57	1.60	[0.04] (0.03)	1.57	1.60	[0.04] (0.03)	1.58	1.61	[0.05] (0.04)	1.65	1.61	[0.02]* (0.03)
% 1572 tribute to:												
Spanish Nobility	59.80	63.82	[1.39]*** (1.36)***	59.98	63.69	[1.56]** (1.53)**	62.01	63.07	[1.12] (1.34)	61.01	63.17	[1.58] (2.21)
Spanish Priests	21.05	19.10	[0.90]** (0.94)**	21.90	19.45	[1.02]** (1.02)**	20.59	19.93	[0.76] (0.92)	21.45	19.98	[1.01] (1.33)
Spanish Justices	13.36	12.58	[0.53] (0.48)*	13.31	12.46	[0.65] (0.60)	12.81	12.48	[0.43] (0.55)	13.06	12.37	[0.56] (0.79)
Indigenous Mayors	5.67	4.40	[0.78] (0.85)	4.55	4.29	[0.26] (0.29)	4.42	4.47	[0.34] (0.33)	4.48	4.42	[0.29] (0.39)
Observations	63	41		47	37		35	30		18	24	

The unit of observation is twenty kilometer by twenty kilometer grid cells for the geospatial measures, the household for *% indigenous*, and the district for the 1572 tribute data. Conley standard errors for the difference in means between *mita* and non-*mita* observations are in brackets. Robust standard errors for the difference in means are in parentheses. For *% indigenous*, the robust standard errors are corrected for clustering at the district level. The geospatial measures are calculated using elevation data at 30 arc second (one kilometer) resolution (SRTM, 2000). The unit of measure for elevation is 1000 meters and for slope is degrees. A household is indigenous if its members primarily speak an indigenous language in the home (ENAHO, 2001). The tribute data are taken from Miranda, 1975 [1583]. In the first three columns, the sample includes only observations located less than 100 kilometers from the *mita* boundary, and this threshold is reduced to 75, 50, and finally 25 kilometers in the succeeding columns. Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 2: Living Standards

	Dependent variable is:						
	Log equiv. hh. consumption (2001)			*Stunted growth, children 6-9 (2005)*			
Sample Within:	<100 km of bound. (1)	<75 km of bound. (2)	<50 km of bound. (3)	<100 km of bound. (4)	<75 km of bound. (5)	<50 km of bound. (6)	border district (7)
A: Cubic Polynomial in Latitude and Longitude							
Mita	-0.284	-0.216	-0.331	0.070	0.084*	0.087*	0.114**
	(0.198)	(0.207)	(0.219)	(0.043)	(0.046)	(0.048)	(0.049)
R^2	0.060	0.060	0.069	0.051	0.020	0.017	0.050
B: Cubic Polynomial in Distance to Potosí							
Mita	-0.337***	-0.307***	-0.329***	0.080***	0.078***	0.078***	0.063*
	(0.087)	(0.101)	(0.096)	(0.021)	(0.022)	(0.024)	(0.032)
R^2	0.046	0.036	0.047	0.049	0.017	0.013	0.047
C: Cubic Polynomial in Distance to Mita Boundary							
Mita	-0.277***	-0.230**	-0.224**	0.073***	0.061***	0.064***	0.055*
	(0.078)	(0.089)	(0.092)	(0.023)	(0.022)	(0.023)	(0.030)
R^2	0.044	0.042	0.040	0.040	0.015	0.013	0.043
Geo. Controls	yes	yes	yes	yes	yes	yes	yes
Boundary F.E.s	yes	yes	yes	yes	yes	yes	yes
Clusters	71	60	52	289	239	185	63
Observations	1,478	1,161	1,013	158,848	115,761	100,446	37,421

The unit of observation is the household in columns (1) through (3) and the individual in columns (4) through (7). Robust standard errors, adjusted for clustering by district, are in parentheses. The dependent variable is log equivalent household consumption (ENAHO, 2001) in columns (1) through (3), and a dummy equal to 1 if the child has stunted growth and equal to 0 otherwise in columns (4) through (7) (Ministry of Education, 2005). Mita is an indicator equal to one if the household's district contributed to the *mita* and equal to zero otherwise (Saignes, 1984; Amat y Juniet, 1947, p. 249, 284). Panel A includes a cubic polynomial in the latitude and longitude of the observation's district capital, Panel B includes a cubic polynomial in Euclidean distance from the observation's district capital to Potosí, and Panel C includes a cubic polynomial in Euclidean distance to the nearest point on the *mita* boundary. All regressions include controls for elevation and slope, as well as boundary segment fixed effects. Columns (1) through (3) include demographic controls for the number of infants, children, and adults in the household. In the first and fourth columns, the sample includes observations whose district capitals are located within 100 kilometers of the *mita* boundary, and this threshold is reduced to 75 and 50 kilometers in the succeeding columns. Column (7) includes only observations whose districts border the *mita* boundary. 78% of the observations are in *mita* districts in column (1), 71% in column (2), 68% in column (3), 78% in column (4), 71% in column (5), 68% in column (6), and 58% in column (7). Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 3: Specification Tests

	Dependent variable is:						
	Log equiv. household consump. (2001)			*Stunted growth, children 6-9 (2005)*			
Sample Within:	<100 km of bound. (1)	<75 km of bound. (2)	<50 km of bound. (3)	<100 km of bound. (4)	<75 km of bound. (5)	<50 km of bound. (6)	border district (7)
Alternative functional forms for RD polynomial: Baseline I							
Linear polynomial in latitude and longitude							
Mita	-0.294***	-0.199	-0.143	0.064***	0.054**	0.062**	0.068**
	(0.092)	(0.126)	(0.128)	(0.021)	(0.022)	(0.026)	(0.031)
Quadratic polynomial in latitude and longitude							
Mita	-0.151	-0.247	-0.361	0.073*	0.091**	0.106**	0.087**
	(0.189)	(0.209)	(0.216)	(0.040)	(0.043)	(0.047)	(0.041)
Quartic polynomial in latitude and longitude							
Mita	-0.392*	-0.324	-0.342	0.073	0.072	0.057	0.104**
	(0.225)	(0.231)	(0.260)	(0.056)	(0.050)	(0.048)	(0.042)
Alternative functional forms for RD polynomial: Baseline II							
Linear polynomial in distance to Potosí							
Mita	-0.297***	-0.273***	-0.220**	0.050**	0.048**	0.049**	0.071**
	(0.079)	(0.093)	(0.092)	(0.022)	(0.022)	(0.024)	(0.031)
Quadratic polynomial in distance to Potosí							
Mita	-0.345***	-0.262***	-0.309***	0.072***	0.064***	0.072***	0.060*
	(0.086)	(0.095)	(0.100)	(0.023)	(0.022)	(0.023)	(0.032)
Quartic polynomial in distance to Potosí							
Mita	-0.331***	-0.310***	-0.330***	0.078***	0.075***	0.071***	0.053*
	(0.086)	(0.100)	(0.097)	(0.021)	(0.020)	(0.021)	(0.031)
Interacted linear polynomial in distance to Potosí							
Mita	-0.307***	-0.280***	-0.227**	0.051**	0.048**	0.043*	0.076***
	(0.092)	(0.094)	(0.095)	(0.022)	(0.021)	(0.022)	(0.029)
Interacted quadratic polynomial in distance to Potosí							
Mita	-0.264***	-0.177*	-0.285**	0.033	0.027	0.039*	0.036
	(0.087)	(0.096)	(0.111)	(0.024)	(0.023)	(0.023)	(0.024)
Alternative functional forms for RD polynomial: Baseline III							
Linear polynomial in distance to mita boundary							
Mita	-0.299***	-0.227**	-0.223**	0.072***	0.060***	0.058**	0.056*
	(0.082)	(0.089)	(0.091)	(0.024)	(0.022)	(0.023)	(0.032)
Quadratic polynomial in distance to mita boundary							
Mita	-0.277***	-0.227**	-0.224**	0.072***	0.060***	0.061***	0.056*
	(0.078)	(0.089)	(0.092)	(0.023)	(0.022)	(0.023)	(0.030)
Quartic polynomial in distance to mita boundary							
Mita	-0.251***	-0.229**	-0.246***	0.073***	0.064***	0.063***	0.055*
	(0.078)	(0.089)	(0.088)	(0.023)	(0.022)	(0.023)	(0.030)
Interacted linear polynomial in distance to mita boundary							
Mita	-0.301*	-0.277	-0.385*	0.082	0.087	0.095	0.132**
	(0.174)	(0.190)	(0.210)	(0.054)	(0.055)	(0.065)	(0.053)
Interacted quadratic polynomial in distance to mita boundary							
Mita	-0.351	-0.505	-0.295	0.140*	0.132	0.136	0.121*
	(0.260)	(0.319)	(0.366)	(0.082)	(0.084)	(0.086)	(0.064)
Ordinary Least Squares							
Mita	-0.294***	-0.288***	-0.227**	0.057**	0.048*	0.049*	0.055*
	(0.083)	(0.089)	(0.090)	(0.025)	(0.024)	(0.026)	(0.031)
Geo. Controls	yes	yes	yes	yes	yes	yes	yes
Boundary F.E.s	yes	yes	yes	yes	yes	yes	yes
Clusters	71	60	52	289	239	185	63
Observations	1,478	1,161	1,013	158,848	115,761	100,446	37,421

Robust standard errors, adjusted for clustering by district, are in parentheses. All regressions include geographic controls and boundary segment fixed effects. Columns (1) through (3) include demographic controls for the number of infants, children, and adults in the household. Coefficients significantly different from zero are denoted by: *10%, **5%, and ***1%.

Table 4: Additional Specification Tests

	Log equivalent household consumption (2001)							Stunted growth, children 6-9 (2005)				
	Baseline	Control for ethnicity	Includes Cusco	Excludes districts with Inca estates	Excludes portions of boundary formed by rivers	Flexible estimation of consump. equivalence	Migration	Baseline	Includes Cusco	Excludes districts with Inca estates	Excludes portions of boundary formed by rivers	Migration
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A: Cubic Polynomial in Latitude and Longitude												
Mita	-0.331	-0.202	-0.465**	-0.281	-0.322	-0.326	-0.223	0.087*	0.147***	0.093*	0.090*	0.069
	(0.219)	(0.157)	(0.207)	(0.265)	(0.215)	(0.230)	(0.198)	(0.048)	(0.048)	(0.048)	(0.048)	(0.049)
R^2	0.069	0.154	0.104	0.065	0.070	0.292	0.067	0.017	0.046	0.019	0.018	0.016
B: Cubic Polynomial in Distance to Potosí												
Mita	-0.329***	-0.282***	-0.450***	-0.354***	-0.376***	-0.328***	-0.263***	0.078***	0.146***	0.077***	0.081***	0.060**
	(0.096)	(0.073)	(0.096)	(0.101)	(0.114)	(0.099)	(0.095)	(0.024)	(0.030)	(0.026)	(0.024)	(0.025)
R^2	0.047	0.140	0.087	0.036	0.049	0.275	0.042	0.013	0.039	0.014	0.013	0.012
C: Cubic Polynomial in Distance to Mita Boundary												
Mita	-0.224**	-0.195***	-0.333***	-0.255**	-0.217**	-0.224**	-0.161*	0.064***	0.132***	0.066***	0.065***	0.046*
	(0.092)	(0.070)	(0.087)	(0.110)	(0.098)	(0.095)	(0.088)	(0.023)	(0.027)	(0.025)	(0.023)	(0.024)
R^2	0.040	0.135	0.088	0.047	0.039	0.270	0.037	0.013	0.042	0.014	0.013	0.012
Geo. Controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Bound. F.E.s	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
Clusters	52	52	57	47	51	52	52	185	195	180	183	185
Obs.	1013	1013	1173	930	992	1013	997	100,446	127,259	96,440	99,940	98,922

Robust standard errors, adjusted for clustering by district, are in parentheses. All regressions include soil type indicators and boundary segment fixed effects. Columns (1) through (5) and (7) include demographic controls for the number of infants, children, and adults in the household. Column (6) includes controls for the log of household size and the ratio of children to household members, using the log of household consumption as the dependent variable. The samples include observations whose district capitals are less than 50 kilometers from the *mita* boundary. Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 5: 1572 Tribute and Population

	Dependent variable is:							
	Log Mean Tribute	Share of Tribute Revenues to Spanish Nobility	Spanish Priests	Spanish Justices	Indig. Mayors	Men	Percent Boys	Females
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Cubic Polynomial in Latitude and Longitude								
Mita	0.020	-0.010	0.004	0.004	0.003	-0.006	0.011	-0.009
	(0.031)	(0.030)	(0.019)	(0.010)	(0.005)	(0.009)	(0.012)	(0.016)
R^2	0.762	0.109	0.090	0.228	0.266	0.596	0.377	0.599
B: Cubic Polynomial in Distance to Potosí								
Mita	0.019	-0.013	0.008	0.006	-0.001	-0.012	0.005	-0.011
	(0.029)	(0.025)	(0.015)	(0.009)	(0.004)	(0.008)	(0.010)	(0.012)
R^2	0.597	0.058	0.073	0.151	0.132	0.315	0.139	0.401
C: Cubic Polynomial in Distance to Mita Boundary								
Mita	0.040	-0.009	0.005	0.003	-0.001	-0.011	0.001	-0.008
	(0.030)	(0.018)	(0.012)	(0.006)	(0.004)	(0.007)	(0.008)	(0.010)
R^2	0.406	0.062	0.096	0.118	0.162	0.267	0.190	0.361
Geo. Controls	yes	yes	yes	yes	yes	yes	yes	yes
Boundary F.E.s	yes	yes	yes	yes	yes	yes	yes	yes
Mean Dep. Var.	1.591	0.625	0.203	0.127	0.044	0.193	0.204	0.544
Observations	65	65	65	65	65	65	65	65

The dependent variable in column (1) is the log of the district's mean 1572 tribute rate (Miranda, 1975 [1583]). In columns (2) through (5), it is the share of tribute revenue allocated to Spanish nobility (*encomenderos*), Spanish priests, Spanish justices, and indigenous mayors (*caciques*), respectively. In Columns (6) through (8), it is the share of 1572 district population composed of males (aged 18 to 50), boys, and females (of all ages), respectively. Panel A includes a cubic polynomial in longitude and latitude, Panel B includes a cubic polynomial in Euclidean distance from the observation's district capital to Potosí, and Panel C includes a cubic polynomial in Euclidean distance to the nearest point on the *mita* boundary. All regressions geographic controls and boundary segment fixed effects. The samples include districts whose capitals are less than 50 kilometers from the *mita* boundary. Column (1) weights by the square root of the district's tributary population and columns (6) through (8) weight by the square root of the district's total population. 66% of the observations are from *mita* districts. Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 6: Land Tenure and Labor Systems

	Dependent variable is:				
	Haciendas per district in 1689	*Haciendas* per 1000 district residents in 1689	Percent of rural tributary population in *haciendas* in c. 1845	Percent of rural population in *haciendas* in 1940	Land gini in 1994
	(1)	(2)	(3)	(4)	(5)
A: Cubic Polynomial in Latitude and Longitude					
Mita	-12.683***	-6.453**	-0.127*	-0.066	0.078
	(3.221)	(2.490)	(0.067)	(0.086)	(0.053)
R^2	0.538	0.582	0.410	0.421	0.245
B: Cubic Polynomial in Distance to Potosí					
Mita	-10.316***	-7.570***	-0.204**	-0.143***	0.107***
	(2.057)	(1.478)	(0.082)	(0.051)	(0.036)
R^2	0.494	0.514	0.308	0.346	0.194
C: Cubic Polynomial in Distance to Mita Boundary					
Mita	-11.336***	-8.516***	-0.212***	-0.120***	0.124***
	(2.074)	(1.665)	(0.060)	(0.045)	(0.033)
R^2	0.494	0.497	0.316	0.336	0.226
Geo. Controls	yes	yes	yes	yes	yes
Boundary F.E.s	yes	yes	yes	yes	yes
Mean Dep. Var.	6.500	5.336	0.135	0.263	0.783
Observations	74	74	81	119	181

The unit of observation is the district. Robust standard errors are in parentheses. The dependent variable in column (1) is *haciendas* per district in 1689 and in column (2) is *haciendas* per 1000 district residents in 1689 (Villanueva Urteaga, 1982). In column (3) it is the percentage of the district's tributary population residing in *haciendas* c. 1845 (Peralta Ruiz, 1991), in column (4) it is the percentage of the district's rural population residing in *haciendas* in 1940 (*Censo Nacional de Población y Ocupación*, 1944), and in column (5) it is the district land gini (*Tercer Censo Nacional Agropecuario*, 1994). Panel A includes a cubic polynomial in the latitude and longitude of the observation's district capital, Panel B includes a cubic polynomial in Euclidean distance from the observation's district capital to Potosí, and Panel C includes a cubic polynomial in Euclidean distance to the nearest point on the *mita* boundary. All regressions include geographic controls and boundary segment fixed effects. The samples include districts whose capitals are less than 50 kilometers from the *mita* boundary. Column (3) is weighted by the square root of the district's rural tributary population and column (4) is weighted by the square root of the district's rural population. 58% of the observations are in *mita* districts in columns (1) and (2), 59% in column (3), 62% in column (4), and 66% in column (5). Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 7: Education

	Dependent variable is:		
	Literacy 1876	Mean years of schooling 1940	Mean years of schooling 2001
	(1)	(2)	(3)
A: Cubic Polynomial in Latitude and Longitude			
Mita	-0.015	-0.265	-1.479*
	(0.012)	(0.177)	(0.872)
R^2	0.401	0.280	0.020
B: Cubic Polynomial in Distance to Potosí			
Mita	-0.020***	-0.181**	-0.341
	(0.007)	(0.078)	(0.451)
R^2	0.345	0.187	0.007
C. Cubic Polynomial in Dist. to Mita Bound.			
Mita	-0.022***	-0.209***	-0.111
	(0.006)	(0.076)	(0.429)
R^2	0.301	0.234	0.004
Geo. Controls	yes	yes	yes
Boundary F.E.s	yes	yes	yes
Mean Dep. Var.	0.036	0.470	4.457
Clusters	95	118	52
Observations	95	118	4,038

The unit of observation is the district in columns (1) and (2) and the individual in column (3). Robust standard errors, adjusted for clustering by district, are in parentheses. The dependent variable is mean literacy in 1876 in column (1) (*Censo general de la republica del Perú, 1878*), mean years of schooling in 1940 in column (2) (*Censo Nacional de Población y Ocupación, 1944*), and individual years of schooling in 2001 in column (3) (ENAHO, 2001).Panel A includes a cubic polynomial in the latitude and longitude of the observation's district capital, Panel B includes a cubic polynomial in Euclidean distance from the observation's district capital to Potosí, and Panel C includes a cubic polynomial in Euclidean distance to the nearest point on the *mita* boundary. All regressions include geographic controls and boundary segment fixed effects. The samples include districts whose capitals are less than 50 kilometers from the *mita* boundary. Columns (1) and (2) are weighted by the square root of the district's population. 64% of the observations are in *mita* districts in column (1), 63% in column (2), and 67% in column (3). Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 8: Roads

	Dependent variable is:		
	Density of local road networks	Density of regional road networks	Density of paved/gravel regional roads
	(1)	(2)	(3)
A: Cubic Polynomial in Latitude and Longitude			
Mita	0.464	-29.276*	-22.426*
	(18.575)	(16.038)	(12.178)
R^2	0.232	0.293	0.271
B: Cubic Polynomial in Distance to Potosí			
Mita	-1.522	-32.644***	-30.698***
	(12.101)	(8.988)	(8.155)
R^2	0.217	0.271	0.256
C. Cubic Polynomial in Dist. to Mita Bound.			
Mita	0.535	-35.831***	-32.458***
	(12.227)	(9.386)	(8.638)
R^2	0.213	0.226	0.208
Geo. Controls	yes	yes	yes
Boundary F.E.s	yes	yes	yes
Mean Dep. Var.	85.34	33.55	22.51
Observations	185	185	185

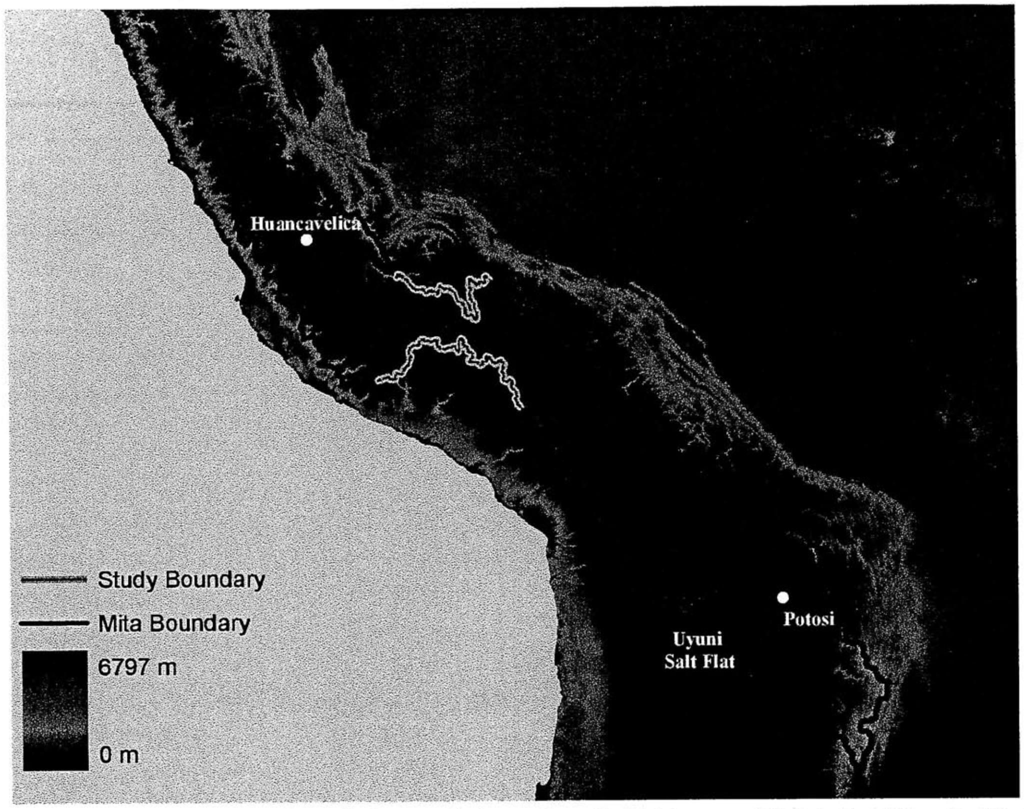
The unit of observation is the district. Robust standard errors are in parentheses. The road densities are defined as total length in meters of the respective road type in each district divided by the district's surface area, in km^2. They are calculated using a GIS map of Peru's road networks (*Ministro de Transporte*, 2006). Panel A includes a cubic polynomial in the latitude and longitude of the observation's district capital, Panel B includes a cubic polynomial in Euclidean distance from the observation's district capital to Potosí, and Panel C includes a cubic polynomial in Euclidean distance to the nearest point on the *mita* boundary. All regressions include geographic controls and boundary segment fixed effects. The samples include districts whose capitals are less than 50 kilometers from the *mita* boundary. 66% of the observations are in *mita* districts. Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Table 9: Consumption Channels

	Dependent variable is:		
	Percent of district labor force in agriculture - 1993	Agricultural household sells part of produce in markets - 1994	Household member employed outside the agricultural unit - 1994
	(1)	(2)	(3)
A: Cubic Polynomial in Latitude and Longitude			
Mita	0.211	-0.074**	-0.013
	(0.140)	(0.036)	(0.032)
R^2	0.177	0.176	0.010
B: Cubic Polynomial in Distance to Potosí			
Mita	0.101	-0.208***	-0.033
	(0.061)	(0.030)	(0.020)
R^2	0.112	0.144	0.008
C: Cubic Polynomial in Distance to Mita Boundary			
Mita	0.092*	-0.225***	-0.038**
	(0.054)	(0.032)	(0.018)
R^2	0.213	0.136	0.006
Geo. Controls	yes	yes	yes
Boundary F.E.s	yes	yes	yes
Mean Dep. Var.	0.697	0.173	0.245
Clusters	179	178	182
Observations	179	160,990	183,596

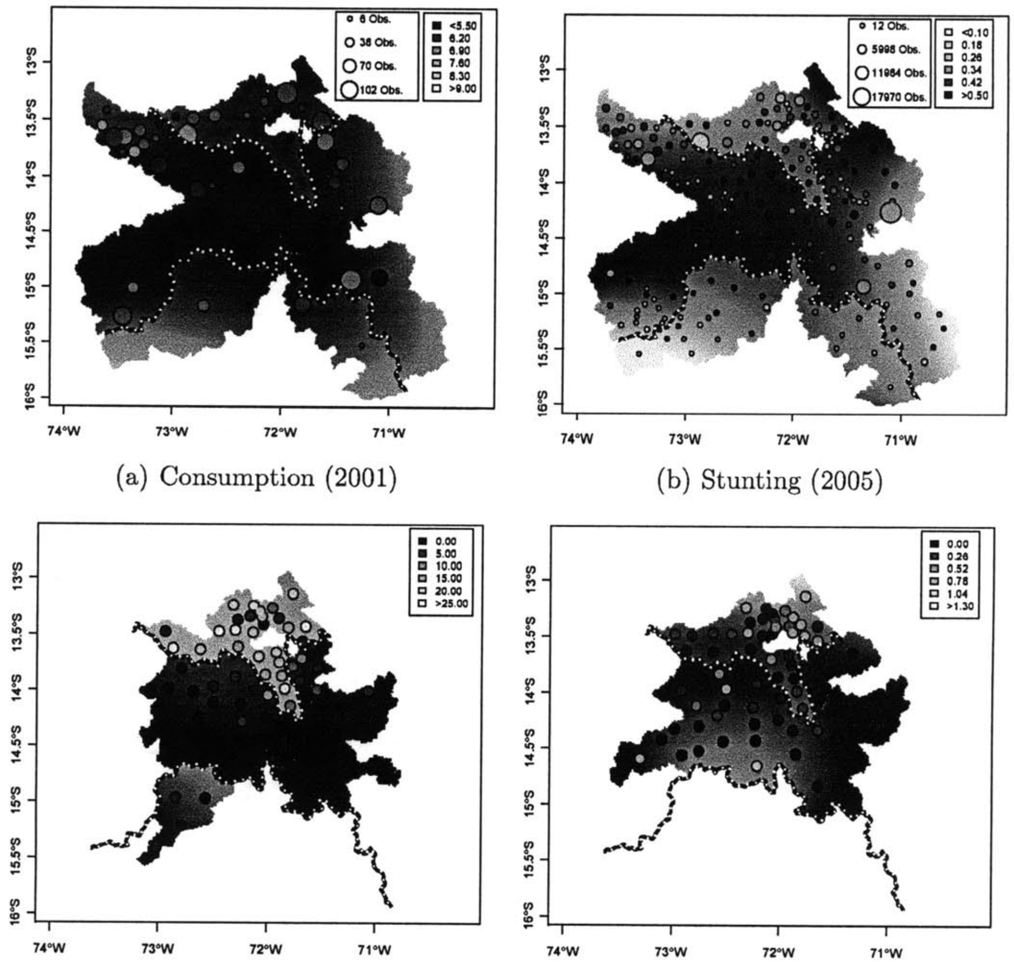
Robust standard errors, adjusted for clustering by district in columns (2) and (3), are in parentheses. The dependent variable in column (1) is the percentage of the district's labor force engaged in agriculture as a primary occupation (*IX Censo de Población*, 1993), in column (2) it is an indicator equal to one if the agricultural unit sells at least part of its produce in markets, and in column (3) it is an indicator equal to one if at least one member of the household pursues secondary employment outside the agricultural unit (*Tercer Censo Nacional Agropecuario*, 1994). Panel A includes a cubic polynomial in the latitude and longitude of the observation's district capital, Panel B includes a cubic polynomial in Euclidean distance from the observation's district capital to Potosí, and Panel C includes a cubic polynomial in Euclidean distance to the nearest point on the *mita* boundary. All regressions include geographic controls and boundary segment fixed effects. Column (1) is weighted by the square root of the district's population. 66% of the observations in column (1) are in *mita* districts, 68% in column (2), and 69% in column (3). Coefficients that are significantly different from zero are denoted by the following system: *10%, **5%, and ***1%.

Figure 1



The *mita* boundary is in black and the study boundary in light gray. Districts falling inside the contiguous area formed by the *mita* boundary contributed to the *mita*. Elevation is shown in the background.

Figure 2



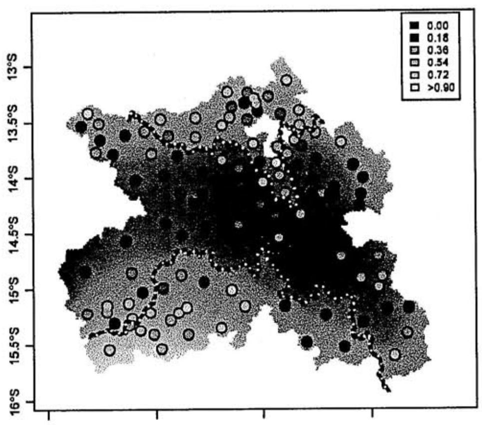
(a) Consumption (2001)

(b) Stunting (2005)

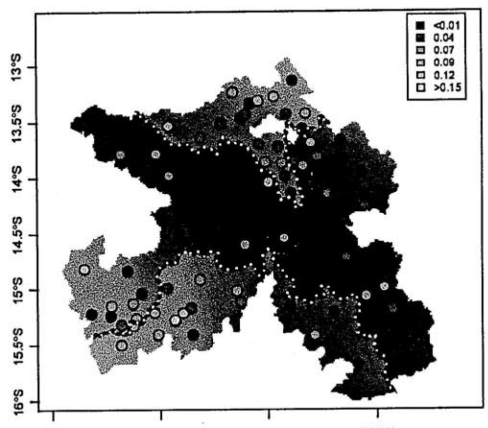
(c) *Haciendas* (1689)

(d) *Haciendas* (1845)

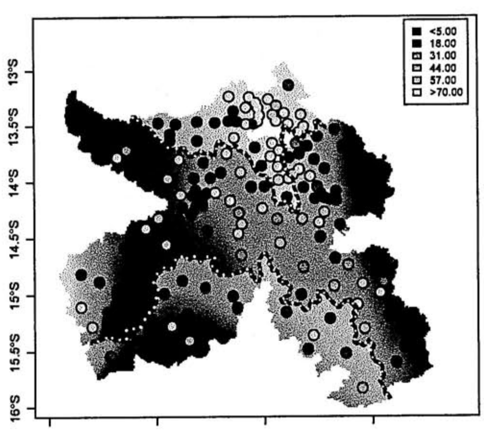
Figure 2 (cont.)



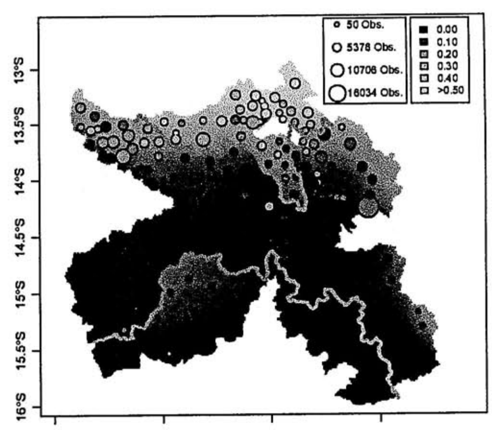
(e) *Haciendas* (1940)



(f) Education (1876)



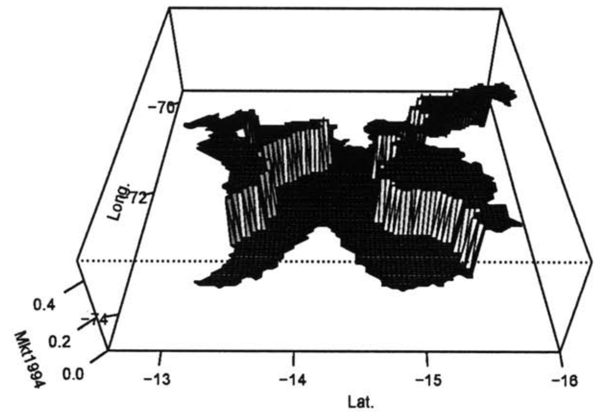
(g) Road Density (2006)



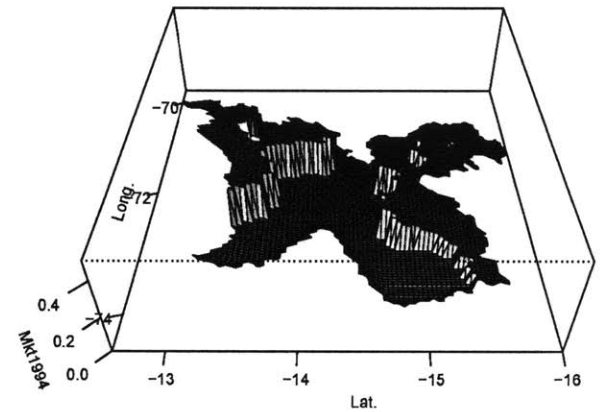
(h) Ag. Market Participation (1994)

Notes: The figures plot various outcomes against longitude and latitude. See the text for a detailed description.

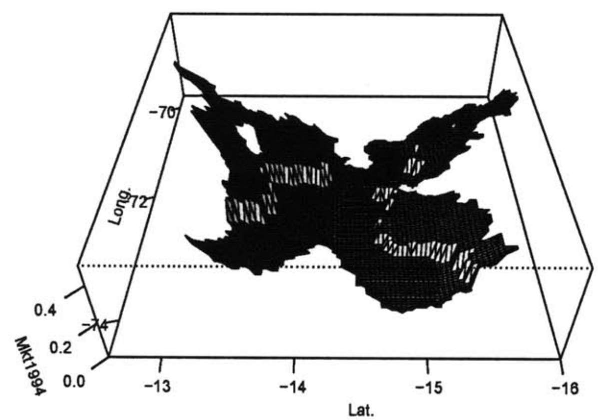
Figure 3



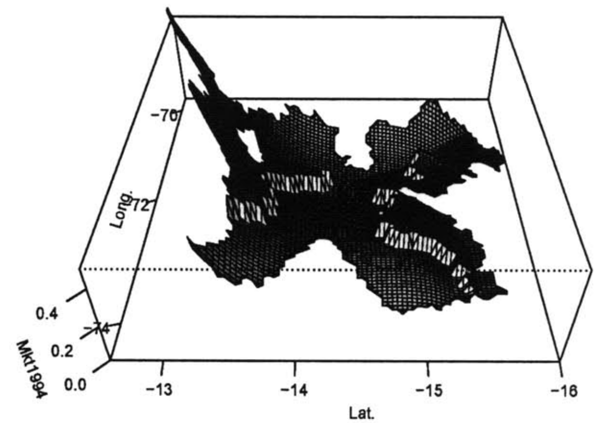
(i) No RD Polynomial



(j) Linear Polynomial in Lon-Lat



(k) Quadratic Polynomial in Lon-Lat



(l) Cubic Polynomial in Lon-Lat

Notes: The figures plot predicted values from regressing a market participation dummy on the *mita* dummy and various degrees of polynomials in longitude and latitude. See the text for a detailed description.

Chapter 2

Trafficking Networks and the Mexican Drug War

2.1 Introduction

Drug trade-related violence has escalated dramatically in Mexico during the past five years, claiming over 40,000 lives and raising concerns about the capacity of the Mexican 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). While drug traffickers are economic actors with clear profit maximization motives, there is little empirical evidence on how traffickers' economic objectives have influenced the effects of Mexican policy towards the drug trade. More generally, it remains controversial whether state policies have caused the marked increase in violence, or whether violence would have risen substantially in any case (Guerrero, 2011; Rios, 2011a; Shirk, 2011). This study uses variation from close mayoral elections and a network model of drug trafficking to examine the direct and spillover effects of crackdowns on drug trafficking.

Mexico is the largest supplier to the U.S. illicit drug market (U.N. World Drug Report, 2011). While Mexican drug traffickers engage in a wide variety of illicit activities - including domestic drug sales, protection rackets, kidnapping, human smuggling, prostitution, oil and fuel theft, money laundering, weapons trafficking, and auto theft - the largest share of their revenues derives from trafficking drugs from Mexico to the U.S. (Guerrero, 2011, p. 10). Official data described later in this paper document that in 2008, drug trafficking organizations maintained operations in two thirds of Mexico's municipalities and illicit drugs were cultivated in 14% of municipalities.

This study begins by specifying a network model of drug trafficking in which traffickers' objective is to minimize the costs incurred in trafficking drugs from producing municipalities in Mexico across the Mexican road network to the United States. This model is used as an empirical tool for analyzing the direct and spillover effects of local policy towards the drug trade. In the simplest version of the model, the cost of traversing each edge in the road network is proportional to the physical length of the edge, and hence traffickers take the shortest route to the nearest U.S. point of entry. After examining the relationships in the data using this shortest paths model, the study specifies and estimates a richer version of the model that imposes congestion costs when trafficking routes coincide.

A challenge of identifying the effects of crackdowns on violence is that the state does not randomly decide to combat drug trafficking in some places but not others, and they may choose to crack down in municipalities where violence is expected to increase. In order to isolate plausibly exogenous variation in policy towards the drug trade, I exploit the outcomes of close mayoral elections involving the PAN party.[1]

[1]See Lee, Moretti, and Butler (2004) for a pioneering example of a regression discontinuity design

The PAN'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 by a wide margin are likely to be different from municipalities where they lose, when we focus on close elections it becomes plausible that the outcomes are driven by idiosyncratic factors that do not themselves affect violence. In fact, the outcomes of close elections involving the PAN are uncorrelated with a large number of municipal characteristics measured prior to the elections.

The network model, variation from close mayoral elections, and data on drug trade-related outcomes between 2007 and 2009 are used to examine three sets of questions. First, the study asks whether the outcomes of close mayoral elections involving the PAN affect drug trade-related violence and explores the economic mechanisms that mediate this relationship. Second, the study examines whether drug trafficking routes are diverted to other municipalities following close PAN victories and tests whether the diversion of drug traffic is accompanied by violence and economic spillover effects. Finally, the study uses the trafficking model to examine the allocation of law enforcement resources.

Regression discontinuity (RD) estimates exploiting the outcomes of close elections show that the probability that a drug trade-related homicide occurs in a municipality in a given month is 8.4 percentage points higher after a PAN mayor takes office than after a non-PAN mayor takes office. This is a large effect, given that six percent of municipality-months in the sample experienced a drug trade-related homicide. The

exploiting close elections. A number of studies have used discontinuous changes in policies, in the cross-section or over time, to examine illicit behavior. These studies examine topics ranging from earnings manipulation (Bollen and Pool, 2009; Bhojraj et al., 2009) to the production of defective products (Krueger and Mas, 2004) to fixing the outcomes of sporting events (Wolfers, 2006; Duggan and Levitt, 2002) to sex-selective abortions in Taiwan (Lin et al., 2008). See Zitzewitz (2011) for a detailed review.

violence response to close PAN victories consists primarily of individuals involved in the drug trade killing each other. Analysis using information on the industrial organization of trafficking suggests that it reflects rival traffickers' attempts to wrest control of territories after crackdowns initiated by PAN mayors have challenged the incumbent criminals.

These results support qualitative and descriptive studies, such as the well-known work by Eduardo Guerrero (2011), which argue that Mexican government policy has been the primary cause of the large increase in violence in recent years. In municipalities with a close PAN loss, violence declines slightly in the six months following the inauguration of new authorities as compared to the six months prior to the election. In municipalities with a narrowly elected PAN mayor, violence - previously at the same average level as in municipalities where the PAN barely lost - increases sharply. The results also relate to work by Josh Angrist and Adriana Kugler (2008) documenting that exogenous increases in coca prices increase violence in rural districts in Colombia because combatant groups fight over the additional rents. In Mexico, crackdowns likely reduce rents from criminal activities while in effect, but by weakening the incumbent criminal group they also reduce the costs of taking control of a municipality. Controlling the municipality is likely to offer substantial rents from trafficking and a variety of other criminal activities once the crackdown subsides.

The paper's second set of results examines whether close PAN victories exert spillover effects. When policy leads one location to become less conducive to illicit activities, organized crime may relocate elsewhere. For example, coca eradication policies in Bolivia and Peru during the late-1990s led cultivation to shift to Colombia, and large-scale coca eradication in Colombia in the early 2000s has since led cultivation to re-expand in Peru and Bolivia, with South American coca cultivation remaining unchanged between 1999 and 2009 (Isacson, 2010; Leech, 2000; UN Office on Drugs and Crime 1999-2009). On a local level, work by Rafael Di Tella and Ernesto Schargrodsky (2004) documents that the allocation of police officers to Jewish institutions in Buenos Aires substantially reduced auto theft in the immediate vicinity of these institutions but may also have diverted some auto theft to as close as two blocks away. While a number of studies have examined the economics of the drug trade and organized crime more generally, to the best of my knowledge this study is the first to empirically estimate spillover patterns in drug trafficking activity.[2]

I begin by showing that the simple model in which traffickers take the shortest route to the nearest U.S. point of entry robustly predicts the diversion of drug traffic following close PAN victories. Specifically, I assume that it becomes more costly to traffic drugs through a municipality after a close PAN victory. Because municipal elections happen at different times throughout the sample period, they generate month-to-month within-municipality variation in predicted trafficking routes. This variation is driven by plausibly exogenous changes in politics elsewhere in Mexico and can be compared to variation in monthly panel data on actual illicit drug confiscations and other outcomes. This approach is illustrated in Figure 2-1. When the shortest paths trafficking model is used, the presence of a predicted drug trafficking route increases the value of illicit drug confiscations in a given municipality-month by around 18.5 percent. Because traffickers may care about the routes that other traffickers take, I also estimate a richer model that imposes congestion costs when routes coincide. Routes predicted by this model for the beginning of the sample period are

[2]Prominent examples of studies of the economics of organized crime include Steve Levitt and Sudhir Venkatesh's analysis of the finances of a U.S. drug gang (2000), sociologist Diego Gambetta's economic analysis of the Sicilian mafia (1996), and Frederico Varese's analysis of the rise of the Russian mafia (2005).

shown in Figure 2-2. The richer model is similarly predictive of within-municipality changes in confiscations, with the presence of a predicted trafficking route increasing the value of illicit drug confiscations by around 19.5 percent. Robustness and placebo checks support the validity of the approach.

When a municipality acquires a predicted trafficking route, the probability that a drug trade-related homicide occurs in a given month increases by around 1.4 percentage points, relative to a baseline probability of 4.4 percent. This relationship is similar regardless of whether the shortest paths model or model with congestion is used. When routes are predicted using the model with congestion, the violence spillovers are concentrated in municipalities where two or more routes coincide.

Moreover, when a municipality acquires a predicted route, wages earned by adult men in the informal sector fall by around 2.5 percent and female labor force participation declines by around one percentage point, relative to a baseline participation rate of 51 percent. Formal sector wages and male labor force participation are not affected. The economic spillovers are noisily estimated and thus should be interpreted with caution. Nevertheless, they are consistent with qualitative evidence discussed in Section 2 that drug trafficking organizations extort informal sector producers via protection rackets.

While high rates of violence and the flexibility of trafficking operations pose major challenges to state efforts to combat the drug trade, this study's results indicate that crackdowns do increase trafficking costs. Decreasing the profits of traffickers is a central goal of Mexican drug policy because it reduces the resources that these organizations have to corrupt Mexican institutions and threaten public security (Shirk, 2011). Thus, the study's third set of results uses the trafficking model to examine how scarce law enforcement resources can be allocated so as to increase trafficking costs by as much as possible. I also discuss how the violent response to this policy may be reduced. While we would not expect there to be any easy or ideal solutions to the challenges facing Mexico, the network approach to trafficking provides unique information with the potential to contribute to a more nuanced and economically informed law enforcement policy.

The next section provides an overview of Mexican drug trafficking and state policies towards the drug trade, and Section 3 develops the network model of drug trafficking. Section 4 tests whether the outcomes of close elections involving the PAN influence drug trade-related violence and examines economic mechanisms underlying this relationship. Section 5 documents that close PAN mayoral victories divert drug traffic elsewhere. It also estimates the version of the trafficking model with congestion and tests whether PAN crackdowns exert violence and economic spillover effects. Section 6 utilizes the trafficking model to examine the allocation of law enforcement resources. Finally, Section 7 offers concluding remarks.

2.2 Background

Mexican drug traffickers dominate the wholesale illicit drug market in the United States. According to the U.N. World Drug Report, Mexico is the largest supplier of heroin to U.S. markets and the largest foreign supplier of marijuana and methamphetamine. Official Mexican government data on drug producing regions, obtained from confidential sources, document that fourteen percent of Mexico's municipalities regularly produce opium poppy seed (used to make heroin) or cannabis. Moreover, Mexico serves as a major transhipment hub for cocaine, with 60 to 90 percent of the cocaine consumed in the U.S. transiting through Mexico (U.S. Drug Enforcement Agency, 2011).

The U.S. State Department estimates that wholesale revenues of Mexican drug traffickers in U.S. markets range from \$13.6 billion to \$48.4 billion annually.[3] While the margin of error on this estimate is large, there is consensus that the U.S. market provides substantially more revenue than Mexico's domestic illicit drug market, which is worth an estimated \$560 million annually (Secretaría de Seguridad Pública, 2010). Data on drug addiction also emphasize the importance of the U.S. illicit drug market. According to the U.S. 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) (Guerrero, 2011, p. 82; National Addiction Survey, 2008).

At the beginning of this study's sample period, there were six major drug trafficking organizations operating in Mexico, as well as many local gangs. Official Mexican data obtained from confidential sources document that 68 percent of Mexico's 2,456 municipalities were known to have a major drug trafficking organization or local drug gang operating within their limits in early 2008. These data also estimate that 49 percent of Mexico's 320 drug producing municipalities were controlled by a major drug trafficking organization in 2008, whereas the remaining 51 percent were controlled by local gangs.

While the term 'cartel' is used colloquially to refer to Mexican drug trafficking organizations, these competing groups do not collude to reduce illicit drug production or to set the price of illicit drugs. As documented in detail by Eduardo Guerrero (2011, p. 106-108), alliances between drug trafficking organizations have been highly unstable during the past five years. Moreover, the number of major trafficking organizations in Mexico expanded from 6 in 2007 to 16 by mid-2011, with cells breaking

[3]Estimates by U.S. Immigration and Customs Enforcement, the U.S. Drug Enforcement Agency, and the Mexican Secretaría de Seguridad Pública are broadly similar and also contain a large margin of error.

away from the larger organizations over disputes about leadership and operational issues (Guerrero, 2011, p. 10). Within drug trafficking organizations, most decisions about day-to-day operations are decentralized. Decisionmaking by semi-independent local cells ensures that no single player will be able to reveal extensive information if he or she is captured by authorities.

While the U.S. illicit drug market provides the main source of revenues for Mexican drug trafficking organizations, they are diversified into a host of other illicit activities, including domestic drug sales, protection rackets, kidnapping, human smuggling, prostitution, oil and fuel theft, money laundering, weapons trafficking, and auto theft (Guerrero, 2011, p. 10). Drug trafficking organizations have substantially expanded their operations in some of these activities in recent years (Rios, 2011a). Most notably, protection rackets involving the general population have increased substantially, with complaints to authorities tripling between 2004 and 2009 (Secretariado Ejecutivo del Sistema Nacional de Seguridad Pública, 2011). In a recent nationwide survey, Díaz-Cayeros et al. (2011) found that drug traffickers are most likely to extort the poor, with 24% to 40% of surveyed households who participate in the poverty alleviation program *Oportunidades* reporting that they had been extorted by traffickers. Such activities affect the lives of many citizens who are unaffiliated with the drug trade, and as of 2011, public opinion surveys found that public security was more likely to be chosen as the largest problem facing the country than concerns about the economy.

For most of the 20th century, Mexican politics were dominated by a single party, the PRI (Institutionalized Revolutionary Party). Both local and federal authorities took a passive stance towards drug trafficking, and there were a number of well-documented instances in which officials engaged in drug trade-related corruption (see for example Shannon, 1988). While the Mexican federal government periodically cracked down on drug trafficking, these operations were limited in size and scope.[4] The PRI's dominance began to erode in the 1990s, and the first opposition president was elected from the National Action Party (PAN) in 2000. Today Mexico is a competitive multi-party democracy.

Government efforts to combat the drug trade have increased substantially in recent years. Soon after taking office in December 2006, PAN president Felipe Calderón deployed 6,500 federal troops to the state of Michoacan to combat drug trade-related violence. The government's operations against drug trafficking have continued to increase since this time, with approximately 45,000 troops involved by 2011. Military and federal police operations have been a centerpiece of Calderón's administration, and major judicial reforms were also legislated in 2008. However, the criminal justice system remains weak, and it is estimated that only 2% of felony crimes are prosecuted (Shirk, 2011).

Since the start of Calderón's presidency in December 2006, violence has increased dramatically in Mexico. Over 40,000 people were killed by drug trade-related violence between 2007 and mid-2011, and drug trade-related violence has increased by at least 30 percent every year during this period (Rios, 2011b). More than 85 percent of the violence consists of people involved in the drug trade killing each other. Whether or not the increase in violence has been caused by state policy has been controversial, with political scientists taking both sides of the debate (see Shirk, 2011, p. 8 for a discussion of this controversy). Some have used qualitative and descriptive evidence to argue that the state's policies have ignited conflicts between traffickers, leading to the large increase in violence in recent years (see Guerrero, 2011 for a detailed dis-

[4]Notable examples include Operation Condor in the 1970s to eradicate illicit drug crops in northern Mexico and the deployment of federal troops to Nuevo Laredo, Tamaulipas by PAN president Vicente Fox in the early 2000s.

cussion), while others argue that violence would have risen substantially in any case as a result of the diversification of drug trafficking organizations into new criminal activities (see Rios, 2011a). This study presents causal evidence linking crackdowns to large increases in drug trade-related violence.

Local authorities command the majority of Mexico's law enforcement officers. In total, there are 2,139 independent state, local, and federal police agencies in Mexico, 2038 of which are municipal police agencies, and 90% of Mexico's approximately 500,000 police officers are under the command of state and municipal authorities (Guerrero, 2011, p. 20). Mayors, who are elected every three years at different times in each of Mexico's 31 states and Federal District, name the municipal police chief and set policies regarding police conduct. Municipal police have a limited mandate, focusing on automobile traffic violations and minor disruptions to public order. It is rare for them to confiscate illicit drugs, and they do not have the training or weaponry typically required to make high level drug arrests. Because their main activities involve patrolling the local environment, municipal police do however serve as critical sources of information for military and federal police attacks on drug trafficking operations. For the same reasons, municipal police are also valuable allies for organized criminals, who need information on who is passing through the municipalities they control so that they can protect local criminal operations and anticipate attacks by their rivals and by federal authorities.[5] The importance of municipal police is reflected by the fact that they form the largest group of public servants killed by drug trade-related violence (Guerrero, 2011).

[5]For example, in a recent meeting on national security, Mexican president Felipe Calderón argued: "The military report that when they enter a city, they tune into the frequency of the municipal police radio and hear them reporting to the criminals every step they [the military] take. 'And right now they are on this avenue arriving at the traffic light on that corner, and they have six trucks and bring this many weapons.' And these municipal police patrols attempt to block their [the military's] access" (*El Pais*, August 26, 2010, translation mine.

Qualitative evidence indicates that PAN mayors are more likely to request law enforcement assistance from the federal government and also suggests that operations involving the federal police and military have tended to be most effective when the relevant local authorities are aligned with the PAN, which controls the federal government (Guerrero, 2011, p. 70).[6] 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 success story of federal intervention. The governor of Baja California belongs to the PAN, which is the party controlling Mexico's executive branch, and the federal intervention began under the auspices of a PAN mayor in Tijuana who was enthusiastic to cooperate with federal authorities. On the other hand, in Ciudad Juarez both the mayor and governor belong to the opposing PRI party, and conflicts and mistrust between municipal and federal police have been rampant.

There are several potential explanations for these patterns. Authorities from the same party may coordinate law enforcement operations more effectively, PAN authorities could be less corrupted by the drug trade, or the preferences of PAN authorities or their constituents could lead them to take a tougher stance on organized crime.[7] While disentangling these explanations is infeasible given data constraints and the inherent empirical challenges in separately identifying politicians' motivations, these plausible channels motivate this study's focus on close elections involving the PAN.

[6]Disaggregated data on federal police assignments and requests by mayors for federal police assistance are not made available to researchers.

[7]I have analyzed official government data on corruption, made available by confidential sources. This data records drug trade-related corruption of mayors in 2008, as measured primarily by intercepted calls from traffickers to political officials. While the data are likely quite noisy, to my knowledge they are the best source of information on drug trade-related corruption available. Corruption was no more common in municipalities where a PAN candidate had been elected mayor by a narrow margin than in municipalities where the PAN candidate had lost by a narrow margin.

2.3 A Network Model of Drug Trafficking

This section develops a simple model of the network structure of drug trafficking, that will serve as an empirical tool for analyzing the direct and spillover effects of local policy towards the drug trade. In this model, traffickers minimize the costs of transporting drugs from origin municipalities in Mexico across the Mexican road network to U.S. points of entry. In the version of the model developed in this section, they incur costs only from the physical distance traversed, and thus take the shortest route to the nearest U.S. point of entry. This simple shortest paths model provides an intuitive starting point for examining the patterns in the data without having to first develop extensive theoretical or empirical machinery.

The trafficking routes predicted by this model are used in Section 4 to explore the mechanisms linking close PAN victories to increases in drug trade-related violence. Section 5 then shows that the model robustly predicts the diversion of drug traffic following close PAN victories and uses the predicted routes to locate violence and economic spillover effects of PAN crackdowns. Specifically, I assume that close PAN victories increase the costs of trafficking drugs through the municipalities that experience them by a pre-specified amount. Close elections occur throughout the sample period, generating plausibly exogenous month-to-month variation in predicted trafficking routes throughout Mexico. I identify spillover effects by comparing this variation in predicted routes to panel variation in illicit drug confiscations, violence, and economic outcomes.

Assuming that trafficking costs depend only on physical distance is a considerable simplification, and in particular it does not allow for interactions between traffickers. After examining the relationships in the data using the intuitive shortest paths model, in Section 5.2 I specify and estimate a richer version of the model that includes congestion costs. I use the simulated method of moments to estimate the parameters of the congestion cost function. The model developed in this section, which assumes that congestion costs are zero, is a special case of the richer model. In practice, both versions of the model robustly predict the diversion of drug traffic following close PAN victories.

I now describe the setup of the model. 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. This network, which contains 17,453 edges, is shown in Figure 2-2. Traffickers transport drugs across the network from a set of origin municipalities to a set of destination municipalities. Destinations consist of U.S. points of entry via terrestrial 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 (U.S. Drug Enforcement Agency, 2011).[8] All destinations pay the same international price for a unit of smuggled drugs. Each origin i produces a given supply of drugs and has a trafficker whose objective is to minimize the cost of trafficking these drugs to U.S. points of entry. I model trafficking decisions as made by local traffickers because, as discussed in Section 2, trafficking operations are typically decentralized. While it does not matter who makes decisions when traffickers only incur costs from distance, this will become relevant when congestion is introduced into the model later in the paper.

[8]There are 370 million entries into the U.S. through terrestrial border crossings each year, and 116 million vehicles cross the land borders with Canada and Mexico (U.S. Drug Enforcement Agency, 2011). More than 90,000 merchant and passenger ships dock at U.S. ports each year, and these ships carry more than 9 million shipping containers. Commerce between the U.S. and Mexico exceeds a billion dollars a day.

The model focuses on domestically produced drugs, and origins are identified from confidential Mexican government data on drug cultivation (heroin and marijuana) and major drug labs (methamphetamine). Opium poppy seed and marijuana have a long history of production in given regions with particularly suitable conditions, and thus we can be confident that the origins for domestically produced drugs are stable and accurate throughout the sample period. In contrast, cocaine - which can only be produced in the Andean region - typically enters Mexico along the Pacific coast via fishing vessels and go-fast boats (U.S. Drug Enforcement Agency, 2011). Thus, the origins for cocaine routes are more flexible, less well-known, and may have changed substantially during the sample period. Moreover, government policies may divert cocaine traffic away from Mexico altogether.[9] For these reasons, the model focuses on domestically produced drugs. In practice, we know little about the quantity of drugs cultivated in each producing municipality, and hence I make the simplifying assumption that each produces a single unit of drugs.

Trafficking paths connect producing municipalities to U.S. points of entry. Formally, a trafficking path is an ordered set of nodes such that an edge exists between two successive nodes. Each edge $e \in \mathcal{E}$ has a cost function $c_e(l_e)$, where l_e is the length of the edge in kilometers. The total cost to traverse path p is $w(p) = \sum_{e \in p} c_e(l_e)$, which equals the length of the path. Let $\mathcal{P}_i$ denote the set of all possible paths between producing municipality i and the United States. Each trafficker solves:

$$\min_{p \in \mathcal{P}_i} w(p) \tag{2.1}$$

This problem, which amounts to choosing the shortest path between each producing

[9]There is some evidence that shipments of cocaine through Haiti have increased in recent years (U.S. Drug Enforcement Agency, 2011).

municipality and the nearest U.S. point of entry, can be solved using Dijkstra's algorithm (Dijkstra, 1959), which is an application of Bellman's principal of optimality.

2.4 Direct Effects of Close PAN Victories on Violence

This section uses a regression discontinuity approach to test whether the outcomes of close mayoral elections involving the National Action Party (PAN) - which has spearheaded the war on drug trafficking - affect violence in the municipalities experiencing these close elections. While disaggregated data on mayoral requests for federal police as well as the allocation of the military and federal police are not made available to researchers, the qualitative evidence discussed in Section 2 suggests that PAN mayors are more likely to crack down on the drug trade by enlisting the assistance of federal law enforcement and coordinating operations with them. This section first describes the data on violence and local politics and then provides a graphical analysis of the relationship between violence and the outcomes of close elections. It then explores the robustness of this relationship and finally uses measures of the industrial organization of drug trafficking to explore mechanisms linking the outcomes of close elections to violence. To the extent that local crackdowns incentivize traffickers to relocate some of their operations, PAN victories could also exert spillover effects. These will be examined in Section 5.

2.4.1 Data

The analysis uses official government data on drug trade-related outcomes, obtained from confidential sources unless otherwise noted. Drug trade related homicides and armed confrontations between authorities and organized criminals occurring between December of 2006 and 2009 were compiled by a committee with representatives from all ministries who are members of the National Council of Public Security (*Consejo Nacional de Seguridad Pública*). This committee meets each week to classify which homicides from the past week are drug trade-related.[10] 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 how many people have been killed in armed clashes between police and organized criminals. Confidential daily data on homicides occurring between 1990 and 2008 were obtained from the National Institute of Statistics and Geography (INEGI). Confidential data on high level drug arrests occurring between December of 2006 and 2009 are employed as well. High level traffickers include the kingpins of the major trafficking organizations, the regional lieutenants of these organizations, hired assassins, and the financiers who conduct money laundering operations.

This section also uses official government data on drug trade-related organizations (DTOs), which include major trafficking organizations as well as local gangs. The data list which of Mexico's 2456 municipalities had at least one DTO operating within their limits in early 2008 and also provide the identity of the group if it is a major trafficking organization. They offer the closest possible approximation to pre-period DTO presence available, given that systematic data about DTOs were not collected before this time.

Finally, electoral data for elections occurring during 2007-2008 were obtained

[10]Previously reported homicides are also considered for reclassification if new information has become available.

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 2.1.

2.4.2 Econometric framework and graphical analysis

I now outline the study's empirical approach for estimating the direct effects of close PAN victories on violence and also provide a graphical analysis of the relationship between local politics and violence. I first analyze cross-sectional violence measures using standard non-parametric regression discontinuity methods (as described in Imbens and Lemieux, 2008), and then exploit the panel variation in the violence data by examining a specification that combines regression discontinuity and differences-in-differences. These approaches yield similar estimates.

In order to perform the regression discontinuity analysis, I restrict the data to a small window around the PAN win-loss threshold, so that only municipalities with a narrow vote spread between the winner and runner-up contribute to the estimate of the discontinuity. I choose this bandwidth using the Imbens-Kalyanaraman bandwidth selection rule (2009).[11] I then estimate a local linear regression using a triangular kernel, which ensures that the weight on each observation decays with the distance from the threshold. Specifically, I estimate the following regression model within the bandwidth:

$$y_{ms} = \alpha_0 + \alpha_1 PANwin_{ms} + \alpha_2 PANwin_{ms} \times spread_{ms} + \alpha_3(1 - PANwin_{ms}) \times spread_{ms} + \delta X'_{ms} + \beta X'_{ms} PANwin_{ms} + \alpha_s + \epsilon_{ms} \quad (2.2)$$

[11]Results (available upon request) are robust to using a variety of different bandwidths.

where y_{ms} is the outcome of interest in municipality m in state s. $PANwin_{ms}$ is an indicator equal to 1 if the PAN candidate won the election, and $spread_{ms}$ is the margin of PAN victory. Some specifications also include α_s, a state-specific intercept and X'_{ms}, demeaned baseline controls. While baseline controls and fixed effects are not necessary for identification, their inclusion improves the precision of the estimates. The sample is restricted to elections where the PAN won or came in second.

Identification requires that all relevant factors besides treatment vary smoothly at the threshold between a PAN victory and a PAN loss. That is, letting y_1 and y_0 denote potential outcomes under a PAN victory and PAN loss, respectively, and *spread* denote the PAN margin of victory, identification requires that $E[y_1|spread]$ and $E[y_0|spread]$ are continuous at the PAN win-loss threshold. This assumption is needed for municipalities where the PAN barely won to be an appropriate counterfactual for municipalities where the PAN barely lost.

To assess the plausibility of this assumption, Table 2.1 compares municipal crime, political, economic, demographic, road network, and geographic characteristics in municipalities where the PAN barely lost to those in municipalities where they barely won. Crime characteristics include the average monthly drug-trade related homicide rate between December of 2006 (when these data were first collected) through June of 2007 (when the first authorities elected during the sample period were inaugurated), as well as the average probability that a drug trade-related homicide occurred in a given month during this period. They also include police-criminal confrontation deaths per 100,000 inhabitants (Dec. 2006 - Jun. 2007), the average probability that police criminal-confrontation deaths occurred, and the long-run average municipal homicide rate (1990-2006). Political characteristics explored are municipal tax collection per capita (2005), municipal taxes per dollar of income (2005), dummies for the party of the mayoral incumbent, the percentage of electoral cycles between 1976 and 2006 in which the party of the mayor alternated, and a dummy equal to 1 if the PRI always controlled the mayorship between 1976 and 2006. Demographic characteristics are population (2005), population density (2005), and migrants per capita (2005). Economic characteristics include income per capita (2005), the municipal Gini index (2005), migrants per capita (2005), malnutrition (2005), mean years of schooling (2005), infant mortality (2005), percent of households without access to sewage (2005), percent of households without access to water (2005), and the municipal marginality index (2005).[12] Road network characteristics are the total detour length in kilometers required for the shortest path drug routes to circumvent the municipality, total length of roads in the municipality (2005), road density, and distance of the municipality to the U.S. border. Finally, the geographic characteristics are average municipal elevation, slope, surface area, low temperature (1950-2000), high temperature (1950-2000), and precipitation (1950-2000). Sources for these variables are listed in the notes to Table 2.1.

Column (1) of Table 2.1 reports the mean value for each variable in municipalities where the PAN barely lost, column (2) does the same for municipalities where the PAN barley won, and column (3) reports the t-statistics on the difference in means. The sample is limited to elections with a vote spread between the winner and the runner-up of five percentage points or less. In no case are there statistically significant differences between municipalities where the PAN lost and municipalities where they won.[13] Moreover, I run the local linear regression specification given in equation (2.2)

[12]The marginality index incorporates information on literacy; primary school completion rates; access to electricity, sewage, and running water; household overcrowding; construction materials used in households; municipal population in rural areas; and household income.

[13]Results are similar if the vote spread is limited to three or seven percentage points instead.

using each of the baseline characteristics as the dependent variable.[14] The coefficient on PAN win is reported in column (4) and the t-statistic on PAN win is reported in column (5). The coefficients on PAN win estimated by local linear regression tend to be small and in no case are they statistically different from zero. Overall, this evidence strongly suggests that municipalities where the PAN barely lost are a valid control group for municipalities where they barely won.

Identification also requires the absence of selective sorting around the PAN win-loss threshold. This assumption would be violated, for example, if PAN candidates could rig elections in their favor in municipalities that would later experience an increase in violence. While there are many historical examples of electoral fraud in Mexico, the political system has become dramatically more open since the early 1990s and elections are coordinated by a multi-partisan state commission. The balancing of the sample on the crime pre-characteristics, the electoral variables, and the many other characteristics in Table 2.1 indicates that rigged elections are unlikely to drive the results.

Due to space constraints, I focus on the probability that drug trade-related homicides occur in a given month in the graphical analysis, and later Table 2.2 documents that the results are robust to instead using the drug trade-related homicide rate per 10,000 municipal inhabitants.[15] Recall that drug trade-related homicides are those in which at least one party is involved in the drug trade. In over 85 percent of these homicides, both the aggressor and victim are involved in the drug trade. While a few municipalities always experience drug trade-related homicides, there is considerable

[14]State fixed effects are omitted to make the specification analogous to the difference in means reported in column (3). Results are similar when state fixed effects are included, and in no case is there a statistically significant discontinuity.

[15]It is not obvious that drug trade-related homicides should be normalized by municipal population, as drug trafficking activity is not necessarily proportional to population.

variation in the extensive margin of violence.

The six panels in Figure 2-3 plot 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 0.0025. The solid line plots predicted values from a local linear regression, with separate vote spread trends estimated on either side of the PAN win-loss threshold. The dashed lines show 95% confidence intervals. The bandwidth is chosen using the Imbens-Kalyanaraman bandwidth selection rule (2009).

The dependent variable in Panel A is the average probability that a drug trade-related homicide occurs in a given municipality-month during the five months following the inauguration of new authorities. Panel A shows that in the post-inauguration period, there is a marked discontinuity in drug trade-related homicides at the threshold between a PAN loss and a PAN victory. The probability that a drug trade-related homicide occurs in a given month is around nine percentage points higher after a PAN mayor takes office than after a non-PAN mayor takes office. This can be compared to the sample average probability of six percent that a drug trade-related homicide occurs in a given month.

Next, Panel B examines drug trade-related homicides during the one to five month period between the election and inauguration of new authorities (the lame duck period), whose length varies by state. The figure documents that drug trade-related violence is similar in municipalities where the PAN barely won as compared to those where they barely lost. Panel C performs a placebo check, examining the average probability of a drug trade-related homicide during the six months prior to the election. There is no discontinuity at the PAN win-loss threshold, supporting the plausible exogeneity of close elections.

While homicides are classified as drug trade-related by a national committee, it is possible that the information in the police reports used to make this classification could systematically differ across municipalities. To explore whether the discontinuity in Panel A could simply reflect the reclassification of homicides by PAN authorities, Panels D through F examine the non-drug trade-related monthly homicide rate per 10,000 municipal inhabitants, for the post-inauguration, lame duck, and pre-election periods, respectively. There are no statistically significant discontinuities, and this is also the case when a dummy measure of non-drug trade-related homicides is used (as documented in Table A1 in the online appendix). These results alleviate concerns that close elections simply affect the classification of violence.

To shed further light on the relationship between violence and close PAN victories, I estimate equation (2.2) separately for each month prior to the election and following the inauguration of new authorities. Figure 2-4 reports the coefficients on PAN win. State fixed effects and controls for the characteristics listed in Table 2.1 are included in order to increase the precision of the estimates.[16] Figure 2-4 plots the coefficients for the period lasting from six months prior to the election to six months following the inauguration of new authorities. The lame duck period is excluded due to its varying length by state, which makes it difficult to examine transparently in a month-by-month analysis. The dashed lines plot 95% confidence intervals.

In Panel A, the dependent variable is a dummy equal to one if a drug trade-related homicide occurred in a given municipality-month. The PAN win coefficients document that before elections, drug trade-related homicides occurred with similar frequency in municipalities where the PAN would later barely lose versus in municipalities where they would barely win. These estimates support the validity of the identification strategy. Following the inauguration of new authorities, the PAN win coefficients become large, positive, and are statistically significant at the five or ten

[16]The coefficient magnitudes are similar when the controls are excluded.

percent level in all periods except for six months following the inauguration.[17]

As an additional check on these results, I explore the relationship from an alternative perspective that exploits the full panel variation available in the monthly homicide series. Specifically, Panel B of Figure 2-4 plots the γ_τ coefficients from the following differences-in-differences specification against time:

$$y_{mst} = \beta_0 + \sum_{\tau=-T_{ms}}^{T_{ms}} \beta_\tau \zeta_{\tau m} + \sum_{\tau=-T_{ms}}^{T_{ms}} \gamma_\tau \zeta_{\tau m} PANwin_{ms} + f(spread_{ms}) Post_{mst} + \psi_{st} + \delta_m + \epsilon_{mst} \quad (2.3)$$

where y_{mst} is the outcome in municipality m in state s in month t and $\{\zeta_\tau\}$ is a set of months-to-election and months-since-inauguration dummies. $Post_{mst}$ is a dummy equal to 1 for all periods t in which the new municipal authorities have assumed power. $f(\cdot)$ is the RD polynomial, which is assumed to take a quadratic form in the graphical analysis. $spread_{ms}$ is the margin of PAN victory, ψ_{st} are state x month fixed effects, and δ_m are municipality fixed effects. ϵ_{mst} is clustered by municipality. The sample is a balanced panel, limited to municipalities with a vote spread of five percentage points or less between the winner and runner-up.[18] Panel B of Figure 2-4 shows that the magnitudes of the γ_τ coefficients are similar to the month-by-month cross-sectional RD estimates plotted in Panel A.

Panels C and D repeat the exercise for non-drug trade-related homicides. The

[17] When the post-period is extended to a year following the inauguration of new authorities, the coefficients are more volatile between seven and twelve months after the inauguration of new authorities (see Appendix Figure 1). Whether this is due to PAN authorities successfully deterring drug trafficking activity or results from these authorities becoming less tough on crime is not possible to establish definitively, but spillover results presented in the next section suggest that drug traffic continues to be diverted to other municipalities beyond the first six months that a PAN mayor has been in office.

[18] Results (available upon request) are similar when alternative windows around the PAN win-loss threshold are used.

month-by-month RD and the panel specification both show the absence of a discontinuity at the PAN win-loss threshold, before and after the inauguration of new authorities. Overall, the evidence in Figures 2-3 and 2-4 strongly support the hypothesis that government policy has exerted important effects on drug trade-related violence in Mexico.

2.4.3 Further results and robustness

The graphical analysis shows that drug trade-related violence in a municipality increases substantially after the close election of a PAN mayor. Before moving on to explore mechanisms, I examine this result in more detail.

Columns (1) through (3) of Table 2.2 report estimates from the local linear RD specification given by equation (2.2). Panel A examines the probability of a drug trade-related homicide in a given month, and Panel B examines the drug trade-related homicide rate.[19] The specification includes state fixed effects and controls for the baseline characteristics listed in Table 2.1. The post-inauguration period extends to five months following the inauguration of new authorities (after which point the month-by-month analysis suggests that the violence effects start to decline somewhat), and the pre-election period extends to six months prior to the election.

Column 1 estimates that the average probability that at least one drug trade-related homicide occurs in a municipality in a given month is 8.4 percentage points higher after a PAN mayor takes office than after a non-PAN mayor takes office, and this effect is statistically significant at the one percent level. The drug trade-related homicide rate per 10,000 municipal inhabitants is around 0.05 (s.e. = 0.02) higher following a close PAN victory, which can be compared to the average monthly

[19]Analysis of the non drug trade-related homicide rate robustly shows no discontinuity at the PAN win-loss threshold and due to space constraints is presented Table A1 in the online appendix.

homicide rate of 0.06. In contrast, the estimated coefficients for the lame duck and pre-inauguration periods reported in columns (2) and (3) are small and statistically insignificant in both panels. Columns (4) and (5) document that the PAN win effect is robust to excluding the state fixed effects as well as to excluding both the baseline controls and state fixed effects, as we would expect if close PAN victories are as if randomly assigned.[20]

Next, column (6) reports results from the following panel specification, which is analogous to the differences-in-differences specification examined in the graphical analysis:

$$\begin{aligned} y_{mst} = \beta_0 &+ \beta_E LameDuck_{mst} + \beta_I PostInnaug_{mst} + \gamma_E LameDuck_{mst} PANwin_{ms} \\ &+ \gamma_I PostInnaug_{mst} PANwin_{ms} + f(spread_{ms}) LameDuck_{mst} \\ &+ f(spread_{ms}) PostInnaug_{mst} + \psi_{st} + \delta_m + \epsilon_{mst} \quad (2.4) \end{aligned}$$

$LameDuck_{mst}$ is a dummy equal to one for all periods t between the election and inauguration of new authorities, $PostInnaug_{mst}$ is a dummy equal to one for all periods t in which the new municipal authorities have assumed power. All other variables are defined as in equation (2.3). Pre-election is the omitted category, and ϵ_{mst} is clustered by municipality. The sample is limited to municipalities with a five percentage point vote spread or less.[21]

Column (6) reports estimates from equation (2.4) when a linear functional form is used for the RD polynomial. This specification estimates that the probability that at least one drug trade-related homicide occurs in a municipality in a given month is 14.7

[20]The estimated effects for the lame duck and pre-election periods are also similar when state fixed effects and baseline controls are excluded.

[21]Results are very similar when a seven or three percentage point vote spread is used instead.

percentage points higher after a PAN mayor takes office than after a non-PAN mayor takes office, and the monthly drug trade-related homicide rate is estimated to be 0.09 higher. The coefficients on lame duck × PAN win are substantially smaller than the post-inauguration × PAN win coefficients and are statistically insignificant. These results provide additional support for the robustness of the relationship between the outcomes of close elections and violence.

Local linear regression will not necessarily provide an unbiased estimate of the magnitude of the discontinuity if the true underlying functional form is not linear (Lee and Lemieux, 2009). While the RD figures suggest that the data are reasonably approximated by a linear functional form, columns (7) through (12) explore robustness to specifying the RD using a variety of functional forms. Columns (7), (9), and (11) estimate equation (2.2) using quadratic, cubic, and quartic vote spread terms, respectively (along with state fixed effects and baseline controls).[22] Columns (8), (10), and (12) estimate the panel specification using quadratic, cubic, and quartic RD polynomials, respectively. The estimated effects of close PAN victories on the probability of drug trade-related violence are large, positive, and statistically significant across specifications. The coefficients tend to increase somewhat in magnitude when higher order polynomials are used. The estimated impacts on the drug trade-related homicide rate are also large, positive, and statistically significant in all specifications except for the cross-sectional RD with a quartic polynomial.[23]

Deaths in police-drug trafficker confrontations and drug trade-related arrests are additional outcomes that we would expect to be affected by the outcomes of close elections. Both of these phenomena are more common in municipalities that have

[22] These specifications use the same bandwidth as the linear specification.

[23] Results (available upon request) are very similar when I use a semi-parametric, cross-sectional RD approach with various orders of RD polynomials, limiting the sample to municipalities with a five percentage point vote spread or less.

experienced a close PAN victory. Deadly conflicts between police and drug traffickers are relatively rare, occurring in only 20 municipality-months and 12 municipalities with a vote spread of five percentage points or less. Following close elections, these confrontations are fifty percent more likely to occur in municipalities where the PAN barely won than in municipalities where they barely lost. As regards arrests, the confidential federal government database of drug-related arrests includes only high level arrests, since most other drug-related arrests are never prosecuted. High level arrests occurred in only 4 municipalities and 15 municipality months during the sample period. During the post-inauguration period, 49 high level arrests occurred in municipalities where the PAN barely won, as compared to only 26 in municipalities where they barely lost. Given the rarity of these events, there is not much power for conducting a rigorous econometric analysis. When I do analyze these outcomes using the RD approach, the coefficients on PAN win are positive and marginally significant in some specifications (results available upon request).

Table 2.2 provides robust evidence that close PAN victories increase drug trade-related violence. Next, I briefly explore whether there are heterogeneous effects based on local political characteristics. The dependent variable in all columns of Table 2.3 is the average probability that a drug trade-related homicide occurs in a given month during the post-inauguration period, and the coefficients are estimated using local linear regression.[24] For comparison purposes, column (1) of Table 2.3 reports the baseline result from column (1) of Table 2.2. Next, column (2) examines a specification that distinguishes between municipalities where the PAN was the incumbent and municipalities where another party held the incumbency.[25] This

[24]As documented Table A2 in the online appendix, the results are very similar when I instead use a panel data specification.

[25]In Mexico, mayors cannot run for re-election, so regardless of the party of the incumbent a new politician always takes office with each electoral cycle.

specification includes the same terms as the baseline RD specification in equation (2.2) and also interacts PAN win, spread, and PAN win $\times$ spread with the PAN incumbency dummy. The estimated effect on violence of a PAN mayor taking office, relative to a non-PAN mayor taking office, is large and statistically significant regardless of whether the PAN held the incumbency. Prior to the close elections, the average probability of a drug trade-related homicide in a given municipality-month is modestly higher in municipalities with a PAN incumbent than it is in municipalities with a mayor from a different party (0.067 as compared to 0.048). Following the inauguration of closely elected PAN authorities, violence increases sharply regardless of the party of the incumbent. In contrast, following the inauguration of closely elected non-PAN authorities, violence decreases slightly and by a similar amount regardless of the party of the incumbent. These findings are consistent with descriptive evidence on Mexican violence outbreaks since 2006. This evidence shows that once violence increases, it may increase further but typically does not decline to pre-outbreak levels (Guerrero, 2011). The findings also suggest that PAN mayors narrowly elected in 2007 and 2008 may have been tougher on drug trafficking than their PAN predecessors, who were elected before drug trafficking became a major policy issue.

Column (3) reports a specification that distinguishes between whether the PAN candidate faced an opponent from the historically dominant PRI party, which opposed the PAN in around three quarters of elections. There are not statistically significant differences in drug trade-related violence in municipalities where the PAN mayor faced a PRI opponent as compared to municipalities where the PAN mayor faced an opponent from another party.

Next, columns (4) and(5) present further evidence that the effects documented in Table 2.2 result specifically from the PAN taking office. Recall that the sample for the results presented thus far includes municipalities with close elections where a PAN candidate was the winner or runner-up. In contrast, column (4) examines close elections where the PRI and PRD - Mexico's two other major parties - received the two highest vote shares. The PAN win dummy in the RD specification is replaced by a PRI win dummy. While the coefficient on PRI win is positive, it is about half the magnitude of the coefficient on PAN win in the baseline specification and is not statistically significant. Column (5) includes all close elections in the sample, regardless of which parties received the two highest vote shares. The PAN win dummy is replaced by a dummy equal to one if there was an alternation in the political party of the mayor. As expected given the results in columns (2) and (4), the coefficient on the alternation indicator is small and statistically insignificant.

Finally, column (6) examines all municipalities where a PAN candidate was the winner or runner-up, reporting results from an ordinary least squares regression of the probability that a drug trade-related homicide occurs in a given month during the post-inauguration period on the PAN win dummy, controls, and state fixed effects. While the coefficient on PAN win is positive, it is small in comparison to the estimate from the RD, with a magnitude of 0.01, and it is not statistically significant. Politics are likely to be meaningfully different in municipalities with very competitive elections as compared to municipalities with uncompetitive elections, and omitted variables bias in the ordinary least squares regression could also explain the difference between the estimates in column (1) versus column (6). Section 5 will use only the plausibly exogenous variation generated by close PAN elections to identify spillover effects. It will use all of these close elections, since the direct effects on violence are similar regardless of the party of the incumbent or the opponent.

2.4.4 Trafficking Industrial Organization and Violence

Regression discontinuity estimates show that drug trade-related homicides in a municipality increase substantially after the close election of a PAN mayor. I now examine potential mechanisms linking close PAN victories to violence. Over eighty-five percent of the drug trade-related violence consists of people involved in the drug trade killing each other. The evidence presented in this section suggests that the violence reflects rival traffickers' attempts to wrest control of territories after crackdowns initiated by PAN mayors have challenged the incumbent criminals.

I begin by categorizing municipalities into four groups using confidential government data on drug trafficking organizations (DTOs). The categories are: 1) municipalities controlled by a major DTO that border territory controlled by a rival DTO (9.5% of the sample), 2) municipalities controlled by a major DTO that do not border territory controlled by a rival DTO (20% of the sample), 3) municipalities controlled by a local drug gang (33% of the sample), and 4) no known drug trade presence (37.5% of the sample).[26] Municipalities with no known drug trade presence had not experienced any 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 to federal authorities.

For comparison purposes, column (1) of Table 2.4 reports the baseline local linear regression result from column (1) of Table 2.2 and column (2) reports the baseline differences-in-differences estimate from column (6) of Table 2.2. Next, column (3) uses a local linear regression specification to explore the relationship between the violence response to a close PAN victory and the structure of the drug trade. The dependent variable is the average probability that a drug trade-related homicide

[26]The major DTOs during the sample period are Beltran, Familia Michoacana, Golfo, Juarez, Sinaloa, Tijuana, and Zetas.

occurs in a given month during the post-inauguration period. The specification includes the same terms as the baseline RD specification in equation (2.2), as well as interacting PAN win, spread, and PAN win $\times$ spread with the dummies for the three categories of drug trade presence. No known drug trade presence is the omitted category.

The estimates show that the effect of close PAN victories on violence is extremely large in municipalities controlled by a major DTO that border a rival DTO's territory. A close PAN victory increases the probability that a drug trade-related homicide occurs in a given month by a highly significant 53 percentage points. The estimated effect of 14.6 percentage points for municipalities controlled by a major DTO that do not border territory controlled by a rival is considerably smaller but still statistically significant at the 5% level. The estimated effects of close PAN victories on violence in municipalities with a local drug gang or with no known drug trade presence are small and statistically insignificant.

To examine the robustness of this result, column 4 of Table 2.4 reports a panel specification analogous to equation (2.4), in which dummies for the three categories of drug trade presence are interacted with post-inauguration and post-inauguration $\times$ PAN win. The coefficient on post-inauguration $\times$ PAN win $\times$ borders rival, equal to 0.524, is nearly identical to the analogous coefficient on PAN win $\times$ borders rival in column (3) and is statistically significant at the one percent level. The violence effect for municipalities with a major DTO that do not border territory controlled by rival DTOs is also similar to the effect estimated by local linear regression. In municipalities with only a local drug gang or with no known drug trafficking presence, a close PAN win is estimated to increase the probability of a drug trade-related homicide by a statistically significant 12.7 and 10.7 percentage points, respectively.[27]

[27]The fact that the estimates are similar for these groups of municipalities suggests that local

While these effects are larger than those estimated by local linear regression, both specifications estimate that the violence effects for these municipalities are smaller than the effects for municipalities with a major DTO. Columns (5) and (6) show that when a quadratic instead of a linear functional form is used for the vote spread terms, the results are qualitatively similar (although the still very large effect for municipalities bordering a rival is no longer statistically significant in column 5).[28]

Next, I use the shortest paths trafficking model to calculate the total detour costs that would be imposed if trafficking routes could no longer pass through a municipality. Total detour costs equal the sum of the lengths (in kilometers) of shortest paths from all producing municipalities to the U.S. when paths are not allowed to pass through the municipality under consideration minus the sum of the lengths of all shortest paths when they can pass through any municipality in Mexico. Columns (7) through (12) interact PAN win or PAN win $\times$ post with standardized total detour costs.[29] A one standard deviation increase in detour costs increases the probability of a drug trade-related homicide following a close PAN victory by around seven percentage points in the local linear regression specification. This can be compared to the 8.7 percentage point effect of close PAN victories at the sample mean of detour costs. Similar patterns arise when the panel specification is used (column 8) and when higher order vote spread terms are used (columns 9 through 12). In summary, violence increases the most in municipalities that impose the greatest detour costs to circumvent, and hence are likely to be valuable to control.

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drug gang presence may be under-reported.

[28]There is not enough variation in the data to precisely estimate a local regression with four separate cubic vote spread trends on either side of the discontinuity. When I estimate a specification with separate PAN win x territorial ownership dummies and a single cubic vote spread term estimated separately on either side of the discontinuity, the results are similar to those presented in Table 2.4.

[29]Results are similar, but more difficult to interpret, when I do not standardize the detour costs measure.

The characteristics examined in Table 2.4 are highly correlated, and moreover the presence of drug trafficking groups is likely an outcome of the network structure of drug trafficking. Thus, I cannot separately identify the impacts of territorial ownership from the impacts of the routes structure. Nevertheless, together the results strongly suggest that the industrial organization of drug trafficking exerts important effects on the violent response to close PAN victories. While crackdowns likely reduce rents from illicit activities in the short-to-medium run, by weakening the incumbent criminal group they also reduce the costs of taking control of a municipality. Controlling the municipality is likely to offer substantial rents from trafficking and the variety of other criminal activities that DTOs control once the crackdown has subsided. Because crackdowns may divert drug traffic while in effect, I now turn to an investigation of spillover effects.

2.5 A Network Analysis of Spillover Effects

This section uses the network model of the drug trade and plausibly exogenous variation provided by the outcomes of close elections to identify the spillover effects of local crackdowns. Correlations between policy in one municipality and drug trade-related outcomes elsewhere could occur for several reasons, as highlighted more generally by Manski's formal treatment of spillover effects (1993). First, correlations could result from environmental factors unrelated to the local policies under consideration. Second, they could occur because traffickers choose to operate in given geographic arrangements for reasons unrelated to policy. Finally, local drug trafficking policies in one municipality could exert spillover effects, influencing outcomes elsewhere. The plausibly exogenous variation provided by close elections allows spillovers to be isolated from other correlations in outcomes across municipalities.

This section begins by examining whether the simple shortest paths network model is predictive of the diversion of drug traffic following close PAN victories. Then, I specify and estimate a richer version of the model that incorporates congestion costs. Finally, I test whether PAN crackdowns exert spillover effects on violence and economic outcomes. The section concludes by discussing possible extensions to the analysis.

2.5.1 Do close PAN victories divert drug traffic?

I begin by examining whether close PAN victories divert drug traffic to alternative routes predicted by the shortest paths trafficking model. The costs of trafficking drugs through a municipality are assumed to increase to infinity (or, in the robustness checks, by some other positive proportion) for the remainder of the sample period following the inauguration of PAN mayors elected by a vote spread of five percentage points or less.[30] Because municipal elections happen at different times throughout the sample period, this generates month-to-month plausibly exogenous variation in the shortest routes from producing municipalities to U.S. points of entry. I examine the relationship between model predicted variation in routes and variation in actual illicit drug confiscations, the best available proxy for actual illicit drug traffic, using the following regression specification:

$$conf_{mst} = \beta_0 + \beta_1 Routes_{mst} + \psi_{st} + \delta_m + \epsilon_{mst} \tag{2.5}$$

where $conf_{mst}$ is actual illicit drug confiscations of domestically produced drugs in municipality m in month t. Both an indicator measure and a continuous measure are

[30]Results (available upon request) are similar when I instead use municipalities with a vote spread of three percentage points or less or with a vote spread of seven percentage points or less.

explored. $Routes_{mst}$ is a measure of predicted drug trafficking routes, ψ_{st} is a month x state fixed effect, and δ_m is a municipality fixed effect. Because variation in routes may be correlated across space, the error term is clustered simultaneously by municipality and state-month (following the two-way clustering of Cameron, Gelbach, and Miller, 2011). The sample excludes municipalities with close elections, since the aim of the model is to predict spillovers from these elections.[31] This empirical approach is summarized in Figure 2-1.

The confiscations data provide the value of all illicit drug confiscations made by Mexican authorities between December of 2006 and December of 2009 and were made available by confidential sources. The value of confiscations (evaluated at Mexican illicit drug prices) in a municipality-month must be equal to at least $1,000 USD to be included in the sample. Occasionally the total value of confiscations in a municipality-month is both less than $1,000 and positive, and such confiscations are very likely to be from individual consumers and not from drug traffickers.[32] The confiscations rate per unit of drug traffic likely differs depending on the political environment. However, because municipal elections only occur once every three years, local authorities typically do not change and the municipality fixed effects will absorb time invariant differences in the probability of confiscations across municipalities.

Panel A of Table 2.5 reports estimates from equation (2.5), using an indicator variable equal to one if a municipality has a predicted trafficking route in a given month as the routes measure. In column (1), the dependent variable is a dummy equal to one if domestically produced illicit drugs are confiscated in a given

[31]It also excludes producing municipalities, since the analysis focuses on the extensive margin of predicted trafficking routes, and the producing municipalities mechanically contain a trafficking route. Results (available upon request) are robust to including these municipalities.

[32]Estimates are robust to using a variety of different cut-offs for the minimum value of drugs confiscated to construct the confiscations dummy variable (results available upon request).

municipality-month. When a municipality acquires a predicted trafficking route, drug confiscations increase by around 1.6 percentage points, relative to a sample average probability of confiscations in a given municipality-month of 5.3 percent. This correlation is statistically significant at the 1% level. In column (2), the dependent variable is equal to the log of the value (in US dollars) of domestic illicit drug confiscations in the municipality-month if confiscations are positive and equal to zero otherwise. Because all positive confiscation values are at least equal to 1,000 USD, this measure is always positive.[33] The correlation between the log value of confiscations and predicted trafficking routes is large, positive, and statistically significant at the one percent level. Acquiring a predicted trafficking route is associated with an increase in the value of confiscated drugs of around 18.5 percent. In Table A3 of the online appendix, I repeat the analysis in Table 2.5 using the number of predicted routes instead of the indicator routes measure and find similar results. The value of confiscations increases by 2.3 percent for each additional trafficking route acquired, and this effect is statistically significant at the one percent level.

One concern in interpreting these results is that the relationship between predicted trafficking routes and actual confiscations could result from the direct effects of PAN crackdowns. For example, if alternative shortest paths traverse nearby municipalities and if PAN authorities coordinate with the military and federal police, who become active in an entire region, this could lead to a correlation between changes in shortest routes and changes in confiscations in nearby municipalities. It is much more difficult to tell a story in which close PAN victories directly affect drug trade outcomes in municipalities located further away. Thus, columns (3) and (4)

[33]Working in logs is attractive because drug confiscations are highly right-skewed, with several major drug busts resulting in tens of millions of dollars worth of confiscated drugs. Using log values makes the data more normally distributed and aids in the interpretation of the results.

examine whether the model remains predictive when municipalities bordering those that have experienced a close PAN victory are dropped from the sample. The estimated coefficients are similar in magnitude to those reported in columns (1) and (2) and are statistically significant at the 5 percent level.

To shed further light on the plausibility of the model, columns (5) through (8) report placebo checks. First I assume, contrary to the regression discontinuity evidence, that the costs of passing through municipalities that have experienced a close PAN loss are infinity, whereas there is no additional cost beyond traversing the physical distance to traffic drugs through a municipality that has experienced a close PAN win. This provides a basic test of whether the model loses its predictive power when it uses the wrong shocks. Columns (5) and (6) show that the model does lose its predictive power when this implausible assumption is made.

Next, columns (7) and (8) test whether variation in routes induced by close PAN victories is correlated with variation in cocaine confiscations. Because cocaine origins are different from the origins for domestically produced drugs, the predicted domestic drug routes should be uncorrelated with cocaine confiscations. Columns (7) and (8) document that the coefficients on the predicted routes dummy are small and statistically insignificant, whether a dummy or value measure of cocaine confiscations is used as the dependent variable. These results lend further support to the validity of the model.

Thus far, I have assumed that the cost of passing through a municipality that has experienced a close PAN victory is infinity. Figure 2-5 explores whether the relationship between predicted trafficking routes and the value of domestic drug confiscations is robust to assuming that a close PAN victory proportionally increases the effective length of the edges in a municipality by a given factor α.[34] The x-axis

[34]For example, if the length of a road through a municipality equals 10 kilometers and $\alpha = 2$,

plots values of α ranging from 0.25 to 10 and the y-axis plots the coefficient on the routes dummy when a close PAN victory is assumed to proportionately increase the effective length of edges in a municipality by α. 95% confidence bands are shown with a thin black line and 90% confidence bands with a slightly thicker black line.

Moving from left to right across the x-axis, the first two cost factors, 0.25 and 0.5, serve as placebo checks. The RD evidence indicates that a close PAN victory makes trafficking drugs more costly, whereas $\alpha = 0.25$ and $\alpha = 0.5$ imply that PAN victories reduce trafficking costs. These placebo estimates are small, and none are statistically significant at the 10% level. In contrast, the estimates for cost values greater than one are similar to the baseline estimate in Table 2.5 and all are statistically significant at the 5% level.[35]

2.5.2 A richer trafficking model

While the shortest paths model robustly predicts the diversion of drug traffic following PAN victories, assuming that traffickers take shortest distance routes is clearly a simplification. In particular, we might expect that traffickers care about what routes other traffickers are taking. There are (at least) several reasons why the costs of traversing an edge may change as traffic on the edge increases. The probability of violent conflict with other traffickers may change, the quality of hiding places may decline (particularly at U.S. points of entry), and law enforcement authorities may direct more or less attention per unit of traffic. Thus, before examining whether the diversion of drug traffic is accompanied by violence and economic spillover effects, I develop a richer version of the trafficking model that incorporates congestion costs

before a close PAN victory it would cost 10 to traffic drugs through the municipality and afterwards it would cost 20.

[35]Cost values between 0.5 and 1.5 are not informative, as values close to 1 do not generate enough variation in the edge costs over time to create within-municipality variation in trafficking routes.

when routes coincide. These costs introduce interactions between traffickers, producing a potentially more nuanced framework for locating spillovers and conducting policy analysis. Because congestion costs are unknown, I specify a congestion cost function and estimate its parameters using the simulated method of moments and cross-sectional data on confiscations from the beginning of the sample period.

Recall from Section 3 that each origin i produces a unit of drugs and has a trafficker who decides how to transport the municipality's supply of drugs to U.S. points of entry. Each of the destinations has a given size, approximated by the number of commercial lanes for terrestrial border crossings and the container capacity for ports. All destinations pay the same international price for a unit of smuggled drugs.

Let $\mathcal{P}_i$ denote the set of all possible paths between producing municipality i and the United States, let $\mathcal{P} = \cup_i \mathcal{P}_i$ denote the set of all paths between all producing municipalities and the United States, and let x_p denote the flow on path $p \in P$. Each edge $e \in E$ has a cost function $c_e(l_e, x_e)$, where l_e is the length of the edge in kilometers and $x_e = \sum_{p \in \mathcal{P} | e \in p} x_p$ is the total flow on edge e, which equals the sum of flows across the paths that traverse it. I do not impose costs for trafficking drugs through territory controlled by traffickers in a rival trafficking organization because 51% 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 to the United States. This simplification is discussed in more detail at the end of this section.

Each trafficker's objective is to minimize the costs of transporting drugs to U.S. points of entry, taking aggregate flows as given. Since the amount of drugs transported by a single agent is small relative to the total supply of drugs, this assumption appears reasonable and simplifies the analysis considerably. In equilibrium, the costs of all routes actually used to transport drugs from a given producing municipality to the U.S. are equal and less than those that would be experienced by reallocating traffic to an unused route. Formally, an equilibrium must satisfy the following conditions, first published by transportation analyst John Wardrop in 1952:

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

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

The equilibrium routing pattern satisfying these conditions is the Nash equilibrium of the game. Beckmann, McGuire, and Winsten (1956) proved that the equilibrium can be characterized by a straightforward optimization problem.[36] An equilibrium always exists, and if each c_e is strictly increasing, then the equilibrium is unique. Traffickers ignore the externalities that their use of a route imposes via congestion costs, and thus the equilibrium routing pattern will typically not be socially optimal.

While this game does not have a closed-form solution, for a given network, set of supplies, and specification of the congestion costs $c_e(\cdot)$ it can be solved using numerical methods. I use the Frank-Wolfe algorithm (1956), which generalizes Dantzig's

[36] Specifically, the routing pattern x^{WE} is an equilibrium if and only if it is a solution to:

$$\min \sum_{e \in E} \int_0^{x_e} c_e(z)dz \tag{2.6}$$

$$s.t. \quad \sum_{p \in \mathcal{P} | e \in p} x_p = x_e \quad \forall e \in E \tag{2.7}$$

$$\sum_{p \in \mathcal{P}_i} x_p = 1 \quad \forall i = 1, 2, \ldots \tag{2.8}$$

$$x_p \geq 0 \quad \forall p \in \mathcal{P} \tag{2.9}$$

well-known simplex algorithm to non-linear programming problems. Details are provided in the online estimation appendix, which describes the paper's estimation procedures.

Because the congestion costs are unknown, solving the problem requires specifying a functional form for the edge costs $c_e(l_e, x_e)$ and estimating the unknown parameters. I assume that the congestion costs on each edge take a Cobb-Douglas form, and explore the robustness of the model's predictions to several different specifications of these costs. In the most parsimonious version, border crossings impose a congestion cost equal to $\phi_t(flow_e/lanes)^\delta$ for terrestrial border crossings and $\phi_p(flow_e/containers)^\delta$ for ports, where $\{\phi_t, \phi_p, \delta\}$ are congestion parameters, *lanes* is the number of commercial lanes of a terrestrial crossing, and *containers* is the container capacity of a port. δ captures the shape of congestion costs, and congestion costs are scaled to the same units as physical distance costs by the parameters $\{\phi_t, \phi_p\}$. One might be concerned that this functional form is overly restrictive. While there is not enough variation in the data to estimate a separate congestion parameter for each of the 26 points of entry into the U.S., I do estimate a version of the model with six ϕ parameters: one for terrestrial points of entry in the bottom quartile of the size distribution (i.e. crossings with a single lane), three more for terrestrial points of entry in the other three quartiles (2 lanes, 3 to 9 lanes, and 10 to 17 lanes, respectively), one for ports with below median container capacity, and one for ports with above median container capacity.[37] This allows the model to more flexibly capture the relationship between congestion costs and the size of the U.S. points of entry. In the final version of the model, I estimate the seven congestion cost parameters for U.S. points of entry, as well as parameters for congestion costs on the

[37]Median container capacity is 160,000 TEUs, which is divided by 10,000 to be in units comparable to the size of the terrestrial crossings. TEU stands for "twenty-foot equivalent units."

interior edges. The congestion costs on the interior edges take the form: $l_e \phi_{int} flow_e^{\gamma}$, where l_e is the length of the edge, and ϕ_{int} and γ are congestion parameters whose interpretation is analogous to the ϕ and δ parameters on U.S. points of entry.[38]

The congestion parameters are estimated using the simulated method of moments (SMM) and cross-sectional data on the value of domestically produced illicit drugs confiscated during the beginning of the sample period, which lasts from December of 2006, when the confiscations data become available, until the first authorities elected during the sample period took office in July of 2007. Every choice of the model's parameters generates a set of moments that summarize the patterns of model-predicted confiscations, and I estimate the congestion parameters by matching these moments to their counterparts generated from data on the value of actual illicit drug confiscations. Specifically, let $\{x_m\}$ denote the flows predicted by the trafficking equilibrium problem, aggregated to the municipal level, and let $\theta_0 \in \mathbb{R}^P$ denote the vector of congestion parameters plus one scaling parameter κ that maps model-predicted flows to model-predicted confiscations: $\hat{conf}_m = \kappa x_m, \kappa \in (0, \infty)$.[39] Let $g(x_m, \theta_0) \in \mathbb{R}^L$ denote a vector of moment functions that specifies the difference between observed confiscations and those predicted by the model, given the congestion costs described by θ_0. The number of moment conditions must be greater than or equal to the number of parameters for the model to be identified. The SMM estimator $\hat{\theta}$ minimizes a

[38]When I instead specify the congestion costs on interior edges as $\phi_{int} flow_e^{\gamma}$ (i.e. congestion costs to traverse an edge are the same regardless of the length of the edge), the estimated routes and spillover patterns are broadly similar (results available upon request).

[39]κ likely varies with the local environment, but it is not possible to estimate this dependence. To the extent that the model is robustly predictive of the evolution of drug trade-related outcomes within municipalities over time, this suggests that the estimated parameters are reasonable despite the fact that confiscations are an imperfect proxy of actual illicit drug flows.

weighted quadratic form:

$$\theta = \underset{\theta \in \Theta}{\operatorname{argmin}} \frac{1}{M} \left[\sum_{m=1}^{M} \hat{g}(x_m, \theta) \right]' \Sigma \left[\sum_{m=1}^{M} \hat{g}(x_m, \theta) \right] \tag{2.10}$$

where $\hat{g}(\cdot)$ is an estimate of the true moment function, M is the number of municipalities in the sample, and Σ is an L x L positive semi-definite weighting matrix. If $\hat{\Lambda}$ is a consistent estimator of $\text{Var}[g(x_m, \theta_0)]$ and $\Sigma = \hat{\Lambda}^{-1}$, then the SMM estimator will be asymptotically efficient. The optimal SMM estimator can be obtained by first minimizing (2.10) using $\Sigma = I_L$, the identity matrix. From this first step, $\hat{\Lambda}$ can be calculated, and (2.10) can be re-optimized in a second step using $\Sigma = \hat{\Lambda}^{-1}$. Further details about the asymptotic properties of the SMM estimator can be found in Pakes and Pollard (1989) and McFadden (1989).

Predicted confiscations on a given edge are not independent of predicted confiscations elsewhere in the network, introducing spatial dependence. Following Conley (1999), I replace the asymptotic covariance matrix Λ with a weighted average of spatial autocovariance terms with zero weights for observations further than a certain distance. I allow for correlation between municipalities located within 250 km of each other. More details are provided in the online estimation appendix.

Depending on the specification, there are four to ten parameters in θ. The moments match the mean model predicted and observed confiscations at ports, at terrestrial bordering crossings, and on interior edges. They also match the interactions between port confiscations and the port's container capacity, between terrestrial crossing confiscations and the crossing's number of commercial lanes, between interior confiscations and the length of the interior edge, and between interior confiscations and the length of the detour required to circumvent the edge. For models that estimate six separate crossing congestion parameters, the moment conditions match predicted confiscations within a 100 kilometer radius of each U.S. point of entry to actual confiscations within a 100 kilometer radius. The estimates for the model with three congestion parameters are similar when these moments are included in the estimation, but I omit them in the baseline estimation because they are not necessary for identification and adding more moment conditions raises the risk of finite sample bias and related problems similar to those that arise with weak instruments in linear models (see Stock, Wright, and Yogo, 2002). Finally, the moment conditions match the model predicted and observed variance of confiscations across U.S. points of entry and across interior edges.

As is often the case with choice problems, the SMM objective function is not globally convex and hence minimizing it is non-trivial. Standard gradient methods may perform poorly, and thus I instead use simulated annealing, which is more suited to problems that lack a globally convex objective (Kirkpatrick, Gelatt, and Vecchi, 1983). Details about the estimation are provided in the online appendix.

Table 2.6 reports the simulated method of moments estimates of the cost function parameters. Conley standard errors are in brackets and robust standard errors are in parentheses. Column 1 reports estimates for the specification with parsimonious congestion costs on U.S. points of entry, column 2 reports estimates for the specification with more flexible congestion costs on U.S. points of entry, and column 3 reports estimates for the specification with congestion costs on both U.S. points of entry and interior edges of the road network.

All parameters are precisely estimated. δ, which captures the shape of the congestion costs on U.S. points of entry, ranges from 1.57 to 1.88, depending on the model specification. This implies that the costs of congestion increase as illicit drug traffic through a point of entry into the U.S. increases. This is an intuitive finding given that hiding places may become worse and authorities may direct more attention per unit of traffic towards a given point of entry as illicit flows through that point of entry increase. When the congestion costs on points of entry are allowed to take a more flexible form in column 2, the $\phi_t^{Q_i}$'s tend to increase somewhat with the size of the border crossing. In other words, the model estimates higher congestion costs on a ten lane border crossing edge with ten units of traffic than on a one lane edge with one unit of traffic. The model in column 3, which includes congestion costs on interior edges, estimates $\gamma = 0.11$. This implies that congestion costs on interior edges are concave. In equilibrium, the total costs imposed by congestion on U.S. points of entry in this specification are about 39 times larger than the total costs imposed by congestion on interior edges, suggesting that congestion at U.S. points of entry is more important than congestion within Mexico. This is not surprising, given the greater law enforcement presence at U.S. points of entry and the bottlenecks that they impose. All three models estimate that total congestion costs are nearly as large as total distance costs. Predicted routes for the pre-period, estimated using the parameters in column 1 of Table 2.6, are show in Figure 2-2. The routing patterns predicted by the other congestion cost specifications are similar.

The model parameters are estimated to match the cross-section of pre-period confiscation values, and the model is highly predictive of these confiscations. In a linear regression of actual municipal-level confiscations on model predicted confiscations, all three specifications have a t-statistic of between 7 and 8. The squared correlation coefficient is between 0.02 and 0.03. As would be expected, as more parameters are added to the model, the correlation coefficient increases somewhat. The model is also highly predictive of the extensive margin of confiscations. When comparing the actual confiscations indicator variable to the predicted confiscations indicator variable, the Pearson chi-squared statistic has a value of 100 for the 4 parameter model, 116 for the 8 parameter model, and 76 for the 10 parameter model, all of which have p-values near zero.

I now turn to an examination of whether the trafficking model, fitted on data from the beginning of the sample period, predicts the diversion of trafficking routes in response to close PAN victories occurring later in the sample period. The empirical approach is the same as that used for the shortest paths trafficking model. Due to space constraints, in the main text I report results for the routes measure calculated using the parameters in Column 1 of Table 2.6, as variation in these routes is the most highly correlated with variation in actual illicit drug confiscations. Tables A4, A5, and A6 in the online appendix repeat all analyses from the main text using the parameters in columns (2) and (3) of Table 2.6, documenting that the estimated spillovers in drug traffic, violence, and economic outcomes are broadly similar regardless of the congestion parameters used to predict trafficking routes.

Panel B of Table 2.5 examines the relationship between actual illicit drug confiscations and routes predicted using the model with congestion. As in the analysis with the shortest distance routes, I focus on the extensive margin of trafficking. In column (1), the dependent variable is a dummy equal to one if domestically produced illicit drugs are confiscated in a given municipality-month and equal to zero otherwise. When a municipality acquires a predicted trafficking route, drug confiscations increase by around 1.5 percentage points, and this correlation is statistically significant at the 1% level. In column (2), the dependent variable is equal to the log of the value (in US dollars) of domestic illicit drug confiscations in the municipality-month if confiscations are positive and equal to zero otherwise. Acquiring a predicted trafficking route is associated with an increase in the value of confiscated drugs of around 19.5 percent. Columns (3) and (4) document that these results are robust to excluding municipalities that border a municipality that has experienced a close PAN victory. Columns (5) and (6) show that the model loses its predictive power when the wrong shocks are used (that is, when I assume that PAN losses instead of PAN victories increase trafficking costs). Next, columns (7) and (8) document that the correlations between predicted domestic trafficking routes and the presence or value of cocaine confiscations are small and statistically insignificant. Finally, Figure 2-5 shows that the results are robust to varying the costs imposed by a close PAN victory. When close PAN victories are implausibly assumed to reduce the costs of trafficking drugs through a municipality, the model loses its predictive power. As shown in Table A4 in the online appendix, the above results are robust to using the other specifications of the congestion costs reported in Table 2.6. In the online appendix, I also repeat the analysis in Table 2.5 using the number of predicted routes instead of the indicator routes measure. While the value of confiscations increases for each additional trafficking route acquired, the relationship is not statistically significant at conventional levels.

Overall, while congestion does change the predicted routes somewhat, the model with congestion costs offers at best modest improvements in predictive power over the shortest paths model. The partial correlation coefficient between the routes dummy calculated using the model with congestion and the value of confiscations is 0.018, as compared to 0.014 for the shortest paths model. I now turn to an examination of whether PAN crackdowns exert violence and economic spillovers.

2.5.3 Violence and economic spillovers

The results in Table 2.5 indicate that the trafficking model predicts the diversion of drug traffic following close PAN victories. Now, I use the model's predictions about the diversion of drug traffic to locate violence and economic spillover effects. Ideally, I would use predicted trafficking routes as an instrument for actual trafficking routes, but actual trafficking routes are unobserved. Thus, I focus on testing whether there is a reduced form relationship between predicted drug trafficking presence and violence or economic outcomes.

First I explore violence spillovers. Table 2.7 reports estimates from equation (2.5), with violence measures used as the dependent variable. Panel A uses the shortest paths routes, and Panel B uses the routes measure from the model with congestion. Column (1) of Panel A shows that the presence of a predicted trafficking route increases the probability that a drug trade-related homicide occurs in a given month by 1.3 percentage points (s.e.=0.5). This can be compared to the average sample probability of a drug trade-related homicide, which is equal to 4.4 percent. The estimated effect of 1.5 percentage points in Panel B is similar. When municipalities bordering a municipality that has experienced a close PAN victory are excluded from the sample, the effect estimated in Panel B increases slightly, to 1.8 percentage points, and remains statistically significant. In contrast, the effect in Panel A becomes smaller and statistically insignificant. This suggests that the model with congestion costs may be better at predicting violence spillovers that are not in adjacent municipalities.

Congestion costs may partially reflect a higher probability of violent clashes when traffickers' routes coincide. I examine whether there is evidence for this in column (2), by regressing the drug trade-related homicide dummy on an indicator equal to one if a municipality contains one predicted trafficking route in the relevant month and a separate indicator equal to one if it contains more than one predicted trafficking route. No trafficking routes is the omitted category. The coefficients reported in Panel B, which uses the model with congestion, provide suggestive evidence that traffickers are more likely to engage in violent conflicts when their routes coincide. The coefficient on the more than one route dummy is equal to 0.019 (s.e.= 0.007), whereas the coefficient on the single route dummy is equal to 0.008 (s.e.= 0.006).

However, when the shortest path routes are used, the spillover effects on violence are estimated to be similar regardless of whether there is one or multiple routes in a municipality, indicating that the evidence in Panel B should be interpreted cautiously.

In columns (3) and (4), I report analogous specifications using monthly drug trade-related homicides per 10,000 municipal inhabitants as the dependent variable. The pattern of results is qualitatively similar to that in columns (1) and (2). However, the correlation between the routes dummy predicted using the model with congestion and the homicide rate, while positive and large relative to the sample mean, is not statistically significant.

Column (7) documents that the presence of a predicted trafficking route has no effect on the non-drug trade-related homicide rate. Finally, columns (8) and (9) report results from the placebo check in which close PAN losses rather than close PAN victories increase the costs of trafficking drugs. As expected, there is no longer a relationship between predicted routes and the drug trade-related homicide probability or rate.

Next, I turn to economic spillovers. I use quarterly data from the National Survey of Occupation and Employment (*Encuesta Nacional de Ocupación y Empleo*), collected by the National Institute of Statistics and Geography (INEGI), to construct measures of labor market outcomes for 2007 through 2009. These data are available for a sample of municipalities.[40] Table 2.8 reports the results from regressing male and female labor force participation and formal and informal sector wages of prime age males on the predicted trafficking routes dummy, state x quarter fixed effects,

[40] I have also examined the direct effects of close PAN victories on economic outcomes. Because the economic data are from a sample, municipalities near the PAN win-loss threshold with economic data are limited in number, and the direct economic effects (available upon request) are imprecisely estimated.

and municipality fixed effects.[41] Panel A predicts routes using the shortest paths model, and Panel B uses the model with congestion costs.

Overall, the economic spillover effects tend to be imprecisely estimated and thus should be interpreted cautiously. Nevertheless, they provide suggestive evidence - consistent with the qualitative evidence discussed in Section 2 - that drug trafficking organizations engage in widespread extortion, particularly of poorer citizens. The correlations between the routes dummy and male labor force participation (column 1) and formal sector wages (column 3) are statistically insignificant and tend to be relatively small. In contrast, columns (2) and (4) of Panel B document that the presence of a predicted trafficking route lowers informal sector wages by around 2.3 percent (s.e.= 1.3) and lowers female labor force participation by 1.26 percentage points (s.e.= 0.57), relative to an average female participation rate of 51 percent. The coefficients on the routes dummy in Panel A, which uses the shortest paths model, are also large but are noisily estimated. Columns (5) and (6) show that the estimates in both panels are similar when municipalities bordering a municipality that has experienced a close PAN victory are excluded from the sample. Finally, columns (7) and (8) use the placebo model in which close PAN losses increase the costs of trafficking drugs through a municipality. The correlations between the routes dummy and both female labor force participation and informal sector wages, while imprecisely estimated, are smaller than the correlations shown in columns (2) and (4) and are statistically insignificant. While the economic data do not provide as much power as the confiscations and violence data, overall the evidence suggests that drug trafficking presence exerts economic effects on the general population.

[41] The analysis of wages is limited to prime age males because they tend to participate inelastically in the labor force, reducing concerns about selection bias in the wage regressions.

2.5.4 Possible Extensions

Both versions of the trafficking model predict the diversion of drug traffic following close PAN victories. With access to additional data, it would be straightforward to further enrich the analysis. For example, I do not examine cocaine trafficking because of the absence of reliable data on cocaine points of entry into Mexico. Once cocaine enters Mexico, however, terrestrial trafficking is similar to that of other domestically produced drugs. Thus, with reliable data on cocaine points of entry, a similar approach could be applied to this drug.

With better data, the trafficking model could also explicitly incorporate territorial ownership by drug trafficking organizations. 51 percent of producing municipalities were controlled by a local drug gang in the period examined in this study, and we do not know which trafficking organizations, if any, these gangs allied with to transport their drugs to the United States. Thus, I am unable to incorporate territorial ownership into the model. I do, however, explore the importance of territorial ownership by conducting the following exercise. I begin by limiting the sample to edges emanating from nodes that meet the following criteria: 1) the node is traversed in the equilibrium with congestion costs by a trafficking route whose origin is controlled by a major DTO, 2) the node forks into at least one edge which is controlled by a DTO that controls the origin of a route traversing that node, and 3) the node also forks into at least one edge that is not controlled by the above-mentioned DTO. This sample includes 633 edges (217 nodes), as compared to 17,453 edges (13,969 nodes) in the full network. I find that the trafficking model on average under-predicts flows on edges described by the second criterion and over-predicts flows on edges described by the third criterion. This suggests that territorial ownership is relevant. The network model likely predicts drug traffic without incorporating territorial ownership because territorial ownership has responded endogenously to network characteristics. If additional information were to become available, this result suggests that the model's predictions could be further improved by incorporating territorial ownership.

The network approach developed in this study could also be extended to other contexts in which combating drug trafficking is a policy priority. This includes areas such as Afghanistan, Myanmar, Thailand, Vietnam, Laos, certain provinces in China, Colombia, Peru, and Bolivia, which are major centers of heroin and cocaine production and trafficking. Given that these regions have a much sparser road network than Mexico, presumably a network analysis of terrestrial drug trafficking would be more straightforward.

2.6 Policy Implications

As we have seen, Mexico is currently facing an important and difficult challenge. Mexican traffickers earn tens of billions of dollars each year in profits and have often used these proceeds to corrupt Mexican institutions and threaten public security, with potentially quite detrimental consequences for Mexican citizens. At the same time, attempts to combat drug trafficking have had unintended consequences, leading to large increases in violence.

Reducing the profits of organized criminal groups is a central objective of Mexican drug policy, as it reduces the resources that traffickers have to corrupt government officials and to threaten public security (Shirk, 2011). It may (or may not) be that policies in consumer countries, such as the legalization of marijuana or more concerted efforts to reduce illicit drug consumption by individuals already under the supervision of the criminal justice system, would reduce profits of drug traffickers more than well-coordinated interdiction efforts in Mexico. Alternatively, some have called on Mexico to abandon interdiction altogether. However, legalization in the U.S. is unlikely to be politically feasible in the immediate future, and ending interdiction is also improbable given the threats that traffickers pose through their varied criminal activities and influences on public institutions. Thus, there is likely to be a continued emphasis on how interdiction policies in Mexico can be improved.

This section uses the trafficking model and spillover patterns to examine how scarce law enforcement resources can be allocated to increase trafficking costs by as much as possible. Of course policies that increase trafficking costs may have the unintended consequence of increasing violence, and thus this section also discusses how the violent response to this policy may be reduced.

There are several reasons why explicitly considering the network structure of trafficking is relevant to the allocation of law enforcement resources. Most obviously, allocating a police checkpoint or other resources to a road with extensive drug traffic will not necessarily increase trafficking costs substantially, since there could be a cheap detour. Moreover, increasing the cost of traversing an edge could actually decrease total trafficking costs because of the externalities from congestion. This result, known as Braess's paradox after mathematician Dietrich Braess, has been documented in a number of real world traffic congestion examples.[42] It occurs for around fifteen percent of edges in the trafficking equilibrium predicted by this study's analysis. Finally, the effects of law enforcement in different locations will be interconnected through the network structure of trafficking. The network model, combined with a simple algorithm for the allocation of law enforcement resources, incorporates all three of these phenomena.

[42]For examples of Braess's paradox in traffic congestion in Seoul, New York, Berlin, Boston, and London, see Youn, Gastner, and Jeong (2008); Easley and Kleinberg (2008, p. 71); and Knodel (1969, p. 57-59).

I specify the government's resource allocation problem as follows: the government's objective is to maximize the total costs that traffickers incur by allocating resources, such as police checkpoints, to edges in the road network. This policy increases the cost of trafficking drugs on each of the selected edges, which are referred to as *vital edges*, by a pre-specified amount p_e. Traffickers respond by selecting the cheapest routes given the new set of edge costs, accounting for changes in congestion. The government faces a resource constraint and must pay a cost r_e to send a unit of law enforcement to each edge e.

With data about the types of law enforcement resources employed in PAN crackdowns, one could use the network model to estimate p_e, and r_e could be calculated from administrative data. However, these data cannot be released to researchers, and conducting a comprehensive analysis of the deployment of Mexican military and law enforcement resources is beyond the scope of this study in any case.[43] Instead, I provide the following illustrative example of how the network approach can contribute to the design of law enforcement policies. Suppose that the government's budget allows it to allocate a pre-specified number of police checkpoints. I estimate a reasonable value for p_e, the amount that checkpoints increase the effective length of an edge, by using the impacts of close PAN victories as a benchmark. The network model best predicts the diversion of drug traffic following PAN victories when I assume that they increase trafficking costs by a factor of three. Thus, I assume that each police checkpoint increases the effective length of selected edges by $3 \times 9 = 27$ kilometers, where 9 kilometers is the average edge length in the network.[44]

[43]With the appropriate data, it would be straightforward to extend the analysis to multiple types of law enforcement resources (Israeli and Wood, 2002).

[44]An alternative assumption is that police checkpoints multiply the effective length of edges by a given factor. However, this would imply that checkpoints increase the costs of longer edges by more than they increase the costs of shorter edges. The multiplicative costs assumption appears reasonable for PAN crackdowns, as larger municipalities have more police and are likely to receive

The government's resource allocation problem is a Stackelberg network game with a large number of Nash followers (Baş and Srikant, 2002; Stackelberg, 1952). In the first stage, the government allocates police checkpoints to k vital edges. In the second stage, traffickers respond by simultaneously selecting trafficking routes. The scenario in which traffickers respond to the government's action by choosing the shortest path to the U.S. is a special case in which congestion costs are zero. Ball, Golden, and Vohra (1989) showed that this special case is NP hard, and thus it follows that the more general problem is also NP-hard. That is, the time required to solve for the optimum increases quickly as the size of the problem grows. Even if we focused on the simpler model with no congestion costs, solving for the optimum using an exhaustive search would have an order of complexity of $O(V!)$, where V (the number of vertices) equals 13,969, and thus would take trillions of years to run. Intuitively, the problem is challenging because allocating police resources to two edges at the same time might increase the objective function more than the summation of changes in the objective function when resources are allocated to each edge separately, and hence the order in which a solution algorithm proceeds may matter.

Developing algorithms for problems similar to the one described here is an active area of operations research and computer science. For example, researchers have examined the problem of identifying vital edges in critical infrastructure networks, such as oil pipelines and electricity grids, so that these edges can be better defended against terrorist attacks and the systems made more robust (see, for example, Brown, Carlyle, Salmerón and Wood, 2005). To the best of my knowledge there are currently no known algorithms for solving the government's resource allocation problem that are both exact (guaranteed to converge to optimality) and feasible given the size of

larger federal police and military contingents, but the assumption appears less appropriate for police checkpoints.

the network, either for the network with congestion or for the simpler problem in which congestion costs are zero.[45] Developing a fast, exact algorithm for this problem is a challenging endeavor that is significantly beyond the scope of the current study. Thus, I instead use the following approximate heuristic to solve for the k vital edges:

1. For each of k iterations, calculate how total trafficking costs respond to individually increasing the edge lengths of each of the N most trafficked edges in the network.
2. Assign each element of this set of N edges a rank, $m = 1 \dots N$, such that the removal of edge $m = 1$ would increase trafficking costs the most, the removal of edge $m = 2$ would increase trafficking costs the second most ... and the removal of edge $m = N$ would increase trafficking costs the least.
3. Increase the effective length of the edge with $m = 1$ by a pre-specified amount.
4. Terminate if k iterations have been completed and return to step 1 otherwise.

While this algorithm does not guarantee convergence to the optimum, encouragingly it offers a solution that is robust to varying the details of the algorithm. Figure 2-6 plots the results of this exercise with $k = 25$ and $N = 250$, highlighting municipalities that contain a vital edge in red. The average monthly drug trade-related homicide rate between 2007 and 2009 is plotted in the background. Allocating police checkpoints to these 25 edges increases the total length of the network by 0.0043

[45]Malik, Mittal, and Gupta (1989) suggest an algorithm for finding k vital edges in the shortest path problem, but unfortunately it is theoretically flawed (see Israeli and Wood (2002) for a discussion). The most closely related work is by Israeli and Wood (2002), who develop an efficient algorithm for solving for k vital edges in the context of a shortest path problem on a directed graph with a single origin and destination. Even if the algorithm, which involves considerable mathematical machinery, could be extended to this paper's undirected graph with multiple origins, it is unlikely to be feasible on a network of the size examined here and does not accommodate congestion costs. Existing vital edge algorithms focus on shortest path or max flow problems (i.e. Lim and Smith, 2007) , and to the best of my knowledge researchers have not examined the vital edge problem in a congested network.

percent and increases total trafficking costs by 17 percent. Results are similar when I instead: a) choose values of N ranging from 100 to 500, b) alternate in step 3 between selecting the edges with $m = 1$ and $m = 2$, c) alternate in step 3 between selecting the edges with $m = 1$, $m = 2$, and $m = 3$, and d) remove the edge with $m = 2$, $m = 3$, $m = 4$, or $m = 5$ when $k = 1$ and remove the edge with $m = 1$ when $k = 2 \ldots 25$.

One concern with targeting law enforcement resources towards vital edges is that this may increase violence substantially. To the extent that the government aims to both increase trafficking costs and minimize the probability of a large violence response, vital edges can be distinguished based on a crackdown's predicted direct and spillover effects on violence. Moreover, if the government credibly signals a permanent law enforcement presence on vital edges, this could plausibly reduce the violent response since the returns to controlling those edges would be permanently reduced. Hence the expected returns to traffickers of fighting over the edge would be lower.

A potential alternative strategy that does not require a network analysis would be for the government to place checkpoints in municipalities with the most violence. These municipalities may contain a disproportionate number of vital edges if the control of vital edges is contested. Figure 2-6 shows that while some vital edges are in high violence municipalities, others are not. By incorporating spillovers and well-defined predictions about traffickers' behavior, the network model provides unique information. A frequent critique of the Mexican government's policy towards drug trafficking is that it has tended to indiscriminately target drug traffickers, rather than focusing resources on a more systematic and theoretically informed approach (Guerrero, 2011; Kleiman, 2011). While we would not expect there to be any easy or ideal solutions to the challenges facing Mexico, the network approach to trafficking provides unique information with the potential to contribute to a more nuanced and economically informed law enforcement policy.

2.7 Conclusion

This study examines the direct and spillover effects of Mexican policy towards the drug trade, developing three sets of results. First, regression discontinuity estimates show that drug trade-related violence in a municipality increases substantially after the close election of a PAN mayor. 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. Second, an economic model of equilibrium trafficking routes predicts the diversion of drug traffic following close PAN victories. When drug traffic is diverted to other municipalities, violence in these municipalities increases. Moreover, there is some evidence that female labor force participation declines and informal sector wages fall. These results corroborate qualitative evidence that traffickers extort informal sector producers. Finally, I show that the network approach can serve as a tool for the efficient allocation of law enforcement resources.

These results demonstrate how traffickers' economic objectives and constraints imposed by the routes network affect the policy outcomes of the Mexican Drug War. While there are unlikely to be any ideal solutions to the challenges posed by the drug trade, the results nevertheless suggest that developing a more detailed understanding of how governments and organized criminals interact in networks could improve the allocation of scarce public resources, in Mexico and a number of other contexts. In addition to helping governments address the immediate challenges of reducing the profits and other operational objectives of organized criminals, a network-informed approach may aid in pursuing longer-term policy goals. In making the case for why combating drug trafficking is important for Mexico's long-term interests, President Felipe Calderón argued that organized criminals had infiltrated public institutions to such an extent that challenging these groups was necessary for protecting national security and preventing institutional deterioration. This study has focused on the shorter-term consequences of the Mexican Drug War because at the time of writing, any longer term impacts on institutional quality and security had yet to be realized. Examining the conditions under which crackdowns on organized crime lead to long-term changes in these outcomes is a particularly central question for future research.

Table 2.1: Pre-characteristics

	(1)	(2)	(3)	(4)	(5)
	5% vote spread		t-stat on		t-stat on
	PAN lost	PAN won	means difference	RD estimate	RD estimate
Crime characteristics					
Monthly drug-trade related homicides (Dec. '06 - Jun. '07)	0.113	0.176	(0.41)	0.07	(0.36)
Monthly drug-trade related homicide dummy (Dec. '06 - Jun. '07)	0.042	0.031	(-0.51)	-0.01	(-0.46)
Monthly police-criminal confrontation deaths (Dec. '06 - Jun. '07)	0.146	0.086	(-0.57)	-0.20	(-0.75)
Monthly confrontation deaths dummy (Dec. '06 - Jun. '07)	0.043	0.043	(-0.01)	-0.02	(-0.66)
Annual homicide rate per 100,000 inhab. (1990-2006)	15.01	13.68	(-0.46)	-0.19	(-0.63)
Municipal political characteristics					
Mun. taxes per capita (2005)	56.75	59.84	(0.22)	13.9	(0.73)
Mun. taxes per $ income (2005)	0.001	0.001	(0.05)	0.0001	(0.95)
PAN incumbent	0.366	0.371	(0.07)	-0.02	(-0.37)
PRD incumbent	0.037	0.057	(0.59)	0.01	(0.16)
% alternations (1976-2006)	0.418	0.400	(-0.34)	0.006	(0.30)
PRI never lost (1976-2006)	0.073	0.071	(-0.97)	-0.03	(-0.39)
Demographic characteristics					
Population (2005)	5099	6026	(0.35)	336	(0.84)
Population density (2005)	191.1	220.2	(0.41)	-118	(-0.80)
Migrants per capita (2005)	0.018	0.016	(-0.69)	-0.0007	(-0.35)
Economic characteristics					
Income per capita (2005)	4483	4285	(-0.53)	0.02	(0.03)
Mun. Gini (2005)	0.421	0.410	(-1.47)	-0.005	(-0.57)
Malnutrition (2005)	31.20	32.76	(0.52)	-0.37	(-0.01)
Mean years schooling (2005)	6.18	6.26	(0.32)	0.06	(0.21)
Infant mortality (2005)	22.26	22.54	(0.22)	0.28	(0.18)
HH w/o access to sewage (2005)	8.436	8.505	(0.05)	2.01	(0.71)
HH w/o access to water (2005)	18.22	16.14	(-0.62)	-0.81	(-0.20)
Marginality index (2005)	-0.119	-0.154	(-0.23)	-0.05	(-0.26)
Road network characteristics					
Average detour length (km)	157	220	(0.46)	23.6	(0.47)
Total roads (km)	66.4	111.5	(1.28)	54.1	(1.34)
Road density (km/km^2)	0.129	0.156	(1.23)	0.003	(0.11)
Distance U.S. (km)	732.3	700.8	(-0.62)	-9.91	(-0.10)
Geographic characteristics					
Elevation (m)	1380	1406	(0.19)	-45.3	(-0.31)
Slope (degrees)	3.25	3.62	(0.89)	0.07	(0.12)
Surface area (km^2)	739	1834	(1.34)	1249	(1.44)
Average min. temperature, C (1950-2000)	12.2	11.8	(-0.57)	-0.04	(-0.04)
Average max. temperature, C (1950-2000)	26.6	26.4	(-0.45)	-0.10	(-0.14)
Average precipitation, cm (1950-2000)	105	115	(0.82)	5.9	(0.46)
Observations	**82**	**70**			

Notes: Data on population, population density, mean years of schooling, and migrants per capita are from *II Conteo 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 U.S. and other road network characteristics are from the author's own calculations, using GIS software. Electoral data are from Mexico Electoral -Banamex and electoral results published by the Electoral Tribunals of each state. The geographic characteristics are from Acemoglu and Dell (2009). Data on homicides (1990-2006) are from INEGI and data on drug trade-related violence are from confidential sources. Column (3) reports the t-statistic on the difference in means between municipalities where the PAN barely won and where they barely lost. Column (4) reports the coefficient on PAN win from equation (2.2) when the respective characteristic is used as the dependent variable, and column (5) reports the t-statistic on PAN win. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 2.2: Close PAN Elections and Violence

	Post inaug.	Lame duck	Pre election	No FE	No FE or controls	Linear	Quadratic		Cubic		Quartic	
						RD polynomial						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Panel A: Probability of drug trade-related homicides												
PAN win						0.019		0.007		0.050		0.046
× lame duck						(0.058)		(0.059)		(0.088)		(0.090)
PAN win						0.147***		0.132***		0.204***		0.204***
× post-inaug.						(0.051)		(0.047)		(0.064)		(0.062)
PAN win	0.084***	0.005	0.014	0.093***	0.093**		0.127***		0.149***		0.179***	
	(0.027)	(0.030)	(0.013)	(0.026)	(0.043)		(0.036)		(0.046)		(0.060)	
R-squared	0.648	0.686	0.868	0.576	0.024	0.237	0.652	0.240	0.653	0.244	0.655	0.244
Clusters						152		152		152		152
Observations	430	430	430	430	430	1,960	430	1,960	430	1,960	430	1,960
Panel B: Drug trade-related homicide rate												
PAN win						0.026		0.018		0.068*		0.068*
× lame duck						(0.025)		(0.025)		(0.038)		(0.038)
PAN win						0.089**		0.088**		0.107**		0.103**
× post-inaug.						(0.038)		(0.038)		(0.041)		(0.040)
PAN win	0.046**	0.007	0.005	0.044**	0.047**		0.066**		0.096***		0.090**	
	(0.020)	(0.023)	(0.005)	(0.020)	(0.023)		(0.029)		(0.037)		(0.040)	
R-squared	0.370	0.250	0.643	0.246	0.021	0.219	0.374	0.220	0.380	0.222	0.386	0.223
Clusters						152		152		152		152
Observations	430	430	430	430	430	1,960	430	1,960	430	1,960	430	1,960

Notes: In columns (1), (4), (5), (7), (9), and (11) the dependent variable is the average monthly homicide probability (Panel A) or rate (Panel B) in the post-inauguration period; in column (2) it is average homicides in the lame duck period, and in column (3) it is average homicides in the pre-election period. In columns (6), (8), (10), and (12), it is the homicide dummy (rate) in a given municipality-month. PAN win is a dummy equal to one if a PAN candidate won the election, lame duck is a dummy equal to one if the observation occurred between the election and inauguration of a new mayor, and post-inaug. is a dummy equal to one if the observation occurred after the inauguration of a new mayor. Columns (6), (8), (10), and (12) include a lame duck main effect, a post-inauguration main effect, month x state and municipality fixed effects, and interactions between the RD polynomial listed in the column headings and the lame duck and post-inauguration dummies. These columns limit the sample to municipalities with a vote spread of five percentage points or less. Columns (1) through (3), (7), (9), and (11) include state fixed effects and controls for baseline characteristics, estimated separately on either side of the PAN win-loss threshold. The coefficients in columns (1) through (5), (7), (9), and (11) are estimated using local regression, with separated trends in vote spread estimated on either side of the PAN win-loss threshold. Robust standard errors, clustered by municipality in columns (6), (8), (10), and (12), are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 2.3: Local Politics in More Detail

	(1)	(2)	(3)	(4)	(5)	(6)
	Elections involving PAN			Alternative samples		
		PAN	PRI	PRI v.	Any	All
	Baseline	Incumbent	Opponent	PRD	alternation	muns.
PAN win	0.084***	0.067**	0.070**			0.012
	(0.027)	(0.028)	(0.029)			(0.010)
PAN win		0.055				
× PAN Incumbent		(0.040)				
PAN win			0.021			
× PRI opponent			(0.032)			
PRI win				0.049		
				(0.035)		
Alternate					0.013	
					(0.018)	
R-squared	0.648	0.648	0.648	0.571	0.554	0.670
Observations	430	430	430	259	780	614
PAN incumbent effect		0.122***				
		(0.040)				
PRI opponent effect			0.090***			
			(0.030)			

Notes: The dependent variable in all columns is the average probability that a drug trade-related homicide occurred in a given municipality-month during the post-inauguration period. PAN win is a dummy equal to one if a PAN candidate won the election, PRI win is a dummy equal to one if a PRI candidate won the election, Alternate is a dummy equal to one for any alternation of the political party controlling the mayorship, Pan Incumbent is a dummy equal to 1 if the municipality had a PAN incumbent, and PRI opponent is a dummy equal to one if the PAN candidate faced a PRI opponent. Columns (1) through (5) are estimated using local regression, with separate trends in vote spread estimated on either side of the PAN win-loss threshold. All columns included state fixed effects, as well as baseline controls estimated separately on either side of the PAN win-loss threshold. Column (2) also includes interactions between the vote spread terms and the PAN incumbent dummy, and Column (3) includes interactions between the vote spread terms and the PRI opponent dummy. Columns (1) through (3) limit the sample to municipalities where a PAN candidate was the winner or runner-up, Column (4) limits the sample to municipalities with a close election between PRI and PRD candidates, and Column (5) includes all municipalities with a close election, regardless of the political parties involved. Column (6) includes all elections where a PAN candidate was the winner or runner-up. Robust standard errors are in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 2.4: Trafficking Industrial Organization and Violence

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Dependent variable is drug trade-related homicides (probability or indicator)											
PAN win	0.084***		0.020		0.037		0.087***		0.132***		0.151***	
	(0.027)		(0.026)		(0.031)		(0.025)		(0.035)		(0.045)	
PAN win × borders rival			0.513***		0.268							
			(0.180)		(0.233)							
PAN win × borders allies			0.126*		0.172*							
			(0.071)		(0.095)							
PAN win × local gang			0.012		0.010							
			(0.032)		(0.045)							
PAN win × detour							0.070***		0.070***		0.070***	
							(0.022)		(0.021)		(0.022)	
PAN win × post		0.147***		0.123**		0.107**		0.134***		0.122***		0.169***
		(0.051)		(0.051)		(0.047)		(0.041)		(0.038)		(0.051)
PAN win × post × borders rival				0.401***		0.411***						
				(0.116)		(0.111)						
PAN win × post × borders allies				0.047		0.049						
				(0.102)		(0.098)						
PAN win × post × local gang				0.009		0.019						
				(0.024)		(0.026)						
PAN win × post × detour								0.150***		0.148***		0.144***
								(0.024)		(0.022)		(0.021)
Vote spread terms	linear	linear	linear	linear	quad.	quad.	linear	linear	quad.	quad.	cubic	cubic
R-squared	0.648	0.247	0.633	0.271	0.646	0.274	0.671	0.279	0.675	0.282	0.676	0.283
Clusters		152		152		152		152		152		152
Observations	430	1,672	430	1,672	430	1,672	430	1,672	430	1,672	430	1,672
Borders rival effect			0.533***	0.524***	0.306	0.518***						
			(0.174)	(0.131)	(0.226)	(0.122)						
Borders allies effect			0.146**	0.169*	0.209**	0.157*						
			(0.069)	(0.092)	(0.090)	(0.091)						
Local gang effect			0.032	0.132**	0.047	0.127**						
			(0.028)	(0.052)	(0.040)	(0.051)						

Notes: In columns (1), (3), (5), (7), (9), and (11), the dependent variable is the average probability that a drug trade-related homicide occurred in a given municipality-month during the post-inauguration period. In columns (2), (4), (6), (8), (10), and (12), the dependent variable is a dummy variable equal to one if a drug trade-related homicide occurred in a given municipality-month. Borders rival is a dummy equal to one if the municipality is controlled by a major DTO and borders territory controlled by a rival DTO, borders allies is a dummy equal to one if the municipality is controlled by a major DTO and does not border territory controlled by a rival, and local gang is a dummy equal to one if the municipality is controlled by a local drug gang. No known drug trade presence is the omitted category. Detour is the standardized increase in total trafficking costs when the municipality's roads are removed from the trafficking network. Post is a dummy equal to one if the observation occurs during the post-inauguration period. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 2.5: The Diversion of Drug Traffic

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full Sample		Limited Sample		Placebo Paths		Full Sample	
	Domestic illicit drug confiscations						Cocaine confiscations	
	Dummy	Value	Dummy	Value	Dummy	Value	Dummy	Value
Panel A: Shortest Paths								
Predicted	0.016***	0.170***	0.015**	0.162**	0.004	0.038	0.004	0.027
routes dummy	(0.005)	(0.050)	(0.006)	(0.063)	(0.004)	(0.038)	(0.004)	(0.020)
Panel B: Model with Congestion Costs								
Predicted	0.015***	0.178***	0.013**	0.159**	0.006	0.036	-0.002	0.014
routes dummy	(0.005)	(0.059)	(0.006)	(0.064)	(0.006)	(0.069)	(0.004)	(0.023)
State x month FE	yes	yes	yes	yes	yes	yes	yes	yes
Municipality FE	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.42	0.47	0.42	0.47	0.42	0.47	0.37	0.37
Municipalities	1869	1869	1574	1574	1869	1869	1869	1869
Observations	69,153	69,153	58,238	58,238	69,153	69,153	69,153	69,153
Mean dep. var.	0.053	0.589	0.055	0.613	0.053	0.589	0.046	0.163

Notes: The dependent variable in columns (1), (3), and (5) is a dummy equal to 1 if domestic illicit drug confiscations are made in a given municipality-month; the dependent variable in columns (2), (4), and (6) is the log value of domestic illicit drug confiscations (or 0 if no confiscations are made); the dependent variable in column (7) is a dummy equal to 1 if cocaine confiscations are made in a given municipality-month; and the dependent variable in column (8) is the log value of confiscated cocaine (or 0 if no confiscations are made). Columns (5) and (6) use the placebo network, as described in the text. 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 x state and municipality fixed effects. Standard errors clustered by municipality and month x state are reported in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 2.6: Trafficking Model Parameter Estimates

	(1)	(2)	(3)
	Crossing Costs		Full
	parsimonious model	flexible model	congestion costs
ϕ_t	62.34***		
	[2.72]		
	(1.41)		
ϕ_p	36.48***		
	[2.07]		
	(1.40)		
ϕ_t^{Q1}		3.24***	13.00***
		[0.30]	[1.27]
		(0.25)	(1.19)
ϕ_t^{Q2}		13.19***	9.29***
		[2.14]	[0.34]
		(1.89)	(0.33)
ϕ_t^{Q3}		13.86***	21.26***
		[4.37]	[0.54]
		(4.08)	(0.52)
ϕ_t^{Q4}		18.81***	20.22***
		[0.86]	[0.62]
		(0.83)	(0.57)
ϕ_p^{small}		64.47***	70.990***
		[9.76]	[1.29]
		(9.16)	(1.28)
ϕ_p^{large}		55.34***	43.50**
		[8.43]	[21.73]
		(7.46)	(17.03)
ϕ_{int}			0.015***
			[0.004]
			(0.003)
δ	1.88***	1.57***	1.86***
	[0.05]	[0.15]	[0.17]
	(0.04)	(0.12)	(0.16)
γ			0.11**
			[0.06]
			(0.05)
κ	0.763***	0.91***	0.79***
	[0.07]	[0.08]	[0.07]
	(0.06)	(0.07)	(0.06)

Notes: Column 1 reports the simulated method of moments parameter estimates for the model with parsimonious congestion costs on U.S. points of entry, Column 2 reports the parameter estimates for the model with flexible congestion costs on U.S. points of entry, and Column 3 reports the parameter estimates for the model with congestion costs on both U.S. points of entry and interior edges. Conley (1999) standard errors are in brackets, and robust standard errors are in parentheses.

Table 2.7: Violence Spillovers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Full Sample		Full Sample Drug-related homicide		Limited Sample		Full Sample Non-drug homicide	Placebo Paths Drug-related homicide	
	dummy	dummy	rate	rate	dummy	rate	rate	dummy	rate
Panel A: Shortest Paths									
Predicted routes dummy	0.013***		0.021*		0.006	0.010	0.017	0.002	0.002
	(0.005)		(0.011)		(0.006)	(0.011)	(0.014)	(0.003)	(0.011)
One route		0.014*		0.020*					
		(0.007)		(0.011)					
More than one route		0.012		0.021					
		(0.008)		(0.017)					
Panel B: Model with Congestion Costs									
Predicted routes dummy	0.015***		0.022		0.018***	0.029	-0.000	0.003	-0.011
	(0.005)		(0.019)		(0.006)	(0.025)	(0.007)	(0.006)	(0.013)
One route		0.008		0.003					
		(0.006)		(0.013)					
More than one route		0.019***		0.035					
		(0.007)		(0.025)					
State x month FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
Municipality FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.36	0.36	0.10	0.10	0.35	0.09	0.07	0.36	0.10
Municipalities	1869	1869	1869	1869	1574	1574	1869	1869	1869
Observations	69,153	69,153	69,153	69,153	58,238	58,238	69,153	69,153	69,153
Mean dep.var.	0.044	0.028	0.044	0.028	0.045	0.026	0.117	0.044	0.028

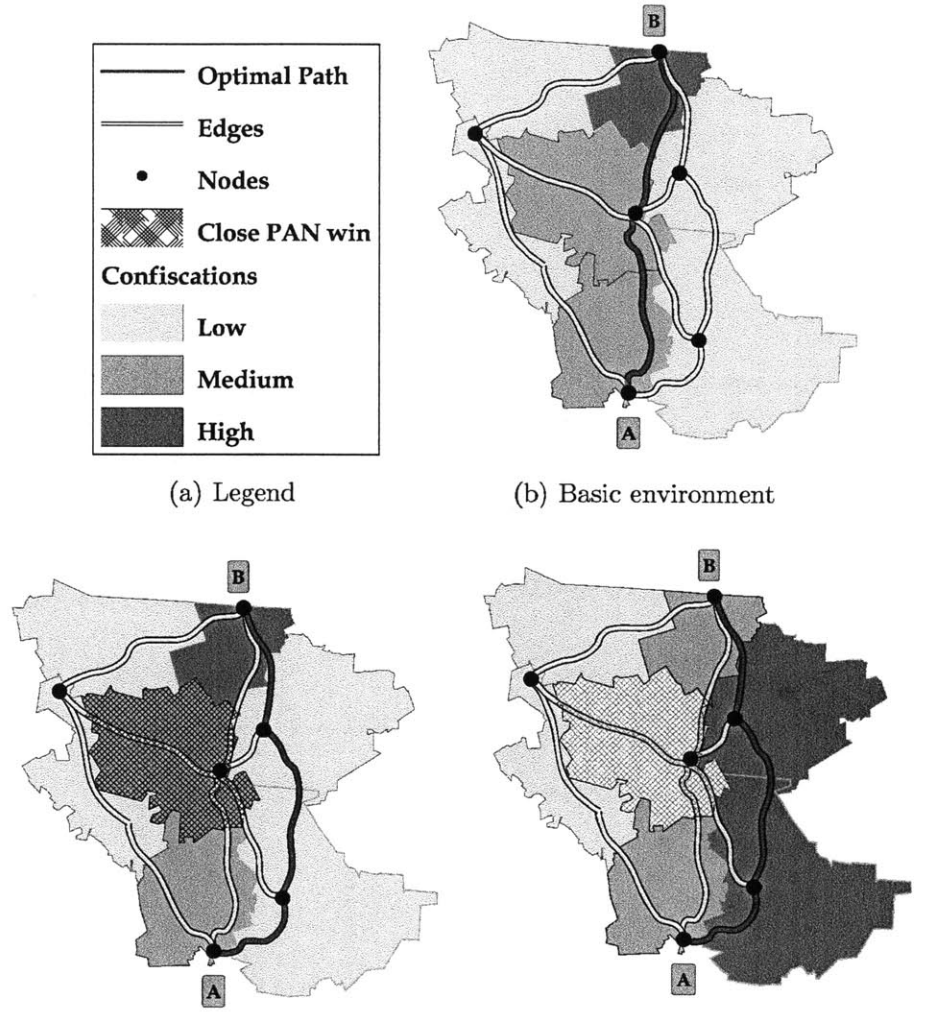
Notes: The dependent variable in columns (1), (2), (5) and (8) is a dummy equal to 1 if a drug trade-related homicide occurred in a given municipality-month; the dependent variable in columns (3), (4), (6), and (9) is the drug trade-related homicide rate per 10,000 municipal inhabitants, and the dependent variable in column (7) is the non-drug trade-related homicide rate per 10,000 municipal inhabitants. Columns (8) and (9) use the placebo network, as described in the text. Columns (5) and (6) limit the sample to municipalities that do not border a municipality that has experienced a close PAN victory. All columns include month x state and municipality fixed effects. Standard errors clustered by municipality and month x state are reported in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Table 2.8: Economic Spillovers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Full sample				Limited sample		Placebo Paths	
	Male participation	Female participation	Formal sector log wages	Informal sector log wages	Female participation	Informal wages	Female participation	Informal wages
Panel A: Shortest Paths								
Predicted routes dummy	-0.124	-0.756	0.020	-0.023	-0.784	-0.030	-0.691	0.022
	(0.513)	(1.038)	(0.022)	(0.020)	(1.622)	(0.027)	(1.040)	(0.023)
Panel B: Model with Congestion Costs								
Predicted routes dummy	-0.242	-1.261**	0.013	-0.022*	-1.558**	-0.028*	-0.520	-0.014
	(0.302)	(0.570)	(0.012)	(0.013)	(0.673)	(0.017)	(0.636)	(0.020)
State x quarter FE	yes	yes	yes	yes	yes	yes	yes	yes
Municipality FE	yes	yes	yes	yes	yes	yes	yes	yes
R^2	0.52	0.79	0.18	0.09	0.79	0.09	0.79	0.13
Municipalities	880	880	879	871	709	703	880	871
Observations	9,821	9,821	407,204	148,302	7,887	114,633	9,821	148,302
Mean Dep. Var.	89.58	51.46	3.34	3.24	44.81	3.03	51.46	3.24

Notes: The dependent variable in column (1) is average municipal male labor force participation; the dependent variable in columns (2), (5), and (7) is average municipal female labor force participation, the dependent variable in column (3) is log wages of formal sector workers; and the dependent variable in columns (4), (6), and (8) is log wages of informal sector workers. Columns (7) and (8) use the placebo network, as described in the text. All columns include quarter x state and municipality fixed effects. Column (1) weights by the square root of the municipality's male population and columns (2), (5), and (7) weight by the square root of the municipality's female population. The sample in columns (5) and (6) excludes municipalities that border a municipality that has experienced a close PAN victory. Standard errors clustered by municipality and quarter x state are reported in parentheses. * significant at 10%, ** significant at 5%, *** significant at 1%.

Figure 2-1: Illustration of Spillovers Methodology



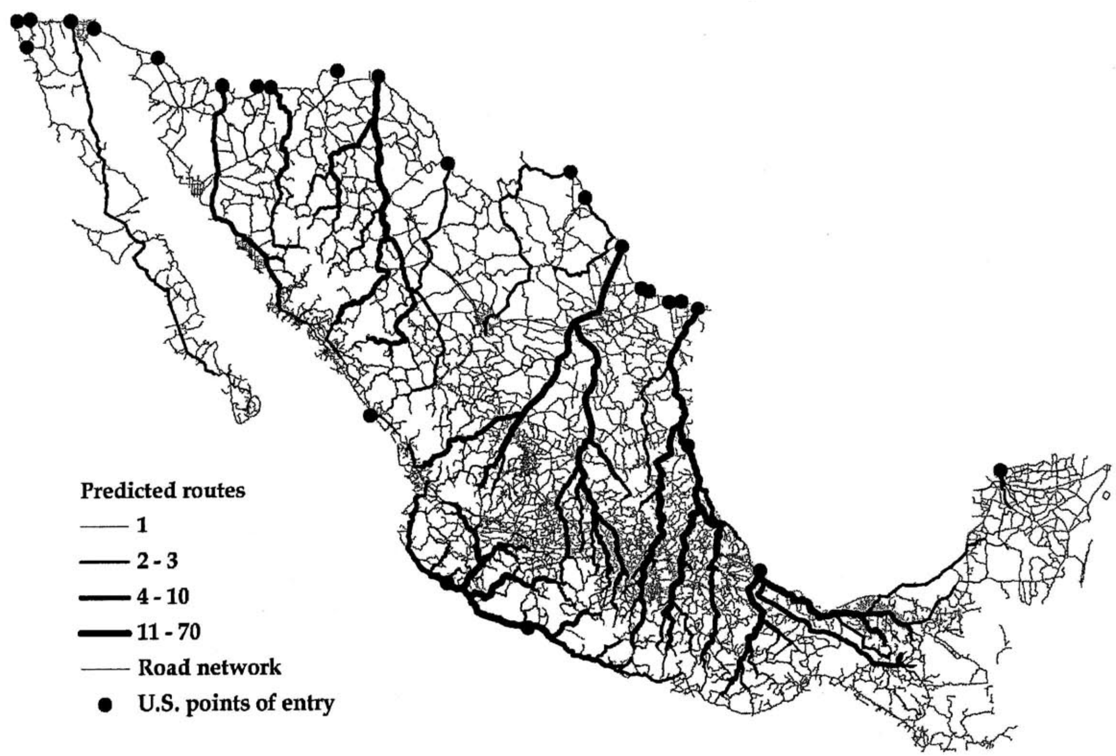
(a) Legend

(b) Basic environment

(c) Close PAN victory increases costs; routes change

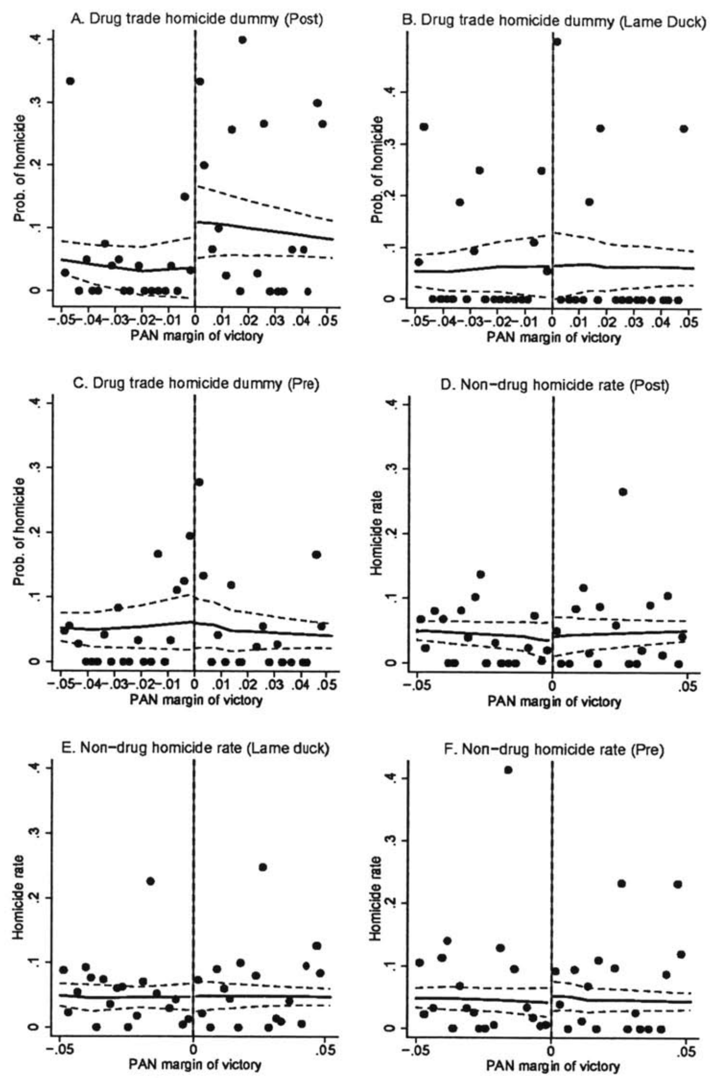
(d) Do confiscation patterns also change?

Figure 2-2: Road Network and Predicted Trafficking Routes



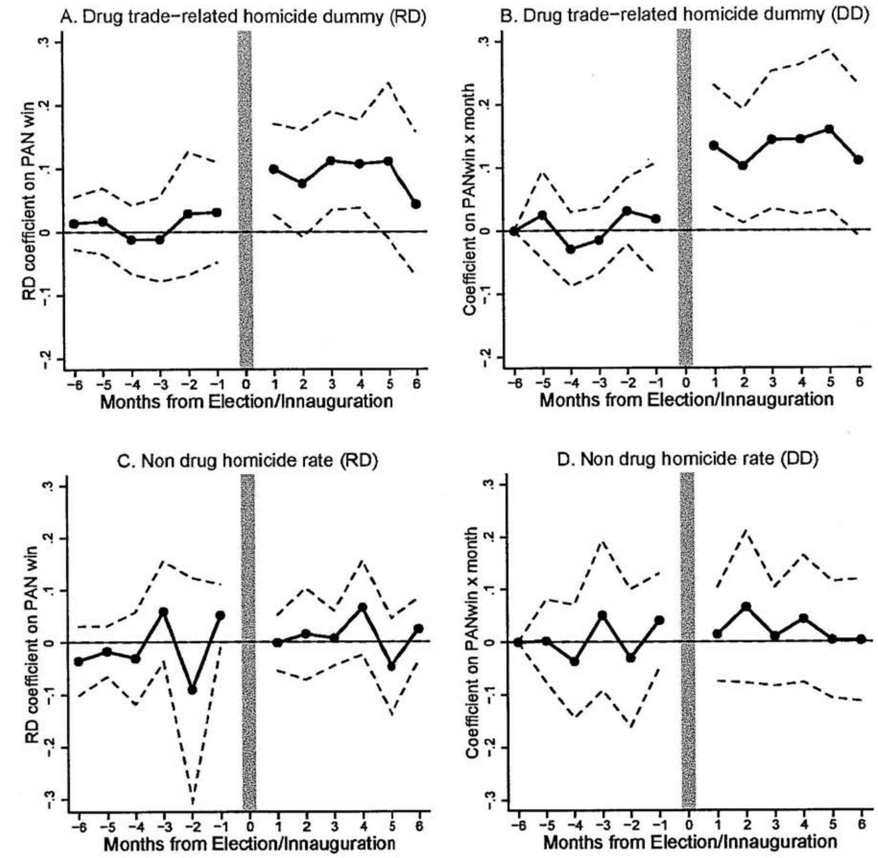
Notes: The least cost routes plotted in this figure are predicted using the network model with congestion costs.

Figure 2-3: RD Results: Close PAN Victories and Violence



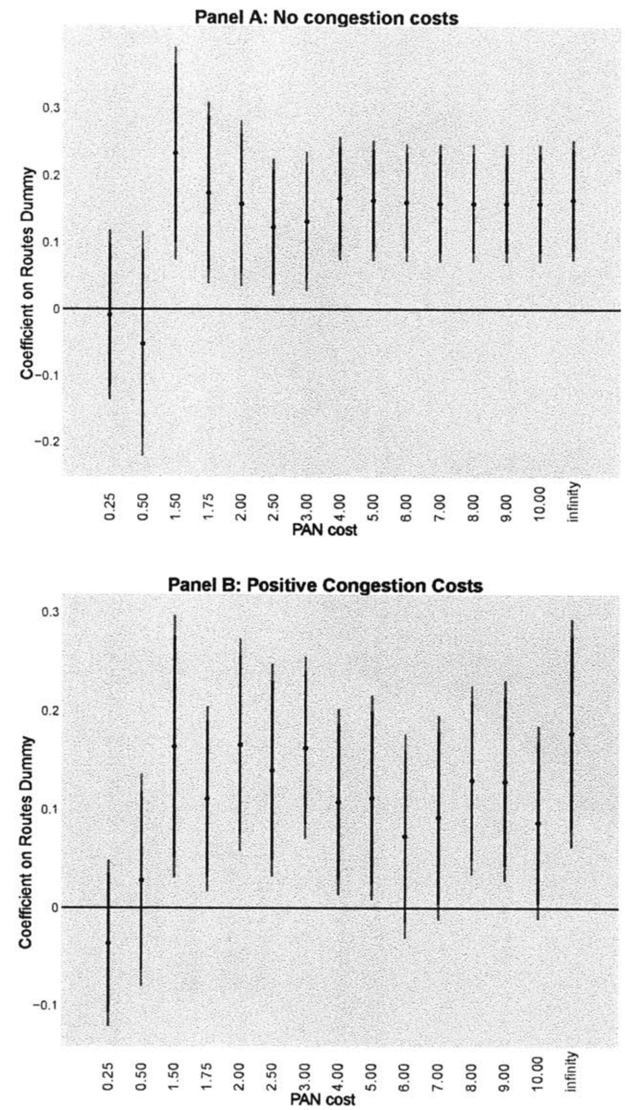
Notes: This figure plots violence measures against the PAN margin of victory, with a negative margin indicating a PAN loss. Each point represents the average value of the outcome in vote spread bins of width 0.0025. The solid line plots predicted values from a local linear regression, with separate vote spread trends estimated on either side of the PAN win-loss threshold. The dashed lines show 95% confidence intervals. The bandwidth is chosen using the Imbens-Kalyanaraman bandwidth selection rule (2009).

Figure 2-4: Estimates by Month



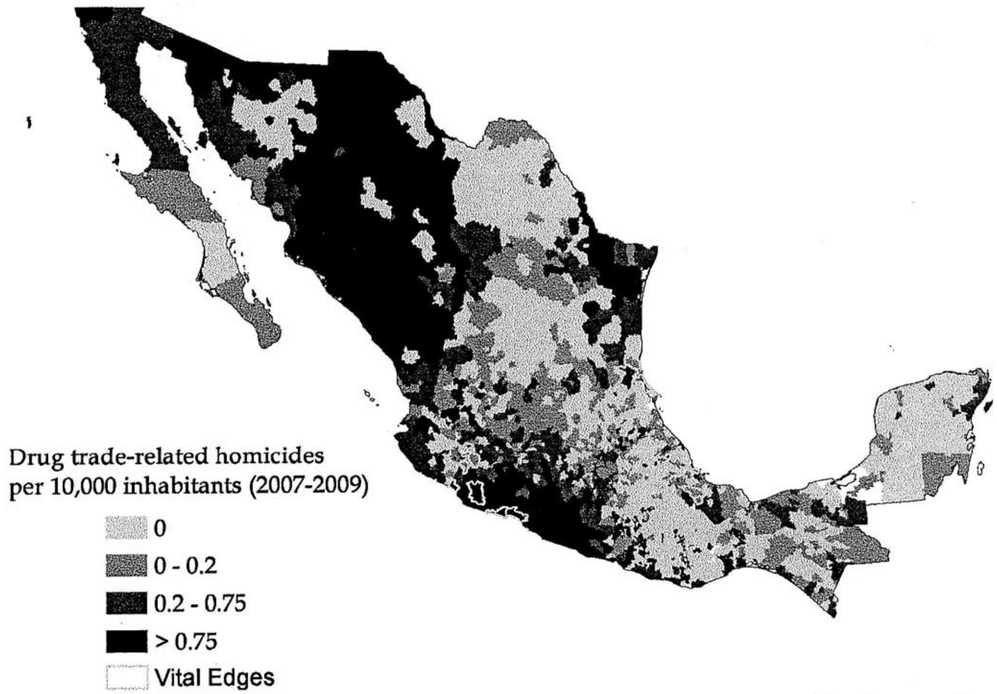
Notes: Panels A and C plot the RD coefficients on PAN win from equation (2.2), estimated separately for each month prior to the election and following the inauguration of new authorities. Panels B and D plot the γ_τ coefficients from equation (2.3). The dashed lines plot 95% confidence intervals.

Figure 2-5: Varying the Costs Imposed by PAN Victories



Notes: The y-axis plots the coefficient on the routes dummy in equation (2.5) when a close PAN victory is assumed to proportionately increase the effective length of edges in the municipality by a factor of α. The x-axis plots values of α ranging from 0.25 to 10. 95% confidence bands are shown with a thin black line and 90% confidence bands with a slightly thicker black line.

Figure 2-6: Vital Edges



Notes: Municipalities that contain a vital edge are highlighted in red. The average monthly drug trade-related homicide rate between 2007 and 2009 is plotted in the background.

Chapter 3

Insurgency and Long-Run Development: Lessons from the Mexican Revolution

3.1 Introduction

Bringing conflicted regions under the control of the central state has been a paramount motivation of state policy in the modern world. The extension of the state's authority often involves fundamental economic and political changes designed to weaken local elites and resolve longstanding disputes. While these reforms plausibly exert large persistent economic effects, few attempts have been made to quantify the long-run impacts of specific policies. This study examines how the central state brought conflicted regions in Mexico under control historically and relates this to long-run economic outcomes.

Specifically, I examine the Mexican Revolution, a multi-sided civil war that began in 1910 with the overthrow of long-time autocrat Porfirio Díaz. "Land and liberty" served as the battle cry of the Revolution, and at its heart were disputes about land distribution and the degree to which political power should be centralized. When fighting abated in 1918, rampant conflicts over land remained unresolved and the central state had limited authority outside the national capital. In the decades following the Revolution, major state priorities included a massive expansion of the federal bureaucracy and large scale agrarian reform. Mexico redistributed over half of its surface area in the form of *ejidos*: farms comprised of individual and communal plots that were granted to a group of petitioners. While individuals had inalienable usage rights to *ejido* plots as long as they remained in the community, *ejidal* land could not be sold, rented, or legally converted to non-agricultural use. In the decades following the Revolution, Mexico's single party bureaucracy controlled access to credit for essential inputs, as *ejidal* farmers could not use their land as collateral to access private credit markets, and state credit served patronage purposes (Deininger and Bresciani, 2001; Benjamin, 1989; de Janvry et. al, 1997; Ronfeldt, 1973). *Ejidos* still account for 54% of Mexico's land area and about half of its rural population today.

This study identifies how insurgency affected subsequent policies and economic development by using drought severity to instrument for insurgent activity, conditional on state fixed effects.[1] Miguel used There is a strong correlation between drought severity and insurgency, which is defined as the sustained use of violent force by local residents to subvert representatives of the government. Moving from half of long-run average precipitation - a severe drought - to average precipitation

[1]Ted Miguel, Satyanath Shanker, and Ernest Sergenti (2004) pioneered the use of rainfall shocks to empirically examine conflict. They utilize rainfall as an instrument for growth in order to identify the causal effect of growth on conflict in Africa.

decreased the probability of insurgent activity by around 38 percentage points, and the first stage F-statistic on this relationship is equal to 19. In contrast, drought severity is uncorrelated with a host of characteristics measured in 1900.

Instrumental variables estimates document that municipalities with sustained revolutionary insurgency had substantially more land reform in the years following the Revolution and are today poorer and more agricultural. Around 22 percentage points more of municipal surface area was redistributed through land reform in municipalities that experienced historical insurgency. Today incomes in municipalities with insurgency historically are around 30 percent lower, the fraction of the municipal labor force working in agricultural is 20 percentage points higher, and the fraction of the labor force working in industry is six percentage points lower. Moreover, alternations between political parties for the mayorship have been 33 percentage points less common over the past forty years. In contrast, I find little evidence for a persistent impact of insurgency on violence or public goods provision, which is largely determined at the state level.

There are many potential mechanisms through which insurgency could impact long-run development. This study focuses on agrarian structure, which historical and quantitative evidence emphasize as a particularly plausible mechanism. I hypothesize that the Mexican state promoted stability in insurgent regions by implementing large-scale agrarian reform, and this in turn lowered industrialization, income, and political competition in the long run.

Empirically identifying how Mexican land reform has affected long-run development requires alternative samples and identification approaches, and hence space constraints require that this question be examined in separate work. Here, I discuss existing empirical and historical evidence supporting the plausibility of agrarian reform as a central channel through which the effects of historical insurgency persist.

Most closely related to this paper is work by Beatriz Magaloni, Barry Weingast, and Alberto Diaz-Cayeros (2008) that uses a standard growth regression framework applied to Mexico's 31 states between 1950 and 1995 to measure the economic impact of land reform. They estimate that Mexican GDP per capita would have been 124 percent higher in 1995 had there been no land reform in Mexico. They also provide evidence that the distribution of *ejidal* lands was a key patronage instrument for generating support for the PRI (Institutionalized Revolutionary Party), which dominated Mexican politics from the 1920s through the end of the 20th century. Additionally, in ongoing work I examine the empirical relationship between agrarian structure and industrial development. I show that while high-productivity, irrigated agricultural areas tended to industrialize, this effect has been muted in regions with a high concentration of *ejidal* lands. Agribusinesses have preferred to locate in areas where they could vertically integrate by purchasing farms, which has not been possible in *ejidal* areas due to the prohibitions on land market transactions (Johnston et al., 1987). Moreover, the federal government, which for much of the 20th century controlled nearly a quarter of the Mexican food processing industry, tended to purchase food and locate state-owned processing industries near well-connected private farmers (Ochoa, 2000). Descriptive and historical evidence discussed later in this paper also points to large inefficiencies in the *ejidal* sector (see Deininger and Bresciani, 2001 for a review).

This study contributes to a growing literature on the economic effects of conflict (see Blattman and Miguel, 2010 for a review). The literature focuses primarily on the impacts of conflict on the labor, capital, and human capital stocks, whereas empirical work on the institutional effects of conflict is limited.[2] Studies have found

[2]For example, Blattman and Miguel (2010) argue: "The social and institutional legacies of conflict are arguably the most important but least understood of all war impacts."

rapid recovery of population following bombings (Davis and Weinstein, 2002; Brakman, Garretsen, and Schramm, 2004; Miguel and Roland, 2011). Miguel and Roland (2011) also find that in Vietnam local living standards and human capital levels converged rapidly across regions after the war, leaving few visible economic legacies of bombings twenty-five years later.[3] In the specific context of the Mexican Revolution, Stephen Haber, Armando Razo, and Noel Maurer (2003) document that while the latter years of the revolution were disruptive of commerce, for the most part they did not result in the destruction of assets, as insurgents had strong incentives to use productive assets to finance their activities. Manufacturing and mining recovered rapidly once the railroads resumed operations, and export agriculture boomed during the 1920s. These findings, combined with the large literature on the persistent impacts of institutions (see Nunn, 2009 for a review), suggest that persistent effects of insurgency are likely to work at least in part through institutional channels. This study shows that the Mexican Revolution exerted major long-run effects on property rights institutions, which are in turn likely to have had persistent economic consequences.

The Mexican government's use of agrarian reform to promote stability and extend its control over the agricultural sector is by no means unique. For example, parallels exist between the Mexican case and the Iraqi land reforms of the 1950s and early 1960s, in which the state did not fully distribute confiscated lands to private holders but rather became "a very large absentee landowner" (Warriner, 1969, p. 92). "Those who were responsible for carrying out the [land reform] law did not believe in the principle of small ownership; they aimed at keeping the peasants in the hire of the

[3]In contrast, other researchers have found impacts of conflict on children's height and school attendance (Alderman, Hoddinott, and Kinsey, 2006; Bundervoet, Verwimp, and Akresh, 2009; Shemyakina, 2006).

government to use for narrow party gains" (Warriner, 1969, p. 99). Communist bloc countries such as China, Cuba, and Vietnam went even further, with revolutionary regimes collectivizing farming (King, 1977). In contexts ranging from 6th century BCE Greece to Bolivia and Egypt in the 1950s, revolutionary regimes have used agrarian reform to quell unrest and bring peasant support to their coalitions (Tuma, 1965; Warriner, 1969, p. 242; King, 1977, p. 377). While many land reforms have extended state control over the agrarian sector, others have emphasized the transfer of land to private smallholders. For example, during the occupation of Japan the Allied Forces administered large-scale agrarian reforms that aimed to reduce peasant unrest by transferring land to individual, private producers (King, 1977, p. 192). In South Korea, which implemented a large scale agrarian reform in the 1950s, many landowners transacted with peasants in private land markets under the threat of confiscation if the maximum farm size threshold was exceeded (Putzel, 2000).[4] Land distribution remains central in many conflicts today, including those in Afghanistan, Iraq, Uganda, Guatemala, Indonesia, Cambodia, and elsewhere. This paper's empirical results highlight the potential for persistent economic inefficiencies to arise when reforms to resolve land disputes extend the state's power by replacing market interactions with political patronage.

In the next section, I provide historical background on the Mexican Revolution, and Section 3 examines the relationship between drought and revolutionary insurgency. Section 4 tests whether insurgency impacted agrarian reform and long-run economic development by using drought as an instrument for insurgency. Section 5 discusses the mechanisms relating historical insurgency, land reform, and long-run

[4]Because the state offered minimal compensation for the confiscation of lands from Korean landowners who violated the maximum farm size threshold and required peasants to pay a relatively high price to purchase confiscated lands, large landowners and landless farmers had strong incentives to engage in market transactions.

development. Finally, Section 6 offers concluding remarks.

3.2 Historical Background

3.2.1 The Mexican Revolution

The Mexican Revolution began in 1910 with the overthrow of autocrat Porfirio Díaz, who first ascended to the presidency of Mexico in 1876. While Mexico's constitution stipulated a democratic government with no re-election of the president, Díaz repeatedly claimed that Mexico was not yet ready for democracy and remained in power through rigged elections. His 35 year tenure was marked by industrialization, a dramatic increase in land concentration, and the centralization of political power.

In a 1908 interview with the U.S. journalist James Creelman, Díaz - then approaching eighty - stated that he would retire and allow other candidates to compete for the presidency. However, Díaz ultimately changed his mind and ran again for re-election in 1910, allowing northern opposition politician Francisco Madero to run against him. Despite widespread popular support for Madero, Díaz claimed to have been re-elected almost unanimously and had Madero jailed. Madero subsequently issued a letter from jail calling for popular revolt, and his vague promises of agrarian reform attracted peasants throughout Mexico, leading to numerous rebellions. The time was particularly ripe for successful rebellion, given the advanced age of Díaz and many of his military allies, and Díaz and the federal army were defeated in May of 1911.

Madero was elected to the presidency, but proved an unpopular leader. He angered the peasant revolutionaries who had brought him to power by failing to implement agrarian reform and by increasingly allying with members of the Porfirian elite to quell unrest amongst those demanding radical change (Knight, 1986). While he faced armed opposition from the left, Madero was ultimately overthrown in a 1913 coup by counter-revolutionary General Victoriano Huerta. Numerous revolutionary movements against Huerta's military government arose in 1913, occurring largely in the same places that had witnessed revolutionary activity in 1910-1911. These disparate movements were able to unite in their opposition to Huerta and overthrow his regime in July of 1914. However, their differences proved irreconcilable and soon after Huerta's defeat the conflict deteriorated into a multi-sided civil war.

The ultimately victorious faction, referred to as Constitutionalism, emphasized economic modernization and state centralization under a political class that was different and somewhat broader than the Porfirian elite. Constitutionalism was centralized under a single military and political command and unambiguously sought national power. The movement's backbone was in the northern Mexican states of Sonora and Coahuila, which were relatively prosperous. It garnered its primary support from middle class, urban, and industrial interests and also gained the official recognition of the U.S. government. The victorious Constitutionalists ultimately formed the Institutionalized Revolutionary Party (PRI), which dominated Mexico as a single party political system for most of the twentieth century.

In contrast, the most widespread type of revolutionary movement sought to defend local political structures against incursions by the central government. Many of these movements called for agrarian reform to return lands confiscated by large estates during the Díaz regime to the peasants who had previously held them, and they were typically local in their demands, scope, and political aspirations.[5] The

[5]Prominent examples of movements calling for radical agrarian reform include the Zapatistas operating in Morelos as well as parts of Puebla, Mexico state, and some other regions; the Cedillo brothers in San Luis Potosi; and Calixto Contreras in Durango. Examples of movements that did not pursue agrarian goals including rebellions in the Misantla and Huatusco-Córdoba regions of

rebellions led by Pancho Villa in northern Mexico and Emiliano Zapata in central Mexico are the largest and most well-known of these movements.

The Revolution witnessed some traditional pitched battles, fought primarily between the Constitutionalists and Pancho Villa in northern and north-central Mexico, but much of the fighting consisted of guerrilla warfare. By the end of 1915, Villa had been reduced to guerrilla tactics in his home base of Chihuahua, and it took the Constitutionalist army several more years of fighting to defeat him and the large number of other local guerrilla movements, one of the most tenacious of which was the Zapatistas in Morelos. Purges of local rebel leaders continued throughout the 1920s and in some cases into the 1930s.

3.2.2 Bringing insurgent regions under the control of the state

The rallying battle cry of the Revolution was "land and liberty" (*tierra y libertad*), and while not all insurgent groups were fighting for land redistribution, this was a central demand of many insurgent movements (Knight, 1986). Towards the close of the Revolution, Mexico ratified a new constitution that stated that centers of population that lacked access to adequate land would be granted land in sufficient quantities for their inhabitants' needs. According to Article 27, estates whose size exceeded a maximum limit could be expropriated; and religious institutions could not hold, administer, or acquire land. All properties belonging to the Catholic Church, which held extensive property, reverted to the state.

In the decades following the Revolution, the government operationalized Article

Veracruz led by large landowners, the Manuel Peláez rebellion in the Huasteca, and the Natividad brothers in Tepic (Knight, 1986).

27 through the creation of state-owned properties called *ejidos.* *Ejidos* consisted of communal and individual plots that were granted by presidential decree to a group of petitioners. Communal plots were devoted to purposes such as grazing and firewood, whereas crops were typically cultivated on individual *ejidal* plots. *Ejido* members received usufruct rights to their plots, but ownership rights were held by the community and ultimately controlled by the central state. While the rights to the produce from one's plot were inalienable as long as one remained on the land, they could not be transferred, and the rental of *ejidal* land and hiring of labor to work *ejidal* plots were also prohibited.

Ejidos were central to Mexican agriculture, as nearly half of Mexico's surface area entered the *ejidal* sector during the 1920s and 1930s. The state obtained the land for the *ejdial* sector from estates that had been confiscated from the pre-revolutionary elite during the Revolution, from the extensive holdings confiscated from the Church, and from the purchase of lands from large landowners whose estates exceeded the legal maximum size.

Ejidos met the demands of Mexico's peasant revolutionaries for access to land, and simultaneously served as a central vehicle for the state to extend its control into the countryside. The central state controlled access to essential inputs, such as water resources (including wells on *ejidal* properties) and credit. *Ejido* producers could not use their land as collateral to access private credit markets since the state owned the land, and thus were reliant on the state for the credit necessary to buy seeds, fertilizer, and other inputs. Pervasive corruption in the state bank serving the *ejidal* sector has been well-documented (DeWalt, 1979; Wilkie, 1971). *Ejidal* elites exerted disproportionate influence over decisions about the reallocation of vacated *ejidal* lands, with plots became concentrated in the hands of individuals who held political positions in the *ejidos* (DeWalt, 1979; Wilkie, 1971). Many decisions about land allocation and credit had to be countersigned by state politicians from Mexico's single party bureaucracy (Deininger and Bresciani, 2001; Benjamin, 1989; de Janvry et. al, 1997; Ronfeldt, 1973).

Ejidos today account for 54% of Mexico's land area, and about half of its rural population. In 1992, Mexico reformed Article 27 of the constitution so that the state no longer has an obligation to provide *ejidos*. The state began an *ejido* titling program called PROCEDE in the same year, seeking to resolve conflicts over plot boundaries within and between *ejidos* and to facilitate investment and markets. Through PROCEDE, an *ejido* assembly selects which parts of the *ejido* will be designated for common use and which parts will be designated for private plots. Boundaries are delineated and rental of the plots designated for private use is permitted. Once the individual plots have been delineated, producers have the option of registering their plot in the private domain - allowing it to be bought and sold in land markets - if a super-majority of the *ejido* members agree. To date, approximately 90% of *ejidos* have had their internal and external boundaries delineated through PROCEDE, and around 2.5% of the surface area in the *ejidal* sector has entered the private domain.

There are various mechanisms through which agrarian organization could affect long-run development. These will be discussed in more detail in Section 5, after the relationships between insurgency and land reform and between insurgency and long-run economic outcomes have been examined.

3.3 Drought and Insurgency

In order to examine the impact of insurgency on subsequent state policies and long-run economic development, I use drought severity to instrument for insurgent activity. Scholars have argued that a major drought occurring between 1907 and 1910 was central in spurring revolutionary activity (see Tutino, 1981; LaFrance, 1990), but this hypothesis has not been tested empirically. In this section, I explore the first stage relationship between drought severity and insurgency, conditional on state fixed effects. I also test whether drought is correlated with a number of important pre-characteristics and examine whether the size of the first stage relationship differs across various sub-groups.

3.3.1 Identification Strategy

I test whether drought severity in the years leading up to the Revolution affected insurgency by running the following regression:

$$insurgency_{ms} = \gamma_0 + \gamma_1 drought_{ms} + X'_{ms}\beta + \alpha_s + \epsilon_{ms} \tag{3.1}$$

where $insurgency_{ms}$ is a dummy variable equal to 1 if the citizens of municipality m - during the period between 1910 and 1918 - used violent force in a sustained attempt to subvert representatives of the Mexican government (i.e. local authorities and the military) or to confiscate others' property. $drought_{ms}$ measures the severity of drought during the 1906-1910 period, X_{ms} contains a vector of time invariant geographic characteristics, and α_s is a state fixed effect. All variables are described in more detail in the following section.

I then use drought severity as an instrument for insurgency in the following regression:

$$y_{ms} = \delta_0 + \delta_1 insurgency_{ms} + X_{ms}\beta + \alpha_s + \mu_{ms} \tag{3.2}$$

where y_{ms} is the outcome of interest.

This instrumental variables approach requires the two following assumptions (Angrist, 2009). First, drought must be correlated with insurgency. If this correlation is only marginally different from zero, the resulting instrumental variables estimates are unlikely to be informative. Second, drought must be uncorrelated with any other determinants of the outcomes of interest: in other words, $\text{corr}(drought_{ms}, \mu_{ms}) = 0$. This condition is referred to as the exclusion restriction. It will obtain if drought is as good as randomly assigned, conditional on state fixed effects, and if drought has no effect on long-run economic and political outcomes other than through the insurgency channel.

While the exclusion restriction relies on the instrument being uncorrelated with unobserved determinants of the outcomes and hence is untestable, I shed light on its plausibility by running two sets of placebo checks. First, I test whether drought is uncorrelated with a number of important observable characteristics measured in 1900. Second, I examine whether drought in other five year periods exerts persistent effects on long-run development. To the extent that similar droughts in other periods do not have persistent effects, this would increase our confidence that any long-run effects of the 1906-1910 drought are acting primarily through its impacts on insurgency.

3.3.2 Data

Monthly five year averages of precipitation during 1906-1910 are available for 217 municipalities, located in district seats throughout Mexico.[6] These data are preserved in a government publication *Atlas termopluviometrica* that was sent to the World's Fair and is now held by Tulane University. I measure drought severity as the ratio of av-

[6]Districts are a political unit larger than municipalities but smaller than states that were abolished in 1916.

erage monthly precipitation in 1906-1910 to long-run average monthly precipitation, which it taken from World Clim 4's monthly long-run average precipitation rasters. Motivated by the historical and agronomic literature (i.e. Hollinger and Changnon, 1993), I censor the measure at one[7] I focus on rainfall during non-harvest months for corn - Mexico's main staple crop in 1910. While drought is harmful during most of the year - lowering soil moisture content and reducing plant growth - it is beneficial during the harvest season. Results are generally robust to using drought severity over the year as a whole, but the first stage is weaker. Drought severity is plotted in Figure 1.

I have also examined robustness to using the standardized precipitation index to measure drought severity. The standardized precipitation index is calculated as (1906-1910 mean precipitation - long run mean precipitation)/(long run standard deviation of precipitation). The long-run standard deviation is calculated from the Mexican government's precipitation records, maintained in the government's climate database ERIC 3. The long-run precipitation record is highly incomplete, with weather stations shifting locations over time, and it takes more data to estimate a long-run standard deviation than it does to estimate long-run mean.[8] The sample for which there is enough information to calculate the long-run standard deviation is smaller than the sample for which information on 1906-1910 precipitation is available, and hence my main focus is on the larger sample for which the percent normal measure is available.

Data on insurgency were compiled from multiple regional histories and from de-

[7]In field experiments, agronomists have found only marginal benefits to corn yields from augmenting precipitation above long-run average levels. On the other hand, additional precipitation is beneficial in dry years (see for example Holliinger and Changnon, 1993).

[8]While climate models can be effective in predicting long-run average precipitation with relatively limited data, models have not been extensively developed to predict the long-run standard deviation.

tailed municipal timelines available in the *Encyclopedia of Mexican Municipalities.* These sources are listed in the appendix. A municipality is classified as having insurgent activity if - during the period between 1910 and 1918 - its citizens used violent force in a sustained attempt to subvert representatives of the Mexican government (i.e. local authorities and the military) or to confiscate others' property. Because the literature on the Revolution is enormous, with multiple regional histories existing for each state as well as detailed municipal histories, I am able to explicitly document the absence of revolutionary activity. In the appendix, I create a timeline for each municipality in the sample, documenting what occurred during the revolutionary period and whether this included insurgent activity. Insurgency is plotted in Figure 2.

For example, for the town of Torreon (Coahuila), I document revolutionary activity beginning in 1911 with the overthrow of the federal military garrison and widespread popular attacks on large property holders. I document that revolutionary activity persisted into 1914, when Torreon fell to Pancho Villa's forces, and so forth. In contrast, I document that the town of Coatepec (Mexico state), did not witness insurgent activity. In listing notable events in the town's history, the Encyclopedia of Mexican Municipalities focuses for the revolutionary period on the construction of a three room schoolhouse in 1915. It explicitly states "In Coatepec Harinas, the revolutionary era did not cause the disruptions that other areas suffered." Moreover, maps in *Revolucion en el estado de Mexico* (Palafox, 1988) show that this municipality is not near the areas of the state that experienced rebel activity or widespread banditry. While the binary insurgency measure is a considerable simplification of a complex event, more detailed measures would be difficult to construct in a systematic way. Summary statistics for the drought and insurgency variables are presented in Table 1.

3.3.3 Results

Table 2 documents the first stage relationship between 1906-1910 drought severity and insurgency during the Mexican Revolution and examines whether this relationship is robust to different measurements of drought severity. Robust standard errors are in parentheses and Conley standard errors that correct for spatial correlation are in brackets. All columns include controls for time-invariant municipal geographic characteristics: long-run average precipitation (1950-2000), long run average minimum and maximum temperature (1950-2000), elevation, and slope, as well as state fixed effects.[9]

Column 1 uses the percent normal measure of drought severity in non-harvest months, the measure that concurs most closely with the biology of plant growth. This measure divides precipitation in 1906-1910 by long-run average precipitation and is available for the full sample of municipalities for which we have data on 1906-1910 precipitation. Column 2 examines the percent normal measure calculated over the entire year. In both cases, the correlation between drought severity and revolution is statistically significant at the one percent level, with a somewhat stronger relationship when the non-harvest drought measure is used (first stage F-statistic of 19.1) than when the full year drought measure is used (first stage F-statistic of 9.9). Moving from half of long-run average precipitation - a severe drought - to average precipitation decreases the probability of insurgent activity by around 38 percentage points. In the sample as a whole, the probability of insurgency is 59%.

Columns 3 through 6 examine the relationship between standardized precipitation indices and insurgency. Recall that the standardized precipitation index subtracts the long-run monthly mean from the 1906-1910 monthly mean and the divides by the

[9]Results (not shown) are similar when the geographic controls are excluded or when municipal-level characteristics measured in 1900 are included.

long-run standardized deviation, averaging these standardized monthly deviations across the year. Columns 3 and 4 construct the standardized precipitation index by using weather stations within 25 km of a 1906-1910 weather station to calculate the long-run standard deviation of precipitation. Columns 5 and 6 use data from weather stations within 10 km of a 1906-1910 weather station to calculate the long-run standard deviation. In 182 municipalities, a weather station appearing for at least 25 years in the weather record is located within 25 kilometers of the 1906-1910 weather station, and 120 municipalities have one of these stations within 10 kilometers of their 1906-1910 weather station. These samples compare to the full sample of 210 municipalities for which 1906-1910 precipitation data are available. Columns 3 and 5 calculate the standardized precipitation index averaged over non-harvest months, and columns 4 and 6 average the monthly standardized precipitation index over all months in the year.

There is typically a first stage relationship between the drought measure and insurgency, but the first stage F-statistics are smaller than they are in columns 1 and 2. Moving from one standard deviation below normal precipitation to normal precipitation decreases the probability of insurgency by around 17 percentage points. Because the first stage is not as strong for the standardized indices and the sample size is smaller, I use the percent normal drought measure for the remainder of the analysis. Results using the standardized precipitation index as an instrument for insurgency can be found in the appendix.

The instrumental variables approach will estimate the impact of insurgency on various outcomes for those municipalities that were induced by drought to participate in insurgent activity. While we cannot observe whether citizens in a given municipality took up arms in response to drought, Table 3 sheds light on which sorts of municipalities were influenced by the drought by examining the size of the first stage for different sub-populations.

Column 1 reports the baseline first stage relationship from the full sample, reproducing column 1 of Table 2 for comparison purposes. Column 2 limits the sample to municipalities in states that are closer than the median distance to the U.S., and column 3 limits the sample to municipalities in states that are farther than the median distance from the U.S. The coefficient on drought severity in the full sample is -0.770 (s.e.= 0.254). This coefficient is -0.650 for places nearer the U.S. and -0.917 (s.e.= 0.224) for places further from the U.S. Both are statistically different from zero, and they are not statistically distinguishable from each other.

Columns 4 and 5 divide the sample by whether the municipality had a higher percentage of its male population working in agricultural in 1900 than the median municipality. The correlation between drought severity and insurgency is statistically significant in both samples but is substantially larger in the more agricultural sample, at -1.288, than in the less agricultural sample. In the more agricultural sample, moving from half of long-run average rainfall in 1906-1910 (a severe drought) to long-run average rainfall decreases the probability of insurgency by around 64 percentage points. Next, columns 6 and 7 divide the sample by whether the municipality had more or less of its population living in an *hacienda* in 1900 relative to the median municipality. The relationship between drought severity and insurgency is large and highly statistically significant for both samples. Finally, columns 8 and 9 divide the sample by whether a higher or lower percentage of the municipality's population spoke an indigenous language in 1900 than in the median municipality. The relationship between drought and insurgency is large and statistically significant in both sub-samples. Overall, these results document that drought led to insurgency in municipalities with a wide variety of initial characteristics, and this relationship was particularly pronounced in more agricultural municipalities.

Identification requires drought severity between 1906 and 1910 to be as if randomly assigned. In other words, in the absence of differences in rainfall during these years, municipalities that suffered drought would not have been different on average from municipalities that received above average rainfall. To shed light on the plausibility of this assumption, Table 4 regresses a variety of outcomes from the 1900 Mexican Population Census on drought severity, measured as the percent of normal precipitation in non-harvest months. The sample sizes are slightly different across outcomes, as portions of the 1900 census volumes have not been preserved for the state of Oaxaca.

For comparison purposes, column 1 reports the first stage relationship between drought severity and insurgency from Table 2. The dependent variable in column 2 is the percentage of the municipal male population that worked in agricultural in 1900.[10] The dependent variable in column 3 is the percentage of the population living in *haciendas*, landed estates with an attached labor force. The dependent variable in column 4 is the percentage of the population that primarily spoke an indigenous language, in column 5 it is the percentage that was literate, in column 6 it is the number of public employees per 1,000 inhabitants, and in column 7 it is the number of police per 1,000 inhabitants.

The correlations between these outcomes and drought severity are all statistically insignificant. The magnitudes of these relationship also tend to be smaller, relative to the sample mean, than the magnitude of the relationship between drought and insurgency. The next section will document that some of these outcomes diverge in municipalities with insurgent activity as compared to municipalities that did not experience insurgent activity, in the years following the Revolution.

[10]Nearly all women are counted in the 1900 census as having a domestic occupation, and hence are excluded from this measure.

3.4 Insurgency's impacts on policy and development

This section uses an instrumental variables approach to test whether insurgency affected two of the most central Mexican federal government policies in the years following the Revolution: agrarian reform and the expansion of the federal bureaucracy. During the 1920s and 1930s, the federal government redistributed almost half of Mexico's surface area through a major agrarian reform program, and the size of the federal bureaucracy increased more than fivefold. I find that agrarian reform was concentrated in insurgent municipalities, whereas there is little relationship between insurgency and the expansion of the federal bureaucracy. After examining insurgency's impacts on these major policies, I test whether it has exerted persistent effects on economic outcomes. Instrumental variables estimates document that insurgency has lowered income and industrialization and increased the percentage of the labor force in agriculture in the long-run. In the next section, I provide evidence that agrarian reform is a plausible central mechanism explaining insurgency's persistent economic impacts.

3.4.1 Data

I obtained data on agrarian reform from Mexico's online *Sistema de Información del Padrón e historial de núcleos agrarios*, which compiles information on all government actions related to agrarian reform and titling, at the level of the *ejido*, from 1916 until the present. Data on over 31,000 ejidos were used to calculate information on municipal-level agrarian reform, titling, and entry of ejidal plots into the private domain.

Data on income, the labor force, public employees, and education are taken from Mexican census data for the years 1900, 1910, 1930, 1940, 1960, 2000, and 2010. Data on household access to water are from CONAPO (National Population Council) (2005). Electoral data are from *Electoral -Banamex* and electoral results published by the Electoral Tribunals in each of Mexico's 31 states. The geographic characteristics are from Acemoglu and Dell (2010). Data on homicides (1990-2006) are from INEGI and data on drug trade-related violence and subversion are from confidential government sources. Data on municipal tax collection are from *Sistema de Cuentas Municipales, INEGI.*

3.4.2 Insurgency and government policies

Table 5 tests whether insurgency influenced the targeting of agrarian reform, using the percentage of a municipality's surface area redistributed by agrarian reform as the dependent variable. Panel A reports the IV estimates, and for comparison purposes Panel B reports the OLS estimates. The percent normal drought measure is used as the instrument. Robustness to alternative measures of drought and to the inclusion of additional controls is documented in the appendix.

Overall, Table 5 provides strong evidence that insurgency led to increased land reform. The IV estimates in column 1 document that in the sample as a whole, insurgency increased the percentage of municipal surface area redistributed as *ejidos* by 21.8 percentage points (s.e.= 0.111), relative to the sample mean of 48.9 percent. When we focus attention on municipalities where participation in agricultural was above the sample median in 1900, the estimated effect is 31 percentage points, and this effect is statistically significant at the one percent level. In contrast, the impact of insurgency is smaller and not statistically different from zero in the sample with below median participation in agricultural in 1900. This pattern is what we would expect, given that the demand for agrarian reform is likely to be higher in areas where a greater percentage of the population is employed in agriculture. While producers now have the option of registering *ejidal* lands in the private domain, only 2.5% of *ejidal* lands have been registered thus far, so agrarian reform numbers are largely reflective of how much land remains in the *ejidal* sector today.

In columns 1 and 2, the IV estimates are somewhat larger than the OLS estimates. There are a number of reasons why this could occur: because of measurement error in the insurgency variable, because of omitted variables bias in the OLS specification, because the IV measures a local average treatment effect on municipalities induced to take up arms by drought severity (whereas the OLS measures the correlation across the full sample), or because drought severity violates the IV exclusion restriction. It is not possible to fully disentangle or explicitly test these possibilities since they depend on characteristics the researcher does not observe. However, it is unlikely that a violation of the exclusion restriction is the primary reason why the IV estimates are larger than the OLS estimates, since the instrument is uncorrelated with a host of pre-characteristics (Table 4). Moreover, when I examine the estimates across the different sub-samples in Table 4, the insurgency coefficient is not substantially different in the sub-samples with larger first stages. Thus, it appears unlikely that the IV estimates reflect a local average treatment effect that only applies to a small sub-population. In contrast, both omitted variables bias and measurement error appear plausible, given the inherent difficulties in quantifying insurgent activities and the many factors that could influence the decision to take up arms. For example, while there are some well-known examples of indigenous revolutionary leaders, on average municipalities with revolutionary activity were substantially less indigenous in 1900. In turn, it is likely that more indigenous places received more land reform, since their land was more likely to be taken in the first place, and this would lead to a downward bias in OLS coefficient on revolution.

A major expansion of the federal bureaucracy also occurred in the decades following the Revolution. In 1900, there were 3.2 government employees per 1,000 municipal residents in the sample examined in this paper. By 1940, this number had risen to 20. On average, 16 of these were federal government employees. Table 6 explores whether insurgency differentially affected the number of public employees present in a municipality in 1940. Given the greater amount of agrarian reform, we might expect that the number of government employees would be higher in municipalities that had experienced insurgent activity. On the other hand, since a major demand of many insurgents was greater autonomy from the central government, to the extent that these demands were met we would expect lower federal bureaucracy presence. We see from Table 5 that the IV does not estimate statistically significant differences between municipalities that experienced insurgent activity and those that did not. If anything the number of federal and state employees was lower in municipalities that experienced insurgency. Moreover, important measures of government bureaucracy today do not differ. Column (5) documents that in 2005 the number of school teachers per 1000 school aged children was statistically identical in municipalities with historical insurgency and in those without historical insurgency. Column (6) documents that local tax receipts per dollar of municipal income in 2005 also did not differ.

3.4.3 Insurgency and long-run development

The previous section documented a large impact of insurgency on subsequent agrarian reform. This section examines whether insurgency has exerted long-run impacts on economic prosperity.

First, Table 7 tests whether insurgency impacts economic prosperity today. Columns 1 through 4 examine income, using microdata from the 2000 Mexican Population Census. The IV coefficient reported in column 1 estimates that historical insurgency has lowered income by around 33%. Columns (2) through (4) divide the sample into individuals working in agriculture, in industry, and in services. Care should be taken in interpreting the results given that insurgency may also influence selection into economic sectors, but the exercise is nevertheless informative about where the income effects are concentrated. The IV coefficient is large and negative in all three sectors, though it is not statistically significant for agriculture. This is not surprising given that income tends to be poorly measured for household agricultural producers. As in Table 6, the IV coefficients are larger than the OLS coefficients, likely for the same reasons that were discussed in the previous section.

Next, columns 5 and 6 examine the percentage of households in a municipality that lack access to running water and to electricity. Households in municipalities with insurgent activity historically are 14 percentage points more likely to lack access to running water and around three percentage points more likely to lack access to electricity. These effects are large, given that in the sample as a whole around 11 percent of households lack access to water and three percent lack access to electricity.

Next, Table 8 tests whether insurgency has influenced the sectoral allocation of the labor force. Columns 1 and 2 examine the percentage of the municipal labor force working in agriculture and industry in 2010, columns 3 and 4 do the same for 1960, and columns 5 and 6 examine the percentage of the male population working in agriculture and industry in 1940. The instrumental variables estimates document that today, the percentage of the labor force working in agricultural is around 20 percentage points higher in municipalities that experienced insurgent activity historically, and the percentage of the population working in industry is around six percentage points lower. The insurgency coefficient in the agricultural labor force regression is significant at the 5% level and the estimated impact on industry is marginally significant. The point estimates are of similar magnitude in both 1960 and 1940, though they are very noisily estimated for 1960.

While there are many channels through which historical insurgency could impact income, Tables 4 through 7 - combined with the qualitative literature - provide strong suggestive evidence that the restrictions imposed by agrarian reform in Mexico have discouraged people from leaving agriculture and reduced industrial development, lowering income in the long-run. Lower agricultural productivity in municipalities with more land reform could also help explain the persistent impacts of historical insurgency on income. Section 5 provides a detailed historical and empirical examination of the relationship between land reform and current economic outcomes.

It is also well-documented that agrarian reform created and sustained a political patronage system linking *ejidal* elites to government officials at the municipal and state levels (Varley, 1989; DeWalt, 1979; Wilkie, 1971). This patronage system may plausibly have stifled political competition in the long-run. The evidence in Table 9 supports this hypothesis, documenting that alternations between political parties for the mayorship have been substantially less common in municipalities with insurgent activity historically. The IV coefficient is large and highly significant, estimating that insurgency has lowered alternations between parties holding the mayorship by 32.7 percentage points between 1974 and 2009, relative to a sample mean of 24.4 percent. The effect is present both before 1994, when Mexico was less politically competitive, and after 1994, when it became more democratic. It is particularly pronounced for the post-1994 period. However, differences in local political competition do not appear to have lowered the provision of important public goods, which are provided by the state and national governments. Results, available upon request, fail to find a large or statistically significant relationship between historical insurgency and the public provision of education, health care, or road infrastructure in recent years.

3.5 Mechanisms

There are many potential mechanisms through which insurgency could exert persistent economic effects. While it is infeasible to examine all possible channels of persistence, the existing literature provides considerable guidance on mechanisms that are especially likely to be important. In particular, there is a large literature emphasizing the economic effects of Mexico's agrarian organization. I first discuss the relationship between land reform and economic outcomes and then present empirical evidence on potential alternative mechanisms relating insurgency to long-run economic development.

3.5.1 Land reform

Empirically identifying how Mexican land reform has affected long-run development requires alternative samples and identification approaches, and hence space constraints require that this question be examined in separate work. Here, I discuss existing empirical and historical evidence supporting the plausibility of agrarian reform as a central channel through which the effects of historical insurgency persist.

Most closely related to this paper is work by Beatriz Magaloni, Barry Weingast, and Alberto Diaz-Cayeros (2008) that empirically examines the impact of land reform on economic growth and politics. Magaloni et al. use a standard growth regression framework applied to Mexico's 31 states between 1950 and 1995 to measure the economic impact of land reform. They estimate that Mexican GDP per capita would have been 124 percent higher in 1995 had there been no land reform in Mexico. They also provide evidence that the distribution of *ejidal* lands was a key instrument for generating political support for the PRI - Mexico's historically dominant party - and show that land was distributed as a function of the presidential election cycle and social unrest. These results are consistent with large and highly significant municipal-level correlations between land reform and contemporary economic and political outcomes. They are also consistent with the hypothesis that insurgency affects current economic and political outcomes at least in part through land reform. Overall, this evidence suggests that while Mexico's policy of creating *ejidos* was highly inefficient, the government pursued it because it furthered the PRI's goals of reducing instability and maintaining political control.

Additionally, in ongoing work I examine the empirical relationship between agrarian structure and industrial development. I show that while high-productivity, irrigated agricultural areas tended to industrialize, this effect has been muted in regions with a high concentration of *ejidal* lands. Agribusinesses have preferred to locate in areas where they could vertically integrate by purchasing farms, which has not been possible in *ejidal* areas due to the prohibitions on land market transactions (Johnston et al., 1987). Moreover, the federal government, which for much of the 20th century controlled nearly a quarter of the Mexican food processing industry, tended to purchase food and locate state-owned processing industries near well-connected private farmers (Ochoa, 2000). Given that industry is a relatively high-paying sector, differences in industry are likely to translate into disparities in income.

Historical and descriptive evidence also point to large inefficiencies in the *ejidal* sector. Evidence reviewed in Deininger and Bresciani (2001) suggests that land reform reduced long-run agricultural productivity and opened up various possibilities for political manipulation in the distribution of land and credit (see also Varley, 1989; DeWalt, 1979; Wilkie, 1971). By the 1990s, 53% of *ejido* households, as compared to 26% of the total population, earned less than the minimum wage. The land-labor ratio in the private sector was more than double that in the *ejidal* sector, and the median private farm size was more than twice as high as the median farm size in the *ejidal* sector. Private farmers had larger herds, owned more machinery, and were more likely to use improved seeds. As discussed in Deininger and Bresciani, similar results hold even when only nearby municipalities are compared. While measuring agricultural productivity for the sample in this paper is not feasible due to data limitations, this evidence suggests that insurgency may have significantly lowered long-run agricultural productivity. Overall, this study's empirical results highlight the potential for persistent economic inefficiencies to arise when reforms to resolve land disputes extend the state's power by replacing market interactions with political patronage.

3.5.2 Other mechanisms

While I have argued that agrarian reform is an important mechanism linking historical insurgency to modern economic and political outcomes, an alternative hypothesis is that insurgency permanently increases the level of violence and conflict in a community, making it more difficult for the state to monopolize violence in the future. For example, Besley and Reynal-Querol (2012) find that places in Africa that experienced more conflicts during the pre-colonial period have had more civil wars recently. To the extent that violence and instability lower productivity, such forces could reduce economic prosperity in the long-run.

This hypothesis is explored in Table 10. Column 1 examines the municipal homicide rate between 1992 and 2008, Column 2 the number of police deaths caused by confrontations with drug traffickers since late 2006, Column 3 drug trade related homicides since late 2006, and Column 4 the presence of guerrilla insurgents in 2008. None of the IV or OLS coefficients are statistically significant, and the coefficients in columns 1 through 3 are relatively small in magnitude. The coefficient on insurgency in the guerrilla activity regression, equal to 0.13 (s.e.= 0.16), is large but very noisily estimated. Based on these results, it appears unlikely that the impacts of revolutionary insurgency persist primarily through the state's long-run capacity to monopolize violence. As discussed above, I also do not find a large or statistically significant relationship between historical insurgency and the public provision of education, health care, or road infrastructure in recent years.

Another alternative interpretation of the results is that historical drought severity could exert direct impacts on modern outcomes, violating the IV exclusion restriction. To examine this possibility, Table 11 regresses the study's main outcome variables on separate measures of drought severity in 1906-1910 and in all five year periods between 1960 and 1995. The dependent variable in column 1 is the historical insurgency indicator. The dependent variable in column 2 is the percentage of municipal surface area in the *ejidal* sector, the dependent variable in column 3 is the percentage of the municipal labor force working in industry in 2010, the dependent variable in column 4 is log income in 2000, and the dependent variable in column 5 is the percentage of elections held between 1974 and 2009 in which the party controlling the mayorship changed. The coefficient on 1906-1910 drought severity is large and statistically significant in all columns. In contrast, of the 35 drought variables from other periods (7 of which enter each regression), only one is marginally significant. This supports the study's assumption that 1906-1910 drought interacted with specific historical circumstances to produce long-lasting effects through insurgency.

I have also explored droughts occurring between 1925 and 1960. Because weather data for this period is quite limited, it would reduce the sample size substantially to include these drought measures in the regression analysis reported in Table 11. However, I have looked at these periods separately and do not find evidence of a relationship between drought and current outcomes, either for the sub-sample of municipalities that also have weather data for 1905-1910 or for Mexico as a whole (results available upon request).

3.6 Concluding Remarks

This study identifies how insurgency during the Mexican Revolution affected subsequent policies and economic development by using drought severity to instrument for revolutionary activity, conditional on state fixed effects. Instrumental variables estimates document that insurgent municipalities received substantially more land reform in the years following the Revolution and are today poorer, more agricultural, less industrial, and less politically competitive.

Based on the quantitative and historical evidence, I hypothesize that the Mexican state gained a monopoly on violence in rebellious regions through implementing large-scale agrarian reform in the years following the Revolution. Agrarian reform imposed considerable restrictions on redistributed lands and fostered a clientalistic political system dominated by a single party bureaucracy. This study discusses evidence that agrarian reform stifled industrialization and economic growth in places that received more land reform relative to places that received less.

While agrarian reform may be important for promoting stability and reducing inequality in conflicted regions, this study highlights the potential costs when agrarian reform places major restrictions on markets and fosters clientalistic politics. How agrarian disputes can be most effectively resolved remains an important question for future research.

Table 3.1: Summary Statistics

	Mean	S.D.	p10	p90
	(1)	(2)	(3)	(4)
Rainfall	0.78	0.21	0.49	1.00
Insurgency	0.59	0.49	0.0	1.0
Agrarian reform	0.49	0.26	0.13	0.82
Public employees/1,000 inhab. (1940)	20.03	9.5	2.0	25.4
Log income (2000)	7.7	0.9	6.8	8.8
Percent agricultural (2010)	0.07	0.10	0.02	0.45
Percent industrial (2010)	0.25	0.08	0.14	0.37
Percent agricultural (1960)	0.26	0.28	0.16	1.00
Percent industrial (1960)	0.21	0.12	0.01	0.23
Percent agricultural (1940)	0.24	0.19	0.19	0.53
Percent industrial (1940)	0.12	0.09	0.01	0.14
Percent party alternations (1974-2009)	0.24	0.19	0.00	0.50
Percent party alternations (1974-1993)	0.08	0.19	0.00	0.40
Percent party alternations (1994-2009)	0.37	0.27	0.00	0.80

Notes: This table provides the mean, standard deviation, 10th percentile, and 90th percentile for rainfall and for the paper's main outcome variables. *Rainfall* is 1906-1910 precipitation during non-harvest months as a percentage of long-run average precipitation, censored above at one. *Insurgency* is a dummy equal to one if the municipality experienced insurgent activity during 1910-1918 and equal to zero otherwise. *Agrarian reform* is the percentage of a municipality's surface area redistributed through agrarian reform. *Log income* is the log of mean municipal income in 2000. *Percent agricultural (2010)* and *Percent agricultural (1960)* are the percentage of the municipal labor force working in agriculture in the years 2010 and 1960, respectively. *Percent industrial (2010)* and *Percent industrial (1960)* are the percentage of the municipal labor force working in industry in the years 2010 and 1960, respectively. *Percent Agricultural (1940)* is the percentage of municipal male population working in agriculture in 1940, and *Percent Industrial (1940)* is the percentage of municipal male population working in industry in 1940. *Percent party alternations* gives the percentage of elections in which the party controlling the mayorship changed, during the time period listed in parentheses. Sources for all variables are provided in the text.

Table 3.2: First Stage

	Dependent variable is insurgency Drought measured by:					
	Percent normal		Standardized Deviation			
	Across months		Station within 25km		Station within 10km	
	Non-harvest	All mos.	Non-harvest	All mos.	Non-harvest	All mos.
	(1)	(2)	(3)	(4)	(5)	(6)
Rainfall	-0.770***	-0.586***	-0.165**	-0.139**	-0.176**	-0.134
	(0.176)	(0.186)	(0.068)	(0.065)	(0.081)	(0.082)
	[0.183]	[0.183]	[0.066]	[0.063]	[0.081]	[0.083]
F-statistic on drought variables	19.1	9.9	6.0	4.6	4.7	2.7
R-squared	0.449	0.422	0.406	0.398	0.467	0.453
Observations	210	210	182	182	120	120

Notes: Rainfall is a drought severity measure, as described in the column headings, with lower values indicating more severe drought. The dependent variable is insurgency, a dummy equal to one if the municipality experienced insurgent activity during 1910-1918 and equal to zero otherwise. All columns include controls for long-run average precipitation, long-run average minimum and maximum temperature, slope, and elevation, as well as state fixed effects. Robust standard errors are in parentheses. Conley standard errors corrected for spatial correlation are in brackets.

Table 3.3: Compliers

	Dependent variable is insurgency. Sample is:								
	Baseline	Close U.S.	Far U.S.	More agricultural	Less agricultural	More *haciendas*	Less *haciendas*	More indigenous	Less indigenous
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Rainfall	-0.770***	-0.650**	-0.917***	-1.288***	-0.498*	-0.646***	-0.935***	-0.649*	-0.671***
	(0.176)	(0.254)	(0.224)	(0.293)	(0.265)	(0.243)	(0.268)	(0.334)	(0.242)
R-squared	0.449	0.371	0.502	0.534	0.499	0.522	0.475	0.530	0.434
Observations	210	97	113	104	103	106	103	100	105
Mean Dep. Var.	0.59	0.68	0.51	0.58	0.62	0.57	0.62	0.51	0.67

Notes: Rainfall measures 1906-1910 precipitation during non-harvest months as a percentage of long-run average precipitation. The dependent variable is insurgency, a dummy equal to one if the municipality experienced insurgent activity during 1910-1918 and equal to zero otherwise. All columns include controls for long-run average precipitation, long-run average minimum and maximum temperature, slope, and elevation, as well as state fixed effects. The samples are defined in the column headings. Column 1 examines the full sample. Municipalities in states that are closer than the median distance to the U.S. are examined in column 2, whereas municipalities in states farther than the median distance from the U.S. are examined in column 3. Municipalities that had a higher percentage of their male population working in agriculture in 1900 than the median municipality are examined in column 4, whereas municipalities that had a lower percentage of their population working in agriculture as compared to the median municipality are examined in column 5. Municipalities that had a higher percentage of their population living in an *hacienda* in 1900 than the median municipality are examined in column 6, whereas municipalities that had a lower percentage of their population living in an *hacienda* as compared to the median municipality are examined in column 7. Municipalities in which a higher percentage of the population spoke an indigenous language in 1900 than in the median municipality are examined in column 8, whereas municipalities in which a lower than average percentage of the population spoke an indigenous language are examined in column 9. Robust standard errors are in parentheses.

Table 3.4: Placebo Checks

	Insurgent activity	Percent 1900 population agricultural	in *haciendas*	indigenous	literate	Pub. employ. per 1,000 inhab.	Police
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Rainfall	-0.770***	0.089	-0.057	-0.044	0.057	-0.711	-0.260
	(0.176)	(0.122)	(0.052)	(0.063)	(0.047)	(1.108)	(0.603)
F-statistic	19.1						
R-squared	0.449	0.497	0.421	0.573	0.634	0.585	0.362
Observations	210	207	209	205	204	205	205
Mean Dep. Var.	0.58	0.50	0.10	0.06	0.23	3.22	0.98

Notes: Rainfall measures 1906-1910 precipitation during non-harvest months as a percentage of long-run average precipitation. All columns include controls for long-run average precipitation, long-run average minimum and maximum temperature, slope, and elevation, as well as state fixed effects. The dependent variable in column 1 is insurgency, a dummy equal to one if the municipality experienced insurgent activity during 1910-1918 and equal to zero otherwise. The dependent variable in column 2 is the percentage of male municipal population working in agriculture in 1900, in column 3 it is the percentage of municipal population living in *haciendas* in 1900, and in column 4 it is the percentage of municipal population that was literate in 1900. The dependent variable in column 5 is public employees per 1,000 municipal inhabitants in 1900, and in column 6 it is the number of police per 1,000 municipal inhabitants in 1900. Robust standard errors are in parentheses.

Table 3.5: Agrarian Reform

	Full sample	More agricultural in 1900	Less
	(1)	(2)	(3)
Panel A: IV			
Insurgency	0.218*	0.311***	0.052
	(0.111)	(0.110)	(0.234)
Panel B: OLS			
Insurgency	0.100***	0.146***	0.087
	(0.037)	(0.054)	(0.060)
Observations	210	104	103
Mean Dep. Var.	0.49	0.52	0.45

Notes: Insurgency is a dummy equal to one if the municipality experienced insurgent activity during 1910-1918 and equal to zero otherwise. The dependent variable is the percentage of municipal surface area redistributed by agrarian reform. Panel A reports instrumental variables estimates, with insurgency instrumented by 1906-1910 precipitation during non-harvest months as a percentage of long-run average precipitation. Panel B reports ordinary least squares estimates. All columns include controls for long-run average precipitation, long-run average minimum and maximum temperature, slope, and elevation, as well as state fixed effects. The samples are defined in the column headings. Column 1 examines the full sample. Municipalities that had a higher percentage of their male population working in agriculture in 1900 than the median municipality are examined in column 2, whereas municipalities that had a lower percentage of their population working in agriculture as compared to the median municipality are examined in column 3. Robust standard errors are in parentheses.

Table 3.6: Public Employees

	All	Federal	State	Local	Teachers per 1000 students	Local taxes per income
	authorities/1000 inhabitants in 1940					
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: IV						
Insurgency	-5.648	-3.769	-0.811	0.298	-3.694	-0.001
	(4.564)	(3.276)	(1.848)	(0.750)	(4.746)	(0.001)
Panel B: OLS						
Insurgency	-4.134***	-2.880**	-0.748	-0.827***	-0.400	-0.001
	(1.571)	(1.129)	(0.638)	(0.242)	(1.325)	(0.001)
Observations	205	205	205	205	205	205
Mean Dep. Var.	20.03	16.27	2.436	1.104	53.89	0.002

Notes: Insurgency is a dummy equal to one if the municipality experienced insurgent activity during 1910-1918 and equal to zero otherwise. Panel A reports instrumental variables estimates, with insurgency instrumented by 1906-1910 precipitation during non-harvest months as a percentage of long-run average precipitation. Panel B reports ordinary least squares estimates. All columns include controls for long-run average precipitation, long-run average minimum and maximum temperature, slope, and elevation, as well as state fixed effects. The dependent variable in column 1 is total public employees per 1,000 municipal inhabitants in 1940, in column 2 it is federal employees per 1,000 inhabitants in 1940, in column 3 it is state employees per 1,000 inhabitants in 1940, and in column 4 it is local employees per 1,000 inhabitants in 1940. The dependent variable in column 5 is school teachers per 1,000 school-age children in 2005, and in column 6 it is municipal tax receipts per dollar of municipal income in 2005. Robust standard errors are in parentheses.

Table 3.7: Economic outcomes today

	Overall	Agricultural	Industrial	Services	Percent	
		log wage			no water	no electricity
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: IV						
Insurgency	-0.292**	-0.322	-0.289*	-0.218**	14.095**	2.922*
	(0.141)	(0.274)	(0.169)	(0.109)	(6.255)	(1.657)
Panel B: OLS						
Insurgency	-0.109***	-0.082*	-0.122***	-0.086***	0.715	0.404
	(0.021)	(0.044)	(0.021)	(0.019)	(1.603)	(0.465)
Observations	734,127	53,363	222,267	458,497	210	210
Clusters	210	210	210	210	210	210
Mean Dep. Var.	7.72	7.13	7.73	7.78	11.12	3.32

Notes: Insurgency is a dummy equal to one if the municipality experienced insurgent activity during 1910-1918 and equal to zero otherwise. Panel A reports instrumental variables estimates, with insurgency instrumented by 1906-1910 precipitation during non-harvest months as a percentage of long-run average precipitation. Panel B reports ordinary least squares estimates. All columns include controls for long-run average precipitation, long-run average minimum and maximum temperature, slope, and elevation, as well as state fixed effects. The dependent variable in columns 1 through 4 is log income in 2000. The sample in column 1 includes all individuals earning positive income, in column 2 it includes individuals working in agriculture, in column 3 it includes individuals working in industry, and in column 4 it includes individuals working in services. In column 5 the dependent variable is the percentage of households in a municipality who lack access to running water, and in column 6 it is the percentage of households who lack access to electricity. Robust standard errors, clustered by municipality, are in parentheses.

Table 3.8: Economic organization

	Percent labor force in				Percent male pop. in	
	Ag	Industry	Ag	Industry	Ag	Industry
	2010		1960		1940	
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: IV						
Insurgency	0.206***	-0.060*	0.171	-0.077	0.164*	-0.094**
	(0.077)	(0.037)	(0.117)	(0.050)	(0.086)	(0.040)
Panel B: OLS						
Insurgency	0.059***	-0.020*	0.086**	-0.078***	0.069**	-0.036***
	(0.020)	(0.011)	(0.043)	(0.018)	(0.380)	(0.170)
Observations	210	210	190	190	188	188
Mean Dep. Var.	0.07	0.25	0.26	0.21	0.243	0.127

Notes: Insurgency is a dummy equal to one if the municipality experienced insurgent activity during 1910-1918 and equal to zero otherwise. Panel A reports instrumental variables estimates, with insurgency instrumented by 1906-1910 precipitation during non-harvest months as a percentage of long-run average precipitation. Panel B reports ordinary least squares estimates. All columns include controls for long-run average precipitation, long-run average minimum and maximum temperature, slope, and elevation, as well as state fixed effects. The dependent variables in columns 1 and 3 are the percentage of the municipal labor force working in agriculture in the years 2010 and 1960, respectively. The dependent variables in columns 2 and 4 are the percentage of the municipal labor force working in industry in the years 2010 and 1960, respectively. The dependent variable in column 5 is the percentage of municipal male population working in agriculture in 1940, and the dependent variable in column 6 is the percentage of municipal male population working in industry in 1940. Robust standard errors are in parentheses.

Table 3.9: Political competition

	Percent alternations		
	74-09	94-09	74-93
	(1)	(2)	(3)
Panel A: IV			
Insurgency	-0.327***	-0.382***	-0.210*
	(0.101)	(0.143)	(0.111)
Panel B: OLS			
Insurgency	-0.028	-0.050	0.006
	(0.031)	(0.048)	(0.037)
Observations	205	205	203
Mean dep. var	0.244	0.369	0.088

Notes: Insurgency is a dummy equal to one if the municipality experienced insurgent activity during 1910-1918 and equal to zero otherwise. Panel A reports instrumental variables estimates, with insurgency instrumented by 1906-1910 precipitation during non-harvest months as a percentage of long-run average precipitation. Panel B reports ordinary least squares estimates. All columns include controls for long-run average precipitation, long-run average minimum and maximum temperature, slope, and elevation, as well as state fixed effects. The dependent variable is the percentage of elections in which the party controlling the mayorship changed, during the time period listed in parentheses in the column headings. Robust standard errors are in parentheses.

Table 3.10: Violence today

	Homicides 92-08	Police deaths	Drug trade homicides	Guerrillas 2008
	(1)	(2)	(3)	(4)
Panel A: IV				
Insurgency	0.441	-0.578	-0.008	0.130
	(0.564)	(0.745)	(0.042)	(0.158)
Panel B: OLS				
Insurgency	-0.080	-0.194	-0.014	0.032
	(0.168)	(0.382)	(0.015)	(0.056)
Observations	210	210	210	210
Mean dep. var	1.544	0.750	0.058	0.118

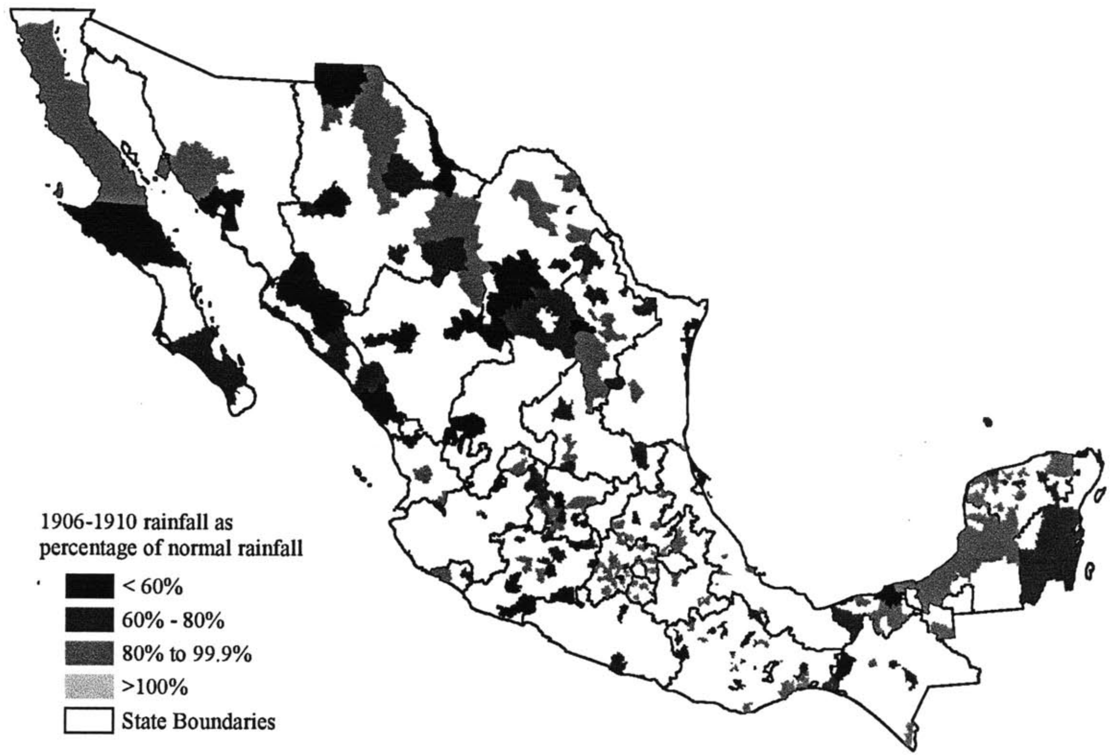
Notes: Insurgency is a dummy equal to one if the municipality experienced insurgent activity during 1910-1918 and equal to zero otherwise. Panel A reports instrumental variables estimates, with insurgency instrumented by 1906-1910 precipitation during non-harvest months as a percentage of long-run average precipitation. Panel B reports ordinary least squares estimates. All columns include controls for long-run average precipitation, long-run average minimum and maximum temperature, slope, and elevation, as well as state fixed effects. The dependent variable in column 1 is the average municipal homicide rate between 1992 and 2008, in column 2 it is the number of police deaths caused by confrontations with drug traffickers since late 2006, in column 3 it is the average rate of drug trade related homicides since late 2006, and in column 4 it is a dummy equal to 1 if guerrilla insurgents were present in the municipality in 2008 and equal to zero otherwise. Robust standard errors are in parentheses.

Table 3.11: Droughts in other periods

	Insurgency	% ejido	% agricultural	log income	% alternations
	(1)	(2)	(3)	(4)	(5)
Rainfall 1906	-0.793***	-0.182*	-17.490**	0.153**	0.240***
	(0.230)	(0.089)	(8.382)	(0.072)	(0.072)
Rainfall 1960	0.425	0.161	12.114	-0.038	-0.082
	(0.289)	(0.186)	(12.781)	(0.141)	(0.148)
Rainfall 1965	-0.014	0.134	47.100	0.075	-0.332
	(0.456)	(0.406)	(33.486)	(0.261)	(0.307)
Rainfall 1970	0.247	0.245	-21.345	-0.112	0.135
	(0.715)	(0.280)	(19.224)	(0.150)	(0.234)
Rainfall 1975	0.072	0.251	-26.571	0.412	-0.244
	(0.527)	(0.513)	(32.687)	(0.257)	(0.146)
Rainfall 1980	0.586	0.077	3.426	-0.411*	0.155
	(1.002)	(0.368)	(16.466)	(0.205)	(0.289)
Rainfall 1985	0.245	-0.299	-20.326	0.064	0.219
	(0.424)	(0.368)	(30.188)	(0.180)	(0.247)
Rainfall 1990	0.116	0.112	29.701	-0.079	-0.336
	(0.547)	(0.308)	(19.970)	(0.173)	(0.267)
Observations	210	210	210	733,153	205
R-squared	0.467	0.460	0.340	0.357	0.358

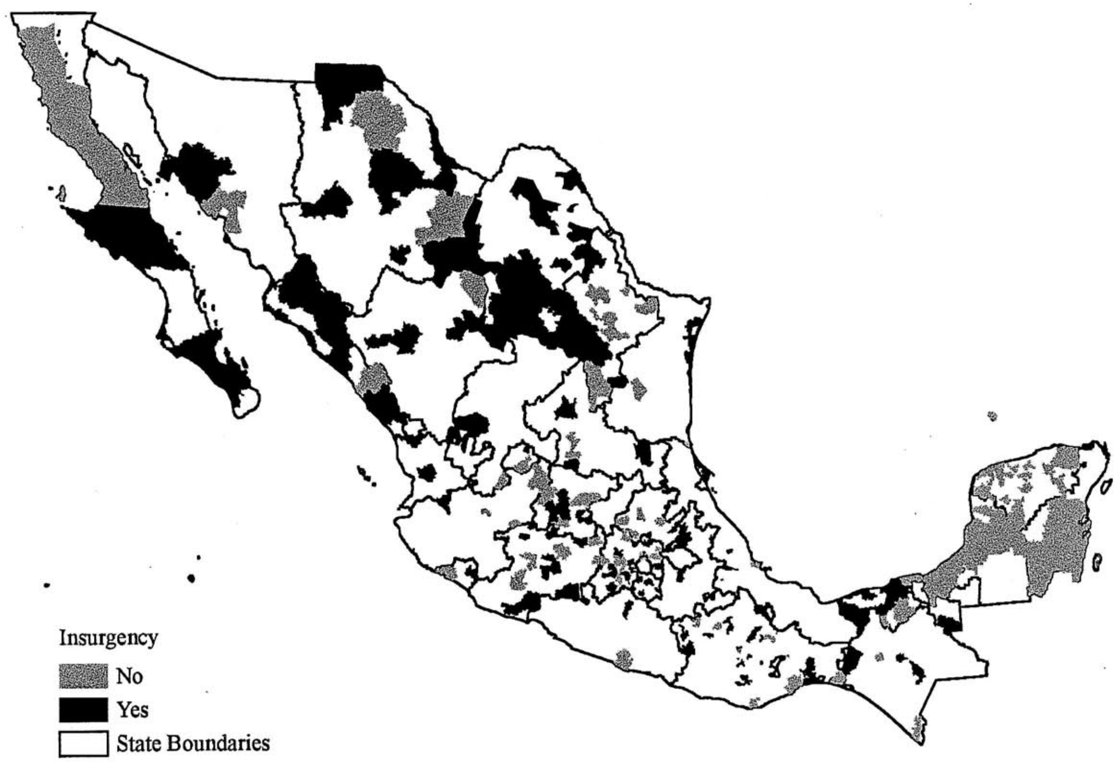
Notes: Rainfall measures precipitation during non-harvest months as a percentage of long-run average precipitation for the respective period. All columns include controls for long-run average precipitation, long-run average minimum and maximum temperature, slope, and elevation, as well as state fixed effects. The dependent variable in column 1 is insurgency, a dummy equal to one if the municipality experienced insurgent activity during 1910-1918 and equal to zero otherwise. The dependent variable in column 2 is the percentage of municipal surface area in the *ejidal* sector, the dependent variable in column 3 is the percentage of the municipal labor force working in agriculture in 2010, the dependent variable in column 4 is log income in 2000, and the dependent variable in column 5 is the percentage of elections held between 1974 and 2009 in which the party controlling the mayorship changed. Robust standard errors are in parentheses.

Figure 3-1: Drought Severity



Notes: 1906-1910 precipitation as a percentage of long-run average precipitation is plotted, following the scheme given in the legend. State boundaries are in black.

Figure 3-2: Insurgency



Notes: Insurgency during 1910-1918 is plotted, following the scheme given in the legend. State boundaries are in black.

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