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Essays in the Political Economy of Development

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Essays in the Political Economy of Development

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An abstract of
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Abstract

Essays in the Political Economy of Development
By Donald Grasse

This dissertation studies the relationship between economic development and violent conflict in three separate essays studying different cases: Cambodia, Indonesia, and Brazil.

The first asks: does mass repression have a long-term economic legacy, and if so, what explains persistence? I argue repression can undermine development by delimiting human capital. I study the aftermath of the Khmer Rouge in Cambodia. I exploit an arbitrary border that allocated villages to either the loyalist Mok or the relatively moderate Sy in Kampong Speu province, which shaped the intensity of repression. Using a regression discontinuity design, I find villages in the more extremist Southwest zone are poorer today compared to villages in the adjacent West zone, and had lower human capital immediately after the regime. Exposure to more intense repression shapes labor markets and child health, explaining intergenerational persistence. I find no conclusive evidence for other persistence channels.

The second asks when do agricultural transformations impact social stability? I argue agricultural booms may spur violent conflict over resource allocation by pitting would-be producers against incumbent landowners when the gains from production are concentrated and the negative externalities are diffuse. I study the rapid expansion of oil palm in Indonesia. I find when oil palm grows more valuable and expands within producing districts, violent resource conflicts increase.

The third asks: does public insecurity have deep historical roots? I argue colonization led to a path dependent alliance between the state's security sector and economic elites, spurring high crime rates and social polarization. I leverage a geographic discontinuity in colonial state presence in Brazil, which was determined by the Treaty of Tordesillas, for identification. Municipalities east of the Tordesillas line had more slavery in 1872, and are more proximate to historical revolts and run-away slave communities (quilombos). Today, municipalities east of the line have higher homicide rates, more police killings of civilians, higher expenditure on security, and a higher probability of having an auxiliary police force. They are also more economically segregated and more likely to have informal housing settlements.

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Chapter 1

Developmental Legacies of Draconian Dictatorship: Evidence from the Khmer Rouge

1 Introduction

Dictatorships often engage in mass repression to control society. Several authoritarian regimes have demographically targeted repression to purge intellectuals, the educated, or middle class members of society to weaken opposition movements. Whereas the political legacies of such repression are more often studied [17, 137, 163], the long-run developmental consequences - and the mechanisms of persistence - are less clear [56]. Does mass repression have long-term developmental effects, and if so, why do the consequences persist?

I argue repression of educated members of society and skilled laborers undermines long-term economic development by undermining human capital. A paucity of skilled intellectuals in the aftermath of state violence places communities on diverging development paths: places more adversely impacted compensate for low-education levels

with behaviors that reinforce low income - such as underinvesting in education, health, and specialized training - since doing so is too costly when starting from a level of extreme poverty and low education. The consequence of mass repression is a poverty trap.

Identifying the long-term effect of repression on economic welfare is challenging because state violence is strategically allocated. Both cross-national and within-country analyses may be biased by intervening events, diverging pre-trends, or the endogeneity of repression to local economic conditions [180].

I surmount identification issues by exploiting an administrative redistricting during the Democratic Kampuchea (DK) regime in Cambodia, which placed similar, nearby villages into the control of radically different cadres. DK divided a single combat theater into two governing zones, the West and the Southwest, along National Highway 4 (NR4) in Kampong Speu province. Villages on either side of the natural border were then governed by either Ta Mok in the Southwest zone - a brutal loyalist to the extreme doctrine of the dictator Pol Pot - or Sy in the West, who was a relatively more moderate communist.[1] Conditional on village proximity to the border, villages were arbitrarily assigned exposure to more or less intense state repression.

Using a geographic regression discontinuity design, I find villages in the more repressive zone are significantly poorer today (0.42σ, 20% of the control group mean). The results replicate while using administrative and nighttime lights data, at the village and individual level, and at different points in time. The estimates suggest a persistent wealth gap between former zones. The results are robust to alternative estimation windows, weighting kernels, and are not likely to be an artifact of spatial noise [117].

[1] I use moderate and relatively moderate interchangeably in some places. Note that Sy was moderate in comparison to extremists within Pol Pot's regime; since he was a communist insurgent, he cannot be considered a moderate in an absolute sense.

Next, I evaluate channels of persistence. I argue repression that demographically targets the educated, which is not uncommon in authoritarian contexts or under coerced labor regimes more generally, creates a skill gap between generations, leaving a lacuna of trained and schooled individuals in a locality. The consequences reverberate overtime through a poverty trap mechanism, wherein people in poverty take actions that keep them poor because of constraints created by their lower income status.

I show evidence consistent with the poverty trap explanation, wherein the human capital shock from the regime's repression reinforces behavior that keeps income lower in affected areas. I find literacy and education rates are much lower in the former Southwest (0.50 σ) in 1998. Specifically, I show age cohorts whose secondary schooling years overlapped with the Khmer Rouge experience the largest drop in education in the former Southwest (extreme) zone. Consistent with qualitative evidence that links generational skill gaps to contemporary Cambodian poverty [110, 118], I find persons living in the former Southwest are more likely to be informally employed, earn less, and have lower productivity. Further, I show intergenerational consequences by finding child health is lower in the Southwest, which is a key predictor of future socioeconomic status.

I draw from a variety of data sources to evaluate other causal mechanisms linking poverty to historical repression. Scholarship on the developmental legacies of coercive institutions suggest several persistence channels, including institutional path dependence (political competition, public goods provision [63], conflict over property rights [94]), and cultural persistence (social trust [135]).

Using data on elections, newly collected data on land disputes, survey data on community trust, and georeferenced data on public goods access, I find no evidence of persistence via commonly cited pathways. The finding suggests poverty traps are a unique channel that may be added to the library of mechanisms linking coercion to contemporary development.

My study contributes to three sets of literature. First, I add to debates about the legacies of state repression [17, 137, 163]. As [56] recently argue, "substantially more research is needed to uncover whether and how repression hurts economies." Research on the developmental impact of repression is divided. Some show positive impacts of state violence for decedents of victims and surrounding communities [26, 187], while others show null [46, 162] or negative long-term effects [7, 132, 143, 146, 193].

My study shows repression can cause underdevelopment when victims are targeted in a way that changes underlying factors of production - specifically human capital. I highlight how the state's objectives during repression determine the direction of the relationship. The argument is also related to the large literature on the economic consequences of civil war, where scholars debate whether civil conflict can improve economic well-being by reducing inequality [169], or if it undermines development by reducing human capital [30, 44, 113].

Next, my findings elucidate the mechanisms by which coercive institutions have persistent economic effects [50, 176]. The durable impacts of historical institutions have been well-established, but the pathways by which defunct coercive regimes continue to shape development are ambiguous. Using a variety of data sources, I test several plausible candidate causal pathways to show the uniqueness of the poverty trap mechanism. Understanding persistence channels is critically important because it implies much different solutions to persistence; for instance, technical interventions such as cash transfers may better address poverty traps, whereas fundamental reforms are required to address institutional path dependence.

Finally, my study highlights the long-term importance of regional executives in dictatorships [42, 159, 166]. Authoritarian politics scholars have increasingly highlighted the importance of subnational administration in autocracies, especially how dictators delegate control to loyalists. My study shows the consequences of regional executive loyalty in repressive regimes are substantial and persistent, providing an

additional normative impetus for understanding how autocrats manage agency problems with their subnational subordinates.

2 State Repression and Economic Development

Repression is the "original sin" of dictatorship [182, p.10]. State violence has clear short-term economic implications: population transfers, mass killings, or detentions impact local labor markets along with individuals and their families. However, since such violence is transitory, effects may only persist if the fundamentals of economic growth - land, labor, capital, or social and formal institutions - change as a result of repression.

Since the economic legacy of repression is contingent on changes in factors of production, scholars have found mixed evidence in a variety of contexts. Some show positive impacts of state violence for decedents of victims and surrounding communities due to changing preferences [26] or relocation [187]. Genocide may increase development in the short-run through property theft [46], or Malthusian channels [162], however, effects do not persist. Others document negative legacies, either leveraging instrumental variables [143, 146, 193] or selection on observables [7] for identification.

Understanding the legacy of repression on development requires a particular focus on the technology of coercion along with who the state is attempting to victimize. Transitory episodes of mass arrests that target citizens indiscriminately may not permanently alter underlying factors of production, whereas lethal repression that targets persons based on certain socioeconomic traits may have more lasting effects by changing the composition of the labor force, creating multiple developmental equilibria.

A common form of demographically targeted repression is coercion of educators, educational institutions, and middle income persons. Several authoritarian regimes have demographically targeted repression in a manner intended to eliminate higher

income or educated segments of society. Franquist Spain purged public school teachers [18] as did Mao Zedong's Anti-Rightist Campaign in China [193]. Francisco Macias Nguema's rule of Equatorial Guinea included the closure of all private schools and the use of repression to support coerced labor [75]. Joseph Stalin's dekulakization was intended to "break the back of the independent peasantry" [144, p.54], and targeted more middle-income peasants. In the early stages of genocide, the Ottoman Empire and Nazi Germany both targeted intellectuals within minority groups.

Even less extreme dictatorships have targeted skilled workers and educators. Argentina's last dictatorship targeted secondary schools and architects, due to the regime's belief that persons in these sectors produced subversives [161, p. 228-230]. Pinochet's dictatorship in Chile similarly defunded universities, creating a crunch on higher education [23].

Repression of the educated, however, is not a thing of the past: Hungary, Turkey, and Zimbabwe have all engaged in state repression of higher education and teachers.[2] Outside of dictatorships, insurgent groups including the Islamic State (IS), the Free Aceh Movement (GAM), the Shining Path, the Taliban, and anglophone separatists in Cameroon, have similarly repressed educated persons and formalized schooling.

Repression that demographically targets more educated, intellectual, and professional classes may directly undermine human capital in a locality by eliminating the learned members of society, causing developmental divergences through poverty traps. Poverty traps are situations where poverty is so extreme that individuals cannot afford to take poverty-reducing actions. For instance, if education levels are already

[2]Suzy Hansen. " 'The Era of People Like You Is Over': How Turkey Purged Its Intellectuals." New York Times. July 24, 2019. https://www.nytimes.com/2019/07/24/magazine/the-era-of-people-like-you-is-over-how-turkey-purged-its-intellectuals.html. Jennifer Rankin. "'How dictatorship works': Hungarian academic quits in censorship row." The Guardian. 11-30-2021. https://www.theguardian.com/world/2021/nov/30/hungarian-academic-andrea-peto-quits-in-censorship-row. Barbara Wall. "Teachers flee poverty and repression : Zimbabwe brain drain." New York Times. 02-12-2002. https://www.nytimes.com/2002/02/12/news/teachers-flee-poverty-and-repression-zimbabwe-brain-drain.html.

low and individuals are poor, the opportunity cost of seeking additional training may be larger than the return to schooling, since another year spent in school means a year out of the labor market [15]. Alternatively, low income and education may force an individual into a low-earning job, since the time and effort required to find alternative means of employment are too expensive for the already poor. Poverty may lead individuals to become unhealthy, and worse health could reinforce low income [19]. This explanation posits that multiple developmental equilibria exist, and that a locality or person may find themselves in one equilibrium versus another due to path-dependent processes sprung by historical factors.

Repression may undermine development through a variety of other mechanisms. Coercion can change social capital [150, 135, 132], formal government institutions [6], local elite strength [63], or cause property conflict [94], reducing economic development. The variety of possible mechanisms and related papers are displayed in Table 1.1.

3 Context: Democratic Kampuchea (DK)

I study a historically important case of demographically targeted repression: the Democratic Kampuchea (DK) regime in Cambodia. During the regime's short tenure of less than four years, nearly one in five Cambodians are estimated to have died. Due to the gravity of the event, it is important to understand the impacts of the DK regime in its own right.

Aside from its intrinsic importance, the DK case is of interest because it presents an opportunity to uniquely identify multiple equilibria - poverty traps - as the causal mechanism connecting state repression to long-term development. The observable implications of poverty traps could be explained by institutional or cultural changes that occur as a result of state violence. Many coercive regimes institute extractive

Table 1.1: Causes of Persistent Underdevelopment from Coercive Institutions*

Category	Theory	Literature	Empirical Implications
Multiple Equilibria	Poverty trap	Present paper	Lower human capital intergenerational poverty
Institutions		[6] [63] [94]	Elite capture, Property conflict, Fewer public goods
Culture	Social Capital	[150], [135] [132]	Difference in generalized trust†

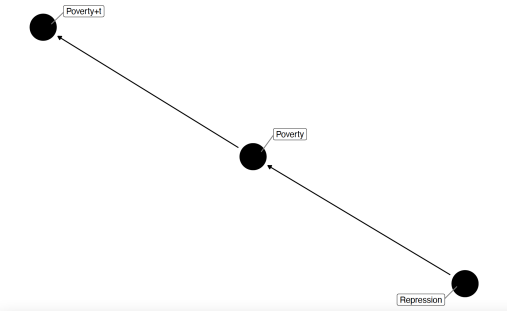
† Scholars are divided on the direction of the trust effect after exposure to coercive institutions; [135] argue the legacy of violence after the Congo Free State increased trust, in line with the large literature on civil conflict legacies, whereas [150] and [132] argue coercion reduced trust in the long-term.
Three categories drawn from [149].

rules of the game that outlast the regime, are controlled by the same economic elites overtime, and reshape cultural norms. These forces may link patterns of individual behavior that reinforce poverty, such as underinvesting in health and education, rather than poverty traps, which are individual feedback loops. Figure 1.1 outlines the logic: in all three subfigures, poverty in the past shapes poverty in the future (poverty+t), but in Panels B and C the repression shapes poverty overtime through its impact on culture and institutions.

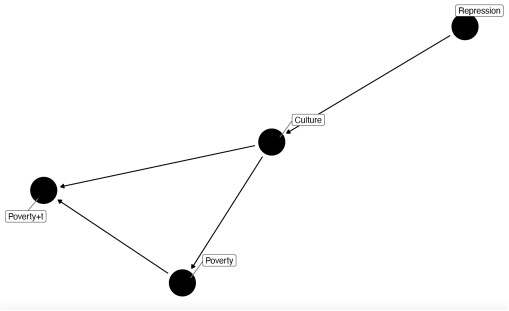
In the DK case, institutional and cultural persistence are implausible explanations for the persistence of poverty from repression at the subnational level. The typical institutions-based story is implausible because formal rules created by DK did not survive the regime, and the extreme faction of DK was supplanted by the moderate

Figure 1.1: Theoretical Pathways Linking Repression to Long-run Poverty

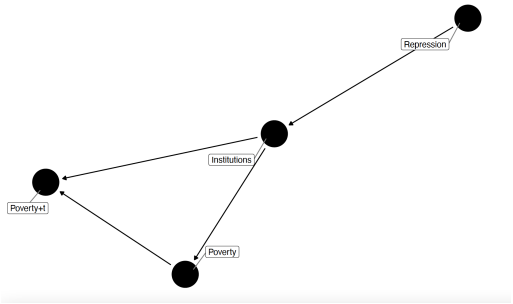
(a) Poverty Trap (Multiple Equilibria)



(b) Institutional Path Dependence



(c) Cultural Persistence



Notes: Directed Acyclic Graphs diagraming competing causal pathways linking repression to long-run poverty. Multiple equilibria via the poverty trap, wherein repression causes poverty which feedbacks into itself over time, is depicted in Panel A. Institutional and Cultural channels (Panels B and C) illustrate paths where poverty feeds into itself, but culture and institutions explain persistent effects. Persistence is denoted with the Poverty+t for any t$>$ 0.

faction, now represented by the Cambodian People's Party (CPP). Next, since the identifying variation I explore is in terms of intensity rather than existence, it is unlikely that cultural norms sharply diverge based on subnational exposure to violence: to the extent experiencing horrific violence can alter fundamental beliefs about the trustworthiness of others, some exposure to mass repression is likely sufficient to generate changes in trust. In Section 6, I examine the plausibility of all three causal diagrams outlined in Figure 1.1.

In the next four subsections, I explain the historical context behind the regime, motivating the identification strategy and the channel of persistence.

3.1 DK: Administration and Legacy

From 1975-1979, DK abolished private property and currency, collectivized agriculture, closed formal schools, and forced citizens into highly stratified labor groups according to their age, sex, residence, and educational background. The regime's development strategy hinged on a significant increase in rice production, which would be supported by building irrigation infrastructure through forced labor. Nominally, the regime pursued a mass literacy campaign, but labor requirements and the purging of former educators served a cross purpose. The regime relied on coercion to enact its social reorganization [24, 125, 188].

The DK regime was ideologically fragmented on a spectrum of moderate communists to repressive extremists. Fissures were consequential; after capturing Phnom Penh, it took months for combat commanders to come together to form a government. The fractionalization across ideology was highly regionalized: during the civil war from 1970-1975, commanders operated largely in isolation, even wearing different uniforms.

3.2 DK Zones: The Southwest and West

To manage tensions between regional commanders, the central party leadership divided the country into several different zones, which closely overlapped with combat theaters and at times ignored pre-existing provincial boundaries. The leaders of zones were called zone secretaries. Zones were organized with rigid hierarchy internally, but the ability of the central government to coordinate policy between zones was constrained by the influence of the zone secretaries within their territory [188, p.68]. Mass killing directives were handed down by Party leadership, but zone secretaries ultimately carried out orders [68, p.126]. Since secretaries had a great deal of de facto authority, the implementation of regime mandates varied across zones based on the idiosyncrasies of personal leadership [24, p.176].

I focus on a salient border that divided very different commanders: Mok in the Southwest and Sy in the West. Mok and Sy shared jurisdiction during the civil war, and their two respective zones were one area of operation during the civil war. After a major dispute concerning the brutality of Mok's approach to the revolution, their jurisdiction was divided into two separate zones which they commanded in isolation, divided along National Road 4 (NR4).

The border road - NR4 - was constructed cheaply by the United States in the 1950s, with the goal of connecting the port city of Sihanoukville to the capital Phnom Penh in the least costly way possible. In Congressional hearings about the US's construction of the road, officials concede "[t]he principal justification was a political justification" unrelated to economics: the United States constructed the road as a favor to France and as a way to reduce Cambodian dependency on Vietnam for trade. Indeed, one engineer remarked "[t]he decision to give support to the construction of this highway in the first instance was not based on detailed studies of such matters as the volume of traffic and the precise economic benefits expected to result, nor indeed were they considered to be the determining factors under the circumstances" [102]. Consistent with a lack of meticulous planning, the road quickly fell into disrepair, straining the US-Cambodian relationship [103].[3]

Whereas the idea of the road was driven by political considerations, the exact location was less of a reflection of local economics and more of a matter of convenience. Figure 1.2 maps the Southwest and West zone. Binding geographic constraints outside of Kampong Speu (dashed lines) caused the road to bisect the province. The simplest route to connect the port and the capital was through a narrow stretch of flat land, across Kampong Speu. Building the road slightly differently would have led to the route to cross over elevated terrain (dark shading), adding cost and complexity to the

[3]Prince Sihanouk was so "disgusted" with the roads conditions he once took a helicopter after hitting potholes on a drive. Time Magazine. July 1961. Accessed January 20, 2022. `http://content.time.com/time/subscriber/article/0,33009,897811,00.html`

project. As such, the location of the highway is largely arbitrary within Kampong Speu, since the terrain features that induced the road to be built in such a way are beyond the borders of the province. Within Kampong Speu, terrain, river, and road access appear largely similar on either side of NR4.

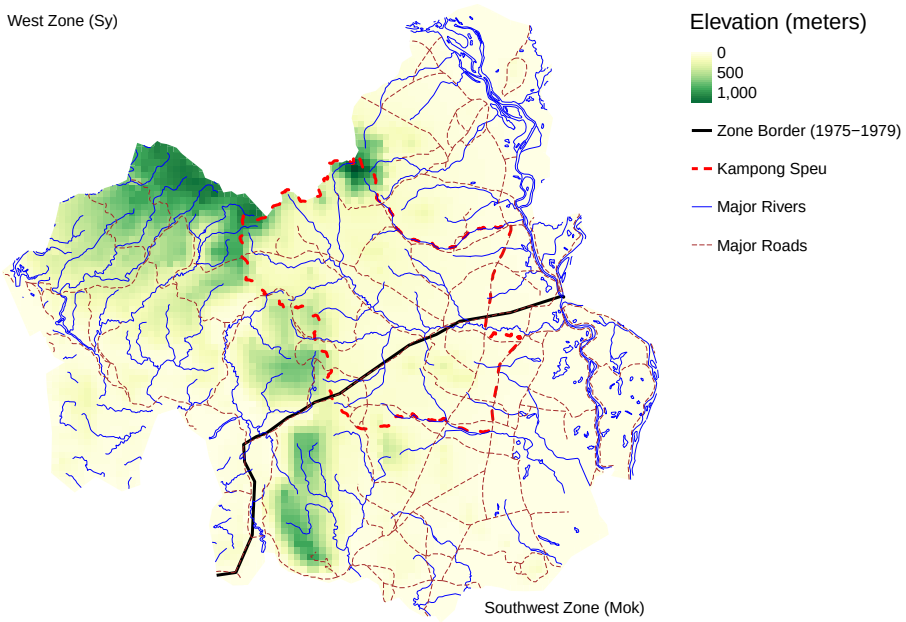
The road was a natural choice for a border for DK since it provided a visible marker to divide rivals. Clear lines of demarcation of authority between areas was important because zone leaders were conflictual - trespassing on zones were known to cause skirmishes between troops in places were lines were less clear, such as the boundary between the Southwestern and Eastern zone [188, p.77]. One natural solution to this problem was the use of natural landmarks as borders, like roads, which DK also used to divide smaller Damban regions such as Damban 1 and 4 in the Northwest zone [188, p.111], or rivers, such as the Mekong, which divided the North-Central and East Zone in Kampong Cham.

3.3 Repression in the Southwest versus West

Both primary and secondary sources, along with descriptive quantitive evidence, point to a difference in repression intensity between the West and Southwest zones. Whereas Mok is credited with being among the most ruthless towards "new people" - individuals who were educated, urbanites, or in other ways connected to the Lon Nol or Sihanouk regime - Sy in comparison was less brutally violent towards these groups.

Sy and Mok are illustrative of the ideological divide within DK, which shaped how violent and repressive their rule was during the regime [24, 51]. Sy was an intellectual, member of old reformist socialist groups, and less ruthless than the more extreme factions. In contrast to Mok and other extremists, he believed old regime members could be incorporated into DK rather than executed. During the civil war, interviewees report "Sangha Hoeun and Chou Chet [Sy] re-educated and taught these people [Lon Nol soldiers]. I saw this; they did not kill them. But Mok did kill such

Figure 1.2: West and Southwest Zones During Democratic Kampuchea Regime



Note: Zone border from Yale Genocide Studies Program `https://gsp.yale.edu/dk-zones-english`. Shading shows elevation (in meters) in grid cells across the zones. Red dashed line is the provincial boundary of Kampong Speu, which was salient pre and post (but not during) the DK era. The thick black line shows the border between the West and Southwest zone, drawn over National Road 4, which was only a political border during the DK era. Brown dashed lines are major roads, thin blue lines are rivers.

people, and he became angry with what the other two were doing" [124].

Mok executed the moderate communist Sangha Hoeun, which created a strong division between him and Sy: an interviewee reported Sy "...didn't agree with that [the execution of Sangha Hoeun] so he was transferred and the Southwest Zone was divided into two, and the Western Zone created [122]. Consistent with Sy's approach

of incorporating old regime members rather than executing them, party meeting reports in 1977 indicate that a "fair number" of cooperatives were staffed by individuals who were not members of the peasant class in Sector 32 of the West [41, p.86]. Sector 32 of the West was Sy's headquarters, which covered the half of Kampong Speu that was split into his zone. Staffing cooperatives with non-peasants is suggestive of Sy choosing to incorporate the social groups that Mok executed into government [4]

Primary source interviews corroborate the qualitative differences in cruel repression between Mok and Sy. One interviewee remarked "Mok was cruel since 1971-1972. Different from Chou Chet [Sy] and Phouk Chlag. Mok was fierce (khlang), a killer [120]."[5] A former Communist Party of Kampuchea (CPK) district chief expanded upon how the differences in ferociousness - *khlang* - impacted the intensity of DK cruelty: he said Sy was "not very harsh (khlang)...Sy didn't set targets: when he took me to Tonle Sap, on the boat with me, he said 'friend, grow 1,000 ha of rice here, if there is enough water. If water is short, well, it depends on the concrete situation.' " [121]. Given the excessive death caused by unrealistic project goals during the regime, this distinction suggests a critical difference in repression intensity. One interviewee who lived in Sector 32 of the West - Sy's headquarters - reports "no killings" in 1975, and described the area as "softer" since "they didn't kill many people in R32" [123].

Meanwhile, the Southwest committed most to the development of cooperatives according to the revolution's ideology [125, p.87-90]. The zone was the "power core" of the regime [188, p.86]. Mok staffed his zone with close family members, who would remain loyal to his orders. Sector 33, run by Mok's son, exemplifies the pattern: the area, in modern Kampong Speu province, was "the toughest sanctuary of the Khmer Rouge movement" [188, p.98]; affiliates of the old regime were forced to work in the fields [188, p.93] and refugee reports suggest "new people" received less rations [188, p.99]. A US embassy report quoted a refuge who said "[a]n error, if discovered, means

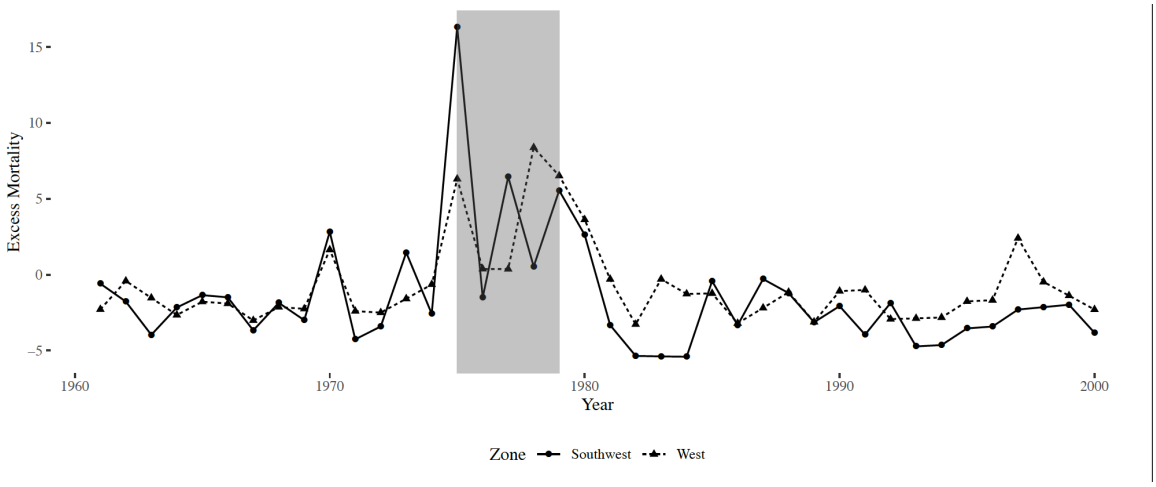
[4]Further detail on both secretaries in SI A.6.1.

[5]Chou Chet was another nome de guerre of Sy.

death in the south" [125, p.194].

Descriptive evidence is consistent with interviewee accounts of the differences between Mok and Sy. I use data on sibling deaths from the 2000 Demographic Health Survey round to estimate annual mortality trends by zone in Kampong Speu. I develop a predictive model of mortality based on gender, age, and residence (out of sample) and then use the model to compute expected mortalities by year. Figure 1.3 plots the difference between observed and expected mortalities by zone. In 1975, there is a differential spike in excess mortality in the Southwest, which corresponds with the year Pol Pot gave the order to begin murdering the educated and intellectuals indiscriminately [125].[6]

Figure 1.3: Estimated Mortality Trends by Zone



Note: Shaded region denotes Khmer Rouge period (1975-1979). Vertical axis is the difference between total and expected mortality. Horizontal axis is the year of reported death. Data from 2000 DHS survey round in Cambodia. Details on estimation of excess mortality in SI A.1.5.

To be clear, the West was not free of repression. Refugees report enforcement of the DK's family separation policy, executions, and starvation [119, 120]. From the reports that families were separated, one can reasonably infer education and labor

[6]The increase in excess mortality in the West coincides with the year that Sy was expelled from the zone.

policies did not sharply differ between Sy and Mok: indeed, such a difference would be such an affront to the official DK line it would have been easily observed and broadly recorded. The distinction between governance cannot be understood as night and day, violent or not. Instead, the difference between Sy and Mok is better characterized as a difference in the *intensive margin* of violence.

3.4 Long-run Poverty

Poverty has persisted since DK-era repression [118]. A key dimension of the poverty trap in Cambodia is characterized by a generational skill gap caused by executions. Intergenerational transfers of knowledge were either severely inhibited or eliminated due to the execution of middle aged intelligentsia, educators, and skilled workers [110]. The paucity of educated mentors pushed individuals into precarious lines of work with lower economic returns which did not require education to obtain. Therefore, many worked on family farms without seeking specialized training or higher forms of education after the regime [108], a trend which continues to drive poverty across the country [118].

4 Research Design

4.1 Data

I study the degree of local poverty and economic activity at the village (phum) level in Kampong Speu province. Villages are the smallest unit; in Kampong Speu province, the median size of a village was 401 persons and 78 households in 1998. The compact size of villages provide a reasonable approximation of households distance to the border that separated the West and Southwest.[7]

[7]Households tend to cluster towards village centers. Census data provides coordinates for village centroids, which I use to measure latitude-longitude and distance to the border. See SI A.3 for visualization of village boundaries.

I compare the Southwest and the West within Kampong Speu for three reasons. First, the regions represent the ideological polarization within the regime; Mok was the staunchest ally of the party's genocidal faction, and Sy was a typical moderate communist. Second, the border between zones did not overlap with other political boundaries unlike other DK zones, meaning a discrete change in the outcomes can be attributed to different zone leadership rather than differences between provinces. This mitigates the "compound treatment problem" commonly found in research designs that rely on geography. Third, the density of observations within a narrow bandwidth around the border means villages in the Southwest are being compared to an appropriate counterfactual, which is not true at other boundaries where villages are further from borders, or where the natural borders were excessively wide, like divisions created using the Mekong River.

Poverty data is from the Cambodian National Poverty Identification System (IDPoor). The data is collected in 2011 through a 16 question household survey conducted by elected village representatives who use questions regarding assets, living standards, means of transport, employment, education, and health to score household poverty on a continuous scale which is subsequently used to construct poverty categories. I measure the percentage of poor households. Further detail on the data collection process of IDPoor is available in SI B.1.1.

Second, I use data on the nighttime luminosity of villages to estimate GDP within a 2 km x 2 km grid cell surrounding the village center [86]. Luminosity proxies both formal and informal economic activity, which is important given the role of informal commerce. I use the inverse hyperbolic sine (IHS) transformation to account for skewness and zeros.[8]

[8]IHS of y is $ln(y + \sqrt{1 + y^2})$

4.2 Empirical Strategy

The nature of assignment into the Southwest versus West suggests a regression discontinuity (RD) approach which compares modern outcomes between nearby villages on either side of the boundary. The RD estimator is:

$$y_v = \alpha + \gamma \text{SW Zone}_v + f(\text{geographic location}_v) + \sum_{s=1}^{n} seg_v^s + \varepsilon_v \quad for \quad v \in bw \quad (1.1)$$

where y_v is the outcome of interest, SW Zone_v is a binary indicator scored 1 if a village was in the Southwest and 0 otherwise. $f(\text{geographic location}_v)$ is the forcing variable, which I define as the distance between the village v and the border that divided the Southwest and West.[9] I evaluate equation (1) using distance in a linear and quadratic form.

I follow [63] and include boundary segment fixed effects $\sum_{s=1}^{n} seg_v^s$, which are computed by splitting the border into n segments s and then scoring 1 if a village is closest to segment s. Segment fixed effects compare villages that lie in the same neighborhood around the border, avoiding imprecise comparisons that may occur if villages have similar absolute distances at extreme ends of the boundary.[10]

The robust error term is ε_v. I adjust standard errors for spatial heteroskedasticity and autocorrelation (HAC), following the data-driven procedure developed by [117] for selecting a HAC spatial kernel based on the spatial structure of each respective outcome. I estimate equation (1) within narrow MSE optimal bandwidths bw [38], however results are robust to alternative bandwidth choices. I include the distance to the provincial capital as an adjusting covariate in some regressions to account for proximity to the provincial center.

[9] A problem with distance as a forcing variable is that proximity to a line may not be a strong correlate of the outcome of interest, meaning local linear regressions fitted on either side of the discontinuity may fit the data in a noisy way, producing inconsistent or biased estimates. However, since distance to a highway is economically important, the use of a univariate assignment variable is theoretically justified.

[10] For segments, I use the boundary points, spaced approximately 10 kilometers apart.

The RD approach is advantageous relative to a selection on observables strategy. Since DK demographically targeted repression against the more well-off, repression levels are likely correlated with positive developmental trajectories, leading to an upward biased estimate of the effect on repression. Since administrative microdata before the regime was destroyed by DK cadres, one cannot adequately adjust for pre-existing development levels.

Since my RD design compares villages which were arbitrarily split into more or less extreme administrative zones, in expectation, confounding factors ought to be similar between villages within a narrow bandwidth, conditional on location. So long as the design assumptions hold, my strategy identifies a local average treatment effect (LATE) of exposure to an extreme DK administrator. I turn to discussing these assumptions now.

4.3 Design Assumptions and Inferential Threats

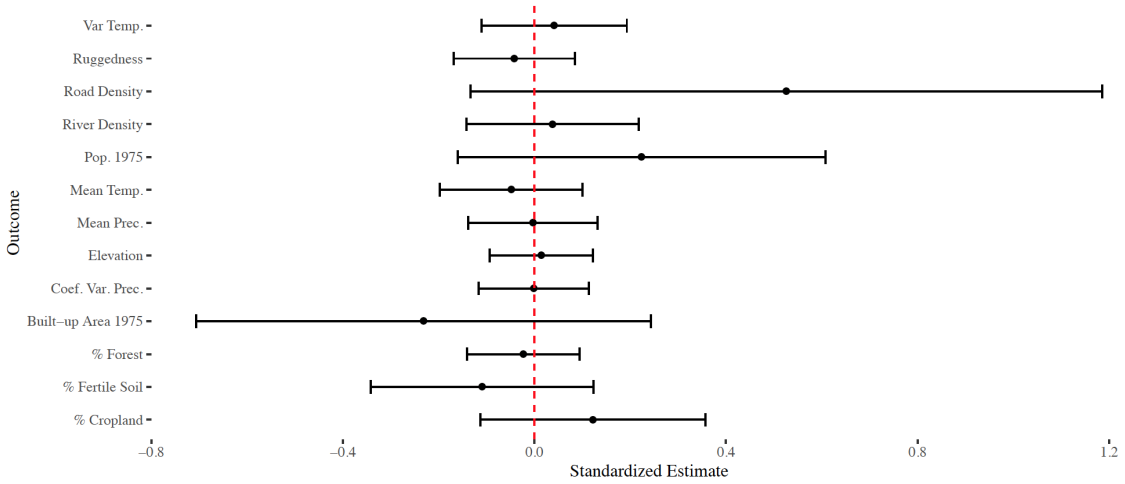
My design relies on two core assumptions: the smoothness of confounders at the discontinuity, and the absence of strategic line drawing to sort observations in a particular way.

4.3.1 Balance Tests

The assumption that confounders are smooth at the discontinuity is reasonable given the arbitrary placement of the road with respect to Kampong Speu. The historical record suggests one confounding feature: the Pol Pot faction wished to divide territory in a way to give Mok control over more productive areas and Sy with less well-endowed land, a goal that would be accomplished by splitting the relatively soil-poor Kampong Speu in two, leaving Mok with the entirety of the soil-rich Kampot province and leaving Sy with the more desolate Koh Kong and the more rugged areas in the far-north of Kampong Speu [45, 188].

Geographic variables are "slow moving" in the sense that they vary little within small bandwidths; given the arbitrary placement of the road, there is no reason to believe differences in productivity are sharp at the discontinuity despite the fact Mok was given more rich endowments in the far South outside of Kampong Speu.

Figure 1.4: Balance Tests: 1×1 Kilometer Grid Cells



Note: Unit of analysis is the 1x1 kilometer grid cell. Outcomes standardized reported in horizontal axis, vertical axis refers to each respective outcome. Spatial heteroskedasticity and autocorrelation consistent standard errors used to construct equivalence confidence interval (ECI). Equivalence range selected using the sensitivity approach $\epsilon \pm .36\sigma$. The null hypothesis is that areas *differ* from one another with at least a magnitude of .36 σ.

I test balance on agro-economic, climatic, and topographic area when crossing from one side of the border to another on 1x1 kilometer grid cells. Figure 2.1 shows the mean and variance of temperature and rainfall, forest and cropland cover, ruggedness, elevation, and soil fertility are similar on either side of the boundary. Critically, since the Cambodian economy was agriculture based in the lead-up to the Khmer Rouge, the similarity of factors of production on either side of the border suggest villages had similar access to sources of productive income.

DK destroyed micro data from the 1962 Cambodian census, making tests of balance on predetermined socioeconomic variables difficult. I use three data sources to probe for pre-existing economic differences from satellite data: estimated population,

built-up areas, and road networks in 1975. As Figure 2.1 shows the equivalence confidence interval contains zero for these outcomes, however, the substantive size of the estimate could suggest some degree of initial imbalance. There is no reason to regard this imbalance as systematic evidence of manipulation rather than chance: first, it would make little sense for a border to be strategically drawn to give built-up areas - the locations DK was most concerned about repressing - to a moderate. Second, to the extent road density could be higher in the former Southwest, increased market access should *boost* development for former-Southwest villages, suggesting bias in the opposite direction of my main prediction. Balance tests are substantively similar with nonparametric estimator within an MSE optimal bandwidth.

4.3.1 Sorting Test

Another possibility is that the road was chosen as the border because it would give Mok more authority. This concern is somewhat mitigated by the fact that the border was a natural landmark: had DK drawn a particular line, the potential for strategic line drawing would be more first order. If it was the case NR4 was chosen strategically with respect to localities, one would expect a discontinuity in the number of villages at the boundary, specifically with more villages under the authority of Mok rather than Sy. I test for strategic sorting of villages along the border, and I find the density of the running variable is continuous (SI B.2.2).

5 Baseline Results

Table 1.2 contains results from equation (1), which shows a substantively large and statistically significant difference between villages in the Southwest and West. Columns 1-4 refer to the poverty outcome whereas 5-8 refer to night lights. Columns 1-2 and 5-6 use a linear forcing variable, and Columns 3-4 and 7-8 use the squared

term. Descriptive RD plots are in SI A.1.6.

Table 1.2: Effect of Southwest on Village Development

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	%Poverty				IHS Luminosity			
1 SW	4.53*	4.43*	5.81**	4.96*	−.68***	−.69***	−.48**	−.64***
	(1.76)	(1.77)	(2.07)	(2.04)	(0.13)	(0.14)	(0.17)	(0.15)
Effective N	334	324	502	484	422	389	452	568
Bandwidth	6.34	5.98	10.99	10.62	8.9	7.95	9.64	12.79
μ Control	20.95	20.95	20.95	20.95	.43	.43	.43	.43
σ DV	10.55	10.55	10.55	10.55	.63	.63	.63	.63
Segment FE	-	✓	-	✓	-	✓	-	✓
Dist. Capital Covariate	-	✓	-	✓	-	✓	-	✓
Linear	✓	✓	-	-	✓	✓	-	-
Quadratic	-	-	✓	✓	-	-	✓	✓

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Note: % Poverty is the count of level 1 and level 2 poverty divided by the number of households per village as measured by IDPoor in 2011. Nighttime lights are the inverse hyperbolic sine of the sum of estimated GDP from luminosity in a 2x2 kilometer grid cell surrounding the village centroid.

The results imply a meaningful increase in poverty and reduction in luminosity at the discontinuity. Conservative estimates in Columns 1 and 3 suggest poverty increased by 4% - .41 standard deviations and 20% of the control average - in the Southwest. Meanwhile, nighttime lights decreased by .6 standard deviations in the Southwest, which is consistent with the increase in poverty, and is robust to alternative aggregation techniques (SI A.3.9). The inclusion of segment fixed effects and alternative functional forms of the running variable do not meaningfully impact the estimates, nor does adjusting for distance to Chbar Mon, the provincial capital.

5.2 Threats to Inference

5.2.1 Confounding (Observable and Unobservable): Sensitivity Analysis

Although regression discontinuity is a credible research design, my study is observational, and adjusting for pre-existing covariates may be important to account for precision or omitted variable bias. First, I show results are robust to covariate adjustment: including the density of roads and built-up area in 1975 does not meaningfully impact the estimates (SI A.4.1). Second, I do a sensitivity analysis to assess how severe unobservable selection would need to be in order to overturn the main findings [48]. In general, I find an unobservable feature would need to explain at least 10% of the residual variation in exposure to Mok and modern development. Since such a confounder would need to be up to four times stronger than built up area in 1975, a strong predictor, it is implausible that a covariate so large is driving the finding (SI A.3.6).

5.2.2 Road Effect: A Placebo Case Study

Roads themselves may be engines of commerce. To the extent distance to a road matters for development, my main specification flexibly controls for this by including a polynomial in village proximity to the highway. Since my focus is on the local discontinuity in development driven by being on one side of the road versus the other, for the road to explain away the main result, it must be the case that being on one side determines a change in development.

Roads may divide areas which follow different developmental trajectories due to separation and isolation from one another, in which case, a spatial discontinuity in development may emerge by virtue of the border being a road rather than differences in administration. I test this possibility using National Road 3 (NR3) in Kampot

Province as a placebo case study. NR3 bisected Kampot in a similar way as NR4, but the entire province was in the Southwest zone during DK. Available qualitative evidence suggests the road seperated different communities before the regime; "[i]n some places the line of demarcation between the two kinds of peasantry was apparently quite clear...north of the road running between Chhouk and Kampot the population was isolated, hostile to everything urban, and, incidentally, revolutionary from long before 1970, while south of that road the peasants interacted with the market, were familiar with urban ways, and considered themselves part of wider Cambodian society" [188, p.4].

I find no evidence of a discontinuity along NR3, suggesting that roads do not generically predict discrete shifts in development across space. The absence of an effect in the context of NR 3 increases our confidence that the main finding is driven by the administrative boundary NR 4 represented rather than the fact it is a road (SI A.3.7).

5.2.3 Civil Conflict Legacy: Falsification Test

The legacy could be driven by civil conflict legacies rather than state repression. This explanation is implausible, since both zones were one combat theater during the civil war (1970-1975). Civil war violence outside of state repression must change underlying factors of production to have a persistent effect [30]. In Cambodia, the most plausible way this could occur would be if explosive remnants of war (ERW, landmines or bombs) were differently allocated between zones, degrading land [133]. I find no evidence of differential ERW (SI A.3.8).

5.3 Robustness

I probe the robustness of my findings in several ways.

DHS Wealth Data To validate my measure of poverty at the village level with

administrative data, I replicate my findings using survey data at the individual level collected by the Demographic Health Survey from 2000-2014. I show rural household wealth is lower in the former Southwest (SI A.3.1).

Two-Dimensional Forcing Variable Nonlinear spatial trends could be mistaken for discrete change in income levels if the univariate forcing variable masks higher-order changes across latitude-longitude space. I include a two-dimensional forcing variable and estimate the RD along border points to account for this (SI A.3.2).

Power Analysis One may be concerned that the number of observations is small in a narrow neighborhood, reducing the statistical power of the tests. Since the effect size I uncover is substantial, not many observations are required. I show that I have sufficient power within an MSE optimal bandwidth to detect the main effects (SI A.3.3).

Falsification Test Spatial autocorrelation could explain the finding if village development clusters geographically. I show the bias-corrected CCT standard errors are robust to spatial noise simulations which create synthetic outcome data following the same spatial structure of the true data [117] (SI A.3.4)

Donut RD I estimate a series of donut-hole RDs, dropping observations close to the border, and find similar results even after 10% of the data is dropped (SI A.3.5). The result guards against the possibility that when approaching the road, villages experience a differential positive development shock unrelated to DK.

6 Mechanisms

6.1 Repression and Poverty Traps: Conceptual Framework

Violence targeted at higher educated segments of society creates a skill gap between generations, leaving younger people without mentors who can transfer skills and

knowledge. This type of violence can be found across autocracies and coerced labor regimes: autocrats prefer low-skilled loyalists to competent persons to extend their survival [71] and under coerced labor, principals are more violent towards productive and skilled persons, who have a larger outside option [5]. The mechanism I study is therefore plausible in other contexts.

In the next sections, I show how poverty becomes self-perpetuating and persistent due to repression: a poverty trap. Educational attainment is lower in the former Southwest, with a particularly strong drop among the age cohort whose secondary schooling years were interrupted by the regime. This created an intergenerational poverty trap in two ways. Individuals in the Southwest were pushed into informal employment, which earns less income. The evidence is consistent with a model of poverty wherein individuals at time t remain poor in $t+1$ because their low income forces them into making decisions that reinforce their poverty, such as working low-earning jobs [19]. Further, children born after the regime in the Southwest have worse health outcomes, which strongly predicts future income. This evidence highlights the intergenerational nature of the poverty trap: although children born in the former Southwest were never repressed by DK, they face deprivation because their parents were driven into poverty, perpetuating the cycle.

6.2.1 Human Capital Declined After DK

First, I evaluate whether trends in schooling by age changed in the Southwest zone. If human capital differentially declined in the Southwest due to the Khmer Rouge, one should observe relatively similar levels of schooling among age cohorts who finished schooling before the regime along with a sharp decline in schooling among villagers whose school-age years overlapped with the regime. I test this argument using a difference-in-differences design leveraging microdata on individual schooling and age from Cambodian Labor Force Surveys in 2000/2001. The common trends assumption

is plausible in this setting, since all villagers educated before DK were in the same province, meaning the institutional differences between regions only emerged after the regime. I placebo test this assumption, regressing schooling on a series of separate cohort dummies among persons past schooling age in 1975, finding no evidence of large or significant breaks in educational trends (SI A.5.7).

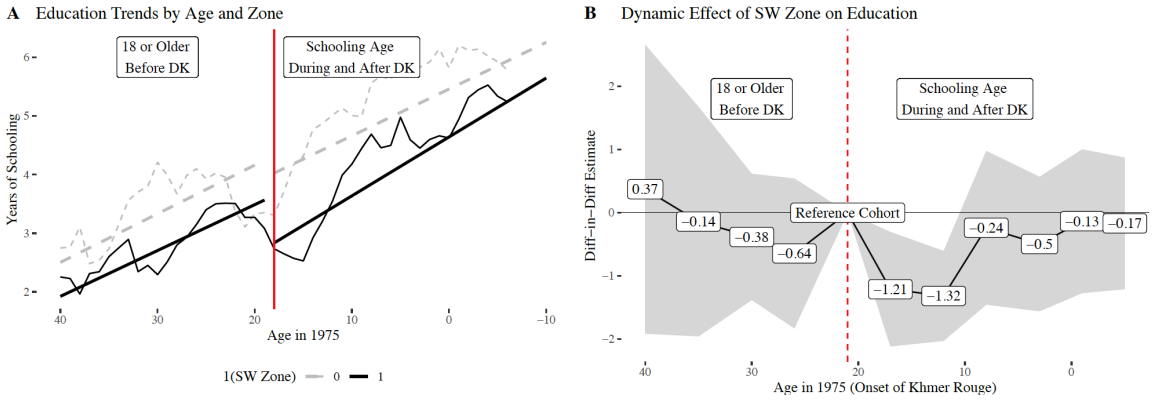
I estimate the following model.

$$y_{i,(v),(c)} = \sum_{c \neq 20-24}^{C} \beta_c \big(\text{Cohort}_{i,(c)} \times 1\{SW_{i,(v)}\}\big) + \mu_v + \lambda_{iy} + \sum_{k=1}^{K} \alpha_k x_i^k + \epsilon_{i,(v),(c)} \quad (1.2)$$

The outcome is years of schooling, $y_{i,(v),(c)}$, measured for individual i in village v among age cohort c. The coefficients of interest are β_c, which capture the differential effect of an individual living in the Southwest zone in an age cohort who would have had primary or secondary schooling after DK, relative to the age cohort who would have completed primary or secondary schooling before the regime (aged 20-24 in 1975). Note β_c captures both pre-trends in educational differences among older cohorts and the dynamic effect of having overlapped with Mok's rule during schooling age. I include birth-decade-by-commune fixed effects λ_{iy} which absorb decadal educational trends over space, village fixed-effects (μ_v) to adjust for village-invariant factors, and K individual controls $\sum_{k=1}^{K} \alpha_k x_i^k$ including age and its square and respondent gender. Standard errors are clustered at the village.

Figure 1.5 graphically displays the identification approach and results. Panel A shows the average years of schooling by cohort per zone, illustrating a (fitted) parallel trend in the pre period. Education levels then sharply decline in the former Southwest zone among 12-17 year olds (in 1975 years). Panel B corroborates the descriptive trend, showing the absence of a difference among older age cohorts between zones and a transitory decline in educational attainment for persons who were schooling

Figure 1.5: Educational Attainment by Age Cohort and Zone



Note: X-axis records birth cohorts in reference to 1975 - individuals born in 1985 are scored -10 whereas birth in 1965 is 10. Y-axis is years of education completed. Controls include, age, age^2, gender, decade-by-commune fixed effects, village fixed effects. Panel A is the raw trend - averages of schooling by age cohorts and zone, with the five year moving average and linear fit separated by the pre- and post-period Panel B plots difference-in-differences coefficients (β_c per equation (2)).

age when the regime began. The evidence suggests schooling fell among school-aged Cambodians in the Southwest.

If human capital decline persists overtime, one should observe lower human capital levels at the village level between zones. Table 1.3 shows the share of persons who have never attended school increases, whereas the literacy rate (of those over 15 years of age) declines. The estimates are largely commensurate with one another; whereas the percentage of persons who never attended school increases by 7% in the baseline, the share of literate persons over 15 declines by 8%. The results are substantively large, near .5 σ in the baseline estimates. In SI A.5.1, I show years of education decline by a year on average, and school attendance declines by 6% in 1998. The result is robust to adjusting for distance to schools (SI A.5.2). The education gap persists into 2008 through tertiary and secondary education (SI A.5.5).

The impacts of repression on schooling could vary by gender: parents may invest less in schooling daughters in the aftermath of repression, pushing female family

Table 1.3: Human Capital: Education and Literacy in 1998

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	%No Educ.				Lit. Rate			
1 SW Zone	7.58**	3.55	7.39**	4.46†	−8.02**	−4.85†	−7.85*	−5.46†
	(2.45)	(2.2)	(2.65)	(2.5)	(2.65)	(2.59)	(3.07)	(2.86)
Effective N	310	316	552	440	308	290	442	412
Bandwidth	5.69	5.95	12.89	9.80	5.62	4.99	9.87	8.95
μ Control	50.48	50.48	50.48	50.48	62.88	62.88	62.88	62.88
σ DV	14.85	14.85	14.85	14.85	17.03	17.03	17.03	17.03
Segment FE	-	✓	-	✓	-	✓	-	✓
Dist. Capital Covariate	-	✓	-	✓	-	✓	-	✓
Linear	✓	✓	-	-	✓	✓	-	-
Quadratic	-	-	✓	✓	-	-	✓	✓

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$

Note: See Table 2. Outcomes are percentage of the population without education and percentage over the age of 15 who can read and write.

members into care-taking roles instead of education. The pattern of lower educational attainment for females is consistent across Cambodia. Notably, this mechanism would not be a rival account to a poverty trap, rather, it would be a dimension by which the poverty trap is perpetuated. I find no consistent evidence of this pattern (SI A.5.6).

6.2.2 Labor Market Outcomes

Qualitative evidence suggests the absence of educated and skilled persons drove individuals to work in low-paying jobs [110]. In the Cambodian context, less schooling strongly predicts individuals being "own account workers" - self employed, typically working agricultural jobs on small-scale family farms or in otherwise informal roles. This line of work is highly labor intensive, has a low level of productivity, and involves low levels of skill and technology [13]. If the decline in education caused by DK repression reshaped local labor markets by pushing individuals into low-earning informal agricultural work, one may expect an increase in the probability an individual

is an own-account worker and a commensurate decline in earnings and productivity.

Table 1.4 shows findings consistent with this pattern. Rural persons in the Southwest are far more likely to be own account workers - self-employed informal laborers (column (1)). Consistent with broader patterns of employment and earnings, column (2) shows lower income from work as well. Finally, column (3) shows earnings per hour are also lower, meaning productivity for workers also diminishes. The evidence is consistent with qualitative accounts of how state repression shaped labor markets and workers in the wake of the human capital shock from the regime. In SI A.5.9, I show descriptive evidence that own-account workers are less educated and earn less.

Table 1.4: Labor Market Effects of Repression

	(1) Pr(Self Employed)	(2) IHS(Income)	(3) Productivity
1 SW	0.16†	-0.86*	-9.12*
	(0.09)	(0.34)	(4.15)
Bandwidth	10.48	13.61	18.10
Effective N	235	285	344
Covariates N	✓	✓	✓
SD DV	0.45	0.86	8.81

Note: Unit of analysis is employed working aged (11-59) individuals. Data from 2000-2001 Labor Force Survey. Pr(Self Employed) is scored 1 for persons who are own account workers. Income is individual wages, remuneration, earnings, tips reported from the last month in 10,000 riels, and productivity is riels divided by working hours. Estimates are obtained using [38] nonparametric RD within MSE optimal bandwidths and a triangular kernel. Covariates in local linear regressions include survey wave fixed effects, age, age squared, and gender of individual. Robust errors clustered at the village. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.10$.

6.2.3 Intergenerational Consequences

Economic impacts can reverberate across generations by reducing child health. Childhood health determines later-life income levels and is partially determined by maternal economic well-being [31]. I evaluate how the DK shock impacted subsequent generations by exploring the health of children between zones with four rounds (2000, 2005, 2010, 2014) of Demographic Health Survey (DHS) data. Parents who lost

Table 1.5: Intergenerational Effects: Child Health Between Zones

Outcome	(1) Health Index	(2) Height/Age	(3) Weight/Age	(4) Weight/Height
1 SW	-0.87^{***}	0.12	-0.61^{**}	-1.17^{***}
	(0.26)	(0.15)	(0.19)	(0.15)
	$[0.31]^{**}$	[0.20]	$[0.22]^{**}$	$[0.40]^{**}$
N. Individuals	243	298	243	195
N. Clusters	29	36	29	23
Bandwidth	11.05	11.55	11.21	9.34
Covariates	✓	✓	✓	✓
SD DV	1.38	1.26	0.99	0.98

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Note: Unit of analysis is the 3-5 year old individual from the 2000-2014 DHS survey waves - the children of the generation exposed to the Khmer Rouge. Health index (Column 1) is the first principal component of individual health measures. Height/Age is the standard deviations from the median of individual height for age (stunting), Weight/Age is standard deviations from the median of weight for age (wasting), Weight/Height is standard deviations from the median of weight for height (underweight). Analysis within rural households to maximize comparability. Controls include the age of the mother and its square and survey year fixed effects. Robust standard errors clustered at the survey area reported in parentheses. Cluster standard errors reported in parentheses, wild cluster bootstraps reported in brackets.

schooling and were therefore poor as a result of Mok's rule may have less healthy children as a result of their low income. Finding worse health outcomes for youth may highlight how repression creates persistent, negative human capital consequences beyond schooling and across generations that were never exposed to violence.

DHS randomly selects a subset of respondents and measures three critical dimensions of child health for persons aged 3-5: height for age (a measure of stunting), weight for age (a measure of wasting), and weight for height (a measure of being underweight). I create an index of health scores using the first principal component of these measures and evaluate each measure individually, and then estimate a version of equation (1) which includes wave year fixed effects and maternal controls among rural households to maximize comparability.

Table 1.5 shows the health index declines by nearly 0.8 σ. The effect is driven by underweight and wasting children, rather than stunting, which suggests childhood

food poverty drives health differences. In SI A.5.8 I show the effect is orders of magnitude larger for mothers without formal schooling, and that the difference between zones attenuates when mothers have some education, suggesting the education shock from the regime plays a crucial role in explaining health differences.

6.3 Alternative Mechanisms

6.3.1 Social Capital

Cultural persistence via social trust has been shown to be an important persistence channel linking coerced labor to modern development. [135, 132, 150]. I use data from Cambodia's Violence Against Women survey, which asks respondent's three questions about communal trust and social cohesion including (1) "Do neighbours in COMMUNITY NAME generally tend to know each other well?", (2) "If there were a street fight in COMMUNITY NAME would people generally do something to stop it?" and (3) "If someone in your family suddenly fell ill or had an accident, would your neighbours offer to help?" I code 1 if a respondent answers "yes" and 0 otherwise. There is little variation across villages in different zones along these dimensions, with respondent's reporting affirmative answers at high and nearly identical rates between zones (Table 1.6). This suggests social capital cannot explain the developmental divergence.

Table 1.6: Social Cohesion and Trust

Zone	Know Neighbors	Help Fight	Help Sick
SW	1.00	1.00	0.96
W	1.00	0.93	0.99

Source: WHO Women's Health and Life Experiences Survey. Outcomes are the share of respondents reporting that they know their neighbors well (Know Neighbors), whether the respondent believes people in the community would provide help if a fight broke out on the street (Help Fight) and whether neighbors would help if one become sick or had an accident (Help Sick). Each cell reports the proportion of respondents who reply in the affirmative divided by total respondents. SW is the Southwest zone and W is the West zone.

6.3.2 Property Rights

Weak property rights institutions are another channel commonly cited in the literature [6, 63]. Since neither the DK ban on private property nor collectivization has persisted, it is unlikely formal institutional persistence explains contemporary maldevelopment. However, the destruction of records of land ownership could have increased the contestability of land, creating tenure insecurity and poverty. Social conflict as a result of extractive institutions has been shown to be a crucial persistence mechanism [94].

To measure respect for property rights, I collect data on the count of village land disputes from Commune Database Online. This measure captures a highly salient aspect of respect for property in the Cambodian context, where the highly agrarian economy has seen increasing poverty due to land grabs and unclear titling [118]. I find no difference in land disputes at the discontinuity (Table 1.7).

Table 1.7: Land Disputes

	Levels (1)	IHS (2)	Binary (3)
1 SW	0.5499	0.2405	0.1094
	(1.0794)	(0.2179)	(0.0692)
N	607	631	615
BW	14.5	15.3	14.7

Note: RD estimates evaluating changes in land disputes when moving from the West to the Southwest zone. Outcomes are level of land disputes in 2008-2010 reported by the Commune Database Online. Column (1) is the count (Levels), column (2) is the inverse hyperbolic sine of the count (IHS) and column (3) is a binary indicator for any dispute (Binary). Standard errors reported in parentheses.

6.3.3 Political Legacy

A third class of explanations relates to the political dominance of former regime elites; historical coercive institutions may create politically uncompetitive environments, which outlast initial conditions. As explained, this mechanism is unlikely, as

Mok and his cadres were driven from the region during Vietnam's invasion. I measure the competitiveness of commune council elections in 2012 and 2017 (Herfindahl index, higher values represent less competitiveness) and the vote share for the ruling Cambodian People's Party (CPP) in 2012/2017. Data is only reported at the commune level, of which there are 87 in Kampong Speu. Since the units are much larger than villages, I include all communes in the RD, but also include district fixed effects to absorb spatial heterogeneity.

Table 1.8: Commune Council Elections (2012/2017)

Outcome	Competitiveness		CPP Vote Share	
	(1)	(2)	(3)	(4)
1 SW Zone	-2.74	-3.61*	-2.82	-0.04
	(2.89)	(1.59)	(3.69)	(0.03)
District FE	✓	✓	✓	✓
Election Year	2012	2017	2012	2017
N	87	87	87	87

Note: RD estimates evaluating the political impact of the Southwest zone on commune council elections. Competitiveness is the share of vote shares squared (higher values mean more concentration, lower values more competitive.) CPP vote share is the count of votes to CPP, the dominant ruling party, out of total votes. SHAC errors reported in parentheses.

Results in Table 1.8 show local elections are largely similar between zones. To the extent competitiveness of elections are different, elections appeared more competitive in the Southwest in 2017, where vote shares were less concentrated by 3.6 points. As such, political competition and partisan support are unlikely explanations. Another observable implication of elite capture and corruption - a key persistence mechanism in the study of extractive institutional persistence - is lower levels of public goods. In Table A.8, I show villages on either side of the border have similar access to hospitals, schools, an commune centers.

6.3.4 Migration

Migration could explain my findings if a substantial proportion of wealthy individuals fled the (former) Southwest zone after the regime for the West. While this would not invalidate the design, it would mean the primary persistence channel was selective migration of more well-off civilians rather than an intergenerational human capital shock.

I treat migration as a post-treatment variable, and back out how large selective migration would need to be in order to explain the result under weak assumptions and a conservative trimming exercise. Selective migrants would need to occupy the entire top 25% of the wealth distribution in the rural West to explain the result - an implausibly high proportion considering low rates of rural-rural migration.

7 Conclusion

Does mass repression have a developmental legacy? If so, why do the consequences persist overtime? I argue mass repression which demographically targets the educated and intellectuals can destroy human capital in more intensely repressed localities, creating a poverty trap. I find evidence by exploiting an administrative redistricting which arbitrarily placed villages under very different rulers during the Khmer Rouge regime, creating quasi-random variation in the intensive margin of state repression. I show substantial impacts on wealth, household poverty, and human capital at the individual and village level, which spillover to the next generation via health. Whereas evidence is consistent with a poverty trap, I find no evidence in support of other important persistence channels linking coercive regimes to long-run development.

The results show the legacy of increased exposure to repression at the intensive rather than the extensive margin. Since villages in the former West zone were also repressed, albeit less severely, the counterfactual of what development would have

looked like absent any repression is not estimated. However, as in [63], the intensive margin estimates presented here are likely a lower bound on the impact of Khmer Rouge rule at the extensive margin. Assuming the human capital destroyed in the West was more destructive to long-run development than the absence of any repression, my results showing more intense repression led to 0.40σ increase in poverty and a 0.50σ reduction in human capital are conservative estimates of how much total wealth was lost from repression. The results are on par with the developmental impact of King Leopold's concessions in the Congo [135].

My study shows that state repression can create multiple developmental equilibria across space, adding human capital shocks to the library of mechanisms that may link coercive institutions to worse development in the modern day [81]. My study also shows when other theories of persistence do not generalize, and provides a within-case explanation that is plausible in contexts where governing institutions eradicate well-off or intellectual groups. Since coerced labor institutions throughout history use more violence against the highly capable, and since several dictatorships and insurgencies throughout history have targeted intellectuals, the channel I identify may be at play in many other settings, which are key avenues for future research.

The findings suggest a focus on the technology of coercion and the targets of repression is a useful starting point for making sense of developmental impacts of state violence. Since DK executed higher class civilians, factors of production changed in a way that created poverty cycles. However, as other research has shown, the impacts of repression on the economy are not homogenous or unidirectional. Unpacking how and why repression shapes poverty in the long-term is not only crucial to understanding the roots of development and consequences of conflict, it can also help scholars begin to understand why autocrats choose certain tools over others to control populations. Recent work by [180] is a start in this direction, illustrating how the impact of repression on wages determines the state's counterinsurgency strategy.

New avenues may interrogate why the state would chose a repression strategy that destroys wealth, or the conditions wherein the negative economic impact of repression may spur "repression traps," wherein by creating conditions favorable for insurgent recruiting through violence, the state is forced to further rely on violence [56].

Future scholarship may consider whether or how the lingering impacts of autocratic repression moderates the effects of democratic transitions on development. Sequencing of events may shape the effect of repression in the long-term; the Cambodian case is one where repression was followed by occupation and more dictatorship. Studying how the legacy of repression on development may change - or stay the same - after regime change may further elucidate mechanisms of, or solutions to, the developmental legacy of draconian dictatorships.

Chapter 2

Oil Crops and Social Conflict: Evidence from Indonesia

Commercial agriculture is increasingly considered a tool for poverty alleviation and peace building in developing states [37, 67, 93].[1] Despite the economic promise of cash crop industries, new agricultural markets have also been linked to a host of negative environmental externalities, including deforestation and biodiversity loss, and have been linked to violent competition for land ownership [183].[2]

When does commercial agriculture intensify social conflicts? Labor intensive agricultural growth tends to be negatively associated with armed conflict [29, 66], however, the relationship between crops and social conflict between non-state actors outside of ongoing civil conflict is less well-known. To the extent scholars and policy-makers conceive of cash crops as a means of providing opportunity to the rural poor, unpacking the link between cash crops and social violence is critical to understanding low-level violence between neighbors and forging effective development policies in

[1] See [54] for a more extensive review of expert discourse on value chains and fragile states.

[2] See also: "Land rights at root of palm oil conflict in Liberia." *Reuters.* https://uk.reuters.com/article/us-liberia-land-palmoil-idUKKCN0XX17U and "Honduras and the dirty war fuelled by the west's drive for clean energy." *The Guardian.* https://www.theguardian.com/global/2014/jan/07/honduras-dirty-war-clean-energy-palm-oil-biofuels.

post-conflict or fragile states.

I argue emerging commercial agricultural markets can disrupt social stability, producing social conflict over the distribution of resources. The growth of commercial crops can crowd out sustenance farmers, damage forests resources which support forest-based communities, and may only slowly bring economic benefits to a locality. Since sectoral growth creates tensions over the distributional consequences of the industry in the short-term, and only brings profits that may offset grievances in the long-term, the incentive for contention wins out over incentives for peaceful production.

I study the case of oil palm in Indonesia. Indonesia is at the center of the recent oil crop boom, the largest agricultural transformation since the green revolution [35, p.1]. Given the expectation that palm production will continue to expand across Latin America, Southeast Asia, and Sub-Saharan Africa, understanding the link between oil palm and stability is critical to fostering inclusive development.

I argue the palm oil boom generates incentives for violent resource conflict between producers and non-producers more quickly than the opportunity cost of conflict increases. Profits from oil palm production do not immediately bring prosperity to surrounding communities: low-skilled and forest-dependent communities lose out from palm oil plantations [151, 167], and the poverty-reducing benefits of oil palm are both slow-moving and come at the expense of the local environment [70]. Since transitory oil palm shocks generate up-front social costs along with delayed income gains, I expect higher prices of oil palm to correspond with increased levels of distributional conflict.

I find the palm oil boom is positively associated with resource conflicts between non-state actors in Indonesia. Resource conflicts involve violent disputes over land, access to markets, and environmental or economic grievances emerging from production, and occur between citizens, communities, and firms. Resource conflicts between

non-state actors are multifaceted. Individuals who claim exclusive rights to produce may attack one another to seize and destroy property, producers hire private security outfits who may forcibly remove tenure insecure landholders, or communities collectively protest, rob, or sabotage plantations to disrupt the production process. I do not find a relationship between the boom and other types of political violence unrelated to resources, such as popular justice, election, governance, law enforcement, and separatist violence, nor do I find a relationship between resource conflict and crops which pose more acute land-use tradeoffs.

I find evidence for two mechanisms. First, I illustrate the importance of socially responsible production practices as a moderator of price shocks. The relationship between palm and conflict is decreasing in areas with more sustainability certified mills licensed by an important non-governmental organization, the Roundtable on Sustainable Palm Oil (RSPO). The finding suggests firms and communities can avoid costly conflicts without legal intervention. Given the institutional weakness in developing states, identifying peaceful extra-legal mechanisms is crucial to understanding the sources of violence and possible solutions [47].

Second, I show the increase in oil palm production at the extensive margin is associated with an increase in resource conflict in the medium term. Districts that saw a larger increase in land area devoted to oil palm overtime experienced a larger increase in resource conflict from the baseline. The finding underscores how land use change impacts distributional conflicts.

My study provides a caveat to existing theoretical and empirical work on commodity shocks and conflict. I argue a commodity that would generally be negatively associated with armed conflict increases social conflict. Palm largely fits the scope conditions for a peaceful commodity: it is labor intensive in an absolute sense, and it is not sequestered by illicit or armed actors [11, 54, 128], Indeed, in a highly influential study, [66] consider oil palm a labor intensive good, and find positive shocks tend to

suppress armed conflict.

While the empirical literature has largely focused on armed conflict [29, 66], social conflict has been relatively under examined. According to extant theory, all else equal, sectoral shocks ought to impact social conflict in similar ways conditional on factor intensities: when the demand for formal employment increases, individuals should be drawn to working in shocked sectors to earn higher wages instead of risking life and limb on contentious behavior [55].

Palm oil, however, has different effects on social conflict because of the nature of the commodity boom. The emergence of palm oil in response to higher prices has led to increased production at the intensive and extensive margin in Indonesia: producers have expanded operations into forested areas, creating tensions between sustenance farmers, forest dependent communities, and commercial interests. The negative externalities associated with palm production generate community grievances, and the sharing of benefits between producers and non-producers does not always perfectly offset the social and environmental harms from increased production in response to greater profits. As a result, communities not producing palm are resistant to industry expansion and resentful of its growth, and producers use coercion to obtain land and resources to expand production.

The findings do not wholly contradict the broader theory regarding the role of factor intensities in conflict. Instead, the findings are fairly consistent: a commodity that increases the incentive to predate more quickly than the opportunity cost effect can take hold results in violence. The key contribution of this paper is highlighting the conditions where the growth of a labor intensive sector (in the absolute sense) can counterintuitively increase social conflict. My study spotlights the importance of considering local economic context surrounding commodity booms when theorizing about the effect of price shocks on stability, and shows the conditions where the pacifying effects of agriculture do not hold.

My final contribution is connecting the large literature on commodity shocks and conflict to research on land and political violence [8, 32, 97]. Using a long differences design, I show that as more land is dedicated to palm production over time, changes in resource conflict are larger. The expansion of production at the extensive margin can pit would-be producers of crops against incumbent landholders who may lose out from increased commercial production, including those with customary claims, forest dependent communities, or sustenance farmers. I show production booms can spur land related violence, at least due to the competition for land that emerges when crops become more profitable [32]. The mechanism suggests commercial crops can upset stability when land is contestable, which is salient in fragile states.

The article proceeds with an outline of the theoretical framework linking resources to violence. I then contextualize this general discussion to the palm oil sector and Indonesia. Next, I describe the data sources, research design, and conclude with the results and discussion.

2 Theoretical Framework

Commodity shocks have countervailing effects on conflict incentives. While the prize of looting a resource increases when commodity value rises, a mechanism called the rapacity effect, the reward for seeking formal employment in the sector increases too, called the opportunity cost effect. A meta-analysis of the commodity shocks and armed conflict literature shows the empirical relationship between prices and armed conflict depends on factor intensities [29]. Scholars argue the opportunity cost effect will dominate the rapacity effect when the commodity is labor intensive, since the sector's growth will generate more demand for employment and higher wages, pulling individuals away from predatory activity [29, 55, 66].

However, even if emerging agricultural sectors are labor intensive, a surge in prof-

itable commercial cash crops may fuel violent conflict between citizens and/or between producing firms and incumbent landowners. Conflict between community members can emerge when land holders seek more exclusive claims to ownership in response to improving commercial value, leading to competing claims that are not easily resolved legally [32]. Violence may also occur across classes; [172] argues peasants historically protest the growth of commercial agriculture, since it disrupts land use for sustenance farming and upsets the traditional balance between locals and the elite.

When property rights are imperfect, increasing resource value increases the incentive for predation, which leads agents to invest in tools of coercion to defend their assets. Firms may hire private security outfits or partner with armed actors to guard plantations, and civilians may invest in weapons to protect their small farms for land grabs [92, 98, 177]. While conflict is costly, the risk becomes more worthwhile when the returns to predation increase, creating a positive association between prices and conflict.

If surging agricultural markets increased the opportunity cost of social conflict faster than incentives for predation, an emerging commercial cash crop would not lead to more violence. But, profitable commercial agriculture does not guarantee balanced growth [69]. When commercial value improves without evenly benefiting producing and non-producing groups, growth will leave community members behind, meaning the opportunity cost of conflict does not increase for excluded segments of society during boom periods. Generically, commercial crop price surges lead to violence over resources when the return from predation increases faster than the return for production [55].

Labor intensive crops increase the return to predation more quickly than the opportunity cost of violence based on two conditions. First, conflict may increase if the negative externalities of production create an incentive for non-producers to take action to stop commercial expansion. If commercial growth harms the local

environment, crowds-out sustenance farmers, or undermines forest resources, a price surge pits commercial interests against community members. With the knowledge that locals will oppose commercial expansion, producers may partner with coercive actors, such as private security guards or local criminals, to violently crush opposition to expansions. Likewise, locals may use force to raise the cost of production to deter commercial interests.

Second, if the growth from commercial agriculture does not trickle down quickly, grievances from inequitable sharing of costs and benefits may fuel conflict. The opportunity cost effect cannot be activated unless transitory shocks increase demand for labor and wages. For example, if a commercial crop produces employment opportunities, but only for those with a particular set of skills and capital, the set of workers without those skills or capital will not be absorbed by the new labor market. If the excluded workers must also bear some cost to commercial production, such as changes in land use which undermine tenure security or degradation of the local environment, negative externalities will fuel conflict.

A critical crop that is growingly important for the global economy fits the scope conditions: oil palm.

3 Institutional Background: Indonesian Palm Oil

Oil palm has played a crucial role in the development of rural economies in Indonesia, but has also contributed to social problems stemming from environmental and land related disputes. In this section, I describe the features of palm oil which lead the sector to be more conflictual compared to other export crops.

3.1 Background on Oil Palm

Palm oil is a labor intensive commodity planted for commercial trade, typically grown as a monocrop [35, p.20]. Growing demand for flexible crops that can serve as food, biofuel, and industrial products has led to a substantial increase in vegetable oil prices and palm oil production, particularly in Southeast Asia [168, 35, p.8-9]. Since 1990, global palm oil production has tripled [35, p.1]. However, growth in the oil palm sector tends to be uneven and the social and economic consequences of the crop growing more profitable can lead to unrest.[3]

While the palm boom has generated windfalls, the gains in income may not translate into immediate living standard improvements or better prospects for laborers for three reasons. First, trees take three years to yield fruit after planting - farmers wait up to six years to earn a profit [35, p.17]. Second, the growing dominance of the palm sector can crowd out alternative livelihoods. Due to barriers to entry, the gains from production tend to be concentrated "above a certain threshold of agricultural skill and income" [151]. The process led farmers to remark "[w]hen oil palm is developed, other people get jobs not us. The jobs are not for us" [52]. Rapid development of palm leaves sustenance farmers and forest dependent communities in a worse economic position [167].

Third, fruit requires immediate processing by mills to be sold as oil, within 24 hours, therefore smallholders often contract to exclusively grow oil palm and sell fruit to a plantation company mill. Larger processing mills have a comparative efficiency advantage over smaller mills, creating "de facto local monopolies" for centralized mills, constraining producers choice over where to sell fruit [175, p. 11]. Debt obligations from start up costs and constrained choice to sell fruit may chip away at profits [141, 175]. As noted by [175]: "[s]ocial conflict between oil palm companies and smallholders is also common because smallholders enter into price contracts with

[3]Further detail on oil palm in SI B.1

companies and are not able to benefit from any marked price rises for CPO."

The pressure to expand production in response to growing demand and higher prices creates three negative externalities. First, oil palm plantation development is mutually exclusive with natural forestry [131]. An estimated 56% of oil palm expansion that occurred in Indonesia from 1990 to 2005 supplanted forest land [127], leading to deforestation and biodiversity loss [40, 189, 190]. Compared to other tree crops, palm oil performs worse in terms of supporting local ecosystems after it supplants forest land [77] degrading the local environment. Land clearing may occur via fire, leading to further damage.

Next, oil palm has a tendency to be monocropped, unlike other tree crops, which further fuels land use tradeoffs. Forest dependent communities rely on rotational farming and abundant forest land for sustenance - both of which are disrupted by mono-cropped plantations which remove naturally forested area [175].

Third, the rapid expansion of oil palm is facilitated by a porous legal structure, meaning the changes in land use are contestable. Rent seeking political elites enable the expansion of commercial activity in forested areas [34, 138]. Corrupt officials and poor protection of customary land rights enables "a race to the bottom", wherein competing national and local regulations are exploited to allow the palm sector's expansion into forested and protected areas which are tenure insecure [35, p.43-44]. Therefore, when the crop becomes more profitable and the benefits to expanding production increase, the competition for finite land intensifies, pitting commercial interests against incumbent landholders.

3.2 How Oil Palm Enflames Social Conflict

Qualitatively, the rapacity effect appears to dominate the opportunity cost effect in the oil palm sector in Indonesia. Scholars have noted that the expansion of the palm oil sector has "raised the stakes" in resource related disputes, coinciding with an

increase in resource violence in Indonesia over time [22]. A recent oil palm price surge was accompanies by an increase in fruit theft and companies hiring additional security.[4]

My theoretical argument suggests the reason conflict incentives dominate the opportunity cost channel is because the oil palm sector creates upfront negative externalities during boom periods, but does not generate income gains in the short-run which raise the opportunity cost of violence.

Two main mechanisms explain why oil palm generates social conflict.

3.2.1 Firm-Community Relations

First, price shocks may enflame tensions between communities and firms, which can escalate to violent conflict. Surging prices can cause this for two reasons. First, communities may resent that palm producers are earning more while investing little into local improvements after environmental damages [47]. Indonesia increasingly experiences land related conflicts surrounding industrial tree plantations, with broken promises between firms and communities after land deals, pollution, and uneven or unequal sharing of benefits typically cited as the underlying cause [153].

Conflicts between communities and palm oil companies are typically fueled by poor compensation after conversion of forest to plantation land, accusations of illegal production, and environmental damage [1]. Plantation company's preferences for hiring outsiders over locals can lead community members to feel deceived, which can "easily lead to conflict" [130]. When firms with hostile relationships with host communities face pressure to expand during a boom period, growth creates incentives for local predation of oil palm, and incentives for producers to grab locals land, causing violent conflict.

[4] "Forbidden fruit: Indonesia palm oil plantations boost security to stop thieves." Reuters. Aug. 9, 2017. Accessed March 29, 2020.

3.2.2 Negative Externalities from Land Use Pressure

Second, higher prices increase the incentive to expand operations, which locals may wish to resist due to negative externalities [173]. Palm oil producers have been accused of violating land and labor rights, and undermining biodiversity through monocropping, deforestation, and water pollution from transforming fruit to oil [141, 175]. Communities may respond to deforestation or fresh water pollution resulting from pesticides with conflict as a way to halt production [165].

The negative externalities and short-run gains can lead to conflict initiated either by producers or non-producers. For instance, reports of palm producers preemptively using violence against communities opposed to plantation expansion suggests members of the industry can be the first to use violence to crush opposition to commercial expansion. On the other hand, community members may initiate conflict as a means of raising the costs of production to deter producers from entering or expanding in the market.

Tension between oil palm producers and communities have escalated to violence across Indonesia, either initiated by the community or firms. In Sambas district in 2008, the chairperson of the Peaceful Allied Peasant Union and a village head were attacked, allegedly due to their rejection of palm oil plantation expansion. Similarly, in Langkat district in 2013, villagers homes were burned when they protested the expansion of a palm oil plantation [22]. Shootings and beatings of farmers resistant to plantation expansion in South Sulawesi, along with the reported deaths of thirty-two villagers in Lampung over plantation disputes, evinces plantation companies and commercial producers may resort to violence to settle disputes [136, p 297]. Community resistance can escalate to violence as well. In North Sumatra, a protest in 2007 over an irrigation canal for a palm plantation led to a large clash between citizens, police, and private security forces hired by the plantation company [141, p.47].[5]

[5]More information on palm oil conflicts in SI B.3.

4 Data

To measure social conflict, I use the National Violence Monitoring System (NVMS) dataset [22]. The database is highly detailed, coding several types of social conflicts reported from local newspapers across Indonesia, including the district (second administrative unit, equivalent to a U.S. country)[6] where violence occurred. The use of local papers mitigates reporting bias that may arise from using national or English language sources when covering local acts of violence. Given the expansive scope of the collection effort, the data do not cover all provinces. Instead, "high" violence provinces are covered from 1998 to 2014, with "low violence" provinces receiving coverage from 2005 to 2014. I use data from 2005 to 2014 to construct a panel, allowing for provinces of different types to be included in the sample.

The main outcome variable I use from the National Violence Monitoring System is resource conflict. NVMS defines resource conflict as: "violence triggered by resource disputes (land, mining, access to employment, salary, pollution, etc.)." A translated example of resource violence from the database is as follows:

> In the Village of Perbangunan and Bangun Baru, Kec. Sei Kapayang, Kab. Asahan, North Sumatra, the destruction of 800 hectares of oil palm plantations belonging to members of the Independent Farmers Cooperative was carried out by an unknown person. As a result of the robbery and destruction, members of the cooperative suffered losses of up to Rp 1,039,000,000. The destruction using a tractor was allegedly carried out by one of the palm oil procurement companies. It is suspected that the motive for this destruction was related to the struggle over 800 hectares of land. (09/30/2011)

The panel includes 5586 resource conflicts, occurring in 1097 of the 1560 district-years. Conditional on having a resource conflict, the average number of conflicts is 5.01, with a maximum of 53. I use other conflict and crime outcomes as placebo checks. Nearly all districts (151 out of 156) experienced at least one resource conflict.[7]

[6] The units are also referred to as the regency (kabupaten) or city (kota).

[7] Summary Statistics in SI

One must account for changes in administrative boundaries to construct a panel. Indonesia has underwent massive district proliferation since democratization (pemekaran) with several units changing names, statistical codes, and borders. I use the 1995 administrative boundaries - the latest period before the democratic transition - as an exogenous point of reference, and use information regarding new district's "parents" as well as the "children" of older districts to construct a balanced panel of 156 districts over 10 years with constant borders.

To measure district exposure to oil palm, I use data from the Indonesian Database for Policy and Economic Research [107] regarding the land area devoted to palm oil production per district. Since palm crops can take years to mature and produce, a simple measure of the fruit yield may not accurately measure sectoral concentration, whereas the land used to grow palm reflects how invested farmers are in using land for oil crops.[8]

5 Empirical Strategy

I exploit cross-sectional variation in palm oil production and overtime variation in crude palm oil price shocks [66]. As local production levels and conflict may be simultaneously determined, I rely on a pre-sample measure of the palm oil intensity. I use the average hectares of land dedicated to palm oil production across 1996-2004. I choose 1996 as it is the first year the data is available annually at the district level, and stop in 2004 as it is the year prior to the beginning of the conflict data. This approach alleviates the concern that some districts may arbitrarily be coded as heavy producers from an uptick in land used in a single year.

Next, I divide this quantity by the sum of average area devoted to palm oil pro-

[8]More detail on data in SI A.

duction across all districts:

$$\text{Production Share}_i = \frac{\text{Avg. Palm Area}_{i,1996-2004}}{\sum_{i=1}^{n} \text{Avg. Palm Area}_{i,1996-2004}} \text{ and } \sum_{i=1}^{n} \text{Production Share}_i \leq 1.$$

which scales the salience of palm relative to other localities. Districts where palm production area makes up a larger proportion of national production area receive more weight than localities whose production makes up a smaller share. The share-based measure scales the relative intensity of production to ease interpretation. Measurement in levels, as in [66], yield similar results where coefficients represent absolute increases in hectarage rather than percentage increases (SI B.3.3).

I use the global price of oil palm to measure overtime variation in palm oil value per district [106]. I standardize the measure by subtracting the mean of palm oil prices over time from each year and dividing by the standard deviation $\left(\frac{\text{Price}_t - \mu}{\sigma}\right)$. This captures how large of a change from average price occurs in each year. I lag this variable by one year to account for the time required for the local market to react to global price swings, and to alleviate concerns of simultaneity.

I interact these variables to construct an exogenous measure of palm oil shocks per district $\text{Palm Shock}_{it} = \text{Production Share}_i \times \text{Price Shock}_{t-1}$. The measure represents the intensity of exposure to shocks.

The baseline model is:

$$ihs\ \text{Conflict}_{it} = \beta_1 \text{Palm Shock}_{it} + \sum_{k=1}^{k} \alpha_k X_i \times \lambda_t + \lambda_t + \mu_i + \varepsilon_{it} \tag{2.1}$$

where the left hand side variable $\text{Resource Conflict}_{it}$ is the count of resource conflicts in each district-year, transformed with the inverse hyperbolic sine (IHS).[9] I choose the inverse hyperbolic sine to accommodate observations with a value of 0. The results are largely insensitive to specification of the functional form (SI B.3.2). The

[9] IHS of x is $ln(x + \sqrt{x^2 + 1})$

interpretation of the coefficients is roughly a percentage change in the outcome for a one unit change in the shock variable [173].

District fixed effects μ_i account for time-invariant heterogeneity that may simultaneously determine levels of resource conflict and shares of palm oil production, for example, district location, factor endowments, local experience during the autocratic regime, or informal institutions born from historical colonization. λ_t is a year fixed effect, which accounts for aggregate shocks to all districts, such as national elections, the global recession, and national policy changes like district splits. The inclusion of two way fixed effects accounts for time invariant production share variable and time series price shock variable with unit and year controls respectively [66].

I include controls X_i that are time-invariant interacted with year fixed effects λ_t to account for the fact that some district traits may have time-varying effects causing different trends. These variables include district terrain features, including ruggedness [174] and forest density in 2000 [105], the share of district GDP from agriculture in 2000, and logged district area. The approach adjusts for the possibility that districts that rely more on farming, are larger, more forested, and with less difficult terrain may be more likely to produce palm but may follow different conflict cycles.

I include province by year fixed effects in some specifications. This compares districts within the same province and the same year experiencing different shocks. Doing so allows different provinces, such as ones belonging to the outer and inner islands, to experience different trends, accounting for the possibility that more remote areas have more palm production and follow different conflict trajectories. I cluster standard errors by the district.[10]

A key assumption is price shocks are exogenous $\mathbb{E}[\varepsilon_{it}|\mu_i, \lambda_t, \text{Palm Shock}_{it}] = 0$. This means that conditional on time-invariant district traits and period effects, the

[10]Results robust to alternative standard error constructions, including province, province-year, and district-year multiway clustering which accounts for cross-sectional (i.e. spatial) dependence in errors. (SI B.3.10)

shock to global prices weighed by the salience of palm to the district economy is not controlled by district i and is mean independent from unobserved transitory shocks ε_{it}.

The lag of prices and use of a pre-sample shares alleviates the concerns of simultaneity. The use of a global measure of prices makes the exogeneity assumption more reasonable. Indonesia has been characterized as a "price taker" of the global crude palm oil price [95]. Due to the rapid processing requirements of fruit, farmers are unable to set prices, making oil palm a buyers market [175, p. 11]. Although Indonesia produces large volumes of crude palm oil, it contributes a smaller share to the international vegetable oil market. Vegetable oils are highly substitutable, which makes it unlikely that Indonesia can swing the global price itself [35, p.166].

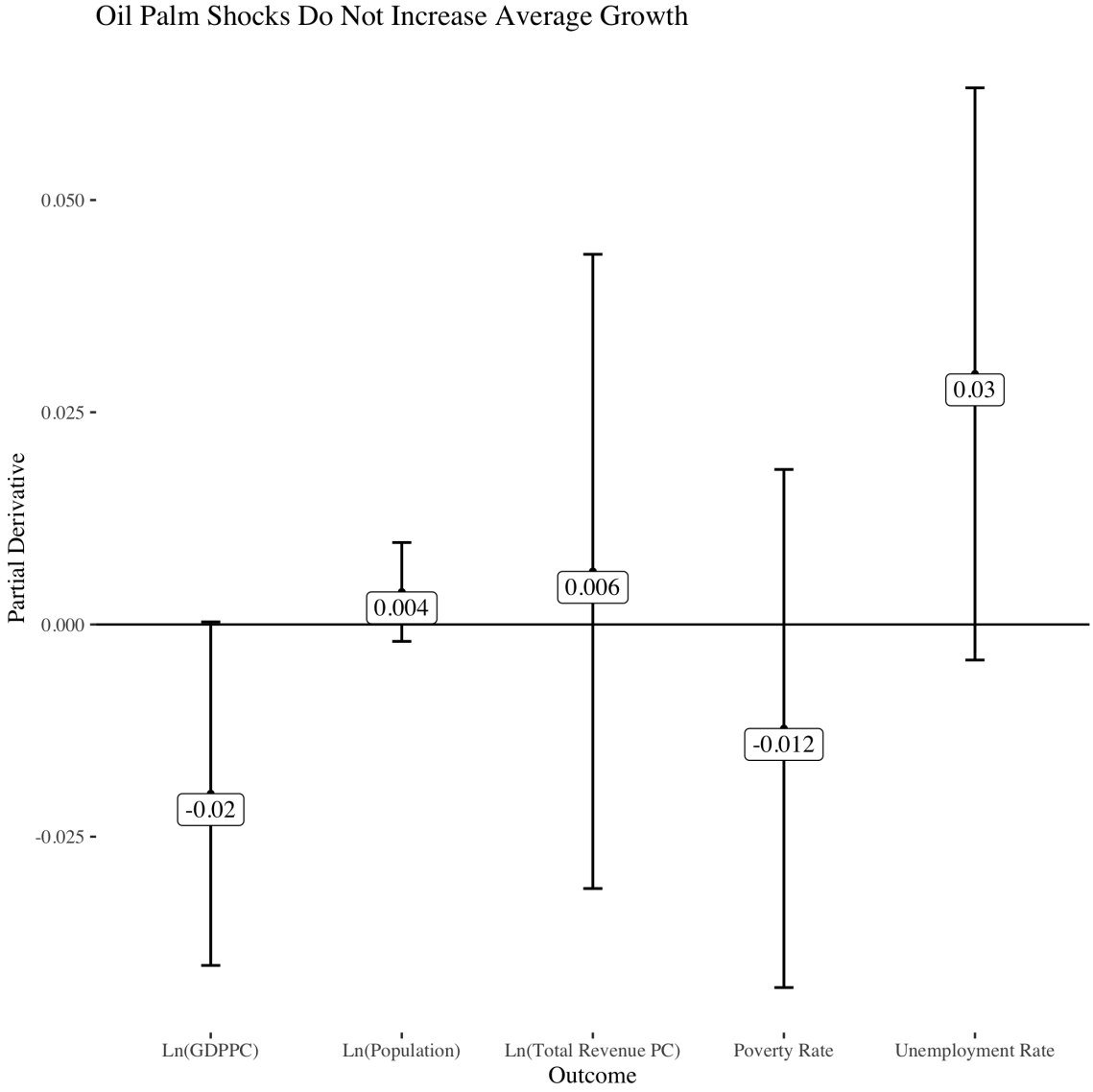
I test this assumption in Figure 2.1. Shocks are uncorrelated with a battery of district economic outcomes, including the employment rate, GDP per capita (log), population (log), revenue (ihs), and the poverty rate.

The results support the argument that price shocks increase the prize of predation faster than economic benefits can reverberate. The estimates are consistent with the argument that oil palm does not bring economic benefits on average for affected communities [151, 52, 167], and with the argument that oil palm brings economic benefits slowly [70].

6 Results

Main results are shown in Table 2.1. The findings show a consistent pattern across models: oil palm price spikes drive a differential increase in resource conflicts among producing districts. Model (1) is the most austere model, including only district and year fixed effects. Model (2) includes province by year fixed effects, Model (3) adjusts for terrain (forestry and ruggedness), and Model (4) includes controls for the

Figure 2.1: Non-Relationship Between Palm Oil Shocks and Local Economic Welfare



Note: Data from Indonesian Database for Policy and Economic Research [107]. Confidence intervals from robust errors clustered at district, all regressions include two way fixed effects. Outcomes standardized for interpretability.

agricultural GDP share (pre-sample) and logged area. The inclusion of covariates results in larger and more precise estimates. Results are unchanged when filtering to rural districts and the outer islands (SI B.3.4) and robust to adjusting for splits overtime (SI B.3.9).

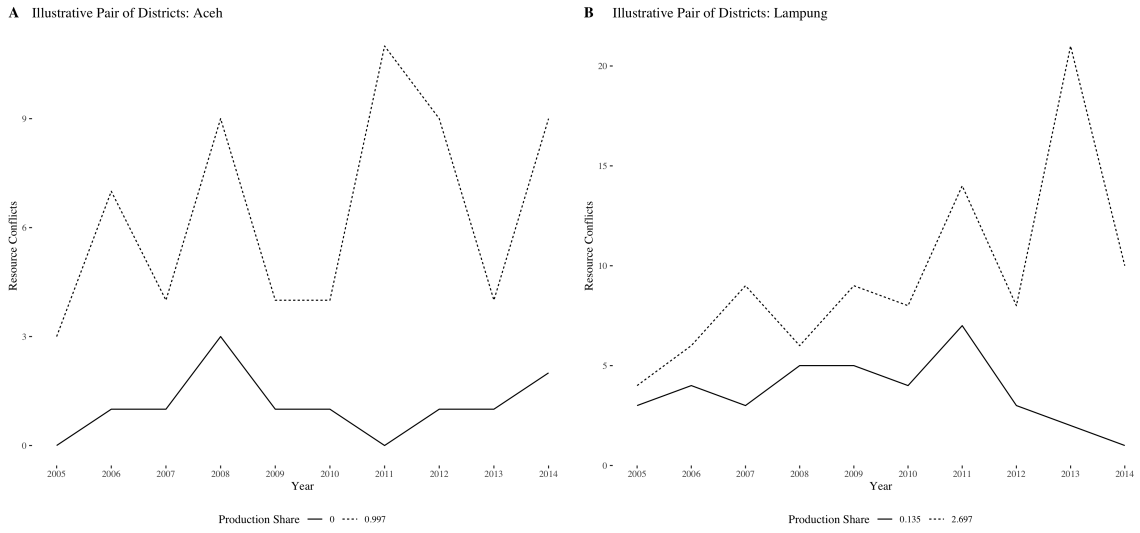
The magnitude of the effect size is on par with meta-analytic benchmarks, although signed in the opposite direction. Meta-analysis from [29] find agricultural commodity have an average effect size of -0.02 on armed conflict. The standardized effect size in this study is 0.0995 in the baseline model (residualized standard deviation of shocks divided by the residualized standard deviation of conflict, multiplied by the estimate in Model (1)). The larger effect size may be attributed to the frequency of social conflict compared to armed conflict. Nonetheless, the size suggests oil palm had an important impact on sub-state conflict in Indonesia.

Consider Aceh Utara, Aceh province, which averaged around 1% production share (13074 hectares), relative to nearby Aceh Tengah, Aceh province, which had near zero production pre-sample. A standard deviation increase in prices corresponds with roughly a 9% increase in resource conflicts in Aceh Utara versus Aceh Tengah. The results suggest when prices increased a standard deviation from 2011-2012, resource conflicts increased by .09 in IHS terms, 7% of the non-producer mean. Figure 2.2 illustrates the effect by plotting the time series for two pairs of districts in Aceh and Lampung province with different production levels.

Figure 2.3 provides more visual support for the relationship. Panel A shows the time series of resource conflict, demeaned by year and district, among districts that produce palm oil, while Panel B shows the time series of standardized global palm oil prices. Panel C plots an event study, regressing resource conflicts on the interaction of year dummies with the cross-sectional measure of palm production shares using the year where price shocks are nearest to zero (2008) as the reference. The coefficients are larger and statistically different from zero after 2008, where prices began surging to their highest level, but attenuate in 2014 when prices began to fall. SI B.3.1 shows the nonparemetric relationship between the price-conflict correlation and production intensity.

To understand mechanism and test for time-varying confounders, I use other con-

Figure 2.2: Time Series for Pairs of Low and High Producers



Note: Panel A shows the time series for two districts in Aceh (Aceh Utara (High Producer) and Aceh Tengah), and Panel B shows the time series for two districts in Lampung (Lampung Utara (High Producer), Lampung Selatan. High producers experience more conflict relative to nearby districts with less production.

Table 2.1: Conflict on Price Shocks: Results

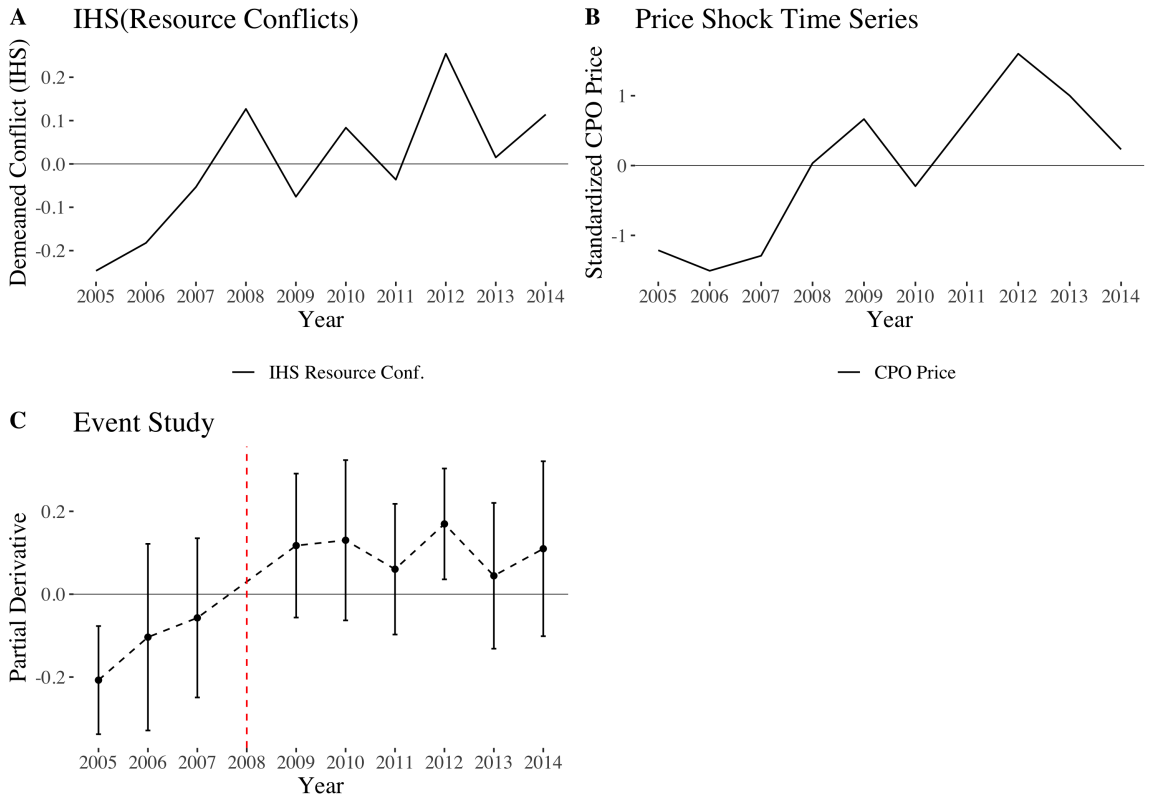
Outcome: Resource Conflict (IHS)	(1)	(2)	(3)	(4)
Palm Oil Shock	0.09***	0.12***	0.11**	0.10**
	(0.02)	(0.03)	(0.03)	(0.03)
SD IHS(Resource Conflict) - Demeaned	0.67	0.67	0.67	0.67
SD Shock - Demeaned	0.73	0.73	0.73	0.73
District & Year FE	✓	✓	✓	✓
Province × Year FE	-	✓	✓	✓
Terrain × Year FE	-	-	✓	✓
Full Controls	-	-	-	✓
N. Clusters	156	156	156	156
Num. obs.	1560	1560	1560	1560

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Note: Robust standard errors clustered at district reported in parenthesis. Outcome is the inverse hyperbolic sine of resource conflicts.

flict outcome in NVMS to conduct a series of falsification tests. If palm producing districts generally followed different conflict cycles versus non-producing districts,

Figure 2.3: Conflict and Shocks



Note: Panel A shows resource conflicts in palm producing districts (inverse hyperbolic sine, demeaned by district and year). Panel B is standardized crude palm oil (CPO) prices normalized in 2001 dollars. Panel C plots event study coefficients interacting time dummies with the cross-sectional measure palm oil production, using 2008 (the year shocks are closest to zero) as the reference category.

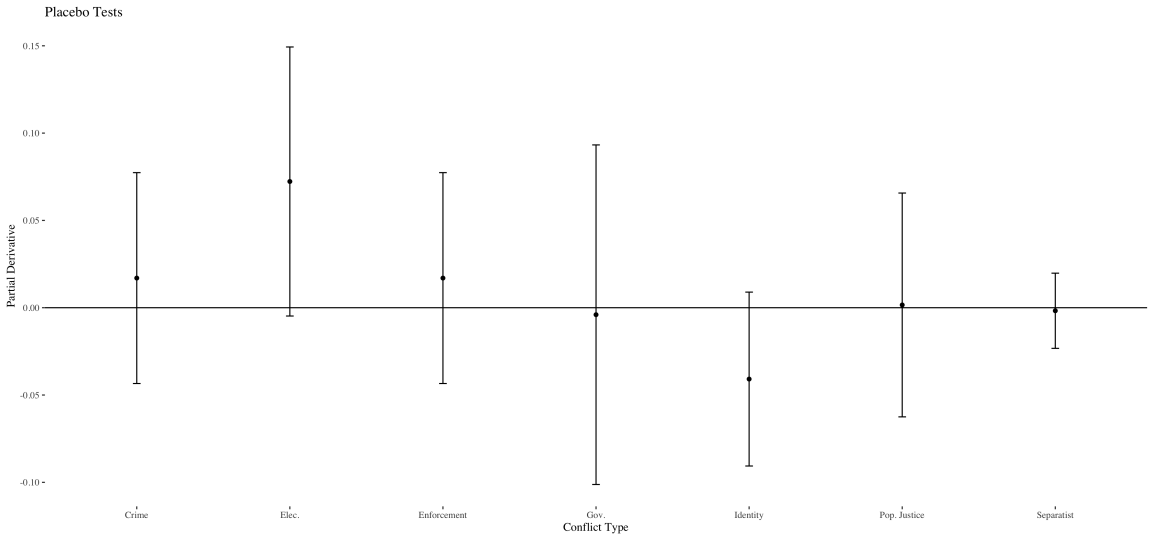
one would detect a non-zero relationship between other forms of political violence and palm shocks.

Figure 2.4 shows the relationship between palm shocks and other conflict outcomes in NVMS - criminal violence, election violence, violence during law enforcement, governance conflict, identity-based conflict, popular justice, and separatist violence.[11] I find a near zero and statistically insignificant relationship between shocks and these conflict outcomes. Failing to reject the null suggests the increase in violence in response to oil palm are not simply an underlying generic conflict trend; price shocks

[11]Details on these outcomes in Appendix A.

have an intrinsic relationship to resource use and access violence.

Figure 2.4: Non-Resource Based Social Conflict and Crime



Note: Horizontal axis refers to conflict outcome on the left hand side of regression, vertical axis is the point estimate, and bands represent 95% confidence intervals. Models include year, district, and province-year fixed effects. SI A includes full descriptions of these outcomes.

Oil palm conflicts may involve state security forces and corrupt local officials, who are typically involved in extending oil palm production into protected forest areas.[12] If resource conflicts were driven by clashes between communities and the state, one would expect violence during law enforcement or violence related to governance to increase. The non-relationship suggests the positive association between palm shocks and resource conflict is not driven by violent interactions between citizens and state officials.

Finally, I find no evidence that separatist conflicts decrease during boom periods. The non-relationship contrasts with prior literature on rebel conflict [66]. However, note the conflict in Aceh had begun deescalating by 2005, therefore separatist conflicts are largely observed in Papua and Maluku provinces at a lower intensity than

[12]For further background on corruption and land deals, see The Gecko Project and Mongabay "How corrupt elections fuel the sell-off of Indonesia?s natural resources." 7 June 2018. `https://news.mongabay.com/series/indonesia-for-sale/`

Colombian civil war.

6.1 Threats to Inference

6.1.1 Anticipatory Effects

One concern is conflict predicts prices, rather than the other way around. If this was the case, future price shocks should predict resource conflict, and past price shocks should have a zero relationship with conflict. If my argument is correct, past price shocks ought to be positively associated with resource conflict, and future price shocks should not. In SI B.3.8, I show price shock lags are positive associated with conflict, whereas price shock leads are not.

6.1.2 Endogenous Exposure

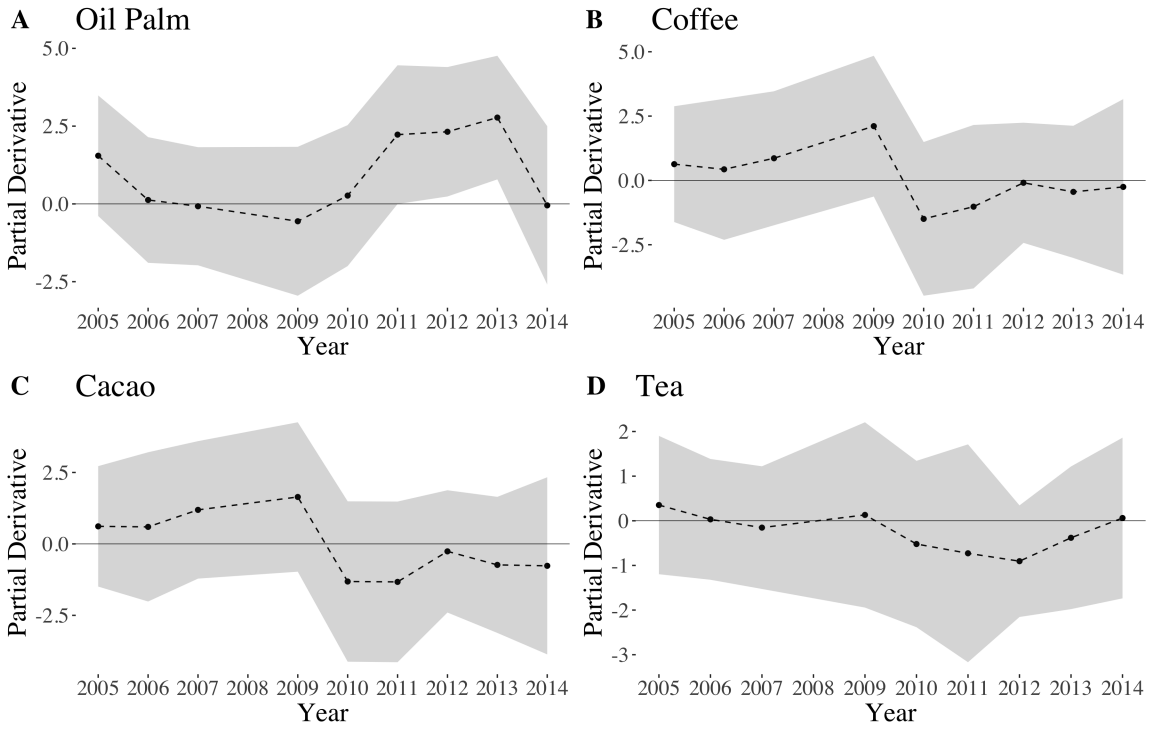
A second concern is the shares of production are not randomly assigned - it could be the case an unobserved spatial process generated the distribution of palm oil production, and the observed positive relationship between shocks and conflict is the artifact of endogenous exposure.

I generate an expected distribution of production shares by averaging 1000 simulations of placebo production weights following the same spatial correlation structure as the observed data. By removing the average of the expected spatial distribution from palm oil production from simulations, the share variable is recentered, and the new measure represents deviations from the expected distribution, purging omitted variable bias [33]. The estimates are again similar to the baseline (SI B.3.6.). Results are also robust to including spatially lagged shocks (SI B.3.5).

6.1.3 Other Cash Crops

To isolate whether the oil palm boom in particular is related to resource conflicts, I use FAO-GAEZ data [105] on district suitability for Indonesia's other primary cash crops, tea, coffee, and cacao for a placebo exercise.[13] These cash crops are quite different from palm oil: palm oil is monocropped, but intercropping is more common with cacao and coffee [35] and these crops pose lest of a threat to forest ecosystems [77], which mitigates the social costs of production. Theoretically, shocks to these commodities should not be positively related to resource conflicts.

Figure 2.5: Cash Crop Suitability and Resource Conflict Trends



Note: Plots show coefficients of fully saturated models interacting period fixed effects with measures of oil palm, coffee, cacao, and tea suitability from FAO-GAEZ. The conflict trends for other cash crop producers do not appear to follow the same trend as palm producers, whose conflict patterns closely follow prices.

Figure 2.5 shows the results of event study regressions of resource conflicts on the

[13]Data from FAO-GAEZ does not exist for rubber crops

interaction of crop suitability and year dummies (2008 reference). Panel A shows oil palm suitable districts experienced more conflict as global prices rose. Meanwhile, Panels B-D show coffee, tea, and cacao suitability does not predict a divergent resource conflict trend. The finding suggests the positive association between agriculture and resource conflict is intrinsic to palm oil. In SI B.3.7, I show price shocks to other commodities are uncorrelated with resource conflict, and that results are robust to crop-specific trends.

7 Mechanisms

I investigate two channels linking palm oil to conflict. First, I study the differential impact of price shocks on conflict in areas where supply bases are sustainability certified, which may moderate the effect of shocks if grievances fueled by inequitable sharing of benefits drives the effect. Second, I examine whether the geographic expansion of crop production influences conflict.

7.1 Firm-Community Relations

If firm and community tensions led to social conflict after price shocks, credible commitments to responsible behavior from producers ought to moderate the effect. Producers that can promise to not grab land and expand operations without consultation, violate worker or community land rights, and carry out operations without ecological harm may not unilaterally use force against locals to expand production. Likewise, communities may be less hostile to producers when their behavior is sustainable.

A mechanism that may enable credible firm commitment to responsible behavior is certification from the Roundtable on Sustainable Palm Oil (RSPO) - a non-governmental organization that sets standards for environmental and social sustainability of palm production, including communities rights to land, soliciting free prior

informed consent from surrounding communities, and abstaining from violence to acquire land [1]. Accredited third party certification bodies verify whether palm oil producers are RSPO certified, and are annually assessed to ensure continued compliance after certification.[14] Palm oil production relies on processing mills to turn fruit into oil. Certified mills must pledge to show all of its fruit suppliers will comply with RSPO guidelines within three years,[15] creating the incentive for mill owners to only accept fruit from socially responsible suppliers who follow RSPO guidelines.[16]

Compliance with RSPO standards and criteria for production can rein in negative externalities of production in two ways. Membership requires producers to publicly declare any plans to expand operations and engage community members before changing production plans; for this reason, activists report working with RSPO certified planters results in more mediation and peaceful resolution of social conflicts [153]. Next, roundtable members typically mediate disputes more often than their counterparts, resulting in negotiation instead of violent conflict [153].

To measure the density of RSPO certification in the local value chain, I use data on the location of RSPO certified supply bases from [88] to construct the following measure of certification intensity:

$$\text{RSPO Intensity}_i = \sum_{j \in J} (1 + distance(\text{district}_i, \text{Concession}_j))^{-1} \text{ for } distance(.) < 100km$$

which captures how many mills within a reasonable traveling distance given processing constraints are certified by RSPO. The measure is motivated by the fact producers need fruit processed quickly - within 24 hours - to create high quality oil for sale, meaning oil palm producers would not be able to travel excessively long distances to sell fruit while still earning a profit.

[14] For more detail on certification visit `https://rspo.org/certification`

[15] See `https://datasets.wri.org/dataset/bc4f0608-aaf4-4a42-a540-5db902d540b7`

[16] More detail on certification process in SI B.2

I estimate an interactive model that includes certification intensity times palm oil prices, palm oil production times palm oil prices, and palm oil production times certification intensity times palm oil prices.

$$ihs\ \text{Conflict}_{it} = \text{Palm Shock}_{it}[\beta_1+\beta_2\text{RSPO}_i]+\delta(\text{RSPO}_i\times\text{Price}_t)+\sum_{k=1}^{k}\alpha_k X_i\lambda_t+\lambda_t+\mu_i+\eta_{it} \tag{2.2}$$

Interacting prices with RSPO certification intensity allows areas with more access to certified mills to follow their own trend net of production shares. Since companies typically own different facilities across the supply chain throughout Indonesia, it is plausible that corporate commitment to certify is orthogonal to local conditions after adjusting district invariant traits.

Table 2.2: Heterogenous Effects

Outcome: Resource Conflict (IHS)	(1)	(2)	(3)	(4)
Shock	0.12^{***}	0.18^{***}	0.17^{***}	0.16^{***}
	(0.03)	(0.03)	(0.03)	(0.03)
Shock x RSPO Centrality	-0.15^{*}	-0.24^{***}	-0.24^{***}	-0.22^{***}
	(0.07)	(0.07)	(0.07)	(0.07)
SD IHS(Resource Conflict) - Demeaned	0.67	0.67	0.67	0.67
SD Shock - Demeaned	0.73	0.73	0.73	0.73
Sd(RPSO)	0.16	0.16	0.16	0.16
District & Year FE	✓	✓	✓	✓
Province × Year FE	-	✓	✓	✓
Terrain × Year FE	-	-	✓	✓
Full Controls	-	-	-	✓
N. Clusters	156	156	156	156
N.	1560	1560	1560	1560

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Note: Robust standard errors clustered at district reported in parenthesis.

I report results in Table 2.2. The interaction of Shock and RSPO is negative,

suggesting that as certified mill centrality increases the effect of price shocks are decreasing. The estimates suggest responsible firm behavior can moderate price shocks.

The moderating effect of RSPO mills suggests firm-community relations play a role in conflict dynamics as prices increase. The result is consistent with [47], who shows firm transparency mitigates the impact of mineral price shocks on contention. However, RSPO may also change producer behavior, which can mitigate conflict via negative externalities. While the available evidence cannot enable a judgement between an information or commitment pathway, it does suggest the producer practices influence resource violence.

In SI B.4.3, I show the result is robust to censoring the data to only analyzing heterogeneity among palm oil producers and analyzing the relationship within districts exposed to RSPO certification or not separately. Moreover, to guard against the possibility that the measure of certification density is solely capturing the concentration of the value chain, I include a placebo test which shows the same measure of non-certified mills does not have the same effect (SI B.4.4). The null result for non-certified mills implies the result is not driven by processing access.

7.2 Land Use Change

A second mechanism is the expansion of oil palm at the extensive margin. The increase in land area dedicated to palm oil production risks displacement, deforestation, and tension over land use.

I borrow from [70] and use a long differences design to examine how the increase in land area dedicated to oil palm relates to resource conflict. Unlike a cross-sectional test, the approach flexibly purges time-invariant confounds that may cause palm production and resource conflict to be correlated. By taking differences between periods, district invariant factors such as land tenure system, historical traits, cultural factors, and climate are removed from the estimates. Unlike a panel, the effect of palm

production increases are allowed to be more slow moving [70]. First differencing also accounts for aggregate shocks that effect all districts equally overtime.

The long difference equation is:

$$(\overline{\text{Conflict}}_{i,2014-2011} - \overline{\text{Conflict}}_{i,2010-2005}) = \alpha_p + \beta \Delta\text{Palm Production}_i + \gamma' \mathbf{X}_i + \varepsilon_i \quad (2.3)$$

where the outcome of interest is resource conflict. I collapse the conflict data into averages after 2010 and before 2010 to measure the change in conflict ($\overline{\text{Conflict}}_{i,2014-2011} - \overline{\text{Conflict}}_{i,2010-2005}$). I use means to prevent a spike in a single year from driving the results and to assess if shifts increase conflict on average. Later, I modify the outcome to look at changes in certain years from 2005. I include province fixed effects α_p, and a vector of controls $\mathbf{X}_i$ (detailed below). ε_i is the HC robust error term.[17] The regressor of interest is ΔPalm Production$_i$ which is the change in the proportion of land area devoted to palm oil production from 2000 to 2010.

To measure the boom in district palm oil, I follow [70] and rely on data regarding the amount of area devoted to oil palm in each district in 2010 and 2000, before and after the large global shock to palm oil demand. I divide the total area of land devoted to palm oil by district area in each year, and compute the difference between the proportion of land devoted to palm oil in 2010 and 2000. This captures the relative increase in production intensity of palm oil within a district overtime. Shares capture the use of land for palm oil relative to alternatives [70].

I exploit cross-sectional variation in palm oil suitability from FAO-GAEZ [105] to instrument the change in production intensity. FAO-GAEZ collects data on the climatic and soil features of land to estimate how well crops may be expected to perform. A concern with regressing changes in conflict on changes in production is strategic producer responses to resource conflict. Production may expand more in areas that are expected to be less hostile to the industry, generating a downward

[17] Results are similar when using Conley spatial HAC standard errors - see SI B.5.2.

biased OLS estimate. I average of high and intermediate suitability for rain-fed palm oil and divide this average by the mean of the sample to captures the relative advantage of choosing to invest in palm production in one district versus another.The 2SLS estimates represent the marginal effect of palm oil production changes induced by better growing conditions on changes in resource conflict.

Instrument relevance and excludability are satisfied if suitability (1) impacts changes in production and (2) only impacts violence through changes in production. As producers choose to grow palm in places where it is more likely to be productive (1) ought to be satisfied. The exclusion restriction (2) is an untestable assumption, however, I include a set of covariates X_i to block backdoor paths, including terrain ruggedness, size, location (latitude and longitude), landlocked status,[18] and the baseline conflict level. Conditional on covariates, it is unlikely fixed climatic and soil attributes determine changes in conflict outside of their influence on changing palm production.

Although one cannot prove the exclusion restriction, I include three tests to assess its reasonability. First, I regress conflict on suitability within a subset without a production shock. If suitability caused conflict through a channel other increased production, one would detect a significant nonzero estimate. Second, I regress conflict changes from their baseline in 2005 from 2006 to 2014 individually (the left hand side in these regressions is $\text{Conflict}_{it} - \text{Conflict}_{i2005}$ for $t = \{2006, 2007, ...2014\}$). I do so using the reduced form and 2SLS specification. If suitability caused conflict outside of the change in production intensity, one would observe a nonzero estimate before changes were completed in 2010. Third, I show suitability is unrelated to changes in conflict and economic fundamentals (SI B.5.1).

I present results in Table 2.3. The OLS estimates in Column (1) are positive but imprecise (p¡.1), suggestive of a sorting process wherein changes in production are smaller in conflict-prone districts. The first stage relationship between changes in

[18]Data from [142]

production and suitability is large, positive, and statistically significant, suggesting that a standard deviation increase in palm oil suitability corresponds to a 2.8% increase in land being dedicated to palm oil production from 2000 to 2010 (Column 2), a .55 standard deviation increase.

I find a positive reduced form relationship in Column (3), but the estimate attenuates and becomes statistically indistinguishable from zero when subsetting to non-shock cases (Column (4)), providing evidence against an exclusion restriction violation. Column (5) shows increases in palm oil production intensity within districts induced by favorable agro-climatic conditions significantly increases the average level of resource conflict from its baseline - suggesting a standard deviation increase in production intensity is related to a 30% increase in resource conflicts.

Table 2.3: Long Differences Results

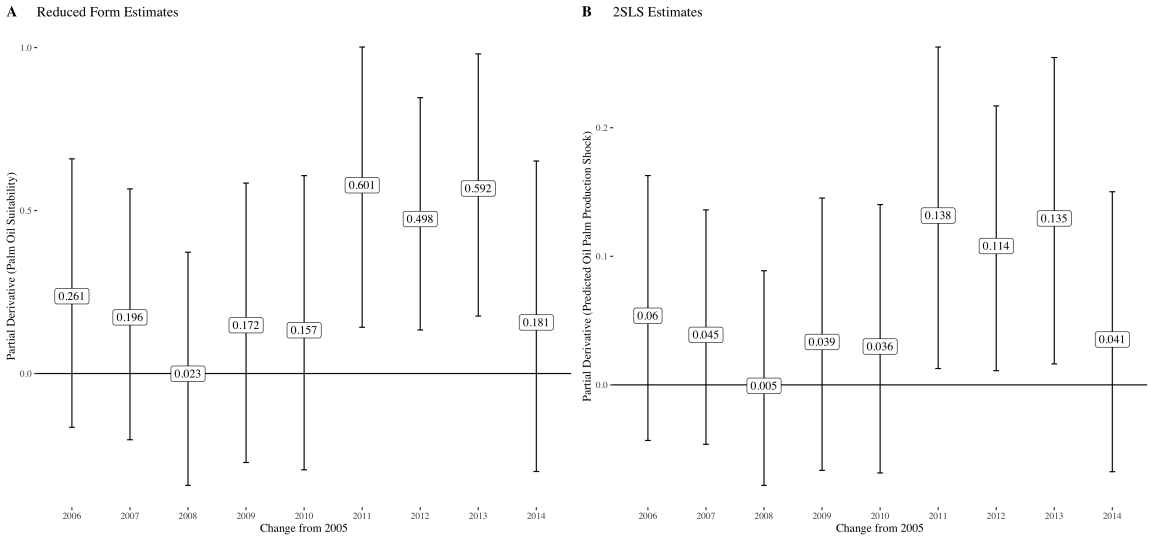
Outcome	Δ Conf. (1)	Δ Palm Prod. (2)	Δ Conf. (3)	Δ Conf. (4)	Δ Conf. (5)
Δ Palm Production	$0.02^{\dagger}$				0.07^{*}
	(0.01)				(0.03)
Palm Oil Suitability		0.04^{**}	0.30^{*}	0.21	
		(0.01)	(0.12)	(0.15)	
Estimator	OLS	OLS	OLS	OLS	2SLS
Province Fixed Effects	✓	✓	✓	✓	✓
Controls?	✓	✓	✓	✓	✓
Num. obs.	156	156	156	90	156
SD Δ Conflict	0.63	-	0.63	0.63	0.63
SD Palm Suitability	0.65	0.65	0.65	0.65	0.65
SD Δ Production	5.14	5.14	5.14	5.14	5.14

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.1$

Next, I regress conflict changes from their baseline in 2005 from 2006 to 2014 (the left hand side in these regressions is $\text{Conflict}_{it} - \text{Conflict}_{i2005}$ for $t = \{2006, 2007, ...2014\}$). I do so with the reduced form equation to check for a correlation between conflict and palm oil suitability before the boom finished as a falsification test, and then do

so with the 2SLS approach. I display the results in Figure B.10. Panel A shows no discernible correlation between palm oil suitability and conflict before the production boom subsided (prior to 2010), which is encouraging evidence that more suitable areas were not simply following a trend of higher conflict. Panel B shows production changes increased violence in 2011-2013 from the 2005 baseline, which corresponds with the years price shocks were at their highest. This is consistent with the theoretical argument that conflict over oil palm is most intense when it is valuable.

Figure 2.6: Long Differences: Conflict Change on Palm Production Change



Outcome is the first difference of resource conflicts across different reference years from 2005 (for example, 2012 refers to $\text{Conflict}_{i,2012} - \text{Conflict}_{i,2005}$). Panel A shows the reduced form where palm oil suitability is the regressor of interest. Panel B shows the 2SLS regression where change in production from 2010-2004 measured as the proportion of district area devoted to palm oil production is the endogenous variable. Points represent estimates of the partial derivative and bands represent 95% confidence intervals.

8 Discussion and Conclusion

I document a notable exception to the empirical regularity that positive agricultural commodity shocks result in social peace. Oil palm enflames tension rather than pacifying conflict due to the concentrated income gains and negative externalities

associated with boom periods. The relationship is intrinsic to palm in this setting, as crops which do not share its features do not result in conflict when prices boom. The effect of price shocks on social conflict is decreasing as socially responsible processing mills become more concentrated. The results connect commodity price shocks to conflicts over land: the spatial expansion of the palm crop spurs conflicts within districts as well.

The finding highlights particular agricultural commodities may not increase the opportunity cost of violence. Since oil palm does not increase the opportunity cost of conflict immediately due to slow maturation of the crop, barriers to entry, and price contracts, positive shocks do not counteract the rapacity effect by immediately increasing wages or employment prospects. Therefore, despite its labor intensity, oil palm surges create incentives for predatory conflict in the short-run.

Understanding when agriculture will not result in social peace informs theoretical and policy debates on conflict prevention. As noted by [54], development experts and states alike have thought of export crops as a powerful tool to improve social peace; yet, as they highlight, agriculture can instead fuel violence in fragile states when rebels can prey on value chains. I show the conditions where agriculture can fuel social conflict are broader: a country need not have its value chain exposed to rebel expropriation for price shocks to fuel conflict. Instead, crop booms can lead to violent conflict between commercial and labor interests under the right scope conditions.

Several avenues for further research exist. First, the theory and evidence outline how tropical oil crop expansion can result in conflict, however, it has only shown one path by which conflict can be moderated (mill certification). Future work can clarify the potential role political institutions, social insurance, or labor protection could play in reducing violence in the wake of positive shocks. Such interventions may balance growth during boom periods, removing the grievances related to conflict.

This may clarify causal mechanisms - RSPO certification provides both credible

commitment and transparency about firm activity, meaning this paper cannot isolate whether private governance dampens conflict by solving information or commitment problems between competing groups. Future work that unpacks which moderator has the largest influence can guide future aid and development policy.

Second, the theory has outlined how transitory oil crop price shocks may fuel conflict, largely due to changes in land use and the inaccessibility of the sector. Yet, as the industry matures, the local economy may restructure and positive cash flows may deter conflict in line with the opportunity cost expectation. Since the oil palm boom is relatively young, studying the persistence and decay of oil crop boom over the long term is difficult. Researchers could instead leverage historical agricultural changes to estimate the persistence of dislocation as a cause of conflict to begin understanding whether or how society moves away from violence after large local economic changes.

Third, studies may explore other oil crops, such as soybeans, in other countries, like Brazil [2]. The absence of an effect for non-oil cash crops which do not pose pressure on land use oil based - coffee, cacao, and tea - suggests oil crops in particular are conflictual. Although evidence on the Indonesian case is intrinsically valuable, understanding whether and if oil crop expansion is related to social conflict in other developing nations may provide important evidence for policymakers as the oil crop sector continues to grow cross-nationally.

Chapter 3

Colonial Origins of Insecurity: Evidence from Brazil

1 Introduction

The distribution of coercion is the state's core task - governments must monopolize the legitimate use of violence, preventing predatory and opportunistic interpersonal lethal conflicts, while also avoiding draconian violations of civil liberties. Many developing states struggle to monopolize legitimate force evenly across their territory, leaving the most vulnerable and disenfranchised citizens subjected to chronic insecurity and police abuse [96].

Why does the efficacy of the law vary considerably within states? The existence of subnational regions with high homicide rates where state coercion is infrequent yet arbitrary poses a direct threat to democratic citizenship and the rule of law [16, 152, 192]. Organized criminal operations within states have proven to be incredibly destructive; in some countries, homicides eclipse causalities from warfare and civil conflict [20]. Despite the persistence and lethality of "ungovernable" areas within states, the historical roots of violent crime across space are not well-understood.

I argue extractive colonial institutions created a path dependent process of unevenly applied state coercion, fueling crime, violent policing, economic polarization, and informality. Extractive colonial institutions were highly socially stratified, creating a wealthy oligarchy in opposition to underprivileged masses [6]. Non-elite citizens, with little hope of social advancement under the extractive system, either attempted to overturn the system or exit the arrangement. Therefore, maintaining extraction required an alliance between the state's security sector and economic elites, who worked together to temper social conflict and sustain coerced labor [5]. Further, survival for the disenfranchised relied on informality, since formal institutions were not designed to enhance their welfare.

Once colonization ended, elites wished to shield themselves from expropriation, and used their de facto power to segregate themselves from the underclass, maintaining their connection with the state's coercive arm to protect their wealth. Since the state relied on elites for economic growth, the pattern of unequal service and protection favoring the wealthy persisted. In fact, allowing some areas to be underpoliced by delegating control to criminal groups allowed the state to invest resources elsewhere, leading to a symbiotic relationship between development and pockets of criminality [12, 58, 57]. As a consequence, historically disenfranchised groups were relatively under-protected by law enforcement, opening space for predatory actors and forcing communities to rely on self-help for protection. Due to weak connections between law enforcement and under-protected groups, the extent of police presence was characterized by coercion rather than collaboration, and the threat of crime led powerful interest groups to lobby for increasingly repressive security measures.

I study the relationship between historical extractive institutions and modern public security in Brazil. Brazil imported more slaves than any other county during the Atlantic slave trade [126]. Today, it reports some of the world's highest homicide rates. I argue the inefficacy of the government's coercive institutions can be explained

by the legacy of colonization at the local level. The initial Portuguese settlement of Brazil was initiated by the creation of Donatary Captaincies (DCs), spanning from the coast to the Tordesillas line, a meridian that separated the areas of the new world that Spain or Portugal could legally settle. DCs were allotted to elites, who could extract resources in exchange for administering the land. The product of the DC system was a highly stratified society, characterized by inequality and slavery. Although the DC system was short-lived, it created the base of the colonial economy, influencing subsequent colonization patterns and social structures [14, 61, 134]. Since the heads of captaincies could extract resources in accordance to their personal will irrespective of community inputs, the institution epitomized extractive colonial institutions.

To credibly isolate the impact of extractive colonization, I follow [80] and exploit modern municipalities distance Tordesillas line under a regression discontinuity (RD) framework. I illustrate my argument in two steps. First, I show this increased extractive social relations at the intensive margin: consistent with [80], the percentage of the enslaved population in 1872 discretely increases at the boundary. Further, municipalities distance to colonial revolts and run-away slave communities increases at the boundary, suggesting conflict between elite and coerced classes was more intense in the areas formerly under the DC system.

Next, I study the persistence of violence and coercion in the modern day. I find municipalities east of the line (1) have more homicides per capita, which is driven by an increase in the rate of Afro-descendent homicides and (2) have more law enforcement killings of citizens. The results are consistent with the theoretical expectation that municipalities more exposed to extractive colonial institutions were placed on the equilibrium path whereby the state provided uneven protection across social classes, leading to informal economic and security provision, which gave way to organized crime and interpersonal violence.

Finally, I also find these municipalities east of the line have (1) a higher probabil-

ity of having a Municipal Guard, an auxiliary policing force rooted in the Portuguese colonial era and (2) spend more on public security. The result is consistent with local elite investment in security. However, the increase in security does not necessarily translate into less homicides overall, because the intention of more police is the protection of wealthy areas rather than informal and hard-to-govern spaces within municipalities. I provide suggestive evidence that this is the case by finding (1) economic segregation increases at the discontinuity and (2) informal housing settlements, such as favelas, increase as well.

My study contributes to debates about conflict and long-term state capacity and the influence of colonial institutions on development. Bellicist theories of state formation emphasize interstate conflict as the driver of state development [186, 28, 85], however scholars argue Latin American regional development was less characterized by interstate war than Europe [179, 184].[1] Inter-elite conflict theories emphasize competition for influence between elite groups [27, 82, 83].

I emphasize elite versus mass conflict as a driver of long-run outcomes [30, 94, 178]. Scholars have shown the sudden reduction in mass threat can facilitate state centralization [84] or push elites away from state provided security [64]. How the persistence of mass threats shapes violent crime and state coercive capacity in the long-term is less well-understood. I argue the coalition of elites, who drove development, and the state's coercive apparatus, which enforced elite interests, formed a stable political pact overtime because elites needed state protection from expropriation from the masses, and the state needed elites to continue driving economic production. However, this alliance had deleterious long-term consequences: it led to more violent crime as underclass citizens were pushed into informal sectors, economic segregation, and support for more repressive policing. My results show the impact on coercive capacity is nuanced; while the ability to coerce increases, the efficacy at providing security for

[1] For an argument in favor of the view that the war made the state in Latin America, see [170].

all citizens declines.

Next, a large literature in comparative development emphasizes how extractive colonial institutions undermined long-run prosperity and fueled inequality [6, 63, 72], specifically reducing public goods [109]. I contribute to this literature on three fronts. First, the extent to which colonial institutions fueled modern crime is less well-known. Second, I show colonial extraction increased the state's attempt to provide security, a public good less discussed in the literature, due to the increase in coercive capacity as a result of elite lobbying. However, in practice, the provision of the good is uneven, as homicide rates remain high. Third, I show extractive colonization did not just increase variance in income, it also affected the spatial distribution of income within municipalities.

Finally, I illustrate how colonization is a deep structural cause of criminal control of state spaces and human rights violations. Scholarship on the historical causes of crime in Latin America tend to emphasize authoritarian legacies [90, 91] or the inheritance of formal legal institutions from colonizers [139]. While important, macro-institutional explanations cannot account for subnational variation in violence. I compare municipalities within the same country, meaning all units in the study were exposed to military dictatorship, and all units were subject to the same de jure colonial legacies. The key difference between municipalities east and west of Tordesillas is a de facto imbalance of political power between white and Afro-descendent Brazilians, rather than different criminal procedures or policing institutions.

2 Critical Junctures, Multiple Equilibria, and Violence

The state can manage internal conflict in several ways, although the distributional consequences of the chosen methods are not equal. One approach is open access or-

ders, where entry and exit into political and economic institutions is relatively free, stimulating competition for power and circulation of rents across society. Since advancement in open access orders is attainable and consumption of rents is contingent on peace to maintain production, actors across society are incentivized to abide by laws, follow rules, and abstain from violence [147].

Another approach is "the natural state," wherein an elite coalition blocks access to rents, sharing society's resources amongst themselves. Elites know factional conflict amongst themselves threatens to breakdown the exclusive economic system, since failing to remain united makes them vulnerable to attempts by the masses to force redistribution through conflict [147].

A particular manifestation of the natural state was the extractive colonial economy. Extractive colonial institutions were designed to maximize rents for an elite coalition that controlled the state, at the expense of a disenfranchised mass population, many of whom were forced into coerced labor [4]. The state's coercive apparatus under extractive colonial institutions was designed to prevent mass uprising, collective dissent, and protect elite wealth, rather than equally serving and protecting all living within the state [73, 96, 185]. The role of law enforcement under an extractive system differs from an open-access order; [73, p.38] describes the open capitalistic system as creating "an atmosphere of submission and of inhibition which lightens the task of policing considerably." In contrast, the threat of violence from below led elites to establish repressive law enforcement organizations to confront revolutionary threats [73, p.38].

Origins of these structures occur at critical junctures of state development. Once the economy is based on an elite coalition with the state, the process of law and order asymmetrically serving the wealthy is path dependent. Although colonial institutions go away, the wealth and power elites acquired from extraction does not always fade in kind. Elites then leverage de facto power to maintain patterns of extraction overtime

[3, 145].

Fearful of redistributive conflict, elites used their pivotal importance to ensure the state would criminalize the poor and protect their wealth. In the American South, this occurred through convict leasing [9, 157]. In Latin America, economic elites leveraged their importance for state development to lobby for police repression of the working class and labor [58, 59, 57]. The state strategically strengthened its coercive apparatus to prevent mass labor movements, but concentrated its efforts at protection towards wealthy areas. In low-income areas, the state accepted its ability to protect was limited, and allowed informal commerce [99] and even criminal governance [12]. By accepting informality, the state was able to channel resources away from low-income communities and towards other objectives, however, a consequence was a languishing relationship between state and society in low-income areas [57, p. 64]. As informal markets grew, so did the influence of illicit actors and criminal organizations, who relied on non-state coercion for contract enforcement, boosting violent crime rates.

The end product of the extractive system was an economically polarized cityscape - in some neighborhoods, wealthy elites enjoyed robust protection from law enforcement, whereas in others, disadvantaged communities were engaged in informal economic activity, where state regulation was supplanted by criminal organizations, which the state and police tolerated [12, 57, 36]. The process created a self-perpetuating cycle: elites, originally concerned about redistributive conflict from the masses, shifted their concern to expropriation from criminals, which further justified more policing focused on repressing the poor and keeping them at bay from wealthy areas. In turn, the reliance on the informal sector became more important for non-wealthy citizens, feeding back into the underlying cause of criminal presence and predation in their neighborhoods.

I contextualize my argument by focusing on the case of Brazil. Focusing on a singular case allows me to study subnational variation in violence, holding constant

macro-institutional legacies such as authoritarian experience and criminal codes which may confound cross-national comparisons. Aside from unique colonization patterns in Brazil which enable credible identification of the colonial legacy, the case is a particularly important one to understand given its size and its relatively high homicide rate.

3 Donatary Captaincies, Tordesillas, and Colonization of Brazil

Colonization of Brazil began in the 16^{th} century after the Spanish and Portuguese negotiated the Treaty of Tordesillas in 1494. Land to the east of the Tordesillas line could be claimed by Portugal, whereas one's to the west belonged to Spain. The treaty was formalized before Brazil was understood by Europeans, during the process of negotiation, both nations were unaware of "what it was giving away or getting..." [60, 166], meaning the line could not have been influenced by political manipulation on the part of the Portuguese to claim desirable territory or formalize preexisting settlements. Due to geographic barriers, Spain's presence in Brazil was considerable lighter than Portugal's, which went on to claim all of contemporary Brazil, albeit with uneven settlement [155, Ch 1].

To incentivize colonization, the Portuguese crown exchanged segments of land to nobles, which stretched from the coast to the Tordesillas line, the meridian that separated the land Portugal could claim from Spain [61]. The tracts of land were called Donatary Captaincies (DC). The crown expected nobles to defend and administer the land under their jurisdiction, in exchange, they were allowed to exploit and profit from natural resource wealth. By allowing individual elites to have such a high degree of control over economic development, the DC system epitomized an extractive colonial institution [4]. The heads of DCs - called captains - were allowed to act under the

assumption lands were"his own personal possessions" [112]. Captains were responsible for taxation, establishing militias, and issuing land grants [14, p.28]. "Relatives and clients of grantees" staffed the administration of small towns and capitals established within the captaincy [14, p.27]. [2]

The DC system influenced the trajectory of local development despite its short-run existence; "...a number of social and economic patterns were established that continued long after direct royal control had been established" [134, p. 186]. The DC system may have been a short-lived legal system of administration, but it "...permitted the implantation of an initial administrative, economic, and social structure within the colony" [14, p. 43]. Captains were given the power to issue private land grants (*sesmeiros*) to persons who could put the land to productive use. Since land was abundant, massive grants were given to individual families, creating large-scale agricultural settlements [61, 134]. The result was a landed elite with ownership of large estates in DC areas.

3.1 Landed Elite: Social Structure and Conflict

The emergence of a landed elite on large-scale plantations created a hierarchical and rigid social structure, centered around coerced labor and economic inequality [145]. Plantations relied on slavery for production, [72], which led to the import of Africans due to Portugal's comparative advantage in the slave trade [80, 126].

Elite landowners were perpetually fearful of slave uprisings, which posed a threat to their economic interests and social position. [126] document "...violence was not only against the slave, but was also a permanent part of the mentality of the owners with their constantly expressed fears of slave rebellion, especially in communities with a high ratio of slaves to free population" [126, p.209]. To repress and deter slave revolts, elites relied on coercion, particularly in the form of police and law

[2]The DC system formally existed for only a decade; it was abolished in 1549.

enforcement organizations. As put by [100] "... the economy and society of Brazil depended fundamentally on slavery, and because slaves were unwilling to submit to their condition without coercion, state authorities and the slavocrats found themselves in uneasy collaboration."

Slaves in Brazil resisted their conditions by flight and direct confrontation with the colonial state. Aside from revolts, runaway slave communities, called *quilombos*, found themselves in violent conflict with colonists, who deployed military and police forces to destroy settlements. Slave rebellion, and the subsequent establishment of *quilombos*, created a militarized response from the colonial state [171].

Economic elites and early state officials relied on one another for production and coercion respectively, leading to a symbiotic relationship between the wealthy and security services [101]. Elites preferred a coercive structure that would treat slaves and slave owners much differently, undermining the rule of law.

As the Brazilian state became more solidified, the social stratification that flowed from the landed oligarchy shaped the production of law and order. The first policing organizations were formed in Brazil in 1808, after the Portuguese crown based themselves in Rio de Janeiro [101, p. 30]. The primary goal was providing order for elites [104, 49]. Financing of police forces came from fees and loans paid by economic elites, giving local landowners leverage over the exercise of state authority [101, 34]. The police were largely a repressive force designed to maintain the extractive economy. On this point, [101] writes:

> "[t]he police force can also be seen as offensive, aimed at establishing control over territory both social and geographical - the public space of the city - by subjecting slaves and restraining the free lower classes through intimidation, exclusion, or subordination as circumstances required" [101, 36-37].

Police posts were often filled as a way to win favor of local elites; [78] offers an

example from a correspondence between a sheriff and the Ministry of Justice, where the sheriff noted a family was "...annoyed with the government for not having named them subdelegados or alternates . . . in spite of their being the richest planters of the area."

3.2 Path Dependence

The colonial period shaped spatial patterns of development across Brazil in path dependent ways. The end of slavery promoted migration of Afro-brazilians to urban areas looking for employment, given their initial state of poverty and employment discrimination, this migration promoted an expansion into low income areas [126, p. 316]. Elites, concerned about crime, moved to areas in cities where they would be more remote from the poor due to concerns of exportation [36]. Economic segregation effectively created two cities in one: wealthy neighborhoods that enjoyed protection from the state, and poor neighborhoods that did not.

The end of colonization and slavery did not change the alliance between the state, its coercive arm, and economic elites at the subnational level. Although de jure rules had changed, elites had the will and capacity to use police as a tool to maintain their position, and since the state relied on elites for continued development, the two cooperated while excluding lower classes. Police were used to suppress organized labor [65, 10, 42-43]. The state's coercive apparatus grew quickly in the beginning of the the 20$^{\text{th}}$ century to enable the control over labor in the growing industrial economy [89, 74-78]. In this vein, [79] argues:

> "[v]iewed in the larger sweep of Brazilian history, the roots of police violence and arbitrariness toward labor in the 1930s and 1940s must be sought in the world of slavery, an institution that had been abolished only fifty years earlier" [79, p.142].

A United States military attachè remarked in 1934 in response to a police union

conflict "[i]n Brazil, certainly in Rio De Janeiro, only the simple minded appeal to 'police protection' - unless its protection from the police" [10, p 61]. Police quickly resorted to violence in response to labor strikes, as one Italian consul noted in a 1906 report, police "hardly distinguish between strikes and revolts" [79, p.130].[3].

The use of law enforcement to protect elite interests in alliance with Brazil's developmental dictatorships, in tandem with spatial polarization of economic groups within municipalities, meant the coercive arm of the state was expanding without addressing violent crime among poor neighborhoods. The absence of a strong state fuels competition among criminal groups vying for influence, driving up homicide rates [192]. Due to a muted state presence, locals in poor neighborhoods rely on police less than informal means of dispute resolution, shying away from cooperation with law enforcement. As a byproduct, police rely on coercion and brutal tactics to control poor neighborhoods, since they cannot obtain collaborators to assist in policing tasks. Brutality, however, feeds back into citizen reluctance to cooperate, reinforcing a cycle of crime and police violence [12, 129, 160].

3.3 Observable Implications

Based on the theory and Brazilian context, I form three sets of hypotheses across two time periods: the colonial/imperial era (1500-1888) and the modern day (defined as 2010, based on most recently available census data). The first set of hypotheses concern municipalities expose to extractive colonialism and social conflict historically. I expect municipalities east of the Tordesillas line to have been exposed to more slavery, since the economies east of the line where characterized by oligarchical plantation agriculture. Further, I expect revolts and quilombos to be concentrated east of the line, since slavery was more prevalent there.

The next set of hypotheses test the core of the argument: I expect homicide

[3]More detail on reforms and organization of police in Appendix A

rates to be larger east of the Tordesillas line due to the legacy of law enforcement gravitating towards protection of the wealthy. Individuals are more likely to commit crimes when the probability of detection and the opportunity cost of violence are low [25]. When law enforcement is less present, predatory actors are more likely to emerge, specifically targeting persons when they anticipate law enforcement will be unlikely to successfully investigate the crime.

I test my mechanism using data on the ethno-racial group of homicide victims. While I lack a precise measure of victim income, whether a victim was a Afro-descenent (black or mixed-race) captures whether they belong to a social group historically not protected by law enforcement. I anticipate the increased homicide rate to be largely driven by victims who were persons of color, who make up a large share of persons within economically disadvantage neighborhoods. Relatedly, I anticipate more police violence due to officers reliance on coercion instead of cooperation for control.

Despite higher homicide rates and languishing police presence in poor areas, I expect more policing overall at the municipality level east of the line. The reason for this somewhat paradoxical relationship is wealthy segments of society demand additional security measures to remain safe from crime as homicide rates increase in poor neighborhoods. While the state's coercive apparatus grows in size, because the distribution of protection is uneven, homicide rates remain high as well.

Finally, I hypothesize economic segregation and informal housing settlements will be higher among municipalities east of the Tordesillas line. The wealthy moved further from the poor after colonization, who moved to cities to search for work but were concentrated in informal housing settlements, which only grew overtime with urbanization. Since informal housing operates outside of the law, citizens are comparatively less reliant on and trusting of the state for services [57]. Economically segregated areas further concentrate persons who have a low opportunity cost to crime

among persons who are less costly to victimize - the poor [115].

4 Research Design

4.1 Data

For the main analysis, I use data from the 2010 census. I focus on municipalities because they are the smallest relevant administrative unit in Brazil. Theoretically, persistence via social structures would predict increased levels of violence and state coercive capacity at the local level, because the mechanism connecting historical extraction and contemporary outcomes is de facto spatial development rather than de jure institutions. A state-level analysis would be inappropriate both because further aggregation may mask variation in local exposure to extraction, obscuring the relationship between history and modern outcomes, and would also bundle state-level policy with state-level history. By comparing municipalities net of the state-specific heterogeneity, my design isolates the impact of colonial boundaries via local de facto channels rather than state-level institutional channels.

I use historical census, georeferenced atlas, and contemporary administrative datasets to measure outcomes across space and time. Data on slavery is available in the 1872 census. I use the proportion of slaves to the population to measure exposure to coerced labor institutions historically [80]. Information about the number of slaves at the local level is not available in earlier years, and 1872 was near the end of slavery in Brazil. Despite this limitation, a larger share of slaves late in Imperial period suggests slavery's persistence at the municipal level over the course of the empire, which captures the degree to which the practice was embedded in the locality. I use areal weighed interpolation to match the intensity of slavery in 1872 municipalities to 2010 municipalities.

I use data from the Digital Atlas of Portuguese America [87] to measure the

location of historical revolts and colonial forts. I use the location of forts and revolts to compute a municipality m's distance to the nearest fort or revolt. Larger values indicate a municipality is further from a colonial fort or revolt, and smaller values indicate the municipality is more proximate to these historical events and structures.

I use mortality data from the Ministry of Health, which measures causes of death per municipality. I use deaths ruled to be caused by aggression to measure homicides. The data include the race of the victim, which I use to construct homicide rates by white and Afro-descendent (*preta* and *parda*) Brazilians. Theoretically, the uneven protection by law enforcement implies that homicide rates ought to be higher among Afro-descendents, who have been afforded less protection by law enforcement historically. I use the same data source to collect deaths caused by law enforcement - due to the relative rarity of the event, I pool this data across 2010-2015.

Systematic information on law enforcement personnel is not available at the local level. Although state governments play the largest role in policing, municipalities are "increasingly important" in crime prevention, and can exceed the floor set by states [39]. I use data on public finances to measure municipality public security spending per capita. Scholars have used the same indicator to study municipal effort towards fighting crime [148]. Although municipalities do not shoulder the majority of the security burden, larger municipal expenditures towards security indicate additional effort towards public safety, which is the quantity of interest.

I measure economic segregation among the top 20 and bottom 80 percent of the income distribution within municipalities using data from [111]. The data compute a spatial dissimilarity index (D) and a spatial information theory index (H) [158]. Each measure achieves its maximum under complete segregation, with smaller values representing individuals living in more diverse environments.

Finally I use the survey of Basic Municipal Information to measure the degree of informal/irregular housing settlements in a municipality The measure is scored 1

if a municipality reports favelas, occupations of lots, irregular/clandestine lots, or tenements, and zero otherwise.

4.2 Empirical Strategy

The discrete change at the Tordesillas boundary suggests a geographic regression discontinuity approach which compares nearby municipalities on either the eastern or western side of the line with the following estimating equation.

$$out_m = \alpha + \gamma \text{East}_m + f(\text{geographic location}_m) + \phi_s + \varepsilon_m \tag{3.1}$$

out_m is the outcome(s) of interest, including historical proximity to forts, revolts, and intensity of slavery, as well as contemporary criminal violence, police violence, and investment in municipal security. $\text{East}_m = \mathbf{1}\{\text{Longitude}_m \geq -48.7\}$ is a binary indicator scored one if a municipality lies on the eastern side of the boundary and zero otherwise. I define the boundary following [80] at 48°42″W, or -48.7 in decimal degrees, although the precise location of the boundary is a lively subject of historical inquiry [49]. The original text of the Tordesillas treaty drew the meridian in terms of distance from Cape Verde, however, whether distance was to be measured from the center or the coast of the island was unspecified.

A benefit of the uncertainty around the line is agents were unable to precisely sort around the border; colonizers would have known they were approaching the boundary but would not have been able to exactly select where to situate themselves to fall on one side or another. Since the meridian was drawn prior to the exploration of Brazil, its unlikely that the boundary separated land with vastly different factor endowments.

A disadvantage of this uncertainty is that the scoring variable and exposure indicator are noisy, introducing classical measurement error. To address attenuation bias and the potential for bias resulting from data heaps, I follow [80] and estimate

a donut RD [21]. The donut RD is a consistent estimator of treatment effect in the presence of measurement error or heaping in the running variable [21]. I drop data within 73 kilometers of the meridian on either side of the Tordesillas line, as in [80].

The scoring variable $f(\text{geographic location}_m)$ is the deterministic function of treatment status. For estimates in the main text, I project latitude-longitude coordinates into a single dimension by measuring the distance from the municipality's center to the line. In appendix, I include estimates using a polynomial in latitude-longitude space.[4]

Contemporary state-by-state policies and institutions are salient for determining modern homicide rates and security spending, since governors exercise considerable control over crime and social policy. Therefore, I use state fixed-effects ϕ_s to remove state-invariant heterogeneity.

The robust error term is ε_m. I cluster standard errors at the microregion level. Microregions are statistical constructs by IBGE which place nearby municipalities with similar economic, geographic, and demographic characteristics into groups. Similarity of municipalities within these clusters suggest similar error terms and treatment probability, meaning clustering at these levels ought to account for spatial dependence in a more principled way than constructing error correlation ranges using a Conely-type approach, where spatial autocorrelation is estimated by the researcher.

I probe the robustness of my primary approach by using the [38] method. I report results in appendix at alternative bandwidths using the [38] robust nonparametric confidence intervals, obtaining similar findings.

[4]Letting x denote longitude and y latitude, I follow [63] and include a linear specification ($x + y + xy$) and a quadratic ($x + y + xy + x^2 + y^2 + x^2y + y^2x$) to adjust for nonlinear functional form when coordinates are the running variable.

4.3 Identification Assumptions

Identification relies on the assumption that confounds are continuous at the discontinuity. Since the line was negotiated prior to exploration of Brazil, it is unlikely that land east or west of the line is significantly different. Discrete changes in factors that existed prior to colonization would suggest an increase in slavery, homicides, or security sector presence cannot be attributed to patterns of colonization induced by the Tordesillas boundary, and instead reflect geographic, climatic, or spatial determinants of prosperity.

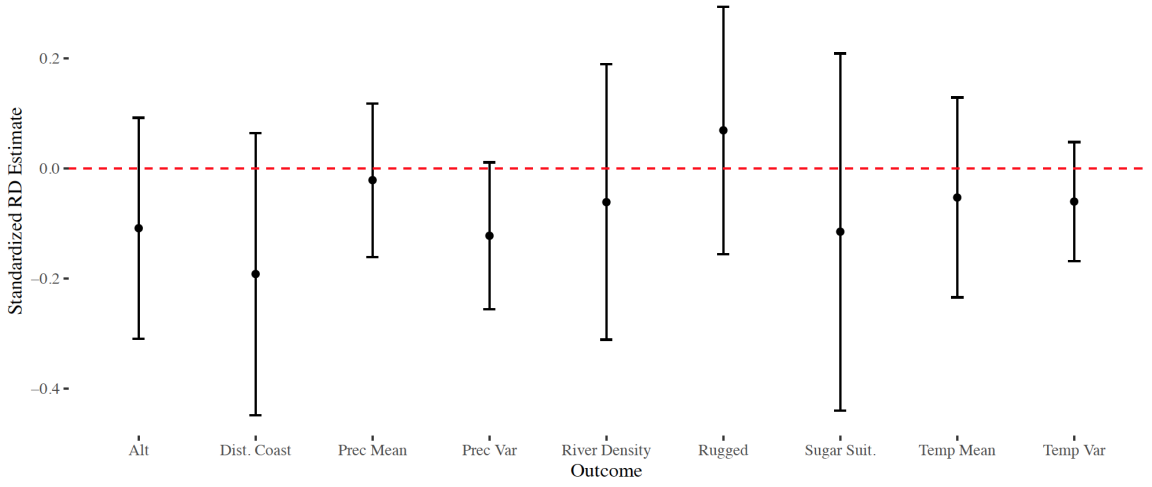
I use data on altitude, terrain ruggedness, sugarcane suitability, temperature (mean and variance), precipitation (mean and variance), river density, and distance to the coast to measure initial factors. Figure 3.1 shows the results, where the outcomes are standardized. I find no jumps in pre-determined outcome variables, consistent with the assumption that the only salient change at the Tordesillas boundary is historical colonization rather than factor endowments. Although the estimates are statistically imprecise, the point estimates are nonzero. As such, I adjust for these covariates in some regressions.

5 Results

5.1 Colonial and Imperial Era: Oligarchy, Revolt, and Military Presence

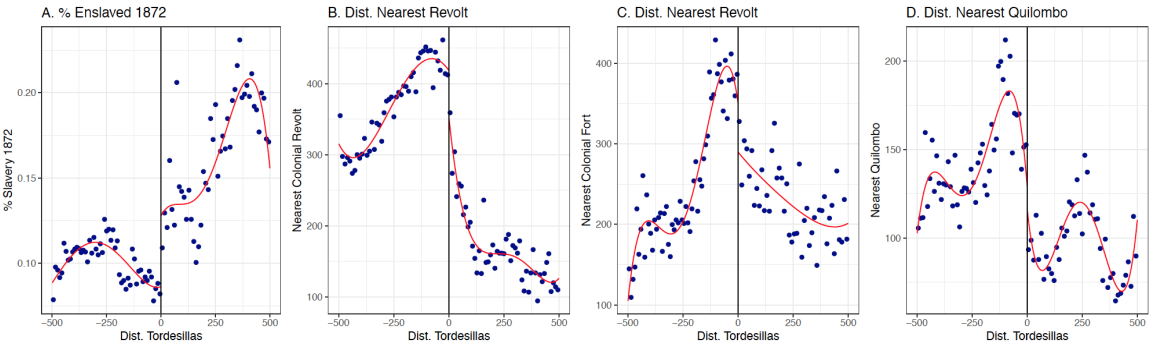
Descriptively, the relationship between a municipality's historical exposure to colonial institutions as a function of distance to the Tordesillas line is displayed in Figure 3.2. The share of the enslaved population increases at the boundary, while the distance between a municipality and a historical slave revolt, fort, and run away slave community decreases.

Figure 3.1: Balance Test



Note: Confidence intervals constructed from microregion clustered standard errors. Outcomes are standardized.

Figure 3.2: Colonial Conflict and Coercion



Note: Univariate forcing variable (nearest distance from centroid to the meridian).

Table 3.1 provides estimates from equation 3.1. All models include state fixed effects, odd columns model the forcing variable as a linear function and even columns use a quadratic in latitude-longitude space. Models 3-4 include baseline covariates (those tested for in Figure 3.1.) Overall, exposure to colonial extractive institutions increases by nearly a standard deviation for each respective outcome in the baseline, and remain robust to further adjustments.

Table 3.1: Colonial and Imperial Outcomes

	(1)	(2)	(3)	(4)
Panel A: Slavery 1872 (σ .08)				
1 East	$0.05^{\dagger}$	0.07^{*}	$0.05^{\dagger}$	0.06^{*}
	(0.03)	(0.03)	(0.03)	(0.03)
Panel B: Revolt (σ 195)				
1 East	-188.97^{***}	-141.30^{***}	-191.57^{***}	-138.10^{***}
	(33.79)	(27.35)	(30.78)	(26.65)
Panel C: Quilombo (σ 110)				
1 East	-111.26^{***}	-102.13^{***}	-111.19^{***}	-109.38^{***}
	(28.40)	(29.57)	(24.87)	(26.59)
Panel D: Fort (σ 183)				
1 East	-177.84^{***}	-146.12^{***}	-151.64^{***}	-97.04^{***}
	(39.59)	(25.42)	(18.77)	(19.75)
N.	1740	1740	1740	1740
State Fixed Effects	✓	✓	✓	✓
Linear	✓	-	✓	-
Covariates	-	-	✓	✓

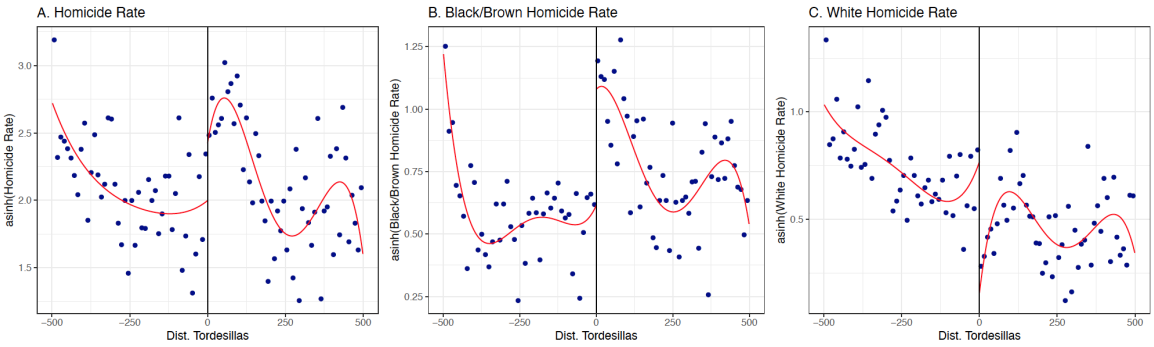
$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.1$

5.2 Persistence: Contemporary Homicides and Policing

Next, I study whether municipalities in the modern day experience differences in violent crime based on their colonial experience. Figure 3.3 shows a discrete change in homicide rates at the boundary, which is driven by homicides of black and brown Brazilians, with homicides of whites remaining continuous.

Table 3.2 provides statistical evidence of the pattern: homicide rates increase overall, but when breaking up homicides by ethno-racial subgroup, it is clear that the homicide rate only creases for Preto/Parda Brazilians, and that white homicide rates remain constant at the discontinuity. This evidence suggests the state provides comparatively worse security for ethno-racial groups who were historically disenfranchised.

Figure 3.3: Homicides by Ethno-Racial Group: 2010



Note: Univariate forcing variable (nearest distance from centroid to the meridian).

5.3 Law Enforcement

I expect increased insecurity for black and brown Brazilians to be paradoxically coupled with a more robust security sector at the municipal level: despite increased policing capacity, since the security sector historically services elite interests, the oversized policing force is not effective at protecting historically disenfranchised groups, and is even hostile towards them.

I provide evidence of this pattern in Table 3.3. Municipalities east of the line are more likely to have a Municipal Guard, spend more on security per capita, and are more likely to have instances of police killing civilians.

5.3 Mechanism

Next, I evaluate economic segregation as a persistence channel. My argument suggests that the security sector is able to provide biased protection in part because of spatial segregation within municipalities: since wealthy segments of society concentrated themselves in areas separate from less wealthy persons, the police are able to patrol some places and not others with the intention of enforcing a separation between the two groups. Figure 3.4 provides descriptive evidence of this pattern, as economic segregation and informal housing discretely increases at the discontinuity.

Table 3.2: Homicides

	(1)	(2)	(3)	(4)
Panel A: IHS Homicide Rate (σ 1.84)				
1 East	0.87^{*}	1.06^{**}	0.95^{**}	1.01^{**}
	(0.34)	(0.32)	(0.33)	(0.34)
Panel B: IHS Parda/Preto Homicide Rate (σ .99)				
1 East	0.47^{**}	0.50^{**}	0.47^{**}	0.45^{*}
	(0.17)	(0.18)	(0.17)	(0.20)
Panel C: IHS White Homicide Rate (σ .82)				
1 East	-0.19	-0.10	-0.27	-0.34
	(0.32)	(0.32)	(0.33)	(0.35)
N.	1740	1740	1740	1740
State Fixed Effects	✓	✓	✓	✓
Linear	✓	-	✓	-
Covariates	-	-	✓	✓

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.1$

Table 3.3: Law Enforcement Outcomes

	(1)	(2)	(3)	(4)
Panel A: **1** Municipal Guard (σ 0.38)				
1 East	0.37^{***}	0.39^{***}	0.38^{***}	0.39^{***}
	(0.08)	(0.09)	(0.08)	(0.10)
Panel B: IHS Security Spending PC (σ 1.32)				
1 East	1.58^{***}	1.61^{***}	1.58^{***}	1.62^{***}
	(0.33)	(0.35)	(0.36)	(0.39)
Panel C: Law Enforcement Killings (σ 0.26)				
1 East	0.09^{*}	0.10^{*}	0.06	0.05
	(0.04)	(0.04)	(0.04)	(0.04)
N.	1740	1740	1740	1740
State Fixed Effects	✓	✓	✓	✓
Linear	✓	-	✓	-
Covariates	-	-	✓	✓

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.1$

Figure 3.4: Economic Segregation and Informality Mechanism

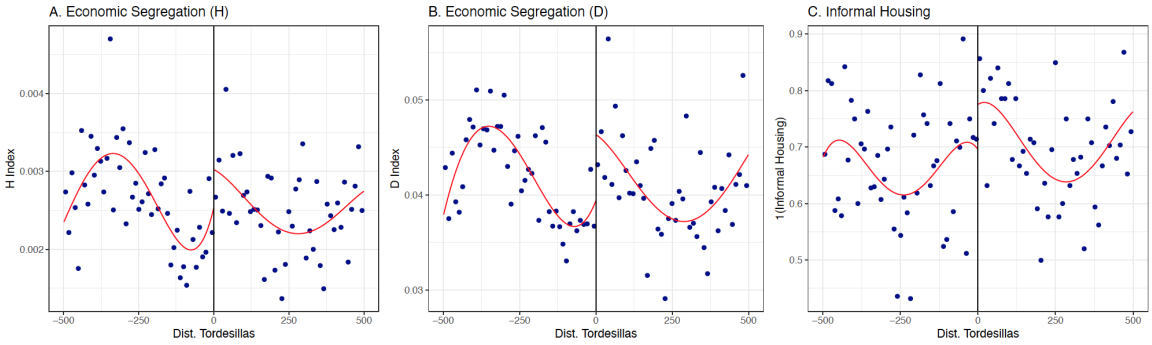


Table 3.4: Mechanism: Economic Segregation

	(1)	(2)	(3)	(4)
Panel A: D Index σ2.21				
east	1.18**	1.00*	1.24**	0.98*
	(0.37)	(0.38)	(0.39)	(0.42)
Panel B H Index σ0.27				
east	0.11**	0.10*	0.12**	0.11*
	(0.04)	(0.04)	(0.04)	(0.04)
N.	1738	1738	1738	1738
State Fixed Effects	✓	✓	✓	✓
Linear	✓	-	✓	-
Covariates	-	-	✓	✓

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.1$

Table 3.4 shows economic segregation increases when moving from the West to the East of the line. The D and H index provide two separate measures of segregation from on [158]. The consistency of the results based on different measures is suggestive that the findings are not an artifact of one particular measurement choice.

5.4 Robustness

In appendix C I show results remain consistent when using the [38] nonparametric RD with a univariate running variable (distance to the meridian) with different band-

widths. The finding provides evidence that the results are not sensitive to a leveraging a particular subset of the data in the neighborhood of the line.

7 Discussion

What explains spatial variance in the state's ability to protect its citizens from violent crime? I theorize contemporary interpersonal violence, security expenditure, and economic segregation are a function of historical legacies of colonization. The state formed coalitions with elite groups during initial phases of colonization and afterwards to sponsor development in exchange for asymmetric law enforcement protection of elite wealth. The legacy of this arrangement is economically segregated municipalities, where wealth is concentrated in place and protected by the state, and poverty is concentrated in another space, exposed to criminal violence.

I provide empirical evidence with a geographic regression discontinuity design in Brazil, showing colonial extraction and social conflict discreetly increased in areas exposed to the DC system, which determined social and economic structures in path dependent ways. The meridian shaped revolts, slavery, and escape from slavery historically, and influences municipal security in the modern day.

The threat to elite wealth emerging from inequalities created through extraction influences the material strength of police organizations, but does not translate into an even distribution of social peace due to asymmetric protection of elites. My theory highlights how colonial legacies have a nuanced impact on state capacity: in the dimension of resource allocation, coercive capacity increases, along the dimension of overall efficacy, capacity declines since resources are not allocated towards protecting and serving the poor.

My theory highlights how extractive institutions do not only affect the variance of income, they also impact spatial distributions of income. The concentration of the

wealthy in some areas along with the concentration of the poor among informal housing settlements explains how security is provided in much different ways across diverse communities: where informality flourishes, the state delegates control to criminal organizations, feeding into the inability of the police to rule without coercion and the overall level of interpersonal violence as a means of conflict mediation in the absence of a credible state [12, 129]. The finding suggests a novel mechanism explaining the legacy of colonization on maldevelopment, along with the importance of security as an outcome for continued exploration.

My findings extend beyond the Brazilian context. Extractive colonization, specifically coerced labor, was a feature in colonial economies across Latin America, notably in Colombia, El Salvador, Honduras, and, Jamaica, which have some the regions (and the world's) highest homicide rates. In Mexico, [57] argues the politicization of policing and violent crime rates were determined at critical junctures which shaped elite alliances with the state. While [57] centers on the Mexican revolution as a key juncture, I posit colonization was the crucial period that shaped development overtime. Was colonization a necessary condition for the elite-state coalition against the poor, or was it only a sufficient one? A related issue concerns the Latin American experience versus that of East and Southeast Asia. Whereas Latin American countries have startlingly high homicide rates, extractive colonization has not appeared to have had the same pattern on violent crime in Indonesia, or among the states that formerly composed French Indochina. Why did colonization lead to persistent crime in Latin America but not Southeast Asia? These questions, among many others, are important topics for future inquiry.

Appendix A

Appendix 1: Developmental Legacies of Draconian Dictatorship

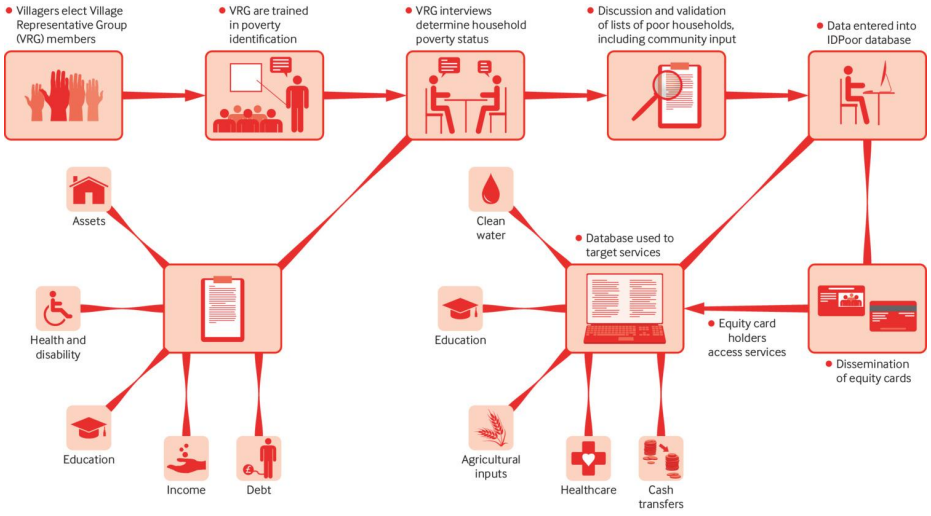
A.1 Data

A.1.1 IDPoor

IDPoor provides a comprehensive measure of poverty in Cambodia. By combining household assets, education levels, and using community consultations, several dimensions of poverty are captured and validated [114]. For example, a measure of consumption alone may not account for individuals who consume the same amount but have different investments or assets.

IDPoor data collection process pictured in Figure A.1 from [114]. Poor level 1 refers to the extreme poverty, with level 2 referring to poor. Those not in poverty are considered average by national standards or better off. By way of illustration, interviewers observe the building material of the roof of the household and score 8 it is made of soft materials (palm leaves or thatch), and 0 if its constructed with concrete. I measure the poverty rate of each village as the sum of households in category 1 or 2 divided by the total number of households to avoid capturing population differences.

Figure A.1: IDPoor Data Collection Process



Note: Flow chart downloaded from kaba2018idpoor.

During the process of data collection, the Ministry of Planning (MOP) monitors the implementation of the process. Once a list of poor households is drafted, the list is published and open to the community to enable validation of the list [114]. Indeed, "World Bank assessment determined that, on average, surveyed households rated the accuracy and implementation of the IDPoor process as high" [114].

A.1.2 Spatial Data

Mean Temperature: Average temperature per grid cell from 1970-2000 [76]

Seasonality Temperature: Standard deviation temperature between months times 100 [76]

Mean Precipitation: Average annual precipitation [76]

Coefficient of Variation: Average divided by standard deviation [76]

Elevation: Sea level elevation [74]

Ruggedness: Standard deviation of elevation from [174]

% Crop Land: Percentage of grid cell classified as crop land [174]

Distance to Roads: Distance to nearest road

Distance to Rivers: Distance to nearest river

Built up Area: Global Human Settlement [154]

Population 1975: Global Human Settlement [154]

A.1.3 Summary Statistics (Pretreatment Covariates)

Table A.1: Summary Statistics (Spatial Data)

Zone		West			Southwest	
Variable	N	Mean	SD	N	Mean	SD
Temp. Mean	1427	272.273	4.049	1442	271.055	3.709
Temp. Var	1427	957.353	68.899	1442	903.258	61.999
Prec. Mean	1427	1458.876	180.991	1442	1442.967	183.759
Prec. Var	1427	67.214	1.884	1442	65.807	2.266
Forest Land	1427	0.741	0.427	1442	0.648	0.463
Crop Land	1427	0.067	0.239	1442	0.087	0.266
Soil Fertility	1427	0.432	0.46	1442	0.369	0.46
Ruggedness	1427	41.919	75.132	1442	75.949	111.381
Elevation	1427	73.708	72.288	1442	89.318	64.537
River Density	1427	214.766	618.628	1442	170.083	561.766
Road Density	1427	231.913	633.369	1442	353.749	779.167
Population (1975)	1427	68.14	365.967	1442	105.304	457.868
Built-up Area (1975)	1427	0.31	3.798	1442	0.136	0.989

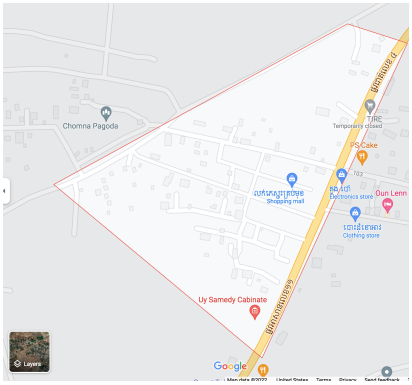
A.1.4 National Road 4: Google Maps Street View

Figure A.2: National Road 4 (Asian Highway 11) Street View in Kampong Speu

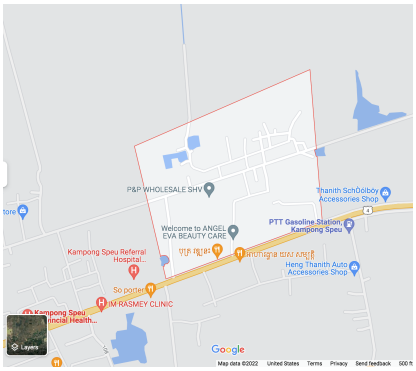
Note: Google Maps Street View of the highway (National Road 4, also known as Asian Highway 11) that separated the former West and Southwest zone. West zone shown on the left, Southwest zone shown on the right.

Figure A.3: Example Border Villages

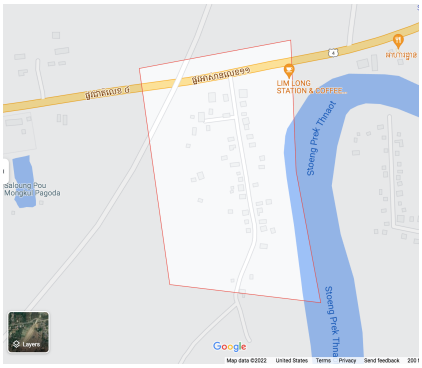
(a) Village A



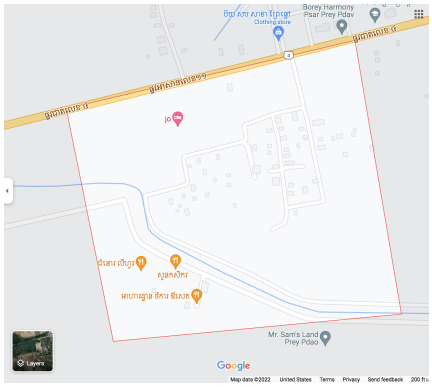
(b) Village B



(c) Village C



(d) Village D



Notes: Illustration of village polygons and their relationship to National Road 4 (the defunct border during the DK era). The maps show that in instances where village borders technically pass the road, villagers still tend to cluster towards the center, meaning classical measurement error of exposure to treatment is unlikely to significantly impact the main results.

A.1.5 Excess Mortality Plot

Exact mortality estimates from the Khmer Rouge are impossible to obtain due to the absence of administrative records. Despite the fact exact mortality counts cannot be computed, I obtain estimates of estimated mortality to construct a relative estimate of excess mortality overtime between Southwestern and Western Kampong Speu. I follow [62] and use the 2000 Demographic Health Survey questionnaire, which asks respondents about (1) their siblings demographic information and (2) the year their siblings died. Notably, the data I use will only record the sibling deaths of those who survived to the year 2000. As such, estimates cannot be taken as absolute levels. However, assuming women's survival probabilities were roughly equal between zones within the province - a reasonable assumption given the excessive targeting of men - one can make a conjecture about the relative intensive of violence overtime between places using the DHS data.

Since the data contain the year siblings died, one can plot deaths over time between spaces. However, death does not necessarily mean Khmer Rouge inflicted mortality, since some siblings may have been more likely to parish for demographic reasons irrespective of regime change. To account for this, I develop a model of mortality based on age, age squared, location of residence (urban/rural), and gender. The logistic regression is as follows.

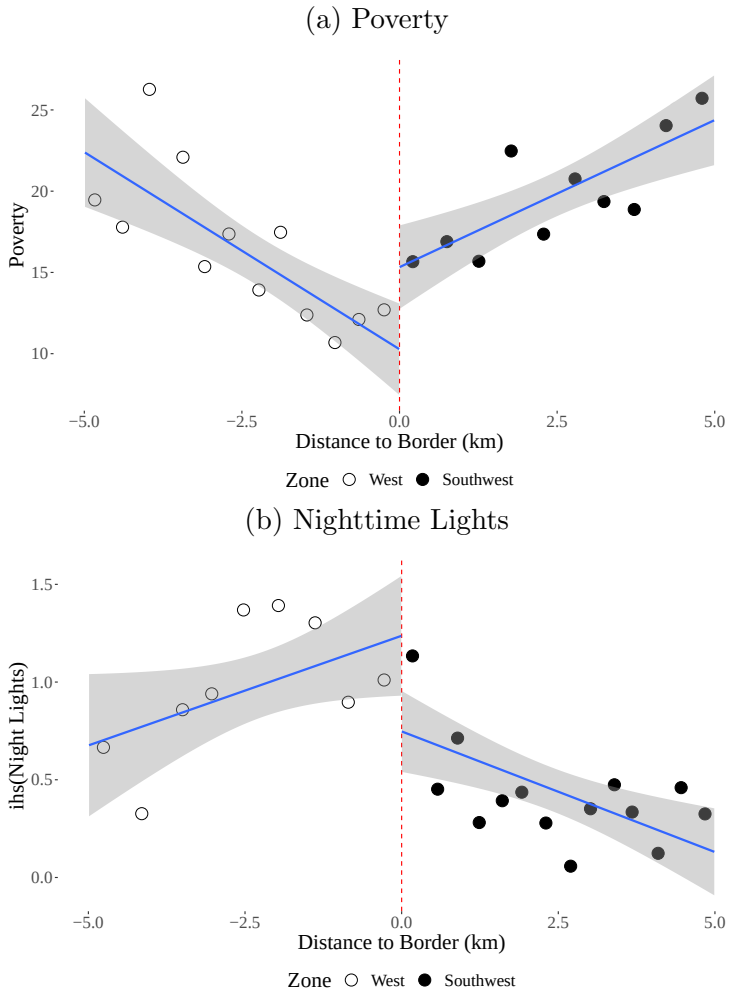
$$Pr(Y_{it} = Dead|X_{it}) = \frac{\exp(\alpha_0 + \alpha_1 Gender_i + \alpha_2 Age_{it} + \alpha_3 Age_{it}^2 + \alpha_4 1Urban_i)}{1 + \exp(\alpha_0 + \alpha_1 Gender_i + \alpha_2 Age_{it} + \alpha_3 Age_{it}^2 + \alpha_4 1Urban_i)}$$

I estimate the model on all siblings from the 2000 DHS survey round who were born at least before 1960 *outside* of Kampong Speu. I estimate the model outside of the province of interest to avoid overfitting. Then, I use the estimated coefficients to predict individual mortality overtime in Kampong Speu using siblings values of the predictor variables. The difference between actual mortalities in a zone-year

from predicted mortality in a zone-year represents excess mortality in the zone - that is, deaths which occurred which were not predicted by an individual's demographic traits.

A.1.6 One-Dimensional RD Plots: Baseline Development

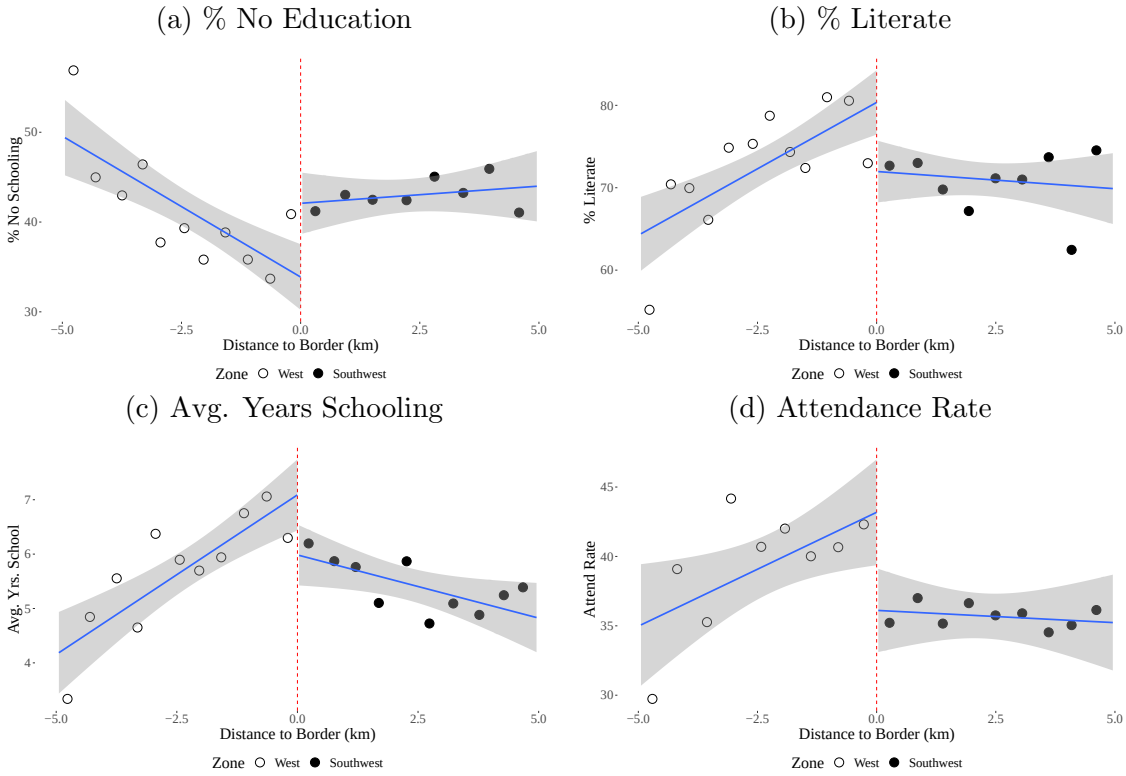
Figure A.4: One-Dimensional RD Plots: Baseline Development



Notes: RD plots illustrating local linear regressions within a 5 kilometer bandwidth. Vertical dashed line marks 0; observations to the right are in the Southwest zone and observations to the left of the line are in the West zone. Dots represent binned averages.

A.1.7 One-Dimensional RD Plots: Human Capital

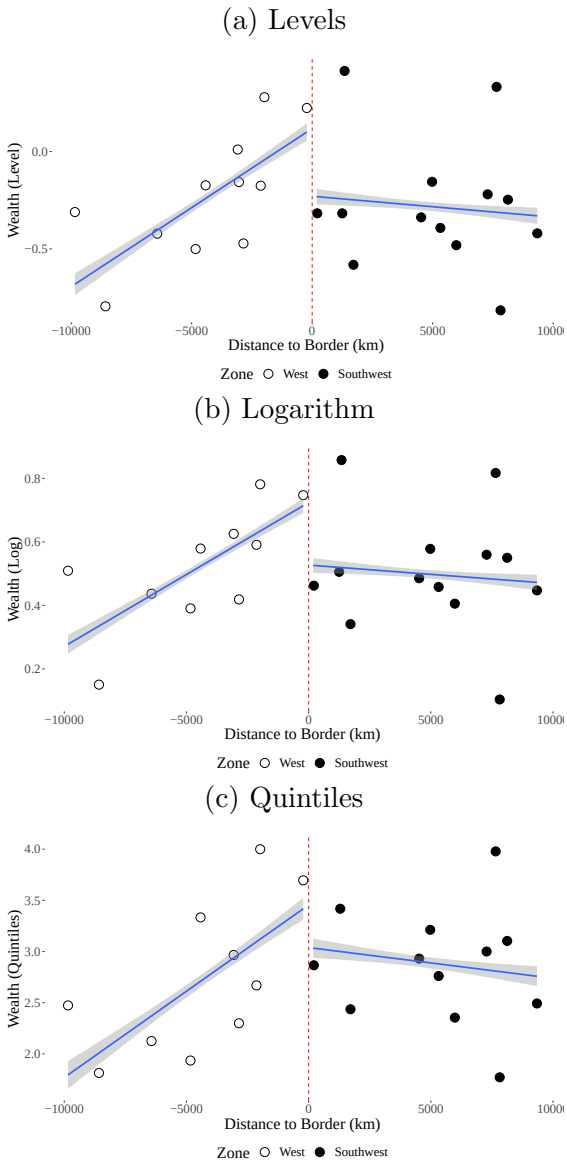
Figure A.5: Human Capital: 1998 Census



Notes: RD plots illustrating local linear regressions within a 5 kilometer bandwidth. Vertical dashed line marks 0; observations to the right are in the Southwest zone and observations to the left of the line are in the West zone. Dots represent binned averages.

A.1.8 One-Dimensional RD Plots: Wealth

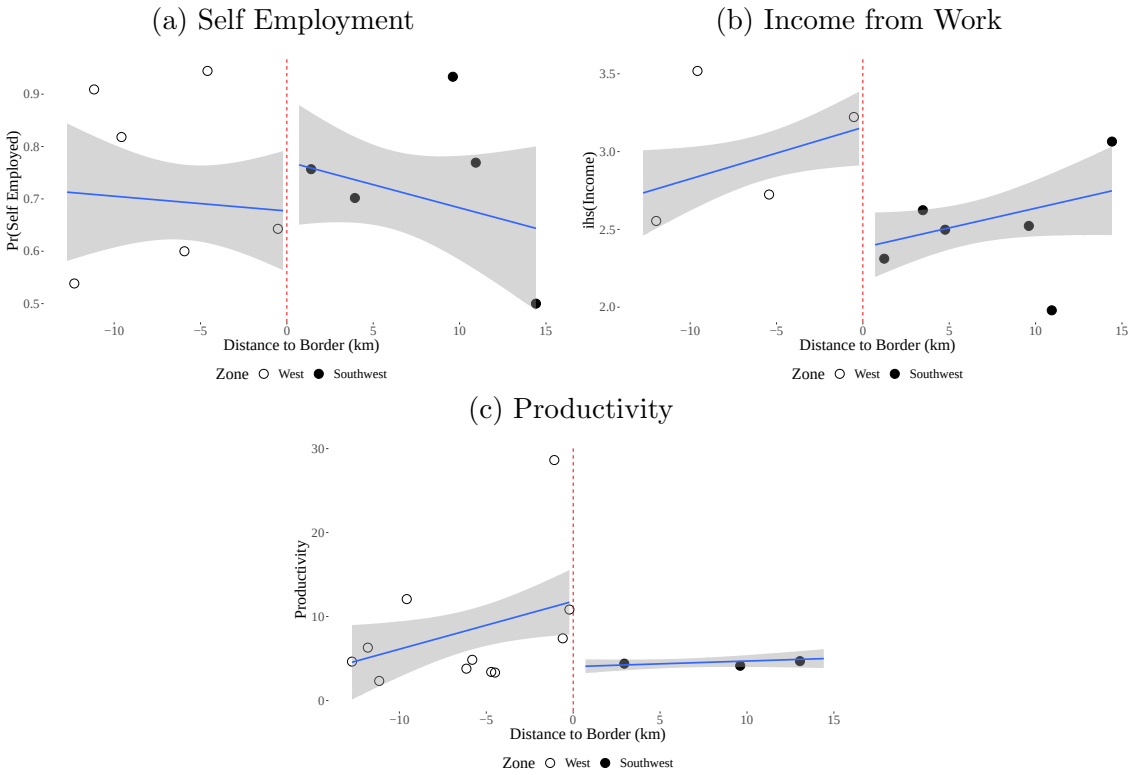
Figure A.6: One-Dimensional RD Plots: Rural DHS Wealth



Notes: RD plots illustrating local linear regressions within a 10 kilometer bandwidth. Vertical dashed line marks 0; observations to the right are in the Southwest zone and observations to the left of the line are in the West zone. Dots represent binned averages.

A.1.9 One-Dimensional RD Plots: Labor Markets

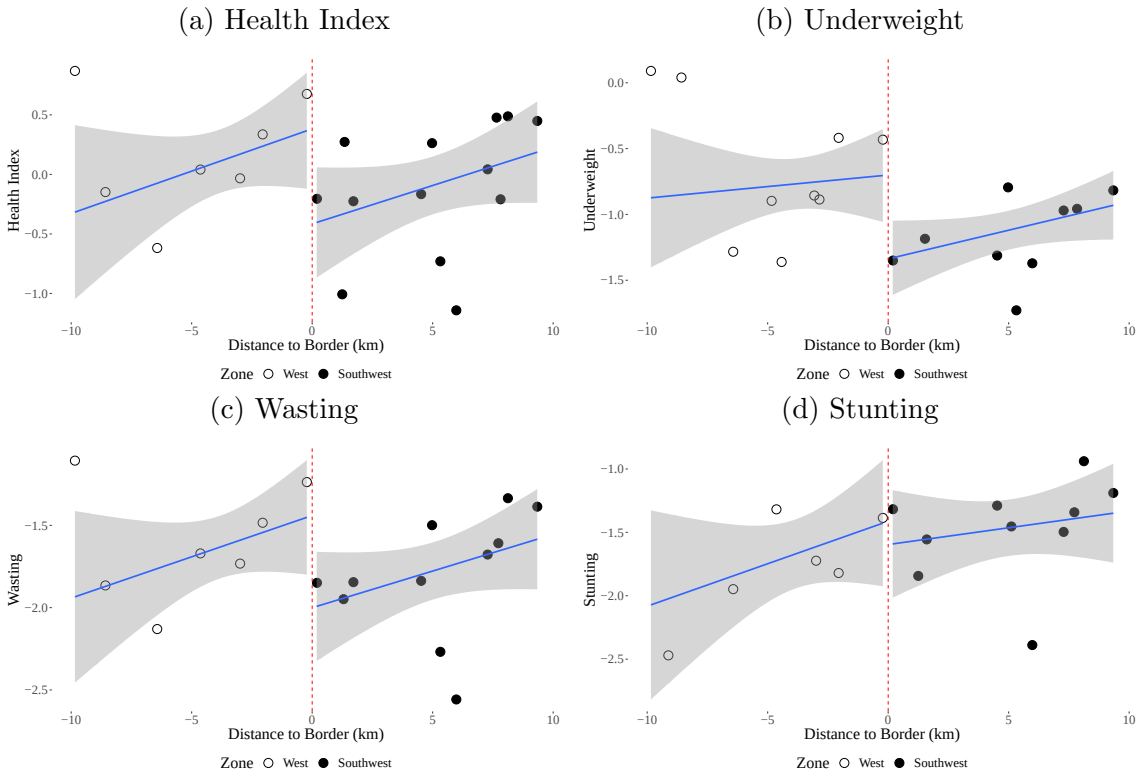
Figure A.7: Labor Force Survey (LFS) Labor Market Outcomes



Notes: RD plots illustrating local linear regressions within a 10 kilometer bandwidth. Vertical dashed line marks 0; observations to the right are in the Southwest zone and observations to the left of the line are in the West zone. Dots represent binned averages.

A.1.10 One-Dimensional RD Plots: Health

Figure A.8: DHS Rural Child Health

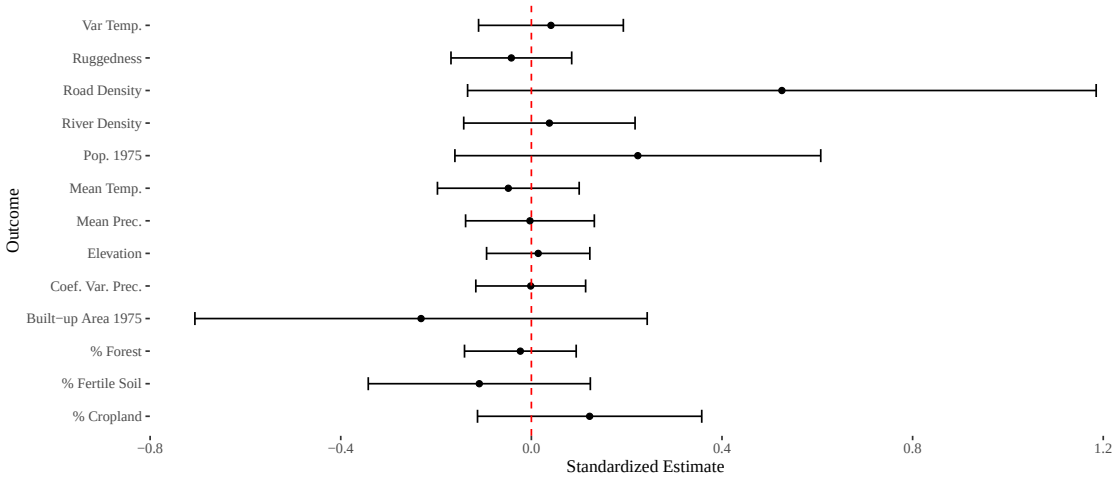


Notes: RD plots illustrating local linear regressions within a 10 kilometer bandwidth. Vertical dashed line marks 0; observations to the right are in the Southwest zone and observations to the left of the line are in the West zone. Dots represent binned averages.

A.2 Identification Checks

A.2.1 Balance (MSE Optimal Bandwidth)

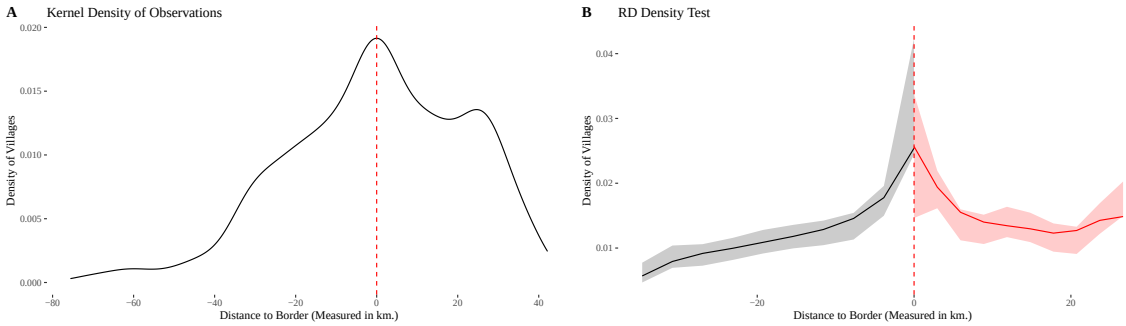
Figure A.9: Balance Tests



Note: Outcomes standardized reported in horizontal axis, vertical axis refers to each respective outcome. Spatial heteroskedasticity and autocorrelation consistent standard errors used to construct equivalence confidence interval (ECI). Equivalence range selected using the sensitivity approach $\epsilon \pm .36\sigma$. Estimates using nonparametric RD within MSE optimal bandwidth.

A.2.2 Density Test

Figure A.10: Density Test



Note: Kernel density of observations by running variable in Panel A. Test for discontinuity in density in Panel B showing smoothness of observations at the cutpoint.

A.3 Robustness

A.3.1 DHS Wealth Outcome

Table A.2: DHS Wealth

	(1) Levels	(2) Logs	(3) Categories
1 SW	-0.80***	-0.42***	-1.23***
	(0.16)	(0.92)	(0.36)
Bandwidth	8.93	9.19	10.32
Effective N	3155	3155	3567

Outcome is the DHS wealth index constructed from the first principal component of household assets. Unit of analysis is the rural individual. Adjusting covariates include gender, age, age squared, and survey wave fixed effects. Column (1) reports the outcome measured in levels, Column (2) reports the natural log of the index, and Column (3) shows the outcome according to categories (quintiles).

A.3.2 Specification of the Assignment Variable $f(.)$

Assignment to treatment is a function of two covariates: latitude and longitude. My approach of measuring treatment assignment through distance to the border collapses these two dimensions into a single measure, called the "pooling" RD estimator. The pooling RD will capture the LATE (γ from equation (1)) under a constant treatment effects assumption, that is, the treatment effect is the same for all villages (or persons) who have the same value of the distance to the border variable [43].

There are a few reasons this assumption is reasonable in this context. The study area I concentrate on is highly dense spatially. One may be rightfully concerned about comparing two units which are one kilometer from some boundary, but reside in very different places along an extremely long boundary: for instance, comparing an American town one kilometer from Mexico's border in Texas to a town in Baja California in Mexico would likely be invalid, even if the town in Mexico was also only one kilometer from the border, since the two places are extremely far from one

another despite each being the same absolute distance to the cutoff. However, my study area is fairly compact, meaning my estimates are not making such extreme comparisons. As such, the constant treatment effects assumption is more reasonable than in may other geographic RD settings, where the space being examined is more vast.

Second, my approach includes border segment fixed effects in addition to the univariate forcing variable. Border segment fixed effects compare villages within the same neighborhood under treatment and control. By doing so, my estimator removes heterogeneity across the border [63]. The constant effects assumption becomes more reasonable conditional on fixed effects. Keeping with the example of a town in Baja California, Mexico, and Texas, each town would be binned into a different boundary segment, meaning they would not be compared against one another.

However, one may be concerned that the univariate forcing variable obscures heterogeneity across space generated by trends across latitude and longitude. This may lead to a rejection of the null hypothesis because a nonlinear spatial trend is mistaken as a discontinuity in the univariate regression function. [63] models assignment to the *Mita* boundary using a polynomial in latitude-longitude space in some specifications, but also cautions that the multidimensional function, while being highly flexible, may not always be suitable for smaller datasets, where overfitting may occur, in Dell's words "a more flexible specification will not necessarily yield a more reliable estimate" [63, Section 4.3].

The high density of observations not only makes the constant treatment effects assumption more reasonable, it also makes the concern of overfitting with more complex polynomials more first-order. My primary strategy for accounting for non-linear spatial trends is to include a squared term for distance to the border while also including border segment fixed effects.

Nonetheless, it is important to explore other approaches to modeling the forcing

variable. I do so in two ways. First, I estimate a polynomial model including a linear and quadratic term for latitude and longitude along with their interactions. I do so using *orthogonal* polynomials to account for multicollinearity which may otherwise yield inconsistent estimates. Second, rather than "dummying out" the heterogeneity across the border, I estimate a more flexible GRD within boundary points, as recommended by [116].

Results in Table A.3 show the baseline outcomes (poverty and luminosity) where the forcing variable is a linear or quadratic function of latitude-longitude coordinates of villages. The results are strikingly similar to the findings in the main text.

Table A.3: Baseline Results: Multidimensional Forcing Variable

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	%Poverty				IHS Luminosity			
1 SW	5.45^{**}	5.75^{**}	6.37^{***}	5.16^{***}	-0.72^{***}	-0.8^{***}	-0.7^{***}	-0.57^{***}
	(1.8)	(1.85)	(1.43)	(1.45)	(0.12)	(0.13)	(0.11)	(0.1)
Effective N	334	324	502	484	422	389	452	568
Bandwidth	6.34	5.98	10.99	10.62	8.9	7.95	9.64	12.79
μ Control	20.95	20.95	20.95	20.95	0.43	0.43	0.43	0.43
σ DV	10.55	10.55	10.55	10.55	0.63	0.63	0.63	0.63

RD results using a polynomial in latitude-longitude space as the forcing variable. Linear forcing variable models latitude and longitude, denoted as x and y, as: $x + y + xy$. Squared model uses: $x + y + xy + x^2 + y^2 + xy^2 + yx^2 + y^2x^2$.

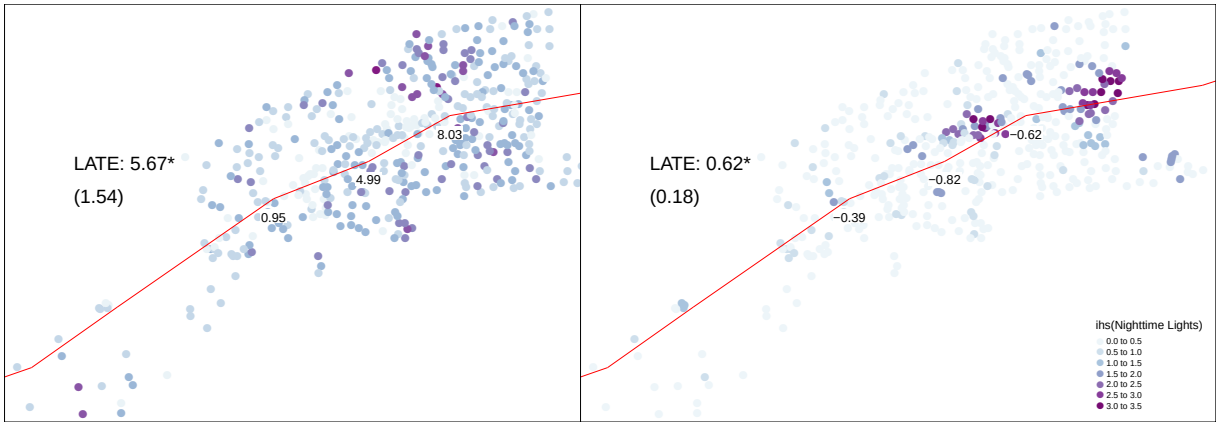
Nonetheless, one may be concerned that my focus on average effects conceals heterogeneity of treatment effects across space, which could reveal contradictory patterns. For instance, villages within one segment may have an effect with a negative sign, while others have a positive sign, and while the average effect shows that the Southwest zone has more depressed development, vastly different treatment effects within evaluation bins could signal that the story is more complicated.

I estimate the RD across border points as recommended by [116]: I estimate three different RD's within border points, and average them together by weighting each

estimate based on the share it contributes to the total number of observations, obtaining the standard error with resampling. I report the treatment effect curve along the border in addition to the average effects. Figure A.11 shows treatment effects are consistently signed and relatively speaking similarly sized. The treatment effects grouped together are similar to the baseline estimates, suggesting that the parametric approach used for the main analysis is not masking geographic heterogeneity in a way that leads to a spurious rejection of the null hypothesis.

Figure A.11: Treatment Effect Curve

(a) Poverty (b) Luminosity



Notes: RD estimates along border points (reported at each particular point) and aggregated LATE (reported in upper left corner). Standard error computed via the bootstrap. Each dot represents a village with associated shading corresponding to level of poverty or luminosity respectively.

A.3.3 Power Analysis

Table A.4: Power Analysis:

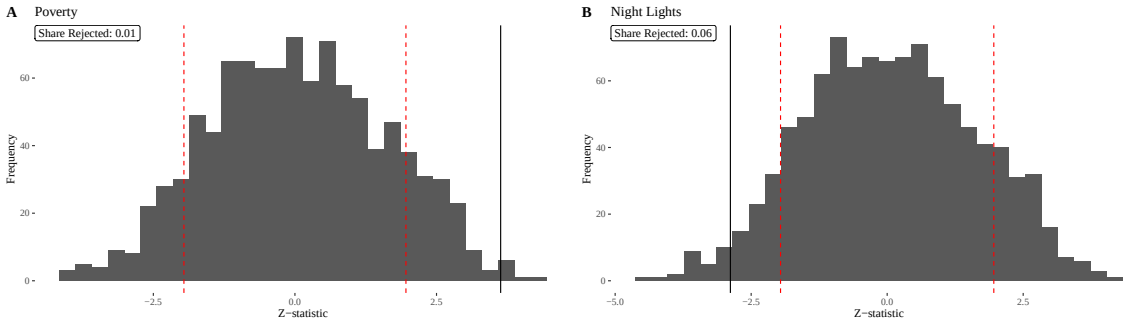
		Power Against			
Kernel	H0: $\tau = 0$	$0.2^*\tau$	$0.5^*\tau$	$.8\ ^*\tau$	$\tau = \widehat{\tau}$
		Panel A: Poverty			
Uniform:	.05	.089	0.304	0 .638	0.824
Triangular:	.05	0.129	0.532	0.904	0.983
		Panel B: Luminosity			
Uniform:	.05	0.145	0.609	0 .947	0.994
Triangular:	.05	0.117	0.47	0 .854	0.965

Note: Power analysis of nonparametric robust bias-corrected regression discontinuity design for primary outcomes of interest (poverty and luminosity). Each column shows the power of the test against various null hypotheses based on the hypothesized effect size. The column to the furthest to the right reports the power against assuming the effect size detected in the study is the true value of τ, moving to the left the size of τ is decreasing. Power analysis includes border segment fixed effects.

A.3.4 Simulated Spatial Noise

I simulate spatial noise following the correlation structure of each outcome and assess how often the true null hypothesis of no effect is rejected at the same significance level as is observed when estimating the same regression on the true data. I use the CCT approach for the simulations, as their method corrects for bias when constructing standard errors and confidence intervals, and since the parametric estimates already adjust for spatial autocorrelation whereas the CCT confidence intervals do not. The top right corner shows the share of regressions which reject the null at a level as extreme as the empirical z-statistic. The vertical dashed red line is the z-statistic from the CCT estimates.

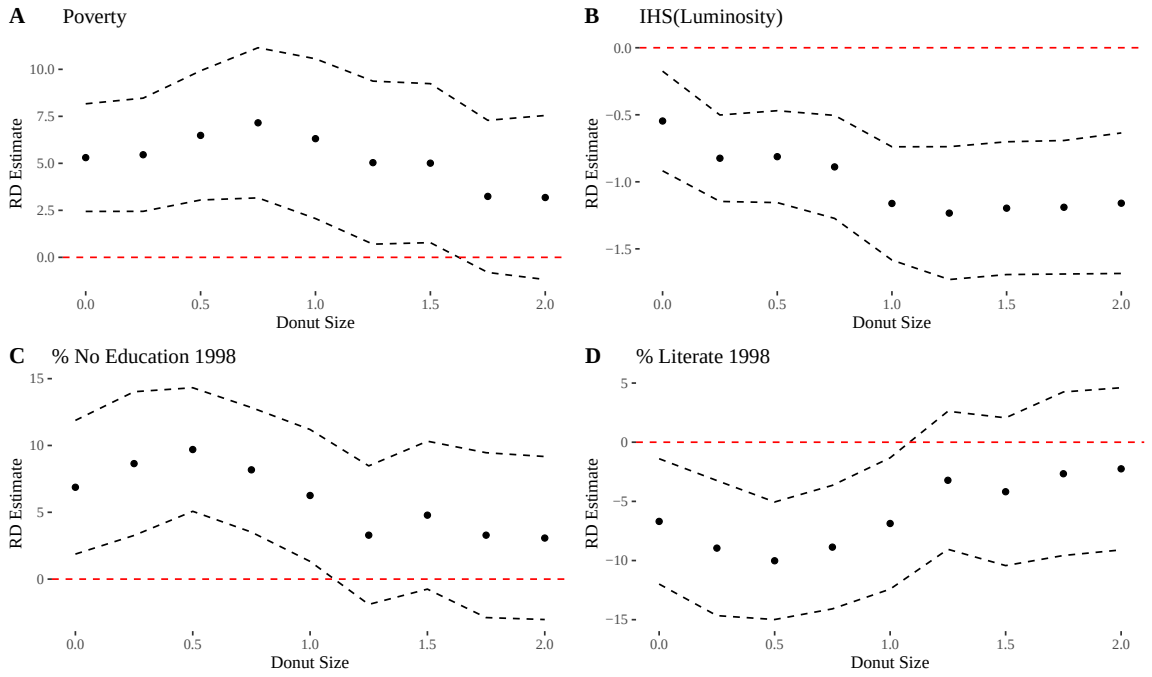
Figure A.12: Noise Simulations



Note: Histograms of z-statistics from CCT robust standard errors. Outcome is simulated spatial noise for each respective outcome. Vertical red line is the z-statistic using the true data. Upper right corner is the proportion of z-statistics from simulations that are more extreme than the estimates from the true data.

A.3.5 Excluding Observations Near Threshold (Donut RD)

Figure A.13: Excluding Observations Near Threshold



Note: Estimation using CCT nonparametric approach and confidence intervals. Size of donut-hole expands at .25 kilometer increments starting with .5 kilometers. Each estimate drops additional data.

A.3.6 Sensitivity Analysis

Table A.5: Sensitivity Analysis

Treatment:	Est.	S.E.	t-value	$R^2_{Y\sim D\|\mathbf{X}}$	$RV_{q=1}$	$RV_{q=1,\alpha=0.05}$
			Outcome: *Poverty*			
1 SW:	3.486	1.783	1.956	1.2%	10.2%	0%
df = 328			*Bound (4x Built Area 1975)*: $R^2_{Y\sim Z\|\mathbf{X},D}$ = 9.4%, $R^2_{D\sim Z\|\mathbf{X}}$ = 15.2%			
			Outcome: *Luminosity*			
1 SW:	-0.574	0.14	-4.113	4.2%	18.9%	10.4%
df = 383			*Bound (4x Built Area 1975)*: $R^2_{Y\sim Z\|\mathbf{X},D}$ = 28%, $R^2_{D\sim Z\|\mathbf{X}}$ = 15.4%			
			Outcome: *Literacy Rate*			
1 SW	-7.262	2.648	-2.743	2.4%	14.4%	4.3%
df = 311			*Bound (4x Built Area 1975)*: $R^2_{Y\sim Z\|\mathbf{X},D}$ = 0%, $R^2_{D\sim Z\|\mathbf{X}}$ = 4.2%			
			Outcome: *No Educ.*			
1 SW	7.42	2.5	2.968	2.8%	15.7%	5.6%
df = 303			*Bound (4x Built Area 1975)*: $R^2_{Y\sim Z\|\mathbf{X},D}$ = 1.2%, $R^2_{D\sim Z\|\mathbf{X}}$ = 4.7%			

Sensitivity analysis results adjusting for road density and density of built up areas in 2 x 2 kilometer grids surrounding village centers. "Est." column is the estimate, "S.E." is the standard error, "t-value" is the t-statistic. $R^2_{Y\sim D|\mathbf{X}}$ reports how much residual variation in treatment exposure unobserved confounder would need to explain in order to erase the effect of treatment conditional on the unobserved confounder explaining all of the left out variance in the outcome of interest. $RV_{q=1}$ is the robustness value for bringing the estimate of Southwest to zero. Unobserved confounders that explain less than the robustness value's worth of both exposure to the Southwest zone and the outcome of interest are not sufficiently strong to explain away the observed effect.

A.3.7 Infrastructure Effect: National Highway 3 Placebo

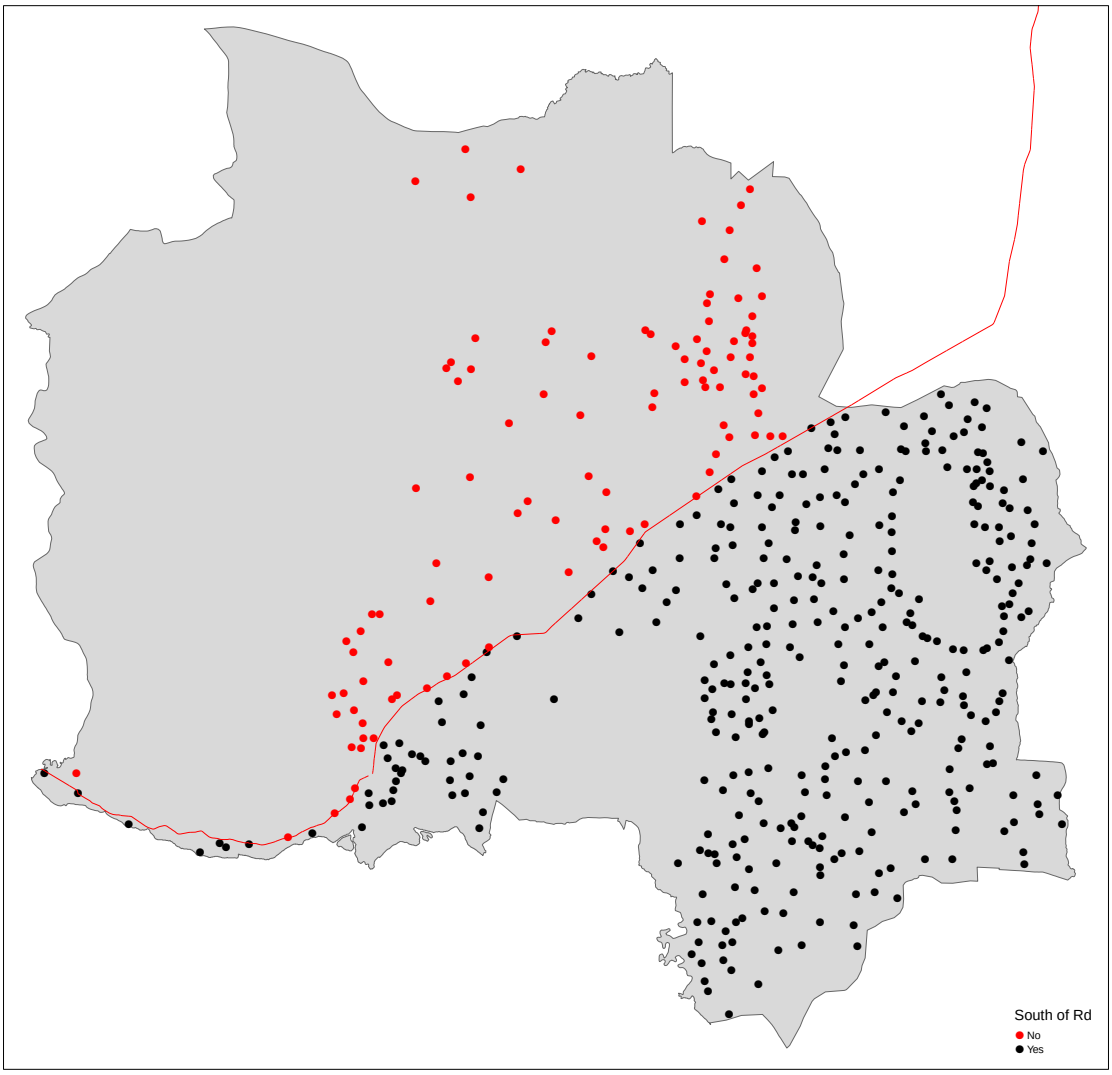
One may be concerned the border captures the effect of National Highway 4 rather than difference in DK administration, since highways may separate economically distinct areas. If this was the case, one would expect to find a discrete change in development outcomes when crossing a nearby road, National Highway 3, in Kampot province. Kampot was entirely within the Southwest, therefore the road does not separate villages with a different administrative experience, but it does cut across villages in a similar way as NH 4.

Table A.6: National Road 3 Placebo: Kampot Province

Outcome	(1) Night Lights	(2) Poverty	(3) No Educ.	(4) Literacy
1 South	-0.14	-1.15	-2.61	1.88
	(0.13)	(1.48)	(2.42)	(2.69)
Effective N	162	112	134	146
Bandwidth	7831.48	5009.04	6427	6974.87
μ Control	0.28	17.11	53.59	56.79
σ DV	0.5	7.38	12.57	14.2

1 South is a binary indicator for a village being South of National Road 3 within Kampot province (See Figure A.14 for reference). All villages within Kampot province, which was entirely in the Southwest Zone during the DK and civil war period (1970-1979).

Figure A.14: Road Placebo: National Highway 3

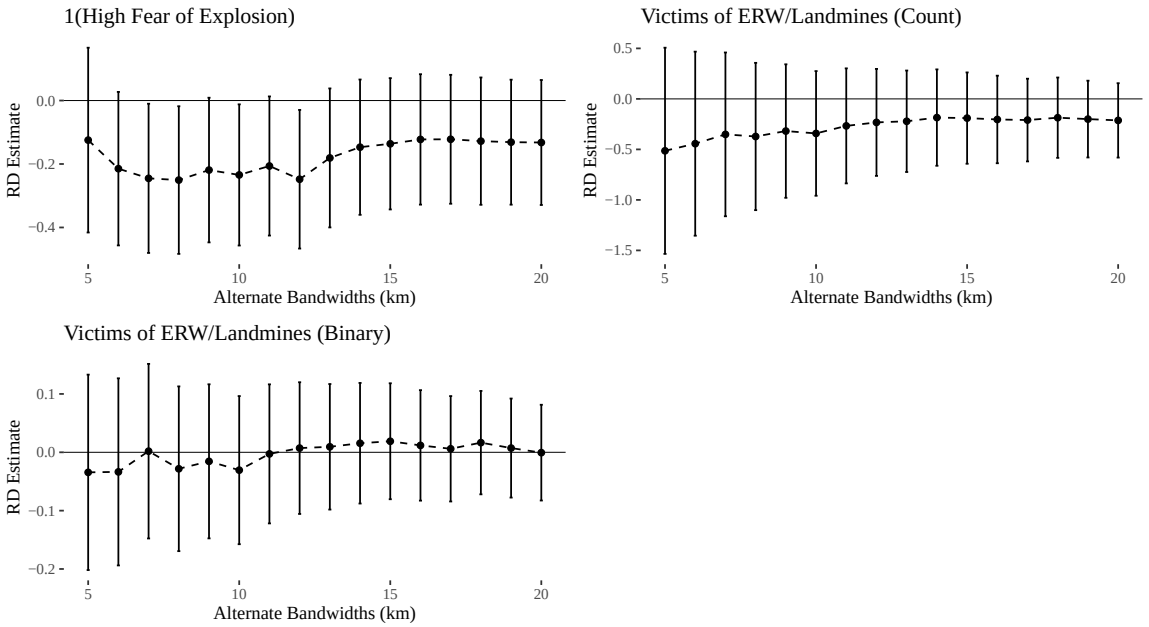


Note: Map showing the province and villages used for the National Highway 3 placebo test.

A.3.8 Falsification (ERW/Landmine Exposure)

I use data from the Cambodian Mine Action and Victim Assistance Authority (CMAA) to use three measures of exposure to landmines and explosive remnants of war (ERW). Panel A uses survey data that assess the risk of civilian exposure to ERW or landmines, scoring 1 if the village is considered in the "high risk" category and 0 otherwise. Panels B and C use data on the count of civilian victims of explosions, which measures actual incidences of explosions rather than latent risks. Consistent with the fact that villages were in the same combat zone during the civil war, I find no clear evidence that villages in either zone face different risks of explosions, providing evidence against the legacy that I find being driven by lingering explosive material.

Figure A.15: Explosive Remnants of War (ERW) and Landmine Exposure: Post 2000

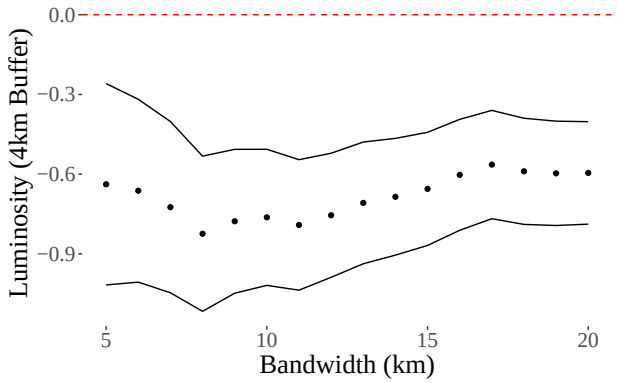


Note: 95% CCT robust confidence intervals shaded around estimates, uniform kernel, alternative bandwidths

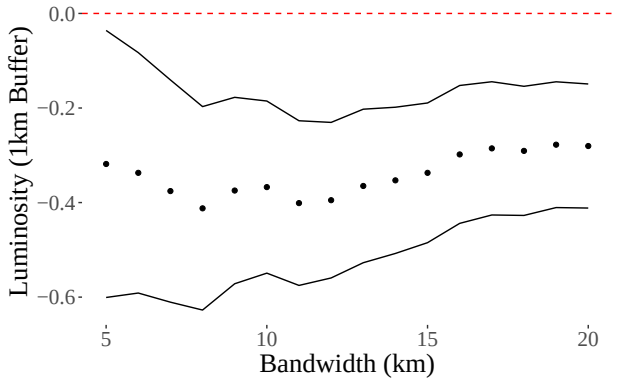
A.3.9 Night lights (Other Aggregation Methods)

Figure A.16: Luminosity: Other Aggregation Grids

(a) Nighttime Lights (4km)



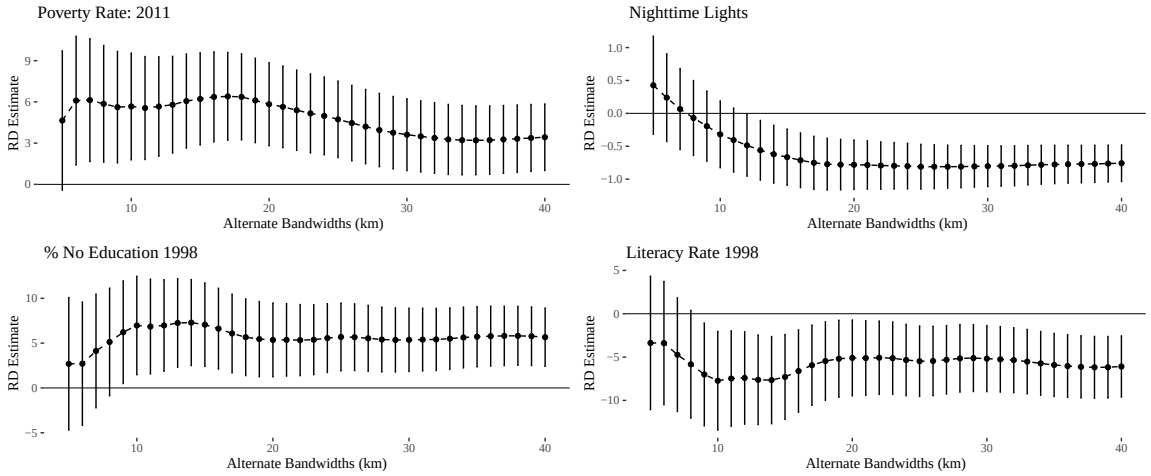
(b) Nighttime Lights (1km)



Notes: Semi-parametric RD estimates at alternative bandwidths. Panel A shows the results where a 4 km buffer is created around villages to compute luminosity. Panel B shows results where a narrower 1 km buffer is used to compute luminosity. SHAC standard errors used to construct 95% confidence bands.

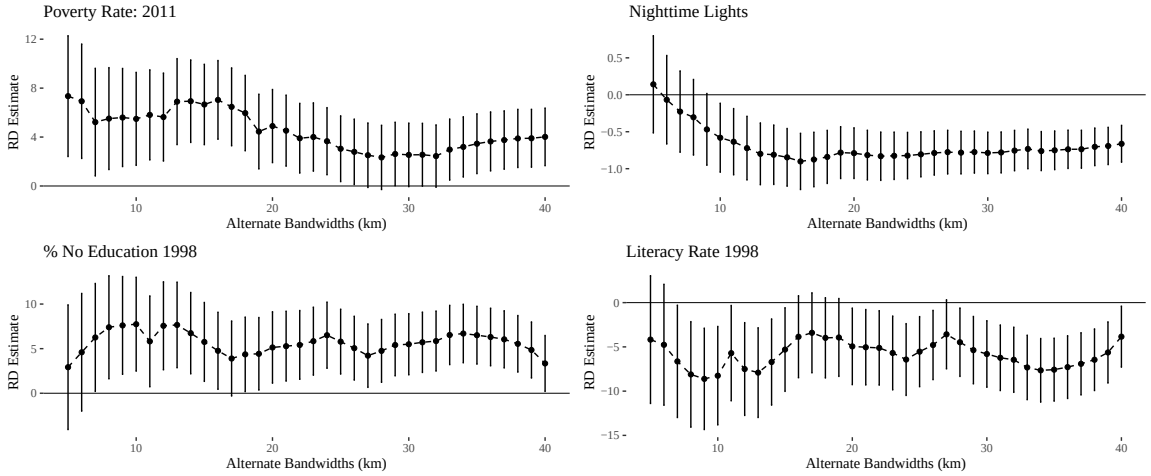
A.3.10 Alternative Bandwidths

Figure A.17: Nonparametric RD: Alternative Bandwidths, Triangular Kernel



Note: Estimation using CCT nonparametric approach and confidence intervals at alternative bandwidths with triangular kernel.

Figure A.18: Nonparametric RD: Alternative Bandwidths, Uniform Kernel



Note: Estimation using CCT nonparametric approach and confidence intervals at alternative bandwidths with uniform kernel.

A.4 Alternative Explanations

A.4.1 Covariate Adjustment

Table A.7: Effect of Southwest on Village Development (Covariate Adjusted)

Outcome	(1)	(2)	(3)	(4)
	%Poverty		IHS Luminosity	
SW	$3.21^{\dagger}$	4.36^{*}	-0.53^{***}	-0.53^{***}
	(1.71)	(2.03)	(0.12)	(0.13)
Effective N	340	505	439	618
Bandwidth	6.58	11.12	9.31	14.37
μ Control	20.95	20.95	0.43	0.43
σ DV	10.55	10.55	0.63	0.63
Segment FE	✓	✓	✓	✓
Dist. Capital Covariate	✓	✓	✓	✓
Pre-DK covariates	✓	✓	✓	✓
Linear	✓	-	✓	-

Note: % Poverty is the count of level 1 and level 2 poverty divided by the number of households per village as measured by IDPoor in 2011. Nighttime lights are the inverse hyperbolic sine of the sum of estimated GDP from luminosity in a 2x2 kilometer grid cell surrounding the village centroid. Estimates include the following pre-DK covariates: distance to the provincial capital, the sum of built up area around the grid cell surrounding the village in 1975, road density in the grid cell surrounding the village.

A.4.2 Public Goods Mechanism

Table A.8: Public Goods Access

Outcome: Distance to:	Hospital (1)	School (2)	Commune Center (3)
1 SW	-0.0788	-0.0039	0.3503
	(0.8141)	(0.1538)	(0.6218)
N	398	457	397
BW	8.2	9.9	8.5

Notes: See Table 2. Outcomes are village distance to nearest public good (kilometers)

A.4.3 Estimates Within Non-Migrants

Migration immediately after - and during - the Khmer Rouge is an important factor to consider. The Khmer Rouge forced urbanities to move across the country. Within rural areas, villagers were forced into mobile work teams, which traveled around sectors (ie, within the administrative area directly below the zone level.) Since I investigate the legacy of the regime among villages within two sectors, the rural areas I focus may have moved during the regime, but would have moved within the geographic area of their treatment assignment (within the West or Southwest zones).

That being said, some degree of internal migration could explain the result giving movement that occurred due to displacement or flight. The choice to move occurs post-treatment for many residents: after the Khmer Rouge administered repression and strictly controlled internal migration, some citizens may have sorted after the regime in ways correlated with their treatment status.

Since movement occurs post-treatment, to estimate the impact of the Southwest Zone among residents who never moved, I adopt a principal stratification framework. Consider migration $\mathcal{M} = \{0, 1\}$ where 1 means an individual has migrated (ie, lives in a place that was not their original place of birth) and 0 means the individual is living

in the place they have always lived and Z = $\{0, 1\}$ where 1 denotes being exposed to the (former) Southwest Zone, which I refer to as "treatment" for brevity. Finally, a respondent's residence $R_i = \{0, 1\}$ is 1 when the person lives in the former Southwest Zone and 0 otherwise.

As in [140], we can define 4 principal strata based on these criteria using potential outcomes notation. Let $\mathcal{M}(0)$ denote migration status under control ($Z = 0$) and $\mathcal{M}(1)$ denote migration status when assigned to the Southwest Zone ($Z = 1$).

Table A.9: Principal Strata

Types	Exposure and (Potential) Migration
Always Moves (AM)	$\mathcal{M}(0) = \mathcal{M}(1) = 1$
Never Moves (NM)	$\mathcal{M}(0) = \mathcal{M}(1) = 0$
Moves if Treated (MT)	$\mathcal{M}(0) = 0, \mathcal{M}(1) = 1$
Stay if Treated (ST)	$\mathcal{M}(0) = 1, \mathcal{M}(1) = 0$

The four strata ($S = \{AM, NM, MT, ST\}$) and labels for their types are presented in Table A.9. Qualitatively, these types are as follows:

- Always moves (AM): migrate regardless of treatment status; individuals who would have been mobile irrespective of the intensive margin of repression they were exposed to. These individuals may have preferences, income, or social ties that would have pushed them to move no matter what the Khmer Rouge did.

- Never Moves (NM): never migrate, whether repressed more or less. These individuals may have strong social ties to their place of residence, or may face very high transaction costs for moving (i.e., the entire immediate family would also need to move, lack of a car makes transportation of goods difficult, fixed assets that could be sold may be insufficient to cover for expenses, barriers to entry to new labor markets, ect).

- Moves if Treated (MT): People that migrate, but were only pushed because of

Khmer Rouge repression. These persons may have gone from the Southwest zone to the nearby West zone after repression to place themselves in a more ideal labor market.

- Stays if Treated (ST): these are persons who had plans to move, or would have migrated, but did not because the Khmer Rouge shocked them into place. This could occur if income shocks were so large that assets that were going to go towards migrating had to allocated towards other expenses. Given low rural-rural migration rates and strong traditional ties to place, this subgroup is likely to be fairly small in practice.

From here, we can break down different movement types into several types displayed in Table A.10.

Table A.10: Individual Respondents Stratified by Exposure Status (Z) and Current Residency (R)

		Zone Historically	
		0	1
Residency	0	MT & NM	MT & AM
Today	1	ST & AM	ST & NM

Assumption 1: No "Stays if Treated" (Monotonicity) $\mathcal{M}_i(1) - \mathcal{M}_i(0) \geq 0 \ \forall \ i = \{1, ..., N\}$**.**

Assumption 1 says there is no type that only wants to live in the Southwest if they are exposed to mass repression. The assumption is substantively motivated and likely to be satisfied in this context. Those that wanted to leave Cambodia often did, as evidenced by the mass migration outflows from the country in the lead up to the Khmer Rouge, and the devastation wrought by violence is not likely to be a pull factor persuading otherwise mobile civilians to stay after treatment. Likewise, it makes little sense to think of a person who was satisfied staying in their home village, and was only promoted to move into the Southwest zone because they were assigned

less repression. As such, I assume individuals will either always move regardless of repression due to factors uncorrelated with treatment, will never move, again for reasons unaffected by treatment status, or will move if repressed.

Selective migration could explain the result of the following migration patterns occurred after the Khmer Rouge: suppose MT's left the Southwest Zone due to repression and poverty, and sought to relocate somewhere nearby that had impacted less by violence. They would likely pick the parts of Kampong Speu within the West Zone, since they have preexisting ties to the province. This could explain the result if the following assumption is true.

In support of Assumption 1, I report the rates of international migration by commune in both the West and Southwest. Table A.11 shows international migration rates are similar between zones. This evidence suggests that pull factors that may attract migrants are relatively equal by area, suggesting there is nothing about having been in the Southwest zone that would lead persons who would otherwise not move to relocate to the zone.

Table A.11: International Migration (Commune)

	(1)	(2)	(3)	(4)
1 SW	0.11	0.07	−0.21	−0.09
	(0.10)	(0.13)	(0.13)	(0.19)
N.	87	87	87	87
Effective N.	87	87	21	21
Bandwidth	-	-	7.46	7.46

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Robust standard errors reported in parentheses. Distance to boundary and its interaction with SW omitted from table for space. Outcome is the share of the population that is international migrants per commune.

Assumption 2: MT wealth exceeds other groups on average $E[Y|S = MT] > E[Y|S \neq MT]$

Assumption 2 implies that those who escaped the Southwest zone - and who live

in the West zone but would have migrated had they instead been assigned to the Southwest - have more wealth on average than other groups. If this assumption were to be untrue, then the estimates in the main paper hold, since without Assumption 2, there is no reason to think that migration spurred by DK motivated wealth to flow to the former West.

Following Assumption 1 and 2, I borrow from [135] and propose the following trimming exercise: I drop the top $x\%$ of wealthy respondents in the West zone, and reestimate the RD. The assumption that the top $x\%$ in the West is a highly conservative and strong assumption, however, making the test more difficult to pass should increase one's confidence in the robustness of the findings.

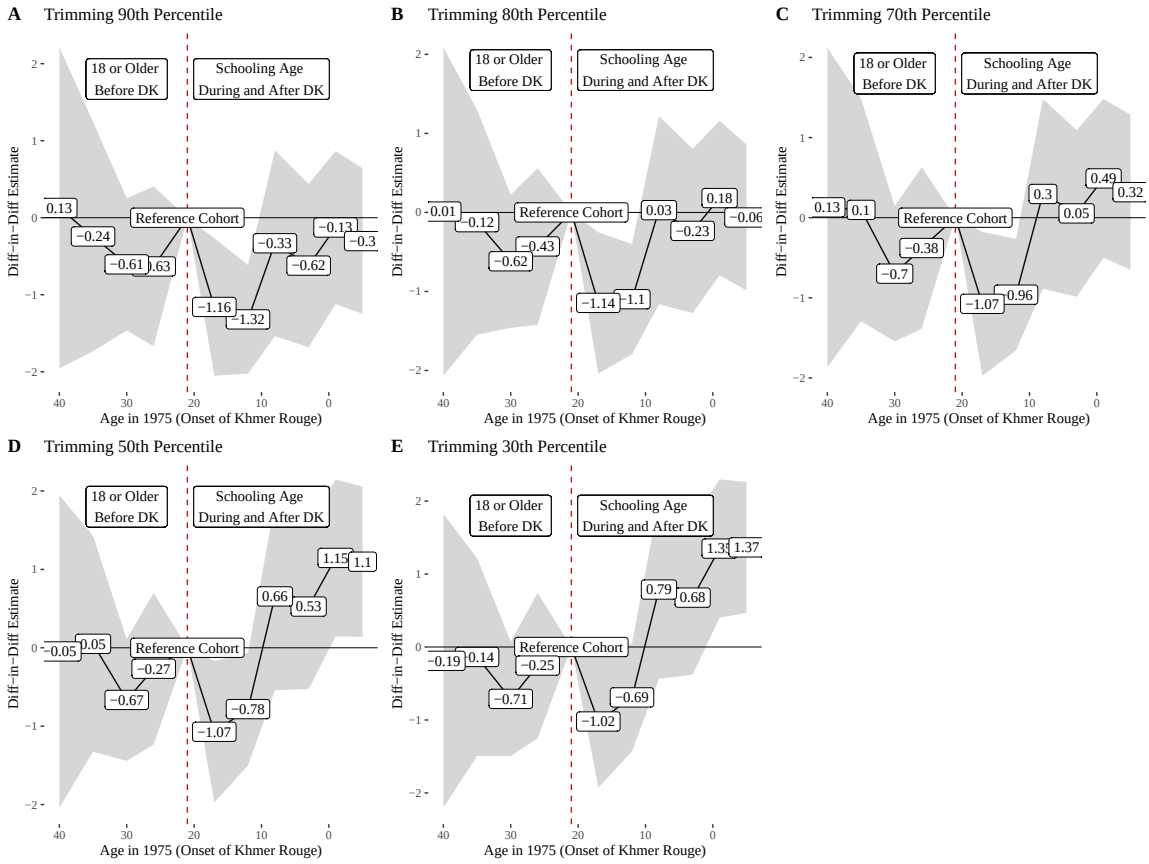
First, I present results using DHS wealth data among rural households in Table A.12. The results hold up to 15% of the wealth distribution assumed to be MT types. Second, in Figure A.19 I reproduce the dynamic difference-in-difference results while dropping the upper percentiles of schooled individuals. The results hold up to dropping the top 50% of schooled persons from the West.

Table A.12: Results from Trimming: Wealth

	(1)	(2)	(3)	(4)	(5)	(6)
$x\%$	95	90	85	80	75	70
1 SW	-0.26***	-0.25***	-0.22***	-0.18***	-0.14**	-0.09
	(0.06)	(0.05)	(0.05)	(0.06)	(0.06)	(0.07)
BW	9125.75	9191.93	9307.24	9764.86	10200.60	11031.62
Total N	13100	13063	12936	12733	12505	12280
Effective N	3152	3138	3103	3148	3352	3388

Note: Outcome is DHS wealth data. Each column drops a percentile of top wealthiest persons in the West zone - e.g. Column (1) drops the top 5% wealthiest from the West zone and retains the bottom 95%, Column (2) drops the top 10% and retains the bottom 90%, ect.

Figure A.19: Event Studies Trimming Upper Education Percentiles



Note: Robust errors clustered at the village. Each panel drops top percentile of schooled persons from the West zone.

A.5 Human Capital Mechanism

A.5.1 Average Years and Attendance: 1998

Table A.13: Human Capital in 1998: Years of Schooling and Attendance

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Yrs Educ.				Attendance Rate			
SW	-1.1^{**}	-0.58	-1.1^{*}	$-0.69^{\dagger}$	-6.02^{**}	-4.64^{*}	-7.79^{**}	-3.42
	(0.41)	(0.39)	(0.43)	(0.38)	(2.15)	(2.16)	(2.43)	(2.29)
Effective N	312	285	442	439	355	313	597	476
Bandwidth	5.78	4.85	9.87	9.66	7.13	5.85	14.27	10.78
μ Control	4.16	4.16	4.16	4.16	30.07	30.07	30.07	30.07
σ DV	1.9	1.9	1.9	1.9	12.8	12.8	12.8	12.8
Segment FE	-	✓	-	✓	-	✓	-	✓
Dist. Capital Covariate	-	✓	-	✓	-	✓	-	✓
Linear	✓	✓	-	-	✓	✓	-	-
Quadratic	-	-	✓	✓	-	-	✓	✓

Note: Yrs. Educ. is the average years of education in a village. Attendance Rate is the share of persons who are enrolled in school below 25 (i.e. schooling aged). $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

A.5.2 Adjusting for Distance to Schools

Table A.14: Human Capital in 1998: Adjusting for Distance to Schools

Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Literacy Rate		% No Educ.		Yrs. Educ.		Attendance Rate	
1 SW	-4.94^{*}	-5.62^{*}	4.23^{*}	5.15^{*}	$-0.62^{\dagger}$	-0.76^{*}	-5.14^{*}	$-4.21^{\dagger}$
	(2.44)	(2.71)	(2.11)	(2.41)	(0.37)	(0.37)	(2.08)	(2.3)
Effective N	302	424	313	430	286	436	313	451
Bandwidth	5343.89	9249.23	5847.32	9352.39	4860.19	9592.27	5843.17	10170.76
μ Control	62.88	62.88	50.48	50.48	4.16	4.16	30.07	30.07
σ DV	17.03	17.03	14.85	14.85	1.9	1.9	12.8	12.8

Note: Literacy Rate is the percentage of persons over 15 who can read write. % No Educ. is the percentage of people who have no schooling. Yrs. Educ. is the average years of education in a village. Attendance Rate is the share of persons who are enrolled in school below 25 (i.e. schooling aged). $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

A.5.3 School Level Outcomes

Table A.15: School Outcomes

School Outcome	Staff/Student Ratio				Students Per Classroom			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
1 Southwest	−0.28	−0.15	−0.30	−0.22	−0.38	−1.33	−2.89	−4.43
	(0.35)	(0.42)	(0.51)	(0.59)	(2.32)	(2.71)	(3.65)	(3.74)
N	495	495	495	495	495	495	495	495
Polynomial	-	✓	-	✓	-	✓	-	✓
Segment FE	-	-	✓	✓	-	-	✓	✓

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Robust standard errors reported in parentheses. Data collected at the school level. Staff/student ratio is the number of employees in the school divided by the number of students. Students per classroom is the number of students divided by the number of rooms in the school.

A.5.4 Human Capital Persistence: 2008

Over the course of the 2000's, Cambodia engaged in the Education for All (EFA) initiative in order to reduce illiteracy and increase school enrollment through a variety of reforms, including reductions of fees for schooling. I study rates of convergence in human capital accumulation - schooling specifically - using 2008 census data. I basic measures of human capital attainment largely converge between villages in either zone by 2008. However, rates of economic development have not. The finding is consistent with theoretical models where universal programs do not eliminate inequalities ravallion2015economics. Since fees for schooling benefited both zones equally, persons who had higher baseline levels of human capital could excel ever more from the elimination of schooling fees compared to less educated persons in the Southwest. However, figure E.1 shows that the development gap persists for tertiary and secondary education, which is robust to dropping outlying observations.

Table A.16: Schooling Persistence: 2008 Census

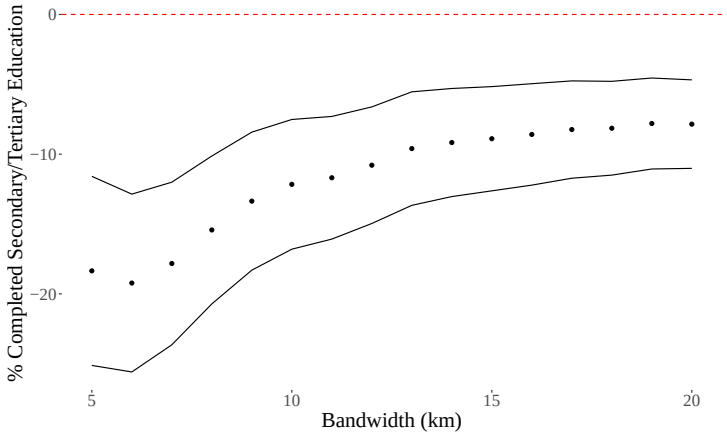
Outcome	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Attendance Rate		Yrs. School		Literacy Rate		% No Educ.	
SW	-1.61	$-2.25^{\dagger}$	-0.13	-0.24	-1.29	-2.27	-0.82^{**}	-1.15^{*}
	(1.24)	(1.23)	(0.12)	(0.15)	(1.89)	(2.18)	(0.3)	(0.47)
Effective N	336	612	294	391	358	569	405	710
Bandwidth	6.48	14.11	5.03	8.04	7.17	12.8	8.49	17.1
μ Control	29.77	29.77	5.16	5.16	75.04	75.04	1.18	1.18
σ DV	6.46	6.46	0.65	0.65	14.07	14.07	2.13	2.13

Note: Unit of analysis is the village. SHAC standard errors reported in parentheses.

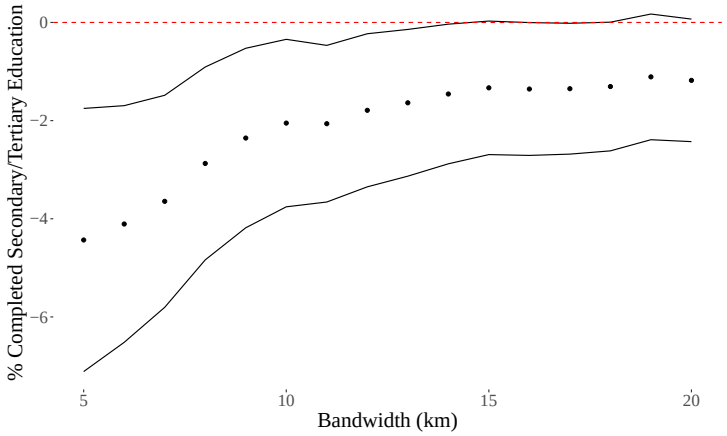
A.5.5 Tertiary & Secondary Schooling

Figure A.20: Tertiary and Secondary Education (All Data)

(a) Tertiary and Secondary Education



(b) Tertiary and Secondary Education (Dropping Outliers)



Notes: Parametric RD estimates at alternative bandwidths. Panel A shows the results where a 4 km buffer is created around villages to compute luminosity. Panel B shows results where a narrower 1 km buffer is used to compute luminosity. SHAC standard errors used to construct 95% confidence bands.

A.5.6 Schooling By Gender

Table A.17: Education Differences by Gender: 1998 Census

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Males	%No Educ. Males				Lit. Rate Males			
SW	6.83**	3.98†	4.31†	3.77	−5.49*	-3.23	−5.86*	−4.59†
	(2.23)	(2.06)	(2.39)	(2.38)	(2.25)	(2.13)	(2.66)	(2.49)
Effective N	320	311	624	452	317	321	528	435
Bandwidth	6157.66	5758.35	15246.56	10249.45	6003.24	6214.12	11983.37	9554.23
B: Females	%No Educ. Females				Lit. Rate Females			
SW	8.33**	3.81	8.35**	4.95†	−9.41**	-4.53	−10.82**	−6.4†
	(2.77)	(2.49)	(2.95)	(2.77)	(3.18)	(3.15)	(3.7)	(3.42)
Effective N	306	312	507	434	309	279	413	412
Bandwidth	5526.79	5808.96	11714.12	9432.27	5632.18	4690.57	8993.07	8951.34
C: Gap	%No Educ. Gender Gap				Lit. Rate Gender Gap			
SW	3.33†	1.64	3.8†	2.48	-1.38	-1.2	-1.52	-1.51
	(1.97)	(1.94)	(2.13)	(2.06)	(0.85)	(0.89)	(1)	(0.97)
Effective N	310	314	447	460	440	417	611	668
Bandwidth	5658.08	5893.41	10076.75	10401.42	9786.66	9125.82	14902.86	16266.69

RD estimates using education by gender as the outcomes of interest. Panel A studies the rates of no education and literacy by males, and Panel B by females. Panel C studies the gender gap in these outcomes, defined as the difference between human capital rates by group. Overall, I find little to no evidence of differential gender effects.

A.5.7 Cohort Placebo Test

Table A.18: Placebo Tests: Cohort Analysis

	(1)	(2)	(3)	(4)
SW × 1(DK age <= 35)	−0.49			
	(1.37)			
SW × 1(DK age <= 30)		−0.58		
		(0.81)		
SW × 1(DK age <= 25)			0.13	
			(0.66)	
SW × 1(DK age <= 20)				0.42
				(1.08)
N.	537	537	537	537
SD DV	3.5	3.5	3.5	3.5
Village FE	✓	✓	✓	✓
Commune by Decade FE	✓	✓	✓	✓
Gender FE	✓	✓	✓	✓
Wave FE	✓	✓	✓	✓

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Note: Village clustered errors reported in parentheses. Outcome is the years of schooling. Sample is individuals who were over 18 years old in 1975, meaning they would have completed schooling before the DK regime began.

A.5.8 Intergenerational Effects by Material Education

Table A.19: Child Health Between Zones by Maternal Education Level

Outcome	(1) Health Index	(2) Height/Age	(3) Weight/Age	(4) Weight/Height
	Panel A: Mothers with No Education			
1 SW	-2.52^{***}	-0.73	-1.74^{**}	-2.77^{***}
	(0.73)	(0.85)	(0.53)	(0.44)
	[0.97]*	[0.80]	[0.69]*	[0.54]***
Bandwidth	12.90	9.32	13.33	9.57
N. Individuals	73	65	73	59
N. Clusters	27	25	27	21
	Panel B: Mothers with Education			
1 SW	-0.30	0.11	-0.22	-0.90^{***}
	(0.40)	(0.22)	(0.29)	(0.15)
	[0.36]	[0.19]	[0.27]	[0.42]*
Bandwidth	15.97	10.25	15.56	10.565
N. Individuals	233	184	232	160
N. Clusters	38	29	37	24
Controls	✓	✓	✓	✓
SD DV	1.38	1.26	0.99	0.98

Note: Unit of analysis is the 3-5 year old individual from the 2000-2014 DHS survey waves - the children of the generation exposed to the Khmer Rouge. Health index (Column 1) is the first principal component of individual health measures. Height/Age is the standard deviations from the median of individual height for age (stunting), Weight/Age is standard deviations from the median of weight for age (wasting), Weight/Height is standard deviations from the median of weight for height (underweight). Analysis within rural households to maximize comparability. Controls include the age of the mother and its square and survey year fixed effects. Robust standard errors clustered at the survey area reported in parentheses. Panel A studies children with mothers without education. Panel B studies children of mothers with at least some education. Clustered standard errors, clustered by survey area, reported in parentheses. Wild cluster bootstrapped errors in brackets. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

A.5.9 Labor Market

Table A.20 presents descriptive statistical evidence that conforms with qualitative information about the relationship between own account workers, schooling, and earnings. Column (1) regresses self-employment status (a binary variable scored 1 when an individual is self-employed and zero otherwise) on years of schooling, age, its square, rural/urban status, and gender. An additional year of schooling reduces the probability of self-employment by 3 percentage points. Next, I regress income on self employment and covariates. The relationship is strongly negative. Column 3 uses a 2sls strategy and shows self employment instrumented by schooling predicts lower income as well. The evidence provides quantitive support for the education, employment, and poverty nexus I argue is the driver of poverty traps in more repressed villages.

Table A.20: Schooling, Self Employment, and Income

	(1) Self Employment	(2) Income	(3) Income
Years of School	-0.03^{***}		
	(0.00)		
Age	4.44^{***}	-0.19	1.64
	(0.42)	(0.92)	(1.72)
Age^2	-2.75^{***}	1.67^{*}	0.69
	(0.41)	(0.82)	(1.13)
Rural	0.09^{**}	-0.29^{***}	-0.24^{***}
	(0.03)	(0.06)	(0.07)
Female	0.00	-0.02	-0.00
	(0.03)	(0.05)	(0.06)
Self Employed		-0.51^{***}	-0.86^{**}
		(0.06)	(0.28)
N.	975	975	975
Adj. R^2	0.23	0.11	0.09
Estimator	OLS	OLS	2SLS

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Labor Force Survey. Unit of analysis is employed working aged (11-59) individuals. Data from 2000-2001 Labor Force Survey. Pr(Self Employed) is scored 1 for persons who are own account workers. Income is individual wages, remuneration, earnings, tips reported from the last month in 10,000 riels, and productivity is riels divided by working hours.

A.6 Background Information

A.6.1 Zone Commanders

Mok (Chhit Choeun) was native to Takeo province in southwestern Cambodia, where he had helped lead an insurgency during the independence era, and at one point served under Sy before becoming a commander himself. Mok's rise to power placed him in direct competition with Sy, an older leftist who harbored less extreme views about the direction of the revolution. Mok promoted his family members - including his sons, daughters, and in-laws - in positions of power throughout the Southwest [p. 87]kiernan2008pol. Mok was charged with purging party elites who were disloyal to the regime. He survived the regime, assisting in military leadership of the Khmer Rouge insurgency after Vietnam deposed the regime.

Sy (Chou Chet) had been a politically active leftist since the 1940s, participating in independence efforts against the French [p. 129]vickery1984cambodia. Sy had been the secretary of the entire Southwest during the civil war (which included the Southwest and West during the DK regime) but was demoted in 1973 in favor of Mok kiernan1989american.

Sy had pro-NVA opinions [p. 79]kiernan2008pol which had set him apart from more nationalist hardliners closer in Pol Pot's circle. During a CPK meeting in Kampong Speu, Sy's address stood out to audience members as he did not refer to the Vietnamese as enemies and spoke of providing housing for individual families [p. 390]kiernan2008pol. Due to the Khmer Rouge's general hostility towards Vietnam, the remarks highlighted a contrast between the ideology of Chet and the rest of DK.

chandler2018brother avers Sy revealed an earnest assessment of the Khmer Rouge's policy failures in the autobiography DK cadres forced him to concoct as a confession while jailed in S-21.

> [I said that] the current regime was a highly dictatorial one, too rigid

> and severe, one that overshot the comprehension and consciousness of the people. Therefore a lot of people were muttering and moaning about how they were doing a lot of work and getting little back for it, how they couldn't get together with their families, couldn't rest, never had any fun, and so on.

Although the passage is presented as a dialogue that was almost certainly fictitious, as the party forced the biography to be a confession of crimes including collaboration with the CIA and the Vietnamese, the remark is consistent with what was reported about Sy: he was a more moderate leader who was skeptical of the regime's brutal approach.

A.6.2 Educational System Before, During, and After DK

Cambodia inherited its contemporary educational system from French colonization ayres2000anatomy. The Cambodian educational system was expanding prior to the onset of the civil war and the Khmer Rouge regime. The national government dedicated a large share of the budget to education, and in 1969 the country boasted "3202 primary schools, 163 secondary schools, and nine universities" ayres1999khmer.

When the Khmer Rouge took power, traditional teachers were targeted for purges. 90% of schools were destroyed clayton1998building. Whereas the regime attempted to provide education, schooling was subordinate to labor and occurred in irregular settings, such as fields and stables. Teachers were not "new people," the regime instead favored "base people" as educators, who had little to no experience. Students lacked supplies due to general resource shortages during the DK period ayres1999khmer. Within a year of the Khmer Rouge's downfall, school reopening was attempted. Vietnam focused on quantity rather than quality, which failed to address the dilapidated school infrastructure or limited teaching corps [p. 148]ayres2000anatomy.

A.7 Genocide Intensity

This section of the appendix presents indirect evidence that exposure to increased genocide intensity is correlated with contemporary development. I leverage data on the location of mass graves in Kampong Speu to construct a gravity-based measure of exposure to genocide. Then, I estimate the relationship between development (poverty and luminosity) and the intensity of genocide exposure instrumented by zone.

A.7.1 Measuring Genocide Intensity

The data on mass graves is from the Cambodia Genocide Program Geographic Database. The data was collected by a team of researchers who, through interviews with locals and archival documents, excavated mass graves during the DK era. The data include an estimated 1 million bodies and over 300 mass grave locations, with 16 of those locations residing in Kampong Speu. The Khmer Rouge era included deaths from other events, including a mass bombing campaign during the civil war. However, traditional Cambodian practices included cremation at the time, and many of those killed in bombings would have been incinerated. Cremation and other traditional practices were banned during the DK regime, meaning mass graves filled with bodies are most likely attributable to Khmer Rouge activity. Forensic evidence breaking down who in the graves were victims of execution versus other causes of death, such as starvation, is unavailable. Nonetheless, starvation was a DK tactic to cheaply eliminate political rivals - to the extent the data include both sets of deaths, it may still capture the degree of DK brutality etcheson2000mapping.

Mass grave sites were almost always constructed near security centers - indeed, most are within a kilometer of a security center. Security centers were managed at the zone level, meaning the head of the each respective zone would have retained

authority over who to send to these centers and what to do with them, including torture, re-education, release, or execution etcheson2000mapping.

One concern with attributing a village's geographic proximity to mass grave sites to genocide intensity is population transfers that occurred during the DK regime. One may reasonably object that a village near a mass grave with hundreds of bodies may have had few residents of that village buried in the grave, since people were forced to migrate internally during the regime to perform forced labor. As such, my measure may contain some error.

A key advantage of my approach is that I study a very localized instance of the genocide, which allows me to circumvent this empirical problem. The Southwest zone was subject to a mass population transfer, but it was an outflow of residents towards the Northwest zone, which was considered the breadbasket of the regime [p. 390]kiernan2008pol. To the extent the mass graves may measure genocide exposure with error, it is likely an underrepresentation in the Southwest zone, since residents forced to move to the Northwest zone may have died there instead of the Southwest.

Nonetheless, population movements occurred within zones overtime. The Khmer Rouge formed mobile work teams who would perform tasks within their zone to construct irrigation canals. A resident may therefore be buried within their zone, but could have been executed near a village that they were not from, and very far from their village of birth.

To address this issue, I adopt a gravity based measure of genocide exposure. The intuition behind the measure is that villages that are closer to more large mass graves were likely more exposed to mass killing themselves. The assumption is reasonable, since a villager living near a security center who was accused of some sort of wrong-doing could be sent to a security center to be executed at a lower cost compared to a village who was far from a prison.

$$\text{Genocide Intensity}_v = \sum_{j \in J_z} \left(1 + distance(\text{Village}_v, \text{Mass Grave}_j)^{-1} \times \frac{\text{Bodies}_j}{\sum_{j \in J_z} \text{Bodies}_j} \right)$$

First, I compute the distance between a village v and each mass grave j within the province/zone z. I add 1 to prevent very proximate mass graves from having excessive influence, and take the inverse of the quantity, so larger values represent being closer to a grave and smaller values representing more distance. Second, for each grave, I multiple this quantity by a weight which equals the proportion of total bodies in zone grave j holds. Third, I sum these values together for each mass grave to obtain a final measure of genocide intensity for each village. Finally, to ease interpretation, I scale the variable from 0-100 as follows.

$$\left(\frac{\text{Genocide Intensity}_v - min(\text{Genocide Intensity})}{max(\text{Genocide Intensity}) - min(\text{Genocide Intensity})} \right) \times 100$$

The measure represents a village's exposure to genocidal violence at the intensive rather than the extensive margin - we cannot gleam from the measure whether a village had executions or not, but it does capture the intensity of mass killing surrounding the village. While imprecise, population transfers outside of the Southwest suggest the measure will be biased against the hypothesized positive effect of the Southwest zone on genocide intensity. Another bias concern may be that the location of mass graves covary with distance to the capital. To partial out the distance to the capital, I adjust for this covariate in my estimates.

Figure A.21 plots the discrete increase in genocide intensity moving from the West to the Southwest zone.

Figure A.22 displays predicted poverty across space, actual poverty rates in 2011 at the village level, and the location and size of mass grave sites.

Figure A.21: Genocide Intensity in Kampong Speu Province

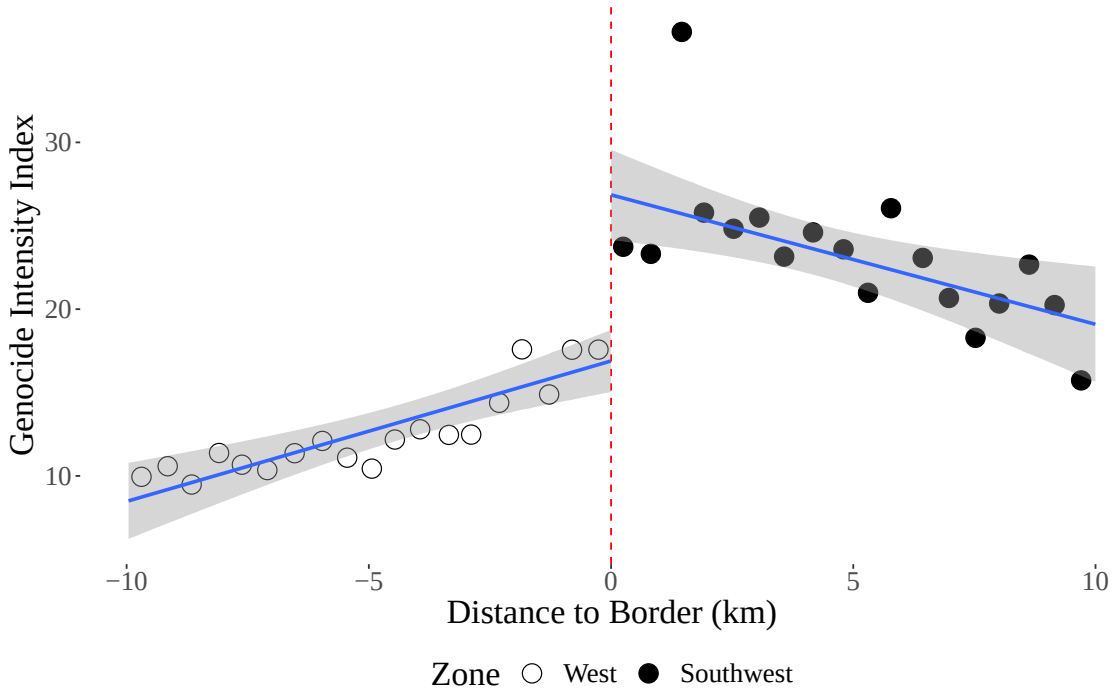
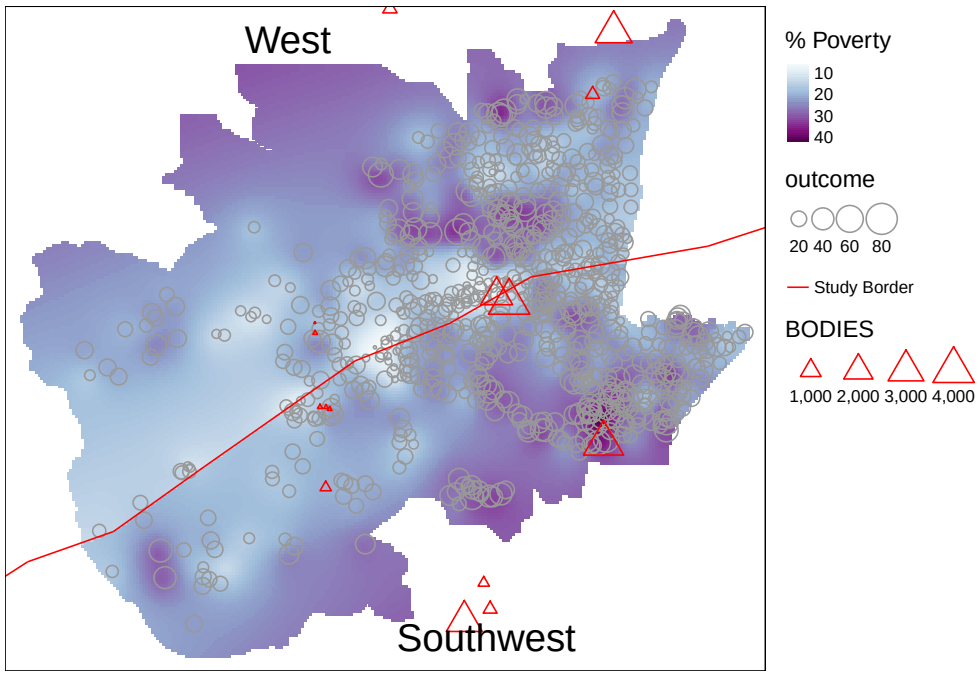


Figure A.22: Poverty and Mass Repression in Kampong Speu Province



Note: Dots are village locations, shaded regions are predicted values using ordinary kriging, Red line shows the border dividing the Southwest and West. Boxes with triangles are mass graves, sized to represent the estimated number of bodies found within them.

A.7.2 Estimating Genocide Intensity

One approach to unpacking the effect of genocide intensity and development would be regressing development directly on the measure of exposure. However, this will likely yield biased estimates. The location of mass graves was not randomly assigned; the Khmer Rouge disproportionately executed educated persons and former officials. This suggests that proximity to mass violence would be positively correlated with past development. If it was the case that genocide has a negative effect on development, and that places which were exposed more to genocide were more developed beforehand, one may recover a positive or zero coefficient from regressing development on repression due to the omission of unobservable, pre-DK factors.

To estimate the first order effect of zone assignment on genocide intensity, along with the influence of genocide intensity on development, I use the following two-staged least squares (instrumental variable) approach.

$$\text{Genocide Intensity}_{cv} = \alpha_c + \delta 1(\text{Southwest Zone}_{cv)} + \sum_{k=1}^{K} \zeta^k X_v^k + \varepsilon_{cv}$$

$$out_{cv} = \alpha_c + \beta \widehat{\text{Genocide Intensity}}_{cv} + \sum_{k=1}^{K} \zeta^k X_v^k + \eta_{cv}$$

The first stage estimates the impact of zone assignment on genocide intensity. The parameter δ captures the average difference between genocide intensity between the Southwest and the West, net of the K covariates X (distance to Chbar Mon and Phnom Penh). I include fixed effects for commune (α_c) which absorbs differences between administrative units and compares villages in a similar environment.

The second stage uses the predicted increase in genocide intensity to estimate the relationship between increased concentration of violence around a village and development. For both equations, standard errors are clustered at the commune, of which there are 87, to account for serial correlation induced by the fixed effects.

The key identifying assumption is zone assignment does not effect development through any channel outside of its effect on genocide intensity, after conditioning on covariates. While this assumption cannot be proven, it is reasonable in this context. As discussed in the main text, there is a dearth of qualitative evidence suggesting the zones differed along non-repression based dimensions.

A.7.3 Genocide Intensity Results

Results reported in Table A.21 show villages in the Southwest zone experience a standard deviation increase in genocide intensity. The result corresponds with qualitative accounts that the Southwest zone was the "toughest sanctuary of the Khmer Rouge movement."

The 2SLS results show a similar pattern. Remarkably, a standard deviation increase in genocide intensity corresponds with a 3.9% increase in poverty, which is fairly close to the RD benchmark of 4.53% in the main text. The luminosity results trend in the same direction, albeit at a smaller magnitude than the RD results. Overall, the evidence suggests villages exposed more to the genocide by virtue of being in the Southwest zone are less developed today.

The results provide evidence consistent with the account that genocide exposure from the Southwest zone reduced contemporary economic development. If it was the case that the Southwest zone indicator was not correlated with genocide intensity, and that instrumented genocide intensity did not predict development, the argument in the paper would be implausible. Nonetheless, I caution that the result does not establish with certainty that repression directly causes poverty.

Table A.21: Genocide Intensity Results

	Genocide Intensity	Household Poverty	Village Luminosity
1(Southwest)	12.84^{***}		
	(2.39)		
$\widehat{\text{Genocide}}$		0.33^{**}	-0.04^{***}
		(0.12)	(0.01)
N. Villages	1359	1359	1359
N. Clusters	87	87	87
Estimator	First Stage	2SLS	2SLS
SD DV	12.04	10.55	0.76
Commune FE	✓	✓	✓
Covariates	✓	✓	✓

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Models include commune fixed effects and covariatates adjusting for distance to Phnom Penh and Chbar Mon. Robust standard errors clustered at the commune reported in parentheses.

Appendix B

Appendix 2: Oil Crops and Social Conflict

B.1 Data

B.1.1 District Sample

District borders from 1995 are chosen because they are pre-sample, therefore the boundaries are likely not determined by endogenous conflict or price trends. Boundaries before the onset of democratization are more exogenous than choosing a time during democratization as group conflict and redistricting during this period are correlated. I aggregate smaller child districts back to their 1995 parents to conduct the main analysis. I use 1995 shapefiles from [164].

B.1.2 NVMS Conflict Data

Conflict data description from [22].

- Governance Conflict: Violence is triggered by government policies or programs (public services, corruption, subsidy, region splitting, etc.)

Table B.1: NVMS Summary Statistics

Conflict	Standard Deviation	Mean	Sum	Min	Max	Median
Resource	5.72	3.58	5586.00	0.00	53.00	2.00
Government	3.40	2.01	3133.00	0.00	33.00	1.00
Election	3.78	1.76	2751.00	0.00	61.00	1.00
Ethnic	6.39	2.26	3520.00	0.00	117.00	0.00
Popular Justice	31.30	15.49	24171.00	0.00	387.00	5.00
Law Enforcement	10.72	5.83	9088.00	0.00	144.00	2.00
Criminal	10.72	5.83	9088.00	0.00	144.00	2.00
Separatist	2.50	0.30	472.00	0.00	55.00	0.00

Table B.2: Resource Conflicts — Resource Conflict ¿ 0

Conflict	Standard Deviation	Mean	Sum	Min	Max	Median
Resource	6.24	5.092	5586.00	1.00	53.00	3.00

- Elections and Appointments: Violence triggered by electoral competition or bureaucratic appointments.
- Identity-based Conflict: Violence triggered by group identity (religion, ethnicity, tribe, etc).
- Popular Justice: Violence perpetrated to respond to/punish actual or perceived wrong (group violence only)
- Violence during law enforcement: Violent action taken by members of formal security forces to perform law-enforcement functions (includes use of violence mandated by law as well as violence that exceeds mandate for example torture or extra-judicial shooting).
- Violent crime: Criminal violence not triggered by prior dispute or directed towards specific targets.
- Separatism: Violence triggered by efforts to secede from the Unitary State of The Republic of Indonesia/NKRI

B.1.3 Spatial Data

Spatial data sources

- District polygons from [164], the centroids are used to construct the latitude and longitude controls as well as compute district area.
- Raster data on terrain ruggedness from [174] measured as mean ruggedness per polygon.
- Raster data on crop suitability and forests from [105] measured as polygon means.

B.1.4 Economic Data

Source is INDO-DAPOER

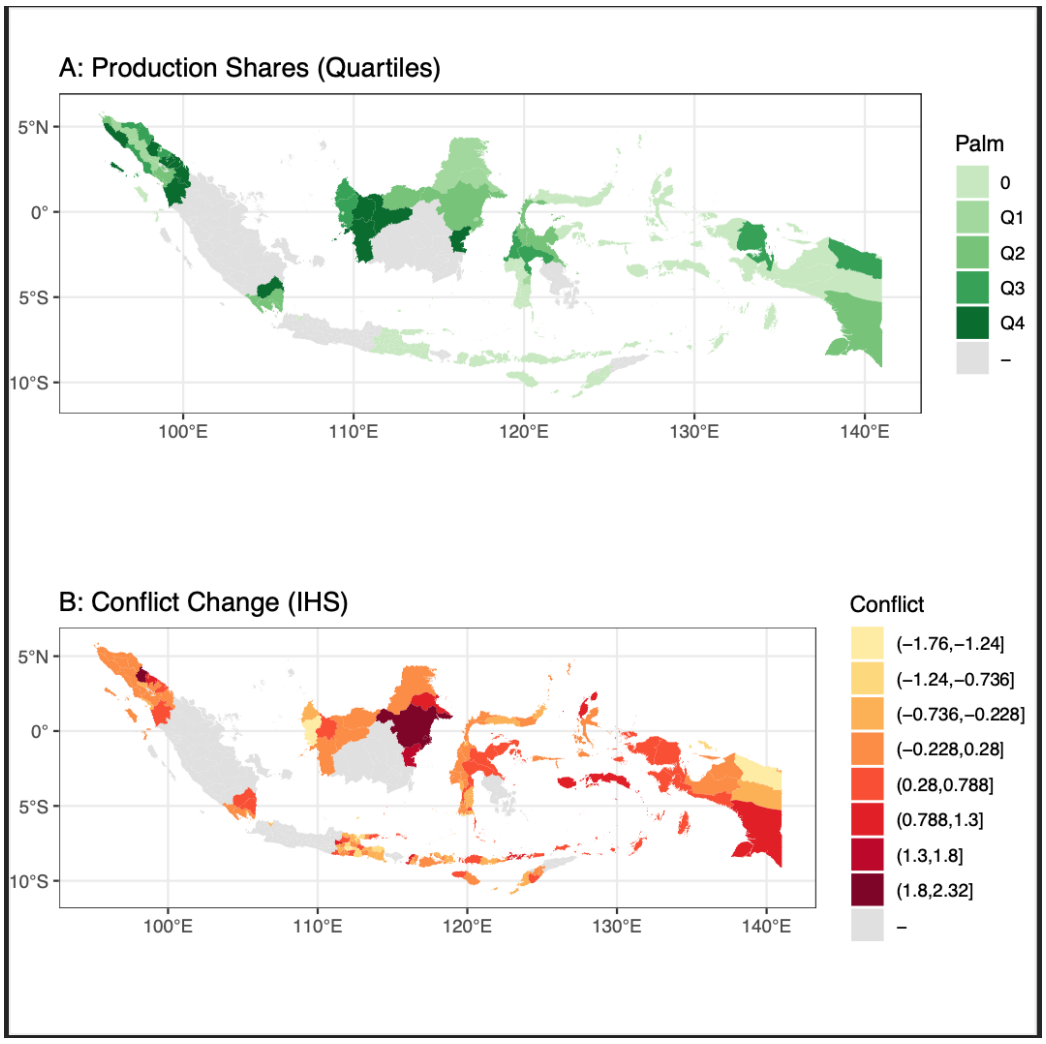
- Poverty rate: the percent of persons living in poverty per district from INDO-DAPOER.
- Unemployment rate INDO-DAPOER (Coverage beins
- GDP gross domestic product in constant IDR per district from INDO-DAPOER
- Population measured as total number of people from INDO-DAPOER
- Revenue is total district revenue in IDR from INDO-DAPOER.

Table B.3: INDO-DAPOER

Outcome	Standard Deviation	Mean	Min	Max	Median	Coverage
Poverty Rate	0.09	0.16	0.00	0.77	0.15	2005-2014
ln(GDPPC)	0.74	1.79	0.42	4.93	1.69	2005-2013
ln(Revenue PC)	0.89	11.50	7.63		11.44	2005-2014
Unemployment Rate	0.03	0.06	0.00	0.20	0.06	2007-2014
ln(Total Population)	0.82	13.14	10.28	14.86	13.16	2005-2014

B.1.5 Map of Sample

Figure B.1: Palm Production and Conflict



Note: Panel A plots the quartiles of palm production shares (lightest color corresponding to zero). Panel B plots the change in resource conflict (ihs terms). Grey in both panels represents districts that are not included in the sample.

B.2 Supplemental Descriptive Information

B.2.1 Oil Palm Value Chain

Palm oil is made by extracting the fruit of oil palms, a tree which is native to West Africa and brought to Southeast Asia during the colonial period. Producers plant trees and wait up to four years for them to mature enough to yield fruit.

After fruit is picked, farmers need to quickly extract the oil from fruit to be made into oil. In order to extract oil from fruit, planters sell fruit to processing mills. There is a pressing need to do so quickly for high quality oil, as the fruit begins to go bad after harvesting. Larger processing mills have a comparative efficiency advantage over smaller mills, creating "de facto local monopolies" for centralized mills [175, p. 11]. After mills produce oil from fruit, the crude palm oil is given to refineries and then manufacturers who make various products from the oil, ranging from food, soap, cosmetics, and biofuel.

Although smallholders grow a large share of palm oil - especially in Indonesia - smallholders do not dominate the industry in the same way as they do in other agricultural sectors. The high upfront capital costs to begin producing, along with the delay in profits, creates a barrier to entry that some can only clear by taking loans and entering into price contracts and land sharing arrangements with mills or larger plantation companies [35, 175, p.192].

Contracts can become a source of tension, since increases in market price may not necessarily translate into more profits for a smallholder depending on the contract, and conditions may stipulate farms exclusively grow oil palm on their land at the expense of other crops [141, 175].

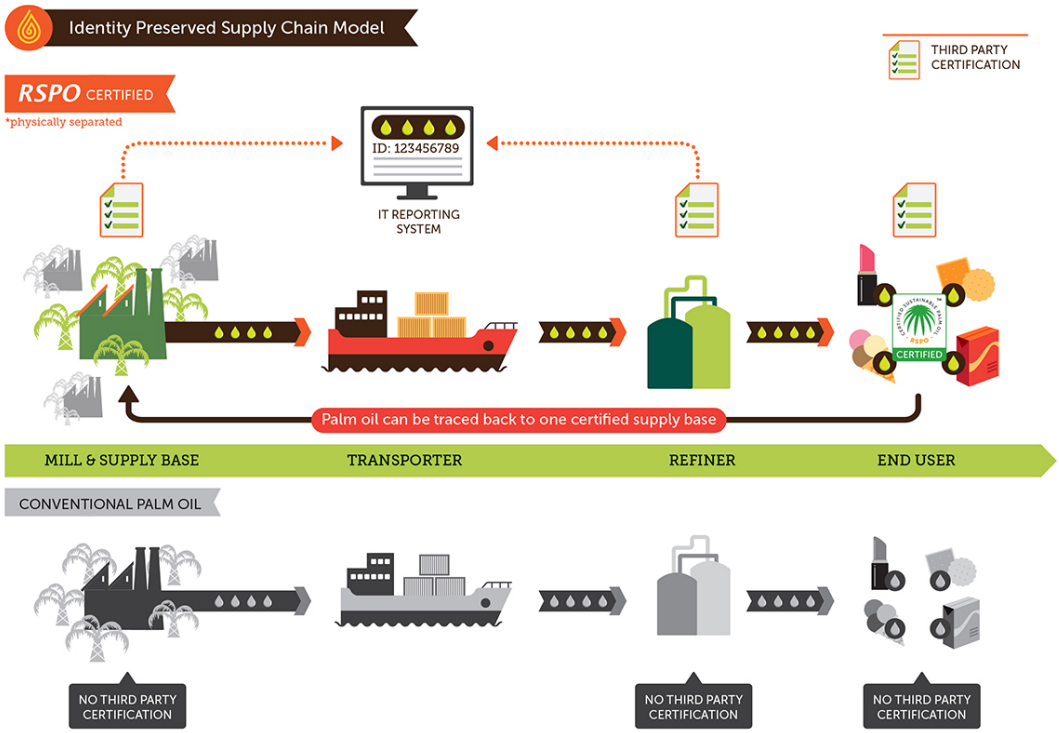
Smallholders can opt for independence rather than reliance on contracts with production mills. Yet, independent smallholding does not come without costs. Working with large mills gives smallholders a chance to sell to global markets; independence

prevents smallholders from selling their product more widely and may increase exposure to theft [175]. Independent smallholders tend to produce less than their counterparts, due to lack of access to information and more expensive inputs [35, p.192].

B.2.2 RSPO Criteria

Figure B.2 shows the value chain and levels of certification.

Figure B.2: RSPO Certification



Source: Roundtable on Sustainable Palm Oil "RSPO Supply Chains." `https://rspo.org/certification/supply-chains`

B.2.3 Additional Background on Indonesian Oil Palm Conflicts

Oil palm conflict involve several players and can occur either between community members or between state backed plantation companies and communities. Horizontal conflicts occur between citizens or community members. For example, a horizontal conflict may be between two smallholders who each wish to use the same plot of land to expand their operations during a boom period, or between a smallholder and non-producer who steals fruit to illegally sell to a processing mill.

Another variety of conflict is vertical, where companies and communities dispute territory, or the share of profit that is invested back into the community after a land deal. Palm producers typically buy support from local government when attempting to expand operations, meaning the company may be supported by the state when it expands into tenure insecure forested areas that communities claim to have exclusive rights over. Vertical conflicts may come in the form of violence against communities, for example, if the company hires private security forces to repress citizens protesting land deals, deforestation or fires used to clear land for production, or broken promises to compensate community members after land was acquired. Vertical conflicts could become horizontal, as plantation companies tend to hire locals for security services [181].

B.2.4 Scope Conditions and External Validity

Table B.4: Countries Fitting Scope Conditions

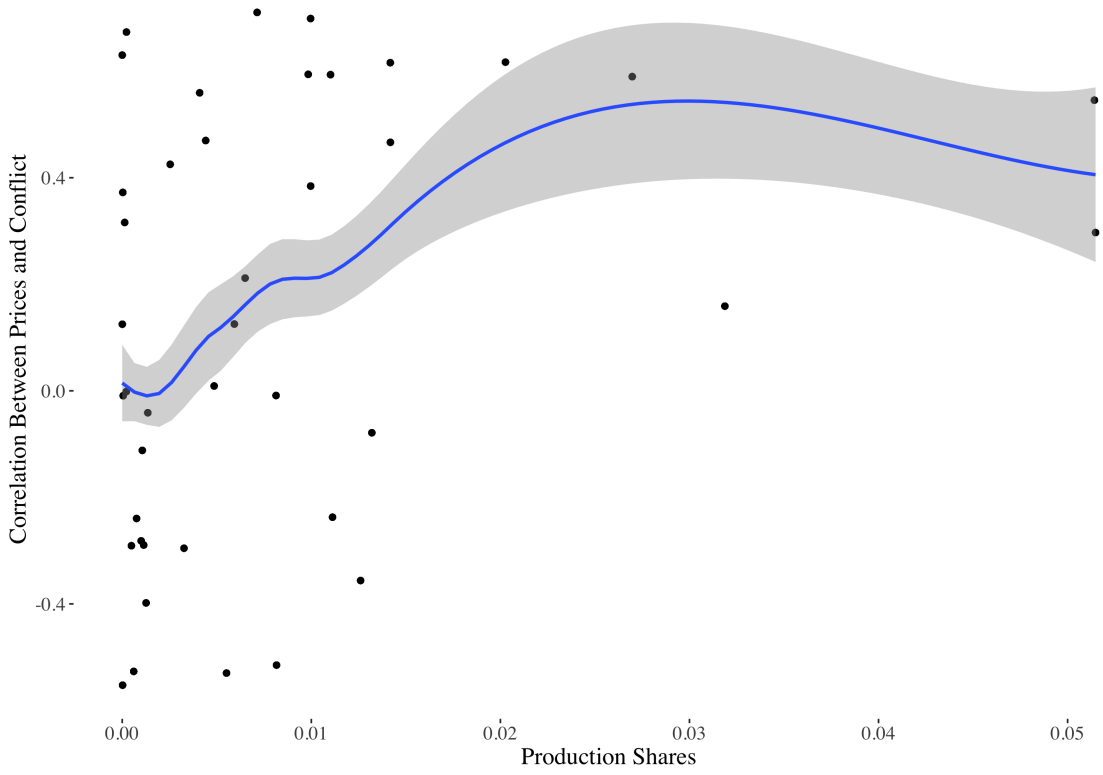
Name	% Reporting Tenure Insecurity	Agriculture GDP Share
Benin	34	22.17
Burkina Faso	44	31.11
Cambodia	33	29.06
Cameroon	31	14.23
Ghana	26	20.76
Indonesia	24	13.41
Ivory Coast	28	21.88
Kenya	28	28.27
Liberia	43	36.68
Madagascar	25	23.87
Mozambique	24	23.53
Niger	28	37.14
Uganda	26	24.85

Note: Tenure Insecurity data from [156] . Scores represent the percent of those surveyed who reported they felt it was "somewhat or very likely that they could lose the right to use their property or part of it against their will in the next 5 years." Agriculture GDP is the share of GDP from fishing, forestry, and agriculture divided by total GDP, and is expressed in percentage terms, and is computed from the 5 year average from 2012 to 2016 from and [191].

B.3 Main Panel Results Robustness

B.3.1 Descriptive Relationship

Figure B.3: LOESS Fit of Production Shares and the Price-Conflict Correlation



Note: Figure plots the correlation of price and conflict overtime (y-axis) against oil palm production shares, and fits a LOESS line to illustrate the relationship. As oil palm market share increases, the correlation between price and conflict rises as well.

B.3.2 Functional Form of DV

Table B.5: Functional From

	(1)	(2)	(3)	(4)
Panel A: Resource Conf. Log(+1)				
Shock	0.07***	0.09**	0.09**	0.08**
	(0.02)	(0.03)	(0.03)	(0.03)
Panel B: Resource Conf. Sqrt()				
Shock	0.09**	0.13**	0.12*	0.10*
	(0.03)	(0.05)	(0.05)	(0.05)
District & Year FE	✓	✓	✓	✓
Province × Year FE	-	✓	✓	✓
Terrain × Year FE	-	-	✓	✓
Full Controls	-	-	-	✓
N. Clusters	156	156	156	156
Num. obs.	1560	1560	1560	1560

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

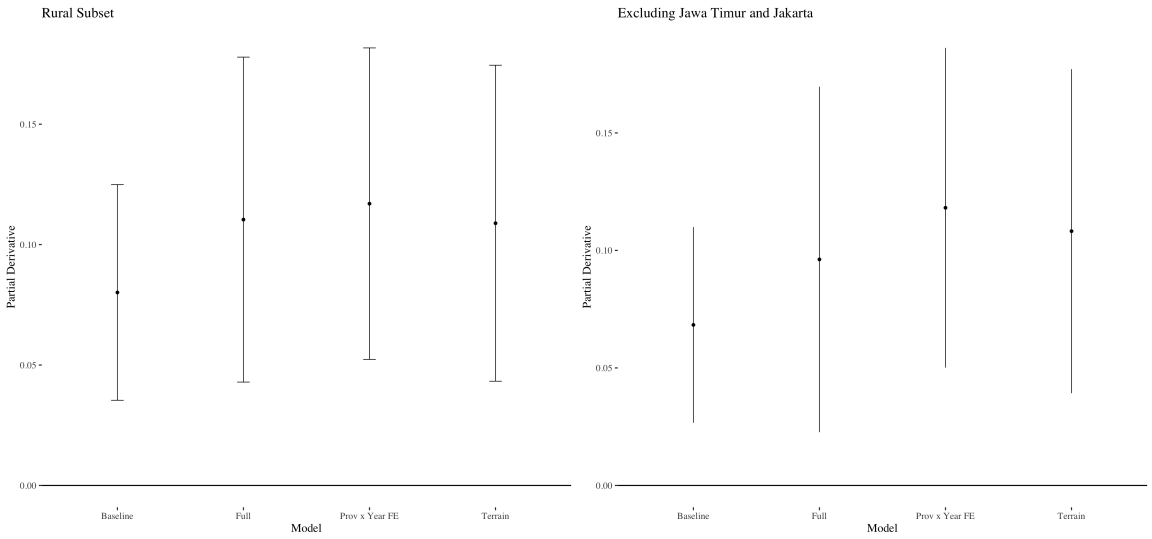
B.3.3 Measurement of Palm Oil Exposure

Table B.6: Alternative Measure of Production Exposure

	(1)	(2)	(3)	(4)
Palm Area divided by District Area x Price	0.06*			
	(0.03)			
Total Average Hectares x Price		0.07***		
		(0.00)		
log(Total Average Hectares) x Price			0.02*	
			(0.01)	
1(Production ¿ 0) x Price				0.14*
				(0.06)
N.	1560	1560	1560	1560
N. Clusters	156	156	156	156
District and Year FE	✓	✓	✓	✓

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Figure B.4: Subsets



Bands are 95% confidence intervals. Panel A reports results of Models 1-4 in Table 1 where data is censored to only include regencies (kabupaten) and cities (kota) are excluded from the sample. Panel B reports another restricted sample with Models 1-4 where observations from Java are dropped.

B.3.4 Robustness to Alternative Subsets

For the main estimates I choose to use all available subnational units. Resource violence in cities could be systematically lower during periods with positive shocks, as cities (kota) are less exposed to the palm sector. Further, the outer islands of Indonesia are more exposed to the palm sector, and may follow different cycles of violence than districts in Java. The time-varying controls ought to deal with this problem by allowing districts with different sizes, forest densities, terrain ruggedness, and pre-sample shares of GDP from agriculture to follow different trends. Urban areas are more forested, less rugged, and have lower shares of their GDP from agriculture. Moreover, province-year fixed effects compare districts within the same province and not between them, partially allaying the concern.

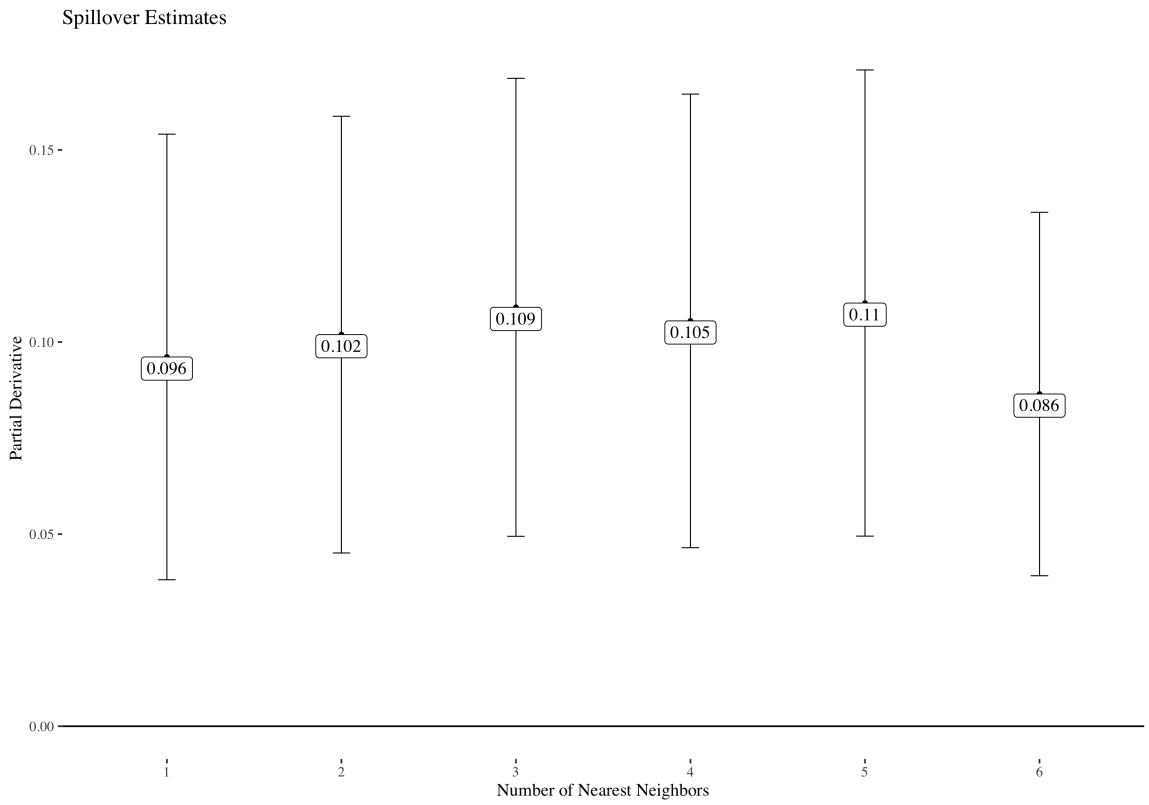
As an additional check, I exclude cities and East Java/Jakarta from the sample, and obtain similar results. Results are presented in Figure B.4.

B.3.5 Spillovers?

A potential source of bias would be spatial conflict spillovers. An underestimate of the relationship between palm shocks and conflict would occur if increased resource value in neighboring districts attracted predatory actors from a given locality to attempt to capture income, land, or palm itself from nearby areas. An overestimate may occur if conflict is contagious and crosses borders, for example, if a dispute over market access leads some citizens to leave town, only to displace other citizens and start another social conflict. The resulting bias would be upward and downward respectively in the two scenarios.

I model spillovers by including spatially lagged shocks for the k nearest neighbors to district i for $k = \{1, 2...6\}$. The coefficients grow slightly larger in size, suggesting the exclusion of spatial lags may generate a downward bias. The overall stability of the results upon their inclusion casts doubt on the relationship being explained by spillovers or neighborhood effects.

Figure B.5: Spillovers



Note: The coefficients of the inverse hyperbolic sine of resource conflicts regressed on shocks, spatially lagged shocks, and two way fixed effects. Only the partial derivatives with respect to the shock at district i are plotted to preserve space.

Table B.7: Recentered Estimates

	(1)	(2)	(3)	(4)
Centered Share x Price	7.96***	11.14***	10.62**	9.81**
	(2.16)	(3.32)	(3.46)	(3.47)
District & Year FE	✓	✓	✓	✓
Prov x Year FE		✓	✓	✓
Terrain Controls			✓	✓
Full Controls				✓
N. obs.	1560	1560	1560	1560
N. Clusters	156	156	156	156

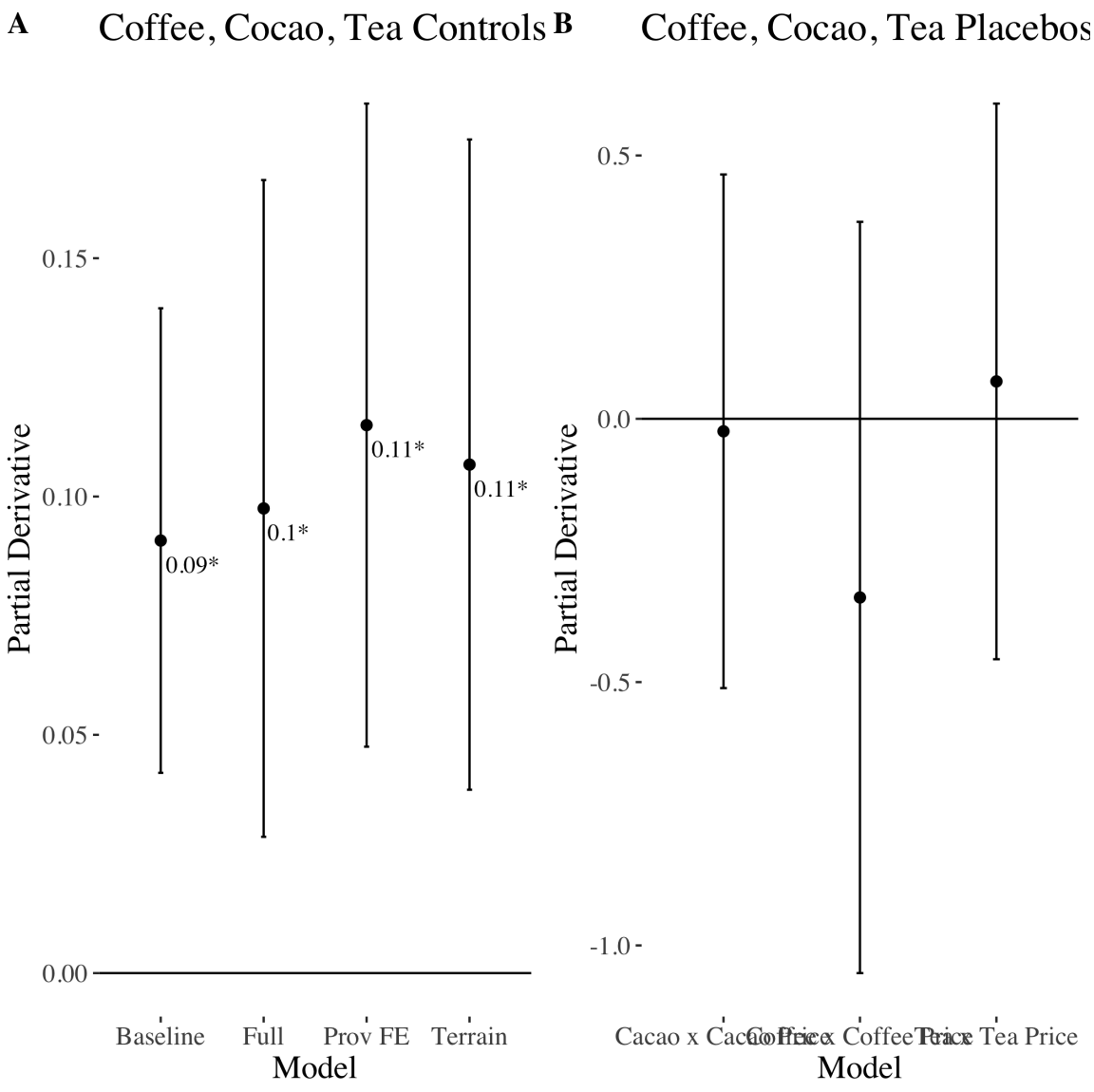
$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

B.3.6 Endogenous Exposure Weights

If exposure was due to unobserved spatial dependence in palm production, the structure of the data can be mimicked by averaging many draws of simulated shares which follow the same spatial structure as observed data. After removing the average of the spatial noise from the exposure weights, the cross-sectional variation represents the deviation of palm production shares from its expected distribution. I simulate 1000 draws of palm production exposure using the same spatial structure as the data, then subtract the average of these draws from the share variable to obtain a recentered measure. Recentered estimates are largely the same, suggesting the relationship between shocks and conflict is most likely not a product of unobserved assignment of palm production across space.

B.3.7 Other Crops?

Figure B.6: Different Cash Crops versus Oil Palm



Note: Plots controlling for other cash crop production. Panel A includes coffee, cacao, and tea suitability interacted with year fixed effects as controls in all 4 models, which otherwise mirror the models presented in Table 1. Panel B shows cacao, coffee, and tea prices interacted with cacao, coffee, and tea suitability as placebo checks.

I include relative measures of suitability for tea, coffee, and cacao and plot results in Figure B.6. First, I flexibly adjust the main estimates by interacting period ef-

fects with suitability measures for other cash crops, to assess whether palm shocks spuriously capture other crops conflict cycles. Panel A shows the main results do not meaningfully change when including commodity specific trends as a control.

Panel B shows negative (but insignificant) estimates of tea, coffee, and cacao shocks on resource conflicts. The result follows from the theory that crops with diffuse negative externalities and concentrated income gains result in social conflict, whereas other crops do not. Since coffee, cacao, and tea are (1) more easily intercropped, alleviating land use tradeoffs and (2) less reliant on monopolistic mills and therefore more smallholder dominant, it follows that we do not detect a positive relationship between shocks and resource violence.[1]

B.3.8 Price Shock Dynamics

The model in the main text assumes instantaneous effects of price shocks with no lag or lead effect. To check for pre-trends in shocks and conflict along with lagged effects, I estimate a dynamic model including lags and leads of price shocks up to four years in either direction. The four year benchmark reflects the time taken for a palm plant to mature after it is initially planted, theoretically motivating the lag structure. Substantively, the panel is 10 years, meaning there is a shock variable for each year -1.

However, the levels of prices are highly correlated overtime creating an issue of multicollinearity, which can lead to misleading point estimates and standard errors. Therefore, for the dynamic model, I transform the shock variable by taking differences between periods to compute changes, which are less dependent overtime. By way of illustration, the correlation between price and its lag 0.687, whereas the correlation between the change of prices in year t and the change in $t-1$ is 0.03.

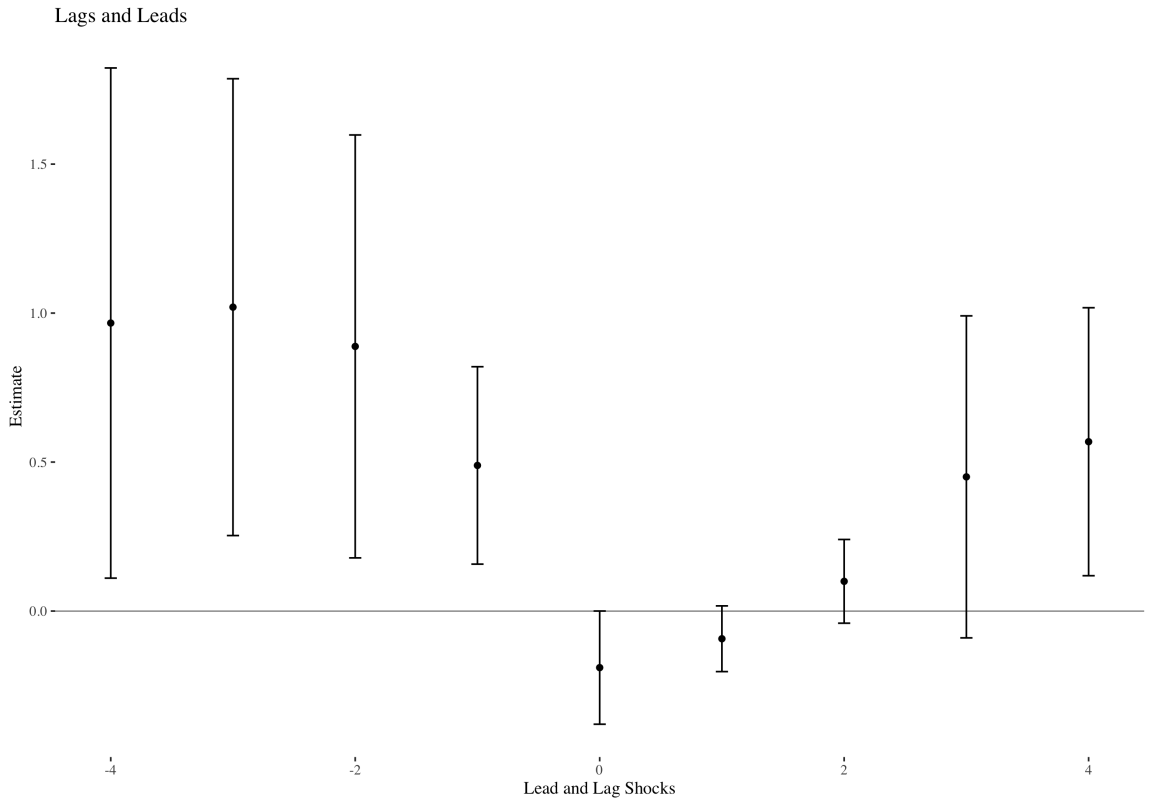
[1] Event study estimates for crop trends in Appendix C.5

The regression takes the following form:

$$ihs\ \text{Conflict}_{it} = \sum_{k=1}^{4} \delta_k(\Delta\text{Price}_{t+k} \times \text{Palm Share}_i) + \beta_j \sum_{j=0}^{5} (\Delta\text{Price}_{t-j} \times \text{Palm Share}_i) + \mu_i + \lambda_t + \varepsilon_{it}$$

Causes ought to precede consequences, therefore the estimates for δ_k should be close to zero whereas estimates for $\beta_j > 0$. I show results in Figure B.7. Future prices have a very close to zero relationship with conflict. Although 4 years after conflict a positive coefficient is detected, the magnitude of the estimate is smaller than the lagged shocks, and the near-zero estimates for the more immediate leads provide evidence against a systematic pattern of higher conflict preceding higher prices. Meanwhile, I find the shock variable is positively related to conflict and statistically different than zero for all four lags.

Figure B.7: Price Shock Dynamics



Note: Model include year and district fixed effects. Robust errors clustered at district.

B.3.9 District Splits

Aggregating districts that split back to their parent district ought to adjust for the possibility that splits overtime and space result in resource conflicts. In order for splits overtime to bias the result, the timing and location of a split would need to be correlated with international prices and pre-sample production shares net of year and district fixed effects. Given the exogeneity of global prices and the (likely) time-invariant influence of production shares on the probability of a district splitting in a given year, it is unlikely that changes in boundaries that occur overtime are driving the result.

I account for this possibility by constructing a binary indicator Split_{it} =(1 Year $\geq$ Year Split). If a 1995 district splits, the variable takes on one the year of the split and thereafter, but zero otherwise. Table B.8 shows the main results are unchanged when including this covariate in the regression.

Table B.8: Adjusting for District Splits

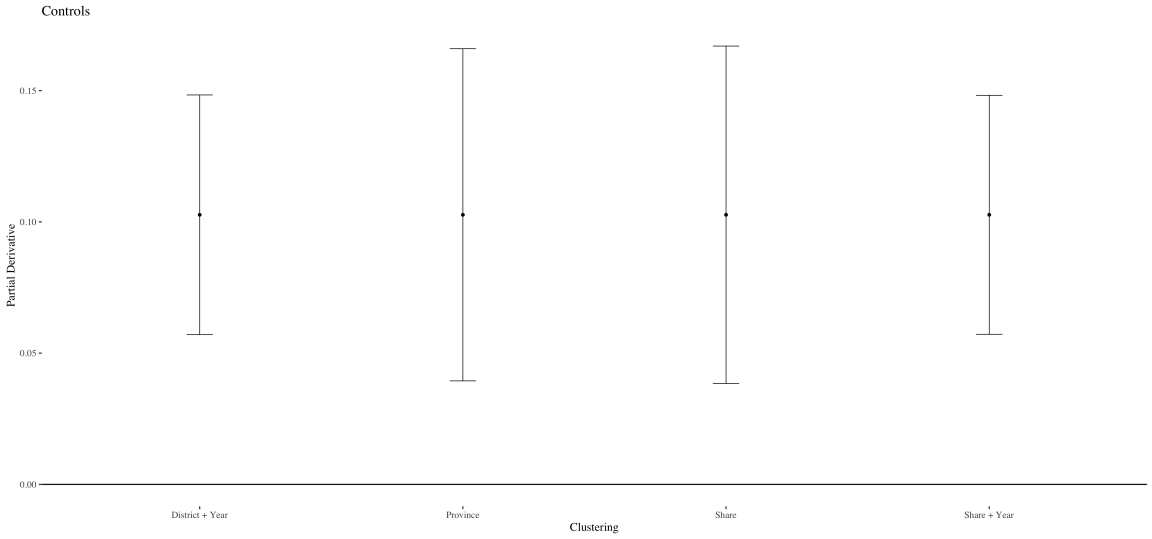
	(1)	(2)	(3)	(4)
Outcome: Resource Conflict (IHS)				
Shock	0.08^{***}	0.12^{***}	0.11^{**}	0.10^{**}
	(0.02)	(0.03)	(0.04)	(0.03)
1 Split(Year $\geq$ Year Split)	-0.12	0.02	0.01	-0.00
	(0.09)	(0.09)	(0.12)	(0.14)
Mean IHS(Resource Conflict)	1.172	1.172	1.172	1.172
Mean Production Share	0.008	0.008	0.008	0.008
District & Year FE	✓	✓	✓	✓
Province × Year FE	-	✓	✓	✓
Terrain × Year FE	-	-	✓	✓
Full Controls	-	-	-	✓
N. Clusters	156	156	156	156
Num. obs.	1560	1560	1560	1560

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Note: Robust standard errors clustered at district reported in parenthesis.

B.3.10 Alternative Standard Error Construction

Figure B.8: Alternative Standard Errors for Resource Conflict



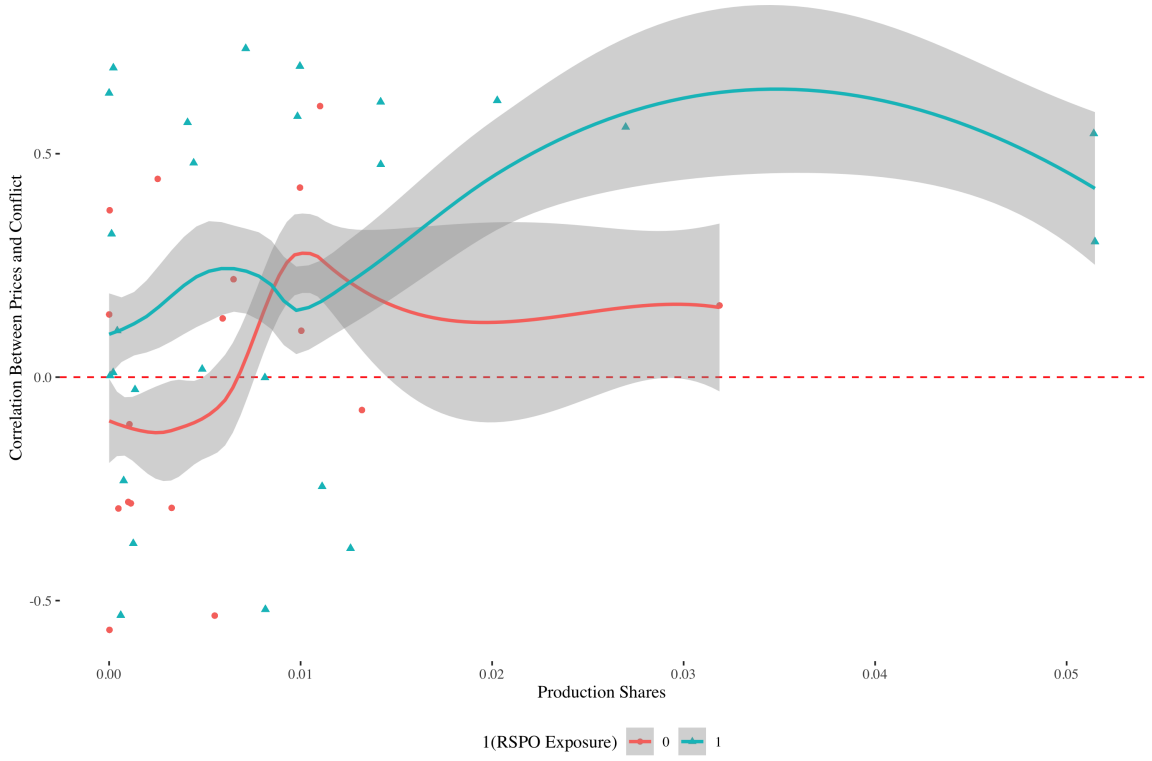
Note: Panel A shows standard errors adjusted from a regression with only two way fixed effects (corresponding to Table 1 Panel A Column (1)) and Panel B shows the adjustment for a regression including all controls (province by year fixed effects, year fixed effects interacted with terrain ruggedness, district area, forest density, share of agricultural GDP in 2000.

B.4 Heterogenous Effects Robustness

B.4.1 Descriptive Relationship

Figure B.9 plots the relationship between the price-conflict correlation among producing districts and plots it against oil palm market shares separately for districts with any RSPO exposure and those with 0 RSPO exposure. As can be seen, the slope for non-exposed districts is much steeper as oil palm market shares increase up to the .01 range. Some extreme values on the far right of the plot show a positive association between RSPO exposure and market share. Note these extreme values bias results against the hypothesized negative coefficient for the interaction of shocks and RSPO intensity. In SI B.12, I show results remain largely the same when subsetting the sample to exclude the few large producers, although with less power some results are more statistically imprecise.

Figure B.9: LOESS Fit of RSPO x Price Shocks and the Price-Conflict Correlation Among Producers



Note: Figure plots the correlation between prices and conflict among producers against market share, and shows the nonparametic relationship between shares and the conflict-price correlation for districts with any RSPO intensity versus districts without any RSPO intensity.

B.4.2 Adjusting for Mill Trends

Table B.9: Heterogenous Effects: Adjusting for RSPO x Price Trends

Outcome: Resource Conflict (IHS)	(1)	(2)	(3)	(4)
Shock	0.12^{***}	0.18^{***}	0.17^{***}	0.16^{***}
	(0.03)	(0.03)	(0.03)	(0.03)
Shock x RSPO Mill Centrality	-0.18^{*}	-0.28^{***}	-0.26^{***}	-0.26^{***}
	(0.07)	(0.07)	(0.07)	(0.07)
Price x RSPO Mill Centrality	0.12	0.22	0.13	0.20
	(0.17)	(0.23)	(0.23)	(0.24)
District & Year FE	✓	✓	✓	✓
Province × Year FE	-	✓	✓	✓
Terrain × Year FE	-	-	✓	✓
Full Controls	-	-	-	✓
N. Clusters	156	156	156	156
Num. obs.	1560	1560	1560	1560

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Note: Robust standard errors clustered at district reported in parenthesis.

B.4.3 Subsetting to Producers

Table B.10: Heterogenous Effects Robustness: Subsetting to Only Producers

Outcome: Resource Conflict (IHS)	(1)	(2)	(3)	(4)
Panel A: Baseline				
Shock	0.09**	0.17***	0.16***	0.15**
	(0.03)	(0.04)	(0.04)	(0.04)
Shock x RSPO Mill Centrality	−0.13*	−0.21*	−0.23*	−0.23*
	(0.06)	(0.09)	(0.09)	(0.10)
Panel B: Adjusting for Price x RSPO				
Shock	0.09**	0.17***	0.16***	0.15***
	(0.03)	(0.04)	(0.04)	(0.05)
Shock x RSPO Centrality	−0.13	−0.29**	−0.26*	−0.25*
	(0.08)	(0.09)	(0.11)	(0.11)
Price x RSPO Centrality	0.02	0.37	0.15	0.10
	(0.31)	(0.37)	(0.46)	(0.48)
District & Year FE	✓	✓	✓	✓
Province × Year FE	-	✓	✓	✓
Terrain × Year FE	-	-	✓	✓
Full Controls	-	-	-	✓
Num. obs.	430	430	430	430
N. Clusters	43	43	43	43

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

B.4.4 Non-Certified Mill Placebo

Table B.11: Heterogenous Effects Placebo Test: Non-Certified Mill Centrality

	(1)	(2)	(3)	(4)
Outcome: Resource Conflict (IHS)				
Shock	0.10***	0.12***	0.11**	0.10**
	(0.03)	(0.03)	(0.04)	(0.04)
Shock x Non-RSPO Mill Centrality	−0.03	0.02	0.02	0.01
	(0.04)	(0.05)	(0.05)	(0.05)
District & Year FE	✓	✓	✓	✓
Province × Year FE	-	✓	✓	✓
Terrain × Year FE	-	-	✓	✓
Full Controls	-	-	-	✓
N. Clusters	156	156	156	156
Num. obs.	1560	1560	1560	1560

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

Note: Robust standard errors clustered at district reported in parenthesis.

B.4.5 Dropping Top Producers

Table B.12: Dropping Top Producers for Interaction

	(1)	(2)	(3)	(4)
Shock	0.22^{*}	0.27^{***}	0.25^{**}	0.25^{**}
	(0.10)	(0.08)	(0.08)	(0.09)
Shock x RSPO	-0.49	$-0.60^{\dagger}$	-0.52	$-0.65^{\dagger}$
	(0.37)	(0.34)	(0.34)	(0.36)
District & Year FE	✓	✓	✓	✓
Province × Year FE	-	✓	✓	✓
Terrain × Year FE	-	-	✓	✓
Full Controls	-	-	-	✓
Num. obs.	1510	1510	1510	1510
N Clusters	151	151	151	151

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{\dagger}p < 0.1$

Table B.13: Economic Change on Palm Suitability

Δ Outcomes:	(1) Poverty	(2) ln(GDPPC)	(3) ln(Revenue PC)	(4) Unemp. Rate	(5) ln(Population)
Palm Suitability	−0.02	−0.03	−0.03	0.01	−0.03
	(0.02)	(0.05)	(0.13)	(0.01)	(0.02)
Num. obs.	156	156	142	156	156
Province Fixed Effects	✓	✓	✓	✓	✓
Controls?	✓	✓	✓	✓	✓

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

B.5 Long Differences Robustness

B.5.1 IV Balance Tests

Table B.13 shows results using changes in population, GDP, revenue, and employment rates from their baseline. Table B.14 shows conflict changes using identity, popular justice, law enforcement, criminal, governance, and election violence.

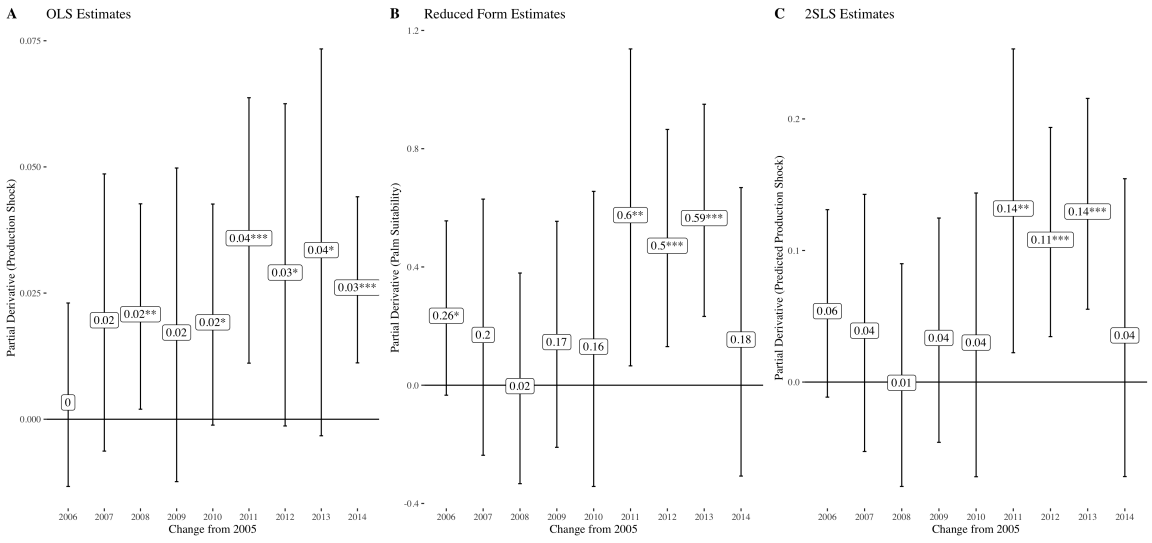
Table B.14: Balance Tests: Conflict

Δ Other Conflicts:	(1) Gov.	(2) Elec.	(3) Pop. Justice	(4) Ethnic	(5) Law	(6) Crime	(7) Separatism
Palm Suitability	−0.02	0.01	0.08	−0.01	0.02	−0.05	−0.11
	(0.12)	(0.12)	(0.16)	(0.12)	(0.12)	(0.12)	(0.07)
Num. obs.	156	156	156	156	156	156	156
Province Fixed Effects	✓	✓	✓	✓	✓	✓	✓
Controls?	✓	✓	✓	✓	✓	✓	✓

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$

B.5.2 SHAC Errors

Figure B.10: Long Differences: Conflict Change on Palm Production Change (Conely Errors)



Outcome is the first difference of the inverse hyperbolic sine of resource conflicts across different reference years from 2005 (for example, 2012 refers to $\text{Conflict}_{i,2012} - \text{Conflict}_{i,2005}$). Panel A shows the reduced form where palm oil suitability is the regressor of interest. Panel B shows the 2SLS regression where change in production from 2010-2004 measured as the proportion of district area devoted to palm oil production is the endogenous variable. Points represent estimates of the partial derivative and bands represent 95% confidence intervals. Conely standard errors constructed to allow 100 kilometers of spatial autocorrelation.

Appendix C

Appendix 3: Colonial Origins of Insecurity

C.1 Additional Historical Context

C.1.1 Background on Brazilian Police

Brazil is a federal republic, having both federal and state police. Federal police control issues of national interest such as immigration and terrorism, and maintain control over indigenous communities. State governments control the majority of policing, with local policing broken into civil and military police organizations. The civilian police are primarily responsible for investigating and prosecuting crimes, whereas military police are tasked with patrolling and maintaining order at the street level. The military police is highly hierarchal in terms of organization and culture, whereas civil police organizations are vertically organized but allow for socialization across ranks more freely [104, p. 48-50]. States have both a chief of civil police and a commander of the military police, along with a secretary of public security [53].

The civil and military police first formed in 1808 and 1809 when the Portuguese imported their own policing institutions modeled after the French; prior to the formal

police force, town councils had organized armed guards [101]. Policing during this period was controlled at the local level. After independence and during the liberal reform period under Pedro I, the 1832 Code of Criminal Procedure established an elected post of justice of the peace at the parish level [78].The reform of the procedural code of 1841 created a policing hierarchy, with chiefs at the province level. Police chiefs were named by the minister of justice, and selected delegados and subdelegados to serve at the municipality and below. The local police had effective judicial functions, since they could charge and prosecute crimes. The central government retained ultimate authority under the empire, however, chiefs at the province level had nominal influence and existed atop the police hierarchy within their respective province [78].

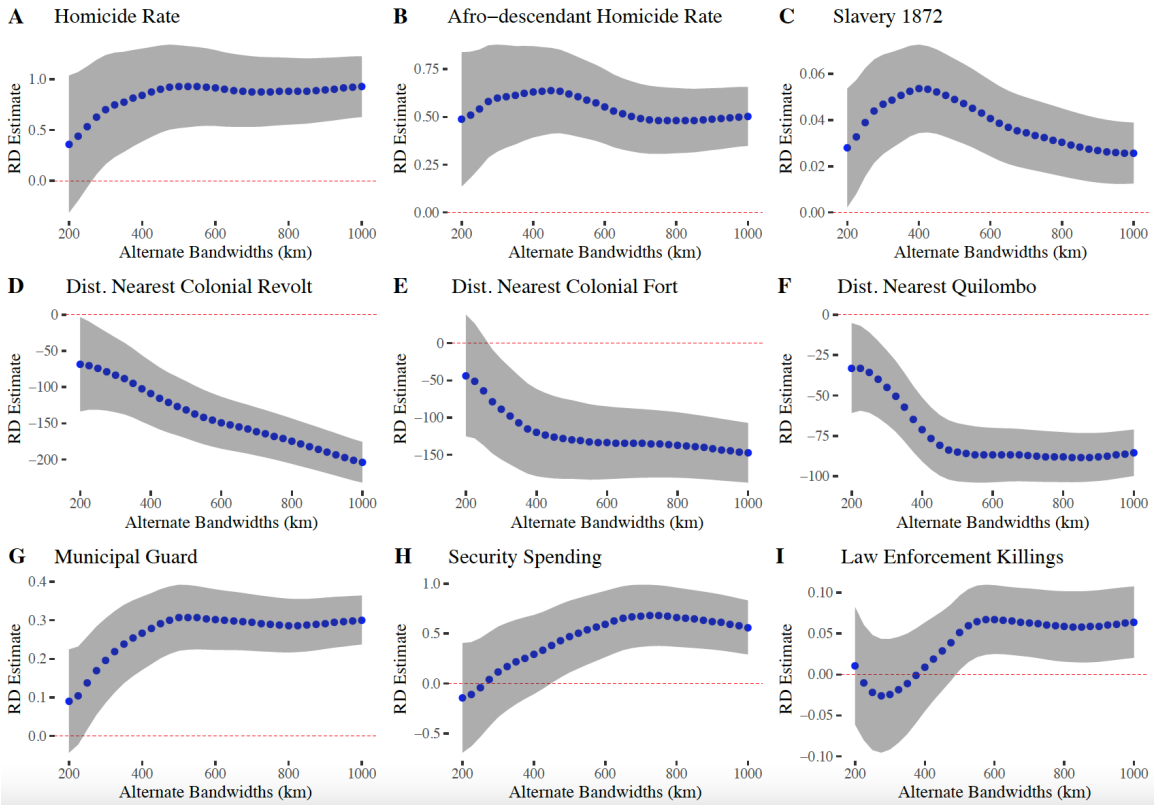
Until 1871, judge and police were considered one in the same. After a reform and thereafter, police investigations follow a similar procedure to other civil law countries, where police collect information through a non-adversarial process regarding a crime, which gives a large degree of control to civil police in the investigation and prosecution of crime relative to the accused. The lack of competition in the investigation process enables brutality, as [53] argues: "[t]he Code of Criminal Procedure accepts confession as a form of evidence without placing greater restrictions on its use, in effect making it an incentive for the use of force." [53]. Police conduct manuals are largely inherited from the military, which mostly instruct officers on their behavior within the organization rather than proper relationships between police and citizens [53].

Policing was federalized during the military dictatorship, but power was returned to the states in 1982 [53]. Although policing is formally controlled by state governments, state police provides a floor rather than a ceiling, as municipalities can invest more in their own preventive policing independent of state police via municipal guards [53]. While not formally police recognized by the constitution, municipal guards serve

auxiliary law enforcement functions assisting in patrol and protection of public space. They can coordinate their activity with the military police in patrols, for example, working alongside one another in a municipality to control crime around bars at night [39].

C.2 Univariate Running Variable

Figure C.1: RD Results at Alternative Bandwidths



Note:

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