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Violence and black markets: Evidence from the Niger Delta conflict*

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Abstract

We use original data on the locations of militant commanders, attacks on the petroleum industry, and oil theft to show that a 2009 amnesty concluding the Niger Delta oil conflict led to sustained declines in militant activity and growth in oil theft. To explain post-conflict resource theft, we propose that a state may allow black markets as a tacit rent-sharing mechanism. Using a no-commitment bargaining model, we show that when faced with militarily powerful rebels in locations with low costs of illicit activity, the state prefers to recover some surplus through bribes over paying the cost of credibly deterring black markets. We find that post-conflict oil theft is elevated and government enforcement against black markets is muted in these areas, with heterogeneous effects and nonlinear patterns consistent with the model mechanisms. Our analysis highlights how local economic conditions and relative military capabilities jointly shape incentives for resource theft.

JEL codes: K42, Q34, O17

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1 Introduction

Clandestine resource markets are a common feature of conflict-affected countries; across the world, black markets for stolen petroleum, gold, rare minerals, precious stones, illicit drugs, and forestry resources thrive in fragile states.[1] Black markets impose substantial efficiency costs – government revenue losses, environmental damage, violence, and corruption that discourages legitimate investment. Why do such shadow economies persist? Governments may lack the capacity to enforce the law in post-conflict contexts (Besley and Persson 2010), or they may face agency problems, resulting in low-level corruption that sustains illegal activity (Banerjee 1997). This paper argues instead that the state may tacitly allow black markets as an optimal strategy in a no-commitment environment (Acemoglu 2003), to serve as a *de facto* rent-sharing mechanism between ruling elites and rebel challengers. We propose that the black market equilibrium is sustained by the threat of violence and favorable cost conditions, under which it is preferred by the state to direct income transfers and costly deterrence.

To that end, we study the emergence of black markets following the Niger Delta conflict, a 2005-2009 insurgency by militant groups targeting the oil industry in Nigeria's oil-rich south. In 2009, a peace agreement (hereafter, the amnesty) provided legal immunity to combatants – and sizable private payoffs to some rebel leaders – in exchange for halting attacks on oil infrastructure. Despite success in reducing violence, the Niger Delta nonetheless experienced substantial growth in oil theft following the amnesty, leading some observers to question whether Nigeria's rulers were creating perverse incentives for rebels (Economist 2016).

In this context, we collect detailed data on the location, amnesty status, and alliance network of militant commanders, in addition to attacks on the petroleum sector, crude oil production, pipeline theft, and state (interchangeably, government) law enforcement actions against black markets in the Niger Delta. We use difference-in-differences (DD) models to show that the amnesty coincided with an immediate and persistent drop in violence targeting the oil sector, but was followed by sustained growth in oil theft concentrated in locations under the control of amnestied rebels, more than doubling the pre-amnesty control group mean.

[1] The value of global oil theft alone has been estimated to be in the region of 100 billion U.S. dollars per year (Ralby 2018).

The rise of the black market was heterogeneous – in locations controlled by commanders awarded with lucrative "pipeline security contracts," the increase in oil theft was muted compared to locations where commanders were not awarded a security contract. We find no evidence that this effect is driven by increased government enforcement in contracted areas after conflict. Importantly, these contracts did not involve actual enforcement on the part of demobilized militants, and were in reality vehicles to transfer rents from the government to rebel leaders.[2,3] The motivating evidence raises the question of why security contracts, *prima facie* effective at deterring oil theft, were not awarded more broadly. Clearly, security contract receipt is an endogenous outcome of the strategic interaction between rebel and government.

We formally study the government's decision to provide incentives against oil theft in a repeated game of conflict bargaining. The game proceeds as follows. In each period the state proposes a division of oil rents between itself and the rebel. Rebels may reject the deal, yielding a period of conflict, or accept it, and then choose whether or not to steal oil. We assume that rebels are a dominant firm facing a competitive fringe in the black market for stolen oil.[4] The government interacts strategically only with groups that have sufficient military strength (rebels), preferring to curtail the activity of militarily weaker noncombatant groups (fringe) in the black market through raids, arrests, and other enforcement actions. When rebels do not steal oil the competitive fringe increases its theft proportionally to local cost conditions, to which the government responds with increased enforcement. The total reduction in oil theft induced by providing incentives to rebels thus depends on the endogenous response of the fringe.

To select an equilibrium and form empirical predictions, we reason that the salient obstacle to deterring oil theft in a no-commitment environment is for the

[2]Stakeholder Democracy (2019c), a local NGO, write that "[the] concept of pipeline surveillance contracts in the Niger Delta is a misnomer. They [are] used as a disguised [vehicle] for channeling funds from government [...] to ex-militants [...] in exchange for these contracts, beneficiaries are supposed to refrain from engaging [in activity] that disrupts oil and gas extraction [...]" See also Ebiede (2017) and Ebiede, Langer, and Tosun (2020).

[3]In an illustration of the sums transferred under these security contracts, Ebiede (2017) reports that Government "Tompolo" Ekpemupolo, a leading militant commander and security contract recipient, received 7.5 million U.S dollars per month. The World Peace Foundation (2018) reports that Tompolo used his funds in 2012 to purchase, among others, six *Hauk*-class motor torpedo boats and a large support craft formerly of the Norwegian navy for some 12 million U.S dollars.

[4]See e.g. Ebiede (2017), Stakeholder Democracy (2019a) and Stakeholder Democracy (2019b).

government to establish a credible threat of costly off-path punishment. We consider equilibria where the rebel rejects inferior offers and continues with oil theft until punished. To sustain an equilibrium without oil theft the government must respond to oil theft with some duration of fighting.[5] We propose that in a stationary equilibrium, the threat of punishment is credible when the opportunity cost of fighting is low relative to anticipated future losses from oil theft. We say an equilibrium is credibly enforced when the government's incentive condition during punishment is satisfied for any discount factor and arbitrarily long punishments. The government's opportunity cost of fighting over allowing oil theft decreases in the equilibrium offer, thus, credible enforcement induces a lower bound on the transfer that is increasing in the net cost of fighting less efficiency losses sustained under oil theft. In equilibrium, militarily weak rebels are credibly enforced by their (modest) reservation payoff while militarily strong rebels may receive a sizable rent. When war is more costly, only large transfers can render credible the government's promise to punish theft with fighting.

The key implication is that when the government faces rebels of high military strength in locations with favorable black-market conditions, there can exist equilibria in which the government tacitly allows oil theft that yield a greater payoff than any credibly enforced equilibrium without oil theft. The main prediction of our model that we take to the data is that local military capability and black-market cost conditions combine to induce, across locations, a discontinuous change in the government's incentive provision to rebels and enforcement of oil theft. This discrete shift induces five testable relationships between military capability, local cost conditions, government enforcement effort, and oil theft. We test and find support for these predictions.

The model predicts *i*) that aggregate oil theft decreases and increases in local black-market cost conditions and rebel military capability, respectively. Lower costs increase fringe entry, reducing the government's incentive to provide rebels with incentives against theft, while greater rebel strength demands more generous offers to maintain a credible threat of punishment, increasing the government's cost of credibly providing incentives. We measure the cost of oil theft using the median wages of young men in the local labor market and distance to the Atlantic

[5] Hence we exclude strategies where the government punishes by allowing oil theft and the rebel transfers surplus back to the government.

coast and Niger River. Employing a triple-difference (TD) specification that interacts amnesty receipt with cost factors we find that growth in the black market is concentrated in low-wage areas near the coast. Military strength is measured by local alliance density. We further find that an additional local ally increases the effect of amnesty on oil theft by between 30.2-103.5% of the baseline effect.

In credibly enforced equilibria, rebel military capability and local black-market costs interact positively in determining the cost of incentive provision because the anticipated fringe response increases the opportunity cost of fighting over allowing oil theft. We therefore expect *ii*) the marginal effect of rebel military strength on oil theft to decrease in local costs. Employing quadruple-difference estimation, we find that the role of military strength diminishes as local costs rise. Furthermore our proposed equilibrium-switching mechanism implies that the independent heterogeneous effects of costs and military strength on oil theft should be nonlinear. We employ a kernel regression to show that quantitatively, the effect of amnesty on oil theft at the 5th percentile of the wage distribution is 2.5 times larger than at the median, after which it falls to zero. Similarly, the average heterogeneous effect of military strength is driven primarily by the most powerful rebels in our sample.

The results provide robust support for our prediction that military capability and local conditions interact to jointly determine oil theft levels, with evidence of non-linearity suggesting the existence of a local threshold around which the state's optimal policy shifts from costly incentive provision to tacit acceptance of oil theft. To provide further evidence of our equilibrium-switching mechanism we turn to our data on law enforcement activity against oil theft.

Our model predicts that the government may selectively opt not to enforce oil theft in low-cost locations, because allowing rebel theft restrains an active fringe. We therefore expect that *iii*) enforcement effort against oil theft to be concave and even non-monotonic in local wages. In contrast, absent an equilibrium-switching mechanism we would expect the government to target low-cost oil theft hotspots with greater enforcement intensity. Employing a kernel regression, we find exactly this inverse-U pattern, with oil theft enforcement roughly 2 times greater at median wages than the 5th- and 95th percentiles. This nonlinearity is absent for non-oil crime enforcement, which decreases monotonically in local wages.

Security contracts offer a direct test of the discontinuous change in govern-

ment's enforcement of oil theft. We interpret the receipt of a security contract as a sufficient condition for government incentive provision. Among non-recipients there may be both militarily weak rebels who are credibly enforced at low payoffs and militarily powerful rebels who are tacitly allowed to steal. In the former case, the fringe enters and enforcement is required, while in the latter case rebel theft displaces the fringe. In contrast, among explicitly contracted rebels variation in enforcement intensity arises only through local costs via fringe activity, not military strength. We therefore *iv*) expect enforcement to fall in military capability for non-contracted rebels but be uncorrelated otherwise. We disaggregate the sample between contracted and non-contracted locations to find support for exactly this pattern, again suggesting rebel military capability is a crucial factor in determining the government's decision to provide incentives and enforcement against oil theft.

Finally we exploit reports from field research which find that rebels predominate the export of stolen crude, while smaller fringe groups are active mostly in the illegal refining sector serving the domestic market. Our data allow us to differentiate between enforcement actions against illegal refineries and crude export. Thus *v*) enforcement targeting *refining* operations in particular should increase differentially in contracted areas as incumbent rebels exit and the fringe enters. Disaggregating enforcement actions by type we find that raids on illegal refineries differentially increase by 220% in contracted locations relative to non-contracted, providing direct support for our entry mechanism. In contrast, we observe null or small negative effects of security contracts on other types of oil theft enforcement.

Our paper contributes to literatures on the political economy of black markets, violence, peace agreements, and resource conflicts. A growing literature has studied the production and trafficking of narcotics in Colombia, Mexico and Afghanistan, providing evidence showing that relative prices create incentives to join the drug trade (Angrist and Kugler 2008; Dube, Garcia-Ponce, and Thom 2016; Castillo, Mejia, and Restrepo 2020) and that government policy shapes incentives for violent competition between drug traffickers (Dell 2015).[6] Along with Saavedra and Romero (2021), we extend this literature to the study of illegal natural resource extraction. Importantly, the existing literature has focused largely on black

[6]See also Melnikov, Schmidt-Padilla, and Sviatschi (2020) and Brown et al. (2021) on the economic consequences of competition between street gangs in El Salvador and Gehring, Langlotz, and Stefan (2019) on the drug trade in Afghanistan.

market responses to exogenous changes in relative prices or government policy. In contrast, our paper leverages a detailed bargaining framework and rich data on all equilibrium outcomes to characterize the government's choice to tacitly share rents through the black market. We therefore present a novel mechanism for the endogenous persistence of black markets, emphasizing the joint role of limited commitment and local market conditions in shaping government enforcement choices.

A related literature has studied theoretically and empirically how armed groups employ stick-and-carrot (*plata-o-plomo*) strategies to manipulate voters and elected politicians.[7] This literature has amassed empirical evidence showing that political violence is directed towards pivotal targets (Alesina, Piccolo, and Pinotti 2019), distorts the incentives of elected officials (Acemoglu, Robinson, and Santos 2013), reduces public good provision (Pinotti 2015), and will generally yield inefficient rent-sharing between elite groups. We extend this literature to a novel context, leveraging micro-level variation in the key underlying features of the government's strategic environment – rebel military strength and local economic conditions. In doing so, we link this literature on strategic political violence and inefficient rent-sharing with the literature on the economics of black markets.

We also contribute to the burgeoning empirical literature studying the formation and effects of agreements ending political violence. Francois, Rainer, and Trebbi (2014) study ethnic power-sharing in African governments. Konig et al. (2017) highlight the importance of alliance networks among combatants in structuring the incentives for peace. Dancy (2018) studies the effectiveness of legal amnesties in peacemaking. These contributions highlight, as we do, the key role played by credible threats of violence for enforcing political bargains. Finally, by showing how black markets serve as a rent-sharing vehicle we also contribute to an extensive literature that links the value of natural resources to civil conflict at national and subnational levels (Berman et al. 2017; McGuirk and Burke 2020).[8]

The paper proceeds as follows. Section 2 provides background information on the Niger Delta conflict, black market, and amnesty. Our data sources and mea-

[7]See Dal Bó and Di Tella (2003), E. D. Bó, P. D. Bó, and Di Tella (2006), North, Wallis, Weingast, et al. (2009), and Cox, North, and Weingast (2019).

[8]This literature is reviewed in Nillesen and Bulte (2014) and Ross (2015). See also Nwokolo (2018), Fetzer and Kyburz (2019), and Sánchez De La Sierra (2020). See also a literature studying the resource curse, e.g. Grossman (1999), Ross (1999) Acemoglu, Verdier, and Robinson (2004), Hodler (2006), Mehlum, Moene, and Torvik (2006), and Robinson, Torvik, and Verdier (2006).

surement assumptions are presented in Section 3, with additional details available in Supplementary Information (SI) C. Section 4 provides evidence on the relationship between security contracts, enforcement and oil theft, facts that motivate our model in Section 5. We test for predicted relationships between local cost conditions, rebel military capability, government incentive provision, enforcement and oil theft in Section 6. Plausible alternative explanations to our mechanism are considered in Section 7. We conclude with a discussion of avenues for future research in Section 8. Robustness tests and additional empirical results are referenced throughout and available in Online Appendix (OA) B and SI D.

2 Background

We present background information the Niger Delta conflict, the amnesty, and the black market for stolen oil.

2.1 The Niger Delta conflict and amnesty

The Niger Delta is Nigeria's oil-rich southern region, responsible for all of the country's 2.3 million barrels per day of oil output.[9] The region has been affected by a decades-long, militarized struggle over the distribution of oil revenues between the federal military and a vast proliferation of local militant groups (Obi and Rustad 2011). The militias originated from university fraternities, street gangs, and oil thieves. Their strength and capacity for violence was developed by political corruption, competition over resource income, and sympathy among the local population (Asuni 2009b). Initially, militants operated independently from camps deep in the mangrove forests of the coastal Niger Delta. In 2005, the Movement for Emancipation of the Niger Delta (MEND) united key rebel commanders in demanding a greater share of oil wealth and organizing systematic attacks on the

[9]The major share of profits in the Nigerian oil industry are captured by the federal government. Petroleum extraction in Nigeria is financially organized around both licensing and production sharing contraction contracts (PSC). Licensed oil producers pay an 85% tax rate on assessable profits per the 2004 Petroleum Profit Tax Act. Under a PSC between the Nigerian National Petroleum Company (NNPC) and multinational oil companies (Shell, Chevron, Exxon Mobil, Agip and Total) the income is first shared according to a predetermined split and the participants' profits are subsequently taxed at 50%. See EY (2019) for more details.

sector, including abductions of foreign oil workers, pipeline bombings, and armed engagement with federal military forces. MEND's entry led to a period of escalation in militant activity, most notably the 2008 Bonga offshore platform attack, which took 10% of Nigerian oil production offline in a single day.[10] Annual Nigerian oil output fell by 15% during the peak conflict years of 2005-2009, reducing state revenue by billions of US dollars.

In July of 2009, the Nigerian federal government announced that members of Niger Delta militant groups would receive amnesty in return for ostensibly disarming and demobilizing.[11] The amnesty covered roughly 30,000 combatants, 86% of whom came from the core Niger Delta states of Delta, Bayelsa, and Rivers. The fighters received cash stipends of approximately 400 U.S. dollars per month (four times the minimum wage) in addition to scholarships and vocational training.

Amnesty was not universally applied. In our sample of 41 militant commanders, 18 did not receive the amnesty. Of these, 7 were defeated in battle while the remaining 11 either were not included, rejected amnesty, or are of unknown status. In 2012, additional payments were made to 7 militant commanders in the form of "pipeline security contracts" that were in reality vehicles for transferring rents to militant-owned contracting firms (Stakeholder Democracy 2019c). The annual costs of the amnesty program have fluctuated between 300 and 500 million U.S. dollars per year (Ebiede, Langer, and Tosun 2020), between 1 and 2% of the total federal budget and amounting to 20% of total military spending.[12]

2.2 The black market for stolen oil

Theft of crude oil for sale on the black market is a longstanding feature of Nigeria's petroleum sector. The region's five thousand kilometer network of onshore oil pipelines traverses vast militant-controlled swamps. Thieves cut into the pipeline using hacksaws and siphon oil to a nearby barge. Stolen oil is then sold either to small-scale artisanal refineries for processing and sale on the local market, or to oil

[10] While ethnic cleavages exist in the Niger Delta, these do not extend systematically to the citizen's relationship with the federal state or oil companies (Asuni 2009a, Ebiede 2017).

[11] Notably, few weapons were handed in and the payments were disbursed by the commanders. See e.g. Ebiede (2017) and Ebiede, Langer, and Tosun (2020).

[12] In contrast, total corporate social responsibility (CSR) spending in the oil sector amounted to 116 million U.S. dollars for 2016.

tankers in nearby offshore waters for export. Alleged collusion between security forces, sub-contractors for oil companies, and local government allows the black market to function relatively unfettered (Stakeholder Democracy 2019b). Due to a lack of accurate production data the black market's size is uncertain; in 2016, theft losses were estimated at 4.2 billion US dollars (NEITI 2016).

Militant groups play an important role in this market. Before amnesty, oil theft emerged among militant groups in order to finance their military endeavors (M. Watts 2007). Post-amnesty, some ex-militants left the market completely while others act as patrons to new gangs who wish to tap in areas still under their influence or participate in oil theft directly. The market for stolen oil has a two-tiered structure, with larger suppliers tapping and exporting oil in crude form while smaller players sell to and operating local illegal refineries (Katsouris and Sayne 2013, Stakeholder Democracy 2019a, and Stakeholder Democracy 2019c).

The black market has grown substantially in size during the post-conflict period, likely due to dwindling fuel subsidies and a chronic shortages of refined products.[13] In response, the black market for stolen oil pivoted towards supplying fuel for domestic consumption, reaping considerable productivity gains. By 2017, 75% of black market sales were locally refined (Stakeholder Democracy 2019b).

3 Data and measurement

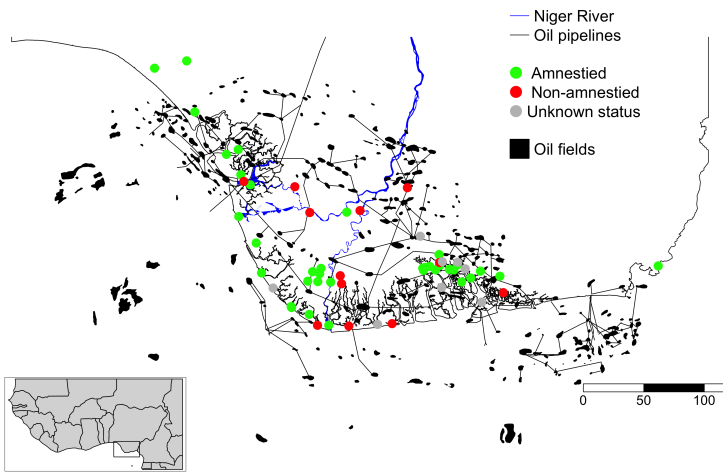
We present our data sources, variable construction, and summary statistics. A detailed exposition of our measures and discussion of sources, potential measurement error, and robustness to arbitrary choices can be found in SI C.

3.1 Data sources

Oil production and infrastructure: Data on oil output comes from Rystad Energy's UCube database. These data contain field-level monthly production quantities for 346 oil fields in Nigeria from 1995-2018. Data on the geographic location

[13]The decrease in federal fuel subsidies, implemented January 2012, increased the pump price of refined products by 40%. Despite producing some 2-2.5 million barrels of crude oil per day, a lack of domestic refining capacity means Nigeria must meet 80% of its fuel requirements through imports (Clements et al. 2013). Mismanagement, corruption, a lack of foreign currency, and chronic payment arrears hamper imports of refined products.

Figure 1: Map of the Niger Delta



Note: This figure shows the geography of amnesty, oil production, and militant activity in the Niger Delta. The southern coastline of the Niger Delta is outlined in black, overlaid with the locations of oil fields, pipelines, and the Niger River. Points indicate the locations of militant camps in our sample, color-coded by their amnesty status. The map inset indicates the location of the Niger Delta on the coast of West Africa.

of oil and gas infrastructure comes from the Department of Petroleum Resources, made available to the authors by that agency, and Google Maps. The region contains an oil pipeline network covering 4,284 km. Figure 1 maps the region's oil infrastructure, along with the locations of militant camps and amnesty status.

Militant attacks: We use the Armed Conflict Location Event Dataset (ACLED, Raleigh et al. 2010) to measure attacks on the oil sector perpetrated by militant groups, e.g. bombings of major oil infrastructure, kidnappings and killings of oil workers, and clashes with the Nigerian military. To identify all such events from 1997-2017, we conduct keyword searches, and further subset events containing the keywords to include only attacks perpetrated by political militias or rebel groups.

Militant camps and amnesty: Data on rebel commanders was collected by the authors from several sources. In 2018, we visited Warri, Delta State, one of the epicenters of the Niger Delta crisis. We first collated a list of militant commanders from previous qualitative work on the program (Ugwu and Oben 2010, Ojakorotu and Dodd Gilbert 2010) and then consulted with AA Peaceworks (AAPW),

a highly informed local non-profit organization. For each militant commander, AAPW provided the following information: *i*) the group that this commander was affiliated with, *ii*) the location of their camp(s), usually denoted by the exact creek or a nearby village, and *iii*) whether they accepted amnesty. We supplement this dataset with a list of pre-amnesty militant camps collected in a similar exercise by Blair and Imai (2013). Gaps in the data and verification of accuracy were addressed by consulting Nigerian newspapers.

Through searches of local Nigerian newspapers and Stakeholder Democracy (2021), we further identify 7 militant commanders on our list who received government contracts to perform security services in the oil sector. We then define three outcomes at the militant camp level: *i*) if the camp commander received any amnesty at all, *ii*) if the camp commander additionally received a pipeline security contract, and *iii*) if the camp commander was defeated in battle. Table 1 provides a summary of the counts in our camp-level data. In total, we identify 69 militant camps, belonging to 41 unique commanders. Of these, 47 camps (23 commanders) are amnestied, 13 camps (11 commanders) are not amnestied, and 9 camps (7 commanders). are of unknown amnesty status. Among the amnestied camps, just over half are controlled by a commander receiving a contract. The reasons for non-amnesty are either that the camp was defeated in battle before amnesty (6 camps)[14] or did not accept the amnesty (7 camps).[15]

For our main analysis, we drop the 9 camps for which amnesty status cannot be verified by our sources.[16] We further drop 2 camps, coded as non-amnestied, where amnesty status does not apply, yielding 58 camps led by 32 unique commanders.[17,18] For each remaining camp, we calculate the distance between that camp and our sample of 9185 Niger Delta villages. A village is considered amnestied if it falls within a 30 kilometer radius of a camp receiving amnesty.[19] Villages are

[14]Note, however, that 12 of the 13 non-amnestied camps were ultimately defeated in battle.

[15]Among this group, we code 3 camps that formally received amnesty, but continued operating, as non-amnestied. These camps were ultimately defeated by government forces.

[16]Of the excluded camps 7 belong to smaller gangs affiliated with larger militant groups (Hazen and Horner 2007) and the remaining 2 are unidentified.

[17]This includes one camp which was amnestied later, in 2016, and one that was not explicitly amnestied but nevertheless disbanded after the conflict ended.

[18]However, for the purposes of camp-level analysis in SI-C.2, we retain the full sample where possible.

[19]We test robustness of our main results to this treatment threshold in SI Figures D6 and D11.

Table 1: Camp-level data

		Camps	Commanders
Amnestied		47	23
	Security contract	25	7
	No security contract	22	16
Non-amnestied		13	11
	Defeated pre-2009	6	4
	Did not take amnesty	7	7
Outcome unknown		9	7
Total		69	41

Summary of camp-level data, providing the count of unique militant camps or commanders (indicated in table header) in each amnesty category.

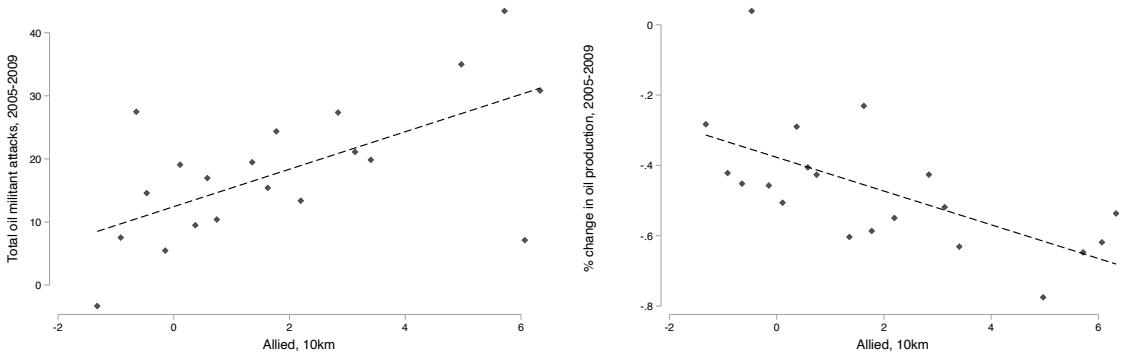
then further designated as security contract recipients if the nearest commander received one. Finally we drop locations that are more than 20 kilometers from any oil infrastructure, leaving 5111 villages. The sample of camps and their designations is mapped in Figure 1.

Military strength: The rebel's ability to execute attacks and destroy oil output is unobserved in the data and a key parameter in our theoretical model. To proxy for a rebel camp's underlying military strength, we note that the Niger Delta conflict is known for a dense and highly connected network of alliances between militant commanders and that rebel strength is inherently localized by its dependence on access to infrastructure targets.[20] We measure camp-level military strength by calculating for each camp the number of allied camps within 10 kilometers along connected oil pipelines, intersecting the networks of militant alliances and oil infrastructure. Figure 2 shows that alliance density is strongly correlated local with attack frequency and oil output destruction during the Niger Delta conflict. For details on the construction and validation of this measure, see SI-C.2 where we report estimated coefficients, comprehensive robustness tests, and network statistics.

[20]The Niger Delta conflict was fought in the vicinity of oil infrastructure, not urban centers. Between 2005-2009 approximately 90 % of oil-related militant attacks occurred within 10 kilometers of the oil pipeline network, in turn comprising some 80% of all conflict events and causing 40% of all combat deaths in Nigeria as a whole over the same period. Further details, including binned scatterplots of attacks against distance are reported in SI D.1.

Figure 2: Local alliance density, attacks, and damage

(a) Militant attacks (b) Damage



Note: This figure shows a binned scatterplot of the militant-camp-level relationship between the number of allied connections along the pipeline and either militant attacks (a) or the change in oil output during the height of the Niger Delta Crisis (b). Militant attacks are the total count of ACLED events within 20 km of the camp between 2005 and 2009 targeting the oil industry. Damage inflicted is measured as the percent change in onshore oil production within 20 km of the militant camp location between 2005 and 2009. The independent variable is number of allied camps within 10 km along the pipeline. Regressions include state fixed-effects and camp-level controls for slope, altitude, average temperature- and precipitation, latitude, and distance to the nearest pipeline, state capital, and Atlantic coast.

Oil theft: Information on the time, location, and details of 11,327 georeferenced oil spills between 2006-2017 comes from the administrative records of the National Oil Spill Detection and Response Agency (NOSDRA). Oil spills are categorized as being caused by equipment malfunction or third party interference; the latter serves as our definition of oil theft.[21] By this definition, 61.7% of all spills are classified as theft, a proportion that rises from 28.8% to 77% between 2006 and 2016. Pipelines may range from smaller flowlines (2-6 inch diameter) to larger trunklines (at least 12 inches in diameter). To increase the validity of our aggregate measure we restrict our attention to theft from economically valuable trunklines.[22]

[21]We emphasize that the NOSDRA-recorded sabotage spill events typically involve the use of hacksaws, valves, and other tools to divert oil, and are thus distinct from attacks aiming to destroy oil infrastructure, e.g. using explosives. Representative descriptions of such third-party pipeline sabotage include "[a]n unknown person cut off the cone of previously repaired oil theft point for the purpose of crude oil theft" and "unknown person(s) installed 3" valve on the facility ... for crude oil theft activities."

[22]We define village-level theft conservatively, using a five kilometer radius around the village.

Law enforcement activity: Data on law enforcement comes from the text of news media reports on raids, seizures, arrests, and other oil theft-related activity from Nigerian and international publications. We assemble a comprehensive sample of possibly crime-related news articles, and then employ local research assistants to identify relevant articles and extract all law enforcement events, defined as a unique interaction between criminals and state security agents in a particular location. From this procedure, we obtain 5682 unique geocoded law enforcement events from 2000-2020, of which 3261 are related to the illegal oil sector. For each event, we observe all involved law enforcement agencies, the criminal activity, the items destroyed or seized, and the number of arrests and fatalities.[23,24] Importantly, our data on illegal activities allow us to disaggregate criminal behavior and enforcement actions along the illegal sector value chain. We collapse these events to the village-year level. Before amnesty, the probability of oil theft-related enforcement for a village in a given year is 1.8%, rising to 7.8% after amnesty.

3.2 Summary statistics

Table 2 presents means and standard deviations of our pre-amnesty outcome and control variables at the village level. Column (1) includes all observations, columns (2)-(4) reports our estimation sample that excludes villages more than 20 kilometers from any oil infrastructure. We code a village as treated if located within 30 kilometers of an amnestied camp and a control if not. The table shows that treated villages are generally located in coastal riverine areas, closer to oil infrastructure, and experience elevated levels of oil theft and conflict prior to amnesty.

4 Motivating evidence

To motivate our theoretical framework we show that *i*) pre-amnesty attacks targeting the oil sector and *ii*) post-amnesty increases in oil theft were significantly and robustly concentrated in amnestied areas and *iii*) that oil theft and government enforcement were lower in locations where security contracts were awarded.

We test robustness of the main results to this arbitrary distance threshold in SI Figure D6.

[23]Examples of criminal activity are transportation of stolen oil, piracy, and pipeline vandalism.

[24]Examples of seized and destroyed items include guns, illegal refineries, stolen oil, and boats.

Table 2: Summary statistics

Sample	Full	Estimation		
Group		All	Treated	Control
	(1)	(2)	(3)	(4)
Panel A: Pre-treatment outcome variables				
Oil theft	0.11	0.20	0.22	0.17
	(0.78)	(1.03)	(0.98)	(1.08)
Militant attacks	0.05	0.09	0.16	0.02
	(0.65)	(0.86)	(1.22)	(0.20)
Anti-oil theft law enforcement	0.09	0.16	0.22	0.10
	(0.70)	(0.93)	(1.11)	(0.74)
Panel B: Cluster-level covariates				
Distance to oilfield (km)	36.72	13.69	9.04	17.95
	(43.99)	(19.76)	(5.32)	(26.15)
Distance to pipeline (km)	42.06	12.93	7.26	18.12
	(50.68)	(17.18)	(6.65)	(21.64)
Distance to capital (km)	62.38	67.00	67.83	66.24
	(47.36)	(42.97)	(48.91)	(36.70)
Distance to Niger River (km)	87.00	58.13	49.72	65.83
	(60.02)	(50.58)	(44.84)	(54.18)
Distance to coast (km)	61.24	39.59	20.84	56.75
	(49.66)	(35.24)	(21.69)	(36.48)
Population density	444.27	425.88	437.72	415.03
	(865.28)	(955.36)	(1139.45)	(748.32)
Distance to amnestied camp (km)	65.31	41.05	15.52	64.40
	(49.91)	(34.09)	(8.08)	(32.00)
Distance to non-amnestied camp (km)	92.15	51.30	32.57	68.43
	(69.04)	(40.70)	(29.35)	(42.07)
Number of amnestied camps within 30 km	1.32	2.25	4.72	0.00
	(3.11)	(3.90)	(4.50)	(0.00)
Number of villages	9185	5111	2442	2669

Note: This table presents village-year-level means of pre-treatment outcomes (Panel A) from 2006-2009 and village-level means of key covariates (Panel B). Standard deviations in parentheses. A village is treated if within 30 kilometers of an amnestied militant camp, while control are all those outside. Full sample is all villages within the nine Niger Delta states. Estimation sample is only oil-producing villages within the Niger Delta states, defined as all villages within 20 kilometers of oil infrastructure.

4.1 Conflict, oil theft and amnesty

The amnesty for Niger Delta militants marked a fundamental shift in the dynamics of violence and oil theft. The amnesty was preceded by a surge in militant attacks, and followed by sharp drop in violence and increase in oil theft. Figure 3 plots trends of militant attacks and oil theft in amnestied communities relative to non-amnestied ones. These trends are estimated by quarterly event-study regressions of the outcomes on dummy variables for periods pre and post amnesty, interacted with a treatment indicator.[25] We report standard errors *i*) clustered at the village level and *ii*) additionally adjusted for spatial correlation within a 50 kilometer radius, following Conley (2010). Neighboring villages are *i*) exposed to shocks that induce unobserved, contemporaneous interdependence and *ii*) will mechanically exhibit spatial correlation in exposure to amnesty because the definition of treatment and outcome are geographic. The clustered standard errors account only for serial correlation within a village and may therefore be underestimated.[26] In the following we consequently report both cluster and spatially corrected standard errors.[27]

Panel (a) shows that, preceding the amnesty, violence targeting the oil sector was greater in amnestied than non-amnestied villages. Dummy variables for pre-event periods are generally positive and sometimes significant, while those for post-amnesty periods are essentially zero. The results demonstrate a sharp drop in attacks targeting the oil sector at exactly the amnesty date. In OA B.1 we investigate the evolution of militant attacks around the amnesty using a regression discontinuity in time model (Hausman and Rapson 2018) on monthly data and find a quantitatively similar and robust effect, suggesting the result is likely not caused by unobserved factors correlated with amnesty or measurement error.[28,29]

[25] All estimates control for village and time fixed effects use 2 quarters before the amnesty serving as the omitted reference period.

[26] See Kelly (2019) on how unaccounted spatial correlation may yield substantial underestimation of standard errors.

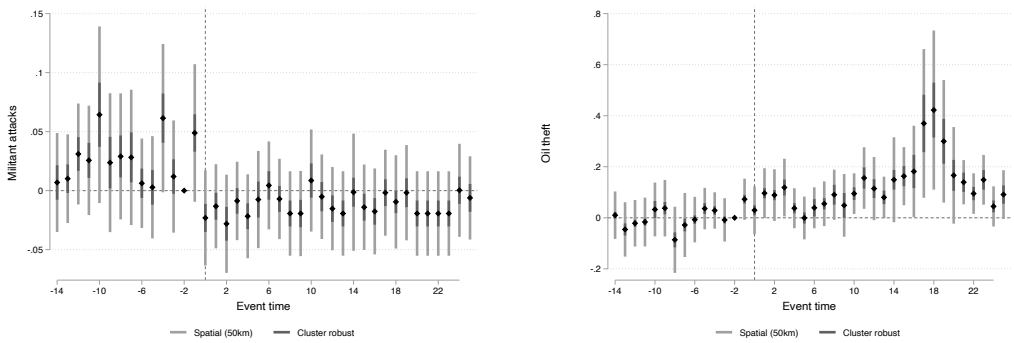
[27] For implementation we follow Hsiang (2010).

[28] We show in OA B.1 that the result is robust to a battery of alternative model specifications (Table B1), placebo tests on event dates and non-oil conflict (Figure B2), and the removal of influential observations (Figure B3). In SI D.2 we further show that the placebo test is robust to specification (Table D1), the result obtains under conventional lag-order selection criteria (Table D2) and, following Perron (2006), that the event date identifies a structural break (Table D3, Figure D3).

[29] Three observations suggest measurement error is unlikely to have caused the reduction in vi-

Figure 3: Militant attacks and oil theft event-study

(a) Militant attacks (b) Theft



Note: This figure shows quarterly event-studies for militant attacks (a) and oil theft (b) before and after amnesty. Coefficients and 95% confidence intervals are from a quarterly event-study regression of the outcome, measured within 5 kilometers of a village, on dummies for periods before and after amnesty, interacted with an indicator for within 30 kilometers from a militant camp. Standard errors are either clustered at the village level, or additionally adjusted for spatial correlation within a 50km radius, following Conley (2010). The amnesty date is indicated by the vertical line, the omitted period is 2 quarters before.

Panel (b) plots the event-study coefficients for oil theft. Differential trends in oil theft between amnestied and non-amnestied areas are zero and insignificant for most of the pre-amnesty quarters. However, oil theft rises immediately in the post-amnesty period in amnestied areas and grows throughout the period of domestic shortage of refined products, peaking five years after amnesty and then declining.

The differential evolution of oil theft in locations controlled by amnestied rebels is central to our paper and is now studied in detail. We estimate difference-in-differences (DD) regression for village i in year t

$$y_{it} = \alpha + \psi T_i^d Post_t + \xi_t + \xi_i + X_{it}'\beta + \varepsilon_{it} \tag{1}$$

olence. Firstly, reduced militant activity coincides with a steep reduction in law enforcement targeting rebel groups. Figure C4, SI C.1 shows that federal actions countering militants and oil theft fell from pre-amnesty maxima to near zero exactly at the amnesty date. Second, oil production quickly rebounded to pre-conflict levels following the amnesty. From 2005, annual oil output in Nigeria fell by some 15% throughout the conflict, reaching a low of 2.2. million barrels per day in 2008. Production increased by 17% from 2009, the year of amnesty, to 2010, with output attaining the pre-conflict 2005 level, e.g. BP (2020). Third, the improved security situation in the Niger Delta post-amnesty would, if anything, facilitate more accurate reporting of incidents and thus reduce underreporting.

where y_{it} is the level of oil theft in village i at time t, T_i^d is an indicator for being d kilometers from an amnestied militant camp, set to $d = 30$ in most specifications, and $Post_t$ is a post-2009 dummy. The ξ_t and ξ_i are year and village fixed effects.[30] The vector of controls X_{it} contains initial conditions interacted with time dummies, including gridded population density in 2005 and distances to the nearest oil field or pipeline, state capital, Niger River, and coast (see Table 2). The random disturbance ε_{it} is assumed, for identification, orthogonal to the treatment indicator $T_i^d Post_t$, amounting to the standard parallel trends assumption required for DD to deliver consistent estimates of ψ.

Table 3 presents the main DD estimation results. The baseline estimate of the standard two-way fixed effects (TWFE) model in column (1) indicates that amnesty increased oil theft by .448 events in amnestied villages relative to control ones, significant at the 1% level for all standard errors. This is large effect, approximately doubling the pre-amnesty control group mean.

The parallel trends assumption is violated if oil theft is differentially affected by time-varying confounds (e.g. oil prices) in amnestied regions, or if the receipt of amnesty is endogenous to location-specific factors that later cause oil theft (e.g. access to valuable targets, or local labor market conditions). We rule out that the effect is driven by local resource endowments or time-varying, possibly village-specific factors unrelated to amnesty, e.g. local economic growth or oil price trends. We measure the availability of local oil infrastructure by distance to oilfields or pipelines and interact these with the post-amnesty dummy in columns (2) and (3), yielding essentially identical estimates. To rule out additional village-specific explanations we include in column (4) the full set of interacted spatial controls X_{it}. Comprehensive robustness tests are reported in OA B.2-B.4 and SI D.3.[31]

[30] In some specifications we include the term $\xi_i t$ allows for a village-specific linear time trend.

[31] We test for robustness to specification in OA B.2. The effect of amnesty is robust to including oil prices, specifications of local trends, and municipality-by-year fixed effects (Table 3). Figure B4 plots a comprehensive summary of 120 different specifications varying the control group, specification, and treatment definition. In OA B.3 we find no evidence of pre-trends (Figure B5). We demonstrate in OA B.4 that a quantitatively comparable effect of amnesty obtains when using sabotage of any infrastructure type as the outcome variable (Figure B6) and show that the effect of amnesty is due almost exclusively to trunkline sabotage, yielding more precise estimates. We show in SI D.3 that the effect of amnesty is robust to control group definitions (Table D4, Figure D4). In particular we consider robustness to a control group consisting of non-amnestied camps only, disaggregating them by outcome (Table D5, Figure D5). The effect of amnesty is significant and quantitatively comparable when using non-amnestied camps as a control. We find slightly smaller

Table 3: The effect of amnesty on oil theft

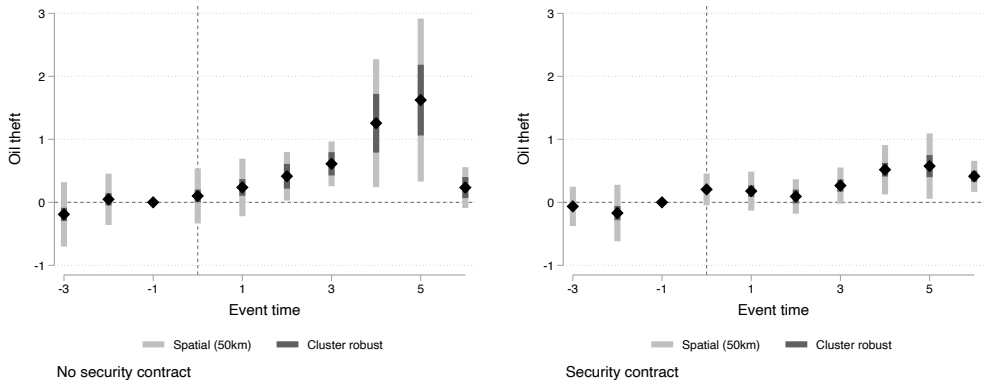
Dependent variable	Oil theft			
	(1)	(2)	(3)	(4)
Amnestied × Post-amnesty	0.448***	0.451***	0.447***	0.443***
	(0.046)	(0.047)	(0.046)	(0.042)
Distance to oilfield (00s km) × Post-amnesty		0.041	0.041	
		(0.043)	(0.043)	
Distance to pipeline (00s km) × Post-amnesty			-0.041	
			(0.067)	
Control mean	0.201	0.201	0.201	0.201
Kilometers	Spatial standard errors			
10	0.076	0.080	0.085	0.092
50	0.101	0.105	0.109	0.103
100	0.095	0.099	0.104	0.100
500	0.104	0.105	0.101	0.079
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls × Year FE	No	No	No	Yes
Number of villages	5111	5111	5111	5111
Observations	61332	61332	61332	61332
R^2	0.396	0.396	0.396	0.404

Note: Standard errors in parentheses are clustered at the village level. Spatial standard errors are calculated following Conley (2010). Outcome variable is the number of annual oil theft events within 5 kilometers of the village. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group is all villages outside of this range. Controls include distance to nearest oilfield, distance to nearest pipeline, distance to state capital, distance to Niger River, distance to the coast, and population density. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

4.2 Oil theft, law enforcement, and security contracts

Providing rebel groups with security contracts may represent an attempt by the government to offer incentives against post-amnesty rebel oil theft. If so, a natural question is whether these contracts effectively reduced oil theft, notwithstanding the substantial overall rise in theft. Figure 4 plots the coefficients from an event-study regression for subsamples of treated villages controlled by militant commanders who received amnesty but no security contract (left) and those that received both amnesty and security contracts (right).

Figure 4: Oil theft, amnesty, and security contracts



Note: This figure shows the coefficients and 95% confidence intervals from an annual event-study regression of oil theft on dummies for years pre-and post amnesty, interacted with the treatment indicator, which equals one for villages within 30 km of an amnestied militant camp and an indicator which equals one if the nearest amnestied commander received a security contract. The omitted reference year is 2008. Outcome variable is the number of annual oil theft events within 5 kilometers of the village. Standard errors are either clustered at the village level, or additionally adjusted for spatial correlation within a 50km radius, following Conley (2010).

While both groups exhibit parallel trends, the post-amnesty spike in oil theft is largely concentrated in the amnesty-only group. Areas where militants received

effects when using only defeated camps as controls, suggesting a crime displacement mechanism. Finally, we show that the effect of amnesty is robust to controlling for presence of neighboring camps (Table D6), rebounding oil output (Table D8), measurement error in oil theft (Table D9), varying geographic definitions of treatment (Figure D6) and maximum likelihood estimation for count data (Table D10). Finally, we find positive and significant effects on the extensive margin of treatment and intensive margin of theft in Tables D11 and D12 respectively.

security contracts see a subdued increase in oil theft. These results suggest that security contracts provided effective incentives for rebels not to steal. Quantitatively, the average DD amnesty effect of 0.448 in Table 3, column (1) rises to 0.528 among groups that received only amnesty and no security contract, significant at 1%. The effect of amnesty on oil theft in areas controlled by groups that received a security contract falls by -0.165 in the base specification. For estimates and robustness to alternative specifications, see Table B3, OA B.5. In the most exacting specifications, the contracted effect is 40-56% smaller than the baseline effect, though the difference is statistically significant only with conventional standard errors. Importantly, we do not observe a corresponding post-conflict increase in government law enforcement activity in contracted regions, suggesting that the localized reduction in oil theft is unlikely to be driven by differential enforcement.[32]

5 Model

This section derives a model of dynamic conflict bargaining between a rebel and government in the presence of a lootable oil resources, formalizing our hypothesis that the government has incentives to tacitly allow resource theft from militarily strong rebels in locations with favorable conditions in the black market.

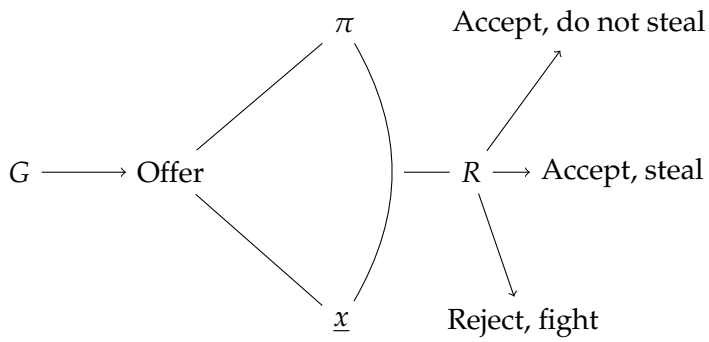
5.1 The game

The stage game proceeds as follows: The government first makes an offer $x \in X = [\underline{x}, \pi] \subset \mathbb{R}$ from a surplus of $\pi > 0$. The rebel either $i)$ accepts the offer and does not steal oil, $ii)$ accepts the offer and steals oil, or $iii)$ rejects the offer outright and fights. The stage game tree is shown in Figure 5.

When the offer x is accepted without theft the payoff vector is $(\pi - x,\ x)$ for government and rebel. If the offer is rejected outright by the rebel there is fighting with payoffs $(\pi - g,\ r)$ where $g \geq r$ measures the government's cost of fighting and $r > 0$ the rebel's conflict payoff. The payoffs when rebels accept the offer x and steal oil are $(\pi - x - \gamma, x + \rho)$ where γ is the government's valuation of stolen oil and $\rho \leq \gamma$ the rebel's income from oil theft.

[32]See results of Table 7. We discuss the role of government law enforcement in Section 6.2.

Figure 5: Stage game timing



Note: This figure represents stage game timing where G and R denote government and rebel.

We assume that the government's cost of fighting a rebel weakly exceeds losses from oil theft, $\gamma \leq g$ and that rebel oil theft income weakly exceeds their payoff under conflict, $r \leq \rho$. Let the least offer $-\infty < \underline{x} < r - \rho < 0$ without loss of generality, hence the transfer may be negative and interpreted as a bribe from rebel to government.

The restrictions imply that $0 < r \leq \rho \leq \gamma \leq g$: Rebels enjoy a positive payoff r from fighting, which is inferior to their income ρ from oil theft. The government's value γ of stolen oil is weakly greater than the rebel income ρ from theft, but γ may substantially exceed ρ due to e.g. oil spills and other efficiency losses. Conflict is weakly more inefficient than oil theft $\gamma \leq g$, capturing the idea that rebels intentionally not only extract, but also deliberately destroy surplus from oil production during conflict.[33]

The repeated game is played over an infinite, discrete time horizon, indexed over $t \in \{0, 1, \ldots\}$, with future payoffs discounted by $\delta \in (0, 1)$.

[33]Conversely the assumption that fighting is less inefficient than allowing oil theft implies that i) the government has no financial incentives to seek peace and ii) insofar as rebel attacks against the oil sector serve to motivate the government to cede more oil revenues in the future, rebels have no incentives to fight, starkly inconsistent with accounts of the Niger Delta conflict reviewed in 2.

5.2 Credibly enforced equilibria without theft

We define and characterize the credibly enforced equilibrium without oil theft and derive conditions under which the government may prefer to tacitly allow oil theft over credible enforcement. To begin we note that any finitely repeated game admits a unique pure-strategy equilibrium with oil theft and a bribe from rebel to government, see Appendix A.1, illustrating that equilibria without oil theft may be sustained only by the threat of future punishment. We next show in Appendix A.2 that for an unrestricted strategy space in the infinitely repeated game, the set of payoffs sustained in an SPE without oil theft tends to the set of individually rational payoffs when the discount factor approaches unity. If the rebel does not anticipate a reduction in payoffs when accepting an inferior offer then subgame perfection will generally demand that the government punishes by tacitly accepting oil theft and recouping surplus net of rebel reservation values through bribes.

We consider in Appendix A.3 equilibria where oil theft is deterred by the off-path threat of fighting and sustained by *i*) the government's expectation that the rebel will continue to steal until punished and that *ii*) the rebel will reject inferior offers, and *iii*) the rebel's expectation that the government will not return to peaceful rent-sharing before having concluded the punishment in full. Given our no-commitment context we propose that the salient incentive condition is for the government's threat of costly punishment to be credible ex-ante. We say an equilibrium is credibly enforced when the government is indifferent between fighting or suffering oil theft and paying the equilibrium transfer. Under credible enforcement the government has no incentive to deviate from punishment and we can dispense with expectation *iii*).

Credible enforcement induces a limiting lower bound $x_c = \min\{r, g - \gamma\}$ on the equilibrium offer when punishments become arbitrarily long and the discount factor is high. We present this limiting case in the following to simplify the presentation with little loss generality.[34] The least transfer that credibly enforces an equilibrium without oil theft increases in rebel military capability g net of effi-

[34]To evaluate the general expression substitute r with the least deterring transfer $\tilde{r}$ from Equation (5), Appendix A.2. Note that $\tilde{r}$ decreases convexly in the duration of off-equilibrium punishment and tends asymptotically to the rebel reservation value r. The least excess payment has hyperbolic decay in the duration of punishment T when the discount factor approaches unity, $\lim_{\delta \to 1} \tilde{r} - r = (r + \rho)/T$.

ciency losses γ to compensate for the increased opportunity cost of conflict over allowing oil theft. The intuition is that the credibly enforced offer publicly ties the government's hands by rendering them indifferent between suffering oil theft and transferring surplus or fighting.

We use credible enforcement as our equilibrium selection criterion. To provide some intuition, consider the Nash bargaining solution of the infinitely repeated game with weight $\omega \in [0, 1]$ on the government:

$$x(\omega) = (1-\omega)g + \omega r =: \arg\max_{x \in [r,g]} (\pi - x - (\pi - g))^{\omega}(x - r)^{1-\omega} \tag{2}$$

tractably capturing the idea that rebel payoffs depend on their ability to damage the government g to an extent governed by the bargaining weight ω. Credible enforcement imposes a lower bound x_c on the equilibrium transfer implying an upper bound

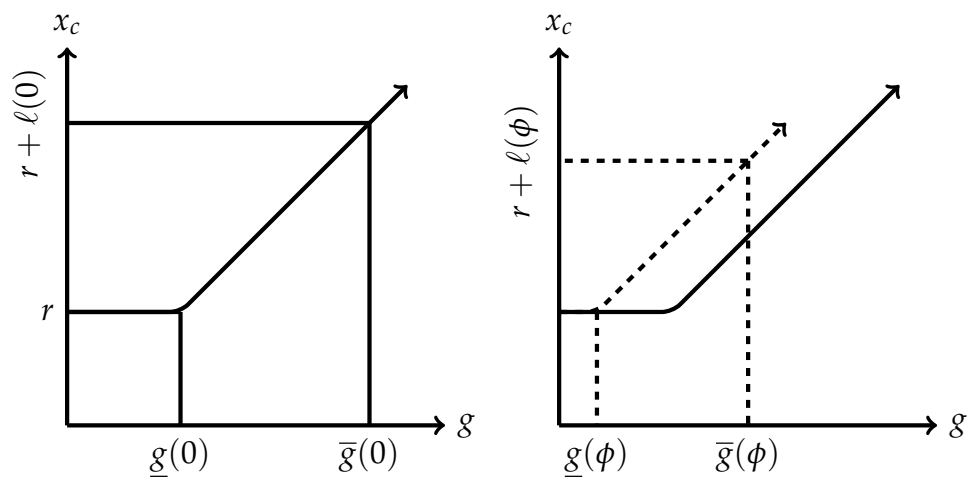
$$\omega^* = \min\left\{1, \frac{\gamma}{g - r}\right\}$$

on the bargaining weight on the government.

Conversely, we note that the government does not need to credibly threaten costly punishment when oil theft occurs on the equilibrium path. If facing a rebel of sufficient militarily capability, there exist equilibria with theft that provide the government a greater payoff than the equilibrium with least-cost credible deterrence. Hence we propose an equilibrium-switching mechanism: in locations where rebels are militarily powerful and efficiency losses of rebel oil theft low the government may prefer to tacitly allow theft and recover lost surplus through a bribe.

Consider the left panel in Figure 6, which plots the least-cost of a credible transfer x_c against rebel military capability g as the discount factor becomes high and punishments arbitrarily long. Weak rebels with low battlefield damage $g \leq \underline{g}$ may be credibly enforced by their reservation value r. The government's least cost of allowing oil theft is $r - \rho + \gamma$, intersecting the x_c schedule at $\overline{g}$. The range of battlefield damage g in which the government strictly prefers to transfer a rent to credibly enforce over allowing oil theft is thus given by the interval $(\underline{g}, \overline{g})$. For rebels with $g > \overline{g}$ there exist SPE with oil theft that the government weakly prefers to the least-cost credibly enforced transfer.

Figure 6: Least credibly enforced offer x_c vs. rebel battlefield damage g and fringe activity ϕ



Note: This figure shows the impact of a competitive fringe in the market for stolen oil on the relationship between a credible transfer $x_c = g - \gamma$ and rebel military capability. The interval $[\underline{g}(\gamma), \overline{g}(\gamma)]$ denotes as a function of the local black market response $\phi \in [0, 1]$ the range for battlefield damage g for which the government prefers to provide costly, credible incentives against oil theft given. The government's least net cost of tacitly accepting oil theft is given by $r + \ell(\phi)$.

5.3 Credible enforcement with a competitive fringe

We now extend the model to include a competitive, endogenously enforced fringe in the black market. Let the government's loss from rebel oil theft net of fringe displacement be $(1 - \phi)\gamma \leq \gamma$. We assume that ϕ is inversely proportional to local black-market costs, see Appendix A.4 for a microfoundation. Let $e \geq 0$ be a measure of government law enforcement effort directed at the fringe, e.g. the number of refinery raids or seizures of stolen oil. Under the assumption of decreasing returns to enforcement we show in Appendix A.5 that optimal effort e^* is also inversely proportional to local costs.

Government payoffs before transfers are now given by π when the rebel does not steal and $\pi - (1 - \phi)\gamma$ when there is theft. Let

$$\ell(\phi) := \underbrace{\gamma - \rho}_{\text{lost surplus from rebel theft}} - \overbrace{\phi\gamma}^{\text{displaced fringe}} = \underbrace{(1 - \phi)\gamma - \rho}_{\text{net loss from rebel theft}} \in [-\rho, \gamma - \rho]$$

be the efficiency loss from rebel oil theft γ net of fringe displacement $\phi\gamma$ and rebel income ρ.[35] The presence of a competitive fringe raises the cost of credible enforcement by reducing the efficiency loss of rebel oil theft and weakening the government's incentives to punish them.[36] The impact of a competitive fringe on credible enforcement schedule is illustrated in the right panel of Figure 6. The cutoff values for military strength conditional on competitive fringe activity are denoted $\underline{g}(\phi)$, $\overline{g}(\phi)$. The solid black and dotted line plot the schedule with and without a competitive fringe, respectively. Notice that the least cost of a credibly enforced transfer x_c increases by $\phi\gamma$ for all rebels exceeding the initial minimum value for military strength $g \geq \underline{g}(0)$. Moreover notice that the range $\overline{g}(\phi) - \underline{g}(\phi)$ for which the government prefers to provide credible incentives against oil theft decreases in ϕ as the rebel's displacement of the competitive fringe weakens the government's incentives to punish rebel oil theft. The implication is that, everything else equal, the government strictly prefers to provide credible enforcement only to a smaller, militarily weaker set of rebels in the presence of a competitive fringe. When facing a powerful rebel in a location with an active fringe, the government prefers to tacitly allow oil theft from the rebel, displacing the fringe and recouping lost surplus through bribes rather than paying the cost of credible enforcement.

6 Testing equilibrium mechanisms

The key empirical implication of our model is that the government may tacitly allow oil theft from military powerful rebels in low-cost locations. The discontinuous change in the government's incentive provision decision predicts nonlinear relationships between black-market costs, rebel military capability, and subsequent patterns of oil theft and enforcement. We now state and test these predic-

[35]Note that rebel oil theft is efficient $\ell \leq 0$ if the fringe is sufficiently displaced, $\phi \geq (\gamma - \rho)/\gamma$. If $\ell \leq 0$ equilibria with rebel oil theft trivially Pareto-dominate any equilibrium without oil theft as the total available surplus $\pi - \ell$ increases.

[36]To verify, substitute $(1-\phi)\gamma$ for γ in Proposition 3, Appendix A.3 and notice that the implied critical bargaining weight on the government

$$\omega \geq \omega^* := \min\left\{1, \frac{(1-\phi)\gamma}{g-r}\right\}$$

decreases in fringe activity ϕ.

tions, proceeding in two steps. Section 6.1 shows that oil theft is indeed elevated in low-cost markets with militarily powerful rebels, locations in which our model predicts that government has the strongest incentives to allow oil theft, driven by a non-linear threshold effect. In Section 6.2 we provide evidence of a discontinuous change in government behavior using data on anti-oil theft law enforcement actions in the Niger Delta taken from local news media sources.

6.1 Oil theft, black market costs, and military capacity

The government's decision to tacitly allow resource theft depends jointly and positively on the level of, and interaction between, the cost structure in the black market and rebel military capability. Our first prediction relates rebel strength and local cost conditions in the black market to oil theft.

Prediction 1. *Oil theft decreases- and increases in cost conditions and rebel military capability, respectively.*

A partial increase in local costs reduces equilibrium oil theft directly and indirectly. The direct effect follows from the price-taking behavior of the black market's competitive fringe, where lower costs lead to weakly greater entry and output. The indirect effect of higher costs is to strengthen the government's incentive to provide rebels with credibly enforced contracts. We expect the indirect effect to induce a discrete jump in oil theft, yielding a convexity in the observed response.[37] A partial increase in military capability increases oil theft by increasing the cost of credible incentive provision.

We measure the black market costs in a given location using three variables: *i*) distance from village to the Atlantic coast and *ii*) the Niger River, and *iii*) median wages for young men (under age 40) in the local labor market.[38,39] Distance

[37]Plausibly lower costs in the black market also increase the rebel income from oil theft ρ. From the government's perspective higher rebel income from oil theft ρ is observationally equivalent with a increased fringe activity ϕ, both decreasing the efficiency losses from rebel oil theft ℓ and weakening the incentive for providing a credibly enforced contract.

[38]Labor markets are defined at the local government area level to minimize spillovers between villages, and wages are measured in 2009.

[39]These cost factors are derived from the qualitative literature, reviewed in Section 2 which finds that labor costs dominate for most small-scale oil theft operations, followed by transport and bribery costs. For experimental evidence on the relationship between local wages and illicit activity see e.g. Blattman and Annan (2016).

to rivers and the coast provides a measure of the transport costs faced by oil theft gangs to move the oil and access export markets, respectively. Local wages determine the opportunity costs faced by young men on the margin of joining organized crime, and thus the prevailing black market wage.

To test for heterogeneity in cost conditions, we index the above-mentioned cost factors over c and estimate

$$y_{it} = \alpha + \psi T_i^d Post_t + \sum_c (\psi^c T_i^d Post_t + \xi_t) f_i^c + \xi_t + \xi_i + X_{it}'\beta + \varepsilon_{it} \tag{3}$$

a triple-differences model that interacts cost factors f_i^c with the time-varying treatment indicator and year fixed effects in Equation (1).[40] Results are given in Table 4, where columns (1)-(3) respectively interact the treatment with each cost factor individually, and column (4) includes all cost factors simultaneously. Further spatial standard errors for all specifications are reported in Table B5, OA B.7.

The results suggest that post-amnesty theft is concentrated primarily in low-cost markets that are attractive targets for fringe entry. In each specification, the amnesty effect is largest for low-cost villages, and falls to zero as costs increase. Using spatial standard errors, the estimated interactions with distance to coast (export costs) and local wages (labor costs) are negative and significant in every specification, while distance to the Niger River (transport costs) is only negative and significant when all cost variables and controls are included in column (6).

Oil theft effects may be greater in low-wage labor markets for a variety of reasons unrelated to the black market. In Table B4, OA B.6, we use wages of other demographic groups as a falsification test, including interactions with median wages for young- and old men or young women. The estimates for young men's wages remain negative and significant, while those for old men and young women are much smaller than the young male estimates and generally insignificant. Only wages in our demographic group of interest display evidence of significant heterogeneity, ruling out generalized income effects.

We turn to our second prediction, arising from the property of credibly enforced

[40] Identification of the triple difference coefficients requires that the relative outcome of low- and high-cost villages to trend similarly in amnestied- and non-amnestied locations, e.g. (Olden and Møen 2020). In Figure D7, SI D.4 we plot event-study coefficients for villages below- and above median wage, revealing no evidence of pre-trends.

Table 4: The effect of amnesty on oil theft by cost factors

Dependent variable	Oil theft			
	(1)	(2)	(3)	(4)
Amnestied × Post-amnesty	0.782***	0.516***	0.783***	1.431***
	(0.064)	(0.086)	(0.134)	(0.178)
Amnestied × Post-amnesty × Distance to Atlantic coast (00s km)	-1.160***			-1.490***
	(0.095)			(0.153)
Amnestied × Post-amnesty × Distance to Niger River (00s km)		-0.148		-0.467***
		(0.104)		(0.109)
Amnestied × Post-amnesty × Median wage, young men			-2.485***	-1.964***
			(0.701)	(0.543)
Control mean	0.201	0.201	0.201	0.201
Spatial standard errors (50km)				
Amnestied × Post-amnesty	0.145	0.175	0.242	0.338
Amnestied × Post-amnesty × Distance to Atlantic coast (00s km)	0.231			0.322
Amnestied × Post-amnesty × Distance to Niger River (00s km)		0.206		0.213
Amnestied × Post-amnesty × Median wage, young men			1.250	0.967
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls × Year FE	Yes	Yes	Yes	Yes
Number of villages	5111	5111	5111	5111
Observations	61332	61332	61332	61332
R^2	0.401	0.400	0.401	0.402

Note: Standard errors in parentheses are clustered at the village level. Spatial standard errors are calculated following Conley (2010) and reported in Table B5, OA B.7. Outcome variable is the number of annual oil theft events within 5 kilometers of the village. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group is all villages outside of this range. Median wage of young men is the median hourly wage of men aged 10-40 in the local government area, measured in thousands of Naira in 2009. Controls include distance to nearest oilfield, distance to nearest pipeline, distance to state capital, and population density. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

equilibria that rebel military capability and local cost conditions interact positively to increase the cost of credible enforcement.

Prediction 2. *The marginal effect of an increase in rebel military capability on oil theft decreases in local costs. Furthermore, the independent effects of local costs and military capability are non-linear.*

A partial increase in strength may trigger a shift to an equilibrium in which theft is implicitly tolerated, generating a positive correlation between theft and military strength. Lower costs in the black market shift down the threshold level of military strength below which high-powered contracts are preferred by the government, see $\overline{g}(\phi)$ in Figure 6. Therefore, as black market costs rise and ϕ falls, the response of oil theft to g should fall.

We test this prediction in the following quadruple-difference specification

$$\begin{aligned} y_{it} =& \alpha_0 + \alpha_1 Post \times T_i^d + \alpha_2 Post_t \times T_i^d \times g_i^\delta + \alpha_3 Post_t \times T_i^d \times f_i^w \\ &+ \alpha_4 Post_t \times T_i^d \times g_i^\delta \times f_i^w + \xi_t (g_i^\delta + f_i^w + g_i^\delta f_i^w) + \xi_i + X_{it}' \beta + u_{it} \end{aligned} \tag{4}$$

where g_i^δ measures rebel military strength as the number of local allies along a pipeline within δ kilometers for village i's nearest militant camp and f_i^w measures costs as young male wages.[41] The quadruple-difference coefficient α_4 tests the null hypothesis that the heterogeneous effect of local alliance density g_i^δ does not vary with wages f_i^w. The triple-interaction terms α_2 and α_3 test heterogeneity of the main effect with respect to g_i^δ and f_i^w, respectively, when the other variable is fixed at zero. Given Prediction 1, we also expect $\alpha_2 > 0$, since greater military strength increases the cost of a credible contract, particularly so when the fringe is responsive. Finally, α_1 gives the main treatment effect for low-cost, militarily weak areas. Fully-saturated 2- and 3-way interactions with the year fixed effects complete the quadruple-difference specification.

The results are given in Table 5, with spatial standard errors for all specifications are reported in OA B.7, Table B6.[42] Columns (1)-(4) consider the triple-difference model that interacts the indicator for amnesty with our measure of militant strength. The results indicate that post-amnesty oil theft increases in military strength. An additional local ally raises the effect of amnesty on oil theft by 0.1-0.4 incidents annually, approximately half and double the control mean, respectively. With 50 km spatial errors, this effect is significant at 5% in column (4) after accounting controlling for overall militant camp density, MEND membership, and the standard control variables.[43] Columns (5)-(6) complete the quadruple-difference model by adding the interaction with young male wages in the local labor market. The results show that while low-wage areas experience oil theft that increases

[41] Figure D8, SI D.4 plots event-study coefficients for treated villages with- and without local allies, revealing no evidence of substantially different pre-trends.

[42] All specifications use $\delta = 10$ kilometers. In Figure B7, OA B.6, we plot the triple-difference coefficients for varying levels of δ from 10 to 100 km. These effects fall to zero as distance along the pipeline grows, implying that military capability is a fundamentally local property dependent on a camp's position in the alliance and infrastructure networks. Figure D11, SI D.6 plots the estimated triple-difference coefficient on alliance density for 275 different specifications that vary threshold distances defining oil theft outcomes, military strength, and treatment definition.

[43] With clustered errors, it is always significant at 5% or lower.

Table 5: The effect of amnesty on oil theft by alliance density and cost factors

Dependent variable	Oil theft					
	(1)	(2)	(3)	(4)	(5)	(6)
Amnestied × Post-amnesty	0.380***	0.370***	0.203**	0.519***	0.425***	0.584***
	(0.042)	(0.044)	(0.080)	(0.052)	(0.109)	(0.100)
Amnestied × Post-amnesty × Allied camps, 10km	0.147***	0.112**	0.119**	0.293***	0.444***	0.301***
	(0.048)	(0.050)	(0.049)	(0.073)	(0.114)	(0.115)
Amnestied × Post-amnesty × Median wage, young men					-0.474	-1.451**
					(0.594)	(0.595)
Amnestied × Post-amnesty × Allied camps, 10km × Median wage, young men					-2.236***	-1.511***
					(0.584)	(0.578)
Control mean	0.201	0.201	0.201	0.201	0.201	0.201
Spatial standard errors (50km)						
Amnestied × Post-amnesty	0.106	0.112	0.170	0.140	0.191	0.192
Amnestied × Post-amnesty × Allied camps, 10km	0.094	0.091	0.089	0.124	0.203	0.187
Amnestied × Post-amnesty × Median wage, young men					1.142	1.099
Amnestied × Post-amnesty × Allied camps, 10km × Median wage, young men					1.041	0.963
Village FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	No
Controls × Year FE	No	Yes	Yes	Yes	No	Yes
MEND controls	No	No	Yes	No	No	No
Density controls	No	No	No	Yes	No	No
Number of villages	5111	5111	5111	5111	5111	5111
Observations	61332	61332	61332	61332	61332	61332
R^2	0.396	0.404	0.404	0.404	0.396	0.398

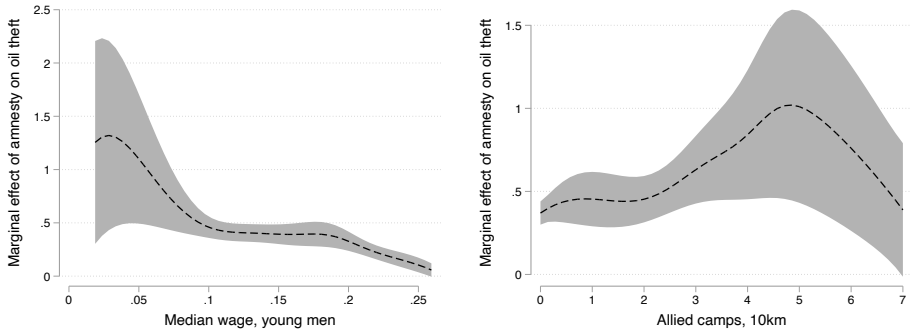
Note: Standard errors in parentheses are clustered at the village level. Spatial standard errors are calculated following Conley (2010) and reported in OA B.7, Table B6. Outcome variable is the number of annual oil theft events within 5 kilometers of the village. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group is all villages outside of this range. Median wage of young men is the median hourly wage of men aged 10-40 in the local government area, measured in thousands of Naira in 2009. Allied camps is the military strength of the nearest amnestied militant camp, measured as the number of allies within 10 kilometers along the pipeline. Controls include distance to nearest oilfield, distance to nearest pipeline, distance to state capital, and population density. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

in the number of local allies, this effect reverses as wages rise. The quadruple-difference coefficient is statistically significant at 5% with spatial errors in (5) and always significant at 1% with clustered errors. Quantitatively, the heterogeneous effect of an additional ally is roughly 2.7-3 times greater when labor costs are zero than in columns (1)-(2). Using the coefficients in (5), the partial derivative $\partial y_{it}/\partial g_i^{\delta}$ falls to zero when $f_i^w = 0.199$ (199 Naira per hour), corresponding to roughly the 90th percentile of median wages across villages. For the highest-cost black markets, the effect of military strength disappears entirely.[44]

Finally, we test for nonlinearity in local wages by non-parametrically estimating the marginal difference-in-difference effect of amnesty across the market-level wage distribution, after partialing out year and village fixed effects. Figure 7, left panel, plots the marginal effect across the distribution, estimated by kernel regression. The results reveal that the linear heterogeneity estimates in Table 4 are

[44]In additional tests, available upon request, we do not find significant quadruple-difference interaction coefficients for our export cost measures, which is consistent with the results of Table 4, and with qualitative reporting that labor costs dominate the cost structure of illegal firms.

Figure 7: Nonlinear effects on oil theft



Note: Left panel shows the marginal effect of amnesty on oil theft by hourly wages for young men in the local labor market. Wages are measured in thousands of Naira per hour. Marginal effects are estimated using a non-parametric kernel regression with a bandwidth of 35 (left panel) or 1 (right panel), after residualizing year and village fixed effects. Sample is all markets with strictly positive wages below 300 Naira per hour. Right panel shows the marginal effect of amnesty on oil theft by military strength as measured by the number of allied groups within 10 kilometers along the pipeline for the nearest militant camp, with a bandwidth of one unit. All standard errors are clustered at the village level.

driven almost exclusively by very low-wage markets. Quantitatively, the effect of amnesty on oil theft is 2.5 times larger at the 5th percentile of the wage distribution than at the median, falling to nearly zero towards the 95th percentile. This result is consistent with a threshold level of wages at which fringe activity falls enough that it becomes optimal for government to restrain oil theft, indicating shift from allowing theft to a credibly enforced equilibrium.[45]

We similarly test for nonlinearity in local alliance density in Figure 7, right panel. The results again reveal nonlinear dynamics that are suggestive of an equilibrium shift, with the mean effect of amnesty on oil theft doubling for the subsample with five or more allies, albeit with less precise estimates.

[45]We demonstrate in Figure D9, SI D.5 that the nonlinearity of oil theft to costs in Figure 7 are robust to choice of bandwidth and sample trimming.

6.2 Law enforcement behavior

Recall that the government's decision to tacitly allow resource theft depends jointly on the cost structure of the black market and rebel military capability through a discontinuous change in incentive provision. The implications for enforcement are twofold. First, the discrete change in incentives to rebels induces a concave and possibly non-monotonic relationship between enforcement effort and local costs. Second, taking security contracts as a sufficient condition for incentive provision, enforcement intensity should decrease in the military capability only among the non-contracted rebels.

A third prediction follows field research reviewed in Section 2, which notes that the competitive fringe is reported to be relatively more active in illegal refining than oil export, the latter dominated by powerful rebel groups. As such, our data allow us to differentiate – albeit coarsely – between enforcement actions against fringe vs. rebel theft. Maintaining this assumption, the award of security contracts should *increase* enforcement on refining but not oil export.

We now test these predictions in turn. Maintaining the assumption of a dominant firm, competitive fringe structure in the black market, decreasing returns to enforcement effort, and discontinuous shifts in incentive provision we have the following prediction:

Prediction 3. *The intensity of government law enforcement is concave and potentially non-monotonic in local black market costs.*

Fringe oil theft, and thus the optimal enforcement effort, is generally decreasing in local costs of oil theft. But when costs fall to the level in which rebel oil theft is optimally allowed by the government, rebels enter the local market as a dominant firm and displace theft from the competitive fringe. If the displacement effect is sufficiently strong the response of enforcement to local costs conditions may be non-monotonic, yielding an inverse-*U* pattern.

We test heterogeneity in law enforcement activity with respect to oil theft costs and rebel military strength, with results reported in Table 6 and further spatial standard errors for all specifications in OA B.7, Table B7. Column (1) estimates the triple-difference specification in Equation (3) using anti-oil theft enforcement as the outcome variable and measuring theft costs with local young male wages. Under

decreasing returns to enforcement and in the absence of strategic interaction, the state should optimally allocate resources to low-cost, highly active black markets where the marginal return to enforcement is greatest. In contrast, we find that post-amnesty oil theft enforcement is uncorrelated with local black market costs.

Table 6: The effect of amnesty on law enforcement by alliance density and costs

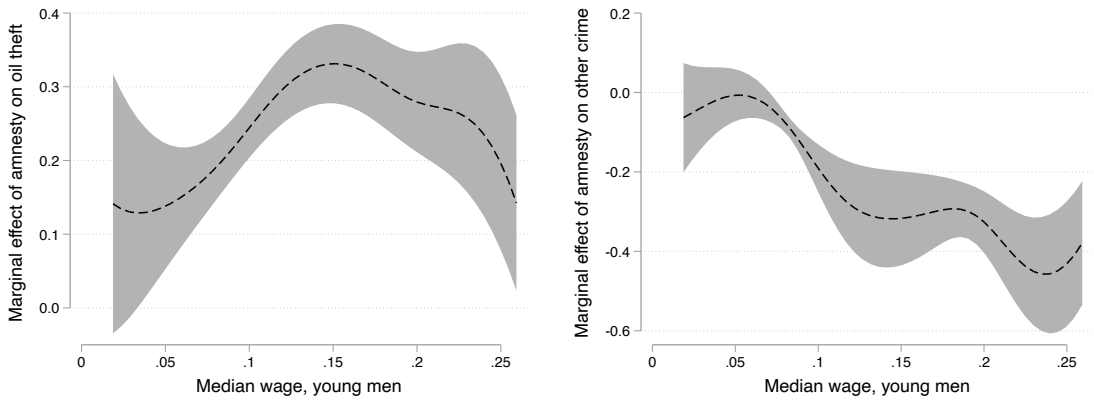
Outcome	Anti-oil theft law enforcement			
Sample	Full		No contract	Contract
	(1)	(2)	(3)	(4)
Amnestied × Post-amnesty	0.095***	0.113***	0.290***	0.110***
	(0.036)	(0.019)	(0.036)	(0.028)
Amnestied × Post-amnesty × Median wage, young men	0.006			
	(0.232)			
Amnestied × Post-amnesty × Allied camps, 10km		-0.022	-0.061***	-0.006
		(0.016)	(0.016)	(0.020)
Control mean	0.069	0.069	0.005	0.113
Spatial standard errors (50 km)				
Amnestied × Post-amnesty	0.144	0.079	0.107	0.111
Amnestied × Post-amnesty × Median wage, young men	0.739			
Amnestied × Post-amnesty × Allied camps, 10km		0.086	0.036	0.101
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls × Year FE	Yes	Yes	Yes	Yes
Number of villages	5111	5111	2097	3014
Observations	61332	61332	25164	36168
R^2	0.394	0.394	0.516	0.362

Note: Standard errors in parentheses are clustered at the village level. Spatial standard errors are calculated following Conley (2010) and reported in OA B.7, Table B7. Outcome variable is the count of all anti-oil theft law enforcement actions within 5km of a village. Sample is indicated in table header. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group is all villages outside of this range. Controls include distance to nearest oilfield, distance to nearest pipeline, distance to state capital, distance to Niger River, distance to the coast, and population density. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

The nonlinearity of enforcement in black market costs explains this unexpected result. In Figure 8, left panel we test for nonlinearity using kernel regression to estimate the non-parametric interaction effect between amnesty and costs over the distribution of local wages. The results show a clear inverted-U shaped curve consistent with the prediction. Quantitatively, the effect of amnesty on anti-oil theft enforcement is approximately equal at the bottom and top of the support of market-level wages. In contrast, we find that the intensity of non-oil crime enforcement falls monotonically in the wage distribution, consistent with the absence of a

discrete change in government policy.[46]

Figure 8: Nonlinear effects on enforcement outcomes



Note: Figure shows the marginal effect of amnesty on anti-oil theft law enforcement (left panel) and other criminal activities (right panel) by hourly wages for young men in the local labor market. Wages are measured in thousands of Naira per hour. Marginal effects are estimated using a non-parametric kernel regression with a bandwidth of 35, after residualizing year and village fixed effects. Sample is all markets with strictly positive wages below 300 Naira per hour. All standard errors are clustered at the village level.

We now make an additional observation which, along with our model, implies another testable prediction on enforcement patterns. Recall that our sample includes rebels with and without security contracts. We interpret contract receipt as a sufficient condition for incentive provision against oil theft. Conversely, the non-receipt of security contracts is a necessary but insufficient condition for tacit allowance of oil theft. Among non-contracted rebels, there are both militarily weak groups who are credibly enforced at a low payoff without explicit contracts and militarily strong rebels who are tacitly allowed to steal. Thus, we expect enforcement to be lowest in areas where strong rebels were not provided with contracts:

Prediction 4. *Enforcement intensity is negatively correlated with military capability in non-contracted locations and uncorrelated otherwise.*

[46]We demonstrate in Figure D10, SI D.5 that the nonlinearity of oil theft enforcement to costs in Figure 8 is robust to choice of bandwidth and sample trimming.

Columns (2)-(4) in Table 6 interact the amnesty treatment indicator with our measure of rebel military strength. In the full sample (column 2), we do not observe any relationship between enforcement and military strength in amnestied regions. However, this aggregate zero effect masks heterogeneity by incentive provision status. Columns (3)-(4) in Table 6 split the sample by contract status to demonstrate exactly this pattern. Post-amnesty oil theft enforcement is decreasing in rebel strength only in the non-contracted villages in column (3).[47] In the contracted sample in columns (4), this relationship is precisely zero.

Finally, recall from Section 2 that the competitive fringe is associated with domestic refining operations.

Prediction 5. *Enforcement against illegal refineries increases differentially where security contracts were granted.*

In locations where security contracts were granted, rebels abstained from oil theft and the fringe increased their activity, leading to greater government enforcement efforts against refining operations. To test this prediction we regress various enforcement outcomes on interactions between the time-varying amnesty treatment and an indicator for the award of security contracts to the nearest amnestied rebel commander. Results are reported in Table 7, with spatial errors in OA B.7, Table B8. Column (1) uses all oil theft enforcement actions as outcome and finds no differential increase in areas where a security contract was provided. Next we disaggregate enforcement by illegal activity. In column (2), we estimate that contracts are associated with a small but insignificant reduction in enforcement on militant-controlled export activities, consistent with militant exit.

However, column (3) show contracted areas actually see a large, two-fold *increase* in raids on illegal refineries relative to non-contracted amnestied areas, though the effect is only significant with clustered and 10km spatial errors. Since refining is an indicator of fringe activity, this suggests entry when rebels are provided incentives to exit the illegal market. Column (4) use enforcement on crimes unrelated to the oil sector as a placebo test, finding no differential effects by contract status.[48] The patterns are therefore unlikely to be driven by differential trends in unobserv-

[47] This relationship is significant at 10, 5, or 1% depending on the chosen standard errors, see Table B7.

[48] Examples of non-oil crime are armed robbery, kidnapping, extortion, and fraud.

ables that affect effort cost of law enforcement provision in general.

Table 7: The effect of amnesty on the supply of law enforcement by contract

Law enforcement activity	All theft	Export	Refining	Non-oil
	(1)	(2)	(3)	(4)
Amnestied × Post-amnesty	0.120***	0.094***	0.029**	-0.210***
	(0.026)	(0.010)	(0.014)	(0.044)
Amnestied × Post-amnesty × Security contract	-0.022	-0.017	0.064***	0.025
	(0.033)	(0.011)	(0.018)	(0.051)
Control mean	0.069	0.031	0.019	0.035
Spatial standard errors (50 km)				
Amnestied × Post-amnesty	0.109	0.033	0.051	0.121
Amnestied × Post-amnesty × Security contract	0.117	0.042	0.046	0.119
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls × Year FE	Yes	Yes	Yes	Yes
Number of villages	5111	5111	5111	5111
Observations	61332	61332	61332	61332
R^2	0.394	0.289	0.308	0.414

Note: Standard errors in parentheses are clustered at the village level. Spatial standard errors are calculated following Conley (2010). Outcome variable is the count of law enforcement actions targeting a particular illegal activity within 5km of a village. The type of illegal activity is given in the table header. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group is all villages outside of this range. Surveillance contract is an indicator that equals one if the nearest amnestied militant commander received a pipeline security contract. Controls include distance to nearest oilfield, distance to nearest pipeline, distance to state capital, distance to Niger River, distance to the coast, and population density. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

7 Alternative explanations

The results in Section 6 provide evidence that the post-amnesty increase in oil theft was caused by discontinuous changes in the government's decision to provide incentives for rebels not to steal. The government decision was in turn a response to the interaction between the credibility costs of rebel military capability and local black-market cost conditions. We briefly review plausible alternative explanations for our results and show that they are not supported in the data. Further details and results are reported in OA B.8-B.13.

Tribal politics. We argue that amnesty and security contracts were granted

on the basis of proven military capability and local economic conditions, not tribal politics. Possibly the amnesty simply reflected favorable treatment of certain tribes. In particular, as several powerful commanders were Ijaw, as was the 2010-2015 President Goodluck Jonathan, the results in Section 6 may have followed incidentally.[49] We include ethnic homeland-by-year fixed effects (Murdock 1967) in our main DD specification (Equation 1) as a robustness test in Table B9, OA B.8. Across specifications the effect of amnesty on oil theft falls by 10%-35%, though it remains significant at 1%. This pattern obtains controlling for Ijaw identity or all groups and using militant-affected or all non-amnestied villages as a control group. It is therefore unlikely that the amnesty was motivated solely by tribal politics.[50]

Agency problem. In our model the inverse-U shape of enforcement intensity in cost conditions is caused by the government's extensive margin decision to grant rebels with incentives against oil theft. An agency friction provides a plausible alternative intensive margin explanation for the observed relationship, where the principal (federal government) prefers to enforce the law against oil theft while their agent (police, military) may be bribed by black market participants. In that case low-cost locations generate more surplus with which to bribe irrespective of the militarily capability of local rebels, while high-cost areas have small markets requiring limited enforcement, generating a similar non-linear response. In Figure B8, OA B.9 we show that the inverse-U shape does not obtain among rebels with security contracts (holding incentive provision constant) but obtains among the non-contracted (where incentive provision varies) suggesting the observed non-linearity is unlikely to be caused by solely by an agency problem.

Oil theft spike. The black market for stolen oil grew gradually following the amnesty, with oil theft from pipelines reaching a maximum level in 2014, some five years after amnesty. The conventional explanation for this timing, detailed in Section 2, is that the increase in oil theft is due to an illegal sector demand shock caused by falling legal refinery output and the inability to import sufficient quantities of refined products. This explanation finds mixed support in the data. Ta-

[49]The Ijaw are the Niger Delta's largest and most prominent tribe; population estimates vary.

[50]The results are consistent with qualitative reporting. For instance Ebiede, Langer, and Tosun (2020) write that "[i]t is commonly argued that the amnesty program favored armed groups that were predominantly members of the Ijaw ethnic nationality [... the] reason for this is because militancy was mainly concentrated in Ijaw communities in the Niger Delta." See also Asuni 2009b who stresses that MEND was a multi-ethnic coalition of commanders.

ble B10, OA B.10 summarizes the results from a triple-difference regression shows that while oil theft does increase during gasoline shortages, it does not do so differentially in amnestied areas, nor does it affect the main amnesty estimate.

Redistributive politics. We argue that the amnesty and security contracts were rent-sharing agreements primarily benefiting federal elites, militant commanders, and to a lesser extent militant fighters. Conceivably, the government's tacit acceptance of oil theft may have reflected a wider redistributive motive, with the government seeking to meet popular demand for broader access to petroleum rents. If true, we would expect amnesty contract status to generate income and consumption effects among local households. To test this proposition we exploit panel data from the General Household Survey (GHS) to regress municipality-level average household consumption on amnesty and security contract receipt interacted with year dummies and fixed effects. The results are in Table B11, OA B.11 and show no discernible effect of amnesty or security contract status on local consumption.

Network effects. We measure rebel military strength by the density of local allied camps along the network of oil pipelines. Plausibly, central locations in the pipeline network, where multiple delivery pipelines join the trunkline transporting oil to the export terminals, are more vulnerable both to attack and later to oil theft, and may therefore be strategically amnestied. The existence of such a network effect might also induce error in the measurement of military capability or even spuriously produce the results in Section 6. We show in Table B12, OA B.12 that such choke points are indeed strategic attack sites, but have no independent effect on post-amnesty oil theft.

Wages endogenous to oil theft. We measure local black market costs with local wages for young men, among others. These wages are potentially endogenous to oil theft due to labor demand effects, biasing our empirical results on black market costs toward zero. We examine the endogeneity of wages in Table B13, OA B.13. We find no significant relationship between local wages and oil theft across specifications with time, state, and village fixed effects, or when instrumenting for oil theft using distance to pipelines interacted with national oil theft levels.

Crime displacement. The differential increase of oil theft in amnestied locations could plausibly be due to a general crime displacement effect. The amnesty provides incentives to reduce militancy. Former rebels may then turn to alterna-

tive forms of crime, e.g. oil theft, to supplant their loss of income. Commanders who received a security contract may be able to deter crime, displacing oil theft to non-contracted but amnestied areas. However, this effect should also obtain for common non-oil crimes as well, such as kidnapping and armed robbery. We test the crime displacement hypothesis in Table B14, OA B.14 using a triple-difference regression with non-oil crime as outcome and including various interactions of amnesty- and contract status. We find no evidence of a crime displacement effect.

8 Conclusion

This paper argues that a state may tacitly allow black markets to develop as a mechanism for sharing rents with armed groups. For militarily powerful rebels the government's cost of armed intervention – and therefore credible incentive contracts – is high. Favorable local cost conditions further erode the benefit of restraining rebel groups by facilitating entry of small-scale black market entrepreneurs. We hypothesize that rebel military capability and local cost conditions combine to render excessive the price of providing credible incentives against rebel oil theft. To test this hypothesis we marshal extensive and novel data on armed groups, black markets, and government law enforcement from the Niger Delta oil conflict, amnesty, and its aftermath. The discontinuous change in incentive provision predicts five nonlinear equilibrium relationships between local costs, rebel military strength, oil theft, and enforcement. Considered jointly, the data offer strong support for our proposed equilibrium-switching mechanism in explaining the post-conflict growth of black markets.

Our analysis illustrates the importance of treating capacity for violence, local economic conditions and resource theft jointly when studying the political economy of black markets in conflict zones. Our results also echo the longstanding observation that monopoly control of illicit markets may be preferred by government to decentralized competition when entry costs are low, highlighting the importance of black market structure in determining law enforcement incentives.

An important implication of our work is that socially efficient outcomes are more likely where government more fully internalizes the efficiency costs of illegal activity. Greater ownership of resource revenue and internalization of the social

and environmental costs of theft will increase the state's willingness to transfer rents directly. A countervailing force, however, is the entry of new combatants in response to existing bargains, which, like entry in the black market, reduces the space for efficient rent-sharing. We view these issues as fruitful opportunities for future research on the economics and politics of black markets in conflict.

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A Model

We now proceed to i) formally define the game and ii) then derive subgame perfect equilibria (SPE) of the finitely and infinitely repeated game. Let the action space of the rebel be $R := \{\text{accept, accept and steal, reject}\}$. For the government it is without loss of generality to consider offers $x \in G := [r - \rho, g]$ as the government prefers fighting to offering $x > g$ and the rebel's best response is to reject any $x < r - \rho$ and fight. Define $A := R \times G$ as the action space and

$$V := \{(\pi - x, x) : x \in [r, g]\} \subset \mathbb{R}^2$$

the set of individually rational payoffs. A history $h_t \in A^t$ of a repeated game denote actions taken from periods 1 up to $t - 1$, where $A^0 = \emptyset$. Let $\Sigma_G^t := \{\sigma_{G,t} : A^t \to G\}$ and $\Sigma_R^t := \{\sigma_{R,t} : A^t \cup G \to R\}$ the set of time-t strategies for government and rebel, respectively. Let $\Sigma^t := \Sigma_R^t \times \Sigma_G^t$ the strategy space.

A.1 Subgame perfect equilibria of the finitely repeated game

Proposition 1. *The finitely repeated game ($1 \leq T < \infty$) admits a unique SPE where the government offers $x_s = r - \rho$ and the rebel accepts and steals.*

Proof. We proceed by backwards induction, considering first the final period T. For any equilibrium strategy profile the continuation payoffs in T are equal to 0. Now consider deviations $x \neq x_s$ from the proposed equilibrium offer x_s. The rebel's best response to any offer $x < x_s$ is to reject and fight, yielding an inferior payoff $\pi - g < \pi - (r - \rho)$ to the government. Therefore an equilibrium offer satisfies $x \geq x_s$, for which the rebel's best response is to accept and steal. But the government's payoff is decreasing in x so their best response to anticipated oil theft is to offer $x = x_s$. Hence the proposed offer obtains uniquely in period T for any subgame perfect equilibrium. Now consider the incentives in period $T - 1$ and notice that continuation payoffs of any equilibrium strategy are $(\pi - r + \gamma - \rho, r)$. By the preceding argument we have that in any subgame perfect equilibrium the government offers $x_s = r - \rho$ and the rebel accepts and steals in $T - 1$. Continuing recursively for periods $T - 2, \ldots, 1$ we conclude that the proposed equilibrium obtains uniquely in each subgame. □

A.2 Subgame perfect equilibria of the infinitely repeated game

We show that the payoffs supported by equilibria without oil theft tend to the set of individually rational payoffs V as $\delta \to 1$. To ease notation let

$$\tilde{r} = \rho \frac{1-\delta}{\delta - \delta^T} + r\delta \frac{1-\delta^T}{\delta - \delta^T} \tag{5}$$

where $\lim_{T\to\infty} \lim_{\delta\to 1} \tilde{r} := r$.

Proposition 2. *Any payoff $\tilde{v} \in \tilde{V} := \{(\pi - x, x) : x \in [\tilde{r}, g]\}$ of the infinitely repeated game may be supported as a SPE without oil theft as $\delta \to 1$.*

Proof. Consider, for all periods $t \in 1, 2 \ldots$, the following strategy profile: The government offers $x_1 = x^*$ in period 1 and else in $t > 1$ if the rebel accepted without theft in $t-1$. If $x_{t-1} \geq x^*$ and the rebel accepts with theft then offer $\underline{x} = r - \rho$ the next $T - 1 \geq 1$ periods and then offer $x_{t+T} = x^*$. The rebel accepts $x_t \geq x^*$ without theft, $x_t \in [r - \rho, x^*]$ with theft, and rejects otherwise. If the rebel accepted an offer $x_t \geq x^*$ with theft then accept with theft any $x_t \geq r - \rho$ and otherwise reject $x_t < r - \rho$ for the next $T - 1$ periods.

We now verify that there are no profitable one-shot deviations from the proposed strategy. Begin with the rebel and notice that for offers $x_t < r - \rho$ and $x_t \in [r - \rho, x^*)$ the equilibrium actions of rejection and acceptance with theft are weakly best responses of the stage game. Accepting with theft is not preferred if $(1-\delta)(x^* + \rho) + \delta(1-\delta^T)r + \delta^T x^* \leq x^*$ which implies $x^* \geq \tilde{r}$. For the government offers $x_t > x^*$ are dominated by the equilibrium offer. Offers $x_t < r - \rho$ that induce rejection yield $\pi - g \leq \pi - x^*$ and are not preferred as $x^* \leq g$. Among offers $x_t \in [r - \rho, x^*)$ the government maximizes payoffs with an offer of $r - \rho$, yielding $\pi - \gamma - r + \rho$. Offers $x_t < r - \rho$ yield $\pi - g \leq \pi - \gamma - r + \rho$ and are therefore not preferred. It follows that the government prefers offers $x = r - \rho$ during the $T - 1$ periods of punishment as a greater offers transfers more surplus to the rebel and an inferior offer yields conflict. □

Rebels must be given a rent $\tilde{r} - r$ to compensate for abstaining from theft. This rent may become arbitrarily small with long punishments ($T \to \infty$) and a high discount factor ($\delta \to 1$) such that supported payoffs $\lim_{T\to\infty} \lim_{\delta\to 1} \lim \tilde{V} = V$ go to the set of individually rational payoffs in the limit.

A.3 Credibly enforced equilibria

The SPE considered in Appendix A.2 above featured punishments where the rebel surrendered rents to the government through negative transfers. We now consider a subset of SPE without oil theft where the government expects that *i*) the rebel rejects any offer inferior to the equilibrium offer and *ii*) oil theft will continue if not punished while *iii*) the rebel does not expect a return to peaceful rent-sharing until the punishment is concluded. Held jointly these beliefs imply that the threatened off-equilibrium punishment must feature fighting:

Proposition 3. *Consider, for all periods $t \in 1, 2, \ldots$ the following strategy profile: The government offers $x_t = x^*$ in period 1 and else in $t > 1$ if the rebel accepted without theft in $t-1$. If the rebel accepts with theft then offer $\underline{x} < r - \rho$ the next $T-1 \geq 1$ periods and then offer $x_{t+T} = x^*$. If ever having offered $x_t \geq x^*$ within $T-1$ periods of oil theft then again offer $\underline{x} < r - \rho$ the next $T-1$ periods and then offer $x_{t+T} = x^*$. If the rebel ever accepts any $x_t < x^*$ then let $x^* = \tilde{r}$ forever. The rebel accepts $x_t \geq x^*$ without theft and rejects otherwise. If $x_t \geq x^*$ within $T-1$ periods of oil theft then accept with theft. If ever accepting $x_t < x^*$ then accept all offers $x \geq \tilde{r}$ without theft forever. This strategy profile is a SPE of the infinitely repeated game if $\tilde{r} \leq x^* \leq g$ and $\delta \to 1$.*

Proof. We verify that there are no profitable one-shot deviations from the proposed strategy. For the government a deviation $x_t > x^*$ is dominated by the equilibrium offer. A deviation $x_t < x^*$ yields $\pi - g$. Let $J := (1-\delta^T)(\pi - g) + \delta^T(\pi - x^*)$ the government's discounted, present-valued payoff following a rebel deviation with theft. The government prefers to fight if $J \geq (\pi - x^* - \gamma)(1-\delta) + \delta J$, an incentive condition that holds when $\delta \to 1$. Turn to the rebel. Rejecting equilibrium offers is not preferred as $x^* \geq \tilde{r} \geq r$ the payoff from fighting. The rebel rejects an inferior offer is rejected when $(1-\delta)r + \delta x^* \geq (1-\delta)x^* + \delta\tilde{r}$ implying $x^* \geq \tilde{r}$ as $\delta \to 1$. The rebel prefers to accept without theft to accepting with theft when $(1-\delta)(x^* + \rho) + \delta(1-\delta^T)r + \delta^T x^* \leq x^*$ implying $x^* \geq \tilde{r}$. Suppose finally the rebel accepted with theft in t and the government offers x^* before $t+T$. Anticipating $T-1$ periods of fighting regardless the rebel strictly prefers to accept with theft. □

We now introduce our equilibrium selection criterion of credible enforcement, capturing the idea that threats of costly punishment are credible when the opportunity cost of conflict over accepting oil theft is low. We propose that the salient

equilibrium condition in the no-commitment context is for the government's ex-ante threat of costly punishment to be credible. We say an equilibrium is credibly enforced if the offer is bounded below by

$$x^* \geq x_c := \{x \geq \tilde{r} : \pi - x - \gamma \leq \pi - g\} \rightarrow x_c \geq \max\{\tilde{r}, g - \gamma\} \tag{6}$$

in which case the government's off-equilibrium threat of fighting satisfies their incentive condition for any duration of punishment $T - 1$ and discount factor δ. Moreover with a credibly enforced offer the government strategy in Proposition 3 may be simplified: By construction there is no incentive for a deviation to an offer $x_t \geq x^* \geq x_c$ during punishment, so therefore it is not necessary for the strategies to anticipate a resumption of conflict. The credibly enforced equilibrium is sustained only by the expectation that rebels reject inferior offers and continue oil theft until punished. By solving $x(\omega) \geq x_c$ with respect to ω the least credible offer x_c maps into a restriction

$$\omega^* := \min\left\{1, \frac{\gamma}{g - \tilde{r}}\right\}$$

on the Nash bargaining weight ω in Equation (2).

When threatening a militarily weak rebel with long punishment under high discounting, $g \leq \underline{g} := r + \gamma$ the implied bargaining weight $\lim_{T\to\infty} \lim_{\delta\to 1} \omega^* = 1$ and the government can credibly enforce no oil theft while capturing the entire surplus. But as fighting becomes costly relative to losses from oil theft the government must concede more surplus to reduce the opportunity cost of fighting. Thus there may exist equilibria with oil theft that the government weakly prefers to credible enforcement if $\pi - (g - \gamma) \leq \pi - (r - \rho) - \gamma \rightarrow g \geq r + 2\gamma - \rho := \overline{g}$.

A.4 Microfounding fringe oil theft

We derive a microfoundation for fringe displacement ϕ. We assume that fringe oil theft occurs irrespective of conflict and that there is no strategic interaction between government and fringe. Let P the price of crude oil and Q total local oil output, both exogenously fixed. A rebel chooses oil theft along the extensive margin, $q_r \in \{0, q\}$ where $0 < q < Q$. The representative fringe maximizes profits, taking $z \in \mathbb{R}_+$ at quadratic cost $Cq_f^2(2[Q - q_r])^{-1}$, tractably capturing the notion

of decreasing returns to scale in total theft. We assume $C \geq P$ to ensure that profit maximization yields an interior solution. The first order condition may be solved for equilibrium fringe theft:

$$q_f^* = \frac{P}{C}(Q - q_r) =: \arg\max_{q_f} Pz - \frac{Cq_f^2}{2(Q - q_r)} \tag{7}$$

so fringe theft increases linearly in residual output after rebel theft by a factor $\phi := P/C \in [0, 1]$.

A.5 Optimal enforcement effort

We derive a microfoundation for optimal enforcement effort e^*. The government enforces a location with effort $e \geq 0$ at cost $c(e)$ such that with probability $p(e)$ the fringe is interdicted and does not steal in that period, leaving the government with more surplus. The fringe, if interdicted, is replaced in the following period. We assume that the government's enforcement effort affects only the extensive, and not the intensive, margin of the fringe oil theft. Conditional on the fringes' participation in the black market let the probability of successful interdiction be strictly increasing- and quasi-concave $\partial p(e)/\partial e > 0$, $\partial^2 p(e)/\partial^2 e^2 \leq 0$ and costs strictly increasing and quasi-convex $\partial c(e)/\partial e > 0$, $\partial c(e)/\partial e \geq 0$, $\partial^2 c(e)/\partial^2 e^2 > 0$. Then optimal enforcement intensity

$$e^* := \left\{ e : \frac{\partial p(e)}{\partial e} P q_f^* = \frac{\partial c(e)}{\partial e} \right\} = \arg\max_e p(e)\pi + (1 - p(e))(\pi - P q_f) \tag{8}$$

is increasing in the value of oil stolen by the fringe Pq_f^*, where using (7) it is immediate that optimal enforcement is decreasing in local costs.

ONLINE APPENDIX
— For Online Publication Only —

B Additional results and robustness tests

B.1 Regression discontinuity in time analysis of militant attacks

To investigate the dynamic short-run relationship between militant attacks and amnesty we estimate an regression discontinuity in time (RDiT, Hausman and Rapson 2018) using a monthly time series of militant attacks on the oil sector.[51] We estimate military attacks m_t for month t

$$m_t = \alpha_1 + \rho m_{t-1} + \theta 1(t \geq \tau) + g(t) + 1(t \geq \tau)h(t) + \delta_m + \epsilon_t$$

for $t \in [\tau - \Delta, \tau + \Delta]$, where Δ is the bandwidth, or event-window, which we vary across specifications, and $g(t)$ and $h(t)$ are flexible polynomial functions of time that vary with the post-amnesty indicator $1(t \geq \tau)$. We allow for an AR(1) process by including the lagged term m_{t-1}, with ρ the autocorrelation coefficient. Finally, δ_m is a fixed effect for either month-of-year or year. The former captures seasonal cycles in militant activity, while the latter accounts for year-specific spikes in attacks, either of which might also be correlated with amnesty timing.[52]

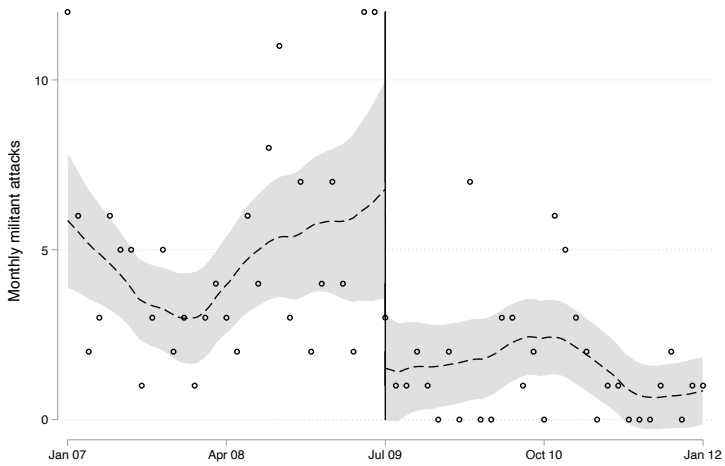
The identifying assumption is that the time trend of attacks would be continuous over the event-date in absence of amnesty. Practically, this requires that other policies or events did not occur in the month of amnesty to cause a precipitous drop in conflict. We also require that the amnesty has immediate effects on attacks, since the effect identified in the RDiT is inherently short-term.

Figure B1 plots monthly outcomes over time with a flexible local polynomial fit estimated separately on either side of the July 2007 cutoff, for an event window of $\Delta = 30$ months. The results indicate a statistically significant reduction in militant violence immediately following the amnesty. Given a relatively small window and

[51] This method has also been called both an interrupted time series or event-study model. However, following convention in applied microeconomics, we refer to difference-in-differences with dynamic treatment effects in a panel data setting as an "event-study regression".

[52] Note that since data is at the month level, they are collinear when included simultaneously with the functions of t, and so are included separately.

Figure B1: Rebel attacks on oil sector



Note: This figure shows the results of a nonparametric RDiT event-study for the July 2009 amnesty date, which is indicated by the vertical solid line. The scatterplot shows the monthly total of oil-related militant attacks over time from ACLED. Dotted lines indicate local polynomial time trends fitted on either side of the event-date cutoff, with shaded area indicating 95% confidence intervals.

sharp discontinuity at the exact event-date, it is unlikely that temporally correlated unobserved shocks are driving the drop.

Given the small number of aggregate time-series observations, there may be worries about the sensitivity of estimates to model specification. We therefore consider numerous specifications of the estimating equation, allowing the polynomial to vary from linear to third-order, and test the following event windows: full sample, 30, 20, and 10 months. The estimates are presented in Table B1, OA B.1 and show that the amnesty reduced militant attacks by roughly 3-7 events per month, a near-complete elimination of conflict from a pre-amnesty mean of 4.9. The estimated effect is robust to the inclusion of annual, seasonal effects and autoregressive terms. The results obtains in alternative models and is also robust to a battery of specification- and placebo tests.[53]

The RDiT estimates of amnesty on conflict are summarized in Table B1. Notice that the results are robust to the inclusion of annual shocks (columns 1, 3, 5, 7),

[53]We show in OA B.1 that the estimated effect is robust to *i*) placebo tests for all possible event-dates (Figure B2), *ii*) tests for outliers and influential observations (Figure B3), and *iii*) placebo tests of non-oil-related conflict outcomes (Table D1). In SI-D.2 we further show that the results are robust to the use of a control group, differences-in-discontinuities estimation, structural break tests, and AR(p) processes with optimal lag selection.

Table B1: The effect of amnesty on militant events

Polynomial	Linear		Quadratic		Quartic		AR(1)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Full sample								
Post-amnesty	-5.319***	-2.669***	-5.325***	-4.781***	-5.549***	-4.418***	-5.051***	-1.665***
	(1.594)	(0.570)	(1.725)	(0.917)	(1.953)	(1.284)	(1.655)	(0.554)
m_{t-1}							0.056	0.379***
							(0.117)	(0.094)
Month FE	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	No	Yes	No	Yes	No	Yes	No
Observations	276	268	276	268	276	268	275	267
R^2	0.506	0.271	0.506	0.385	0.510	0.403	0.507	0.375
$\Delta = 30$								
Post-amnesty	-5.363***	-3.814**	-8.136***	-8.233***	-5.303	-6.245**	-4.677**	-3.077*
	(1.865)	(1.537)	(2.796)	(2.397)	(3.551)	(2.730)	(2.069)	(1.663)
m_{t-1}							0.145	0.199
							(0.178)	(0.157)
Month FE	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	No	Yes	No	Yes	No	Yes	No
Observations	61	59	61	59	61	59	61	59
R^2	0.353	0.390	0.435	0.487	0.463	0.533	0.368	0.416
$\Delta = 20$								
Post-amnesty	-6.793***	-6.610***	-5.197	-7.076**	-5.551	-8.012	-8.005***	-6.953***
	(2.065)	(1.969)	(3.569)	(2.919)	(4.420)	(5.264)	(2.339)	(2.295)
m_{t-1}							-0.178	-0.050
							(0.246)	(0.203)
Month FE	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	No	Yes	No	Yes	No	Yes	No
Observations	41	41	41	41	41	41	41	41
R^2	0.376	0.524	0.383	0.535	0.396	0.543	0.393	0.525
$\Delta = 10$								
Post-amnesty	-4.704		-7.605		-2.738		-6.041	
	(3.528)		(5.065)		(5.577)		(4.491)	
m_{t-1}							-0.166	
							(0.347)	
Year FE	Yes	No	Yes	No	Yes	No	Yes	No
Observations	21	21	21	21	21	21	21	21
R^2	0.383	0.000	0.456	0.000	0.563	0.000	0.396	0.000

Note: Robust standard errors. Outcome variable is the number of monthly oil-related militancy events. Treatment is defined as an indicator for after July 2009. Window refers to the number of months included in the estimation before and after the event date. All windows apart from the full sample are symmetric. AR(1) specifications include a lagged dependent variable and a linear polynomial of event time. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

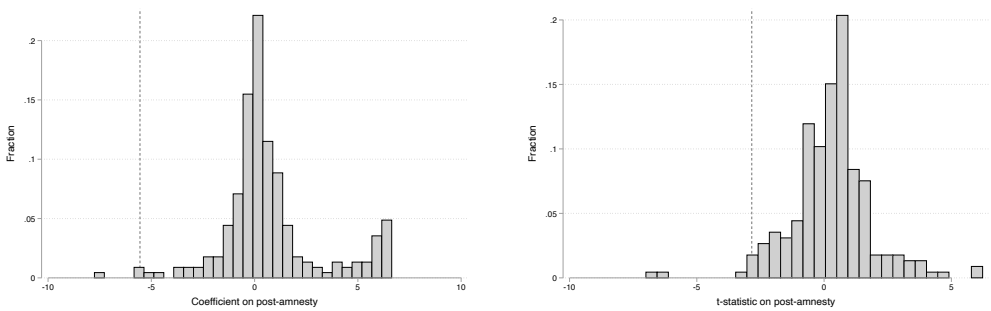
seasonal effects (2, 4, 6, 8) , and AR(1) persistence terms (7, 8). We now show that this effect is robust to the removal of influential observations and does not obtain for non-amnesty dates and non-oil conflict.

Placebo dates. In Figure B2 we consider whether July 2009 may have simply been a "lucky choice" of event date, and whether the results can be replicated with all other event dates. We re-estimate the event-study for 225 placebo dates and compare the estimate of $\hat{\theta}$ to this distribution of coefficients and t-statistics. We find that the results are not replicated by these placebo dates, and the estimated $\hat{\theta}$ falls in the left tail of this distribution.

Influential observations. A similar permutation-type test is conducted in Figure B3 to ensure that influential observations do not drive the results, by using a leave-one-out estimation procedure for each observation in the event window. Again, we plot the histogram of estimates and t-statistics, demonstrating that for any observation that is dropped, the results remain robustly negative and significant, allaying concerns that very high conflict levels immediately before the amnesty are entirely responsible for the observed effect.

Figure B2: RDiT placebo test

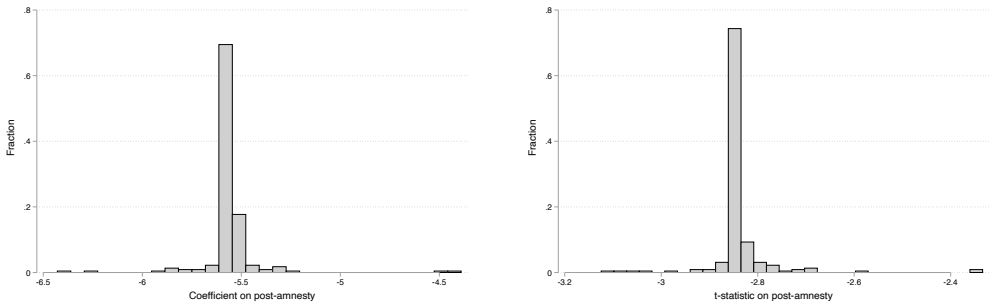
(a) Regression coefficient β (b) t-statistic



Note: This figure plots a histogram of the RDiT event-study placebo test for militant activity. We re-estimate the monthly RDiT equation with quadratic time trends and year fixed effects for each of 225 possible event-dates between July 1997 and April 2016. Panel (a) then plots the distribution of treatment coefficients from these regressions while Panel (b) plots the associated t-statistics. The dotted vertical line indicates the estimate for July 2009, the true amnesty date.

Figure B3: Event-study influential observations test

(a) Regression coefficient β (b) t-statistic



Note: This figure plots RDiT event-study estimates from a leave-one-out influential observations test. We re-estimate the monthly RDiT equation with quadratic time trends and year fixed effects, dropping each of 225 possible event-dates between July 1997 and April 2016. Panel A plots the distribution of treatment coefficients from these regressions while Panel B plots the associated t-statistics.

B.2 Differences-in-differences robustness to specification

We consider robustness of the main differences-in-differences (DD) estimation results in Table 3, Section 4 to various alternative specifications. Table B2 reproduces the main DD estimation results in column (1) and reports six alternative specifications.

The post-amnesty period featured steadily rising oil prices, plausibly increasing the profitability of oil theft. The increased oil prices may have created incentives for oil theft even in absence of amnesty, possibly violating the assumption of parallel trends. In column (2) we include the interaction of amnesty status with oil prices, with column (3) additionally including the full set of controls interacted with year fixed effects. We find in both specifications a significant, but small effect of oil prices on theft, and a slight increase in the main effect. While oil prices are clearly not solely responsible for the overall differential increase in oil theft, the results indicate that oil theft in militant-controlled areas responds positively to increased profitability.

The increase in oil theft may have driven by idiosyncratic local trends. Columns (4) and (5) include the interactions of municipality and village fixed effects with time. Column (6) replicates (5) with the inclusion of controls interacted with year

Table B2: Amnesty and oil theft, robustness to specification

Dependent variable	Oil theft						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Amnestied × Post-amnesty	0.448***	0.500***	0.470***	0.345***	0.400***	0.434***	0.163***
	(0.046)	(0.052)	(0.045)	(0.036)	(0.047)	(0.055)	(0.031)
Amnestied × Oil price (USD/barrel)		0.009***	0.004***				
		(0.001)	(0.001)				
Spatial standard errors (50km)	0.101	0.109	0.106	0.097	0.121	0.127	0.068
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	No
Village FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Year FE	No	No	Yes	No	No	Yes	No
Municipality FE × year	No	No	No	Yes	No	No	No
Municipality FE × Year FE	No	No	No	No	No	No	Yes
Village FE × year	No	No	No	No	Yes	Yes	No
Observations	61332	61332	61332	61332	61332	61332	61332
R^2	0.396	0.397	0.404	0.404	0.481	0.489	0.459

Note: Standard errors in parentheses clustered at the village level. Outcome variable is the number of oil theft incidents within 5 kilometers of the village. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group is all villages outside of this range. Controls are distance to nearest oilfield, distance to nearest pipeline, distance to state capital, distance to Niger River, distance to the coast, and population density. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

fixed-effects. All three specifications reveal significant and quantitatively comparable effects.

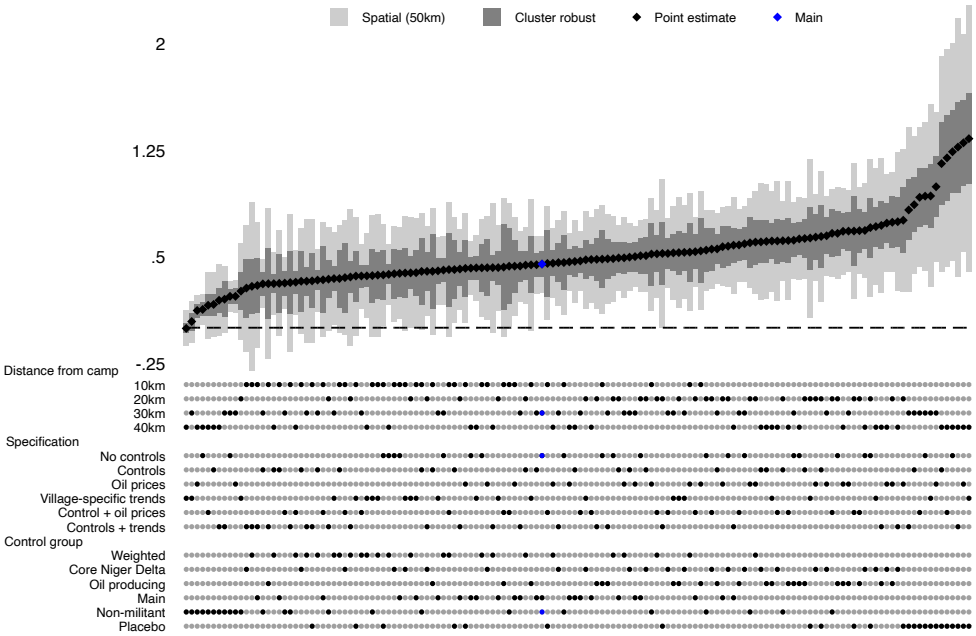
Finally, column (7) includes the interaction of municipality- and year fixed effects, reducing the effect of amnesty by some 64%. The inclusion of interacted municipality- and year fixed effects will mechanically account for much of the amnesty-induced variation in oil theft outcomes because only 5 of the 23 municipalities with any militant camp contain both amnestied- and non-amnestied camps, with remaining municipalities hosting camps that are either all amnestied or non-amnestied. Notice that R^2 essentially unchanged from columns (5) and (6).

In Figure B4, we plot estimates from 144 different specifications for the main difference-in-differences equation. We vary the distance from camp around which the treatment is defined, the specification of controls, and the choice of control group. Across nearly all specifications, the results are positive and significant. We also plot placebo coefficients that use malfunction oil spills as the outcome variable in gray; these generally cluster around zero.

B.3 Differences-in-differences evidence of pre-trends

We evaluate the evidence of pre-trends in the standard event-study regression:

Figure B4: Difference-in-differences oil theft specification plot

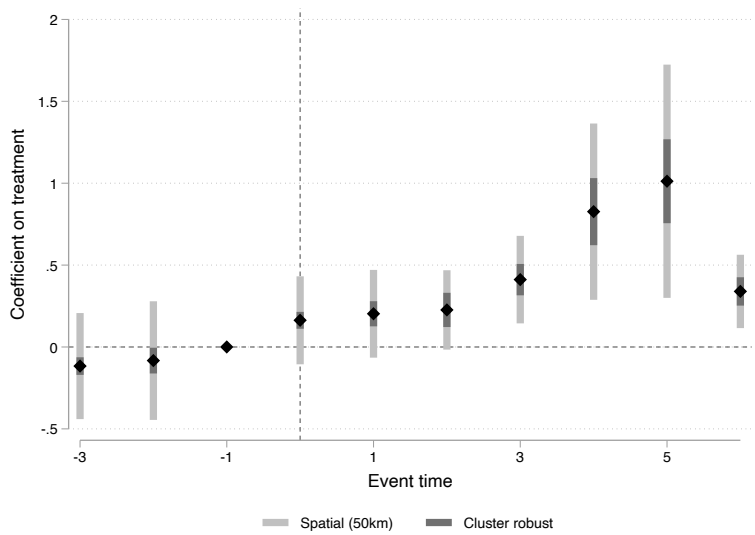


Note: This figure shows robustness across 144 specifications for a difference-in-differences regression of village-level oil theft on the amnesty treatment, as estimated in Equation 1. Each black point represents the estimated coefficient of an individual specification surrounded by 95% confidence intervals, with standard errors clustered at the village level. The preferred estimate is indicated in blue. Specification set is all combinations of: *i*) varying the distance d from an amnestied militant camp within which the treatment is defined from 20 to 50, *ii*) inclusion of controls, oil price interactions, village-specific linear time trends, and combinations of these *iii*) varying the control group between the main specification, the re-weighted synth-DD, the oil producing sample, the core Niger Delta sample, the sample within 30 km of a pipeline, and the militant placebo control group. Specification type is indicated in the figure footer.

$$y_{it} = \alpha + \sum_{\tau} \psi_\tau T_i^d D_\tau + \xi_t + \xi_i + X_{it}'\beta + \varepsilon_{it}. \tag{9}$$

The specification is identical to Equation (1) except that the time-varying treatment indicator is replaced by T_i^d interacted with leads and lags of the treatment period, D_τ. The ψ_τ are coefficients for each year τ before and after the amnesty, with $\tau = -1$ the omitted reference year. Figure B5, Panel A plots ψ_τ by year. The pre-amnesty coefficients are very near zero and precisely estimated. The similar pre-amnesty trends indicate that, in contrast to attacks on oil infrastructure, differential trends in oil theft are unlikely. The post-amnesty coefficients are significantly different from zero at the amnesty date and for all observations thereafter.

Figure B5: Oil theft, annual event-study coefficients



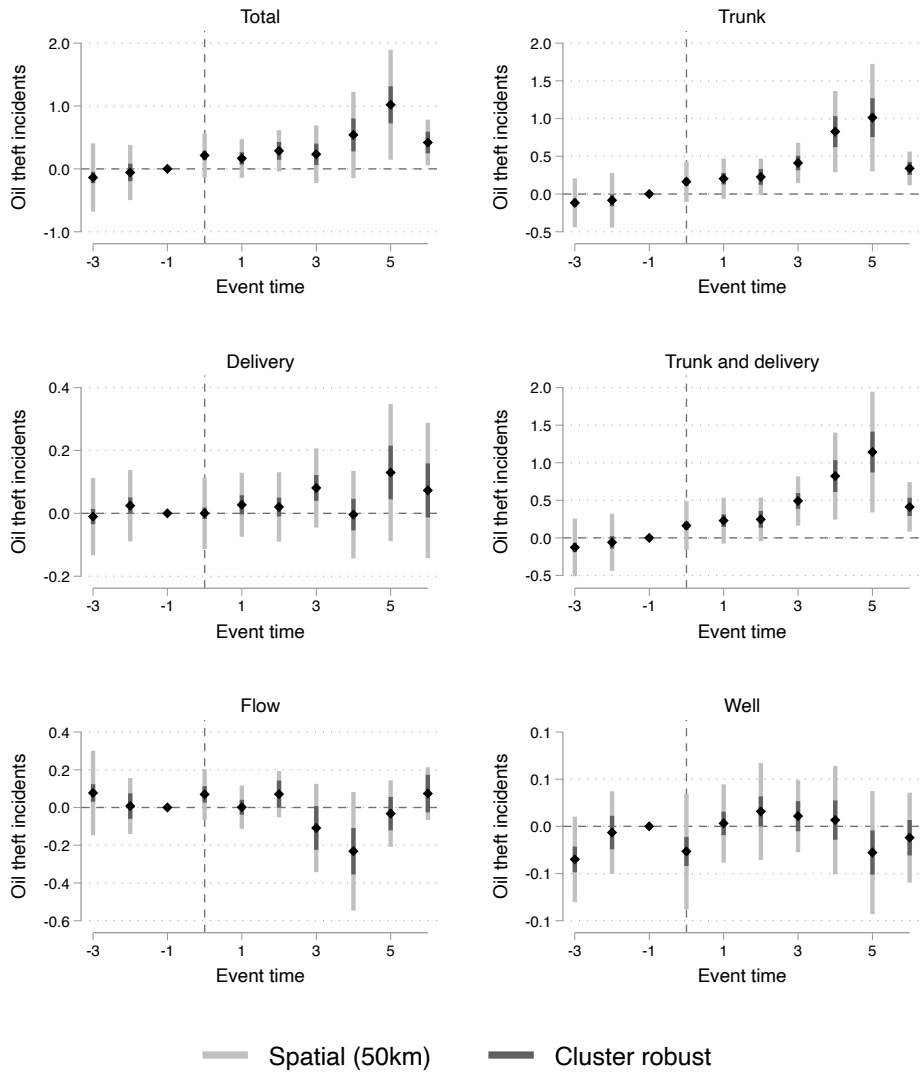
Note: This figure shows the coefficients and 95% confidence intervals from an event-study regression of oil theft on dummies for years pre-and-post amnesty, interacted with the treatment indicator, which equals one for villages within 30 km of an amnestied militant camp (see equation 9). The omitted reference year is 2008.

B.4 Differences-in-differences by infrastructure type

We re-estimate Equation (9) for components and aggregates of pipeline infrastructure types. Figure B6 plots event-study coefficients with 95% confidence intervals, clustered at the village level. Aggregating across infrastructure types (top

left) illustrates the differential oil theft in amnestied areas is due to trunkline theft (top right). The effect of amnesty on theft from lesser pipelines and the wellhead is more variable and smaller by an order of magnitude.

Figure B6: Amnesty impacts by oil infrastructure type



Note: This figure shows estimated event-study coefficients for oil theft incidents with 95% confidence intervals clustered at the village level for various permutations of oil infrastructure type. Total aggregates trunk, delivery, flow- and wellhead theft, remaining figures display various disaggregated components. Dashed vertical line indicates 2009, the year of the amnesty.

B.5 Heterogeneity by contract type

In Figure 4 we show that the post-amnesty spike in oil theft was concentrated primarily in territories where dominant militant groups did not receive security contracts. In Table B3, we estimate the magnitude and significance of these differential effects in a triple-difference regression framework that interacts a security contract indicator with $T_i \times \text{Post}_t$. Column (1) gives the base specification, (2) controls for distance to oil infrastructure and (3) for price effects, (4) includes a comprehensive set of interacted controls and (5) village-specific linear time trends, and finally (6) includes both controls and time trends. A comparison of triple-interaction and baseline coefficients reveals that security contracts reduce the effect of amnesty on oil theft by up to 56%. In columns (5) and (6), our most exacting specifications allowing for local trends, the security contract effect is quantitatively large and significant.

Table B3: Amnesty and oil theft by contract type

Dependent variable	Oil theft					
	(1)	(2)	(3)	(4)	(5)	(6)
Amnestied × Post-amnesty	0.538***	0.418***	0.590***	0.459***	0.594***	0.528***
	(0.094)	(0.081)	(0.101)	(0.051)	(0.088)	(0.062)
Amnestied × Post-amnesty × Security contract	-0.158	-0.046	-0.158	-0.076	-0.334***	-0.211***
	(0.102)	(0.089)	(0.102)	(0.063)	(0.100)	(0.065)
Spatial standard errors (50km)	0.169	0.147	0.175	0.110	0.213	0.138
	0.191	0.164	0.190	0.115	0.242	0.146
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Village FE	Yes	Yes	Yes	Yes	Yes	Yes
Infr controls	No	Yes	No	No	No	No
Price controls	No	No	Yes	No	No	No
All controls × Year FE	No	No	No	Yes	No	Yes
Village FE × year	No	No	No	No	Yes	Yes
Observations	61332	61332	61332	61332	61332	61332
R^2	0.396	0.398	0.397	0.404	0.482	0.490

Note: Standard errors clustered at the village level. Outcome variable is the number of oil theft incidents within 5 kilometers of the village. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group is all villages outside of this range. "Security contract" is a dummy variable equalling one if the commander of the nearest amnestied militant camp received an additional pipeline surveillance contract. Infr controls are distance to nearest oilfield and distance to the nearest pipeline interacted with the post-amnesty dummy. Price controls are all two-and-three way interactions between the annual international crude oil price, the treatment indicator, and the security contract dummy. "All controls" are distance to nearest oilfield, distance to nearest pipeline, distance to state capital, distance to Niger River, distance to the coast, and population density, which are included interacted with year fixed effects. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B.6 Robustness: Heterogeneous effects

We test robustness of the results in Section 6.1 to error in measurement of black market costs and military capability.

Black market costs: It is possible that young male wages may simply be a proxy for local economic conditions overall, and oil theft effects may be greater in poor communities for a variety of reasons unrelated to the fringe entry mechanism. In Table B4, we use wages of other demographic groups as a falsification test. For each triple-difference specification, we include interactions with median wages for old men or young women, with and without controls. If the result is spurious, wages of other groups than young men should produce placebo effects.

However, the results for young men's wages are negative and significant, while those for old men and young women are insignificant when controls are included, and much smaller than the young male estimates. Only wages in our demographic group of interest display evidence of significant heterogeneity, ruling out generalized income effects.

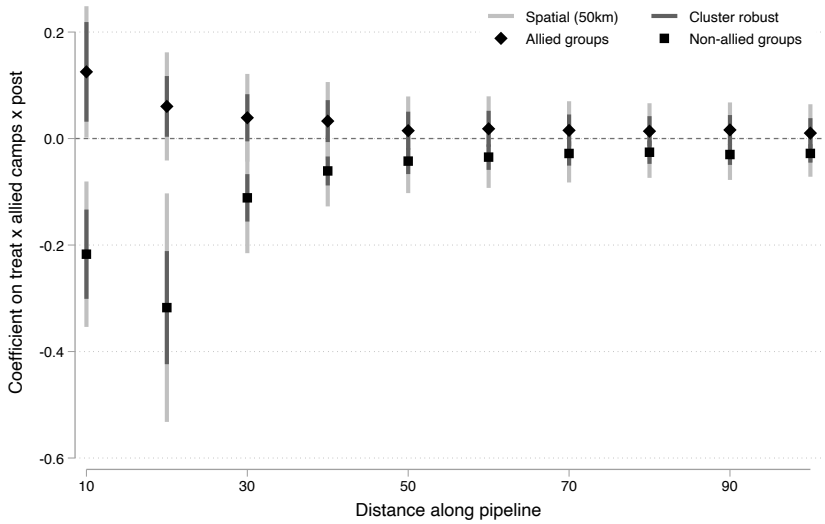
Table B4: Amnesty and oil theft by median wages and demographic group

Dependent variable	Oil theft					
	(1)	(2)	(3)	(4)	(5)	(6)
Amnestied × Post-amnesty	0.686***	0.824***	0.378***	0.437***	0.161*	0.348***
	(0.125)	(0.108)	(0.097)	(0.079)	(0.083)	(0.075)
Amnestied × Post-amnesty × Median wage, young men	-1.842***	-2.660***				
	(0.640)	(0.606)				
Amnestied × Post-amnesty × Median wage, old men			0.403	0.037		
			(0.427)	(0.446)		
Amnestied × Post-amnesty × Median wage, young women					3.039***	1.086
					(0.742)	(0.757)
Spatial standard errors (50km)	0.263	0.243	0.211	0.179	0.187	0.168
	1.381	1.276	1.026	1.018	2.059	1.953
Village FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Year FE	No	Yes	No	Yes	No	Yes
Observations	61332	61332	61332	61332	61332	61332
R^2	0.396	0.404	0.396	0.404	0.396	0.404

Note: Standard errors clustered at the village level. Outcome variable is the annual number of oil theft incidents. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group is all villages outside of this range. Controls include distance to nearest oilfield, distance to nearest pipeline, distance to state capital, distance to Niger River, distance to the coast, and population density. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Military strength: Figure B7 estimates the triple-difference effect of amnesty interacted with local military strength, varying the distance along the pipeline δ over which the local alliance measure is defined between 10 and 100 kilometers. We also estimate the interaction with local non-allies for comparison. The results

Figure B7: Heterogeneity by alliance network



Note: This figure shows the differential effect of amnesty on sabotage spills by density of allied and non-allied groups for varying distances along the pipeline network. The figure plots estimates and 95% confidence intervals from triple difference regressions where the model is re-estimated for varying δ_i from 10 to 100 kilometers. Marker shape indicates whether the interaction is on the number of local allies or non-allies.

indicate that military strength is fundamentally a property of local alliance density. As the geographical radius increases, the estimated triple interaction effect falls from positive and significant to zero. In other words, while nearby allies matter for military strength and ultimately oil theft, the global alliance network exerts a much smaller effect. A similar pattern obtains for non-allies, with the sign of the coefficients inverted.

B.7 Spatial correlation

We report spatial standard errors at the 10, 50, 100 and 500 kilometer radius for Tables 4, 5, 6 and 7 in Tables B5, B6, B7, and B8 respectively. Notice that the columns correspond to the specifications in original table, while the dependent variables are listed in each each panel. Panel headers indicate the distance threshold.

Table B5: The effect of amnesty on oil theft by cost factors

Dependent variable	Oil theft			
	(1)	(2)	(3)	(4)
10km				
Amnestied × Post-amnesty	0.124	0.137	0.199	0.299
Amnestied × Post-amnesty × Distance to Atlantic coast (00s km)	0.184			0.261
Amnestied × Post-amnesty × Distance to Niger River (00s km)		0.158		0.176
Amnestied × Post-amnesty × Median wage, young men			1.016	0.817
50km				
Amnestied × Post-amnesty	0.145	0.175	0.242	0.338
Amnestied × Post-amnesty × Distance to Atlantic coast (00s km)	0.231			0.322
Amnestied × Post-amnesty × Distance to Niger River (00s km)		0.206		0.213
Amnestied × Post-amnesty × Median wage, young men			1.250	0.967
100km				
Amnestied × Post-amnesty	0.147	0.170	0.230	0.330
Amnestied × Post-amnesty × Distance to Atlantic coast (00s km)	0.249			0.346
Amnestied × Post-amnesty × Distance to Niger River (00s km)		0.194		0.204
Amnestied × Post-amnesty × Median wage, young men			1.225	0.957
500km				
Amnestied × Post-amnesty	0.175	0.148	0.216	0.348
Amnestied × Post-amnesty × Distance to Atlantic coast (00s km)	0.319			0.432
Amnestied × Post-amnesty × Distance to Niger River (00s km)		0.139		0.183
Amnestied × Post-amnesty × Median wage, young men			0.977	0.764
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls × Year FE	Yes	Yes	Yes	Yes
Number of villages	5111	5111	5111	5111
Observations	61332	61332	61332	61332
R^2	0.401	0.400	0.401	0.402

Note: Spatial standard errors for Table 4, calculated following Conley (2010). Distance radius is given in left column. Outcome variable is the number of annual oil theft events within 5 kilometers of the village. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group is all villages outside of this range. Median wage of young men is the median hourly wage of men aged 10-40 in the local government area, measured in thousands of Naira in 2009. Controls include distance to nearest oilfield, distance to nearest pipeline, distance to state capital, and population density.

Table B6: The effect of amnesty on oil theft by alliance density and cost factors

Dependent variable	Oil theft					
	(1)	(2)	(3)	(4)	(5)	(6)
10km						
Amnestied × Post-amnesty	0.076	0.093	0.130	0.114	0.161	0.175
Amnestied × Post-amnesty × Allied camps, 10km	0.089	0.086	0.085	0.111	0.212	0.199
Amnestied × Post-amnesty × Median wage, young men					0.927	0.947
Amnestied × Post-amnesty × Allied camps, 10km × Median wage, young men					1.074	1.015
50km						
Amnestied × Post-amnesty	0.106	0.112	0.170	0.140	0.191	0.192
Amnestied × Post-amnesty × Allied camps, 10km	0.094	0.091	0.089	0.124	0.203	0.187
Amnestied × Post-amnesty × Median wage, young men					1.142	1.099
Amnestied × Post-amnesty × Allied camps, 10km × Median wage, young men					1.041	0.963
100km						
Amnestied × Post-amnesty	0.115	0.121	0.177	0.149	0.202	0.202
Amnestied × Post-amnesty × Allied camps, 10km	0.088	0.087	0.084	0.121	0.183	0.168
Amnestied × Post-amnesty × Median wage, young men					1.203	1.157
Amnestied × Post-amnesty × Allied camps, 10km × Median wage, young men					0.971	0.903
500km						
Amnestied × Post-amnesty	0.105	0.082	0.121	0.115	0.176	0.172
Amnestied × Post-amnesty × Allied camps, 10km	0.065	0.069	0.068	0.107	0.134	0.127
Amnestied × Post-amnesty × Median wage, young men					0.863	0.936
Amnestied × Post-amnesty × Allied camps, 10km × Median wage, young men					0.686	0.644
Village FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	No
Controls × Year FE	No	Yes	Yes	Yes	No	Yes
MEND controls	No	No	Yes	No	No	No
Density controls	No	No	No	Yes	No	No
Number of villages	5111	5111	5111	5111	5111	5111
Observations	61332	61332	61332	61332	61332	61332
R^2	0.396	0.404	0.404	0.404	0.396	0.398

Note: Spatial standard errors for Table 5, calculated following Conley (2010). Distance radius is given in left column.. Outcome variable is the number of annual oil theft events within 5 kilometers of the village. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group is all villages outside of this range. Median wage of young men is the median hourly wage of men aged 10-40 in the local government area, measured in thousands of Naira in 2009. Allied camps is the military strength of the nearest amnestied militant camp, measured as the number of allies within 10 kilometers along the pipeline. Controls include distance to nearest oilfield, distance to nearest pipeline, distance to state capital, and population density.

Table B7: The effect of amnesty on the supply of law enforcement by alliance density and local costs

Outcome	Anti-oil theft law enforcement			
Sample	Full		No contract	Contract
	(1)	(2)	(3)	(4)
10km				
Amnestied × Post-amnesty	0.108	0.053	0.084	0.078
Amnestied × Post-amnesty × Median wage, young men	0.554			
Amnestied × Post-amnesty × Allied camps, 10km		0.062	0.030	0.076
50km				
Amnestied × Post-amnesty	0.144	0.079	0.107	0.111
Amnestied × Post-amnesty × Median wage, young men	0.739			
Amnestied × Post-amnesty × Allied camps, 10km		0.086	0.036	0.101
100km				
Amnestied × Post-amnesty	0.140	0.078	0.102	0.107
Amnestied × Post-amnesty × Median wage, young men	0.749			
Amnestied × Post-amnesty × Allied camps, 10km		0.086	0.035	0.102
500km				
Amnestied × Post-amnesty	0.140	0.072	0.086	0.111
Amnestied × Post-amnesty × Median wage, young men	0.638			
Amnestied × Post-amnesty × Allied camps, 10km		0.079	0.030	0.106
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls × Year FE	Yes	Yes	Yes	Yes
Number of villages	5111	5111	2097	3014
Observations	61332	61332	25164	36168
R^2	0.394	0.394	0.516	0.362

Note: Spatial standard errors for Table 6, calculated following Conley (2010). Distance radius is given in left column. Outcome variable is the count of all anti-oil theft law enforcement actions within 5km of a village. Sample is indicated in table header. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group is all villages outside of this range. Controls include distance to nearest oilfield, distance to nearest pipeline, distance to state capital, distance to Niger River, distance to the coast, and population density.

Table B8: The effect of amnesty on the supply of law enforcement by contract

Law enforcement activity	All theft	Export	Refining	Non-oil
	(1)	(2)	(3)	(4)
10km				
Amnestied × Post-amnesty	0.074	0.027	0.033	0.112
Amnestied × Post-amnesty × Security contract	0.091	0.035	0.036	0.115
50km				
Amnestied × Post-amnesty	0.109	0.033	0.051	0.121
Amnestied × Post-amnesty × Security contract	0.117	0.042	0.046	0.119
100km				
Amnestied × Post-amnesty	0.115	0.034	0.054	0.107
Amnestied × Post-amnesty × Security contract	0.122	0.040	0.050	0.111
500km				
Amnestied × Post-amnesty	0.104	0.031	0.042	0.078
Amnestied × Post-amnesty × Security contract	0.147	0.036	0.062	0.078
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls × Year FE	Yes	Yes	Yes	Yes
Number of villages	5111	5111	5111	5111
Observations	61332	61332	61332	61332
R^2	0.394	0.289	0.308	0.414

Note: Spatial standard errors for Table 7, calculated following Conley (2010). Distance radius is given in left column. Outcome variable is the count of law enforcement actions targeting a particular illegal activity within 5km of a village. The type of illegal activity is given in the table header. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group is all villages outside of this range. Surveillance contract is an indicator that equals one if the nearest amnestied militant commander received a pipeline security contract. Controls include distance to nearest oilfield, distance to nearest pipeline, distance to state capital, distance to Niger River, distance to the coast, and population density.

B.8 Alternative explanation: Ethnicity

Plausibly, the amnesty simply reflected favorable treatment of certain ethnic groups – several powerful commanders were Ijaw, as was the 2010-2015 President Goodluck Jonathan. In Table B9, we augment our main DD specification with ethnic homeland fixed effects (Murdock 1967).

Columns (1)-(3) use the full control group whereas (4)-(6) restricts the control group to villages within 30 kilometers of a non-amnestied camp. Columns (1) and (4) include interactions between ethnic homeland fixed effects and linear year trends, columns (2) and (5) include only an interaction between the Ijaw and post-amnesty dummies, and columns (3) and (6) include the full set of interacted ethnic homeland and year fixed effects.

The effect of amnesty on oil theft remains positive and significant at 1% for all specifications. The interaction between oil theft and Ijaw identity a significant and positive, reflecting that Ijaw identity and militancy are themselves correlated.

Table B9: The effect of amnesty on oil theft: robustness to ethnicity

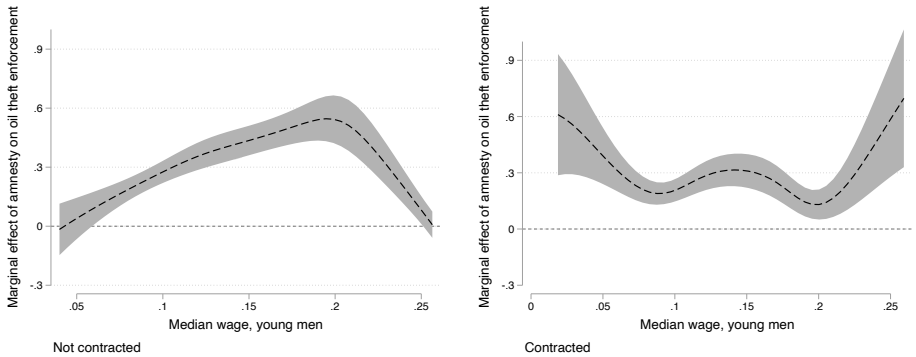
Dependent variable	Oil theft					
Control group	All			Militant active		
	(1)	(2)	(3)	(4)	(5)	(6)
Amnestied × Post-amnesty	0.402***	0.305***	0.289***	0.778***	0.861***	0.570***
	(0.035)	(0.033)	(0.034)	(0.079)	(0.093)	(0.059)
Ijaw × Post-amnesty		0.262***			0.417***	
		(0.050)			(0.064)	
Spatial standard errors (50km)	0.086	0.074	0.077	0.164	0.239	0.100
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Village FE	Yes	Yes	Yes	Yes	Yes	Yes
Tribe FE × year	Yes	No	No	Yes	No	No
Tribe FE × Year FE	No	No	Yes	No	No	Yes
Observations	61332	61332	61332	34116	34116	34116
R^2	0.396	0.396	0.402	0.402	0.402	0.408

Note: Standard errors in parentheses clustered at the village level. Outcome variable is total number of oil theft events within 5 kilometers of the village. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group in columns (1)-(3) is all villages outside of this range. Control group in columns (4)-(6) is all villages outside of this range but also within 30 km of a non-amnestied militant camp. Controls include distance to nearest oilfield, distance to nearest pipeline, distance to state capital, distance to Niger River, distance to the coast, and population density. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B.9 Alternative explanation: Agency problem

In our theoretical framework the inverse-U shape of enforcement intensity in cost conditions is caused by the government's selective decision to grant rebels with incentives against oil theft. An agency friction provides a plausible alternative explanation. The government (principal) prefers to enforce the law against oil theft while their agent (police, military) may be bribed by black market participants. In that case low-cost locations generate more surplus with which to bribe irrespective of the militarily capability of local rebels, while high-cost areas have small markets requiring limited enforcement, generating an inverse-U shape.

Figure B8: Heterogeneous enforcement outcome by contract status



Note: Figure shows the marginal effect of amnesty on anti-oil theft law enforcement activity by γ, measured as hourly wages for young men in the local labor market, and contract status. Wages are measured in thousands of Naira per hour. Marginal effects are estimated using a nonparametric kernel regression with a bandwidth of 30, after residualizing year and village fixed effects. Plot excludes outlier wages above 300 Naira per hour. The left-hand-side plots marginal effects for rebels who received security contracts whereas the right-hand-side plots marginal effects for rebel groups who did not receive a security contract.

We run a falsification test for this alternative explanation. Recall that contract receipt is a sufficient but not a necessary condition for incentive provision. The set of non-contracted rebels contain militarily powerful groups that are tacitly allowed to steal and weak rebels who are incentivized with small or no transfers. If the inverse-U pattern is indeed caused by the government's incentive provision decision we expect the inverse-U pattern to obtain only for non-contracted rebels

and be absent for contracted, among whom there is no variation in incentive provision on the extensive margin. We rerun the kernel regression to compute the marginal effects of amnesty on enforcement over the local wage distribution for rebels with- and without security contracts (Figure B8) and find that the inverse-U pattern obtains only for non-contracted rebels. The evidence suggests that the observed inverse-U pattern is unlikely to be caused primarily by an agency problem.

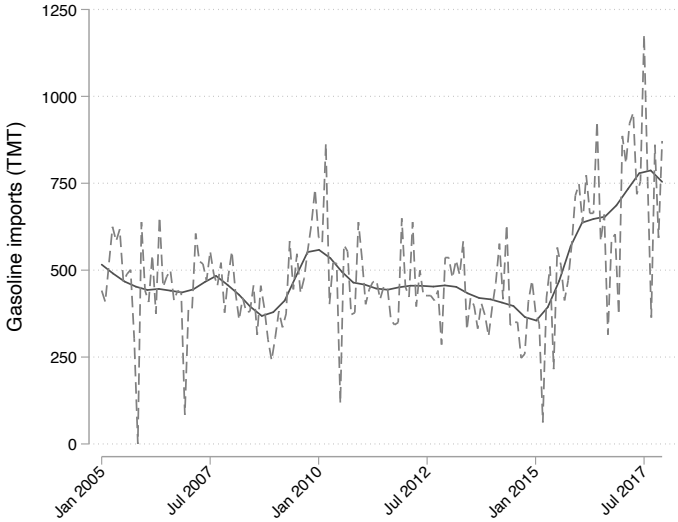
B.10 Timing of theft and fuel shortages

The results in Figure B5 show that the growth of oil theft in amnestied villages occurs only gradually after amnesty: event-study coefficients become significant after two years and peak in 2014, five years following the agreement. What accounts for this lag? We argue that militants and new entrants only began to take advantage of the amnesty incentives when exogenous economic conditions increased the profitability of oil theft. A key source of this profitability was a steadily worsening fuel crisis in the years following the amnesty – driven primarily by the federal government's inability to finance a costly fuel subsidy scheme and unwillingness to raise pump prices – which created space for black market activity.[54] Figure B9 plots monthly Nigerian imports of gasoline from 2005-2018, demonstrating a steady decline in import volumes from early 2010 to 2015, exactly the period over which the coefficients monotonically rise in Figure B5.

If the fuel crisis triggered the effects of amnesty, we expect that the effect of amnesty on oil theft is greatest in periods of fuel shortages. Table B10 tests this hypothesis in a triple-interaction regression that interacts $T_i \times \text{Post}_t$ with the log of national fuel imports and/or consumption. Column (1) reprints the main results, while (2) include the national-level fuel consumption measures interacted only with T_i, and finally column (3) estimates the full triple-interaction model. The results from (2) show that the amnesty itself has an independent effect on theft even after controlling for gasoline imports. However, the independent fuel consumption term is negative and highly significant, suggesting that oil theft spikes in amnestied regions during fuel shortages and falls as fuel supply constraints are eased. Nevertheless, column (3) suggests that the data do not support that the

[54] See contemporaneous news stories in Reuters, National Geographic, and The Financial Times for context.

Figure B9: Gasoline imports for Nigeria, 2005-2018



Note: Figure shows monthly gasoline imports for Nigeria from 2005-2018, measured in thousands of metric tonnes in dashed line. Solid line plots a local polynomial smoother.

effect of amnesty depends critically on fuel shortages.

B.11 Alternative explanation: Oil theft as local redistribution of petroleum rents

We argue that the amnesty and security contracts were rent-sharing agreements primarily benefiting federal elites and militant commanders. However, it is possible that the tacit acceptance of oil theft may reflect the government's attempt to meet popular demands for broader access to petroleum rents, in an effort to dissuade future militancy. If this is the case, we should expect to observe that local communities benefitted economically from the amnesty and/or security contracts. We test this hypothesis using data on household consumption from six rounds of the GHS conducted in the years 2007, 2008, 2010, 2011, 2013, and 2016. In Table B11, we regress mean annual household consumption at the local government area (LGA)-level on indicators of amnesty treatment and two-way fixed effects for

Table B10: The effect of amnesty on oil theft: fuel shortage

Dependent variable	Oil theft		
	(1)	(2)	(3)
Amnestied × Post-amnesty	0.448***	0.572***	5.075*
	(0.034)	(0.035)	(2.996)
Amnestied × Log gasoline imports		-0.904***	-0.407
		(0.079)	(0.340)
Amnestied × Post-amnesty × Log gasoline imports			-0.526
			(0.350)
Spatial standard errors (50km)	0.122	0.156	9.183
		0.341	1.015
			1.073
Year FE	Yes	Yes	Yes
Village FE	Yes	Yes	Yes
Observations	61332	61332	61332
R^2	0.396	0.397	0.397

Note: Standard errors in parentheses clustered at the village level. Outcome variable is total number of oil theft events within 5 kilometers of the village. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group is all villages outside of this range. Gasoline imports or demand are measured in thousand metric tonnes at the country-level annually. Controls include distance to nearest oilfield, distance to nearest pipeline, distance to state capital, distance to Niger River, distance to the coast, and population density. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

a sample of 117 Niger Delta LGAs.[55] We define an LGA as amnestied if it contains at least one amnestied militant camp, and do the same for security contract receipt.

Column (1) presents the main LGA-level difference-in-differences results. If anything, amnesty leads to a modest decline in consumption levels of roughly 10 USD per household (at 2009 official exchange rates), though the magnitude is small (4.7% of the sample mean) and the estimates are imprecise and not statistically significant. Column (2) provides event-study estimates for year dummies interacted with the treatment indicator; these coefficients follow no clear pattern, suggest a null impact of amnesty on local wellbeing. Columns (3) and (4) repeat the exercise using the receipt of security contracts – rather than amnesty alone – as the treatment indicator. Again, there is no clear pattern indicating a positive impact of amnesty, and none of the results are significant. Lastly, column (5) uses distance from the LGA to the nearest militant camp as a measure of treatment, in order to capture the possibility that some LGAs without camps within their boundaries are nevertheless exposed by proximity to amnestied militant camps. Again, we find no significant effect.

B.12 Alternative explanation: Network effects and amnesty

It is possible that government targeted amnesty to rebel groups occupying more central locations in the pipeline network, since these key nodes might constitute critical points at which militant attacks would inflict the greatest damage to oil supply. If these central points also have greater opportunities for oil theft, then the results in Section 6 may spuriously reflect this omitted targeting criterion.[56] We argue that the pipeline network is particularly vulnerable to disruption at points where multiple smaller delivery pipelines which connect to individual fields converge into larger flow and trunklines that ultimately transit oil to export terminals. Since attacks at these "choke points" disrupt oil flow from multiple different origins, they are attractive sites for attack.[57] We therefore operationalize network

[55] This is the lowest level for which geolocated information is available in the 2007 and 2008 GHS surveys.

[56] The effect on government incentive provision in our model is ambiguous because the cost of credible incentives are determined by rebel damage relative to losses from oil theft.

[57] We verify this proposition in the data. In a bivariate village-level regression of annual militant attacks on distance to oil pipelines (in 100km), the coefficient is -0.14 ($t = 8.63$). However, when we

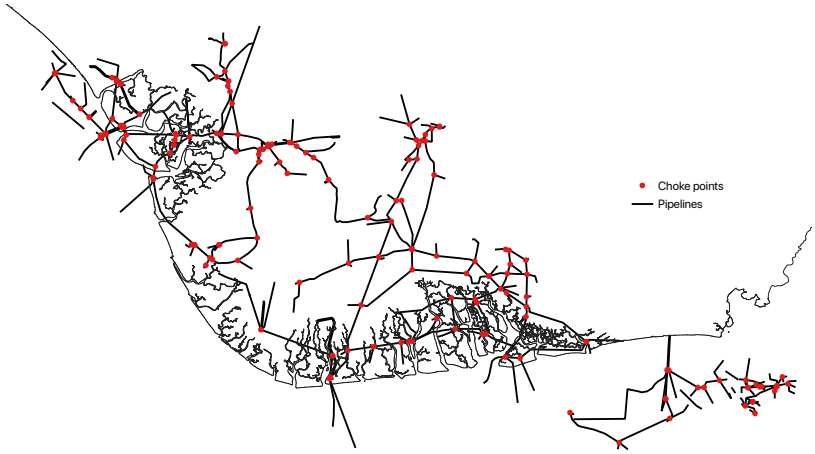
Table B11: The effect of amnesty on consumption

Dependent variable	Average consumption				
	(1)	(2)	(3)	(4)	(5)
Amnesty × Post-amnesty	-1516.882				
	(3577.198)				
Amnesty × 2008		5794.192			
		(4998.564)			
Amnesty × 2010		1776.087			
		(3583.072)			
Amnesty × 2011		1044.284			
		(3628.302)			
Amnesty × 2013		-2133.078			
		(4468.576)			
Amnesty × 2016		7175.820			
		(5137.402)			
Contract × Post-amnesty			-3283.944		
			(4046.363)		
Contract × 2008				6701.783	
				(6096.924)	
Contract × 2010				999.538	
				(4007.205)	
Contract × 2011				-2402.725	
				(3287.402)	
Contract × 2013				-3397.549	
				(4601.573)	
Contract × 2016				6838.004	
				(6064.286)	
Distance to amnestied camp (km) × Post-amnesty					-12.003
					(15.641)
Year FE	Yes	Yes	Yes	Yes	Yes
LGA FE	Yes	Yes	Yes	Yes	Yes
Observations	636	636	636	636	636
R^2	0.778	0.782	0.779	0.783	0.778

Note: Standard errors in parentheses clustered at the local government level. Outcome variable is average household-level annual consumption. Amnesty and contract treatment are defined having any amnestied or contracted militant camp within the boundaries of the LGA. Sample is all Niger Delta LGAs that appear in the 2007, 2008, 2010, 2011, 2013, and 2016 rounds of the Nigeria General Household Survey. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

centrality using choke points, defined as any point at which multiple pipeline segments intersect. These points are mapped over the pipeline infrastructure network in Figure B10.

Figure B10: Choke point map



Note: This figure shows the locations of choke points in the pipeline infrastructure of the Niger Delta.

This explanation is unlikely, since while choke points represent strategic attack sites, they do not have additional value as oil theft sites relative to large pipelines themselves. Nevertheless, we test for this omitted variable by including controls for distance to the nearest choke point at the village-level in Table B12. Column (1) includes a control for choke point distance interacted with amnesty, whereas column (4) includes choke point distance interacted with amnesty status. Columns (2), (3) (4) and (6) additionally include pipeline distance and their interactions. Notice that the independent effect of choke points in the second row disappear in columns (3) and (6) when including the interaction of controls and year fixed-effects. The results suggest that there is no independent effect of choke points once the dynamic effects of distance to infrastructure is accounted for.

include distance to choke pipeline as well, the coefficient on distance to any pipeline falls to -0.08 ($t = 6.67$) while the coefficient on distance to a choke point is -0.068 ($t = 9.37$). Therefore, these choke points represent attack sites that are independently attractive.

Table B12: The effect of amnesty on oil theft: choke points

Dependent variable	Oil theft					
	(1)	(2)	(3)	(4)	(5)	(6)
Amnestied × Post-amnesty	0.463***	0.458***	0.442***	0.957***	0.952***	0.925***
	(0.049)	(0.048)	(0.042)	(0.080)	(0.080)	(0.088)
Distance to choke point (00s km) × Post-amnesty	0.078	0.133***	0.268	0.300***	0.248***	0.279
	(0.053)	(0.050)	(0.367)	(0.052)	(0.043)	(0.219)
Distance to pipeline (00s km) × Post-amnesty		-0.144**	-0.513***		0.142***	-0.157
		(0.057)	(0.176)		(0.031)	(0.126)
Amnestied × Post-amnesty × Distance to choke point (00s km)				-3.624***	-1.833*	-2.049*
				(0.389)	(0.959)	(1.090)
Amnestied × Post-amnesty × Distance to pipeline (00s km)					-2.925**	-2.308*
					(1.219)	(1.329)
Spatial standard errors (50km)	0.131	0.130	0.124	0.213	0.213	0.212
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Village FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Year FE	No	No	Yes	No	No	Yes
Observations	61332	61332	61332	61332	61332	61332
R^2	0.396	0.396	0.404	0.397	0.397	0.405

Note: Standard errors clustered at the village level. Spatial standard errors calculated following Conley (2010) for first row. Outcome variable is total number of oil theft events in the field-year. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group in columns is all villages outside of this range. Choke points are locations at the intersection of more than one pipeline. Controls include distance to nearest oilfield, distance to state capital, distance to Niger River, distance to the coast, and population density. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

B.13 Endogenous wages

Section 6.1 analyzes the impact of local cost conditions – as measured by wages – on post-amnesty oil theft. It is possible, however, that wages in the local labor market are endogenous to oil theft and militant activity. Of course, this form of reverse causality would likely bias our results downward, since increased black market activity should increase labor demand, while we argue (and show empirically) that low-wage markets have stronger direct and indirect incentives for theft. Moreover, in Table 4 we find similar results for geographically-determined exogenous cost shifters like export costs, which should not be subject to reverse causality. Nevertheless, we test this hypothesis using data from Nigeria's General Household Survey (GHS), a comprehensive panel survey covering 500 Nigerian villages across six rounds from 2010-2016.[58] In Table B13, we regress village-level estimates for median hourly wages on the size of the local black market, as measured by the number of oil theft incidents within 10 km of the village.[59]

Column (1) shows the simple bivariate relationship between black market activity and wages over time at the village level, indicating a positive but insignificant correlation. However, this lack of relationship could be driven by omitted

[58] We restrict the sample to 123 Niger Delta villages.

[59] Results are similar for different geographic thresholds; available on request.

Table B13: The effect of oil theft on wages

Dependent variable	Median wages				
	(1)	(2)	(3)	(4)	(5)
Oil theft incidents	5.252	0.278	-8.440	24.446	2.394
	(8.765)	(8.484)	(7.520)	(17.883)	(16.880)
Round FE	Yes	Yes	Yes	Yes	Yes
State FE	No	Yes	No	No	No
Cluster FE	No	No	Yes	No	Yes
Mean Dep Var.	223.543	223.543	223.543	223.543	223.543
F-statistic				14.598	12.313
R^2	0.159	0.174	0.407		
Observations	688	688	688	688	688
Spec.	OLS	OLS	OLS	IV	IV

Note: Standard errors in parentheses clustered at the village level. Outcome variable is the median hourly wage in a village-round. Dependent variable is the total count of oil theft incidents within 10 kilometers of the village in the 6 months prior to the survey. Sample is 123 Niger Delta villages across six GHS survey rounds from 2010-2016. Instrument in column (4) is the distance between the village and the nearest oil pipeline. Instrument in column (5) is the interaction between the national-level count of oil theft incidents in the 6 months prior to the survey round and an indicator if the village is within 5 kilometers of an oil pipeline. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

variables and/or reverse causality. In columns (2)-(5), we employ a variety of strategies to isolate the *causal* effect of black market activity on wages. Columns (2) and (3) include state and village fixed effects, respectively, finding again no relationship between oil theft and wages. The results of column (3) are noteworthy, since they imply that growth in the black market does not predict changes in wages over time, suggesting the absence of a causal relationship in this direction. However, the fixed effects do not solve the reverse causality problem. In columns (4) we use an 2SLS estimation approach that instruments for oil theft with distance to nearest oil pipeline, a measure of access to theft targets. This produces a slightly larger though still insignificant coefficient. However, this instrument is not time-varying and so does not exploit the panel structure of the data. In column (5), our instrument for local oil theft interacts an indicator for whether a village is within 5 kilometers of an oil pipeline with the national-level trend in oil theft incidences. This produces a much smaller positive coefficient, which is still insignificant and is approximately 1% of the average wage. While the individual identification strategies may each have holes, taken together, the results do not suggest a causal relationship between oil theft incidence and wages that would generate a reverse causality bias for the results of Table 4.

B.14 Alternative explanation: crime displacement

To test whether amnesty led to differential increase in general crime across amnestied- and contracted locations we regress non-oil crime on interactions of the post-amnesty period with amnesty and contract status. Results are reported in Table B14. Columns (1)-(3) and (4)-(6) respectively consider the distances to nearest amnestied- or contracted camps, and amnesty- or contract treatment, both individually and jointly. We find no evidence of the crime displacement hypothesis in any of these specifications.

Table B14: The effect of amnesty and security contracts on other crime

Dependent variable	Non-oil crime					
	(1)	(2)	(3)	(4)	(5)	(6)
Distance to nearest amnestied camp × Post-amnesty	0.027		0.101			
	(0.263)		(0.442)			
Distance to nearest contracted camp × Post-amnesty		0.002	-0.068			
		(0.206)	(0.344)			
Amnesty × Post-amnesty				-1.080		-1.247
				(0.697)		(0.827)
Contract × Post-amnesty					-0.911	0.278
					(1.013)	(1.298)
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Municipality FE	Yes	Yes	Yes	Yes	Yes	Yes
R^2	0.696	0.696	0.696	0.698	0.697	0.698
Observations	2220	2220	2220	2220	2220	2220

Note: Standard errors in parentheses clustered at the municipality level. Outcome variable is the count of non-oil crimes in the municipality. Distances are measured in hundreds of kilometers from the municipality centroid. Amnesty/contracts are defined as whether the municipality contained any amnestied/contracted militant camps during the conflict. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

SUPPLEMENTARY INFORMATION
— Not For Publication —

C Data sources and measurement

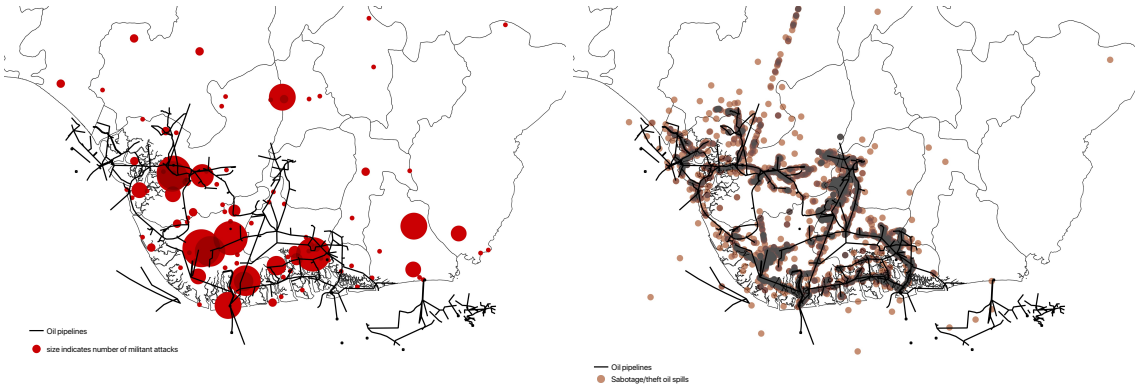
C.1 Data sources

Oil production data: Data on oil production comes Rystad Energy's UCube database, downloaded May 21 2019. These data contain field-level annual production quantities for all oil fields in Nigeria from 1999-2015 covering 109 oil mining leases (OML) and 345 fields of which 328 are reported to ever produced oil. We consider the "birth" year of a field to be the first year in which it enters either of the government records. Since we are interested in capturing both extensive and intensive margins of field production, we include a field for all years after it is born, even if it does not appear in the data in a given year.[60] We then match each of the oilfields and their production history to detailed geographic data on oil and gas infrastructure as described below.

Oil and gas infrastructure: Data on the geographic location of oil and gas infrastructure comes from the administrative records of the Department of Petroleum Resources (DPR), made available to the authors by that agency, and Google Maps. The data also feature a pipeline network of 4,284 kilometers. This latter number underestimates the official length of pipeline network of roughly 5,000 km, per NNPC, by 15%. This is expected, as this data primarily measure the larger flow and trunklines that connect fields to other fields and export terminals, and are unlikely to capture smaller delivery lines that flow within fields from wellheads themselves. A map of the oil infrastructure, along with the locations of militant camps and their amnesty status, can be found in Figure 1. We use infrastructure data in order to identify oil-producing villages by both their distance to the nearest field or pipeline, allowing us to restrict the sample to only oil-producing communities where appropriate. This data also allows us to control flexibly for differential trends in outcomes for oil-producing or pipeline-rich areas around the time of the amnesty.

[60] In such cases, we assume the field did not produce anything in that year.

Figure C1: Militant attacks and oil spills in the Niger Delta



Note: This figure shows the geography of militant attacks (left, red) and pipeline theft (right, brown) in the Niger Delta overlaid with the location of oil pipelines. The red dot's size indicate the number of militant attacks.

Militant attacks: Data on oil-related conflict events comes from several sources. We use the Armed Conflict Location Event Dataset (ACLED) to measure high-profile attacks on the oil sector perpetrated by militant groups. This primarily consists of bombings of major oil infrastructure, kidnappings and killings of oil workers, and battles against the Nigerian military. To identify all such events from 1997-2017, we conduct keyword searches,[61] and further subset events containing the keywords to include only those attacks that were perpetrated by political militias or rebel groups.[62] We also include all additional events perpetrated by MEND, the largest Niger Delta militant group, that may not include the specified strings.[63] This yields at total of 419 events, or 4.2% of Nigeria's total conflict events over the period. The attacks' geographical location is presented in the top panel of Figure C1.[64]

[61]These key words include oil, petroleum, drilling, rig, pipeline, flow, and the names of all major multinational and state-owned oil companies operating in Nigeria.

[62]This excludes communal conflict over oil revenues that is not directly related to militant groups.

[63]Results are not sensitive to this choice.

[64]The ACLED database operates with three spatial precision codes, 1) the exact locality, 2) an identified subset of the local government area (LGA) and finally 3) within a LGA. The distribution of spatial precision codes among the oil-related events is 63%, 30% and 7% for spatial precision 1 (highest), 2 and 3 (lowest), implying that 93% of the oil-related attacks can be located within an LGA.

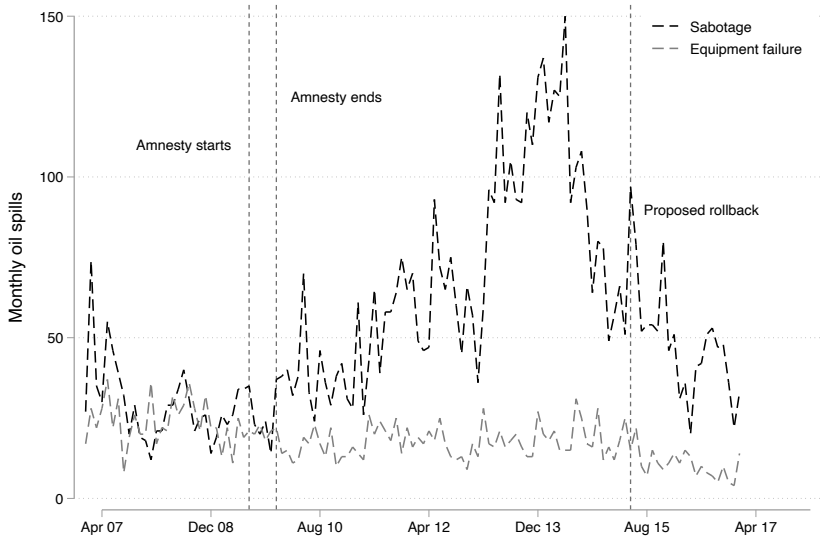
Militant camps and amnesty: Data on rebel commanders was collected by the authors from several sources. In 2018, we visited Warri, Delta State, one of the epicenters of the Niger Delta crisis. We first collated a list of militant commanders from previous qualitative work on the program, including Ugwu and Oben (2010) and Ojakorotu and Dodd Gilbert (2010), as well as consultation with AA Peaceworks (AAPW), a local non-profit organization with more than two decades of experience in conflict mitigation and peace-building in the region. For each militant commander, AAPW provided the following information: *i*) the group that this commander was affiliated with, *ii*) the location of their camp(s), usually denoted by the exact creek or a nearby village, and *iii*) whether they accepted amnesty. Gaps in the data and verification of accuracy were addressed by consulting Nigerian newspapers. We supplement this dataset with a list of pre-amnesty militant camps collected in a similar exercise by Blair and Imai (2013). Using Google searches of Nigerian newspapers we determine which militant commanders on our list received government contracts to perform security services in the oil sector.

This data collection procedure yielded 69 militant camps led by 41 unique commanders belonging to 24 different groups. Of the geo-referenced camps, 47 were controlled by commanders who accepted amnesty, while 13 were controlled by those who did not, and 9 had indeterminate status.[65]

Oil theft data: Information on the time, location, and details of 11,327 georeferenced oil spills comes from the administrative records of the the National Oil Spill Detection and Response Agency (NOSDRA), made publicly available on their Oil Spill Monitor. For every oil spill discovered in Nigeria, a Joint Investigative Report (JIV), is filed by the relevant oil companies, the Department of Petroleum Resources (DPR), NOSDRA, and the affected community. This JIV consists of a field visit to the spill site, in which the catchment area and size of the spill is assessed, as well as other basic data: the cause of the spill (e.g. sabotage or operational failure), a description of the environment (e.g. marsh, wetland, creek, or offshore),

[65]Determining how to define amnesty take-up required some judgment calls. For example, Eziz Ogunboss, a MEND commander, formally accepted amnesty but was rumored to remain involved in the conflict in various clandestine ways. We code such cases as accepting amnesty. In contrast, John Togo of the Niger Delta Liberation Front (NDLF) took amnesty, and then publicly renounced it shortly thereafter and returned to the creeks. In this case, we consider him as not having accepted amnesty, since he did not actually participate in the program.

Figure C2: Oil spills by cause over time



Note: This figure shows total monthly sabotage and non-sabotage malfunction spills from the NOSDRA data over time. Important dates in the amnesty process are indicated by labeled vertical lines.

the site,[66] and of the incident itself.[67] 9,783 spills occur onshore, while 1,544 are offshore. Sabotage accounts for 61.7% of all spills, a proportion that rises over time (see Figure C2). The geography of oil theft is presented in the bottom panel of Figure C1. Using these reports we generate a panel of quarterly and annual sabotage and malfunction spills with their characteristics at the village-level from 2006-2016[68] by designating affected villages as all those within 5 kilometers of a given spill. Controversy abounds over the extent of underreporting and misattribution of spill causes, either because of uneven government access to conflict zones or incentives to misreport on the part of oil companies (Amnesty International 2018). We test the robustness of our main results to measurement error correlated with the amnesty.

Law enforcement activity: Data on law enforcement activity comes from the

[66]For example "18 inch Assa-Rumuekpe Trans Niger Pipeline at Emohua."

[67]One reads "a 6 inch ball valve was installed on the line by suspected oil bunkers. A 4 inch galvanized pipe ran from the valve to water front where the bunkers load from."

[68]JIV reports from oil spills prior to 2006 are not available.

text of Nigerian news media reports. We begin by assembling a comprehensive collection of plausibly relevant news articles covering topics of oil theft, law enforcement, and crime in Nigeria by searching relevant keywords in the Dow Jones Factiva media database. We collect all articles that satisfy each of the following criteria: i) mention the word "oil", ii) mention at least one of a set of enforcement-related keywords[69] , iii) mention at least one of a set of exact oil crime-related phrases.[70] Some examples of relevant articles are shown in Figure C3.

This procedure yields 17146 total articles potentially related to oil theft enforcement.[71] We then hired Nigerian research assistants to first identify all articles that are relevant to law enforcement activity in Nigeria, yielding a total of 3932.[72] From this set of relevant articles we then manually extract all *law enforcement events*, where an event is defined as a unique interaction between law enforcement and suspected criminals that occurs in a specific location. For each event, we code the following variables: i) the location of the event, typically a neighborhood, village, oil asset, or local government area (municipality) ii) the law enforcement agency, iii) the illegal activity committed, selected from a pre-coded list,[73] iv) the items seized or destroyed in the law enforcement action, selected from a pre-coded list,[74] v) the total number of arrests, and vi) the total number of fatalities. Extensive manual quality checks were conducted on weekly researcher submissions.

[69]These are: "raid, raids, raided, seize, seized, seizes, seizure, seizures, destroy, destroys, destroyed, operation, capture, captures, captured, arrest, arrested, arrests, kill, killed, kills, apprehend, apprehended, apprehends, burn, burns, burned, invade, invaded, invades, search, searches, search"

[70]These are: artisanal refineries, artisanal refinery, artisanal refining, bunkerers, bunkering camp, bunkering gang, bunkering site, ex toru, illegal bunkering, illegal diesel, illegal fuel, illegal oil, illegal refineries, illegal refinery, illegal refining, illegally refined, joint task force, Nigerian military, Nigerian Navy, oil bunkerers, oil bunkering, oil smugglers, oil theft, oil thief, oil thieves, oil vandals, operation 777, operation awase, operation crocodile smile, operation delta safe, operation eagle eye, operation pulo shield, operation python dance, operation restore hope, operation river sweep, operation safety check, operation tsare teku, pipeline sabotage, pipeline vandal, pipeline vandalism, pipeline vandals, pirate, pirates, stolen crude, stolen, diesel, stolen oil, swamp buggy.

[71]Of course, these search terms are unlikely to be exhaustive, but they were derived from substantial reading of these articles. Also, note that this figure may be inflated because the same story is sometimes published by multiple different media outlets.

[72]We excluded articles about unrelated conflicts such as Boko Haram in Northern Nigeria, but included articles about non-oil illegal activities such as armed robbery, gang activity, and fraud

[73]These are: oil theft, piracy, illegal refining, pipeline vandalism, transportation of stolen oil, kidnapping, cultism/gang activity, militancy, and other illegal activities.

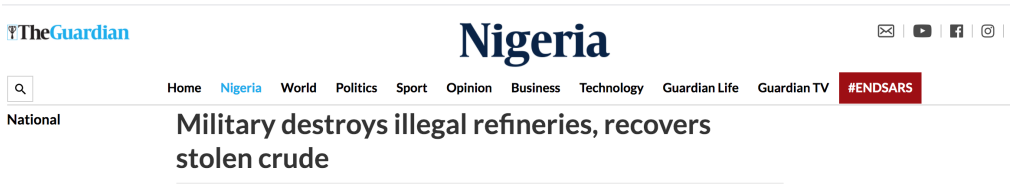
[74]These are: no item, boats, stolen oil, arms, illegal refineries, trucks, oil theft equipment, and other items.

Figure C3: Relevant articles

(a)

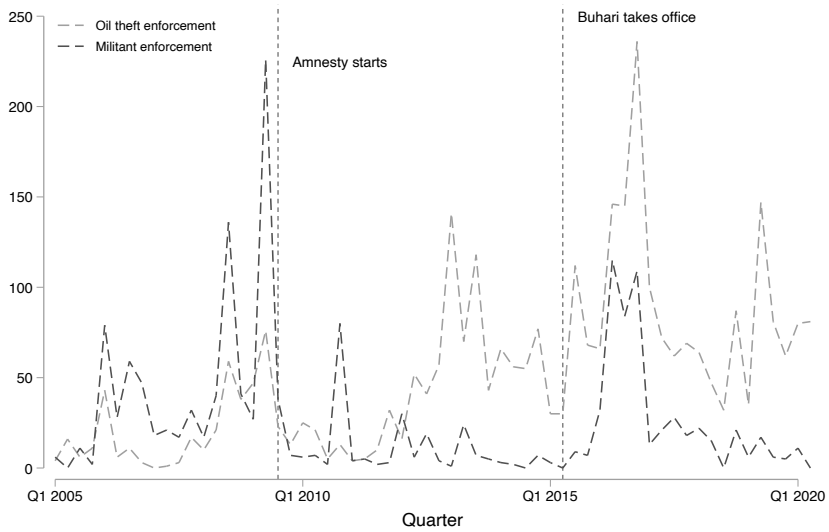
(b)

(c)



Note: This figure shows screenshots from relevant articles in *The Vanguard* (a), *The Sun* (b), and *The Guardian Nigeria* (c), all local Nigerian newspapers.

Figure C4: Enforcement



Note: This figure shows total quarterly militancy and oil theft-related law enforcement actions. Important dates in the amnesty process are indicated by labeled vertical lines.

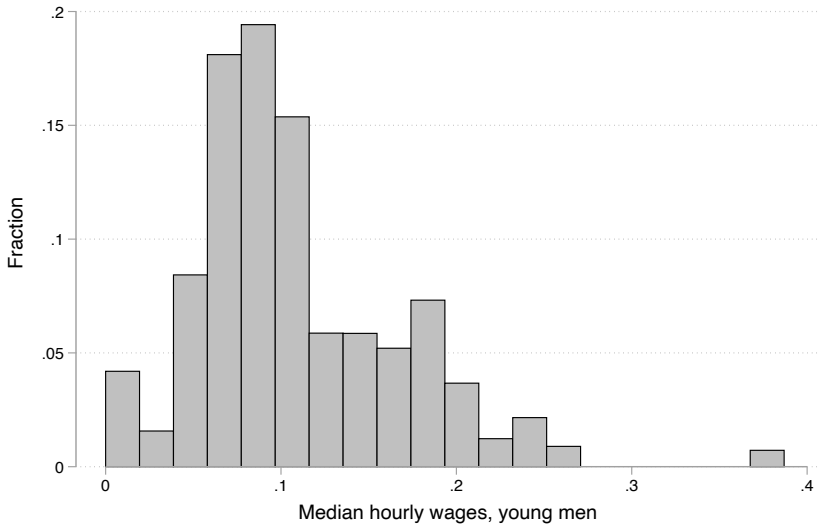
We consider any two articles as duplicates if they are published in the same calendar week and their headlines exceed a string similarity score threshold as defined by Levenshtein edit distance. After grouping duplicates into unique articles, we take the union of all events identified by the researchers to allow that duplicate articles may contain both repeated events as well as independent information.[75] In total, we obtain 5682 law enforcement events for which the location can be reliably geocoded, of which 3261 are related to oil theft. These events cover 3379 unique articles. 89% of all locations mentioned in relevant events were successfully geocoded. We then merge these enforcement events to villages in our sample using 5 kilometer rings, the same criteria used for oil theft. Figure C4 plots quarterly total enforcement actions for anti-militancy and anti-oil theft actions separately.

Additional covariates: We obtain additional covariates from a variety of sources, including household surveys, maps, and geospatial calculations. Figure C5 shows the distribution of median wages of men under 40 – our measure of black market

[75]For example, if there are two articles about the same raid, one may mention a second event, while the other does not.

costs – at the village level.

Figure C5: Distribution of median young male wages



Note: This figure shows the distribution of median wages for men less than 40 at the village-level.

C.2 Rebel military strength

The government's cost of fighting a militant group, determined by their ability to disrupt oil production, is a key parameter in our model. To proxy for the underlying, unobserved strength of a militant camp, we follow qualitative reports to assume that rebel commanders coordinate with nearby allied groups in order to carry out attacks on oil infrastructure. We therefore measure camp-level military strength by leveraging their intersecting position in the network of oil pipelines and between-group alliances.

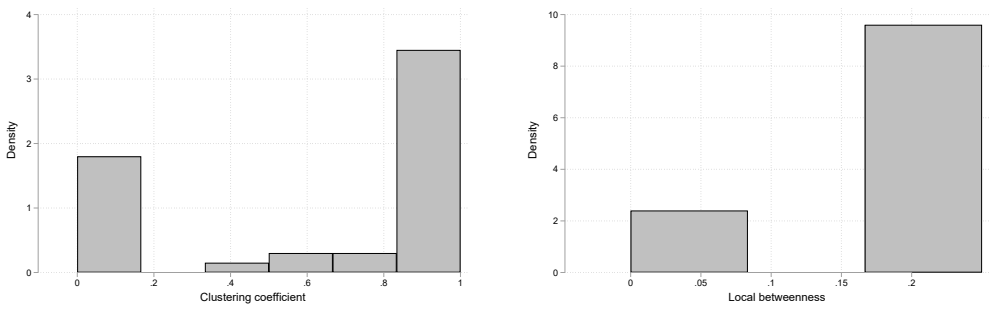
The Niger Delta conflict is known for a complex web of alliances between militant commanders, most prominently the umbrella group MEND. Let Π be the alliance network matrix, where $\pi_{ij} = 1$ if camps i and j belong to the same larger umbrella group (e.g. Movement for the Emancipation of the Niger Delta, Niger Delta People's Volunteer Force, et cetera) or if they are identified as allies in a com-

prehensive 2007 Small Arms Survey review of the state of the Niger Delta conflict (Hazen and Horner, 2007).[76]

We plot both the full local network and three prominent component networks which correspond to the classification in Asuni (2009b). Figure C6 plots histograms over local clustering- and betweenness coefficients, illustrating that the alliance network is generally highly connected and decentralized.[77]

Figure C6: Centrality and connections in local alliance network

(a) Clustering coefficient (b) Centrality



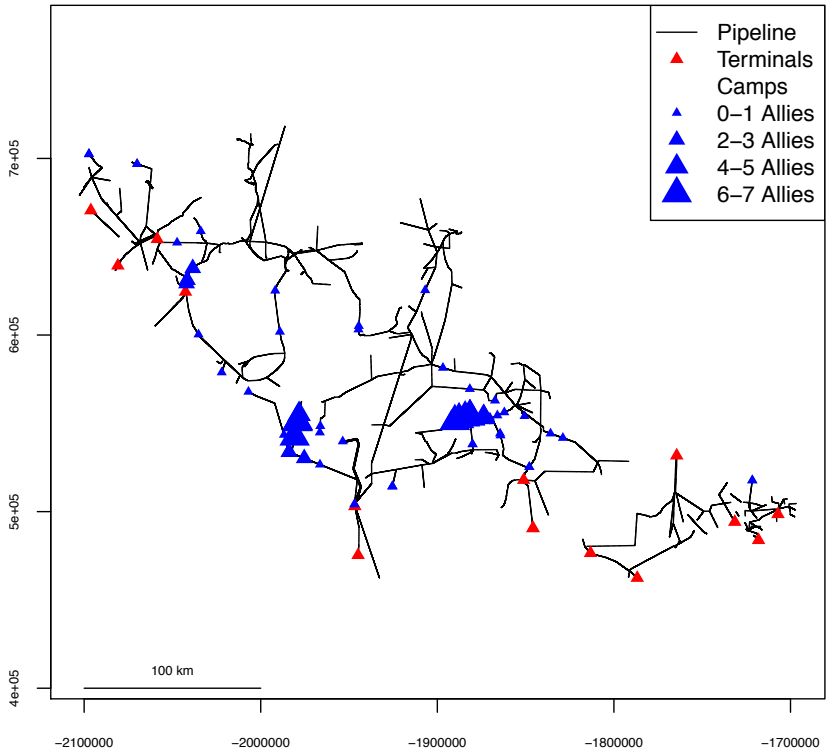
Note: This figure shows histograms of the local clustering coefficient (D. J. Watts and Strogatz 1998) and standardized measure of betweenness centrality (Freeman 1977) at the camp level. The local clustering coefficient of a node shows the connected edges as a share of all feasibly connecting edges. Betweenness centrality shows the share of paths in which a node acts as a bridge along the shortest path between two other nodes.

Since rebel strength is inherently localized by its dependence on access to physical infrastructure targets, we overlay the alliance structure with the network of physical oil pipeline infrastructure, P^{δ}, where $p^{\delta}_{ij} = 1$ if i and j are located on connected pipelines and $d(i,j) \leq \delta$, where $d(\cdot)$ is the distance function that measures the shortest path between two points along the pipeline network. This gives us our measure of strength, $g^{\delta}_{i} = \sum_{j \in Z} \pi_{ij}$ where $Z = \{j \neq i | P^{d}_{ij} = 1\}$, or the number of allies for camp i within δ kilometers along the pipeline.

[76] We take no position on the strength of connections, since some within-group alliances may well be weaker than cross-group alliances, given substantial variance in organizational capabilities of umbrella groups (Asuni 2009b).

[77] Asuni (2009b) emphasizes that alliances were temporary and motivated by tactical expedience, with "[different] groups [coming] together for particular operations before going their own way again."

Figure C7: Pipeline network map



Note: This figure shows the network of oil pipelines in the coastal Niger Delta in black, overlaid with the location of the 69 militant camps in our sample in blue, and the location of oil export terminals (pipeline endpoints) in red. Each militant camp is "snapped" to its nearest pipeline to determine its location in the pipeline network. The size of the camp indicates κ_i^{10}, the number of local allies each camp is connected to within 10 kilometers along the pipeline.

The network of alliances and pipelines is visualized on Figure C7. This map plots the physical pipeline infrastructure, with each militant camp attached to its nearest segment of pipeline. Red triangles indicate oil export terminals, the geographic endpoints of a pipeline. Blue triangles indicate the location of militant camps along pipelines. The map also overlays the geographic distribution of k_i^δ for $\delta = 10$ kilometers. The size of the militant camps markers correspond to a number of allies within 10 kilometers. Variation in g_i^δ is driven by variation in the location of camp i, the location of camps $-i$, and the number and type of alliances camp i possesses. High g_i^{10} camps are concentrated along the eastern pipelines connecting Port Harcourt to Soku field in Rivers state, and the southern pipelines connecting Ogbainbiri field to the coast in Bayelsa state.

We validate local alliance density, our measure of rebel military strength, by showing that it robustly predicts the local reduction in oil production during the 2005-2009 Niger Delta conflict. Using the sample of 69 militant camps, we estimate cross-sectional regressions of the form

$$y_i = \alpha + \varphi g_i^\delta + X_i'\beta + \xi_s + v_i \tag{10}$$

where y_i is an outcome, g_i^δ is a measure of camp strength based on alliance density, X_i are camp-level controls, and ξ_s is a state fixed effect capturing unobserved geographic heterogeneity. Throughout, we present wild bootstrap p-values which adjust for small numbers of clusters, following Cameron, Gelbach, and Miller (2008).

Table C1, top panel demonstrates a negative correlation between alliance density and output change, significant at the 5 or 1% level; each additional local ally is associated with 4.7-7.9 percentage points in output loss. These results are robust to controls (columns 3-5) and state fixed effects (column 5). Local alliance density alone accounts for 18.8% of the variation in output change. Furthermore, these effects are not driven by greater rebel density – columns 2-5 controls for the number of non-allied militant groups within 10 km along a pipeline, which is insignificant and does not affect the main results.

This correlation survives numerous sensitivity checks, including different periods over which conflict is designated, distances over which alliance density is defined, and model specification.

Figure C8 presents results from a comprehensive set of robustness tests on this

specification. We vary the distance δ over which g_i^δ is defined, the control and fixed effects specification, and the years over which the outcome is defined, yielding 125 specifications. All of the estimated coefficients are negative and the vast majority are significant at the 5% level or lower.

We plot the estimated effect of alliance density g_i^δ on output change and number of attacks against distance δ in Figure C9, illustrating that both outcomes decrease monotonically in δ.

As an additional robustness check we show in the remaining panels of Table C1 that alliance density also robustly predicts alternative indicators of military strength, *i*) defeat probability, *ii*) the number of attacks, *iii*) battlefield damage per attack, and *iv*) the total number of camps controlled by a commander. Table C2 reproduces these results for the subsample removing 11 camps of unknown amnesty status, finding significant and quantitatively comparable results, with the exception of Panel D where the effect is significant at the 5% level only.

As a final robustness exercise, in Table C3 we conduct a balance test of alliance density with camp-level covariates X_i in Equation (10). This exercise reveals a negative correlation, significant at the 5% level, between distance to oil infrastructure and alliance density, suggesting that our measure of military strength partially reflects the geographic availability of infrastructure targets, a property that we control for throughout the analysis.

Table C1: Militant strength and battlefield outcomes

	(1)	(2)	(3)	(4)	(5)
Panel A: Δ oil production, 2005-2009					
Allied, 10 km	-0.079***	-0.079***	-0.060***	-0.035**	-0.047***
	(0.017)	(0.017)	(0.016)	(0.014)	(0.014)
Non-allied, 10 km		0.002	0.006	0.002	0.014
		(0.016)	(0.018)	(0.014)	(0.014)
MEND				-0.380***	
				(0.114)	
Wild bootstrap p-value	0.000	0.000	0.002	0.018	0.005
R^2	0.188	0.188	0.471	0.558	0.637
Panel B: Defeated in battle					
Allied, 10 km	-0.047***	-0.047***	-0.052***	-0.033**	-0.055**
	(0.015)	(0.015)	(0.019)	(0.015)	(0.022)
Non-allied, 10 km		0.033	0.055*	0.052*	0.057*
		(0.025)	(0.030)	(0.030)	(0.031)
MEND				-0.296**	
				(0.119)	
Wild bootstrap p-value	0.005	0.009	0.011	0.060	0.017
R^2	0.098	0.133	0.405	0.482	0.430
Panel C: Total oil-related militant attacks, 2005-2009					
Allied, 10 km	4.171***	4.235***	3.610***	3.209***	3.215***
	(0.879)	(1.031)	(0.868)	(0.865)	(0.883)
Non-allied, 10 km		3.223***	2.605***	2.668***	2.419***
		(0.662)	(0.603)	(0.545)	(0.614)
MEND				6.107**	
				(2.850)	
Wild bootstrap p-value	0.001	0.000	0.005	0.002	0.007
R^2	0.244	0.354	0.624	0.634	0.667
Panel D: Δ oil per attack					
Allied, 10 km	0.362***	0.369***	0.361***	0.457***	0.361***
	(0.127)	(0.125)	(0.111)	(0.103)	(0.097)
Non-allied, 10 km		0.247***	0.130	0.100	0.055
		(0.082)	(0.113)	(0.116)	(0.118)
MEND				-1.925	
				(1.147)	
Wild bootstrap p-value	0.020	0.017	0.004	0.001	0.002
R^2	0.085	0.117	0.464	0.498	0.548
Panel E: Number of camps					
Allied, 10 km	0.315***	0.312***	0.266**	0.168	0.248**
	(0.093)	(0.092)	(0.104)	(0.112)	(0.102)
Non-allied, 10 km		-0.145**	-0.183***	-0.168***	-0.178***
		(0.068)	(0.056)	(0.049)	(0.053)
MEND				1.487**	
				(0.676)	
Wild bootstrap p-value	0.012	0.013	0.086	0.281	0.093
R^2	0.237	0.276	0.379	0.485	0.471
Controls	No	No	Yes	Yes	Yes
State FE	No	No	No	No	Yes
Observations	69	69	69	69	69

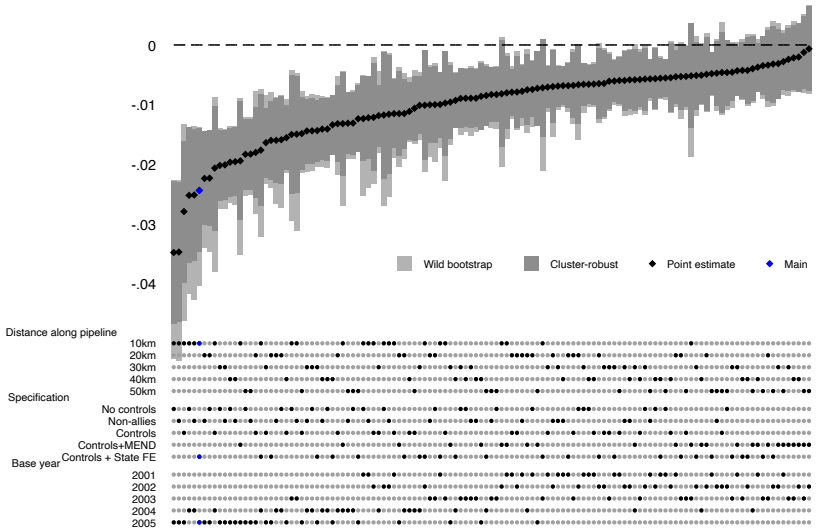
Note: Standard errors in parentheses clustered at the militant commander level. Sample is all camps in the data, except Panel D which is conditional on experiencing a militant attack ($n = 52$). Outcome variable is indicated in panel header. Change in oil production and total oil-related militant attacks calculated within 20 kilometers of the camp. Independent variable is k_i^{10}, the number of allies of camp i within 10 kilometers along the pipeline network. $MEND$ is a dummy variable indicating that the group belongs to the Movement for Emancipation of the Niger Delta. Controls are distance from pipeline, distance from state capital, distance from coast, camp latitude, annual precipitation, monthly average temperature, altitude, and slope. Wild bootstrap p-values adjust for small numbers of clusters following Cameron, Gelbach, and Miller (2008). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table C2: Militant strength and battlefield outcomes, subsample

	(1)	(2)	(3)	(4)	(5)
Panel A: Δ oil production, 2005-2009					
Allied, 10 km	-0.083***	-0.083***	-0.051***	-0.018	-0.047***
	(0.019)	(0.019)	(0.017)	(0.013)	(0.015)
Non-allied, 10 km		0.006	0.015	0.007	0.013
		(0.021)	(0.023)	(0.014)	(0.018)
MEND				-0.486***	
				(0.106)	
Wild bootstrap p-value	0.000	0.001	0.006	0.143	0.002
R^2	0.223	0.224	0.495	0.641	0.677
Panel B: Defeated in battle					
Allied, 10 km	-0.055***	-0.055***	-0.067***	-0.044**	-0.070***
	(0.017)	(0.017)	(0.023)	(0.018)	(0.025)
Non-allied, 10 km		0.052***	0.076***	0.070***	0.078***
		(0.019)	(0.019)	(0.021)	(0.019)
MEND				-0.337**	
				(0.130)	
Wild bootstrap p-value	0.005	0.010	0.009	0.045	0.021
R^2	0.134	0.206	0.477	0.574	0.522
Panel C: Total oil-related militant attacks, 2005-2009					
Allied, 10 km	4.397***	4.404***	3.581***	3.100***	3.361***
	(0.939)	(1.093)	(0.985)	(1.004)	(0.996)
Non-allied, 10 km		3.211***	2.496***	2.622***	2.399***
		(0.810)	(0.719)	(0.607)	(0.820)
MEND				7.126**	
				(3.292)	
Wild bootstrap p-value	0.002	0.001	0.012	0.011	0.006
R^2	0.281	0.371	0.614	0.629	0.670
Panel D: Δ oil per attack					
Allied, 10 km	0.316**	0.315**	0.246**	0.361***	0.238**
	(0.128)	(0.125)	(0.113)	(0.127)	(0.088)
Non-allied, 10 km		0.255***	0.076	0.049	-0.008
		(0.090)	(0.124)	(0.135)	(0.130)
MEND				-1.844	
				(1.380)	
Wild bootstrap p-value	0.037	0.031	0.016	0.001	0.007
R^2	0.080	0.114	0.404	0.435	0.553
Panel E: Number of camps					
Allied, 10 km	0.286***	0.285***	0.200*	0.102	0.205*
	(0.098)	(0.095)	(0.109)	(0.108)	(0.105)
Non-allied, 10 km		-0.190**	-0.273***	-0.247***	-0.252***
		(0.086)	(0.073)	(0.053)	(0.059)
MEND				1.451**	
				(0.692)	
Wild bootstrap p-value	0.032	0.028	0.228	0.473	0.178
R^2	0.205	0.260	0.415	0.515	0.523
Controls	No	No	Yes	Yes	Yes
State FE	No	No	No	No	Yes
Observations	58	58	58	58	58

Note: Standard errors in parentheses clustered at the militant commander level. Sample is all camps used in village treatment assignment, except Panel D which is also conditional on experiencing a militant attack ($n = 42$). Outcome variable is indicated in panel header. Change in oil production and total oil-related militant attacks calculated within 20 kilometers of the camp. Independent variable is k_i^{10}, the number of allies of camp i within 10 kilometers along the pipeline network. $MEND$ is a dummy variable indicating that the group belongs to the Movement for Emancipation of the Niger Delta. Controls are distance from pipeline, distance from state capital, distance from coast, camp latitude, annual precipitation, monthly average temperature, altitude, and slope. Wild bootstrap p-values adjust for small numbers of clusters following Cameron, Gelbach, and Miller (2008). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure C8: Camp-level output change specification plot



Note: This figure shows robustness across 125 specifications for a regression of camp-level percent output change on the number of allied camps within 10 km along the pipeline, as shown in Equation (10). Each point represents the estimated coefficient of an individual specification surrounded by 95% confidence bars. The set of specifications include all combinations of: *i*) varying the distance δ along the pipeline over which k_i^δ is defined from 10 to 50 km, *ii*) inclusion of controls, MEND dummy, state FE, and non-allies, *iii*) varying the base-year over which the output change outcome variable is defined from 2001 to 2005. Specification type is indicated in the figure footer. Main estimate is indicated in blue.

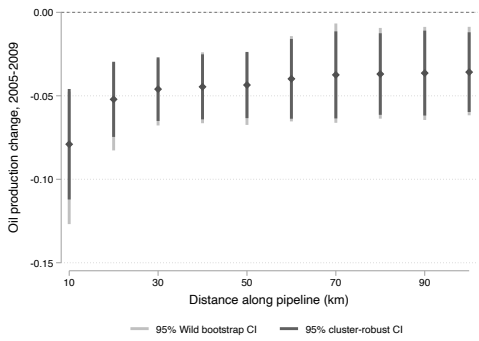
Table C3: Balance tests of camp-level covariates

Dependent variable	Slope	Altitude	Temp.	Precip.	Wages	Distance to		
						Infr.	Coast	Niger
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Allied, 10 km	-0.063	-0.180	-0.007	6.739	-0.006	-0.464**	0.312	-3.448
	(0.091)	(0.279)	(0.007)	(5.152)	(0.004)	(0.204)	(0.806)	(2.198)
Wild bootstrap p-value	0.513	0.561	0.364	0.205	0.217	0.035	0.755	0.137
R^2	0.019	0.118	0.032	0.026	0.333	0.049	0.010	0.093
Observations	69	69	69	69	69	69	69	69

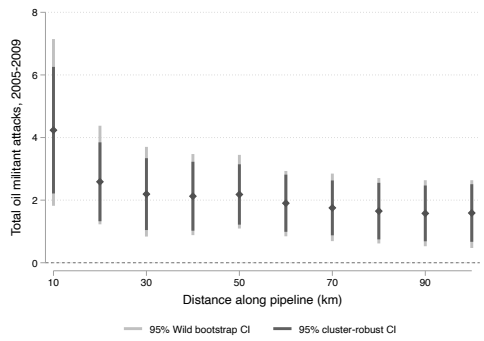
Note: Standard errors in parentheses clustered at the militant commander level. Sample is all camps in the data. Outcome variable is indicated in panel header. Independent variable is k_i^{10}, the number of allies of camp i within 10 kilometers along the pipeline network. All specifications control for the number of non-allied camps within 10 kilometers along the pipeline network. Wild bootstrap p-values adjust for small numbers of clusters following Cameron, Gelbach, and Miller (2008). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure C9: Camp-level output and attacks by distance

(a) Output change, 2005-2009



(b) Attacks, 2005-2009



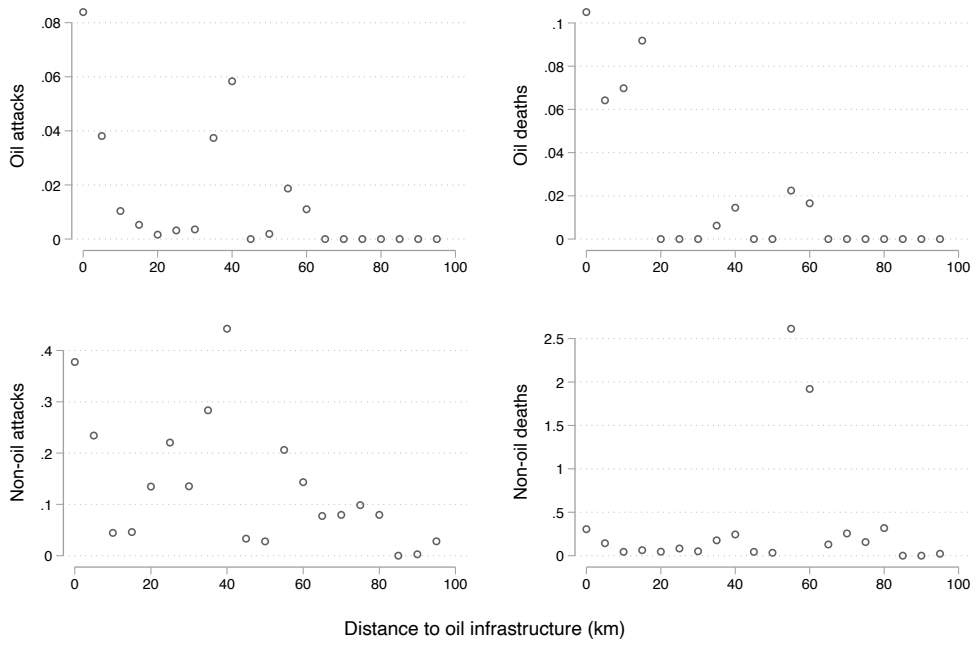
Note: This figure shows robustness across distances. Independent variable is k_i^d, the number of allies of camp i within d kilometers along the pipeline, with d on the x-axis. Change in oil production (Panel A) and total oil-related militant attacks (Panel B) calculated within 20 kilometers of the camp. 95% confidence intervals shown, with standard errors clustered at the militant commander level. Wild bootstrap confidence intervals adjust for small numbers of clusters following Cameron, Gelbach, and Miller (2008).

D Additional empirical results

D.1 Spatial distribution of attacks

Figure D1 plots the level of militant attacks and deaths at the village-level by five-kilometer binds of distance to the nearest oil infrastructure. The top panel plots oil-related conflict outcomes, while the bottom panel plots non-oil conflict incidents. The results suggest that both attacks and fatalities of oil-related militant conflict are primarily concentrated around oil installations, consistent with qualitative accounts of the conflict. Non-oil conflict, instead, appears uncorrelated with distance to oil infrastructure.

Figure D1: Militant activity by distance to infrastructure



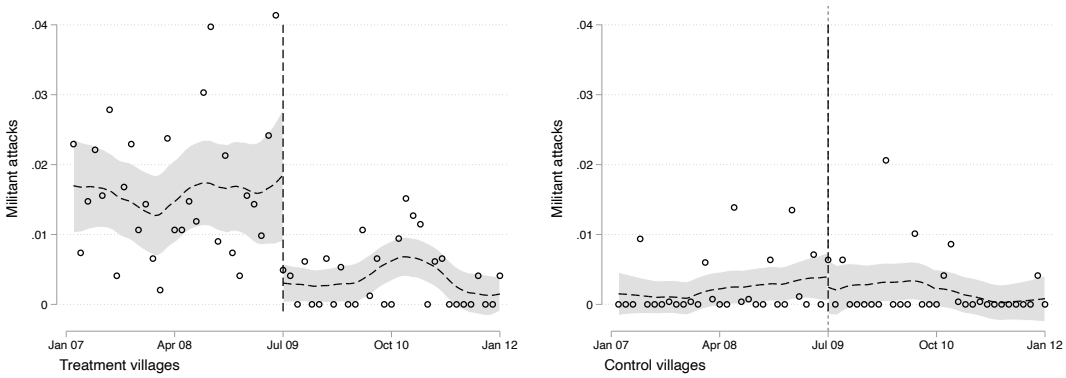
Note: This figure shows the village-level mean of the outcome within five-kilometer bins of distance to the nearest oil infrastructure (field or pipeline). Outcomes are (from top-left, clockwise), oil-related militant attacks, oil-related militant conflict deaths, non-oil conflict events, and non-oil conflict deaths.

D.2 Robustness: regression discontinuity in time

In this section, we conduct several additional robustness tests to support the validity of the estimates in OA B.1.

D.2.1 Differences-in-discontinuities

Figure D2: RDiT event-study by treatment status



Note: This figure shows the monthly RDiT (event-study) on militant activity separately for treatment and control groups. Each panel estimates a nonparametric RDiT event-study for an event-window of 30 months before and after the amnesty date, with trend lines and dashed 95% confidence intervals estimated separately on either side of the event. These trends are overlaid with a scatterplot of the mean monthly village-level oil-related militant attacks over time. Left panel estimates the RDiT only on the sample of treated villages within 30 km of an amnestied camp, while right panel does the same for control villages.

Consider the differences-in-discontinuities estimate, which captures the difference in structural breaks for time series in the treated and control areas separately. Figure D2 plots the event studies on subsamples that are treated (Panel A) and untreated (Panel B), with flexible polynomials in time plotted on either side of the amnesty date for both groups. As Figure D2 reveals, the aggregate event-study effect is driven entirely by areas that received amnesty, ruling out spurious contemporaneous shocks that would affect all parts of the Niger Delta equally. This should bolster confidence that Figure B1 captures the short-run effect of the amnesty itself, rather than global trends or correlated shocks. Interestingly, Fig-

ure D2 also demonstrates the failure of parallel trends, as the pre-amnesty conflict trend slopes upward in Panel A but remains generally flat in Panel B.

D.2.2 Placebo test: non-oil conflict outcomes

We consider robustness to placebo outcomes. In particular, we support the assumption of counterfactual continuity over the event date by showing that non-oil conflict events are not discontinuous at the event date. In Table D1 we estimate the same event-study specifications as in Table B1, only using all non-oil conflict as the outcome of interest. The effect magnitudes and signs do not display any consistent pattern and are generally insignificant. In fact, the estimates are only significant at the 5% level in 2 out of 28 specifications, likely due to noise. In general, it appears as though non-oil conflict does not change discontinuously in the month of the amnesty announcement. This suggests that the treatment effect is probably not being driven by monthly time effects or correlated aggregate shocks that would affect the likelihood of conflict broadly. Indeed, only conflict specifically targeted by the amnesty actually falls in response to the policy.

D.2.3 Specifications

Columns (1)-(2), (3)-(4), and (5)-(6) of Table B1 consider linear, quadratic, and quartic specifications of time trends, respectively. For each specification, we include either annual or month-of-year effects. Each panel of Table B1 contains a different bandwidth. Lastly, columns (7) and (8) revert to a linear time trend specification but include a lagged dependent variable (AR(1)) term. The estimates on the post-amnesty indicator are significant at the 5 or 1% level in all but 5 out of 36 specifications, though even in these insignificant specifications the magnitudes remain large but standard errors increase as sample size falls. Amnesty robustly reduces violence by between 3-7 monthly militant attacks on average.

D.2.4 Optimal lag length

Table B1 allows only persistence of the AR(1) form. In this section, we estimate optimal lag-length across a variety of criteria and then re-estimate. Following the literature on optimal order selection in VAR models (see Lütkepohl 2005

Table D1: The effect of amnesty on non-oil conflict

Polynomial	Linear		Quadratic		Quartic		AR(1)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Full sample								
Post-amnesty	0.377	-4.997***	0.144	-3.203	3.483	3.410	0.999	-2.881*
	(2.344)	(1.750)	(2.665)	(2.705)	(3.448)	(3.063)	(2.338)	(1.582)
m_{t-1}							0.233*	0.386***
							(0.134)	(0.119)
Month FE	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	No	Yes	No	Yes	No	Yes	No
Observations	276	268	276	268	276	268	275	267
R^2	0.783	0.732	0.783	0.735	0.789	0.750	0.795	0.772
$\Delta = 30$								
Post-amnesty	2.128	7.724**	7.312	9.138*	-1.593	-3.083	2.648	4.665*
	(2.811)	(3.031)	(4.896)	(4.817)	(6.870)	(4.480)	(2.644)	(2.522)
m_{t-1}							0.336*	0.530***
							(0.181)	(0.133)
Month FE	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	No	Yes	No	Yes	No	Yes	No
Observations	61	59	61	59	61	59	61	59
R^2	0.514	0.416	0.644	0.419	0.701	0.629	0.572	0.600
$\Delta = 20$								
Post-amnesty	4.485	9.729**	1.045	0.584	-6.393	-8.559	4.474	6.336*
	(3.426)	(4.156)	(6.845)	(4.849)	(5.806)	(6.986)	(3.393)	(3.655)
m_{t-1}							0.093	0.514***
							(0.200)	(0.156)
Month FE	No	Yes	No	Yes	No	Yes	No	Yes
Year FE	Yes	No	Yes	No	Yes	No	Yes	No
Observations	41	41	41	41	41	41	41	41
R^2	0.659	0.441	0.672	0.617	0.752	0.692	0.663	0.605
$\Delta = 10$								
Post-amnesty	-1.186		-4.678		-0.615		-6.713	
	(6.109)		(6.394)		(7.768)		(6.459)	
m_{t-1}							-0.428**	
							(0.165)	
Year FE	Yes	No	Yes	No	Yes	No	Yes	No
Observations	21	21	21	21	21	21	21	21
R^2	0.712	0.000	0.752	0.000	0.811	0.000	0.757	0.000

Note: Robust standard errors. Outcome variable is the number of monthly non-oil-related conflict events. Treatment is defined as an indicator for after July 2009. Window refers to the number of months included in the estimation before and after the event date. All windows apart from the full sample are symmetric. AR(1) specifications include a lagged dependent variable and a linear polynomial of event time. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

and Nielsen 2006), of which our univariate setting is a special case, we estimate the optimal lag length p for a generic autoregressive model of the form

$$m_t = \alpha + \sum_{i=1}^{p} \beta_i m_{t-i} + \epsilon_t$$

by evaluating several model fit statistics for varying p, taking 10 as our maximum lags. In particular, we consider the likelihood ratio (LR) test, which jointly tests the null hypothesis that the coefficient on the pth lag is zero. The optimal lag is then selected by the largest lag with a 5% significant likelihood-ratio test statistic. Alternatively, we also use several additional measures of model fit, including the final prediction error (FPE), as well as likelihood-based metrics such as the Akaike information criterion (AIC), the Hannan-Quinn information criterion (HQIC), and Schwarz's Bayesian information criterion (SBIC). All model fit statistics are calculated without including the constant.

Table D2 provides the results of re-estimating the main linear event-study specification without fixed-effects with optimally-selected lags. The columns give the results of each different model fit statistics, while the each sub-table re-estimates for a different event-window, as in Table B1. The optimal lag-length, indicated in each sub-table footer, varies substantially across event-windows and model fit criteria, varying from 0 to 9 lags. However, for any optimally-selected p, the results remain negative and significant at the 1% or 5% levels.

D.2.5 Structural break test

Another way to validate our event-date is to conduct a data-driven test of the presence and location discontinuous breaks in the time series of militant attacks without imposing our ex-ante known event-date. To do this, we follow the literature on structural breaks, as summarized in Perron (2006). The for the presence of a structural break, we estimate the following model

$$m_t = \alpha + \rho m_{t-1} + e_t$$

And test whether the estimated coefficient $\hat{\alpha} = E[m_t|m_{t-1}] - \hat{\rho} m_{t-1}$, varies over time. In other words, we test whether the mean of the time series, after subtracting

Table D2: Optimal lag selection for an AR(p) process

Criterion	LR	FPE	AIC	HQIC	SBIC
	(1)	(2)	(3)	(4)	(5)
Full sample					
Post-amnesty	-1.592***	-1.592***	-1.592***	-1.592***	-1.585***
	(0.527)	(0.527)	(0.527)	(0.527)	(0.543)
p	9	9	9	9	5
Observations	267	267	267	267	271
R^2	0.436	0.436	0.436	0.436	0.398
$\Delta = 30$					
Post-amnesty	-3.219*	-3.219*	-3.219*	-3.219*	-3.219*
	(1.686)	(1.686)	(1.686)	(1.686)	(1.686)
p	1	1	1	1	1
Observations	61	61	61	61	61
R^2	0.334	0.334	0.334	0.334	0.334
$\Delta = 20$					
Post-amnesty	-6.270***	-7.063***	-7.063***	-7.063***	-7.063***
	(1.854)	(2.157)	(2.157)	(2.157)	(2.157)
p	0	1	1	1	1
Observations	41	41	41	41	41
R^2	0.352	0.361	0.361	0.361	0.361
$\Delta = 10$					
Post-amnesty	-5.458*	-6.262*	-6.262*	-5.458*	-5.458*
	(2.915)	(3.454)	(3.454)	(2.915)	(2.915)
p	0	1	1	0	0
Observations	21	21	21	21	21
R^2	0.373	0.383	0.383	0.373	0.373

Note: Robust standard errors in parentheses. Outcome variable is the number of monthly oil-related militancy events. Treatment is defined as an indicator for months after July 2009. Δ refers to the number of months included in the estimation before and after the event date. All windows apart from the full sample are symmetric. p lags are included in each specification, with p indicated in the table footer. Each AR(p) specification includes a linear polynomial of event time. Optimal lags are selected by the likelihood ratio test of p vs. $p-1$ (1), or minimizing the final prediction error (2), the Akaike information criterion (3), the Hannan-Quinn information criterion (4), and Schwarz's Bayesian information criterion (5). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

the AR(1) persistence term, varies over time. To test both the existence and identify the location of a structural break, we use a supremum Wald test where the test statistic over a sample of size T is $\sup_t S_T(t)$, where $S_T(t)$ is the Wald statistic evaluated at a potential break date t. In other words, the estimated structural break corresponds to the largest estimated Wald statistic over all possible dates, with the p-value derived from the limiting behavior of this supremum (see Perron 2006 for details).

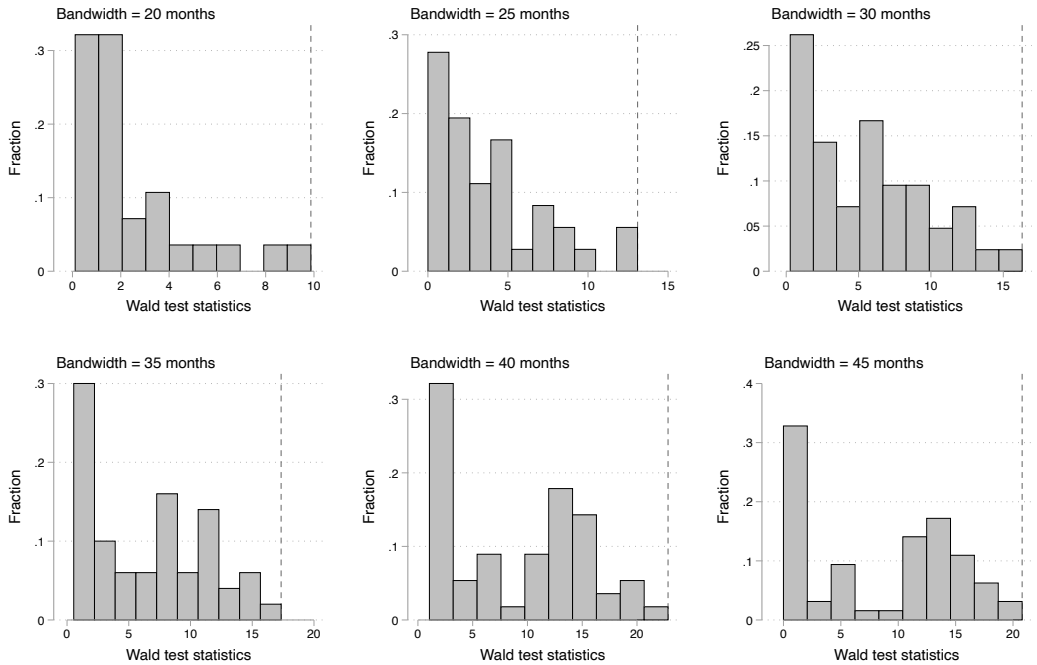
Table D3: Structural break tests

Bandwidth	Break month	Wald statistic	p-value
20	0	9.87	0.028
25	0	13.10	0.006
30	0	16.30	0.001
35	0	17.34	0.001
40	0	22.83	0.000
45	0	20.80	0.000

Note: Table gives results for a structural break test of oil militancy. The null hypothesis for the test is that the constant term, $\hat{\alpha} = E[y_t|y_{t-1}] - \hat{\rho}y_{t-1}$, does not vary over time. The estimated break month refers to the month with the highest Wald statistic for this test, measured in event-time. The test-statistic is the supremum of these χ^2 values for each possible date, while the p-value refers to the rejection probability associated with a given test statistic. We vary the event-window around the amnesty date from 20 to 45 months.

The results are summarized for event-windows varying from 20 to 45 in Figure D3 and Table D3. Figure D3 plots histograms of date-specific Wald statistics for each bandwidth, with a vertical line indicating the magnitude of the Wald statistic on our known amnesty date. The results indicate that the Wald statistic corresponding to the amnesty date is in the far-right tail of this distribution for every bandwidth. In Table D3, we test the significance of these estimates, displaying the corresponding maximal break date in event-time. The break date corresponds to the amnesty for all bandwidths, significant at the 1% level except for the smallest bandwidth with a p-value of 2.8%.

Figure D3: Histograms of Wald statistics for a structural break test at varying bandwidths



Note: This figure displays Wald statistics for structural break tests by month for varying event-windows, indicated in the sub-figure headers. Vertical line indicates the Wald statistic on a structural break test of July 2009, the true amnesty date. The outcome variable is oil-related militant attacks.

D.3 Robustness tests: difference-in-differences

We address several additional alternative explanations that may account for the increase in oil theft concentrated in areas where militant groups received amnesty: outcomes in neighboring camps, the choice of control group, rebounding oil output, measurement error, treatment definition, non-linear specifications, and effects on the extensive- and intensive margins.

D.3.1 Choice of control group

Militants choose their location endogenously. They are more likely to locate near oil assets, in coastal riverine communities, and are disproportionately located in the core oil-producing states of Delta, Bayelsa, and Rivers. It may therefore be the case that oil theft outcomes are affected by fundamental differences between communities that are militant- and non-militant, or oil-producing and non-oil producing. To explore whether comparability on unobservables can be enhanced we consider the following battery of alternative control groups: The synthetic difference-in-difference approach of Arkhangelsky et al. (2019), controlling by using non-amnestied militant regions as a control group, and restricting the sample to respectively the core Niger Delta, oil producing, and using villages more than 30 kilometers from a non-amnestied camp.

Table D4: Amnesty and oil theft: control groups

Control group	All untreated (1)	Synth DD (2)	Core ND (3)	Oil-producing (4)	Militant (5)	Non-militant (6)
Amnestied × Post-amnesty	0.448***	0.405***	0.590***	0.615***	0.925***	0.221***
	(0.046)	(0.042)	(0.058)	(0.060)	(0.102)	(0.027)
Spatial standard errors (50km)	0.101	0.124	0.135	0.136	0.266	0.055
Village FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Treatment villages	2442	2442	2217	1956	2442	871
Control villages	2669	2669	1263	1568	401	2268
Observations	61332	61332	41760	42288	34116	37668
R^2	0.396	0.362	0.393	0.394	0.401	0.289

Note: Standard errors clustered at the village level. Outcome variable is the number of annual oil theft events within 5 kilometers of the village. Treatment is defined as all villages within 30 km of an amnestied militant camp. Column (1) includes full sample of control observation. Column (2) reweights the control sample with synthetic DD weights (Arkhangelsky et al., 2019). Column (3) uses the villages in only the core Niger Delta states (Delta, Bayelsa, and Rivers). Column (4) uses only villages within 10 kilometers of oil infrastructure. Column (5) uses all villages within 30 kilometers of a non-amnestied militant camp. Column (6) uses all villages further than 30 kilometers from a non-amnestied militant camp. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Results are given Table D4, which provides estimates of the main TWFE model, for each control group or sample restriction, and in Figure D4, which displays parallel trends plots. All specifications feature small but significant differences between treatment and control in some of pre-periods, with the militant-only group containing a single outlier. The evolution of post-amnesty oil theft is broadly similar, suggesting together with the parametric estimates in Table D4 that changing the control group or the sample does not materially affect the estimates. Lack of improvement in balance on pre-treatment outcomes suggests there is little benefit in refining the control group to increase comparability, at the cost of less precision.

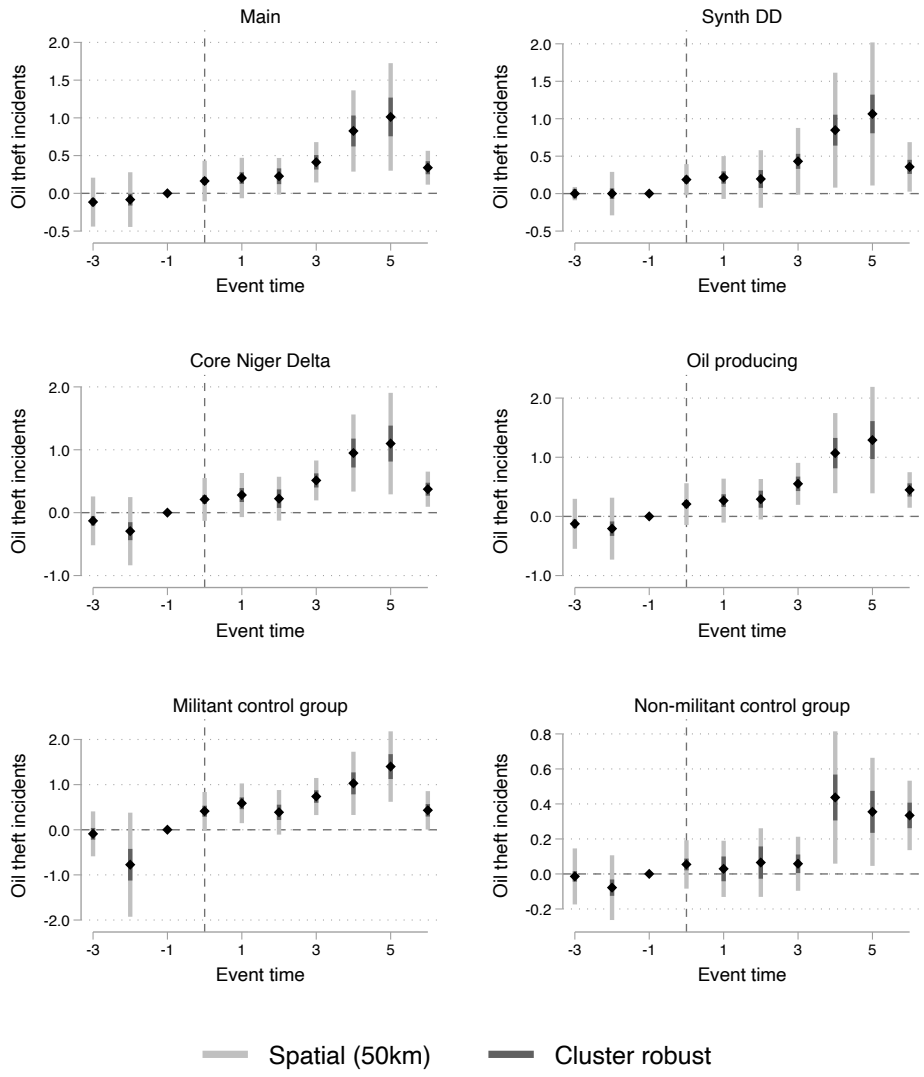
In Table D5 and Figure D5 we repeat exercise, further disaggregating the militant control group by the 4 commanders defeated pre-amnesty and the 7 that were not offered amnesty. The results indicate that defeated locations see an increase in oil theft similar to amnesty, suggesting a crime displacement effect. The increase in non-defeated locations is comparable to other control groups, taking pre-trends into account.

Table D5: The effect of amnesty on oil theft by militant control group outcome

Dependent variable	Oil theft					
Control group	Non-amnestied militant		Defeated pre-amnesty		Not offered or rejected	
	(1)	(2)	(3)	(4)	(5)	(6)
Amnestied × Post-amnesty	0.925***	0.865***	0.305***	0.307***	1.148***	1.087***
	(0.102)	(0.098)	(0.060)	(0.068)	(0.130)	(0.129)
Spatial standard errors (50km)	0.266	0.247	0.146	0.146	0.332	0.325
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Village FE	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Year FE	No	Yes	No	Yes	No	Yes
Treatment villages	2442	2442	2442	2442	2442	2442
Control villages	401	401	106	106	295	295
Observations	34116	34116	30576	30576	32844	32844
R^2	0.401	0.413	0.407	0.419	0.402	0.414

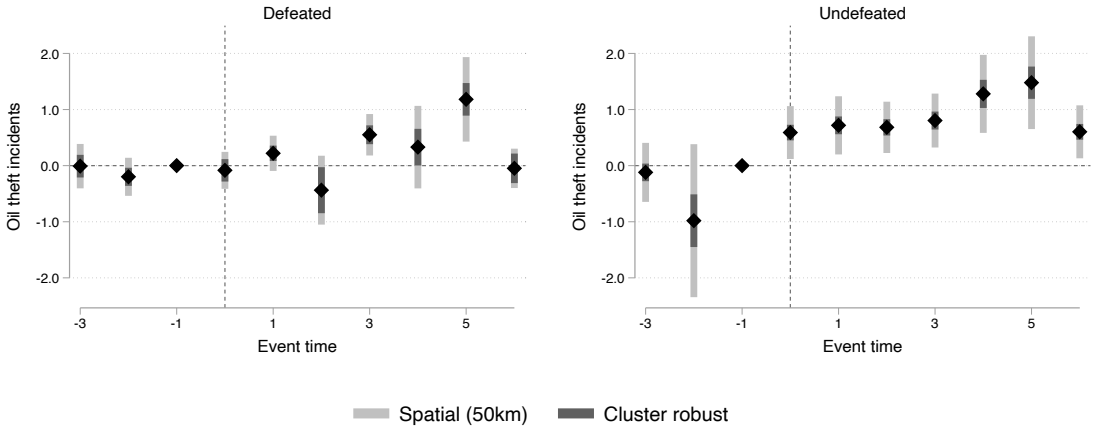
Note: Standard errors in parentheses are clustered at the village level. Spatial standard errors are calculated following Conley (2010). Outcome variable is a dummy variable if there are any oil theft events within 5 kilometers of the village. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group is defined in table header. Controls include distance to nearest oilfield, distance to nearest pipeline, distance to state capital, distance to Niger River, distance to the coast, and population density. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure D4: Amnesty parallel trends by control group



Note: This figure shows estimated event-study regressions 95% confidence intervals clustered at the village level for various permutations of control group. Main uses all villages and defines the treatment group as those within 30 km of an amnestied camp. Synth DD uses the same treatment group but re-weights the control group to match the pre-trends in oil theft, following Arkhangelsky et al. (2019). Militant control group restricts the control group to only villages within 30 km of a non-amnestied militant camp. Core Niger Delta maintains the same treatment definition, but restricts the sample to Bayelsa, Rivers, and Delta states. Oil-producing restricts the sample to villages within 10 km of oil infrastructure. Dashed vertical line indicates 2009, the year of the amnesty. The non-militant control group restricts the control group to only villages at least 30 km from a non-amnestied militant camp.

Figure D5: Amnesty parallel trends by militant group outcome



Note: This figure shows estimated event-study regressions 95% confidence intervals clustered at the village level by militant control group outcome. Defeated are villages within 30 km of camps belonging to group where the commander was arrested or killed pre-amnesty. Undefeated are villages villages within 30 km of non-amnestied camps where the commander was active at the amnesty date.

D.3.2 Controlling for outcomes of non-amnestied neighbors

An alternative explanation for increased oil theft in amnestied locations is that the increase is due to neighboring, non-amnestied militants who turn to theft because they lack amnesty payments. We investigate this displacement by controlling for the distance to and outcomes of neighboring, non-amnestied camps, with results reported in Table D6. Column (1) reproduces the main specification for reference. Columns (2)-(6) consider the presence, distance, and number of non-amnestied camps and disaggregate their status as defeated in battle or rejected. The main effect remains positive, significant and of comparable magnitude throughout. The results provide evidence for a partial displacement effect of non-amnestied camps on local theft. Column (7) controls for camps of unknown status, yielding no significant difference in the main effect.

Table D6: Amnesty and oil theft, militant location choices

Dependent variable	Oil theft						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Amnestied × Post-amnesty	0.448***	0.457***	0.481***	0.353***	0.451***	0.432***	0.432***
	(0.046)	(0.043)	(0.048)	(0.059)	(0.052)	(0.045)	(0.047)
Distance to non-amnestied camp (km) × Post-amnesty		0.002***					
		(0.001)					
Within 30km of non-amnestied camp × Post-amnesty			-0.112**				
			(0.049)				
Non-amnestied camps within 30km × Post-amnesty				0.186**			
				(0.084)			
Within 30km of defeated camp × Post-amnesty					-0.024		
					(0.074)		
Within 30km of rejected camp × Post-amnesty						0.061	
						(0.078)	
Within 30km of unknown status camp × Post-amnesty							0.049
							(0.111)
Spatial standard errors (50km)	0.101	0.104	0.123	0.135	0.122	0.114	0.110
Village FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Year FE	No	Yes	Yes	Yes	Yes	Yes	Yes
Observations	61332	61332	61332	61332	61332	61332	61332
R^2	0.396	0.404	0.404	0.404	0.404	0.404	0.404

Note: Standard errors in parentheses clustered at the village level. Spatial standard errors are calculated following Conley (2010). Outcome variable is a dummy for any oil theft events within 5 kilometers of the village. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group is all untreated villages outside of this range. Controls include distances to nearest oilfield, pipeline, state capital, Niger River, coast, and population density. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

D.3.3 Controlling for indeterminate locations in control group

Recall that we define a village as treated if within 30 kilometers of an amnestied camps. Notice that this definition will count as treated "indeterminate" villages

Table D7: The effect of amnesty on oil theft, robustness to indeterminate locations

Dependent variable	Oil theft							
Sample	Treated or militant				Treated or militant, indeterminate dropped			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Amnestied × Post-amnesty	0.925***	0.947***	0.993***	0.865***	0.783***	0.779***	0.819***	0.803***
	(0.102)	(0.103)	(0.107)	(0.098)	(0.096)	(0.093)	(0.095)	(0.117)
Distance to oilfield (00s km) × Post-amnesty		-2.660***	-0.549			0.419	1.256**	
		(0.711)	(1.012)			(0.572)	(0.553)	
Distance to pipeline (00s km) × Post-amnesty			-3.198***				-1.202***	
			(0.729)				(0.232)	
Spatial standard errors (50km)	0.266	0.266	0.271	0.247	0.250	0.237	0.240	0.223
Village FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls × Year FE	No	No	No	Yes	No	No	No	Yes
Observations	34116	34116	34116	34116	15264	15264	15264	15264
R^2	0.401	0.402	0.402	0.413	0.294	0.295	0.295	0.329

Note: Standard errors in parentheses are clustered at the village level. Spatial standard errors are calculated following Conley (2010). Outcome variable is a dummy for any oil theft events within 5 kilometers of the village. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group is all untreated villages outside of this range but within 30 kilometers of an amnestied militant camp. Sample in columns (1)-(4) is all villages within 30 km of amnestied or non-amnestied militant camp. Sample in columns (5)-(9) drops camps of "indeterminate" status that are within 30 kilometers of *both* amnestied and non-amnestied militant camps. Controls include distance to nearest oilfield, distance to nearest pipeline, distance to state capital, distance to Niger River, distance to the coast, and population density. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

that are within 30 kilometers of both amnestied- and non-amnestied camps. To verify whether this arbitrary definition affect our results we compute the effect of amnesty on oil theft including or excluding the inclusion of indeterminate villages, with results reported in Table D7. Column (1) reproduces column (5) in Table D4 above, the specification with militant-only control group. Column (5) reproduces the main specification with the exclusive definition. Columns (2), (6) and (3), (7) control for distance to oil infrastructure. Finally, columns (4) and (8) include interacted year- and village fixed effects. The effect remains significant and of comparable magnitude throughout.

D.3.4 Oil output

A plausible but uninteresting explanation for the result, which does not necessarily suggest a violation of any key assumptions but is nonetheless outside the model, is that rebounding oil production mechanically increased the level of theft. In this interpretation, the growth in oil theft is entirely – or at least partly – an adverse consequence of bringing oil output back to pre-conflict levels.

We test this hypothesis by estimating a village-level TWFE model using local oil output as i) the outcome variable and ii) as a control with oil theft outcome variable. Columns (1) through (4) expand the threshold distance from village to

Table D8: Amnesty and oil production

Production radius (km)	5	10	15	20
	(1)	(2)	(3)	(4)
Outcome: Oil output (thousand of bpd)				
Amnestied × Post-amnesty	0.354*	1.782***	1.897**	2.444**
	(0.199)	(0.484)	(0.779)	(1.129)
Spatial standard errors (50km)	0.317	1.069	1.913	2.883
Year FE	Yes	Yes	Yes	Yes
Village FE	Yes	Yes	Yes	Yes
Controls × Year FE	Yes	Yes	Yes	Yes
Observations	42288	42288	42288	42288
R^2	0.779	0.742	0.778	0.807
Outcome: Oil theft				
Amnestied × Post-amnesty	0.553***	0.551***	0.551***	0.548***
	(0.052)	(0.052)	(0.052)	(0.052)
Oil output (thousand bpd)	0.003**	0.002***	0.002***	0.002***
	(0.001)	(0.001)	(0.001)	(0.001)
Spatial standard errors (50km)	0.126	0.126	0.126	0.125
	0.003	0.002	0.002	0.001
Year FE	Yes	Yes	Yes	Yes
Village FE	Yes	Yes	Yes	Yes
Controls × Year FE	Yes	Yes	Yes	Yes
Observations	42288	42288	42288	42288
R^2	0.404	0.404	0.404	0.404

Note: Standard errors in parentheses are clustered at the village level. Spatial standard errors are calculated following Conley (2010). Outcome variable is given in panel header. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group is all villages outside of this range. Sample is all oil-producing villages (defined as within 10 kilometers of oil facility). Controls include distance to nearest oilfield, distance to nearest pipeline, distance to state capital, distance to Niger River, distance to the coast, and population density. Production radius indicates distance around village for which oil production is defined. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

field under which oil production is included from 5 to 20 km. All columns include year, village, and year-control interacted fixed effects. The coefficient on oil theft is positive and of similar magnitude throughout all specifications. The data suggest that while local oil output did rebound differentially in amnestied locations, it does not alter the effect of amnesty on oil theft.

D.3.5 Measurement error

Measurement error might contaminate the effect of amnesty on oil theft if data collection is a function of amnesty and militancy over time. In a plausible story, the environmental regulator's capacity may be growing over time, so they conduct more investigations. Further, this secular trend may not be captured by year fixed effects if it is precisely in militant-controlled regions where collecting data is most difficult pre-amnesty, and therefore improves the most post-conflict. If this is the case, we should observe that malfunctions also rise in amnestied territories post-amnesty, since all types of spills would be subject to the same measurement error. To test for this outcome we re-run our main DD specification (Table 3) using spills due to equipment malfunction as the outcome variable. Results are reported in Table D9 and show precisely estimated null effects with no pre-trends.

D.3.6 Treatment definition

The estimated effect may be sensitive to the use of different thresholds for distance from village to camp and oil infrastructure. First, we should expect to see that as distance from village to camp rises, the estimated effect falls in magnitude, as the radius begins to include villages that are only marginally under militant control when the amnesty occurred. Second, as the threshold distance to oil infrastructure increases, we should expect to see a greater effect of treatment as the universe of possible oil theft events expands. Both effects are borne out in Figure D6.

D.3.7 Maximum likelihood estimation

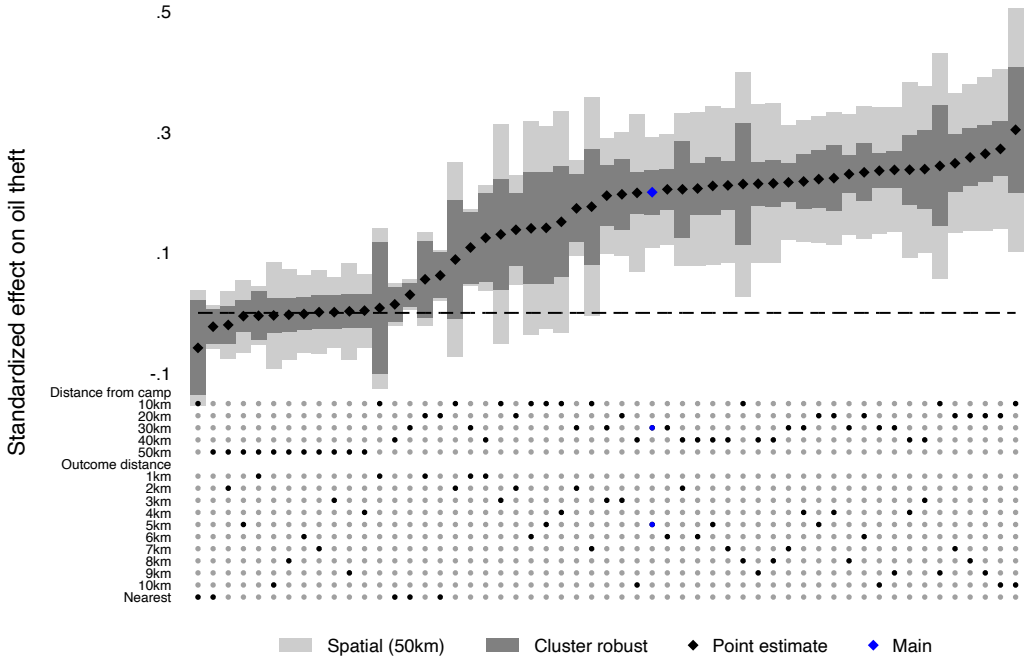
We also consider the robustness of our results to non-linear specifications. In particular, the outcome data is measured as a count and contains zeroes for many

Table D9: Amnesty and operational oil spills

Dependent variable	Operational oil spills			
	(1)	(2)	(3)	(4)
Amnestied × Post-amnesty	-0.006	-0.005	-0.007	0.023
	(0.024)	(0.027)	(0.029)	(0.028)
Distance to oilfield (00s km) × Post-amnesty		0.012		
		(0.044)		
Distance to pipeline (00s km) × Post-amnesty			-0.005	
			(0.054)	
Spatial standard errors (50km)	0.044	0.048	0.051	0.045
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls × Year FE	No	No	No	Yes
Observations	61332	61332	61332	61332
R^2	0.480	0.480	0.480	0.486

Note: Standard errors in parentheses are clustered at the village level. Spatial standard errors are calculated following Conley (2010). Outcome variable is the number of annual operational oil spills within 5 kilometers of the village. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group is all villages outside of this range. Controls include distance to nearest oilfield, distance to nearest pipeline, distance to state capital, distance to Niger River, distance to the coast, and population density. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure D6: Robustness to arbitrary distances



Note: This figure shows robustness across 55 specifications for a difference-in-differences regression of village-level oil theft on the amnesty treatment, as estimated in equation 1. Each black point represents the estimated coefficient of an individual specification surrounded by 95% confidence intervals, with standard errors clustered at the village level. The preferred estimate is indicated in blue. Specification set is all combinations of: *i*) varying the distance from an amnestied militant camp within which the treatment is defined from 10 to 50, *ii*) varying the distance ring around the village for which the oil theft outcome is defined. Specification type is indicated in the figure footer. All specifications include controls for nearest oilfield, distance to nearest pipeline, distance to state capital, distance to Niger River, distance to the coast, and population density interacted with year dummies.

of the village-years. To account for these facts, in Table D10, we re-estimate the main specification (Table 3) using fixed-effects Poisson regressions (Panel A) and a zero-inflated model (Panel B). In the first-stage of the zero-inflated model, we estimate the probability of having nonzero theft as a function of distance to the nearest oilfield and pipeline using a logit model. In both models the estimated effect of amnesty increases between three- and fourfold, all significant at 1%, and the effect is quantitatively similar magnitude across specifications within models. Notice however that the partial effects of distance to infrastructure on oil theft are stronger. The effect of amnesty is quantitatively comparable to the baseline specification in Table 3 when evaluated at mean distances to infrastructure.

D.3.8 Extensive- and intensive margin effects

We show that amnesty receipt predicts the event of any oil theft at the village-year level, and that the effect of amnesty declines in distance to, and increases in the number of, amnestied camps. We therefore rule out that our main results solely reflect variation on either margin. The responses also suggest it is unlikely that countervailing forces on the extensive or intensive margin are skewing our main results.

The extensive margin of oil theft is defined as an indicator for whether a village-year experiences any oil theft. Indeed, oil theft is relatively rare: In our estimation sample we find that 8.5% of the field-years experience any oil theft incident. We re-estimate the main difference-in-differences specifications (i.e. Table 3) using the extensive margin outcome, with results reported in Table D11. The extensive margin effect is statistically significant at the 1% level or below in all specifications. Amnesty increases the probability of oil theft by 5.5-6.5 percentage points, a 88%-104% increase relative to the control group, pre-amnesty mean of 6% prevalence.

We consider the number of amnestied camps within 30 kilometers and distance to the nearest amnestied camp as the intensive margins of treatment. We again re-estimate the main DD specifications using the intensive margin as outcome. The results in Table D12 reveal a positive association between oil theft and the local density of amnestied camps, remain significant at the 1% level, and are of comparable magnitudes across specifications.

Table D10: Amnesty and oil theft: maximum likelihood estimation

Dependent variable	Oil theft			
	(1)	(2)	(3)	(4)
Panel A: Fixed-effects Poisson regression				
Amnestied × Post-amnesty	1.709***	1.709***	1.770***	1.933***
	(0.126)	(0.126)	(0.124)	(0.180)
Distance to oilfield (00s km) × Post-amnesty		-0.003	2.194**	
		(0.824)	(0.924)	
Distance to pipeline (00s km) × Post-amnesty			-11.984***	
			(1.787)	
Year FE	Yes	Yes	Yes	Yes
Controls × Year FE	No	No	No	Yes
Observations	18732	18732	18732	18732
Log-likelihood	-1.76e+04	-1.76e+04	-1.75e+04	-1.73e+04
Panel B: Zero-inflated Poisson regression				
Amnestied × Post-amnesty	1.499***	1.521***	1.543***	1.443***
	(0.135)	(0.141)	(0.144)	(0.181)
Distance to oilfield (00s km) × Post-amnesty		0.923	2.692**	
		(1.135)	(1.135)	
Distance to pipeline (00s km) × Post-amnesty			-9.213***	
			(1.824)	
Year FE	Yes	Yes	Yes	Yes
Controls × Year FE	No	No	No	Yes
Observations	61332	61332	61332	61332
Log-likelihood	-3.13e+04	-3.12e+04	-3.08e+04	-3.01e+04

Note: Standard errors clustered at the village level. Outcome variable is the number of annual oil theft events within 5 kilometers of the village. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group is all villages outside of this range. Top panel uses a fixed-effects poisson regression for estimation. Bottom panel uses a zero-inflated poisson regression, with a first-stage Logit that models selection into oil theft as a function of distance to nearest oilfield and pipeline. Controls include distance to nearest oilfield, distance to nearest pipeline, distance to state capital, distance to Niger River, distance to the coast, and population density. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table D11: The effect of amnesty on the probability of oil theft

Dependent variable	Any oil theft			
	(1)	(2)	(3)	(4)
Amnestied × Post-amnesty	0.060***	0.060***	0.060***	0.076***
	(0.005)	(0.005)	(0.006)	(0.006)
Distance to oilfield (00s km) × Post-amnesty		0.006	0.006	
		(0.006)	(0.006)	
Distance to pipeline (00s km) × Post-amnesty			0.003	
			(0.010)	
Spatial standard errors (50km)	0.017	0.018	0.018	0.020
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls × Year FE	No	No	No	Yes
Observations	61332	61332	61332	61332
R^2	0.414	0.414	0.414	0.427

Note: Standard errors in parentheses are clustered at the village level. Spatial standard errors are calculated following Conley (2010). Outcome variable is a dummy variable if there are any oil theft events within 5 kilometers of the village. Treatment is defined as all villages within 30 km of an amnestied militant camp. Control group is all villages outside of this range. Controls include distance to nearest oilfield, distance to nearest pipeline, distance to state capital, distance to Niger River, distance to the coast, and population density. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table D12: The effect of amnesty on oil theft: intensive margin treatment

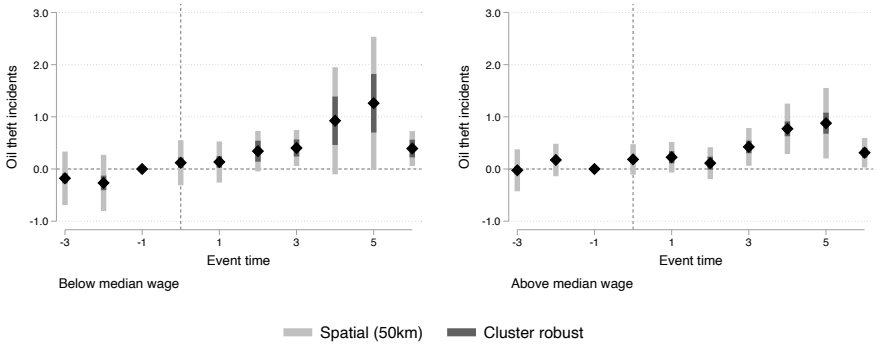
Dependent variable	Oil theft			
	(1)	(2)	(3)	(4)
Treatment variable: Number of amnestied camps within 30 km				
Amnestied camps within 30 km × Post-amnesty	0.066***	0.066***	0.065***	0.073***
	(0.011)	(0.011)	(0.011)	(0.012)
Distance to oilfield (00s km) × Post-amnesty		0.006	0.007	
		(0.043)	(0.042)	
Distance to pipeline (00s km) × Post-amnesty			-0.071	
			(0.054)	
R^2	0.396	0.396	0.396	0.405
Spatial standard errors (50km)	0.023	0.024	0.024	0.026
Treatment variable: distance to amnestied camp				
Distance to nearest amnestied camp (00s km) × Post-amnesty	-0.427***	-0.660***	-0.666***	-0.795***
	(0.054)	(0.088)	(0.092)	(0.108)
Distance to oilfield (00s km) × Post-amnesty		0.576***	0.583***	
		(0.102)	(0.113)	
Distance to pipeline (00s km) × Post-amnesty			0.018	
			(0.072)	
R^2	0.394	0.395	0.395	0.403
Spatial standard errors (50km)	0.122	0.191	0.215	0.308
Village FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Controls × Year FE	No	No	No	Yes
Observations	61332	61332	61332	61332

Note: Standard errors in parentheses are clustered at the village level. Spatial standard errors are calculated following Conley (2010). Outcome variable is the number of annual oil theft events within 5 kilometers of the village. Treatment is defined as either the number of amnestied militant camps within 30 km, or the distance to the nearest amnestied militant camp. Control group is all villages outside of this range. Controls include distance to nearest oilfield, distance to nearest pipeline, distance to state capital, distance to Niger River, distance to the coast, and population density. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

D.4 Heterogeneous event-studies

In Section 6, we show that the effect of amnesty on oil theft is heterogeneous by local black market costs and rebel military strength. In this section, we verify that these heterogeneous effects retain parallel trends in the relevant subsamples. In Figure D7, we split the sample at the median of the market-level young mens' wage distribution and estimate annual event-studies for villages below (left) and above (right) median wages. The plots confirm that both subsamples experience parallel trends, but spike in oil theft in the below-median subsample in the post-conflict period is substantially larger.

Figure D7: Event-study heterogeneity by local wages

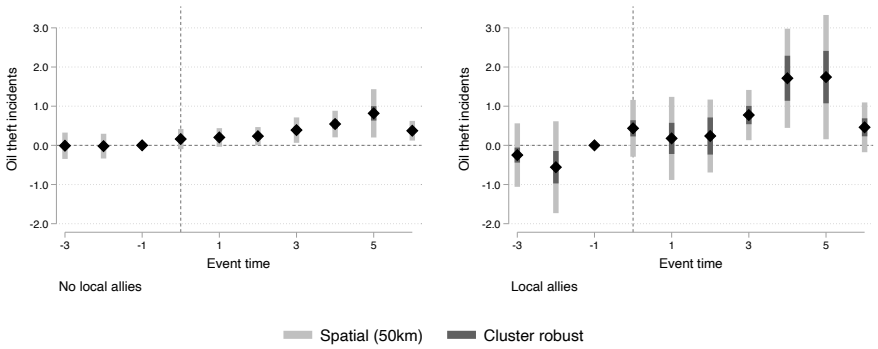


Note: This figure shows the coefficients and 95% confidence intervals from a dynamic differences-in-differences event-study regression of the outcome on dummies for years pre-and-post amnesty, interacted with the treatment indicator, which equals one for villages within 30 km of an amnestied militant camp (see Equation 9). 2008 is the omitted reference group and the outcome variable is oil theft, measured as the total incidents within 5 kilometers of the village. The sample is split by local labor markets below and above the median of the distribution of market-level wages, in the left and righthand subfigures, respectively. Controls include distance to nearest oilfield, distance to nearest pipeline, distance to state capital, distance to Niger River, distance to the coast, and population density interacted with year dummies.

Figure D8 splits the sample by villages in which the nearest militant group has no local allies (left) or more than zero local allies (right). Again, we find that both subsamples experience parallel trends in the years prior to amnesty. However, the post-amnesty increase in oil theft is primarily concentrated in villages where the nearest militant group has more than zero local allies. In contrast, among the

territories of the weakest militant groups there is only a small positive effect of amnesty on oil theft.

Figure D8: Event-study heterogeneity by local alliance density

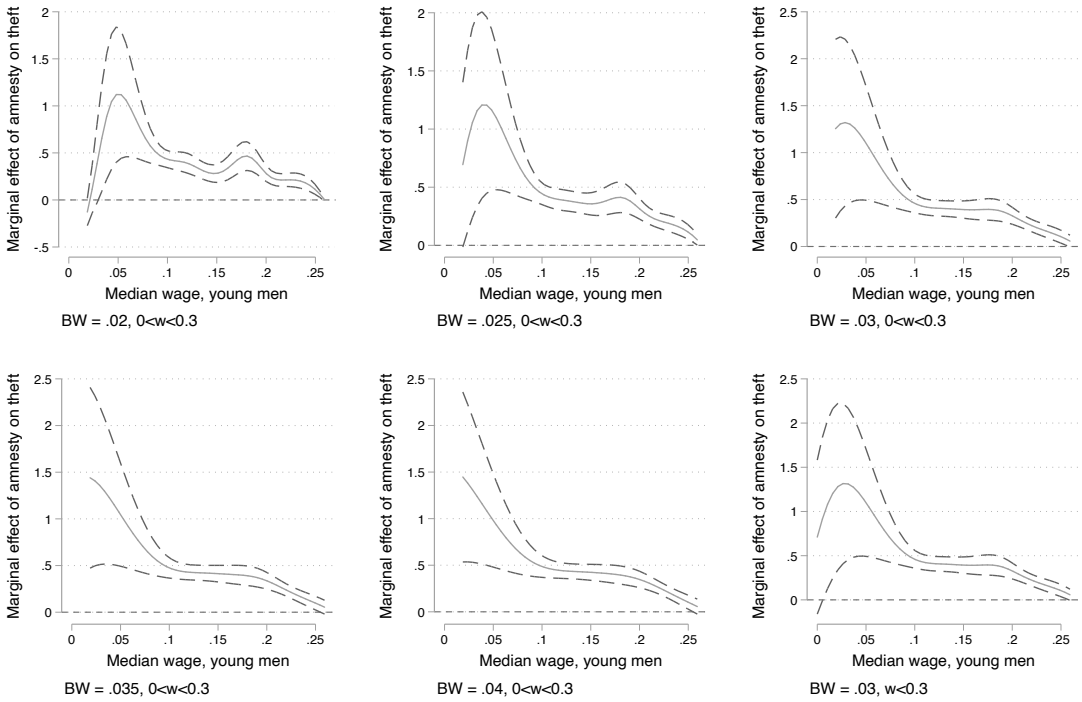


Note: This figure shows the coefficients and 95% confidence intervals from a dynamic differences-in-differences event-study regression of the outcome on dummies for years pre-and-post amnesty, interacted with the treatment indicator, which equals one for villages within 30 km of an amnestied militant camp (see Equation 9). 2008 is the omitted reference group and the outcome variable is oil theft, measured as the total incidents within 5 kilometers of the village. The sample is split by villages for which the nearest militant camp has zero or greater than zero local allies within 10 kilometers along the pipeline, in the left- and right-hand figures, respectively. Controls include distance to nearest oilfield, distance to nearest pipeline, distance to state capital, distance to Niger River, distance to the coast, and population density interacted with year dummies.

D.5 Robustness of nonlinear effects

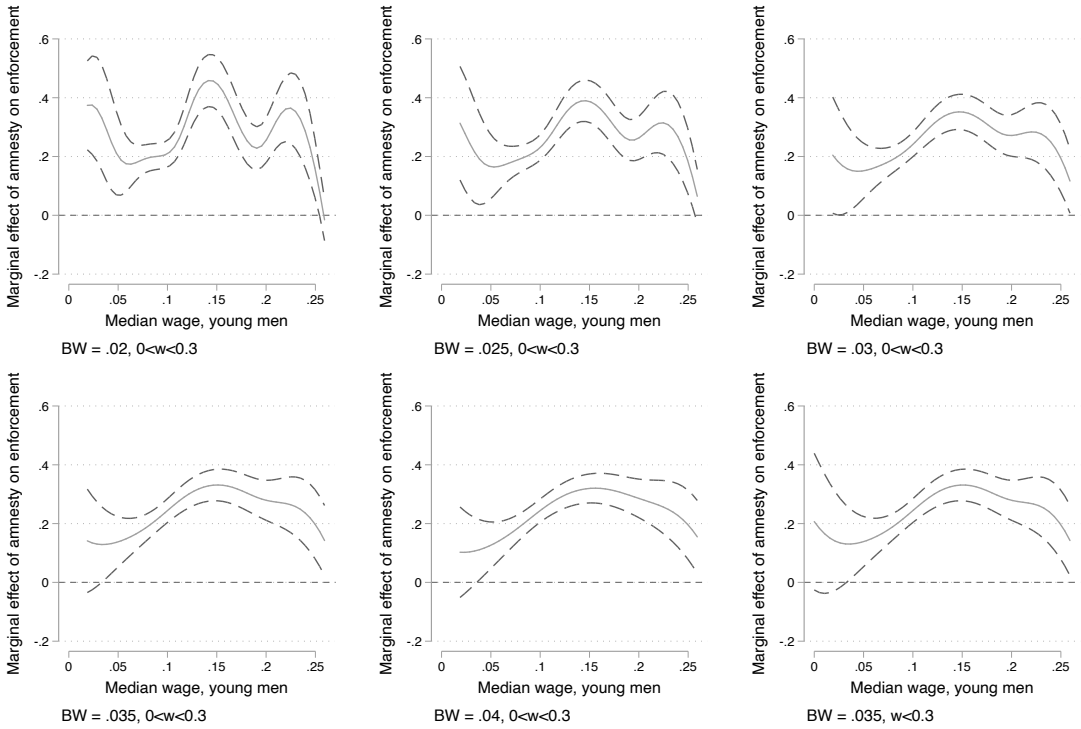
We consider robustness of the nonlinear effects of amnesty on oil theft and enforcement by local black market costs (Figures 7 and 8) to choice of bandwidth and sample trimming. Results for oil theft and enforcement outcomes are reported in Figure D9 and D10 respectively. The permutations across bandwidths and sample trimmings all yield results that are quantitatively comparable to the main specification and qualitatively consistent with the model prediction.

Figure D9: Nonlinear effects on oil theft, robustness to bandwidth and sample trimming



Note: Figure shows shows the marginal effect of amnesty on oil theft by hourly wages for young men in the local labor market. Wages are measured in thousands of Naira per hour. Marginal effects are estimated using a non-parametric kernel regression with varying bandwidth from 20 to 40 Naira per hour, after residualizing year and village fixed effects. Sample is all markets with strictly positive wages below 300 Naira per hour, except in the bottom-right panel, which includes all markets with zero wages (see subfigure notes). All standard errors are clustered at the village level.

Figure D10: Nonlinear effects on enforcement outcomes, robustness to bandwidth and sample trimming

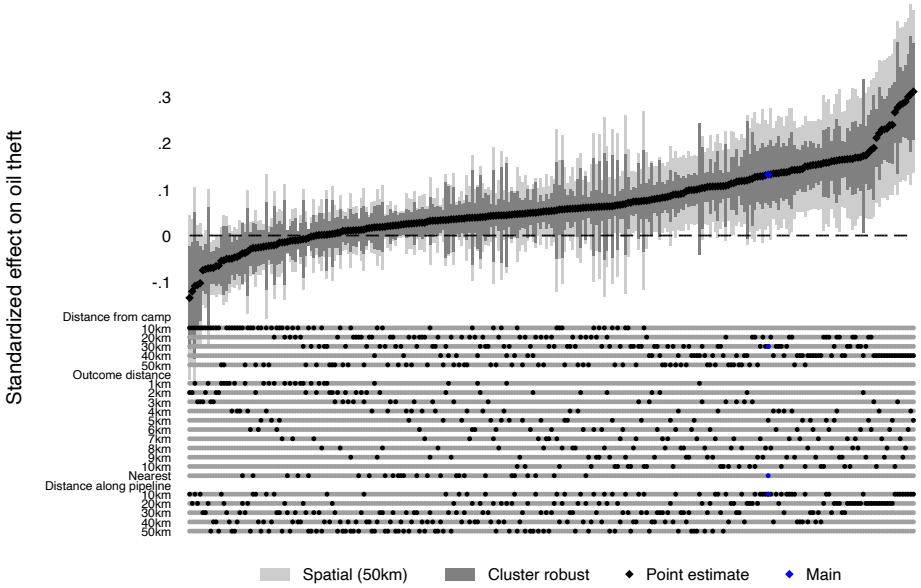


Note: Figure shows shows the marginal effect of amnesty on anti-oil theft law enforcement (right panel) and enforcement against other criminal activities (left panel) by hourly wages for young men in the local labor market. Wages are measured in thousands of Naira per hour. Marginal effects are estimated using a non-parametric kernel regression with varying bandwidth from 20 to 40 Naira per hour, after residualizing year and village fixed effects. Sample is all markets with strictly positive wages below 300 Naira per hour, except in the bottom-right panel, which includes all markets with zero wages (see subfigure notes). All standard errors are clustered at the village level.

D.6 Distance permutations for heterogeneous effects

The results in Table 5 are contingent on *i*) the distances from amnestied camp to nearby villages, *ii*) threshold distances for counting allied camps and *iii*) the distance at which oil theft events are attributed to a village. In Figure D11 we consider robustness of the interaction effect of local alliance density and oil theft to 275 permutations of these definitions. The results are not sensitive to outcome distances. Models with a *i*) more encompassing treatment definition and *ii*) shorter distances for the alliance density count generally yield stronger effects. The former is at least partially due to mechanically counting more oil theft events, the latter likely reflects that military strength is an inherently local property.

Figure D11: Robustness to arbitrary distances; heterogeneous effects



Note: This figure shows robustness across 275 specifications for a triple-differences regression of village-level oil theft on the amnesty treatment and its interaction with rebel military strength. Each black point represents the estimated coefficient of an individual specification surrounded by 95% confidence intervals, with standard errors clustered at the village level. The preferred estimate is indicated in blue. Specification set is all combinations of: *i*) varying the distance from an amnestied militant camp within which the treatment is defined from 10 to 50, *ii*) varying the distance ring around the village for which the oil theft outcome is defined, and *iii*) varying the distance along the pipeline for which military strength is defined. Specification type is indicated in the figure footer. All specifications include controls for nearest oilfield, distance to nearest pipeline, distance to state capital, distance to Niger River, distance to the coast, and population density interacted with year dummies, as well as total camp density along the pipeline interacted with the treatment variable.