Essays on the Economics of Statelessness and State Formation

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ABSTRACT

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This dissertation empirically examines economic exchange in the absence of the state. Analyzing stateless economies is important because traditional economic analysis focuses on an environment in which states are able to enforce property rights, but functioning states are anomalous in the development process and in many developing countries today (Bates, 2011). Standard notions and results of economics may thus not be useful for a large part of the development process (Grossman, 1999). However, in the absence of the state, there is also no systematic data collection capacity. There is therefore no econometric evidence of stateless economies.

In order to observe economic exchange in the absence of a state, I focus on a present-day collapsed state: the Democratic Republic of the Congo (DRC). As a foundation for this dissertation, I managed a team of 12 surveyors, as well as a team of 20 traders to collect data on economic exchange in areas of the Eastern provinces of the DRC that are removed from the central state. Eastern DRC is a well suited location to observe economic behavior in the absence of a functioning state, because the state is unable to operate in large areas. This is a major reason why the DRC is considered the second weakest state in the world, as well as a "failed state".[1] Lacking a state to protect property rights and provide a judicial system, the economy in the East has organized informally. Non-state actors regularly use coercion to define property rights. Contracts are enforced under the threat of social sanctions, or the threat of violence by village armed men who administer disputes.

[1] Available rankings include Fund For Peace (2013).

I draw on this data to analyze economic exchange in the absence of a state. I use standard economic analysis as well as econometrics but introduce different notions than the ones on which traditional economic analysis usually relies (Acemoglu and Wolitzky, 2011, Grossman, 1999, Hirshleifer, 1995). Instead of assuming that property rights are stable, I allow some agents to use coercion in order to define property rights over goods and labor services. In anarchy, coercion can lead to large efficiency losses, since uncoordinated coercive actors do not internalize the distortions they generate on the incentives of those who produce (Grossman, 1999). This leads me to the question of Chapter 1: when will individuals with a comparative advantage in coercion, organize violence in a particular location, and hold a "monopoly of violence"? A dominant approach across disciplines views a "monopoly of violence" as a sufficient characterization of the state. Furthermore, it traces state origins back to coercion and organized crime in medieval Europe (Tilly, 1985). Therefore, the exercise in Chapter 1 uncovers the economic forces that lead to the formation of states - whether these monopolies of violence are recognized as states by the international community or not. However, in doing so, I take the notion of the state as a "monopoly of violence" as given. But is this a useful view of the state? Why should any monopoly of violence, even those with purely criminal motives, act as states? Olson (1993) endows this view of the state with theoretical foundations: since criminals who hold a monopoly of violence are partial residual claimants through the taxes they collect, it is in their interest to protect economic activity and promote growth. Indeed, by increasing economic activity they can increase the tax revenues that they collect. In Chapter 2, I use empirical evidence to test the theoretical foundations that underlie this dominant view of the state. I examine in detail the behavior of village monopolies of violence held by armed men in DRC, and introduce economic analysis to predict state-like behavior: optimal taxation, public goods provision, and popular support. I go a step further to refine this view of the state as a "monopoly of violence", and show

that it is only useful if the monopoly of violence's time horizon is sufficiently long and the monopoly of violence is uncontested. Once I have established empirically the economic causes of state formation and the link between organized crime and states, I turn to the impact of the state on economic organization. In Chapter 3, I implement a field experiment that allows me to observe the impact of the penetration of the state legal system. I find that the state legal system allows for an increase in trade, and does not crowd-out pre-existing social mechanisms generating trust and trade. However, there are limits to the effectiveness of the formal system of contract enforcement. I find that only the groups which have captured the state benefit from the leverage provided by the legal system. This confirms the view of the state as an organization that holds a monopoly of violence, but one which can be captured by a subset of the population. In what follows, I describe each chapter in more detail.

In Chapter 1, I focus on the following question: when do states arise? A dominant view across disciplines is that states arise when violent actors impose a "monopoly of violence" in order to extract taxes (Tilly, 1985). One key fact underlies all existing studies: states precede the existence of statistics. In this chapter, I provide the first econometric evidence on the determinants of nascent states' formation. I conducted fieldwork in areas of DRC removed from the central state, managing a team that collected village-level panel data on armed groups. I develop a model that introduces optimal taxation theory to the decision of armed groups to form local "monopolies of violence", and argue that the returns to such decision hinge on their ability to tax the local population. A sharp, exogenous rise in the price of a bulky commodity used in the electronics industry, coltan, leads armed groups to impose a "monopoly of violence" in coltan villages. A later increase in the price of gold, easier to conceal and hence more difficult to tax, does not. Results from auxiliary tests provide additional support to the theory. The findings support the hypothesis that the expected revenue from taxation, in particular tax base elasticity, is a determinant of state formation.

In Chapter 2, I go a step further, and test the theory associating monopolies of violence to state-like behavior. A dominant view across disciplines defines states as a "monopoly of violence" and argues that states are a developed form of successful organized crime. Is this a useful view of the state? If current states are monopolies of violence and also engage in state-like behavior, this may be the result of a long historical process unrelated to their "monopoly of violence". In this chapter, I examine this view of the state empirically. I provide one of the first studies with econometric evidence linking criminal "monopolies of violence" to state-like behavior. To observe the behavior of criminal "monopolies of violence", I draw on the same data collection exercise as in Chapter 1. I apply optimal taxation theory to armed groups, and show that armed groups will tax and promote growth like states whenever their "monopoly of violence" is uncontested. Based on this data, I find that criminal "monopolies of violence" consistently provide public goods, develop popular support, and elaborate taxation schemes consistent with optimal taxation and European medieval states. In addition, using the timing of a peace agreement as an instrument for the presence of armed men in the village, I establish that the village "monopolies of violence" have a positive impact on economic activity. In 2003, a peace agreement (Sun-city agreement) led a large number of village "monopolies of violence" to relocate, in order to integrate the national government. This led to a security vacuum. I find that the vacuum caused by this peace agreement led to reductions in economic activity. I then use the timing of a large scale military operation in order to instrument for changes in the time horizon of existing village "monopolies of violence", which came under threat as a result of the military operation. I find that when their "monopoly of violence" comes under threat, armed groups turn from partial expropriation through stable taxation with full commitment, to full-scale violent expropriations. These findings support the view that the origins and workings of states can be obtained from criminal "monopolies of violence", but only if they have a stable time horizon.

Having studied the causes that lead to the emergence of states, taxation and coercion, in Chapter 3 I investigate how to take advantage of the state in order to increase trade in contexts where distrust prevents trade to occur. Absent states to enforce contracts, societies may develop group-based mechanisms to generate trade when commitment problems prevail (Greif, 1993). However, groups are often fragmented. Can expanding access to the state legal system complement social mechanisms of contract enforcement sustained by groups? Or does legal intermediation by the state crowd them out? In this chapter, I provide evidence on the *impact* of state contracts on trade. As a foundation for this chapter, I created a home delivery business in DRC. Traders sell a domestic good in households of different ethnic groups, and commit to deliver the good in the future. Shared ethnicity and formal contracts are equally effective at increasing trade by sustaining trust. Furthermore, contracts do not crowd-out ethnic group based mechanisms of trust production. Why do contracts increase trust? Results from a last experiment suggest that contracts are enforceable. However, they only protect claimants of ethnic groups that have captured the state administration. These findings suggest that even the state apparatus is embedded in the social structure, and expanding the reach of the state may have positive welfare effects, only for the groups that have vested control in the state institutions.

Each of the three chapters is self-contained, so a reader interested in only one of these parts can focus on only one chapter. Each chapter contains an overview of the context relevant for that chapter. There are overlaps in the descriptions of the context. Chapters one and two are based on the same data collection exercise, but focus on different variables. Chapter 3 draws on a different field experiment.

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To my whole family,

To the Congolese, and their longing for a better society.

And to all the days I spent in Lehman Library.

Chapter 1

On the Origins of States: Stationary Bandits and Taxation in Eastern Congo

Raúl Sánchez de la Sierra

1.1 Introduction

Without states, coercion can be used in order to expropriate goods and services. Uncoordinated expropriations by actors with access to means of violence can cause large efficiency losses (Grossman, 1999, Hirshleifer, 1995).[1] Indeed, anthropologists have documented that prior to states, levels of arbitrary violence were unusually high. Coordinating coercion, thus, can lead to large welfare gains. When will individuals with a comparative advantage in coercion organize violence in a particular location, and hold a "monopoly of violence"? Since a dominant view across social sciences defines the state as a "monopoly of violence" and empasizes its criminal origins, this question uncovers the process of state formation. This is important: social scientists across disciplines refer to this process as the "Great Transformation" (Bates, 2011) because of its impact on development and violence. It is precisely in a "failed state", the Democratic Republic of Congo, where millions of persons have died in recent years as a consequence of the arbitrary use of violence by armed groups.[2]

Why do states arise? Historical accounts of state formation trace its origins to territorial conquests by medieval lords.[3] A dominant view is that states arise when armed organizations, lords and bandits, impose a "monopoly of violence" in their locality in order to collect tribute and finance wars (Tilly, 1985). Olson (1993) endowed this view of the state with theoretical foundations. He develops a criminal metaphor of states, comparing states to

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[1] "Throughout history the responses of human societies to the problems of distributing property and of allocating resources between productive and appropriative activities probably have had greater consequences for welfare than have their responses to the problem of allocating resources among different productive activities taking property as given, which is the problem on which economic analysis traditionally has focused." (Grossman, 1994)

[2] See I.R.C. (2007). According to Fund For Peace (2013), the DRC is the second weakest state after Somalia.

[3] "The trimmed-down argument stresses the interdependence of war making and state making and the analogy between both of those processes and what, when less successful and smaller in scale, we call organized crime. War makes states, I shall claim." (Tilly, 1985)

"stationary bandits". Stationary bandits own the monopoly of violence, and thus promote growth in order to tax it, in contrast to "roving bandits", who engage in uncoordinated expropriations and discourage economic activity.

A fundamental fact is that prior to the state, there are generally no statistics. There is therefore no formal statistical evidence on the process, let alone the causes of state origins.

In this paper, I provide econometric evidence on the determinants of the formation of "monopolies of violence". I develop a theory that draws on the theory of optimal taxation to explain armed groups' decision to form "monopolies of violence". If medieval lords formed states in order to collect taxes, the choice to form a state must depend on their expected ability to tax. The key insight of the model is that an increase in the value of output increases the returns to form a state, but less so if output cannot be taxed. I test this theory using a novel dataset on armed groups in 140 villages of Eastern Democratic Republic of Congo removed from state control. My results suggest that a positive demand shock on a bulky mineral, coltan, leads armed groups to acquire villages endowed with coltan, and hold a monopoly of violence, where they develop taxation systems. A later demand shock on gold, easier to conceal and hence more difficult to tax, does not have this effect in villages endowed with gold.

As a foundation for this study, I managed a team of data collectors in remote and war-torn areas of Eastern Democratic Republic of Congo in order to collect a village-level panel dataset of yearly activity of armed groups. The data allows me to identify types of violence (territorial conquests and pillage of assets), the formation and collapse of monopolies of violence at the village level, as well as taxation by armed groups on economic activities in the village since 1995.

Eastern Congo is an ideal environment to examine the process that precedes the formation of the state by criminal actors. The Democratic Republic of Congo state is considered

one of the world's two weakest states, as well as a "failed state"; its authority is challenged by armed groups who have proliferated in the East.[4] Armed groups fight to control territory, in which they administer "monopolies of violence", collect taxes, provide public goods, and even enjoy popular support, exactly as in historical accounts of European state formation.[5]

In the main result of the paper, I find that armed groups respond to an increase in the price of coltan by conquering coltan villages, where they establish a monopoly of violence and start collecting regular taxes, especially on mining activity. To establish a causal relationship I exploit a drastic demand shock for coltan. In the year 2000, innovations in the video-games industry led the demand for columbite-tantalum (coltan) to skyrocket.[6] The US price of coltan rose abruptly from 90 US$ per kilogram to 590 US$ per kilogram at the start of the year, and collapsed at the end of the year. The effect of this global demand shock is concentrated in villages where coltan can easily be traded: those in the proximity of local airports.

In the second result of the paper, I find that armed groups *do not* fight over the control of gold villages, and do not create "monopolies of violence" in response to an increase in the price of gold. To establish a causal relationship, I exploit a large rise on the price of gold. Following September 11^{th} and the resulting 2001-2002 global recession, a rush for gold as a safe haven induced the price of gold to rise sharply.

This paper's findings have important implications for theory and policy. The findings provide an economic explanation for the origin of states. They expand a large body of work in economics that emphasizes the role of states and institutions on economic growth and

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[4]See United Nations Security Council (2002), Nest, Grignon, and Kisangani (2011), Stearns (2011) and Verweijen (2013). See also the RRMP program evaluation reports http://www.rrmp.org.

[5]See Sánchez de la Sierra (2014a).

[6]See United Nations Security Council (2002), Nest (2011), Stearns (2011).

explains state choices as economic calculations (Acemoglu and Robinson, 2006, Acemoglu, Ticchi, and Vindigni, 2006, Bates, Greif, and Singh, 2002, Besley and Persson, 2009, Grossman, 1997, North and Weingast, 1989). However, the process of state formation precedes the existence of statistics. The causes that lead to the formation of the state, hence, rest on weak empirical bases. By establishing empirically a causal link from potential tax revenues to the formation of nascent states at the village level, this paper is the first to provide econometric evidence on the process and causes of state formation.

My findings also contribute to existing scholarship outside of economics.[7] Voluntaristic theories argue that states emerged as a result of a mutually beneficial social contract (Hobbes, 1651, Rousseau, 1762). On the other hand, conflict theories of state formation, which are the dominant view, compare the origin of states to organized crime. They suggest that states imposed monopolies of violence by coercion during episodes of war and conquest (Carneiro, 1970, Olson, 1993, Tilly, 1985). My data allows me to go a step ahead of this literature. I first use optimal taxation theory to explain *why* organizations of violence would form "monopolies of violence" in certain locations and not others, and then test the predictions using formal statistical methods.

Historical accounts of European state formation suggest that the growth of trade allowed states to develop (Ardant, 1975). Since much of trade is observable, it can readily be taxed. I find that price shocks give rise to local states particularly around trade routes. My results, therefore, enrich this strand of literature with disaggregated econometric evidence. In a related literature, population mobility is argued to weaken the ability of states to collect taxes and hence to arise (Carneiro, 1970, Herbst, 2011, Scott, 2009). This paper generalizes the problem of factors' mobility into a taxation problem, where the local tax authority, the

[7]The literature on state formation is separated between early and modern state formation. The literature on early state formation is reviewed in Claessen and Van de Velde (1991) and Claessen and Skalnik (2011).

stationary bandit, faces tax bases that can avoid taxes with varying degrees. My findings suggest that the ability of gold producers to hide output is a key barrier to taxation by armed groups. They thus join existing scholarship which emphasizes that population mobility reduces the likelihood that states form. Therefore, in this paper, I test and refine existing theories of state formation in anthropology, sociology, political science, and economics.

This paper also joins a growing civil-war literature. Blattman and Miguel (2010) conclude that the study of civil war is limited by the absence of high quality disaggregated data. I collected a large, detailed dataset on armed groups' behavior, thus, expanding the empirical basis of the civil war literature. Drawing on this novel dataset, I find that demand for labor intensive commodities *increases* violence, aimed at establishing taxation monopolies (unlike pillages). However, Dube and Vargas (2013) find that a rise in the price of a labor intensive commodity increases the returns to productive activities, hence *decreases* participation in violence. By introducing taxation to the set of armed groups' choices previously considered and refining the types of violence, I can provide a new explanation for violence. This paper also complements Weinstein (2007), who proposes a theory where the ability to recruit soldiers determines violence. Last, this paper also joins a growing body of empirical work in the study of civil war (Besley and Persson, 2008, Dube and Naidu, 2010, Dube and Vargas, 2013, Nunn and Qian, 2012). To establish causality, I exploit the timing of large price shocks and political events.

While the state-formation literature takes organized violence as given, the civil war literature explains the motives that lead individuals to engage in violence (Collier and Hoeffler, 2004, Humphreys and Weinstein, 2006, Weinstein, 2007). In this paper, I link these two strands of literature by providing an explanation for why actors that engage in violence may organize violence in a particular location, leading to "monopolies of violence".

The remainder of the paper is organized as follows. Section 1.2 presents the setting and

Section 1.3 presents the theory. Section 1.4 describes the data and data collection strategy and Section 1.5 presents the empirical strategy. Section 1.6 examines the effect of price shocks on armed groups' acquisition and formation of monopolies of violence. Section 1.7 provides a brief discussion and Section 1.8 concludes.

1.2 Background

As a result of the collapse of the central state, armed groups in the Democratic Republic of Congo have proliferated since the 1990's.[8] To finance their operations, they collect taxes on the mineral sector and other activities. Currently, there are 39 identified armed groups.

1.2.1 The conflicts

This study spans two historical phases: the Second Congo War (1998-2003) and the "post-conflict" period (2003-2013).

The Second Congo War (1998-2003) involved a large number of armed groups, and is referred to as the "Great African War". In 1998, the Rassemblement Congolais pour la Démocratie (RCD) launched an offensive to overthrow the former DRC president in office, Laurent-Désiré Kabila.[9] The coup did not succeed, but a myriad of RCD units sustained the control of the Eastern provinces, while the Congolese state defended the West. The RCD struggled to dominate the rural areas, where it faced resistance by the self-defense groups known as the Mayi-Mayi and by the Forces De Libération du Rwanda (FDLR). These groups formed temporary alliances, but they were internally divided. The war also involved the participation of nine foreign armies and thirty local militia, who fought mostly in the east of

[8]The Democratic Republic of Congo was named Zaire until 1997.

[9]The First Congo War lasted from 1996 to 1997. It ended with the overthrow of President Mobutu.

the country.

Despite the support from the UN Peacekeeping force (MONUSCO), in the "post-conflict" period (2003-2013), the Congolese state struggled to regain control over the Eastern Provinces. Following a peace agreement signed in Sun-City (South-Africa) in 2003, the Rassemblement Congolais pour la Démocratie (RCD) agreed to vacate the East of the Congo and integrate the national government. The subsequent withdrawal of the RCD in 2003 and 2004 left large areas in the East without protection. Following the departure of the RCD, the Forces De Libération du Rwanda (FDLR) and local self defense groups progressively increased their territorial control. Only in 2009, the Congolese army, together with the United Nations peacekeeping force and Rwanda, led a major operation against the FDLR. This operation, named Kimia II, successfully weakened the link between the FLDR and its former tax base, although it failed to eradicate it. Armed groups continue to operate in the East, where they control up to 95% of the territory in some administrative divisions.[10] Between May 2012 and November 2013, a new armed group, the M23, established its own administration in a large territory, creating its own Ministries. Ideology, self-defense, and especially economic interests motivate these groups.[11]

1.2.2 The mining sector

Armed groups developed economic interests during the Second Congo War, which persisted during the post-conflict period. One of their major source of revenues is the taxation of

[10]In Shabunda, an administrative divsion in Sud-Kivu that this survey visited, the Raia-Mutombokis control 95% of the territory. See for instance http://radiookapi.net/actualite/2013/02/28/shabunda-la-milice-raia-mutomboki-occupe-95-du-territoire-selon-son-administrateur/

[11]See the following historical accounts: Clark (2002), Nest (2011), Nest, Grignon, and Kisangani (2011), Ngonzola-Ntalaja (2002), Stearns (2011), Vlassenroot and Huggins (2005), Vlassenroot and Raeymaekers (2004), Van Reybrouck (2008), Autesserre (2006), Autesserre (2007), Autesserre (2008), Verweijen (2013).

mineral trade.

Extraction of minerals in Eastern Congo is labor intensive, and is a major livelihood of the local population. World Bank (2008) estimates that there are between 500,000 and 2 millions artisanal miners in DRC, responsible for 90 % of DRC mineral production. Heavy minerals, with high volume productivity of labor (coltan, cassiterite), coexist with light minerals (gold), which are harder to find and, thus, to produce. So-called artisanal mining requires minimal capital and skills. After miners have extracted the heavy minerals, they hand them over to carriers, usually in bags of 50 kilograms. Carriers walk up to one day to the closest village (support village), where local traders purchase the output. Owing to the large volumes produced and a poor road infrastructure, traders ship heavy minerals to Bukavu and Goma by plane. Since trade is vulnerable to pillage, armed groups provide security and collect taxes along the carrier's route. They play a central role in the protection of business (Verweijen, 2013). Gold output, however, is easy to conceal. Because of its large value to weight ratio, gold miners conceal the gold output they are able to find. Miners and traders smuggle gold directly through Burundi, Bukavu or Uvira, circumventing taxes, and Congolese border customs. According to World Bank (2008) estimates, the value of gold exports is US$125 million, most of which is smuggled. A central feature across minerals is that armed groups provide security and collect taxes at the mine as well as on other economic activity at the village.[12]

De jure, property rights are poorly defined. Land belongs to village chiefs according to customary law, who then lease it out to individuals.[13] Formal law, however, often contradicts

[12]The average world price per kilogram of gold in the period was US$17,404, against US$136 for coltan. Daily production per worker is approximately 20 kg in coltan, and between 1 and 10 grams in gold. See De Failly (2001), De Koening (2009), Nest (2011), Geenen (2013) and Vlassenroot and Raeymaekers (2004) for descriptions of the mineral sector.

[13]The Congolese legal system comprises "customary" law, which is the law inherited from local customs. According to customary law, the village chief is the ultimate authority in the village and owner of the land.

traditional law. A large number of state or quasi-state agencies, including criminal groups, tax miners' activity in seemingly arbitrary ways. *De facto*, however, the production process is highly organized, with a well-defined hierarchy. Mining workers are usually residual claimants. Taxes are anticipated, and armed groups often cooperate with the population by offering protection in exchange for taxes. Since production is labor intensive, armed groups rarely mine themselves, but tax miners instead.

In the period for which I have data, the mining sector was affected by two price shocks. First, at the start of 2000, Playstation, a subsidiary of Sony, announced the release for Christmas of a new promising product, which processed columbite-tantalum (coltan). Since coltan extraction, mostly done in Australia by the year 2000, required large investments, supply was inelastic. In response to the announcement, coltan processing firms rushed for coltan from other areas, leading the daily price of coltan to skyrocket from $90 per kilogram to $590 per kilogram, where it persisted until year end. The DRC emerged as a major substitute of Australia's coltan. Second, following 9/11 and the economic downturn, investors rushed for safe assets, including for gold. This led the price on gold to rise in the aftermath of the crisis of 2002, which continued to rise during the post-conflict period (2004-2013). Figure 1.6 shows the world prices of gold and coltan.[14]

1.3 A theory of stationary bandits

In this section, I develop a model where an armed group (henceforth, the bandit) can acquire resources by taxing households' activities. The objective of the model is to elicit how the bandit's ability to tax determines the returns to acquiring monopolies of violence in order to

[14]See Nest (2011), Stearns (2011), and United Nations Security Council (2002) for evidence on the coltan shock.

tax. Empirically, the main causal effect of interest is the effect of mineral demand on armed groups choice to establish or quit taxation monopolies. The model generates three insights. First, an increase in global demand for minerals that are easy to tax (coltan) increases the returns to acquire a monopoly of violence ($P1$). Second, an increase in global demand increases the returns of taxation monopolies *especially* in areas where output can be traded ($P1'$). Third, an increase in global demand for minerals that can easily be concealed, and hence harder to tax, results in a *weaker* effect on the returns to acquire taxation monopolies ($P2$). The model thus applies established results of optimal taxation to the decision to form proto-states. The model is similar to Besley and Persson (2013), although it ignores dynamics. The tax base elasticity can refer to the population mobility across villages, which has played a central role in historical accounts of state development in Africa (Herbst, 2011), and East Asia (Scott, 2009). Tax base elasticity also captures general mobility of factors of production, emphasized in Bates, Ndulu, O'Connell, Collier, and Soludo (2008) and Bates and Lien (1985).

Anecdotal observation dictates the following modeling choices. First, the model considers taxes on inputs and outputs. Armed groups indeed levy taxes on mineral output, taxes on mining labor inputs, taxes on food sales, taxes on transit of persons, taxes on village mills and poll taxes. I focus on labor taxes and output taxes for parsimony, and because they capture in essence the behavioral responses in all other activities. Second, the model considers a static framework where bandits can choose between acquiring civilian assets through uncoordinated pillage or instead attempt to impose a "monopoly of violence", in which case they may extract revenue through taxation, which is announced and anticipated. These extreme forms of expropriation also reflect the anecdotal observation of armed groups' behavior in Eastern Congo. Of 600 recorded detailed violent events in the sample villages, 56% are pillage operations, aimed at capturing village assets. Another 40% are conquest attempts, aimed

at gaining the monopoly of violence in a village, by defeating a competing armed group. Once established, armed groups collect taxes. Figure 1.1 supports the empirical distinction between the two types of expropriation. The figure shows that conquests take place early in the morning, possibly to surprise the defense force, and pillage operations take place at sunset, consistent with a crime deterrence hypothesis (Becker, 1968). Finally, I ignore strategic interactions, because in the period of the coltan shock, one armed group dominates the rest militarily. I discuss limitations of doing so in Section 1.7.3.

1.3.1 Model setup

Consider one village economy and a mass of roving bandits. roving bandits engage in uncoordinated expropriation of households' assets and output. They are roving because they have a short time horizon, and do not internalize future production in the village. One bandit, however, has superior force, and can choose to impose his "monopoly of violence" in the village, turning into a stationary bandit. If he acts as stationary bandit, he announces his theft plan to households (a tax plan) so as to maximize his taxation revenue. The village is composed of k of identical households, $j = 1, 2, ..., k$. In what follows, I drop the household identifiers. Households are endowed with assets and choose the level of the following variables in N sectors $i = 1, 2, \ldots, n$: labor supply, the amount of labor to hide from the tax on labor, and the volumes of output to hide from the tax on output. The stationary bandit is a Stackelberg leader and households are uncoordinated followers.

At step 0, the strong bandit chooses whether to compete over village assets by means of pillaging (as a roving bandit) or to acquire the monopoly of expropriation at the village (become a stationary bandit), at a fixed cost F. The fixed cost captures the costs incurred in the attempt to conquer the village (waging troops, logistics, and expected losses for the group), the investments made to maintain the control of the village, and potentially, the

costs of administering the village. At step 1, the stationary bandit expropriates. If, in step 0, the strong bandit chooses to acquire resources by pillaging, he competes with other roving bandits for expropriation. For simplicity, I assume that roving bandits, since they do not have a long term relationship with households, can't commit not to fully expropriate village assets and output. When there are only roving bandits, therefore, households anticipate that they will be expropriated with probability 1 and do not produce. Therefore, roving bandits can only acquire household assets. If, in step 0, the strong bandit chooses instead to acquire the monopoly of violence (and becomes a stationary bandit), I assume he is able to develop a relationship with the households that allows him to commit to his promise of theft.[15] The stationary bandit expropriates using taxes on output $\tau = (\tau_1, \tau_2, \ldots, \tau_n)$, taxes on labor inputs $t = (t_1, t_2, \ldots, t_n)$. He announces how much he plans to steal (tax) such as to maximize his revenues taking into account behavioral responses by the households. I first present the households' and stationary bandit's problems in step 1, when the strong bandit has acquired the monopoly of violence. I then derive comparative statics of the effect of price shocks on the bandit's choice to acquire a monopoly of violence.

1.3.2 The stationary bandit's economy

This section lays out the optimization problems of households and the stationary bandit, and discusses under which conditions it will not be possible to tax gold output.

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[15]The assumption of binding commitment by the stationary bandit reflects features of a repeated relationship between the bandit and households. Without this assumption, the bandit will never be able to commit and will always expropriate 100% after production. Anticipating this, the households would not produce. However, this scenario leads back to anarchy. Analogous behavior can be obtained by simply introducing a cost in the bandit's payoff from deviating from the taxation plan. For similar treatment of the future, see Dixit (2004)

Optimization problems of the households and the stationary bandit

Labor volume productivity, $\tilde{\alpha} \in R^n$, is stochastic, and has the cumulative distribution function $F(\tilde{\alpha})$. The sector volume productivities, $\tilde{\alpha}_i \in R$ are independent across sectors, and have a known cumulative distribution function $F_i(\tilde{\alpha}_i)$. Output in sector i, $\tilde{y}_i$ is a linear function of labor inputs and volume productivity: $\tilde{y}_i = \tilde{\alpha}_i e_i$. Households are residual claimants. They sell the output they produce in the world markets, where the prices of outputs, p_i, are exogenous. Households' Bernoulli utility is concave in the unique consumption good, and the dis-utility from labor, $c(e)$, is separable across sectors as well as convex in each sector labor supply.

Prior to the realization of uncertainty, but knowing the distribution of volume labor productivities and the vectors of taxes and prices, the households choose how much labor to supply to each sector $e_i,\ i = 1, 2, ...n$ and how much labor to hide from labor taxes $e_i^H,\ i = 1, 2, ...n$. Then, upon observing realized production, households choose the volume of output they conceal from taxes $H_i,\ i = 1, 2, ...n$, given the sector tax on output τ_i and the output price p_i. The stationary bandit expropriates after households have allocated labor supply, hidden labor, and concealed output.

Households' revenues from production in sector i take the following form:

$$\tilde{\pi}_i = (1 - \tau_i) p_i \tilde{\alpha}_i e_i + \tau_i p_i H_i - t_i \left(e_i - e_i^H\right) - G^i(H_i, p_i) - E^i\left(e_i^H, p_i\right)$$

where $G^i(H_i, p_i)$ and $E^i\left(e_i^H, p_i\right)$ are respectively the costs of concealing output and hiding labor. Therefore, $\tau_i p_i H_i - G^i(H_i, p_i)$ and $t_i e_i^H - E^i\left(e_i^H, p_i\right)$ are respectively the profits from concealing output and hiding labor. I assume the two cost functions to be differentiable, monotonically increasing in both arguments, ($G_1^i > 0$, $E_1^i > 0$, $G_2^i > 0$, $E_2^i > 0$) and strictly convex ($E_{11}^i > 0$, $G_{11}^i > 0$). In what follows, I focus on solutions where $e_i \geq 0$, $H_i \geq 0$. The

household's problem is:

$$\begin{aligned}
\max_{e;H;e^H} \quad & \int u\left(\sum_{i=1}^{N}\left(\left(1-\tau_i\right)p_i\tilde{\alpha}_i e_i + \tau_i p_i H_i - t_i\left(e_i - e_i^H\right) - G^i(H_i,p_i) - E^i\left(e_i^H,p_i\right)\right)\right)\mathrm{d}F - c(e) \\
\text{s.t.} \quad & H_i \leq \tilde{\alpha}_i e_i,\ i=1,2,\ldots,N \\
& e_i^H \leq e_i,\ i=1,2,\ldots,N \\
& \sum_{i=1}^{N} e_i^j = L
\end{aligned}$$

where L is the household's total time endowment, and $\tilde{\alpha} \in R^n$ is the vector of labor productivity realizations. The problem is akin to a portfolio allocation problem, albeit here the household allocates its labor to sectors with uncertain volume productivities $\tilde{\alpha}_i$; investment is costly (the dis-utility of labor supply); investment is taxed (the taxes on labor inputs); the returns to investment are taxed (the taxes on output); and the household can conceal part of the returns to investment (by concealing output). The household's first order condition implies conditions under which the household will conceal the totality of output produced. For a given combination of taxes, prices, labor supply, and volume productivity realization, the household will conceal all output produced if and only if the marginal revenue from concealing an additional unit exceeds its marginal cost, i.e. when $\tau_i p_i > G_1^i(\tilde{\alpha}_i e_i, p_i)$. In that case, the optimal volume concealed is a function of τ_i, p_i, e_i, but also of the productivity realization $\tilde{\alpha}_i$, i.e. $H_i^* = H_i(p_i, \tilde{\alpha}_i e_i, \tau_i)$. However, when the household conceals only part of the output he produced (at interior solutions), the volume concealed is independent on the realization, i.e. $H_i^* = H_i(p_i, \tau_i)$. The volume concealed, as a function of realized output has the shape represented in Figure 1.2. The household's optimal choice is characterized by the following functions: $e_i^*(p, F_\alpha, \tau, t)$, $e_i^{H*}(p_i, t_i)$, $H_i^*(p_i, \tilde{\alpha}_i e_i, \tau_i)$. The sector i labor supply, e_i^*, is decreasing in p_j, $\forall j \neq i$. In the absence of income effects, it is increasing in p_i and in $E\alpha_i$.

Finally, with sufficient prudence (large u'''), e_j^* is decreasing in $Var(\alpha_j)$.[16]

The stationary bandit chooses taxes to maximize his tax revenue.[17] Since taxes are communicated to households before they make their choices, the stationary bandit internalizes the households' behavioral responses at the time of choosing the taxes. The stationary bandit's problem is:

$$\max_{\tau,t} \quad \sum_{i=1}^{N} \left[p_i \tau_i \left(\alpha_i e_i^* \left(p, F_\alpha, \tau, t \right) - H_i^* \left(p_i, \tilde{\alpha}_i e_i, \tau_i \right) \right) - t \left(e_i^* \left(p, F_\alpha, \tau, t \right) - e_i^{H*}(p_i, t_i) \right) \right]$$

When input taxes are not available, the revenue maximizing tax rates on output (optimal tax) satisfy the following relationship:

$$\frac{\tau_l^*}{1-\tau_l^*} = \frac{1}{\tilde{\varepsilon}_l^{Y_l} - \sum_{i \neq l}^{N} \frac{\tau_i p_i Y_i}{\tau_l p_l Y_l} \tilde{\varepsilon}_l^{Y_i}} \tag{1.1}$$

where:

$$\tilde{\varepsilon}_l^{Y_i} = \frac{\frac{\partial \tilde{Y}_i}{\partial (1-\tau_l)}}{\tilde{Y}_i} (1-\tau_l)$$

is the elasticity of *observable* output in the sector, and $\tilde{Y}_i = (\alpha_i e_i - H_i) p_i$. Define

$$s_i^e = \frac{\alpha_i e_i}{\alpha_i e_i - H_i}, \; s_i^H = \frac{H_i}{\alpha_i e_i - H_i}$$

The elasticity of observable output $\tilde{\varepsilon}_i^{Y_i}$, can be rewritten as $s_i^e \tilde{\varepsilon}_i^{e_i} + s_i^H \tilde{\varepsilon}_i^{H_i}$. This formula

[16] This reflects the standard precautionary savings motive.

[17] If the bandit would instead maximize a social welfare function, such as a Bergson-Samuelson functional, the optimal taxes would lie between the revenue maximizing taxes and zero. Revenue maximizing optimal taxes can be derived from the maximization of a social welfare function when the weighted average of the social marginal utilities is zero. For a review of the optimal taxation literature, see Salanie (2011) and Piketty and Saez (2012).

captures that the optimal tax is proportional to the inverse of factor elasticities, a well-established result of optimal taxation (Ramsey, 1927). In the absence of distributional concerns, the indexes of relative discouragement do not enter the optimal tax formula. The higher the elasticity of the sector's taxable output, the smaller the optimal tax rate. The last term in the denominator captures the fiscal externalities arising in the presence of multiple taxable activities. Increasing the tax rate in sector l might induce reallocation of labor to other sectors raising the optimal tax.

The impossibility to tax gold output

"Gold is immaterial. We cannot see it." Interview with armed group member

While a worker in coltan mines might produce up to 50 kilograms of coltan per day, a typical day of work in gold yields between 0.1 and 10 grams of gold output. In the absence of sophisticated monitoring technology, concealing gold output must be costless (Geenen, 2013). This feature of gold production is reflected on this project's qualitative fieldwork. Furthermore, anecdotal evidence on gold mines elsewhere, shows that employers frequently use X-rays to monitor gold miners and prevent theft.[18] The majority of villages in Eastern Congo is not electrified, and importing X-rays is unlikely to be affordable. Armed groups' inability to monitor gold output is therefore an important characteristic affecting the choice whether to become the stationary bandit at gold mines in response to price shocks. In what follows $i = g$ denotes the gold sector.

Assumption G1: Let $\underline{H_i} \in R$ such that $\forall H_i < \underline{H_i},\ G_1^i(H_i, p_i) = 0$. Let $\overline{\alpha}_g = sup\{A_g\}$,

[18] "A[...]workers at the end of their shift are ushered into a corridor surrounded by glass and monitored by video cameras. Security guards carefully pick through the workers clothes and give them random full body X-ray." See: http://factsanddetails.com/world.php?itemid=1235 & subcatid=324.

where A_g is the set of gold volume labor productivity realizations. I assume that: $\overline{\alpha_g} L \leq \underline{H_i}$.

Assumption G2: Let F_i be the fixed cost of levying an output tax on sector i. I assume that the expected volume productivity of labor in gold, $E\tilde{\alpha}_g$, is sufficiently low, so that the expected returns from taxing output are lower than the fixed cost of taxing output: $\tau_g^* p_g E\left(\tilde{\alpha}_g e_g - H_g\right) < F_g$.[19]

Proposition 1: Under assumption G1, $\forall \tau_i > 0$, $H_i^* = \tilde{\alpha}_i e_i$. Under assumption G2, $\tau_i^* = 0$.

Assumption G1 fits the anecdotal evidence, and guarantees that it is optimal to conceal any output realization in gold. Under assumption G1, a tax on gold output is irrelevant. Assumption G2 guarantees that it is not profitable to tax gold output. Assumptions G1 and G2 are therefore both plausible and separately sufficient conditions for $\tau_g^* = 0$. This result is non-trivial: while lower average volume labor productivity $E\tilde{\alpha}_i$ leads to lower output taxes, it does not necessarily lead to absence of output taxes, as can be seen from Equation 2.1.[20]

1.3.3 Main predictions: Prices and monopolies of violence

The strong bandit will choose to impose a monopoly of violence if:

$$\tilde{\pi} V\left(p, F_\alpha, a_0\right) - F > V_r$$

[19]This assumes that the left hand side is increasing in $E\tilde{\alpha}_g$. From the first order conditions, it can be seen that e_g is increasing in $E\tilde{\alpha}_g$ in the absence of income effects, and H_g is independent on $\tilde{\alpha}_g$ at an interior solution. Finally, under the same assumptions, from equation 2.1, τ_g^* is increasing in $E\tilde{\alpha}_g$.

[20]In particular, the optimal volume concealed H_i^* is a function of τ_i. Concealing all realized output for any τ_i is only possible if $\forall \tau_i > 0$: $\tau_i p_i > G_1^i(\tilde{\alpha}_i e_i, p_i)$. However, under the assumption that $G_1^i > 0$, $\forall H_i$, there exists always some $\tau_i\left(\tilde{\alpha}_i e_i\right) > 0$, such that $\forall \tau_i < \tau_i\left(\tilde{\alpha}_i e_i\right)$, $H_i^* < \tilde{\alpha}_i e_i$. It must therefore be that there is either a fixed cost of imposing a tax, or that the marginal cost of hiding is approximately zero at low levels of volume produced.

Where $\tilde{\pi} = \pi\beta$ is the effective discount factor. The term π is the probability that the stationary bandit maintains the monopoly of violence until production realizes, and β is the time preference. As above, let $i = c$ denote the coltan sector, $i = g$ denote the gold sector, and $\tilde{e} = e_i - e_i^H$ the observable labor input in sector i. Assume a village is endowed only with one sector (either gold or coltan), and that either assumption G1 or G2 holds. Then:

$$\frac{\partial V}{\partial p_c} = \tau_c \alpha_c \left(e_c + p_c \frac{\partial \tilde{e}_c}{\partial p_c} \right) + \frac{\partial \tilde{e}_c}{\partial p_c} t_c > 0,$$

and

$$\frac{\partial V}{\partial p_g} = \frac{\partial \tilde{e}_g}{\partial p_g} t_g > 0$$

This expression shows that a rise in the output price increases the revenues from becoming a stationary bandit in the village through three distinct channels. First, it raises the value of output taxed, for a given level of output volume and tax level. Second, by leading to an increase in labor supply, it increases the volume of output produced, and hence the value of output taxed. Finally, since labor supply increases, the price shock also increases the revenues from taxing labor inputs. In gold villages, only the third channel changes the revenues to the bandit. The relative effects in gold and coltan depend on the labor supply responses and the level of input taxes. Proposition 2 establishes sufficient conditions for this result formally.

Proposition 2: Let $G(H) = \frac{ph}{2} H^2$, $G(e^H) = \frac{c}{2} e^{H^2}$, and $c(e) = \frac{1}{2} e$. Then,

$$\frac{\partial V}{\partial p_c} - \frac{\partial V}{\partial p_g} > 0$$

These results lead to the main predictions. In what follows, I refer to village"monopoly of violence" and state interchangeably.

Prediction $P1$: Price shocks and the returns to form states. A rise in the price of coltan increases the value of establishing a monopoly of violence in coltan villages. As a result, in coltan villages, armed groups will impose a village monopoly of violence in order to tax. In villages in which there was no state, armed groups will establish a monopoly of violence; in villages in which there was already a monopoly of violence, they will attempt to conquer it, leading to the emergence of new stationary bandits when conquests are successful. In addition, in both cases, a rise in the price of coltan should lead to investments aimed at deterring entry by competing invaders in coltan villages. In the empirical section, I use acquisition of firearms to proxy for investments in deterrence.

Prediction $P1'$: Price shocks, trade infrastructure, and the returns to form states. A rise in the price of coltan increases the value of establishing a monopoly of violence, *especially* in coltan villages near trade infrastructure. Since coltan can only be shipped by air, Prediction $P1'$ implies that Prediction $P1$ will hold *especially* in villages sufficiently close to airports.

Prediction $P2$: Price shocks, gold, and the returns to form states. A rise in the price of gold increases the value of establishing a monopoly of violence in gold villages *less* so than an identical value per unit increase in the price of coltan in coltan villages. As a result, armed groups will respond to a price rise in gold *less* than in response to an increase in the price of coltan of identical value per unit.

1.3.4 Auxiliary predictions

I have now established that stationary bandits will be more likely when the price of a taxable commodity is high. There are additional testable implications from the theory. If setting up a monopoly of violence capable of collecting taxes entails a fixed cost F and the marginal cost of taxing an additional household in the village is negligible, then villages with a larger number of households, k, should be more profitable for stationary bandits, $\frac{\partial V}{\partial k} > 0$, and stationary bandits should be thus more likely in villages with large k. Furthermore, since $\frac{\partial V}{\partial p} > 0$, then villages endowed with minerals in addition to agriculture will generate higher tax revenues to stationary bandits. stationary bandits should thus be more likely in villages endowed with valuable minerals than in villages relying only on agricultural activities.

1.4 The data

This section describes the data collection, defines the main variables used in this paper, and presents strategies used to minimize measurement error. I then present the summary statistics.

1.4.1 Data collection

In order to obtain data on armed groups' violence and taxation, I designed a data collection project in Eastern Congo. I trained and monitored 11 local surveyors who collected the data between May 2012 and September 2013. In each village, the surveyors trained a group of village history specialists (village chiefs, elders, and mining sector experts) for the collection of non-sensitive historical data under adequate compensation (henceforth, the village specialists). The data collection activities followed a strict protocol, designed to last 7 days per village. During the day, the surveyors implemented retrospective household surveys, in

private. In the evenings they provided guidance and corrected the progress of the village specialists. In the last day of data collection activities in each village, after having implemented 8 household surveys, the surveyors held a day-long meeting with the village specialists. In this meeting, the researchers and village specialists confronted the data gathered from the different sources, and surveyors collected additional sensitive data based on the information they gathered in the household surveys. The data from this meeting compiles information from all activities and is the main source of data used in this paper.

Within each administrative division, I drew a random sample of gold villages, and the population of coltan villages.[21] Figure 1.3 maps the villages in the sample based on their endowment of minerals. The villages endowed with coltan and gold are well distributed across the various administrative divisions, with no clustering of villages by mineral endowment.

1.4.2 Measurement

This section describes the main variables I use in this paper.

The village mineral endowment was already identified during *prospection* work. I did not record any discrepancy between the mineral endowment from prospection and survey data indicating whether the village had exploited a given mineral. Therefore, I define the mineral

[21]To identify the villages endowed with gold, coltan, or other minerals, I used the data from a mapping implemented in 2009 by IPIS, a Belgian research institute, and SAESSCAM, a local mining institute. To verify these lists, I interviewed in Bukavu a large number of mineral dealers working through Sud-Kivu, during 3 months of preparatory work. Based on the compiled dataset, I endowed the surveyors with a list containing all mining villages of each administrative division. In order to strengthen the representativeness of the sample to be selected, I designed a strict *prospection* protocol. Following this protocol, in the first week of work in each administrative division, the surveyors verified the existence and endowments of the villages in the list, and added mining villages they discovered to exist. Upon finishing *prospection* work, and in areas where they had access to electricity, the surveyors communicated the cross-checked lists by satellite phone. I then implemented random sampling of gold villages using a statistical package and communicated the selection using satellite phone. In areas without access to electricity, the surveyors followed a strict protocol for selecting gold villages, based on a list of random numbers I previously generated using a statistical package.This was only applied in one administrative unit (Shabunda).

endowment for this paper as whether the mineral has been exploited at any point for which I have data.

I take the yearly prices of minerals from United States Geological Survey (2010). Figure 1.6 shows the world prices of gold and coltan.

I define the presence of a stationary bandit in a given village as whether an armed person or group had a monopoly of violence in the village for at least 6 months in a given year. stationary bandits are common in Eastern Congo, and villagers faced no difficulties identifying stationary bandits from roving bandits; the former have no intention to leave the village in the short-term, and usually provide protection to the whole village. The language used to identify stationary bandits is "organization of security". Therefore, implicitly, stationary bandits always provide security.

To measure violent events, focus on two major types of attacks, which reflect the anecdotal evidence. I measure *conquest attempts* by whether an armed group engages in violence with another armed group, with the aim of acquiring the monopoly of violence of the village. I define *pillages* as violent events on the village aimed *exclusively* at capturing assets. This definition reflects the observation that local populations are able to distinguish the purpose of the attacks unambiguously. *Pillages* also differ from *conquest attempts* on observable characteristics, which facilitates their distinction for local populations. In a *pillage*, armed groups usually arrive at sunset and flee with the village assets within a narrow period of time. *Conquest attempts* take place early in the morning and violence is targeted at the group controlling the village, with the aim to defeat them.

To measure taxes, I collected both whether a tax was levied on a given activity, and the level of the tax. There was always consensus on whether an expropriation was a tax. Local populations are familiar with distinct types of expropriation by various actors. While a tax is always anticipated and reflects an implicit long-term contract between those who collect

taxes and the population, other types of expropriation (*pillages*, or arbitrary expropriations, known as "tracasseries") are not.

Having collected the geographic coordinates during the survey, I can link my dataset to geographical shapefiles I obtained from the Réferentiel Geographique Commun.[22] This source contains the map of the road network of the DRC, all airports (including small landing lanes), the location of forests, rivers, lakes, and the regional capitals. I compute the shortest distance of each village to the road, the lake, the forest, the regional capitals, Rwanda, and the closest airport. In the analysis, I use a dummy variable indicating whether the distance to the closest airport is above the 50% percentile in the sample. The results are unchanged when I use the continuous measure. The next section describes the strategies implemented to address measurement error and potential reporting biases at the data collection phase.

1.4.3 Design based strategies to minimize measurement error

To address measurement error at the design phase, I used three strategies. First, I designed the village protocols so that surveyors received multiple signals on the village history from private conversations during the 7 days of implementation. Surveyors compiled the information collected in household surveys with the data assembled by the village specialists. This strategy was highly effective as an averaging method. It also helped surveyors correct information provided by the village specialists during the final meeting. Indeed, households were *overwhelmingly* willing to reveal armed groups' data in private, and there was consensus that this was not problematic.[23] To provide additional confidence on the survey measures,

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[22] See Référentiel Géographique Commun (2010).

[23] Armed groups were absent in most villages. The Congolese army already protected a large number of the sampled villages. In the Territoire of Shabunda, certain villages were under the control of the Raia-Mutombokis at time of the interview. The Raia-Mutombokis did not consider sensitive the information collected by this survey. This armed group had existed for less than one year. Surveyors collected historical

Section 1.7.1 compares the data to other data sources, as well as well-known political events. Second, the teams implemented an exhaustive set of time cues, based on information gathered in the survey pilot, and on well known regional and national political events. Time cues allow respondents and surveyors to assign with certainty events to years, based on common knowledge of regional events (de Nicola and Gine, 2012). Third, based on the pilot villages, survey questions focused on transitions and events easy to memorize. For instance, there are two types of bags that carry the heavy minerals (50kg and 75kg). To measure output taxes, surveyors obtained the fee paid on one bag and bag size used. I then compute the tax per kilogram based on these variables. To measure taxes on labor, surveyors obtained the daily fee paid at the entry of the mine to obtain the right to work. These fees are stable. Crucially, because mining taxes are the heavier daily expense of miners, they were straightforward to collect. The responses of households and village specialists were highly correlated at time of the interviews.

1.4.4 Descriptive statistics

Figure 1.4 shows armed groups' presence in the sample villages over this period. There are 18 armed organizations in the sample, although most of the variation is concentrated in four groups: the Congolese Army (40% of control year*villages and 5% of attacks), Mayi-Mayi's (20% of control year*villages and 21 % of attacks), RCD-Goma (15% of control year*villages and 11% of attacks), and FDLR (12% of control year*villages and 45% of attacks). While the RCD and the Mayi-Mayi's were the main actors during the Second Congo war, the Congolese Army and the FDLR are 85 % of the occupation year*villages after 2006. Figure 1.5 shows the distribution of the length stationary bandits' episodes. The median length of episodes

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data on events preceding the Raias and about the Raias without problems. Surveyors were confident that they achieved the same degree of quality as in other villages, including for data on the present years.

with each stationary bandit is 4 years, exactly as the median unprotected episode. Table 1.1 shows summary statistics. In Sánchez de la Sierra (2014a), I show that these stationary bandits provide security, administer justice, administer village bureaucracies, and even enjoy legitimacy in a large fraction of the villages in this sample.

1.5 Empirical strategy

This section examines the effect of the coltan and gold price shocks on armed groups activity. I first present the econometric specification and discuss how I address potential threats to identification. I then turn to the results of the baseline specification as well as their robustness to alternative specifications.

1.5.1 Main predictions: econometric specification

The main causal effect of interest is the effect of mineral demand on armed groups' conquest and emergence of new village stationary bandits. To measure the rise in the value of acquiring a violence monopoly, I use the following variables: conquest operations aimed at capturing the village; emergence of new stationary bandits; investment in deterrence (acquisition of weapons). However, identifying demand shifters poses a challenge. First, local prices are endogenous and might reflect changes in supply. For instance, by exercising local monopoly power or depressing supply, armed groups might inflate local prices. Second, local prices are retrospective, and despite efforts to increase the quality of data collection, they might contain large measurement error; this may be systematically different across minerals or periods. I use the world prices and the timing of world price shocks instead. The main specification is an OLS regression on the world prices interacted with dummies indicating the (constant) mineral endowments at the village-level, with village and year fixed

effects.[24] While constant unobserved heterogeneity and common year effects are absorbed in the fixed effects, imbalance between coltan villages and gold villages could be associated with differential time trends. To reassure that coltan and gold villages are comparable on observable characteristics, Table 1.2 shows the balance on observables between coltan and gold villages. Of 11 outcomes considered, there is only imbalance in one (distance to closest bridge in 2010), although this variable is post-treatment. In Section 1.6, I describe alternative methods I have used to analyze rare events data. Equation 1.2 presents the baseline specification:

$$Y_{it} = \beta_t + \alpha_i + \gamma_c C_i P_{ct}^{world} + X_{it}^{'}\beta + \varepsilon_{it} \tag{1.2}$$

where Y_{it}, depending on the specification, is a dummy indicating the presence of a village monopoly of violence (stationary bandit) in village i in year t, whether the village was subject to a conquest attempt, whether the village was subject to pillages, as well as whether the a village stationary bandit acquired weapons. α_i are village fixed effects, β_t are the year fixed effects, and X_{it} is a vector of village level time varying controls. C_i is a dummy for whether village is endowed with coltan, and is constant over time. P_{ct}^{world} is the world price of coltan at year t. Regressors P_{ct}^{world} and C_i are absorbed respectively by the year and village fixed effects. I do not use time-varying controls, since these are potentially endogenous to treatment and could lead to bias.[25] However, I address a number of omitted variable problems by estimating the time-varying effects of time invariant covariates, which I describe in this section. To account for over-rejection stemming from autocorrelation of errors and prices, I cluster the standard errors in all regressions at the level of the village.[26]

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[24]The results obtained from instrumenting local prices with world prices are identical.

[25]This is the "bad control" problem in Angrist and Pischke (2009).

[26]Bertrand, Duflo, and Mullainathan (2004) demonstrate that autocorrelations in the treatment and outcome lead to underestimation of the standard errors in studies based on differences in differences.

A test of prediction $P1$ is $\gamma_c \leq 0$ against the alternative $\gamma_c > 0$. In what follows, I refer to γ_c as the main coefficient. To test prediction $P1^{'}$, I include a dummy indicating whether the distance to the closest airport is above the sample median. I choose this strategy for ease of interpretation and to avoid making linearity assumptions if I instead used the continuous distance variable. The results are identical when I use the distance in kilometers. Equation 1.3 presents the specification I use to test prediction $P1^{'}$:

$$Y_{it} = \tilde{\beta}_t + \tilde{\alpha}_i + \tilde{\gamma}_c C_i P_{ct}^{world} + \tilde{\gamma}_{ca} C_i P_{ct}^{world} d_i^a + \tilde{\gamma}_{pa} P_{ct}^{world} d_i^a + X_{it}^{'} \tilde{\beta} + \tilde{\varepsilon}_{it} \tag{1.3}$$

where $\tilde{\alpha}_i$ are village fixed effects, $\tilde{\beta}_t$ are the year fixed effects. The term d_i^a is a dummy variable indicating whether the time invariant distance to the closest airport is above the median closest distance to an airport in the sample. Prediction $P1^{'}$ therefore implies that $\tilde{\gamma}_c > 0$ and $\tilde{\gamma}_{ca} < 0$. Finally, when I include the main interaction for gold, $\gamma_g G_i P_{gt}^{world}$, a test of prediction $P2$ is $\gamma_c \leq \gamma_g$ against the alternative $\gamma_c > \gamma_g$.

1.5.2 Main predictions: threats to validity

In what follows, I discuss the threats to identification of the main effect ($\gamma_c > 0$).

One may worry that the coltan shock could be endogenous. The source of the coltan shock, however is well documented. A demand shock in the coltan processing industry in response to innovations in video-games generated a rush for coltan in DRC. It is therefore very unlikely that the world price armed groups actions had any causal effect on the level of the

worl price.[27][28]

A plausible worry is that, given it only takes place in one year, the *timing* of the coltan shock may coincide with events that occur systematically in coltan villages for reasons unrelated to the price. Since the coltan shock occurred during an episode of war, it is possible that the difference in conquests between coltan and gold villages is larger during the war than on average. This will be the case if geographic characteristics interact with coltan endowments and are related to the war but unrelated to the price shock. For instance, the presence of the Congolese Army in the second period might act as a deterrent on armed groups conquests in coltan villages. To address this, in regressions on the whole period, I include controls in the specifications for presence of the Congolese army. To measure the presence of the Congolese army, I use a dummy indicating whether the army controls a given village in a given year. In addition, I also include the proportion of neighboring villages that are under the control of the Congolese army in a given year. I operationalize "neighboring villages" by estimating the average number of sample villages in the same administrative division that are under the control of the Congolese army, replicating this procedure for all

[27]Local supply response could have had a dampening effect on the world equilibrium price. In this case the observed price is lesser than the price that would have obtained in the absence of supply response. Since I focus on the coefficient sign, this does not affect my interpretation. The results are qualitatively identical when I use the timing of the coltan demand shock instead of the price level.

[28]As a validity check on the relevance of the coltan shock, I collected economic data in the households surveys. The retrospective households' economic data reveals large economic effects of the mineral price shock. Figure A.2 shows the number of marriages per village, yearly as collected in the village survey. Since marriage in the survey area requires the payment of a Bride Price, marriage is a normal good. The coltan shock drastically increased the number of celebrated marriages and led to large reallocation of labor to the mining sector. Second, nighttime satellite lights data confirm the evidence of the coltan shock. Figure A.1 shows that a new bright town emerges in the year 2000 near Goma, the capital of the coltan province. This lighting is absent prior to 2000, and vanishes progressively in the following years. Henderson, Storeygard, and Weil (2011) introduced the nighttime data as a proxy for GDP. While not reported, this lighting is reflected in the provinces average stable lights. In addition, I computed zonal statistics of stable lights in each Province of the DRC and compared the change over time between coltan provinces and non-coltan provinces. The results show an increase in stable lights in 2000 in coltan Provinces, but remain constant in the remaining provinces.

levels of the administration. Since the results are identical with all strategies, I include only the village-level variable. However, the presence of the Congolese army is potentially a bad control (Angrist and Pischke, 2009). For robustness of the main estimate, I therefore run the baseline regression restricting the years to the Second Congo War (1998-2004), when the Congolese army is absent and the identity of the main armed groups is stable. Furthermore, since the coltan shock occurred during an episode of war, the *sequence* of conquest could reflect omitted variables that are correlated with mineral endowments. In particular, even when the analysis is restricted to the war years, it could be that coltan villages are more remote, and that armed groups first occupied villages near the road, followed by coltan villages. In that case, the timing of conquest of coltan villages could coincide with the timing of the coltan shock. This is unlikely. As Table 1.2 shows, coltan and gold villages are not distinguishable on any observable geographic characteristics, except for the distance to bridges in the year 2010. To also account for geographic omitted variables related to the *sequence* of conquest, I replicate the results including as controls year dummies interacted with distance to the road, distance to airports, bridges, parks, lakes, and main trading towns. These controls capture the time-varying impacts of time-invariant geographical variables that may predict the timing of conquest. I only report the results based on the interaction with distance to the road, since results are unchanged when I use the other variables instead. In what follows, I discuss threats to the interpretation that $\gamma_c - \gamma_g$ is caused by heterogeneous tax base elasticities.

The coltan shock and the gold shocks are of different magnitudes, and take place at different times. This might invalidate interpretations of $\gamma_c > \gamma_g$ in terms of tax elasticities. Shocks of different *magnitudes* might generate very different armed groups' responses if the armed groups' cost functions are concave. For instance, if there is a fixed cost of engaging in conquest, a larger price shock might result in stronger outcomes for reasons unrelated to the

physical properties of production of the mineral, but only related to the *magnitude* of the shock. Furthermore, the *timing* of the shocks may be correlated with omitted variables. For instance, following the Second Congo war in 2003, the Congolese Army progressively regains control. If the Congolese Army acts as a deterrent, even when the difference in differences identifying assumptions for each the separate effects are valid, the estimated value of $\gamma_c - \gamma_g$ would be biased. To address this, I follow the same two strategies that I introduced to estimate the main coltan effect: I control flexibly for presence of the Congolese army, and I restrict the analysis for the Second Congo War years.

Another possible threat to the interpretation that $\gamma_c - \gamma_g$ is caused by heterogeneous tax base elasticities is that, even if coltan and gold villages are distributed "randomly" in the space, populations might not. If populations self select into gold or coltan villages, the patterns of sorting by individual characteristics might be associated with different individual treatment effects of price shocks. In particular, it could be that because of the features of gold production, miners and armed groups specializing in gold have different endowments and might react differently to price shocks. This a weaker concern for the effects on conquest operations, since the decision to engage in conquest is made by bandits outside the village. In what follows, I discuss bias arising in the estimation of the standard errors.

Finally, since the econometric strategy exploits price changes across multiple years, standard errors may be underestimated. This may lead to over-rejection. If neighboring villages are subject to common shocks, an error structure with positive intra-cluster correlation within village clusters could drive differences in outcomes for reasons unrelated to the timing of the shocks (Moulton, 1986). It is unlikely that *spatial* clustering leads to bias in OLS standard errors, because the distribution mineral endowments is balanced within most administrative divisions, as shown in Figure 1.3. For robustness, I replicated all regressions clustering the standard errors at the level of the year interacted with the lowest level of the

administration above the village (Groupement).[29] To account for autocorrelation of village errors over time, all regressions are presented with clustering of standard errors at the village level. To go a step further, I use randomization inference (Gerber and Green, 2012). This allows me to account for arbitrary error structures underlying the main coltan shock. To implement randomization inference, I simulate a large number of fake allocations of the world prices to years and recompute for each price allocation the corresponding coefficient from the OLS estimation. This generates a distribution of coefficients estimated through OLS. Under the null hypothesis that the coltan price has no effect, and ignoring violations of the strict exogeneity assumption, this is the true distribution of the estimated coefficient in the sample. This also allows me to avoid making assumptions on the distribution of errors. If there is a problem of over-rejection because of the error structure, the real coefficient should lie within the 95% of simulated coefficients around the mean. I also use randomization inference to estimate the effects of the distance to airports, given the inherently spatial structure of the distance problem. I next turn to the results.

1.5.3 Auxiliary predictions: econometric specification

To capture village size, I use the number of households listed in the village census at the time of the survey, P_i. In addition, I use the migration data: number of immigrants I_{it} and emigrants by year e_{it}. To capture the tax potential of a village stemming from the value of economic activities, I use a dummy indicating whether the village is endowed with minerals,

[29]Groupement is the lowest administrative division above the village. It generally comprises no more than twenty villages. There are 31 Groupements in the sample, which generate more than 300 clusters for regressions including all years, and 180 clusters for regressions including only the Second Congo War. In addition, I replicated the regressions including coltan*year clustering of standard errors to account for any correlation structure among villages with the same coltan endowment that might not be captured in spatial shocks. This could be the case, for instance, if coltan villages have a common shock structure through economic networks that are not tied to geography.

M_{it}. While agricultural activities are a potential source of tax revenue, minerals are generally more profitable. I implement a pooled of the OLS stationary bandits dummy, Y_{it}, on village size, lagged immigration, lagged emigration and a dummy indicating whether the village is endowed with minerals. Since I do not use fixed effects, and since the dependent variable is likely to have strong autocorrelation, I include in all specifications a lagged dependent variable. Furthermore, since I use repeated observations, and to account for autocorrelation in the regressors and the dependent variables over time, I cluster the standard errors at the village level. Equation 1.4 presents the baseline specification:

$$Y_{it} = \beta_M M_{it} + \beta_P P_i + \beta_I I_{it} + \beta_E E_{it} + \varepsilon_{it} \tag{1.4}$$

The auxiliary testable implications are $\beta_M > 0$, $\beta_P > 0$, $\beta_I > 0$, $\beta_E < 0$.

1.6 Results

This section implements this econometric strategy and takes the predictions of the effect of price shocks to the data. I first describe the results, and in a second part, I show that the results are robust to alternative econometric strategies.

1.6.1 Main predictions: baseline results

The theoretical model considers how changes in the prices of outputs in individual villages affect the choices of armed groups in these villages. In particular, when the value of the village taxable production increases, armed groups will attempt to acquire the village. If the village is unprotected, they might simply march in the village, but if an armed group already controls the village, armed groups may attempt to conquer the village. If the conquest is

successful, they will emerge as a new stationary bandit in the village, and collect taxes. I use these outcomes to test the main theoretical predictions: ($P1$) the coltan shock leads armed groups to attempt to acquire the coltan villages. ($P1'$) this effect must exclusively take place near airports.[30] ($P2$) the effect of the gold price shock is lower than the effect of the coltan shock.[31] I start with the analysis of territorial conquests.

Figure 1.7 shows the proportion of mines in the sample in which armed groups attacked, by mineral endowment: gold (left) and coltan (right). I focus on mines in which there was already a stationary bandit collecting taxes in 1999, since acquisition of the village monopoly of violence is peaceful when the mines have no stationary bandits. While the proportion of gold mines attacked stays around 15% between 1999 and 2000, the proportion of coltan mines attacked increases from 13% to 40%. I next show that these attacks are motivated by the conquest of territory, not pillage. Figure 1.8 is identical to Figure 1.7, but uses conquest operations instead, which I observe for the village hosting the mine. Again, I focus on villages in which there was a stationary bandit in 1999 collecting taxes in the vilalge mine. The spike in conquest attempts in the year 2000 in coltan villages suggests that the coltan shock led to an increase in territorial conquests.

Table 1.3 presents the results on conquests from the OLS regression. I use all villages in the sample. While this reduces the coefficient size (conquests do not occur in villages without stationary bandits), it allows me to increase precision of the coefficient estimators. Starting

[30] A possible concern is the airports may be constructed at a low cost. The qualitative fieldwork suggests that mining areas were often located in remote forests under the control of numerous adverse armed groups. This made building new landing lanes impossible. It is not implausible, however, that had the coltan shock lasted longer, the armed groups might have engaged in creating new airports.

[31] In what follows, I present the results pooling all villages. However, the results on conquests are concentrated in the villages that had already a security force present in the previous year, and the results on stationary bandits are concentrated in the villages that did not have one previously as expected. Pooling all villages weakens my result because it captures the average effects, but I always include all villages for consistency.

with prediction $P1$ and $P1'$, the results confirm that the coltan shock led to an increase in conquest attempts in coltan villages near airports. Columns (1)-(4) present the results based on the whole period, while columns (4)-(9) restrict the years to the Second Congo War (1998-2003), and columns (10) and (11) focus on the years 1999 and 2000. Column (1) presents the results from the baseline specification. As expected from prediction $P1$, an increase in the coltan price leads to an increase in conquest attempts on coltan villages. Column (2) adds an interaction of the main explanatory variable (Coltan X Coltan Price) with a dummy indicating whether the village is above the median distance to the closest airport in the sample. As expected from prediction P', the effect of the coltan price on conquests is concentrated near airports. Column (3) controls for the presence of the Congolese army. One might indeed worry that since columns (1) and (2) use the full period, unobservables related to the timing of the coltan shock may be associated with armed groups' activity for reasons unrelated to the price shock. If, in the period following the Second Congo War, the Congolese army acts as a deterrent on armed groups especially in coltan villages, estimating the effect of the coltan shock considering all years may be biased. While the coefficient on the Congolese army dummy is significant, including it leaves the main result unaffected. The results are identical when instead of the presence of the Congolese army I use three alternative proxies for presence of the Congolese army, described in Section 1.5.1. Column (4) replicates the baseline specification, but includes controls for linear time trends common to coltan villages. To implement this, I allow time trends to differ the period prior to the shock and the perior after the shock. I therefore control for the coltan endowment dummy interacted with years, before and after the shock separately.

Columns (5)-(9) present the results of the baseline specification when the sample is restricted to the years of the Second Congo War (1998-2003). The Congolese army is indeed potentially a "bad control" (Angrist and Pischke, 2009). Restricting the sample to the years

of the Second Congo War allows me to eliminate omitted variables correlated with the change in environment associated with the post-war period. Comparing column (5) to the baseline specification in column (1), the main coefficient is larger in column (5). Column (6) includes the interaction with the distance to airports dummy. The coefficient on the main interaction (Coltan X Coltan Price) is larger, while its interaction with the distance to airports dummy is negative. This confirms that the effect of the coltan price is concentrated near airports. Column (7) controls for distance to the road, interacted with year dummies, to account for flexible time-varying effects of distance to the road. I do this because the *sequence* of conquests may be associated with geographical characteristics of coltan and gold villages unrelated to the coltan price (remotedness). The inclusion of this control leaves the coefficient on the coltan and coltan price interaction unaffected. To account for year region-specific shocks, column (8) includes region-year fixed effects. One could indeed worry that the best coltan villages are concentrated in one region, where most conquests take place in the year 2000 for reasons unrelated to the shock. Including region*year fixed effects leaves the main coefficient unaffected. Column (9) includes village linear trends as in column (4).

Columns (10) and (11) restrict the sample to 1999 and 2000. I exclude all other years to prevent the serial correlation across years to lead to under-estimation the standard errors. If we use information from many years around the coltan shock, we may indeed understate the standard errors on the coefficient estimates (Bertrand, Duflo, and Mullainathan, 2004). When I restrict the years to 1999-2000, the "treatment" spans only one period. I therefore do not need to cluster the standard errors at the village level. The coefficient is still significant, and as column (10) shows, the main effect is concentrated near airports. My baseline estimate suggests that the coltan shock increased the attempted conquest attacks by 20%.

Turning to prediction $P2$, an increase in the price of coltan should have a larger effect than an equivalent value per unit increase in the price of gold. The point estimates provide

support for this prediction across columns. The gold price is *negatively* related to conquest operations in gold villages in the whole period (columns 1-4). This negative relationship turns zero when I consider only the years of the Second Congo War (columns 4-8). It is also not robust to village linear time trends in the whole sample (yielding a p-value of 0.68), suggesting it is likely due to unobservables.[32] Confirming these results, a t-test of the null hypothesis that the coefficient on coltan is smaller or equal than the coefficient on gold in the baseline specification rejects the null hypothesis with a p-value of 0.03. However, while the discrepancy in coefficients is consistent with the theoretical results, it is no evidence that the underlying mechanism is the inability to tax gold output, since this result obtains even when gold output can be taxed (because of its smaller volume productivity). I then implement a test rescaling the estimated coefficients obtained from the baseline specification as derived in Appendix A.1.3, and find evidence that the impossibility to tax gold output alone can explain the magnitude of the coefficients. Indeed, I reject the null Hypothesis with p-value 0.00. In sum, the results on conquest attempts confirm predictions P1-P2.

I now turn to the results on pillages. Following predictions P1-P2, the price shock should not increase pillages, and if at all, it should decrease pillages by the strong armed group. Table 1.4 runs the analysis of Table 1.3, for pillages instead of conquests. Across all specifications, the estimates show that pillage operations are unaffected by the coltan shock. Consistent with the model, an increase in the price of coltan increases fighting for control of coltan villages, not pillaging.

Does this violence lead to new monopolies of violence? Figure 1.9 shows the proportion of villages in which a stationary bandit is collecting regular taxes in the mining sector. As a result of the coltan shock, the proportion of such villages increases from 0 to 50% in coltan

[32]While I report here results with linear trends during the Second Congo war, they are identical to the results with time trends in the whole sample and are hence unreported.

villages that did not have one in 1999, but only from 0 to 15% in gold villages that did not have one in 1999. Table 1.5 considers all villages, and runs the analysis of Table 1.3 focusing instead on whether a new stationary bandit acquires the monopoly of violence in the village. The "entry" variable I use here is thus the union of emergence of stationary bandits in areas where there was previously no stationary bandit, and the emergence of a new stationary bandit in areas where there was already one stationary bandit. The results, across specifications, are identical. Thus, the price shock effectively leads to the entry of new stationary bandits as a result of conquests.

Finally, Tables 1.6 and 1.7 present the same specifications for the complete set of outcome variables that characterize the transitions of individual villages in and out of stationary bandits occupation. I focus on the following variables: presence of a stationary bandit; transitions of villages from stationary bandit to no stationary bandit; presence of taxation by armed groups in the village; acquisition of firearms by armed groups (which captures investment in deterrence of invaders).[33] Since the coltan shock and the gold shock take place in different years, I separate the estimations for each shock. For the coltan shock, I focus on 1999 and 2000, which is the most conservative set of years. For the gold shock, I use the years 2001-2008. Since the rise in the price of gold begins in 2002, this allows me to exclude the contemporaneous effect of the coltan shock, as well as a major military intervention in 2009 and onwards by the Congolese Army (Kimia II).[34] The results are invariant to the choice of years. Across Tables 1.6 and 1.7, columns (1), (3), (5), (7), report the results considering the baseline specification, while columns (2), (4), (6), (8), include the interaction with the

[33]While investment in deterrence should unambiguously increase in the presence of strategic interactions, it may reflect an income effect. As the price rises, groups in coltan villages may have more means to acquire weaponse. For that reason, acquisition of firearms is not unambiguously indicating that the value of territorial control has increased.

[34]For a description of Kimia II, see Sánchez de la Sierra (2014a).

distance to airport dummy. The results are consistent with the interpretation of the findings on conquest attempts: the coltan price shock increases the value of acquiring monopolies of violence, especially near airports. When the village is empty, it is more likely that a stationary bandit emerges, while if the village already has a stationary bandit (which was the case for 55% of cotlan villages already by 1999), armed groups attempt a territorial conquest. These attempts are sometimes successful, in which case armed groups that dominate the local resistance acquire the monopoly of violence, collect taxes, consistent with historical accounts of state formation in medieval Europe. Table 1.7 shows that this effect is absent in gold villages. Figures 1.10 and 1.11 present a summary of results in tree form. They separate outcomes by whether the village already had a stationary bandit and otherwise.

I now consider the effects on taxation. In table 1.8, I use the baseline specification, but use as dependent variable a dummy for whether a stationary bandits raise a given type of taxes. I use dummies indicating the following tax instruments in columns (1)-(6) respectively: Output tax at Roadblocks (mining output tax, per kilogram), Entry tax (labor input tax levied to work in the mine, daily), Food sales tax (a daily tax levied on cassava producers in the local market), Poll tax (periodic tax per household), Transit tax (tax to enter and exit the village), Mill tax (tax on activity at the local mill, per day). Columns (1) and (2) shows that the coltan shock led to a sharp increase in mining taxes (both input and output). Column (4) shows that the coltan shock also drastically rose the probability that a stationary bandit collects poll taxes. In addition, the effect of the gold price leaves most taxes unaffected. It has a marginal positive effect on the probability that a stationary bandit collects poll taxes, a a negative effect on the probability that a stationary bandit collects taxes on transit. Turning to tax levels, Table 1.9 replicates Table 1.8 using instead the level of taxes in a tobit regression model. Columns (1) and (4) confirm the main finding: the coltan price shock attracted stationary bandits, who initiated tax collection on coltan

output and on households (poll taxes). This is reasonable, since households are richer in response to the coltan shock. I next consider alternative specifications.

1.6.2 Main predictions: results from alternative specifications

In addition, I implement three strategies to increase confidence in the main OLS estimates.

First, I implement conditional differences-in-differences matching, introduced by Heckman, Ichimura, and Todd (1998).[35] As matching variables, I use distance to airports and distance to roads, as well as upper level administrative divisions (Territoire). Since airport areas might be more urbanized, and urbanization might affect the behavior of armed groups, I also consider distance to roads. Finally, to control for constant unobserved heterogeneity across regions (Territoires), I also match on Territoire. This procedure is thus equivalent to conditional differences-in-differences within calipers defined by administrative divisions (Cochran and Rubin, 1973). Figure 1.12 shows the yearly coefficients estimated on the matched sample, based on the main outcome variables: conquest attempts, pillages, entry of new stationary bandits, and acquisition of firearms. The results are identical.

Second, I account for the observation that conquest attempts are rare events. In that case, OLS estimation could lead to biases in the estimated probabilities and standard errors (King and Zeng, 2001). I therefore run a logistic version of the baseline specification, that corrects for small sample and rare events in order to generate approximately unbiased and lower-variance estimates, introduced in King and Zeng (2001). The results are unchanged.

Third I account for the spatial problem in regressions that include the distance to airports. Neighboring villages have similar distances to the closest airport, and they are also

[35]This method is similar in spirit to case control methods, presented in Goldstone, Bates, Epstein, Gurr, Lustik, Marshall, Ulfelder, and Woodward (2010), where I select observations based on whether I observe coltan endowment and then select matches.

likely to be subject to common shocks stemming from their economic and social proximity. The problem is not limited to villages in same clusters. Villages close to an airport, *even if they are close to different airports*, may be subject to common shocks, if they are better integrated in the regional and world economy. This error group structure can lead to drastic under-estimation of standard errors in the OLS framework. One way to address this is to cluster the standard errors at the level of "Groupements"*years, which could account for a grouped error structure if errors are subject to common year shocks within "Groupements". As announced in Section 1.5.1, I implemented this for all regressions, and the results remain unchanged. However, the particular error structure is unknown, and therefore the correct clustering is also unknown. Alternatively, one could use Conley standard errors, which account for an error structure of AR(1) type (Conley, 1999). However, estimation of a spatial AR(1) structure on the errors will not capture arbitrary correlations. This problem is akin to imposing a serial AR(1) in the errors for panel regressions (Bertrand, Duflo, and Mullainathan, 2004). To exploit the spatial structure inherent in the data, I instead use randomization inference, which I take from the statistics literature. While rarely used in economics, it allows to exploit the structure of the data without making assumptions on the data generating process. I generate 5,000 times 40 imaginary airports' locations in the space around the survey villages. For each of these simulated airport locations, I run the OLS regression, where I interact the distance to airport dummy with the main regressor (Coltan X Coltan Price). I then estimate the coefficient on the triple interaction for each simulation, and plot the resulting distribution of coefficients in Figure 1.13. Since these airports are not real, and thus have no effect in expectation, the distribution of the coefficients is centered around 0. If a spatial structure leads to over-rejection, the distribution of coefficients estimated through simulations should have a relatively high variance. My coefficient estimated with real airports would lie within the 95% of the data around the mean. On the contrary, I

find that only 9% of the coefficients obtained through simulations are larger in absolute value than the coefficient estimated based on the real airports, a 0.09 p-value of a two sided test of statistical significance. The main result, therefore, remains when I estimate the standard errors using randomization inference.[36]

This section has shown that the coltan shock leads armed groups to acquire control of coltan villages near airports and collect taxes. The later shock to the price of gold, which can easily be hidden from taxes, does not. The next section examines the activity of armed groups in the village where they are stationary bandits.

1.6.3 Auxiliary predictions: results

Table 1.10 reports the results of the auxiliary predictions. I regress a dummy variable indicating whether a stationary bandit is present in the village on whether the village is endowed with minerals, the size of the village (number of households, in hundreds), number of immigrants in the past year (in hundreds of persons), number of emigrants in the past year, in hundreds of persons. In columns (1)-(3), I implement a simple OLS, and in Column (4) I add all regressors on the OLS. As shown in Columns (1) and (4) the presence of minerals in the village is a strong predictor of whether a stationary bandit is present. Stationary bandits are 15% more likely to be present in villages endowed with minerals.[37] There are on average 200 households per village in the sample. As shown in columns (2) and (4), the coefficient on current village size is not statistically significant. Its magnitude suggests, however, that an increase in the number of households by one standard deviation (170 households) increases

[36]I also implemented this procedure simulating allocations of mineral endowments to villages as well as price to years. Figure 1.13 shows that the estimated p-value with randomization inference is 0.065.

[37]The coefficient rises to 22% when I use instead as dependent variable stationary bandit who collects regular taxes.

the probability to have a stationary bandit by 4%, and has the expected sign. Columns (3) and (4) reveal that last year immigration also predicts the presence of a stationary bandit. Its coefficient suggests that an increase in past immigration of one standard deviation (63 persons) increases the probability to have a stationary bandit by 4 %.

1.7 Discussion

In this section, I demonstrate that the outcome variables compare surprisingly well with known historical events, as well as with external data sources. I then discuss how expectations might play a central role in the main estimates, and finally address how these results fit other explanations from the literature on civil war.

1.7.1 Measurement error

Classical measurement error in the outcome variable might lead to noisy estimates and can threaten identification of the parameters in the linear probability model (Hausman, 2001).[38] In addition, if the data is of low quality, arbitrary correlations stemming from systematic mistakes by enumerators might underlie arbitrary results unrelated with real effects. I first provide reassuring graphical evidence and then compare the data to the other sources.

Figure 1.4 in Section 1.2 plotted the survey-based measures of armed groups' occupation on years. Since historical trends are known, this allows to verify the quality of the main variables. The changes in the data correspond precisely to commonly known historical events. stationary bandits emerge and substitute for the state in villages in 1996 and 1997 with the AFDL rebellion, but particularly in 1998 with the RCD conquests. The data also coincides

[38]Classical measurement error in the explanatory variable leads to attenuation bias. However, to avoid this source of bias, the specifications do not use local prices of coltan.

with known dates of RCD occupation: it emerged in 1998 with the second Congo War and retreats in in 2003/2004 following the 2002 peace agreements of Sun City. The timing of the attacks also corresponds precisely to well known historical rebellions. Figure A.3 shows that the data maps precisely to the known phases of the war: the AFDL led its rebellion in 1996 into 1997, the RCD between 1998 and 2003, and the CNDP in 2004. Finally, survey-based recall prices closely track the international world price. Overall the data on prices, armed groups, and attacks benchmarks extremely well to well-known historical events.

To verify the data quality a step further I also assigned violent events geo-coded by an external dataset (ACLED) to circles around the survey villages. The ACLED dataset contains 3,500 violence events since 1997, coded by perpetrator and type of event. As opposed to the data collected in this survey, these data are based on reports. When an event falls in circles assigned to more than one village, I allocated the event to all village circles in which it fell. I then compare this data to my source that contains the number of attacks on villages.[39] Figure A.4 shows that the ACLED dataset systematically reports less violence data around the selected villages than what the current survey. While it is possible that this difference is due to villagers over-reports of violence events, under-reporting by villagers in the survey is more likely to be a concern, due to memory loss and fear of retaliation. If data from this survey is more likely to under-report violent events, this suggests that my survey perfoms well, possibly improving upon the ACLED dataset for the specific locations. To cross-validate this survey data econometrically, I correlated the data to ACLED's data in an OLS framework. Regression estimates of the correlation between ACLED and survey conquest attacks in the neighborhood of the village are positive, significant, but never larger than 10% across all distances (Table A.1). In addition, attacks reported by this survey are less likely

[39]This source contains larger numbers of violent events than the other survey source because villagers could not remember the details of some attacks.

to be reported in ACLED when the villages were under RCD occupation (suggesting that the source of under-reporting in ACLED could be obstruction of information), and when the attacks were pillages (ACLED may specifically under-estimate the number pillages).[40] The data collected in this survey also captures the well-known evolution in conflict intensity in the region. Figure A.4 shows the increase in violence during the Second Congo War, especially during the coltan shock, its decrease after 2003, and its drastic increase as a result of the Kimia II operations, which consists mostly of FDLR pillages.

1.7.2 Expectations

First, if villagers anticipated the coltan shock, estimates of the coltan shock may be a lower bound of the real effect. For instance, groups could have made the required investments during 1999 knowing that the prise would rise. Second, following the price increase, if local populations anticipated that the shock was going to be short-lived the estimates of the effect of the shock will underestimate real effects of permanent price shocks, especially if groups' cost functions have non-convexities. Third, following the sudden price drop in international markets, if populations expected that the price would rise back to its high level, the persistence of the coltan shock on institutions will be overestimated and will be led by wrong local expectations about future prices.

There is significant first-hand evidence on expectations about the coltan price level. This anecdotal evidence has three implications. First, surprise innovations on the electronics markets led to the rush to coltan. Second, the unexpected bad performance of Playstation II on the market in Christmas led the demand for coltan to vanish. It is therefore very unlikely that local populations expected a sudden drop in demand for this mineral of this magnitude.

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[40]This result applies specifically to RCD perpetrated violent events, consistent with reporting bias.

Third, following the sudden price drop in 2001, local traders expected the world demand to rise back to its peak level of 2000, and engaged in mineral hoarding. This is confirmed by the data on local prices.[41]

In order to strengthen the anecdotal evidence, the survey also collected retrospective measures of expectations. The sudden increase and decrease were both unexpected. However, there is variation in expectations about the duration of the shock. Figure 1.16 shows the emergence of new stationary bandits. The upper quadrants show the entry of new stationary bandits. I separate villages in which villagers declared that they had expected the shock to be permanent, and villages in which villagers declared they had expected the shock to be temporary.[42] The price shock had an almost three times larger effect (entry of 50% instead of 20%) amongst the villages that reported to expect that the price shock would be permanent. Finally, the lower quadrant replicates the analysis by expectations on the return to high prices. Of places where villagers expected a temporary decrease, stationary bandits vacated 25% in 2001. Of places where villagers expected no return to the high price, stationary bandits vacated progressively. This suggests that expectations may explain part of the persistence, and that had the villagers expected the prise rise to be temporary, the behavioral responses may have been dampened.

1.7.3 Alternative mechanisms

A large part of the conflict literature emphasizes the role of the trade-offs between production and expropriation, in particular the opportunity cost of fighting (Collier and Hoeffler,

[41] United Nations Security Council (2002).

[42] I have defined a temporary shock to be 5 years or less, which constitutes less than 20% of all villages. The rest report an expected duration of more years, or reported to expect the price to remain at high level for an indefinite amount of time.

2004, Dube and Vargas, 2013). My results suggest an additional constraint that determines the choices of armed groups: their ability to tax. The shock to the price of coltan implied a drastic increase in the economic opportunities from engaging in productive activities. Therefore, if the opportunity cost of fighting was the determinant of violence, there should have been a *decrease* in participation, and ultimately in violence, as found in Dube and Vargas (2013). On the contrary, I find an increase in violence, aimed at gaining territorial control. Hence, when the price of the labor intensive commodity increases, the relevant alternative for armed groups might not be to engage in productive activities in a labor intensive sector, but potentially to provide protection and extract revenue taxing output and labor. This channel is absent in the opportunity cost of fighting literature.

Second, my results complement theories of civil war that emphasize the role of the internal organization of the group (Weinstein, 2007). As the revenues from stable extraction increase, the stronger bandit acquires control of the valuable villages. In this case, a large part of the effect (not all) is driven by the RCD. It is possible that the RCD had a better disciplined internal organization that gave them dominance over uncoordinated Mayi-Mayi groups. However, if recruitment of soldiers' types is the only explanation for violence, it cannot explain the effects of the Kimia II. As the FDLR lost access to its mineral tax base, violence should have decreased if *bad* recruits had been replaced with *better* recruits. On the contrary, violence *increased*. The armed group just changed their strategy to acquire resources: they pillaged households' assets. This result thus complements Weinstein (2007). Once established, it is the fighters' relevant alternative revenue generating activity that determines the use of violence, which may not be production but alternative modes of expropriation.

Finally, this paper ignores strategic interactions. Future work will examine how behavior of armed groups is affected when other bandits can pillage production. If the stationary

bandit does not have a sufficiently strong protection technology, this mechanism can unravel uncoordinated roving banditry. However, this is also the case in the current decision theoretic framework. This framework captures well the dominance of the RCD against other weaker groups.

1.8 Conclusion

The origins of the state have attracted scholars of all social sciences. However, a fundamental fact is that prior to the state, there is no data collection capacity. The causes of state origins have thus weak bases in the existing literatures.

I provide the first econometric evidence on the *causes* of state formation. To observe state formation in its initial phases, I managed a team of data collectors in stateless and war-torn areas of Eastern Congo and collect a village-level panel dataset of yearly activity of armed groups. I develop a theory in which the ability to tax output plays a central role in the decision of armed organizations to establish or acquire an existing "monopoly of violence" in order to collect taxes.

Consistent with this theory, a drastic shock to the demand of coltan, a voluminous mineral easy to tax, leads armed groups to impose monopolies of violence in the proximity of coltan extraction and trade routes. A shock to gold in the immediate aftermath, does not. Since gold is easy to conceal, this reveals that taxation is a leading force in the decision to form states. As additional support to this theory, I find that armed groups, even when they are settled in gold villages, do not tax gold output.

This paper's results contribute to a large literature in economics (Acemoglu and Robinson, 2006, Acemoglu, Ticchi, and Vindigni, 2006, Bates, Greif, and Singh, 2002, Besley and Persson, 2009, Grossman, 1997, North and Weingast, 1989), but also to disciplines outside of

economics. The anthropological literature on early state formation suggests that economic returns to circumscribed locations played a central role in the formation of states (Carneiro, 1970). I find that the effect of mineral demand is particularly strong in locations where output can be traded. Furthermore, since price shocks have weaker effects on output that can be easily hidden by producers, I am able to provide disaggregated evidence on the role of the elasticity of the tax. This complements historical accounts of the role of population mobility and fixed investments on state formation (Herbst, 2011, Scott, 2009).

This paper has implications for policy. Armed groups are a topic of growing interest among Western governments because of the instability they generate. This study is the first to provide econometric evidence of the relationship between violence and mineral endowments in the DRC. While there are vivid debates and speculations around the motives of armed groups, I find that price shocks lead to armed groups' violence, between groups and in order to acquire territory hosting the mineral (not to more pillages). This result is in line with other reports based on qualitative data, such UN (2002).

Furthermore, understanding the origins of the state may help better understand the workings of governments. Strong states are an exception in the process of historical development (Bates, 2011). This paper demonstrates that states are more likely to emerge in locations where there is a surplus that can be expropriated. Policies that aim at providing military support to states who, conditional on current technology, have been created in locations where they struggle to collect taxes as a result of historical accidents, may be misguided.

My findings suggest a fundamental similarity between violent organizations and states. Like states, armed groups have a comparative advantage in violence, and they can exert coercion in order to expropriate, but also to maintain a monopoly of violence and provide protection. Their strategies might turn in favor of the population when the population can be a profitable tax base, which may in some cases lead to the formation states.

1.9 Tables

Table 1.1: Summary statistics

	No minerals	Coltan	Gold
	Mean	Mean	Mean
	(sd)	(sd)	(sd)
Number of external attacks at the mining center	.	0.64	0.02
	.	(2.21)	(0.21)
Number of internal attacks at the mining center	.	0.22	0.01
	.	(0.95)	(0.11)
Number of external attacks at village	0.18	0.13	0.22
	(0.38)	(0.34)	(0.42)
Number of internal attacks at village	0.02	0.04	0.03
	(0.14)	(0.20)	(0.17)
Armed organization stationed at the village	0.40	0.37	0.30
	(0.49)	(0.48)	(0.46)
Armed organization living inside the village	0.37	0.32	0.27
	(0.48)	(0.47)	(0.44)
Armed organization sharing power with the village authorities*	0.26	0.52	0.47
	(0.44)	(0.50)	(0.50)
Armed organization administering the village	0.07	0.19	0.07
	(0.25)	(0.40)	(0.25)
Size of the village (Number of households in 2012)	187.15	172.13	102.56
	(116.39)	(101.38)	(44.07)
Number of individuals immigrating per year	24.4	21.37	16.19
	(69.67)	(76.19)	(60.10)
Number of individuals emigrating per year	15.22	15.20	13.57
	(49.67)	(59.42)	(52.49)
Observations	460	368	207

Notes: Standard errors in parentheses. The sign * indicates that the variable was added after survey start. This table presents the mean and standard deviation of the main outcomes, by endowment of minerals at the village

Table 1.2: Balance on observables: gold and coltan villages

	Mean difference: Gold - Coltan (t-statistic)
Distance to closest Road, 2010	0.642 (1.06) 100
Distance to Natural reserve	-0.0254 (-0.01) 100
Distance to Lake	-14.38 (-1.29) 100
Distance to Bukavu	-7.357 (-0.87) 100
Distance to Rwanda	-10.94 (-1.07) 100
Distance to closest river	-0.343 (-0.32) 100
Distance to closest bridge, 2010	-8.048*** (-4.16) 100
Distance to closest Airport, 2010	-1.845 (-0.94) 100
Accessible by car in 1999	0.0587 (0.70) 87
Access by moto in 1999	0 (0.00) 87
Access to phone coverage in 1999	-0.0190 (-0.38) 87

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table presents the resulting t-statistics from a t-test of the difference in levels between gold and coltan villages. A positive t-statistic indicates a larger average value in gold villages.

Table 1.3: Effects of price shocks, conquests

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Coltan X Pc	0.25*	0.61***	0.26*	0.58**	0.30**	0.60**	0.59**	0.60**	0.46*	0.30**	0.66***
	(0.14)	(0.14)	(0.14)	(0.23)	(0.14)	(0.24)	(0.24)	(0.25)	(0.26)	(0.14)	(0.20)
Coltan X Pc X Far		-0.59***		-0.37***		-0.57*	-0.56*	-0.57*	-0.32*		-0.66**
		(0.19)		(0.14)		(0.29)	(0.29)	(0.29)	(0.17)		(0.27)
Gold X Pg	-0.01***	0.00	-0.01***		-0.02	-0.02	-0.02	-0.03			
	(0.00)	(0.01)	(0.00)		(0.02)	(0.03)	(0.03)	(0.04)			
Gold X Pg X Far		-0.01				0.01	0.02	0.02			
		(0.01)				(0.05)	(0.04)	(0.04)			
Congolese Army			-0.06**								
			(0.03)								
Constant	0.12***	-71.79	0.12***	50.59	0.15*	-0.05	-0.07	-0.07	-3.84	0.09***	0.09
	(0.03)	(51.69)	(0.03)	(54.78)	(0.09)	(0.10)	(0.09)	(0.10)	(63.34)	(0.02)	(0.20)
Observations	1,342	1,342	1,342	1,342	732	732	678	732	732	244	244
R-squared	0.21	0.33	0.22	0.20	0.34	0.35	0.29	0.37	0.34	0.68	0.69
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Village FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Village time trends	NO	NO	NO	YES	NO	NO	NO	NO	YES	NO	NO
Region*Year FE	NO	NO	NO	NO	NO	NO	NO	YES	NO	NO	NO
Distance to road	NO	NO	NO	NO	NO	NO	YES	NO	NO	NO	NO
Sample	98-09	98-09	98-09	98-09	98-03	98-03	98-03	98-03	98-03	99-00	99-00

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table presents the results on the linear probability model for *attempted conquests*. Standard errors are clustered at the level of the village. Pc is the US price of coltan, in US$ per kilogram, and Coltan indicates whether the village is endowed with coltan. Pg is the US price of gold, in US$ per kilogram, and Gold indicates whether the village is endowed with gold. Column (1) presents the baseline specification; column (2) adds the interaction of the main regressor with a dummy indicating whether the village distance to the closest airport is in above the median of the sample; column (3) adds a control for the Congolese army; column (4) controls for coltan time trends, allowed to differ in the pre-2000 and post-2000 period. Columns (5)-(9) restrict the observations to the years of the Second Congo War (1998-2003); column (5) presents the baseline specification; column (6) adds the distance to airport interaction; column (7) controls for the closest distance to the road interacted with year dummies; column (8) adds region*year fixed effects; column (9) controls for coltan time trends, allowed to differ in the pre-2000 and post-2000 period. Columns (9) and (10) restrict the years to 1999 and 2000; column (10) presents the baseline specification, and column (11) adds the distance to airport interaction. This table includes all villages in the sample. When I include only villages in which there was already a stationary bandit prior to the shock, the results are stronger and significant in all columns.

Table 1.4: Effects of price shocks, pillages

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Coltan X Pc	0.01	0.01	0.02	0.24	0.10	0.13	0.13	0.06	0.33	-0.67	0.11
	(0.15)	(0.22)	(0.15)	(0.28)	(0.16)	(0.24)	(0.24)	(0.24)	(0.34)	(0.93)	(0.27)
Coltan X Pc X Far		0.01		-0.03		-0.05	-0.05	0.05	-0.09		0.12
		(0.30)		(0.17)		(0.33)	(0.34)	(0.34)	(0.24)		(0.37)
Gold X Pg	-0.00	0.00	-0.00		-0.01	-0.00	-0.01	-0.01			
	(0.00)	(0.00)	(0.00)		(0.03)	(0.04)	(0.04)	(0.04)			
Gold X Pg X Far		-0.00				-0.02	-0.01	-0.01			
		(0.01)				(0.06)	(0.06)	(0.06)			
Congolese Army			-0.02								
			(0.04)								
Constant	0.13***	0.15***	0.14***	66.96	0.17	0.43***	0.37***	0.41***	2.37	0.10	0.18
	(0.03)	(0.05)	(0.03)	(66.82)	(0.11)	(0.12)	(0.12)	(0.13)	(79.91)	(0.17)	(0.27)
Observations	1,342	1,342	1,342	1,342	732	732	678	732	366	67	244
R-squared	0.14	0.15	0.14	0.15	0.26	0.27	0.27	0.29	0.37	0.82	0.55
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Village FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Village time trends	NO	NO	NO	YES	NO	NO	NO	NO	YES	NO	NO
Region*Year FE	NO	NO	NO	NO	NO	NO	NO	YES	NO	NO	NO
Distance to road	NO	NO	NO	NO	NO	NO	YES	NO	NO	NO	NO
Sample	98-09	98-09	98-09	98-09	98-03	98-03	98-03	98-03	98-03	99-00	99-00

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table presents the results on the linear probability model for *pillages*. Standard errors are clustered at the level of the village. Pc is the US price of coltan, in US$ per kilogram, and Coltan indicates whether the village is endowed with coltan. Pg is the US price of gold, in US$ per kilogram, and Gold indicates whether the village is endowed with gold. Column (1) presents the baseline specification; column (2) adds the interaction of the main regressor with a dummy indicating whether the village distance to the closest airport is in above the median of the sample; column (3) adds a control for the Congolese army; column (4) controls for coltan time trends, allowed to differ in the pre-2000 and post-2000 period. Columns (5)-(9) restrict the observations to the years of the Second Congo War (1998-2003); column (5) presents the baseline specification; column (6) adds the distance to airport interaction; column (7) controls for the closest distance to the road interacted with year dummies; column (8) adds region*year fixed effects; column (9) controls for coltan time trends, allowed to differ in the pre-2000 and post-2000 period. Columns (9) and (10) restrict the years to 1999 and 2000; column (10) presents the baseline specification, and column (11) adds the distance to airport interaction.This table includes all villages in the sample.

Table 1.5: Effects of price shocks, new stationary bandit

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Coltan X Pc	0.50***	0.84***	0.49***	0.70**	0.53***	0.84***	0.85***	0.82***	0.60*	0.51**	0.69**
	(0.17)	(0.26)	(0.17)	(0.33)	(0.19)	(0.29)	(0.28)	(0.31)	(0.36)	(0.21)	(0.31)
Coltan X Pc X Far		-0.63*		-0.63***		-0.58	-0.40	-0.57	-0.53**		-0.33
		(0.34)		(0.21)		(0.37)	(0.37)	(0.39)	(0.23)		(0.43)
Gold X Pg	-0.00	-0.00	0.00		-0.05*	-0.09**	-0.09**	-0.09**			
	(0.00)	(0.00)	(0.00)		(0.03)	(0.04)	(0.04)	(0.04)			
Gold X Pg X Far		0.00				0.06	0.10	0.07			
		(0.01)				(0.06)	(0.07)	(0.07)			
Congolese Army			0.19***								
			(0.03)								
Constant	0.32***	0.40***	0.31***	-74.52	0.52***	0.37***	0.38***	0.38**	159.34*	0.16***	0.23
	(0.04)	(0.07)	(0.04)	(79.40)	(0.12)	(0.14)	(0.14)	(0.15)	(86.95)	(0.04)	(0.32)
Observations	1,342	1,342	1,342	1,342	732	732	678	732	732	244	244
R-squared	0.15	0.16	0.17	0.16	0.21	0.22	0.25	0.24	0.22	0.52	0.52
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Village FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Village time trends	NO	NO	NO	YES	NO	NO	NO	NO	YES	NO	NO
Region*Year FE	NO	NO	NO	NO	NO	NO	NO	YES	NO	NO	NO
Distance to road	NO	NO	NO	NO	NO	NO	YES	NO	NO	NO	NO
Sample	98-09	98-09	98-09	98-09	98-03	98-03	98-03	98-03	98-03	99-00	99-00

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table presents the results on the linear probability model for *new stationary bandit*. Standard errors are clustered at the level of the village. Pc is the US price of coltan, in US$ per kilogram, and Coltan indicates whether the village is endowed with coltan. Pg is the US price of gold, in US$ per kilogram, and Gold indicates whether the village is endowed with gold. Column (1) presents the baseline specification; column (2) adds the interaction of the main regressor with a dummy indicating whether the village distance to the closest airport is in above the median of the sample; column (3) adds a control for the Congolese army; column (4) controls for coltan time trends, allowed to differ in the pre-2000 and post-2000 period. Columns (5)-(9) restrict the observations to the years of the Second Congo War (1998-2003); column (5) presents the baseline specification; column (6) adds the distance to airport interaction; column (7) controls for the closest distance to the road interacted with year dummies; column (8) adds region*year fixed effects; column (9) controls for coltan time trends, allowed to differ in the pre-2000 and post-2000 period. Columns (9) and (10) restrict the years to 1999 and 2000; column (10) presents the baseline specification, and column (11) adds the distance to airport interaction. This table includes all villages in the sample.

Table 1.6: Effects of the coltan shock, all outcomes

VARIABLES	(1) Stationary Bandit	(2) Stationary Bandit	(3) Village Vacated	(4) Village Vacated	(5) Mining Taxes	(6) Mining Taxes	(7) Firearms Acquisition	(8) Firearms Acquisition
Coltan X Pc	0.24*	0.54***	0.08*	0.00	0.47***	0.61***	0.23**	0.24*
	(0.14)	(0.20)	(0.05)	(0.07)	(0.13)	(0.19)	(0.10)	(0.14)
Coltan X Pc X Far		-0.58**		0.15		-0.28		-0.01
		(0.27)		(0.09)		(0.26)		(0.19)
Constant	0.53***	0.53***	-0.00	-0.00	0.33***	0.33***	0.02	0.02
	(0.03)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	234	234	244	244	234	234	234	234
R-squared	0.85	0.86	0.52	0.53	0.88	0.88	0.61	0.61
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Village FE	YES	YES	YES	YES	YES	YES	YES	YES
Sample	99-00	99-00	99-00	99-00	99-00	99-00	99-00	99-00

Notes: *** p<0.01, ** p<0.05, * p<0.1. Years are restricted to 1999 and 2000 (the year preceding the coltan shock and the year of the coltan shock). This table presents the results on the baseline specification for the following outcomes: presence of a stationary bandit, whether the village transitions from having a stationary bandit to not having one, whether an armed group is collecting taxes in the village, and a dummy variable indicating whether an armed group acquires weapons in this village. A stationary bandit is defined as an armed group that is settled in the village for at least 6 months and has the monopoly of violence in the village. Pc is the US price of coltan, in US$ per kilogram, and Coltan is a dummy indicating whether the village is endowed with coltan. Pg is the US price of gold, in US$ per kilogram, and Gold indicates whether the village is endowed with gold. Standard errors are clustered at the level of the village. Columns (1), (3), (5), (7), report the results considering the baseline specification, while columns (2), (4), (6), (8), add the interaction of the main regressor (Coltan X Coltan Price) with a dummy indicating whether the village distance to the closest airport is in above the median of the sample. This table includes all villages in the sample.

Table 1.7: Effects of the gold shock, all outcomes

VARIABLES	(1) Stationary Bandit	(2) Stationary Bandit	(3) Village Vacated	(4) Village Vacated	(5) Mining Taxation	(6) Mining Taxation	(7) Firearms Acquisition	(8) Firearms Acquisition
Gold X Pg	0.00	0.00	0.00	0.01	0.00	-0.00	0.00*	0.00
	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)
Gold X Pg X Far		-0.00		-0.00		0.01		0.00
		(0.01)		(0.00)		(0.01)		(0.00)
Constant	0.70***	0.86***	-0.01	-0.29***	0.67***	0.86***	0.01	-0.00
	(0.04)	(0.07)	(0.01)	(0.02)	(0.04)	(0.07)	(0.01)	(0.01)
Observations	936	935	976	975	936	935	936	935
R-squared	0.60	0.61	0.11	0.13	0.58	0.59	0.15	0.15
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Village FE	YES	YES	YES	YES	YES	YES	YES	YES
Sample	01-08	01-08	01-08	01-08	01-08	01-08	01-08	01-08

Notes: *** p<0.01, ** p<0.05, * p<0.1. Years are restricted to 2001-2008 (the years following the coltan shock until the Kimia II intervention). This table presents the results on the baseline specification for the following outcomes: presence of a stationary bandit, whether the village transitions from having a stationary bandit to not having one, whether an armed group is collecting taxes in the village, and a dummy variable indicating whether an armed group acquires weapons in this village. A stationary bandit is defined as an armed group that is settled in the village for at least 6 months and has the monopoly of violence in the village. Pc is the US price of coltan, in US$ per kilogram, and Coltan is a dummy indicating whether the village is endowed with coltan. Pg is the US price of gold, in US$ per kilogram, and Gold indicates whether the village is endowed with gold. Standard errors are clustered at the level of the village. Columns (1), (3), (5), (7), report the results considering the baseline specification, while columns (2), (4), (6), (8), add the interaction of the main regressor (Gold X Gold Price) with a dummy indicating whether the village distance to the closest airport is in above the median of the sample. This table includes all villages in the sample.

Table 1.8: Price shocks and taxes

VARIABLES	(1) Output tax roadblocks	(2) Entry tax	(3) Food sales tax	(4) Poll tax	(5) Transit tax	(6) Mill tax
Coltan X Pc	0.515***	0.393***	-0.008	0.337**	0.080	0.057
	(0.105)	(0.093)	(0.095)	(0.138)	(0.092)	(0.054)
Gold X Pg		0.001	0.001	0.006*	-0.006**	0.002
		(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Coltan	0.110**	-0.083	-0.038	0.068	-0.070	-0.010
	(0.043)	(0.069)	(0.057)	(0.076)	(0.069)	(0.035)
Constant	-0.026*	0.106**	0.118***	0.237***	0.189***	0.051**
	(0.015)	(0.052)	(0.039)	(0.050)	(0.047)	(0.023)
Observations	1,586	1,586	1,342	1,586	1,586	1,586
R-squared	0.132	0.017	0.014	0.052	0.026	0.014

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table presents the results on the baseline specification for dummies indicating whether armed groups collect a particular tax in the village as dependent variables. Standard errors are clustered at the village level. The dependent variable in column (1) is a dummy indicating whether an armed group collects a tax on output at roadblocks outside the mine. The dependent variable in column (2) is a dummy indicating whether an armed group collects a tax on labor at the mine (entry tax). The dependent variable in column (3) is a dummy indicating whether an armed group collects a tax cassava sales at the local market. The dependent variable in column (4) is a dummy indicating whether an armed group collects a tax on households (poll tax). The dependent variable in column (5) is a dummy indicating whether an armed group collects a tax on transit of persons in and out of the village. Finally, the dependent variable in column (6) is a dummy indicating whether an armed group collects a tax on a local mill in the village. Regressions include all years in the 1998-2012 interval.

Table 1.9: Price shocks and tax levels

VARIABLES	(1) Output tax roadblocks	(2) Entry tax	(3) Food sales tax	(4) Poll tax	(5) Transit tax	(6) Mill tax
Coltan X Pc	3.08***	2.29	0.01	7.50***	8.60**	5.03
	(0.75)	(2.56)	(0.20)	(2.06)	(3.81)	(5.99)
Gold X Pg	-0.14**	-0.02	0.00	0.06	0.10	0.06
	(0.06)	(0.10)	(0.01)	(0.08)	(0.17)	(0.27)
Constant	-1.64***	-2.14**	-0.37***	-0.29	-7.22***	-14.15***
	(0.45)	(0.92)	(0.08)	(0.60)	(1.64)	(0.53)
Observations	805	506	829	837	846	848

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table presents the results on the baseline tobit specification for left-truncated variables indicating the level of different taxes dependent variables. Standard errors are clustered at the village level. The dependent variable in column (1) is the tax on output at roadblocks outside the mine, in US$ per kilogram. The dependent variable in column (2) is the tax on labor at the mine (US$ per entry to the mine). The dependent variable in column (3) is the tax cassava sales at the local market, in US$ per day of sale. The dependent variable in column (4) is the tax on households (US$ per week). The dependent variable in column (5) the tax on transit of persons in and out of the village, in US$ per passage. Finally, the dependent variable in column (6) a tax on a local mill in the village, in US$ per day of activity. Regressions include all years in the 1998-2012 interval.

Table 1.10: Auxiliary predictions

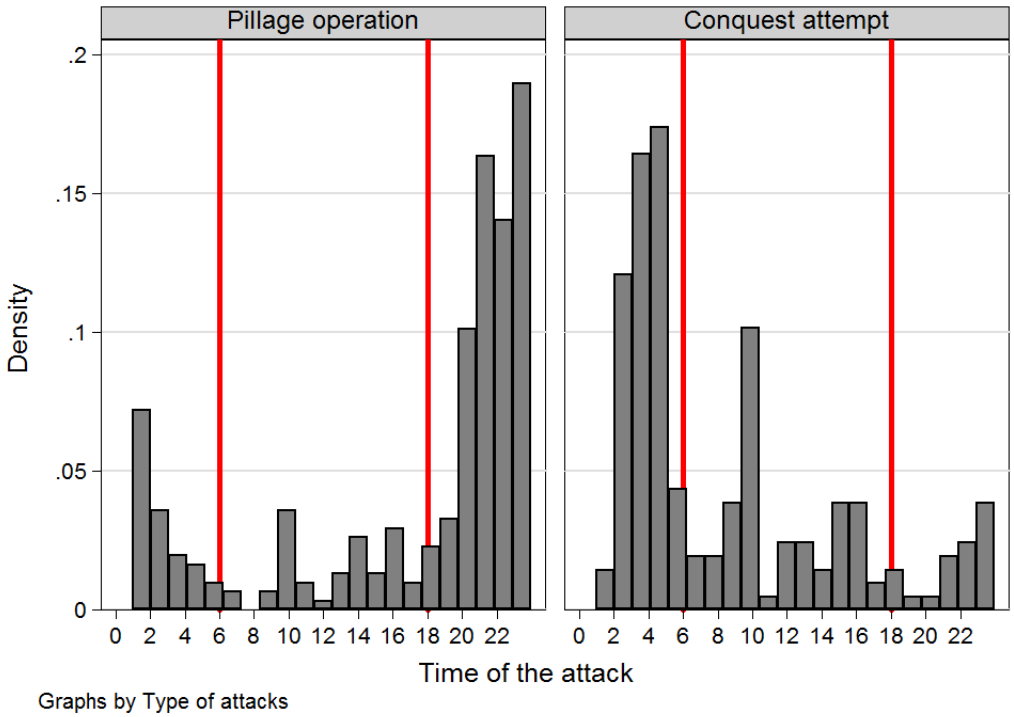
VARIABLES	(1) Stationary Bandit	(2) Stationary Bandit	(3) Stationary Bandit	(4) Stationary Bandit
Minerals in village	0.15***			0.15***
	(0.03)			(0.03)
Village Size		0.02		0.01
		(0.01)		(0.01)
Number of migrants (lag)			0.05***	0.04***
			(0.02)	(0.01)
Number of emigrants (lag)			0.00	-0.00
			(0.01)	(0.01)
Constant	0.05***	0.11***	0.13***	0.02
	(0.02)	(0.03)	(0.01)	(0.03)
Observations	2,760	2,714	2,760	2,714
R-squared	0.24	0.22	0.22	0.24
Cluster	Village	Village	Village	Village

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table presents the results on the auxiliary predictions. The dependent variable is a dummy indicating whether a stationary bandit is settled in village i at year t. Column (1) regresses stationary bandit on a dummy indicating whether the village is endowed with minerals. Column (2) regresses stationary bandit on the number of households in the village, recorded in 2012 (in hundreds). Column (3) regresses stationary bandit on the lagged number of immigrants and emigrants (in hundreds). Finally, Column (4) includes all regressors in a same specification. To account for autocorrelation in the dependent variable, all specifications include a lagged dependent variable. Results are unchanged when I drop it. Furthermore, I cluster the standard errors at the level of the village in each specification.

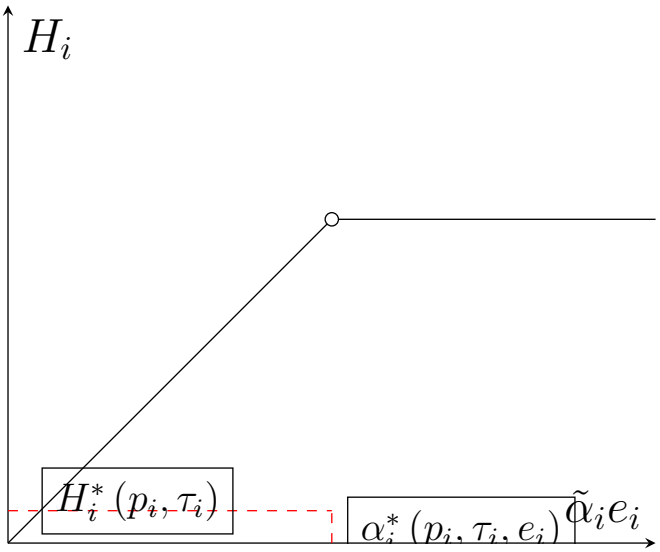
1.10 Figures

Figure 1.1: Timing of pillages and conquest attempts



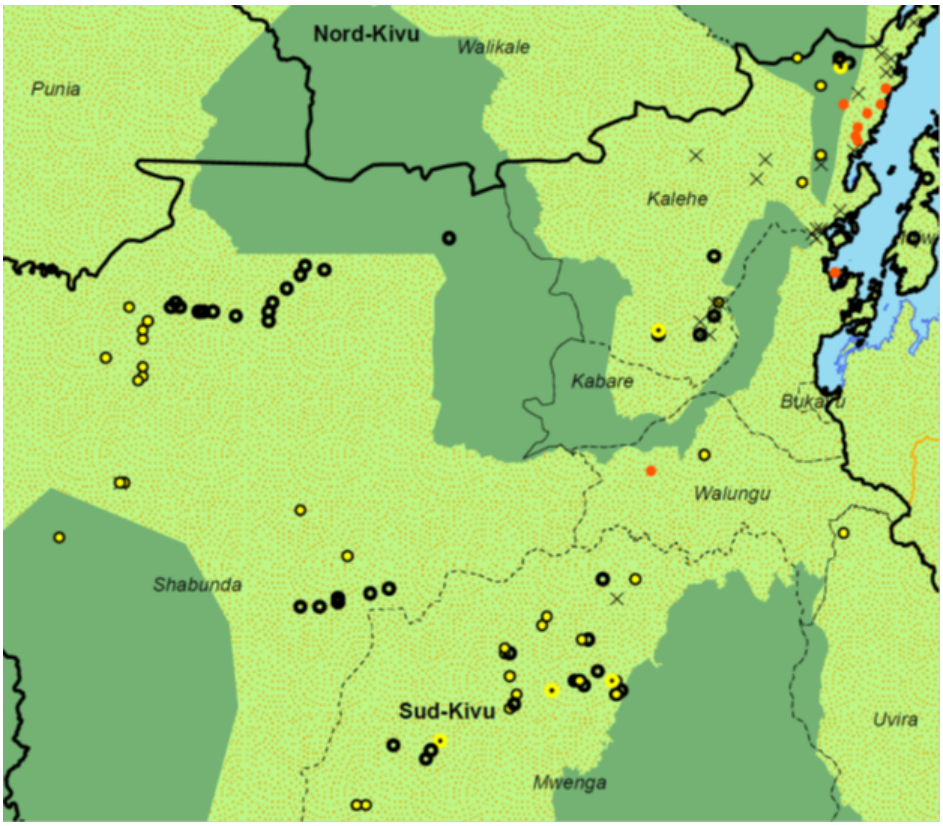
Notes: This figure draws on the attacks level dataset collected in this survey. In total, there are 600 attacks. The figure plots the distribution of the recorded *hour* of the attack. The horizontal axes indicate the hour of the attack in 24h format. The vertical axes indicate the mass of observations. The left quadrant plots the distribution for recored pillages, and the right quadrant plots the distribution for recorded conquest attempts. The red, vertical lines indicate the hours at which the sun rises (left line) and sets (right line). In the outer of these lines the village is in the dark.

Figure 1.2: Labor real productivity and volume hidden by households



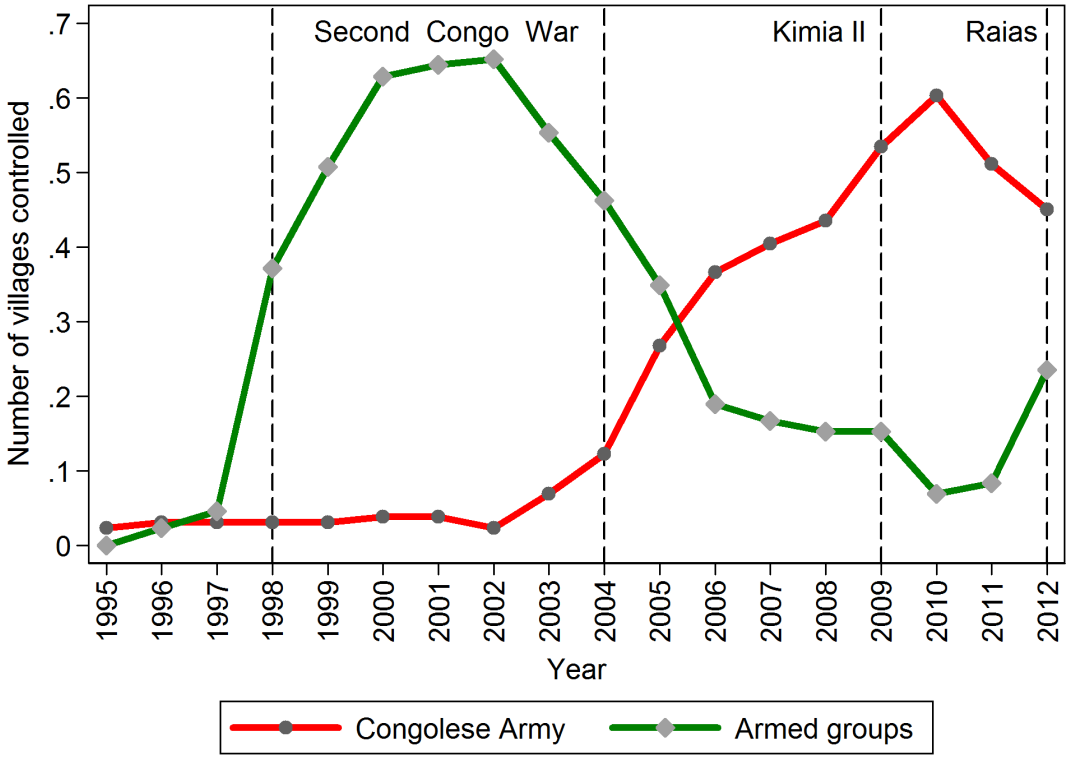
Notes: This figure represents the relationship between the volume of output hidden in sector i, H_i, and the realized output in sector i, $\tilde{\alpha}_i e_i$ derived from the household's first order conditions. If the realized labor volume productivity is below a certain threshold, the household is at a corner solution and hides all output. If the realized labor volume productivity is above the threshold, the volume hidden is an interior solution. Any additions to the realized volume productivity do not increase the volume hidden by the household.

Figure 1.3: Villages in sample



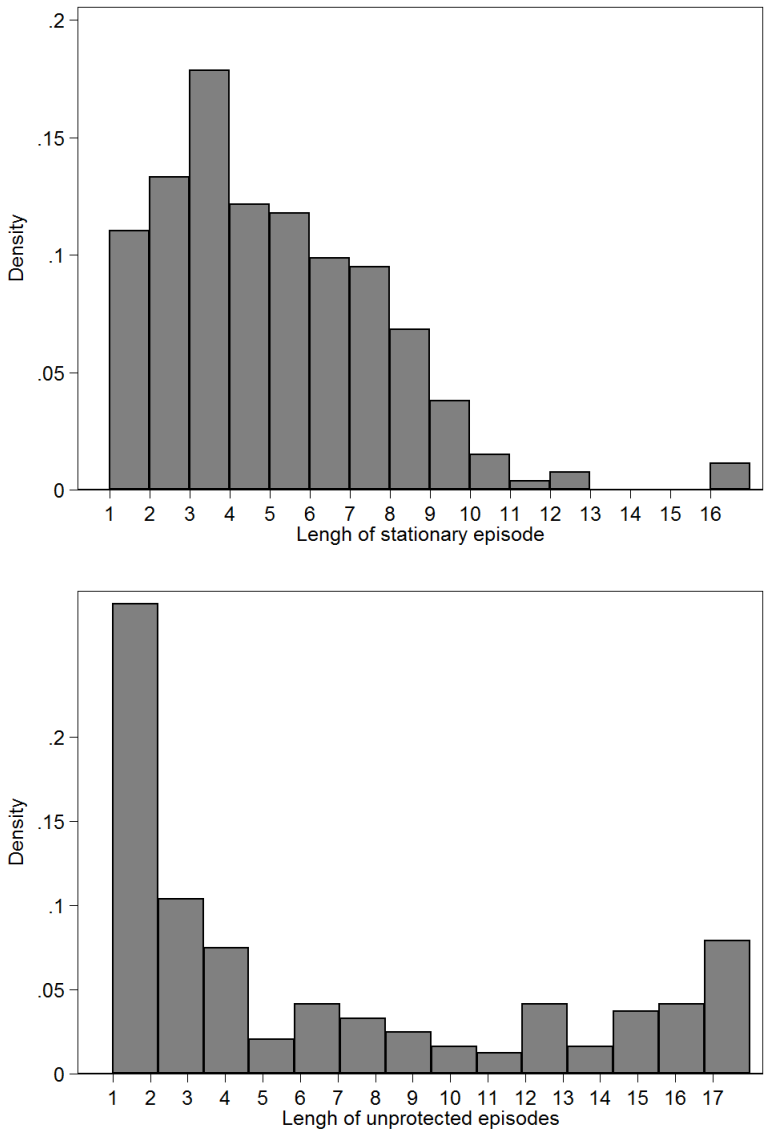
Notes: This map presents the location of the survey villages across Sud Kivu. Thick lines indicate frontiers between Provinces, dashed lines indicate fronteers between Territoires. Dark green indicates a natural reserve (forest). Blue indicates the lake Kivu. The villages endowed with coltan only are indicated in black circles. The villages endowed with coltan and gold are indicated as black dots with golden circles. Villages endowed with cassiterite only are indicated with orange dots, and agricultural villages are indicated as crosses. In each Territoire (Shabunda, Mwenga, Walungu, Kabare, Kalehe) the survey visited the population of coltan villages that were accessible to the survey teams. In addition, it visited a sample of gold villages randomly selected within the Territoire. In addition to this, in the Territoire of Kalehe, the survey visited all mining villages, as well as a matched sample of agricultural villages. To match agricultural villages to the population of mining villages in Kalehe prior to the survey, I matched multiple data sources, including SAESSCAM (Congolese institute of mining), the Institute of Peace (Belgium), and data I collected with my own qualitative fiedlwork in May-July 2012 with mining traders. After compiling the population of mining villages of Kalehe, I then used their geographic coordinates to assign to each a distance to: the lake, the natural reserve (forest), Bukavu, Goma, Rwanda, and the road. Since mining villages are likely to be more remote by the fact that they are more valuable and populations self select into locations, I matched the resulting population of mining villages on the population of villages based on the Mahalanobis metric on these variables. To obtain geo-locations of the population of villages prior to the survey, I used publicly available data from Référentiel Géographique Commun (2010), that I compiled with a dataset collected by the International Rescue Committee and Humphreys, Sánchez de la Sierra, and Van der Windt (2012, 2013a), who mapped all villages in most Territoires of Sud Kivu and collected geo-locations for each.

Figure 1.4: Group affiliation of the village monopoly of violence



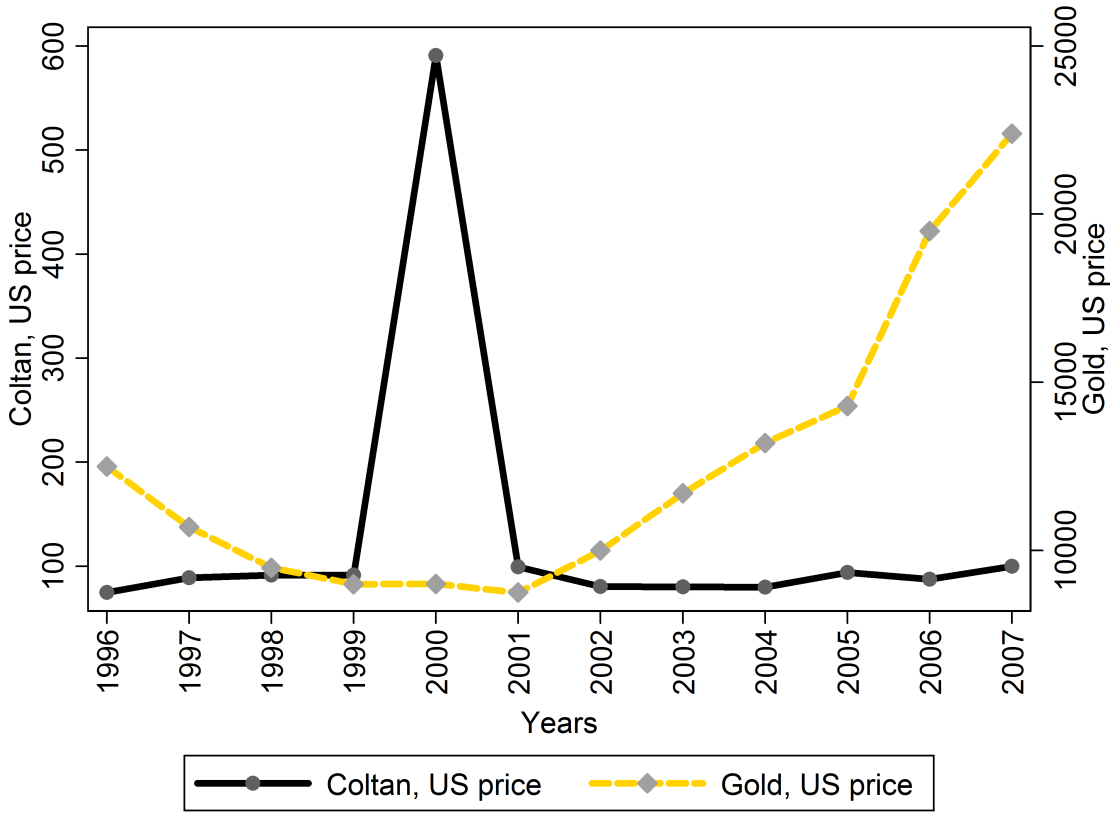
Notes: This figure graphs the share of villages in the sample under the control of a given armed group. The vertical axis represents the number of villages, and the horizontal axis represents the years. The green (light grey) line indicates the proportion of villages in the sample controlled by armed groups. The red (dark) line indicates the proportion of villages in the sample controlled by the Congolese army. The data fits well-known historical phases of the conflict and post-conflict periods. The dashed vertical lines indicate the start and end of the rebellion by the Rassemblement Congolais pour la Démocratie. The thick vertical line on the left indicates the timing of the Kimia II operations, and the thick vertical line on the right indicates the timing of the emergence of the Raia Mutombokis (Raias). The Congolese Army progressively replaces armed groups after 2004, the end of the second Congo War. However, local armed groups were integrated into the national army after 2003, without relocation. Armed groups structures of command and network remained intact. The distinction between the Congolese Army and armed groups is thus blurred.

Figure 1.5: Duration of stationary bandits



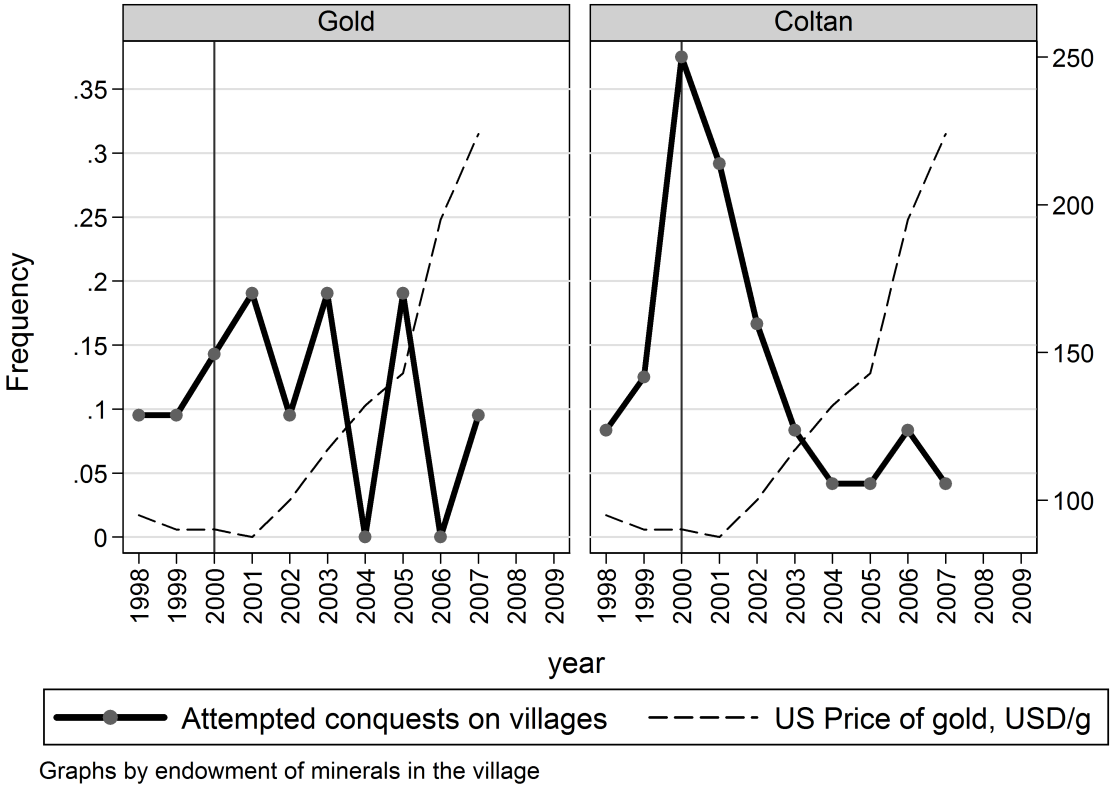
Notes: The upper figure plots the distribution of the duration for all episodes in the sample in which a village was under the control of a given stationary bandit. The lower figure plots the distribution of the duration for all episodes in the sample in which a village was under the control of *no* stationary bandit. For both figures, the vertical axes represent the density and the horizontal axes indicate the number of years.

Figure 1.6: World prices of coltan and gold, by year



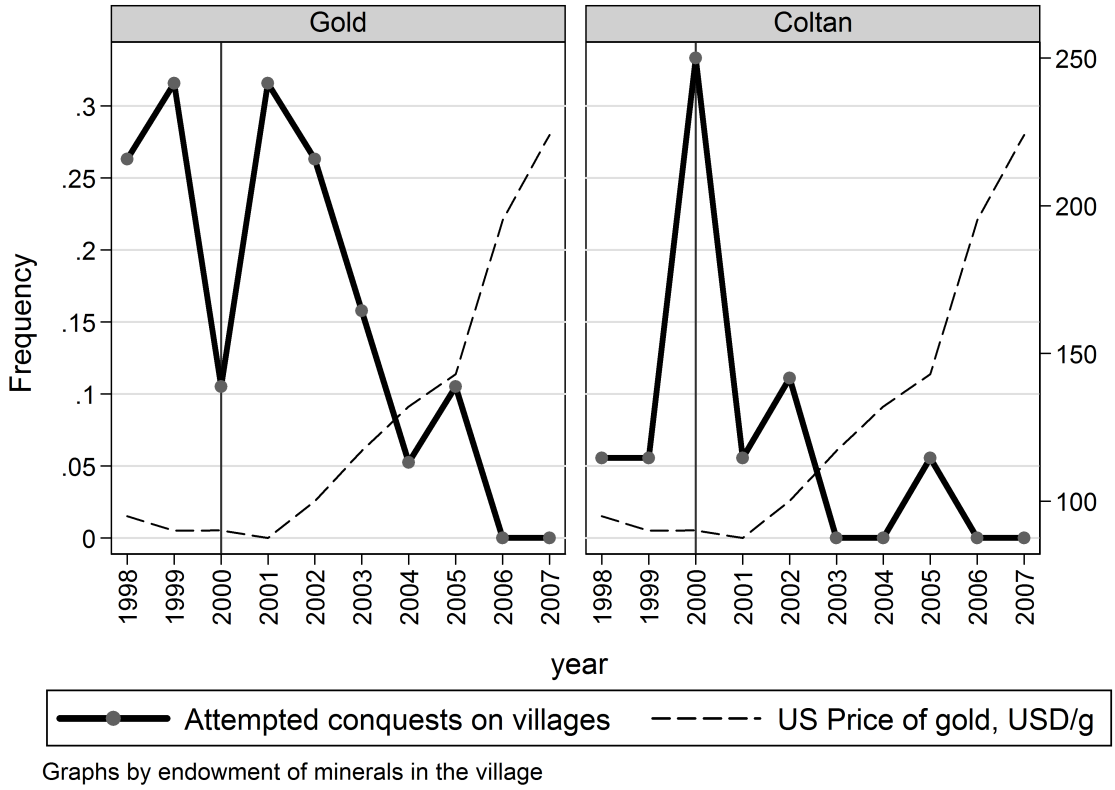
Notes: This figure plots the yearly average price of gold and coltan in the US market, in USD per kilogram. The left vertical axis indicates the price of coltan and the right vertical axis indicates the price of gold. The horizontal axis indicates the years. Source: United States Geological Survey (2010).

Figure 1.7: Main predictions: attacks on mining camps



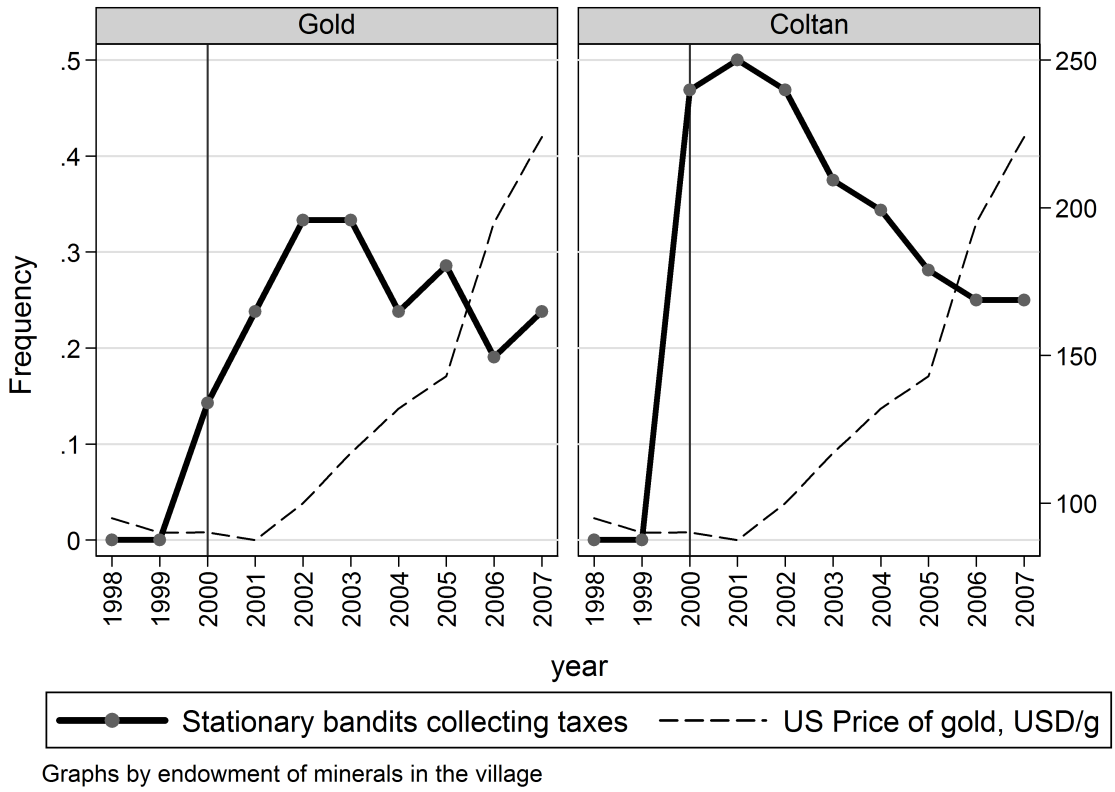
Notes: This figure plots the proportion of mines that have been attacked, by year. The left quadrant plots this relationship for gold mines, and the right quadrant for coltan mines. The thick line indicates the proportion of villages in which an attack took place. The right vertical axis indicates the local price of gold in USD per gram. The dashed line represents the price of gold and the vertical line indicates the timing of the coltan shock. I include only mines in which there was a monopoly of violence established in 1999, collecting regular taxes, since occupation of mines in which there is no monopoly of violence occurs peacefully.

Figure 1.8: Main predictions: conquest attempts on villages



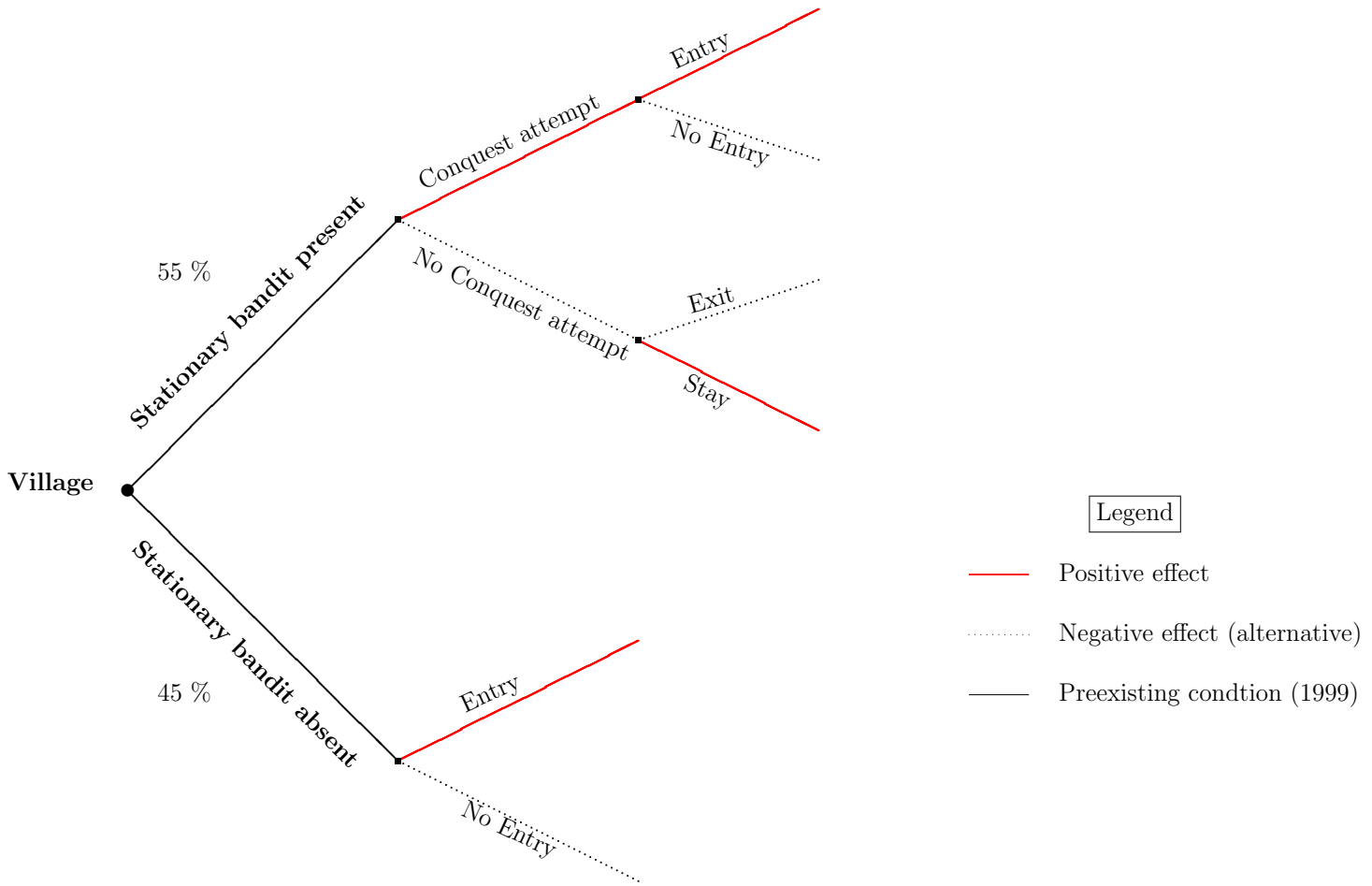
Notes: This figure plots the proportion of villages that experienced a conquest attempt, by year. The left quadrant plots this relationship for gold villages, and the right quadrant for coltan villages. The thick line indicates the proportion of villages in which a conquest attack took place. The right vertical axis indicates the local price of gold in USD per gram. The dashed line represents the price of gold and the vertical line indicates the timing of the coltan shock. I include only villages in which there was a monopoly of violence established in 1999, collecting regular taxes in the mining sector, since occupation of villages in which there is no monopoly of violence occurs peacefully.

Figure 1.9: Main predictions: village stationary bandits



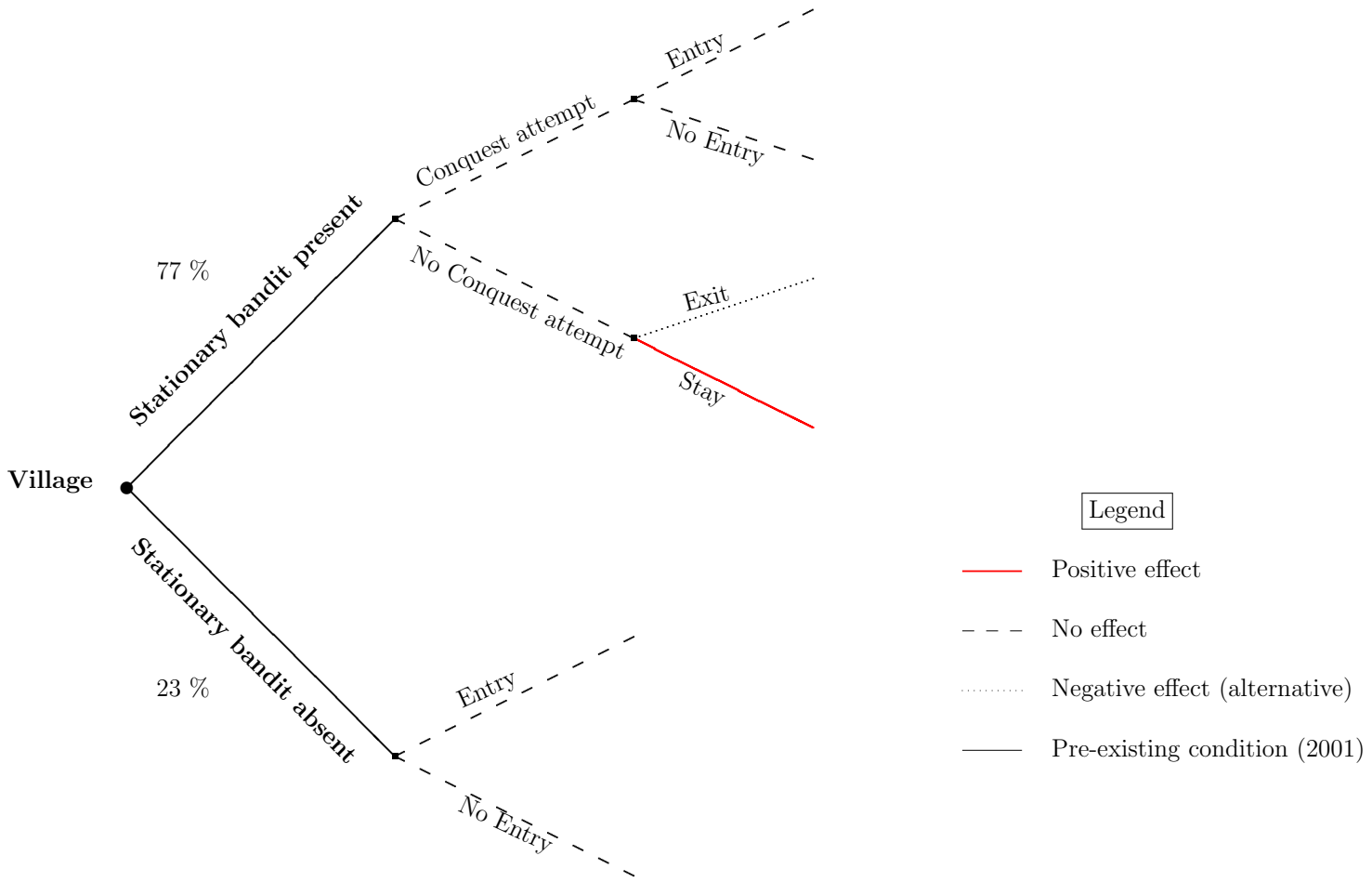
Notes: This figure plots the proportion of villages that have a stationary bandit, by year. The left quadrant plots this relationship for gold villages, and the right quadrant for coltan villages. The thick line indicates the proportion of villages in which a there is a stationary bandit. The right vertical axis indicates the local price of gold in USD per gram. The dashed line represents the price of gold and the vertical line indicates the timing of the coltan shock. I include only villages in which there was not a monopoly of violence established in 1999, collecting regular taxes in the mining sector, since the creation of village monopolies of violence can only take place in those villages that did not have one before.

Figure 1.10: Main predictions: summary of results of the coltan shock



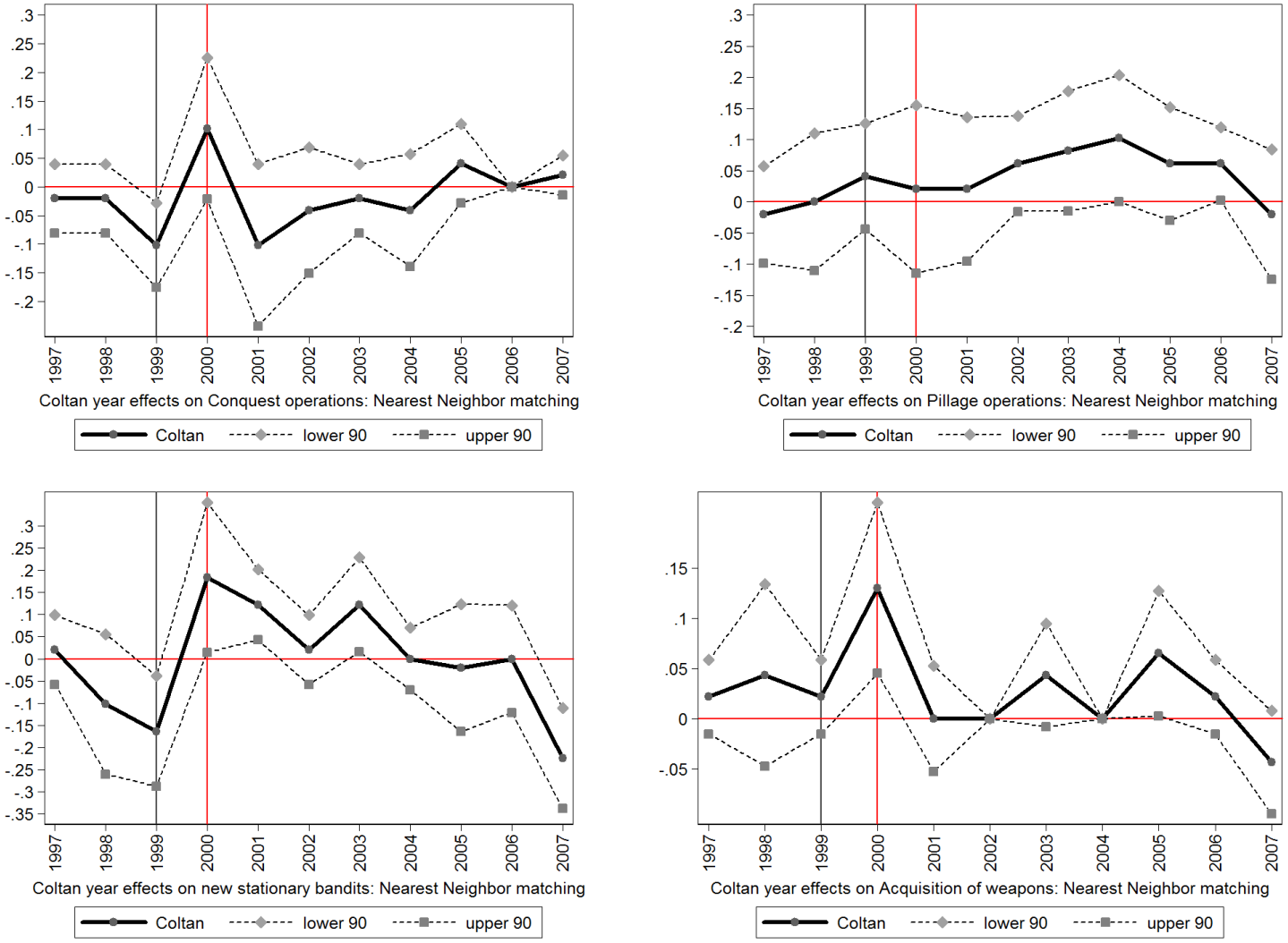
Notes: This graph summarizes the results on the coltan price shock (Prediction P1). At the time of the coltan shock, villages were either occupied by an armed group (stationary bandit present) or not (Stationary bandit absent). For an occupied village, armed groups may attempt a conquest ("Conquest attempt") or not ("No conquest attempt"). The attempt may be successful, in which case the armed group emerges as the new stationary bandit ("Entry") or it may fail ("No Entry"). If there is no conquest attempt, the stationary bandit present at the village may also decide to leave ("Exit") or remain as stationary bandit ("Stay"). If no stationary bandit is present, an armed group may emerge as the village stationary bandit ("Entry") or the village may remain unoccupied ("No Entry").

Figure 1.11: Main predictions: summary of results of the gold shock



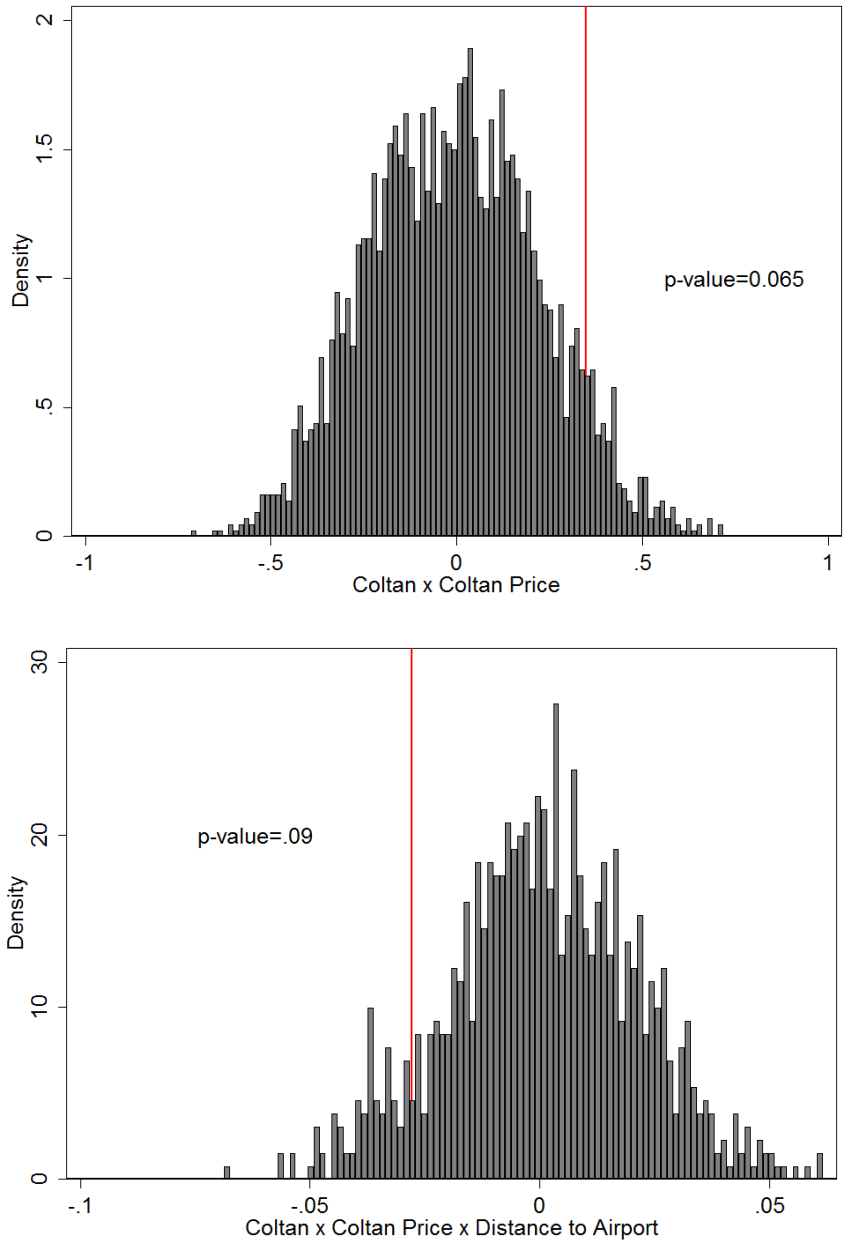
Notes: This graph summarizes the results on the gold price shock (Prediction P2). At the time of the gold shock, villages were either occupied by an armed group (stationary bandit present) or not (Stationary bandit absent). For an occupied village, armed groups may attempt a conquest ("Conquest attempt") or not ("No conquest attempt"). The attempt may be successful, in which case the armed group emerges as the new stationary bandit ("Entry") or it may fail ("No Entry"). If there is no conquest attempt, the stationary bandit present at the village may also decide to leave ("Exit") or remain as stationary bandit ("Stay"). If no stationary bandit is present, an armed group may emerge as the village stationary bandit ("Entry") or the village may remain unoccupied ("No Entry").

Figure 1.12: Main predictions: matching



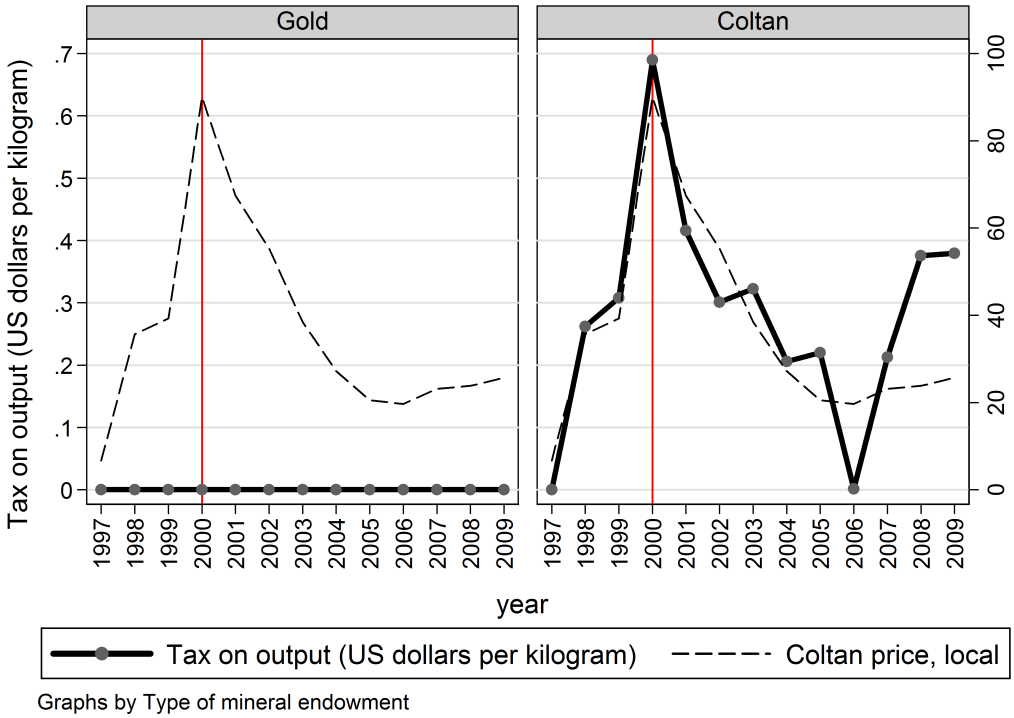
Notes: These figures present the results from matching. I matched coltan and non-coltan villages on distances to airports and roads, and on administrative divisions. I regress pair-level difference in outcomes on year dummies. The graphs present the coefficient on the year dummies, and their 90% confidence intervals. The black line shows the yearly coltan coefficients. Dotted lines represent the confidence intervals. Standard errors are clustered at the pair level.

Figure 1.13: Randomization Inference results



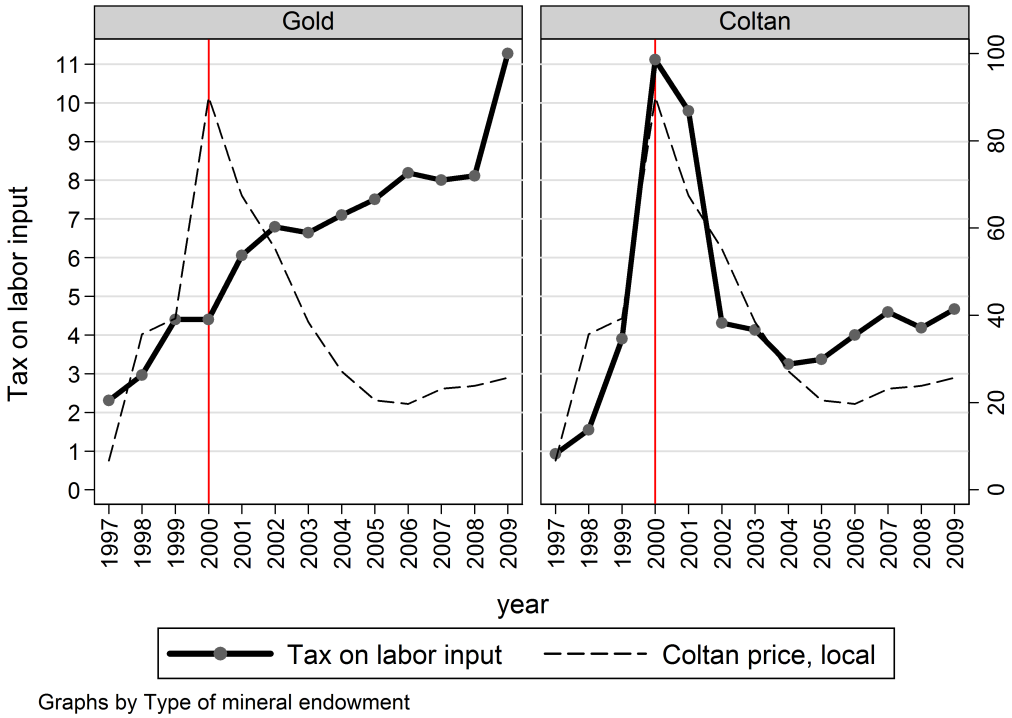
Notes: These figures present the results from the randomization inference exercise. The upper figure plots the distribution of coefficients on the main interaction Coltan X Coltan Price obtained by randomly assigning prices of coltan to different years. The vertical line indicates the value of the coefficient estimated using real prices. The resulting p-value is 0.065. The bottom figure plots the distribution of coefficients on the variable Coltan X Coltan Price X d, where d is a dummy indicating whether the village distance to the closest airport is above the median distance to the closest airport in the sample villages. The resulting p-value is 0.09.

Figure 1.14: Average tax on output, by year and mineral endowment



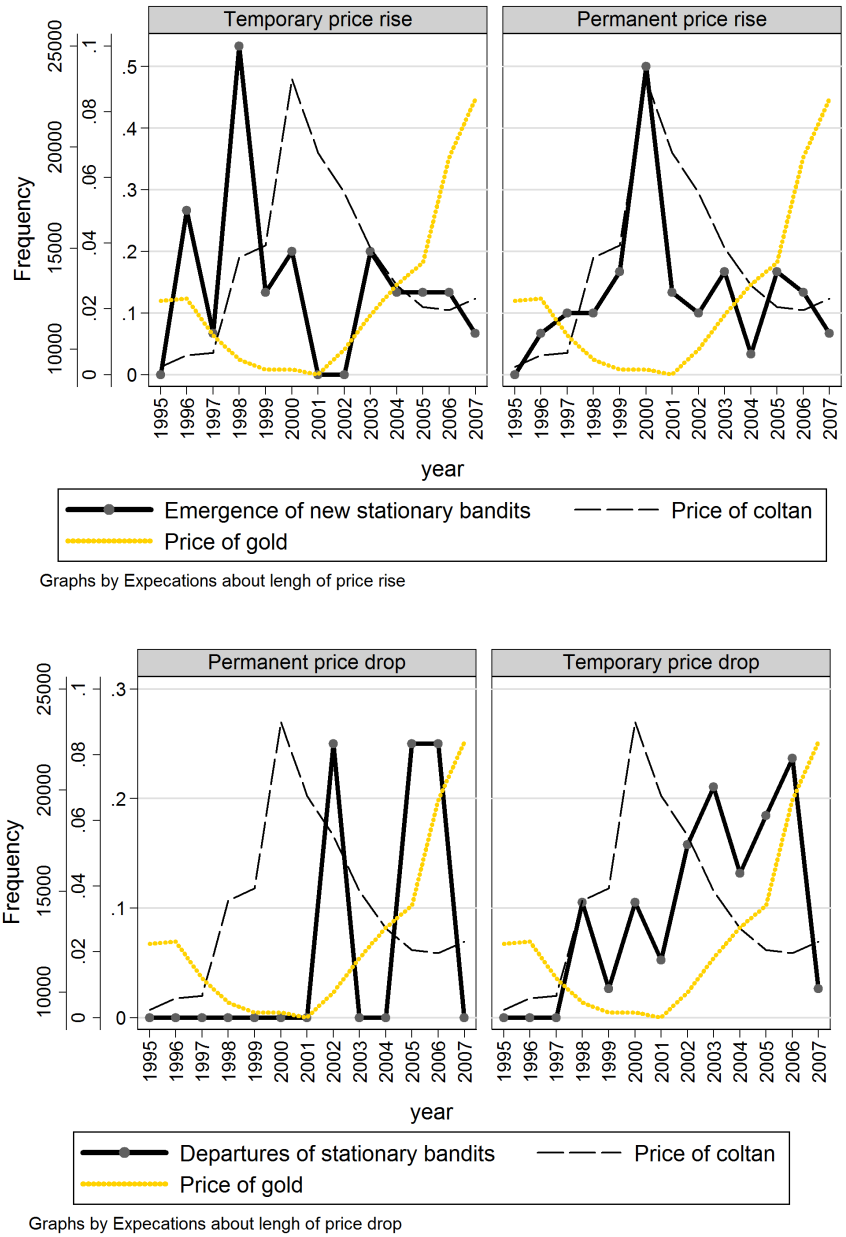
Notes: This figure plots the average output tax level on the mineral, collected at the roadblock at mine exit, per year. The left quadrant plots this relationship for gold villages, and the right quadrant plots this relationship for coltan villages. The solid line indicates the specific tax rate on output. The tax rate is converted to USD per kilogram and its axis is reported on the left vertical axis. The local price of coltan, represented as a dashed line, is expressed in USD per kilogram, and reported on the right vertical axis. Output taxes are collected based on the fee that miners have to pay for each bag of coltan.

Figure 1.15: Average tax on daily mining labor, by year and mineral endowment



Notes: This figure plots the average level for the tax on labor (fee to enter the mine), collected at the entry at mine, per year. The left quadrant plots this relationship for gold villages, and the right quadrant plots this relationship for coltan villages. The solid line indicates the tax level on mining labor, captured at the entry of the mine. It is the fee paid for one day of work. The tax on labor is in USD per day and the values are reported on the left vertical axis. The local price of coltan, represented as a dashed line, is expressed in USD per kilogram, and its values are reported on the right vertical axis.

Figure 1.16: Expectations of price shocks and stationary bandits



Notes: The horizontal axis indicates the year and the vertical axis on the right indicates the proportion of villages in which an event takes place. The vertical axis in the middle indicates the price of coltan per gram, in US$. The left vertical axis indicates the gold price per kilogram, in US$. The dashed line represents the local price of coltan, the dotted yellow line indicates the world price of gold. In the two figures above, the black, solid, line is the entry of a new stationary bandit. The left quadrant uses villages in which the price rise was expected to be temporary, permanent on the right quadrant. In the lower figures, the black, solid, line is the *departure* of a stationary bandit from the village. The left quadrant uses villages in which the price drop was expected to be permanent and temporary in the right quadrant.

Chapter 2

When do Monopolies of Violence act like States? Evidence from Stationary Bandits

Raúl Sánchez de la Sierra

"The study of the economics of crime [...] promises to offer profound insights into the origins and workings of governments, not as most of us know them, but like those that have ruled the bulk of humanity in the past, and continue their sway in many countries today."
William Baumol, 1995

2.1 Introduction

Is a "monopoly of violence" a satisfactory view of the state? Historical accounts of state formation argue that its origins are coercive and define the state as "monopolies of violence". The dominant view is that Lords and bandits with a "monopoly of violence" in their locality protected economic activity and developed fiscal capacity to finance wars of defense (Tilly, 1985), which evolved into the states of the current era.[1] Supporting the view that the origins of states lie in organized crime, Olson (1993) developed a criminal metaphor of states as "stationary bandits". Stationary bandits who control the "monopoly of violence" are residual claimants on economic activity, and thus develop an "encompassing interest" in growth. In contrast, "roving bandits", lacking a "monopoly of violence", engage in uncoordinated expropriations and discourage economic activity. However, statistics emerge only with well functionning states. This absence of data on preliminary states has shaped our conceptualization of the state, and biased it towards governments as most of us know them: stable, bureaucratic, capable of taxing income, subject to the rule of law and enjoying (often) legitimacy. But since there are no statistics before states as we know them today, there is no statistical evidence of whether and when monopolies of violence develop state-like behavior.

[1] "The trimmed-down argument stresses the interdependence of war making and state making and the analogy between both of those processes and what, when less successful and smaller in scale, we call organized crime. War makes states, I shall claim." (Tilly, 1985)

In this paper, I confront the view of the state as a "monopoly of violence" to statistical evidence. I provide unique evidence linking criminal organizations holding local "monopolies of violence" to state-like behavior, and the conditions on which this relationship hinges. I start by developing a theory that integrates state-like behavior by armed groups with the theory of optimal taxation. The key insight of the model is that armed groups holding a monopoly of violence should expropriate only partially through stable taxation, and promote economic activity. This behavior, however, should unravel when their monopoly comes under threat and their horizon shrinks. I test this theory using a novel dataset on armed groups in 140 villages of East Democratic Republic of Congo, where the Congo state is absent. I find that armed groups that are settled as "stationary bandits" consistently provide public goods. They develop tax plans consistent with optimal taxation as well as with historical accounts of taxation by medieval states with rudimentary fiscal capacity, which were unable to observe income directly. Exploiting the timing of a peace agreement, I find that their activity has positive impact on economic activity. Finally, I use the timing of a military threat to show that stationary bandits who relied on stable taxation and protection turn instead into violent arbitrary expropriations when their monopoly of violence comes under threat.

As a foundation for this paper, I managed a team of data collectors in stateless and war-torn areas of Eastern Democratic Republic of Congo in order to collect a village-level yearly panel dataset of activity of armed groups. The data allows me to identify types of violence, armed groups' monopolies of violence at the village level, types of taxes levied by armed groups, and various types of public goods provision by armed groups since 1995.

Eastern Congo is a well-suited environment to examine states at their early stages. The state is considered a "failed state". As a result, armed groups have proliferated in the East, where they implement violent robberies but also control individual villages, removed from

the state.[2] They fight to control territory, in which they administer "monopolies of violence", collect taxes, provide public goods, and even enjoy popular support, as in historical accounts of European state formation.

In the first result of the paper, I examine the patterns of taxation, and find that armed groups' taxes are consistent with optimal taxation theory, and with the historical accounts of taxation strategies of medieval European states with low fiscal capacity. They tax observable mineral output, households, population transit, observable market activity, labor, and develop elaborate tax plans at production sites that reflect optimal taxation. Furthermore, they consistently provide public goods, such as protection, justice, and village administration, and they enjoy high levels of popular support.

If armed groups holding a monopoly of violence are residual claimants of economic activity through taxation, they should promote economic activity in order to tax it. In the second result of the paper, I examine the impact of armed groups' control on economic activity. I exploit the heterogeneous effects of price shocks, as well as the timing and targeting of a national peace agreement to establish that settled armed groups may have a positive impact on growth. In 2003 the Rassemblement Congolais pour la Démocratie (RCD) agreed to vacate the villages under its control, leaving these villages unprotected. I find that the withdrawal of the RCD led to a decrease in economic activity in the villages it formerly controlled, compared to similar villages. This suggests that armed groups holding a monopoly of violence can have a positive effect on output in a context where their alternative is close to anarchical.

However, armed groups' "state-like" behavior depends on their ability to defend their monopoly of violence. In the third result of the paper, I exploit the timing, and the spatial

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[2]See United Nations Security Council (2002), Nest, Grignon, and Kisangani (2011), Stearns (2011) and Verweijen (2013). See also the RRMP program evaluation reports http://www.rrmp.org.

targeting, of a large military intervention against an armed group. In 2009, the Congolese Army led a large-scale operation aimed at eradicating the Forces de Libération du Rwanda (FDLR), which drastically reduced their time-horizon. Using this shock, I establish that in response to the intervention, the FDLR abandoned partial expropriation through regular taxation and increased arbitrary and violent expropriations in the villages they controlled (pillages). This result confirms that a "monopoly of violence" is not a satisfactory view of the state. The stability of the monopoly of violence is a necessary condition to observe state-like behavior.

This paper's findings have important implications for existing scholarship in economics and other disciplines.[3] The state at its origins has been studied across disciplines. Voluntaristic theories argue that states emerged as a result of a mutually beneficial social contract (Hobbes, 1651, Rousseau, 1762). On the other hand, conflict theories of the state, which are the dominant view, trace the origins of states to the motives of organized crime of maximizing their revenues from expropriation of the private sector. They suggest that states arise from violent organizations able to secure monopolies of violence (Carneiro, 1970, Tilly, 1985). Olson (1993) endowed this work with theoretical foundations, justifying why organized crime should develop state-like behavior. This paper also expands a large body of work in economics which rationalizes the behavior of states (Acemoglu and Robinson, 2006, Acemoglu, Ticchi, and Vindigni, 2006, Bates, Greif, and Singh, 2002, Besley and Persson, 2009, Grossman, 1997, North and Weingast, 1989). Despite having been studied across all social sciences, nascent states and their links with organized crime are unexplored with formal statistical methods, since the formation of states precedes the creation of statistics. As Tilly (1985), I identify organized criminals that use coercion to maintain states and develop fiscal

[3]The literature on state formation is separated between early and modern state formation. The literature on early state formation is reviewed in Claessen and Van de Velde (1991) and Claessen and Skalnik (2011).

capacity in order to extract surplus. As Olson (1993) and McGuire and Olson (1996), I use optimal taxation to explain *why* organizations of violence holding a "monopoly of violence" would exhibit state-like behavior. I then challenge this view, and question under what conditions the "monopoly of violence" can be a sufficient characterization of the state. However, I go a step further: I am able to test these predictions and their refinements on disaggregated data about criminal "monopolies of violence". Acemoglu, Robinson, and Santos (2013) use village level data to show that Colombia does not hold a monopoly of violence, and instead relies on alliances with paramilitaries to maintain its sovereignty. In so doing, they demonstrate that a monopoly of violence is not a necessary feature of a state that is recognized by the international community. In this paper, I do the reverse exercise: I provide evidence suggesting that a monopoly of violence, if uncontested, can be a sufficient characterization of the state.

This paper also contributes to the civil-war literature. Blattman and Miguel (2010) conclude that the study of civil war is limited by the absence of high quality disaggregated data. The data I was able to collect contributes to this literature. Furthermore, I introduce taxation to the set of armed groups' choices that have been considered to date in this literature. By showing that the ability of armed groups to sustain their monopoly of violence reduces violence against civilians, this paper has implications for explaining violence as well as the *types* of violence.

While the political economy of institutions literature takes the "monopoly of violence" as given, the civil war literature explains the motives that lead individuals to use violence - assumed to be for a detrimental purpose (Collier and Hoeffler, 2004, Humphreys and Weinstein, 2006, Weinstein, 2007). In this paper, I link these two strands of literature by showing that actors that have access to violent means may reduce levels of expropriation and promote economic activity in a particular location if they hold a sustainable "monopoly of violence",

leading to state-like behavior.

The remainder of the paper is organized as follows. Section 2.2 presents the setting and Section 2.3 presents the theory. Section 2.4 describes the data and data collection strategy. Sections 2.5 to 2.7 present the three results. Section 2.5 describes the patterns of taxation and public goods provision of armed groups that control local monopolies of violence. Section 2.6 presents the results on the economic impacts of armed groups. Section 2.7 presents the results on the time horizon. Section 2.8 provides a brief discussion and Section 2.9 concludes.

2.2 Background

Benefiting from the collapse of the central state, armed groups in the Democratic Republic of Congo have proliferated since the 1990's.[4] To finance their operations, they collect taxes on the mineral sector and other activities. Currently, there are 39 identified armed groups. My data spans two historical phases: the Second Congo War (1998-2003) and the "post-conflict" period (2003-2013).

The Second Congo War (1998-2003) involved a large number of armed groups. In 1998, the Rassemblement Congolais pour la Démocratie (RCD) launched an offensive to overthrow the former DRC president in office, Laurent-Désiré Kabila.[5] The coup did not succeed, but a divided RCD units controled the Eastern provinces, and the Congolese state defended the West. In rural areas, the RCD was opposed by the self-defense groups known as the Mayi-Mayi and by the Forces De Libération du Rwanda (FDLR). These groups were internally divided and their local strategies decentralized. Ultimately, nine foreign armies and thirty

[4]The country was named Zaire until 1997.

[5]The First Congo War lasted from 1996 to 1997. It ended with the overthrow of President Mobutu.

local militia participated in the war mostly in the East.

In the "post-conflict" period (2003-2013), the Congolese state, assisted by the United Nations Peacekeeping force (MONUSCO), struggled to regain control over the East. In a peace agreement signed in Sun-City (South-Africa) in 2003, the Rassemblement Congolais pour la Démocratie (RCD) agreed to vacate the East of the Congo in order to integrate the national government. This left its former villages without protection and exposed to predation by other armed groups. In the aftermath, the Forces De Libération du Rwanda (FDLR) and local self defense groups regained territorial control until 2009, when the Congolese army, the United Nations peacekeeping force and Rwanda, removed them from a large part of the East. This operation, named Kimia II, relocated the FLDR to the forest. Currently, however, armed groups still control up to 95% of the territory in some Eastern district.[6] Between May 2012 and November 2013, the armed group named M23 established its own administration in a large territory, and formed numerous Ministries. These groups are animated by ideology, self-defense, as well as economic interests. Economic motives stand out as one of the major motivations across the two periods.[7]

2.3 A theory of stationary bandits

"It is normal. Everyone who occupies a village develops strategies to maximize the revenues in the village. When we know that we are going to be relocated out of the village, we make sure to steal as much as possible [...]. This is why the armed man is never friend if he finds

[6]In Shabunda, an administrative divsion in Sud-Kivu that this survey visited, the Raia-Mutombokis control 95% of the territory. See for instance http://radiookapi.net/actualite/2013/02/28/shabunda-la-milice-raia-mutomboki-occupe-95-du-territoire-selon-son-administrateur/

[7]See the following historical accounts: Clark (2002), Nest (2011), Nest, Grignon, and Kisangani (2011), Ngonzola-Ntalaja (2002), Stearns (2011), Vlassenroot and Huggins (2005), Vlassenroot and Raeymaekers (2004), Van Reybrouck (2008), Autesserre (2006), Autesserre (2007), Autesserre (2008), Verweijen (2013).

no interest in it." Interview with armed group member.

In this section, I develop a model where an armed group (henceforth, the bandit) can acquire resources by taxing households' activities. The model generates three insights. First, when income is not observed, taxes should be levied on observable economic activity and be inversely proportional to tax base elasticities; in particular, since gold is easy to hide, the tax on gold output should be very small, or absent. Second, when they control a monopoly of violence, stationary bandits can have a positive effect on economic activity. Third, the time horizon increases the returns to refrain from full expropriation (pillages) and to protect economic activity. The model is similar to Besley and Persson (2013), although it ignores dynamics.

Anecdotal observation dictates the following modeling choices. First, the model considers taxes on inputs and outputs. Armed groups indeed levy taxes on mineral output, taxes on mining labor inputs, taxes on food sales, taxes on transit of persons, taxes on village mills and poll taxes. I focus on labor taxes and output taxes for parsimony, and because they capture in essence the behavioral responses in all other activities. Second, the model considers a static framework where bandits can choose between acquiring civilian assets through uncoordinated pillage or instead attempt to impose a "monopoly of violence", in which case they may extract revenue through taxation, which is announced to the households. These extreme forms of expropriation also reflect the anecdotal observation of armed groups' behavior in Eastern Congo. Of 600 recorded violent events in the sample villages, 56% are pillage operations, aimed at capturing village assets. On the contrary, 40% are conquest attempts, aimed at gaining the monopoly of violence in a village, by defeating a competing armed group. Once established, armed groups collect taxes. Figure 2.1 supports the empirical distinction between the two types of expropriation. The figure shows that conquests take place early

in the morning, possibly to surprise the defense force, and pillage operations take place at sunset, consistent with a crime deterrence hypothesis (Becker, 1968).

2.3.1 Model setup

Consider one village economy and a mass of roving bandits. roving bandits engage in uncoordinated expropriation of households' assets and output. They are roving because they have a short time horizon, and do not internalize future production in the village. One bandit, however, has superior force, and can choose to impose his "monopoly of violence" in the village, turning into a stationary bandit. If he acts as stationary bandit, he announces his theft plan to households (a tax plan) so as to maximize his taxation revenue. The village is composed of k of identical households, $j = 1, 2, ..., k$. In what follows, I drop the household identifiers. Households are endowed with assets and choose the level of the following variables in N sectors $i = 1, 2, \ldots, n$: labor supply, the amount of labor to hide from the tax on labor, and the volumes of output to hide from the tax on output. The stationary bandit is a Stackelberg leader and households are uncoordinated followers.

At step 0, the strong bandit chooses whether to compete over village assets by means of pillaging (as a roving bandit) or to acquire the monopoly of expropriation at the village (become a stationary bandit), at a fixed cost F. The fixed cost captures the costs incurred in the attempt to conquer the village (waging troops, logistics, and expected losses for the group), the investments made to maintain the control of the village, and potentially, the costs of administering the village. At step 1, the stationary bandit expropriates. If, in step 0, the strong bandit chooses to acquire resources by pillaging, he competes with other roving bandits for expropriation. For simplicity, I assume that roving bandits, since they do not have a long term relationship with households, can't commit not to fully expropriate village assets and output. When there are only roving bandits, therefore, households anticipate

that they will be expropriated with probability 1 and do not produce. Therefore, roving bandits can only acquire household assets. If, in step 0, the strong bandit chooses instead to acquire the monopoly of violence (and becomes a stationary bandit), I assume he is able to develop a relationship with the households that allows him to commit to his promise of theft.[8] The stationary bandit expropriates using taxes on output $\tau = (\tau_1, \tau_2, \ldots, \tau_n)$, taxes on labor inputs $t = (t_1, t_2, \ldots, t_n)$. He announces how much he plans to steal (tax) such as to maximize his revenues taking into account behavioral responses by the households. I first present the households' and stationary bandit's problems in step 1, when the strong bandit has acquired the monopoly of violence. In a second part, I derive testable implications reflecting the relationship the stationary bandit and the economy. I focus on the three sets of implications: properties of optimal taxes; economic impact of the stationary bandit, and effect of the time horizon.

2.3.2 The stationary bandit's economy

This section lays out the optimization problems of households and the stationary bandit, and discusses under which conditions it will not be possible to tax gold output.

Labor volume productivity, $\tilde{\alpha} \in R^n$, is stochastic, and has the cumulative distribution function $F(\tilde{\alpha})$. The sector volume productivities, $\tilde{\alpha}_i \in R$ are independent across sectors, and have a known cumulative distribution function $F_i(\tilde{\alpha}_i)$. Output in sector i, $\tilde{y}_i$ is a linear function of labor inputs and volume productivity: $\tilde{y}_i = \tilde{\alpha}_i e_i$. Households are residual claimants. They sell the output they produce in the world markets, where the prices of outputs, p_i, are

[8]The assumption of binding commitment by the stationary bandit reflects features of a repeated relationship between the bandit and households. Without this assumption, the bandit will never be able to commit and will always expropriate 100% after production. Anticipating this, the households would not produce. However, this scenario leads back to anarchy. Analogous behavior can be obtained by simply introducing a cost in the bandit's payoff from deviating from the taxation plan. For similar treatment of the future, see Dixit (2004)

exogenous. Households' Bernoulli utility is concave in the unique consumption good, and the disutility from labor, $c(e)$, is separable across sectors as well as convex in each sector labor supply.

Prior to the realization of uncertainty, but knowing the distribution of real labor productivities and the vectors of taxes and prices, the households choose how much labor to supply to each sector $e_i,\ i = 1, 2, ...n$ and how much labor to hide from labor taxes $e_i^H,\ i = 1, 2, ...n$. Then, upon observing realized production, households choose the volume of output they conceal from taxes $H_i,\ i = 1, 2, ...n$, given the sector tax on output τ_i and the output price p_i. The stationary bandit expropriates after households have allocated labor supply, hidden labor, and concealed output.

Households' revenues from production in sector i take the following form:

$$\tilde{\pi}_i = (1 - \tau_i)p_i\tilde{\alpha}_i e_i + \tau_i p_i H_i - t_i\left(e_i - e_i^H\right) - G^i(H_i, p_i) - E^i\left(e_i^H, p_i\right)$$

where $G^i(H_i, p_i)$ and $E^i\left(e_i^H, p_i\right)$ are respectively the costs of concealing output and hiding labor. Therefore, $\tau_i p_i H_i - G^i(H_i, p_i)$ and $t_i e_i^H - E^i\left(e_i^H, p_i\right)$ are respectively the profits from concealing output and hiding labor. I assume the two cost functions to be differentiable, monotonically increasing in both arguments, ($G_1^i > 0$, $E_1^i > 0$, $G_2^i > 0$, $E_2^i > 0$) and strictly convex ($E_{11}^i > 0$, $G_{11}^i > 0$). In what follows, I focus on solutions where $e_i \geq 0$, $H_i \geq 0$. The household's problem is:

$$\begin{aligned}
\max_{e;H;e^H} \quad & \int u\left(\sum_{i=1}^{N}\left(\left(1-\tau_i\right)p_i\tilde{\alpha}_i e_i + \tau_i p_i H_i - t_i\left(e_i - e_i^H\right) - G^i(H_i, p_i) - E^i\left(e_i^H, p_i\right)\right)\right)\mathrm{d}F - c(e) \\
\text{s.t.} \quad & H_i \leq \tilde{\alpha}_i e_i,\ i = 1, 2, \ldots, N \\
& e_i^H \leq e_i,\ i = 1, 2, \ldots, N \\
& \sum_{i=1}^{N} e_i^j = L
\end{aligned}$$

where L is the household's total time endowment, and $\tilde{\alpha} \in R^n$ is the vector of labor productivity realizations. The problem is akin to a portfolio allocation problem, albeit here the household allocates its labor to sectors with uncertain volume productivities $\tilde{\alpha}_i$; investment is costly (the disutility of labor supply); investment is taxed (the taxes on labor inputs); the returns to investment are taxed (the taxes on output); and the household can conceal part of the returns to investment (by concealing output). The household's first order condition implies conditions under which the household will conceal the totality of output produced. For a given combination of taxes, prices, labor supply, and volume productivity realization, the household will conceal all output produced if and only if the marginal revenue from concealing an additional unit exceeds its marginal cost, i.e. when $\tau_i p_i > G_1^i(\tilde{\alpha}_i e_i, p_i)$. In that case, the optimal volume concealed is a function of τ_i, p_i, e_i, but also of the productivity realization $\tilde{\alpha}_i$, i.e. $H_i^* = H_i(p_i, \tilde{\alpha}_i e_i, \tau_i)$. However, when the household conceals only part of the output he produced (at interior solutions), the volume concealed is independent of the realization, i.e. $H_i^* = H_i(p_i, \tau_i)$. The volume concealed, as a function of realized output has the shape represented in Figure 2.2. The household's optimal choice is characterized by the following functions: $e_i^*(p, F_\alpha, \tau, t)$, $e_i^{H*}(p_i, t_i)$, $H_i^*(p_i, \tilde{\alpha}_i e_i, \tau_i)$. The sector i labor supply, e_i^*, is decreasing in p_j, $\forall j \neq i$. In the absence of income effects, it is increasing in p_i and in $E\alpha_i$. Finally, with sufficient prudence (large u'''), e_j^* is decreasing in $Var(\alpha_j)$.

The stationary bandit chooses taxes to maximize his tax revenue.[9] Since taxes are communicated to households before they make their choices, the stationary bandit internalizes the households' behavioral responses at the time of choosing the taxes. The stationary bandit's problem is:

[9]If the bandit would instead maximize a social welfare function, such as a Bergson-Samuelson functional, the optimal taxes would lie between the revenue maximizing taxes and zero. Revenue maximizing optimal taxes can be derived from the maximization of a social welfare function when the weighted average of the social marginal utilities is zero. For a review of the optimal taxation literature, see Salanie (2011) and Piketty and Saez (2012).

$$\max_{\tau,t} \quad \sum_{i=1}^{N} \left[p_i \tau_i \left(\alpha_i e_i^* \left(p, F_\alpha, \tau, t \right) - H_i^* \left(p_i, \tilde{\alpha}_i e_i, \tau_i \right) \right) - t \left(e_i^* \left(p, F_\alpha, \tau, t \right) - e_i^{H*}(p_i, t_i) \right) \right]$$

When input taxes are not available, the revenue maximizing tax rates on output (optimal tax) satisfy the following relationship:

$$\frac{\tau_l^*}{1-\tau_l^*} = \frac{1}{\tilde{\varepsilon}_l^{Y_l} - \sum_{i \neq l}^{N} \frac{\tau_i p_i Y_i}{\tau_l p_l Y_l} \tilde{\varepsilon}_l^{Y_i}} \tag{2.1}$$

where:

$$\tilde{\varepsilon}_l^{Y_i} = \frac{\frac{\partial \tilde{Y}_i}{\partial (1-\tau_l)}}{\tilde{Y}_i} (1-\tau_l)$$

is the elasticity of *observable* output in the sector, and $\tilde{Y}_i = (\alpha_i e_i - H_i) p_i$. Define

$$s_i^e = \frac{\alpha_i e_i}{\alpha_i e_i - H_i}, \; s_i^H = \frac{H_i}{\alpha_i e_i - H_i}$$

The elasticity of observable output $\tilde{\varepsilon}_i^{Y_i}$, can be rewritten as $s_i^e \tilde{\varepsilon}_i^{e_i} + s_i^H \tilde{\varepsilon}_i^{H_i}$. This formula captures that the optimal tax is proportional to the inverse of factor elasticities, a well-established result of optimal taxation (Ramsey, 1927). In the absence of distributional concerns, the indices of relative discouragement do not enter the optimal tax formula. The higher the elasticity of the sector's taxable output, the smaller the optimal tax rate. The last term in the denominator captures the fiscal externalities arising in the presence of multiple taxable activities. Increasing the tax rate in sector l might induce reallocation of labor to other sectors raising the optimal tax.

2.3.3 Testable implications

In this section, I first derive properties of the armed group optimal tax and discuss implications for the provision of public goods. I then discuss the effects of stationary bandits on economic activity. Finally, I discuss the importance of the time horizon.

stationary bandits, Taxation and Public Goods: seeing like a state

It is straightforward from the inverse elasticities formula that the stationary bandit will only tax economic activity that cannot be fully hidden. Were a tax to be levied in activities that can be perfectly hidden, households would leave no taxable revenue to the stationary bandit. As a result, the stationary bandit's taxation is limited to the following activities: *observable* transit, *observable* output, *observable* labor, sales of agricultural products at markets (where they can be observed), and taxes per household in the village (poll tax). Indeed, if the cost of migrating is high, the elasticity of households' location is low. Poll taxes are common among medieval Western states (Ardant, 1975), in which income was not observed.

If possible, the stationary bandit will increase the households' cost of concealing output or hiding labor. By increasing the cost of sheltering activities, bandits decrease elasticities, increase the optimal tax rate, and increase tax revenues. To increase the tax revenues from the transit of persons, the stationary bandit may regulate village entries, since regulation of transit decreases the elasticity of activities related to transit. To increase the tax revenues from agricultural produce, the stationary bandit may prohibit access to alternative markets to the one they tax. These actions are consistent with evidence on early modern states (Ardant, 1975). Three auxiliary predictions follow from the optimal tax.[10]

First, the stationary bandit taxing optimally one sector will offer a mix of labor input

[10]The proofs of these predictions are in the Appendix.

and output taxes. This result obtains whenever the household and the bandit are risk-averse, or when labor and outputs can be partially hidden from taxes by the household. Absent uncertainty and with perfect monitoring of labor or output, a tax on output is perfectly equivalent to a tax on labor. In the case of risk aversion, since output is stochastic and the bandit can tax both output and non-stochastic labor input, the optimal tax is an insurance contract. The risk from variable output is shared between the household and the bandit. The higher is the tax on output, the more risk is transfered away from the household to the bandit. The optimal mix with thus rely more on the output tax the more risk averse the household is relative to the bandit. In the case of imperfect monitoring of output and labor input, the bandit will maximize tax revenues by designing a mix of output and input taxes (even if there is no uncertainty). Since both risk aversion and hiding activities are plausible, a first auxiliary prediction is that tax plans at mining sites should involve a mix of output and labor taxes.

Second, in the presence of multiple sectors, taxes will reflect the presence of fiscal externalities:

$$cov\left(\tau_l, \sum_{i \neq l}^{N} \tau_i p_i Y_i\right) > 0$$

This captures the role of reallocation of factors of production across sectors. If the stationary bandit collects taxes in various sectors, raising the tax in one sector leads to a reallocation of factors towards other sectors, which he may be able to tax, increasing his tax revenues from other sectors. This "fiscal externality" raises the stationary bandit's optimal tax.

Third, when there is only one sector, and in the absence of income effects, a rise in the price of output, by decreasing the elasticity of labor supply, may under certain conditions *increase* the optimal output tax.

I do not provide a formal treatment of the optimal provision of public goods here. As in

Bates and Lien (1985) and McGuire and Olson (1996) the "monopoly of violence" will have an interest in increasing the provision of public goods up to the point where the additional dollar spent on public goods equates the marginal impact of public goods on output, discounted by the rate of taxation. Public goods provision, therefore, will be suboptimal since the stationary bandit is only a partial residual claimant on economic activity - he only internalizes a percentage of output through the optimal tax. However, the stationary bandit should provide public goods in order to increase tax revenues. The public goods I consider in this paper are protection, provision of order (Justice), and provision of infrastructure (road construction and cleaning), which are the most common public goods needed in the area under study.

Economic impact of stationary bandits

Stationary bandits are residual claimants on economic activity since they are in principle the sole actor able to expropriate systematically. Therefore, through taxes, they internalize economic activity, and develop an "encompassing interest" (Bates and Lien, 1985, McGuire and Olson, 1996, Olson, 1993). While their expropriation distorts economic incentives, they have an interest in providing security and increase economic activity to increase their tax collection. If the gains from protection by the stationary bandit overcome the distortions generated by his taxation, stationary bandits can have a positive impact on economic activity. Since he also introduces a distortion by taxing observable economic activity, the stationary bandit will be more likely to have a positive impact on economic activity when there is anarchy in his absence.

Under the assumption that in the absence of the stationary bandit, households are expropriated with probability one, $e_R^* = 0$ and $y_R = 0$. Suppose, furthermore, that the costs of hiding output are prohibitive, so output cannot be hidden ($G_1(H, p) = +\infty \; \forall H, p$). In this

case, the households' labor supply under a stationary bandit are positive. Since production is zero when there is no protection by a stationary bandit, the stationary bandit leads to increases in output.

Of course, the village may not be expropriated with probability 1 in the absence of a stationary bandit, and crime may also occur by roving bandits in villages with a stationary bandit. Suppose that an unprotected village may be expropriated by roving bandits with some positive probability, q. When households are risk neutral, output with a stationary bandit is larger than output without a stationary bandit if and only if the probability to be expropriated, q, is larger than the optimal tax rate under the stationary bandit. Introducing risk aversion reinforces the impact of the stationary bandit on economic activity, since by having the monopoly of violence, he can reduce uncertainty by developing a predictable and credible taxation plan. When households' preferences are such that they have sufficient prudence, they will respond to the reduction in uncertainty generated by the predictable expropriation plan by increasing their labor supply and thus output.

Therefore, when a village has a stationary bandit, production is likely to be higher than otherwise, especially in a context of anarchy.

Stationary bandits and the time horizon

"If an armed man knows he must live in a village, he needs the population. Groups who prefer to pillage, it's because they know they are transitory. Thus, pillage is the opposed of settling. " Interview with armed group's member.

Consider a stationary bandit with infinite periods. Let $\tilde{\pi} = \pi\beta$ be the the effective time discount factor, where β is the time discount factor, and π is the contemporaneous probability that the stationary bandit loses its monopoly, identical for all periods. When the effective

time horizon shortens, for instance from a decrease in the contest success probability, π, the returns from sustaining a monopoly of violence net of the costs of maintaining it decrease. In this case, pillage becomes relatively more attractive. A reduction in $\tilde{\pi}$ can arise from strategic interactions, if the stationary bandit's monopoly of violence in the village comes under threat.

To see this, let $V(p, F_\alpha, a_0)$ be the stationary bandit's value function. The bandit chooses to be stationary if:

$$\tilde{\pi} V(p, F_\alpha, a_0) - F > V_r$$

Clearly the left hand side is monotonically increasing in the effective time discount factor $\tilde{\pi}$ and $\exists\, \overline{\pi}$ such that: $\forall\, \tilde{\pi} \geq \overline{\pi}$, the bandit chooses to be stationary bandit; $\forall \tilde{\pi} < \overline{\pi}$, the bandit chooses to be roving bandit.

The role of the time horizon can also be elicited through its effect on the optimal tax. Suppose now that the bandit is stationary forever. Let the contemporaneous tax revenue be R. Suppose that taxes today affect production tomorrow. This is reasonable, for instance, if households' production and assets are complementary, or if households may migrate out of the village in response to high taxes, and a migration response is slow. Let this state variable be n_t, where $n_{t+1} = n(n_t, \tau_t)$ and $\frac{\partial n(n_t, \tau_t)}{\partial \tau_t} < 0$. The stationary bandit's value function is:

$$V(n_0, p, F_\alpha, a_0) = \max_{\{\tau_t\}} \sum_{t=0}^{\infty} \tilde{\pi}^t R(\tau_t, n_t)$$

By the principle of optimality, this can be rewritten as:

$$\begin{aligned} V(n_0, p, F_\alpha, a_0) &= \max_{\{\tau_0\}} \{R(\tau_0, n_0)\} + \tilde{\pi} V(n_1, p, F_\alpha, a_0.) \\ &= \max_{\{\tau\}} \{R(\tau, n)\} + \tilde{\pi} V(n(n, \tau), p, F_\alpha, a_0) \end{aligned}$$

From the first order condition and the envelope theorem:

$$\frac{\partial R\left(\tau_t, n_t\right)}{\partial \tau_t} = \tilde{\pi} \frac{\partial R\left(\tau_{t+1}, n_{t+1}\right)}{\partial \tau_t} \lambda$$

where

$$\lambda = -\frac{\partial n\left(n_t, \tau_t\right)}{\partial \tau_t} > 0$$

Clearly, when the effective time discount factor is null, $\tilde{\pi} = 0$, $\frac{\partial R\left(\tau_t^{\tilde{\pi}=0}, n_t\right)}{\partial \tau_t} = 0$, which determines the optimal tax in the static problem. However, if $\tilde{\pi} > 0$, $\frac{\partial R\left(\tau_t^{\tilde{\pi}=\tilde{\pi}^*}, n_t\right)}{\partial \tau_t} > 0$. When the contemporaneous tax revenue function, $R\left(\tau_t, n_t\right)$, is concave, this translates to:

$$\tau_t^{\tilde{\pi}=\tilde{\pi}^*} < \tau_t^{\tilde{\pi}=0}$$

Furthermore, it is straightforward to show that the optimal tax decreases with the size of the the time horizon, $\frac{\partial \tau^*(\tilde{\pi})}{\partial \tilde{\pi}} < 0$ and $\frac{\partial V(n_0, p, F_\alpha, a_0, L)}{\partial \tilde{\pi}} > 0$. Therefore, when the time horizon shrinks, or when the monopoly of violence comes under threat by a challenging armed group, an established stationary bandit will increase the rate of expropriation and may even lose interest in maintaining the monopoly of violence. But when the monopoly of violence breaks down, the bandit is no longer residual claimant. This unravels the collection of stable taxes and protection, and induces arbitrary, full expropriation by the same stationary bandit.

2.4 The data

This section describes the data. I first describe how I collected the data. I then describe how I defined the main variables in the survey and present the summary statistics.

2.4.1 Data collection

In order to obtain data on armed groups' violence and taxation, I designed a data collection project in Eastern Congo. I trained and monitored 11 local surveyors who collected the data between May 2012 and October 2013. In each village, the surveyors trained a group of village history specialists (village chiefs, elders, and mining sector experts) for the collection of non-sensitive historical data under adequate compensation (henceforth, the village specialists). The data collection activities followed a strict protocol, designed to last 7 days per village. During the day, the surveyors implemented retrospective household surveys, in private. In the evenings they provided guidance and corrected the progress of the village specialists. In the last day of data collection activities in each village, after having implemented 8 household surveys, the surveyors held a day-long meeting with the village specialists. In this meeting, the researchers and village specialists confronted the data gathered from the different sources, and surveyors collected additional sensitive data based on the information they gathered in the household surveys. The data from this meeting compiles information from all activities and is the main source of data used in this paper.

Since armed groups are mostly active around mining activity, this study focuses on villages endowed with minerals. I also sampled a subset of agricultural villages, which I selected matching agricultural villages to all mining villages based on all geographic characteristics, prior to data collection. In each administrative division, I drew a random sample of gold villages, and the population of coltan villages. Figure 2.3 maps the villages in the sample based on their endowment of minerals. The villages endowed with coltan and gold are well distributed across the various administrative divisions, with no clustering of villages by mineral endowment.[11]

[11]To identify the villages endowed with gold, coltan, or other minerals, I used the data from a mapping implemented in 2009 by IPIS, a Belgian research institute, and SAESSCAM, a local mining institute. To

2.4.2 Measurement

This section defines the variables I use in this paper .

I define the presence of a stationary bandit in a given village as whether an armed person or group had a monopoly of violence in the village for at least 6 months in a given year. stationary bandits are common in Eastern Congo, and villagers faced no difficulties identifying stationary bandits from roving bandits; the former have no intention to leave the village in the short-term, and usually provide protection to the whole village. The language used to identify stationary bandits is "organization of security". Therefore, implicitly, stationary bandits always provide security.

To measure violent events, I focus on two major types of attacks, consistent with the anecdotal evidence. I measure *conquest attempts* by whether an armed group engages in violence with another armed group, with the aim of acquiring the monopoly of violence of the village. I define *pillages* as violent events on the village aimed *exclusively* at capturing assets. Local populations were able to distinguish the purpose of the attacks unambiguously. *Pillages* also differ from *conquest attempts* on observable characteristics, which facilitates their distinction for the local population. In a *pillage*, armed groups usually arrive at sunset and flee with the village assets within a narrow period of time. Figure 2.1 shows the distribution

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verify these lists, I interviewed in Bukavu a large number of mineral dealers working through Sud-Kivu, during 3 months of preparatory work. Based on the compiled dataset, I endowed the surveyors with a list containing all mining villages of each administrative division. In order to strengthen the representativeness of the sample to be selected, I designed a strict *prospection* protocol. Following this protocol, in the first week of work in each administrative division, the surveyors verified the existence and endowments of the villages in the list, and added mining villages they discovered to exist. Upon finishing *prospection* work, and in areas where they had access to electricity, the surveyors communicated the cross-checked lists by satellite phone. I then implemented random sampling of gold villages using a statistical package and communicated the selection using satellite phone. In areas without access to electricity, the surveyors followed a strict protocol for selecting gold villages, based on a list of random numbers I previously generated using a statistical package.[12]

of attack times, separating conquests from pillages.[13]

To measure taxes, I collected both the intensive and extensive margin of taxation on different activities. There was always consensus on whether an expropriation was a tax. Local populations are familiar with distinct types of expropriation by various actors. Empirically, the distinction was straightforward: a tax is always anticipated and perceived part of an implicit contract with the population, but other types of expropriation (*pillages*, or arbitrary expropriations, known as "tracasseries") are neither anticipated nor agreed upon.

To measure public goods, I collected data from three sources. First, the definition of stationary bandits implies that they provide protection. Second, to go a step further, and since the provision of security may just be a claim, I asked the villagers whether the armed group was *effectively* providing protection. This captures the difference between the running level of security and the level of security that would otherwise have occurred in the absence of effort by the stationary bandit to maintain security. Third, I collected data on the provision of justice, as well as the establishment of new administration of justice by armed groups. Fourth, I collected yearly data on public works by armed groups, and public works under the demands of armed groups - road construction and road cleaning.[14]

The next section describes the strategies implemented to address measurement error and potential reporting biases at the data collection phase.

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[13]The following quote is extracted from an interview with an armed group's member. *"In case of fast pillage, such as FDLR operations, it is only 30 minutes. Certain goods are not pillaged (heavy goods: cows, beans, heavy minerals, household members). It is when the pillager is certain that there will be no external intervention that he can take all and can use some village members for their transport."*

[14]The village mineral endowment was already identified during *prospection* work. I did not record any discrepancy between the mineral endowment from prospection and survey data indicating whether the village had exploited a given mineral. I use yearly prices of minerals from United States Geological Survey (2010).

2.4.3 Design based strategies to minimize measurement error

To address measurement error at the design phase, I used three strategies. First, I designed the village protocols so that surveyors extracted multiple signals on the village history from private conversations in 8 household surveys. During the last day meeting, surveyors then confronted the information collected in household surveys to the information they collected with the assistance of the village specialists. This strategy was highly effective as an averaging method. It also helped surveyors correct information provided by the village specialists during the final meeting. Indeed, households were overwhelmingly willing to reveal armed groups' data in private, and there was consensus that this was not problematic.[15] To increase confidence in the survey measures, Section 2.8.2 compares the data to other data sources, as well as well-known political events. Second, as suggested in (de Nicola and Gine, 2012), I developed an exhaustive set of time cues, based on information gathered in the survey pilot, and on well known regional and national political events. Time cues allowed respondents and surveyors to assign with years to regional events known to all. Third, survey questions focused on events easy to memorize. For instance, there are two types of bags that carry the heavy minerals (50kg and 75kg). To measure the output tax, surveyors obtained the fee paid on one bag and bag size used. I then compute the tax per kilogram based on these variables. To measure the tax on labor input, surveyors obtained the fee paid to obtain the right to work one day in the mine. These fees are stable. Because mining taxes are the heavier daily expense of miners, they were straightforward to collect. The responses of households and village specialists were highly correlated at time of the interviews.

[15] Armed groups were absent in most villages. The Congolese army already protected a large number of the sampled villages. In the Territoire of Shabunda, certain villages were under the control of the Raia-Mutombokis at time of the interview. The Raia-Mutombokis did not consider sensitive the information collected by this survey. This armed group had existed for less than one year. Surveyors collected historical data on events preceding the Raias and about the Raias without problems. Surveyors were confident that they achieved the same degree of quality as in other villages, including for data on the present years.

2.4.4 Descriptive statistics

Figure 2.4 shows armed groups' presence in the sample. There are 18 armed organizations. Most of the variation is concentrated in four groups: the Congolese Army (40% of control year*villages and 5% of attacks), Mayi-Mayi's (20% of control year*villages and 21 % of attacks), RCD-Goma (15% of control year*villages and 11% of attacks), and FDLR (12% of control year*villages and 45% of attacks). The RCD and the Mayi-Mayi's were the main actors during the Second Congo war. The Congolese Army and the FDLR are 85 % of the occupation year*villages after 2006. Figure 2.6 shows the distribution of stationary bandits episodes duration. The median duration of each stationary bandit is 4 years, so is the median unprotected episode. Table 2.1 shows summary statistics.

The next sections establish three results. First, Section 2.5 reveals that the stationary bandits tax is consistent with optimal taxation when households' income is unobserved, and provide public goods. This behavior is comparable to feudal European states strategies to maximize tax revenues, as described in Ardant (1975), and provides support to Tilly (1985) and Olson (1993)'s criminal account of states. Second, Section 2.6 shows that control by stationary bandits has positive impacts on economic activity. Third, Section 2.7 establishes that stationary bandits limit their expropriation rate and commit to taxation plans only when their monopoly of violence is not under threat.

2.5 Stationary bandits' taxes and public goods: seeing like a state

This section examines the taxation and public goods provision patterns of stationary bandits. I demonstrate that stationary bandits' expropriation strategies, taxation, is comparable to

medieval European states and consistent with optimal taxation. Furthermore, I observe that stationary bandits consistently provide basic public goods and enjoy some popular support.

2.5.1 Stationary Bandits' Taxation

Armed groups use taxes on various observable economic activities. Table 2.2 shows all taxes that I was able to observe in this study. I report the proportion of village*years in the sample in which armed groups collect any of the observed taxes. There are 1,977 such observations since 1998.

As shown in Table 2.2, in 30% of observations in which a stationary bandit controls a mining village, stationary bandits raise taxes on the mining sector, where they focus on taxing mining output and mining labor. To tax mining output, they create barriers at the exit of the production site, where workers carrying mineral are asked to pay a fixed fee that is tied to the size of the bag they carry. These results provide support to the inverse elasticities rule: while the elasticity of gold concealing activities is high, the tax on gold output is low.

As shown in Table 2.2, to tax mining input, stationary bandits create barriers at the entry of the production site, where workers intending to work are required to pay a fee per day of work. This is a tax on observable labor inputs, albeit implemented in dollars per day of work. In Section 2.3.3, I argued that stationary bandits, as European feudal states, should attempt to take actions that increase the cost of concealing economic activity. Furthermore qualitative evidence I collected demonstrates that at production sites, stationary bandits make efforts to increase the costs of hiding mineral output. They use violence in order to racket mineral output when civilians are hiding it, and confiscate clothes in order to inspect them.

Table 2.2 also shows that in 24% of observations with a stationary bandit, the stationary bandit raises taxes on sales of agricultural output in the local market. To collect these taxes,

stationary bandits station at the local market where traders of cassava are asked to pay a daily tax for the right to sell cassava in the market. While taxes differ by the type of good sold at the market, I collected only taxes on cassava for parsimony. This tax is similar to taxes raised by European feudal states in rural markets. Furthermore, as documented in Ardant (1975), in order to increase tax revenues, medieval states imposed the location of rural fairs so as to diminish the elasticity of observable economic activity that they could tax. I find that the stationary bandits in my sample engage in the same practice. The qualitative evidence collected by the surveyors and myself demonstrates that settled armed groups create and impose local agricultural markets in order to collect taxes. They prohibit households to use alternative markets. This decreases the tax elasticity of observable economic activity and raises the tax revenues.

In addition, Table 2.2 reveals that in 35% of observations in which a stationary bandits is present, the stationary bandits raises a tax on transit of persons. To implement this tax, they impose roadblocks at the village entries and exits, where they require a regular fee per passage. Furthermore, the qualitative data reveals that to maximize the tax collected from this activity, they regulate population movement, and only allow the population to transit through a limited set of checkpoints. This behavior is analogous to European feudal states' strategies to raise taxes on economic activity when income was unobserved. As described in Ardant (1975), states used to regulate population movements in and out of towns by creating town walls and collect taxes on transit at the town major entry and exit doors. This tax is levied disproportionately in agricultural villages, in which income is lowest and a large mass of the population lives at subsistence.

Table 2.2 also shows that stationary bandits collect regular poll taxes in 65% of the observations in which they are settled in the village. This is consistent with the discussion

in Section 2.3.3.[16] This strategy compares to modern states, which used to collect poll taxes when income was difficult to observe. This tax is levied disproportionately in mining villages, possibly because the population is wealthier.

I now turn to the strategies stationary bandits employ to collect poll taxes. As discussed in Section 2.3.3, the village Chief is a useful intermediary between stationary bandits and the population to collect taxes. This can lead armed groups, like European feudal states and colonial powers, to delegate tax collection to village Chiefs and to indirect rule. The following explains why armed groups rely on village Chiefs to collect poll taxes: the Chief knows the distribution of wealth of his village and enjoys legitimacy. The second challenge is that poll taxes generate resentment by the population if they are collected by armed groups. The following quote is an extract from interviews implemented on armed groups' members: *"If they ask the [poll] tax themselves they can be poisoned by the households who give against their will. The only difficulty is that in the village there are rich and poor households, the latter have nothing to give. The groups do not know who are the wealthy taxpayers."*[17] Stationary bandits thus collect poll taxes using the Chief as intermediary to maximize tax revenues, as European feudal states. Furthermore, the level of the poll tax is often decided collectively, with the participation of villagers. In 9.7 % of the observations with a stationary bandit for which I have data, the villagers report to participate in the decision of the tax level and allocation. It is thus no surprise that in 8.9 % of observations

[16]Poll taxes are defined as periodic taxes collected per head or per household. These taxes were also called head taxes, or capitation taxes.

[17]Next are additional quotes supporting this view. *"If the group prefers to delegate tax collection to the Chief it is to avoid to be considered as beggers. If they ask themselves for the tax, the population can quit the village." "If the armed men prefer to use the channel of the Chief [to collect the poll tax], it is first for strategic reasons. If they ask the population directly, the latter will be scared and will leave the village, but the population is a necessary potentiality to their survival in the village." "If the armed men prefer to pass by the Chief [to collect the poll tax] it is for two big reasons. First, if this is a group that will stay in the village, they are afraid that the tax will be considered as a "tracasserie". Second, the group does not know the number of households in the village, they can tax some and miss other households".*

in the sample, the population perceives the tax level to be fair. The strategy of delegating the allocation of the tax burden to the communities was frequently used in feudal states, as described in Ardant (1975). This system, experimented in the Paris district in the year 1781, allowed to exploit local information so wealthier tax payers were charged more, but also to discipline the incentives of individual tax payers to comply to the taxation plan - since non-compliance was easy to monitor by the community.

In what follows, I test the three auxiliary implications presented in Section 2.3.3.

First, the mix of input and output taxes is consistent with the theoretical predictions under the standard assumptions on risk aversion and imperfect monitoring. Table 2.2 shows that armed groups provide a mix of input and output taxes around coltan mines. This elaborate contract is consistent with optimal taxation if both the bandits and the producers are risk averse and their incomes are tied to mineral extraction. Alternatively, it is also consistent with imperfect monitoring and hiding activities. Both risk aversion and hiding activities match the anecdotal and qualitative evidence on the mining sector.[18]

Second, I find evidence that the stationary bandit's taxes are consistent with the fiscal externality channel. In order to estimate $cov\left(\tau_l, \sum_{i \neq l}^{N} \tau_i p_i Y_i\right)$, I computed the day equivalent tax in dollars in various sectors. For the mining sector, I used taxes at the mine of the village, while for the agricultural sector I take taxes at the market on cassava sales. I then computed a daily sector i tax index for each sector as follows:

$$I_i = \left(\hat{Y}_i \tau_i + t_i\right) p_i$$

[18]With non-linear tax instruments, bandits could set tax rates that depend on production. It is however unlikely that this type of contract is implementable, especially with the technology at hand.

where $\hat{Y}_i$ is the estimated average volume product per worker for one day of work, τ_i is sector i tax on output, t_i is sector i tax levied on one day of work, and p_i is the local output price in sector i.[19] Since the daily tax indexes have a large mass of observations at 0, I use a tobit regression of daily tax index in the relevant mining sector, on the daily tax index in agriculture, clustering standard errors at the village level. Table 2.3 presents the result. There is a positive association between the mining tax index and the estimated tax revenues from agriculture, consistent with the prediction from the model.

Third, Table 2.4 shows the OLS results of the extensive margin of taxation regressed on mineral prices and endowments. Positive shocks to both gold and coltan lead to the emergence of taxes on mineral activity. Table 2.5 shows the OLS results of the intensive margin of taxation regressed on mineral prices and endowments. Positive shocks to both gold and coltan lead to higher tax rates, consistent with a reduction of labor supply elasticities.

2.5.2 Stationary Bandits' Public Goods Provision and popular support

Figure 2.5 presents the provision of public goods by armed groups in the period. Since the collapse of the DRC state in 1997, stationary bandits emerged and occupied consistently around 70% of villages in the sample. However, their behavior is not limited to expropriation.

If armed groups have a concern with economic activity, they should protect property rights and guarantee physical security. Figure 2.5 shows that in 50% of the villages they controlled in 2004, stationary bandits were considered to provide "effective" security services.

[19]Since I do not observe taxes on output for gold and since the tax on cassava is charged on one day of sales at the market, $\tau_i = 0$ in gold and agriculture. I therefore only need to estimate production for heavy minerals, coltan and cassiterite. Based on the average production reported in all year-village observations, I imputed 20 kilograms both for cassiterite and coltan.

As discussed in Section, stationary bandits should have an interest in promoting order by providing their location with a functioning judiciary. As shown in Figure 2.5, in 50% of villages they controlled in 2004, stationary bandits provided Justice in the village by administering village disputes. These disputes include marriage disputes, land disputes, and other bilateral conflicts. I collected qualitative data that supports this result. The following quote is extracted with one interview with an armed group member: *"Once they established it, all civil and penal affair must pass by their hands. They required a lot of money to the plaintiff and the defendant"*.[20]

In 50 % of the villages they controlled, stationary bandits developed rudimentary bureaucracies, which they used to administer the entire village affairs. It is unclear what type of activities this administration entails. There is no evidence that any stationary bandit in the sample participated to, or initiated public goods works. This does not mean that stationary bandit are not maximizing. The following quote, extracted from an interview with an armed group's member shows why the road decreases armed groups' revenues: *"The one who knows he is doing bad things must hide. Such is the case of armed men who can rise barriers on the road. The armed men are afraid that the authorities will catch them. Plus, on the main road, there are many people passing; they can reveal these practices to the hierarchy"*.[21]

Figure 2.5 also reports the proportion of villages in which stationary bandits faced popular support. To measure popular support, I asked in the household and experts' survey whether the villagers opposed the stationary bandit that was present in the village, for each year in which a stationary bandit was present. The figure reveals that on two-thirds of

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[20]The following quote suggests that the administration of justice was motivated by extraction of rents: *"If for instance, two persons had a conflict, the moment one came to ask us to kill their opponent, they would give us a lot of money. Either we executed the mission, or we would communicate it to the other one, so he also gives us a lot of money."*

[21]Another armed man revealed: "[...] the armed men must fear to be seen in such [taxation] practices. They can settle in some areas and operate, but not in the main road"

villages in which a stationary bandit is present, there are no popular oppositions agains the stationary bandits. This share decreases slightly during the Second Congo War (1998-2003) as a result of conquest by external armed groups (especially the Rassemblement Congolais pour la Democratie), but the share of villages in which the population does not oppose the stationary bandit is always above 50%.

This section has shown that armed groups' taxation is consistent with the predictions associated with optimal tax, as well as with the documented taxation by medieval states who were unable to observe income. Furthermore, armed groups provide basic public goods. This has an important implication: settled armed groups internalize the behavioral responses of the population and intend to minimize distortions, as speculated in Olson (1993). If, absent stationary bandits, there is anarchy, then stationary bandits should have a positive impact on economic activity. The next section provides empirical support to this prediction.

2.6 Economic impact of stationary bandits

This section examines empirically the economic effects of stationary bandits in the village they occupy. If armed groups internalize the value of economic activity they can extract in taxes, they should provide protection and promote growth. If property rights are sufficiently insecure in the absence of a stationary bandit, his protection will have a positive effect on output. Control by a stationary bandit has ambiguous distributional implications: indeed, while they provide protection, they also maximize tax revenues for their own consumption.

2.6.1 Estimation using heterogeneous effects of price shocks

At the beginning of the year 2000, Playstation, a subsidiary of Sony, announced the sale in Christmas Playstation II which relied on processed columbite-tantalum (coltan). Coltan

extraction was at the time mostly done in Australia, and required large investments. In response to the announcement, coltan processing firms rushed for coltan from other areas where supply could respond, leading the daily price of coltan to skyrocket from $90 per kilogram to $590 per kilogram, where it persisted until year end. Most of the supply response came for Eastern Congo.

I exploit the timing of this price shock and the fact that 55 villages in the sample were endowed with coltan to identify the heterogeneous effect of the price shock on coltan villages with and without stationary bandits. If stationary bandits promote economic activity and there are no unobserved variables determining both the location of stationary bandits and output, then the output effect of the price shock should be larger in villages with a stationary bandit. Figure 2.7 shows the estimates of output for coltan villages in the period (in kilograms per miner per day). The left quadrant shows this estimate for villages that did not have a stationary bandit in 2000 and the right quadrant shows this estimate for villages that had a stationary bandit in 2000. Clearly, the price shock led to massive output response in villages that had a stationary bandit. Since stationary bandits may have selected the villages with largest output in response to the price shock, I then plug the residuals of output regressed on village and year fixed effects, and year dummies interacted with distance to the airport. In Sánchez de la Sierra (2014b) I show that the impact of the year 2000 price shock is concentrated in villages in the proximity to airports because coltan is bulky and requires air transportation and distance to the airport is a major determinant of the effect of the price sock. The residuals are reported in the dashed line. Clearly, even after controlling for observables determining the potential value of a coltan village, the residual effects of the price shock is larger in villages where a stationary bandit is present than otherwise. However, this analysis relies on selection on observables only. The next section exploits a different identification strategy with a shock to the presence of a stationary bandit directly.

2.6.2 Estimation using using the timing of a peace agreement

The intervention: the 2003 Sun-city peace agreement

In order to estimate the *impact* of stationary bandits on the local economy, I exploit the timing of a Peace agreement singed in 2003 that affected all villages controlled by the Rassemblement Congolais pour la Démocratie equally. This agreement led the RCD to abandon the villages it used to control. The withdrawal of the RCD resulted in a security vacuum. The villages that were under the control of the RCD in 2003 are sparsely distributed across the survey area. By 2005, the RCD had abandoned all villages.

Econometric strategy

To identify the Intention to Treat Effect, I can Difference in Differences to estimate the impact of RCD affiliated stationary bandits on economic activity. However, compliance is partial: the RCD did not vacate all villages immediately. Difference in Differences would lead me to underestimate the causal impact of stationary bandits on economic activity. I therefore instrument presence of the RCD with the timing of the Sun-City agreement interacted with the dummy indicating the presence of the RCD in 2002.[22] The two stage least squares model is:

$$\begin{aligned} Y_{it} &= \alpha_i + \beta_t + a\hat{RCD}_{it} + \varepsilon_{it} \\ RCD_{it} &= \eta_i + \gamma_t + bPOST_t * RCD_i^{2002} + u_{it} \end{aligned}$$

where i indicates the village and t indicates the year. $POST_t$ is a dummy taking the value 1 for all observations after 2002 and 0 otherwise, and RCD_i^{2002} a dummy indicating whether a

[22] I do not report the Difference in Difference results here but they are identical, smaller in magnitude.

village was under the control of the RCD in 2002. Y_{it} are village level outcomes, in particular, dummies indicating mining activity and recorded output; α_i are village fixed effects, and β_t are year fixed effects. Since the RCD location is potentially endogenous, I use the timing of the RCD withdrawal interacted with its initial location as an instrument. The coefficient of interest, a captures the changes in economic indicators in RCD villages since the Sun-City Peace agreements (henceforth, treated villages), compared to the respective changes in economic indicators in non-RCD villages, under the identifying assumption that the timing of the agreement did not coincide with time trends that affected villages under the RCD control differentially. In particular, it may be that the RCD was occupying mostly coltan villages, and that the world price of coltan decreased at the same time of the agreement. This is particularly worrisome if the agreement was signed in anticipation of a decrease in prices. To account for this, I include controls for mineral endowments interacted with their prices as well as geographic controls. A test of the prediction on the *impact* of stationary bandits is thus H_0: $a \leq 0$, estimated using the timing of the RCD withdraw as an instrument, against the alternative H_1: $a > 0$, estimated using the timing of the RCD withdraw as an instrument.

Results

Figure 2.8 provides a graphical representation of the first stage and the reduced form. The timing of the Sun-City peace agreement is clearly associated with a mild decrease in economic activity in the villages formerly controlled by the RCD.

Table 2.6 reports the corresponding IV estimates, in which I include village and year fixed effects across specifications. Since the price of certain minerals was falling at the time of the agreement, it is possible that the agreement may reflect omitted variables associated with anticipation of the mineral prices. One may also worry that the RCD will have vacated first

the villages of lowest economic potential. Since all specifications include village fixed effects the result cannot be due to selection out of villages of particular constant characteristics. Furthermore, since all specifications include year fixed effects, the results cannot be due to a downward trend common to all villages. However, the potential tax revenues of certain villages may vary differently over time. To control for this, Table 2.6 also includes specifications with controls for mineral endowments interacted with the world price of minerals and other geographic controls interacted with year dummies. The results are unchanged, although turn marginally significant in some specifications. The results suggest that the RCD presence had mild, positive effects on economic activity.

2.7 Stationary bandits and the time horizon: effect of a military intervention

"This is why the armed man is never friend if he finds no interest in it."

This section shows that when stationary bandits' time horizon shrinks, their state-like behavior breaks down to full violent expropriation of the villages under their control.

2.7.1 The intervention: the Kimia II military operation

To identify the causal impact of a reduction in the time horizon of a stationary bandit, I exploit the timing at which a military strategy that targeted aggressively the Forces De Libération du Rwanda (FDLR) went under way and credibly threatened the FDLR control. In 2009 and 2010, the Congolese Army, with the help of United Nations forces and the Rwandan army, targeted villages under FDLR control in order to weaken their link with the tax base. The Congolese army scaled up the operation in 2010 and thereafter, and renamed

it Amani Leo. Henceforth, I refer to both as the Kimia II operations, consistent with the villagers' parlance.[23] On the one hand, the threat posed by Kimia II abruptly reduced the time horizon of the FDLR. On the other hand, consistent with historical accounts of the formation of fiscal capacity during war (Tilly, 1985), the FDLR might have had incentives to increase tax collection in order to secure their territorial control. The second effect is implausible. The FDLR was very weak and had no hope of resisting the operation. It is natural to interpret the operation as a shock to the time horizon. I collected qualitative data that supports this interpretation. The following is an extract from an interview with an armed group member: *"Kimia II was a large operation targeted against the FDLR. The latter, knowing that they could not face the government coalition, before the coalition arrived, attacked multiple times the villages to have something to eat"*.

2.7.2 Econometric strategy

The main outcome variable I use is a dummy indicating whether village i in year t was pillaged by the FDLR. I also considered other variables, such as evidence of FDLR taxation and FDLR public goods provision and the results are identical, so I just report the results on pillages. As introduced in Section 2.4.2 pillages are an extreme form of expropriation. This view is reflected in the following interview extract with an armed group member: *"In case of fast pillage, such as FDLR operations, it is only 30 minutes. Certain goods are*

[23] "In South Kivu, Kimia II operations also resulted in some important successes as FARDC gained control of much of Kalehe, Kabare and Shabunda territories after years of domination by FDLR. On 28 July, FARDC succeeded in dislodging FDLR from Kashindaba, its main headquarters in South Kivu. In August, FARDC operations challenged FDLR strongholds in Walungu, Mwenga and Sange and around Uvira. On 20 August, FARDC successfully concluded operations to clear FDLR presence from the Kahuzi-Biega National Park as well as Tchivanga and Nindja areas in Kabare territory". Following Kimia II, the Congolese army continued its implementation under the label of Amani Leo. "Amani Leo also aims to regain control of mining sites from the FDLR and combat illegal trafficking in minerals and other natural resources." Source: monusco.unmissions.org.

not pillaged (heavy goods: cows, beans, heavy minerals, household members). It is when the pillager is certain that there will be no external intervention that he can take all and can use some village members for their transport." The presence of a pillage by the stationary bandit signals the collapse of the proto-state. This is visible in the following interview: *"To pillage is a negative sign signifying that we have no more interests in the village. To settle is a sign that we find a village eligible according to our criteria. We must collaborate with the population"*. I hence use pillage as an indication of state collapse.[24] The econometric specification is:

$$Y_{it} = \alpha_i + \beta_t + aPOST_t * FDLR_i^{2008} + \varepsilon_{it}$$

where i indicates the village and t indicates the year. Y_{it} are village level pillages by the FDLR. α_i are village fixed effects, β_t are year fixed effects, $POST_t$ is a dummy taking the value 1 for all observations after 2008 and 0 otherwise, and $FDLR_i^{2008}$ a dummy indicating whether a village was under the control of the FDLR in 2008. The coefficient of interest, a captures the increase in pillages since Kimia II operations in FDLR villages (henceforth, treated villages), compared to the increase in pillages since Kimia II operations in non-FDLR villages. A test of the prediction is thus H_0: $a \leq 0$ against the alternative H_1: $a > 0$.

2.7.3 Results

Figure 2.9 presents graphical evidence on the intervention. Following the Kimia II intervention, FDLR taxation drops from 100% to 20% in the former FLDR villages within 2 years.

[24]The following quote, also from an interview with an armed group member suggests that the maximization of revenues is a powerful tool to explain the transition from taxation to pillage: *"It is normal. Everyone who occupies a village develops strategies to maximize the revenues in the village. When we know that we are going to be relocated out of the village, we make sure to steal as much as possible [...]. This is why the armed man is never friend if he finds no interest in it."*

Figure 2.9 also presents the results. While the number of FDLR pillaging operations remains around 10% of villages in the control group, it rises from 0% to 30% in the treated group (the former FDLR villages). Table 2.7 provides Difference in Differences estimates. Pillaging operations by the FDLR increase drastically against their formerly controlled villages. They can therefore be implemented despite the presence of the Congolese Army in neighboring villages, while stable taxation cannot. It is well documented that during the Kimia II operations, the FDLR relocated in the neighboring forests, and increased their pillages on the villages they used to control in order to guarantee access to resources. These results are also strongly supported by the qualitative reports elaborated by the surveyors. The villagers narrations collected in the surveyors' qualitative reports indicate that the increase in pillages was caused by the arrival of the Kimia II operations, which shortened the hopes of the FDLR to perpetuate taxation. Before fleeing, the FDLR pillaged. Judiciary and journalistic evidence also supports this finding.[25] This result suggests that the time horizon, by affecting the expected revenues from taxation, is a necessary condition for stationary bandits to tax and provide public goods like states. The next section provides a discussion.

[25]See http://www.icc-cpi.int/iccdocs/doc/doc1225453.pdf. The following quotes are taken from interviews with armed group's members: *"These attacks, it was before the Congolese Army often. When they appreciated that the Congolese Army was going to arrive, they started being atrocious towards their population only to have something to eat where they were going." "I told you that if armed men know they will lose the battle, before leaving, they pillage. Kimia II was a large operation targeted against the FDLR. The latter, knowing that they could not face the government coalition, before the coalition arrived, attacked multiple times the villages to have something to eat". "In some villages, the Congolese Army battled the FDLR. But most times, when the FDLR learnt that the operations were approaching, they let their families go first and they followed. On their way, they pillaged before the arrival of FARDC. Sometimes, after the passage of the latter, the FDLR would come back to attack the village."*

2.8 Discussion

In this section, I discuss whether my results fit alternative explanations and then compare the outcome variables to various benchmarks.

2.8.1 Alternative mechanisms

A large part of the violence literature emphasizes the role of the trade-offs between production and expropriation, in particular the opportunity cost of fighting (Collier and Hoeffler, 2004, Dube and Vargas, 2013). My results suggest an additional determinant of armed groups' behavior: their ability to commit to complex taxation plans over a long horizon when they own a monopoly of violence. In what follows, I discuss the two causal empirical results of this paper in light of alternative explanations: the impact of the Sun-city peace agreement, and the impact of Kimia II.

First, I find that in response to the timing of a peace agreement, stationary bandits abandon a large number of villages and this leads economic activity to plummet. While I am not able to identify the exact channel through which presence of stationary bandits causes economic activity, I find support for a causal explanation. This result provides support to Olson (1993) criminal metaphor, especially in areas where the alternative to stationary bandits is anarchical. Another implication of the peace agreement could be that since a major armed group is required to leave, a large mass of criminals that had previously been mobilized by the armed group as stationary bandits may suddenly become unemployed. Since the RCD was the major employer of violent labor, its departure would have depressed the returns to engage in violent activities. If the opportunity cost of fighting is a relevant explanation for violence, the peace agreement would have increased the supply of labor in productive activities, rising output. This prediction does not find support in the data: I find on the

contrary that indicators of economic activity plummet after the departure of the stationary bandits.

Second, I find that in response to a large military operation against the FDLR, pillages by the FDLR in the former FDLR villages increase. This result is consistent with a contestation/time horizon channel: as the FDLR sees their monopoly of violence under threat, they have no incentives to refrain from full expropriation, provide public goods and maintain the village states. Another implication of the military intervention could have been that the returns to mobilize fighters in FDLR villages increase in response to the external threat, as described for instance in accounts of European state formation (Tilly, 1985). As a result, villages may have increased efforts to recruit fighters, which would have increased the returns to fighting relative to production. This would have increase the mass of fighters and violence. However, it is unlikely that an increase in the number of fighters recruited to fight the Congolese Army coalition would have resulted in more pillages against the population they were recruited to protect. Furthermore, I find no spike in recruitment following hte Kimia II intervention.[26] This interpretation, furthermore, goes against all evidence collected in interviews auxiliary to this paper, which suggests the FDLR was too weak to fight and their time horizon was depressed by Kimia II.[27]

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[26] Unreported here.

[27] "I told you that if armed men know they will lose the battle, before leaving, they pillage. Kimia II was a large operation targeted against the FDLR. The latter, knowing that they could not face the government coalition, before the coalition arrived, attacked multiple times the villages to have something to eat". "In some villages, the Congolese Army battled the FDLR. But most times, when the FDLR learnt that the operations were approaching, they let their families go first and they followed. On their way, they pillaged before the arrival of FARDC. Sometimes, after the passage of the latter, the FDLR would come back to attack the village." Interviews with armed groups' members.

2.8.2 Measurement error

Classical measurement error in the outcome variable might lead to noisy estimates and can threaten identification of the parameters in the linear probability model (Frisch, 1934, Hausman, 2001). Worse, if the data is of low quality, arbitrary correlations stemming from systematic mistakes by enumerators might underlie arbitrary results unrelated with real effects. I first provide reassuring graphical evidence and then compare the data to the alternative data sources.

Figure 2.5 in section 2.2 plotted the survey-based measures of armed groups' occupation on years. Since historical trends are known, this allows to verify the quality of the main variables. The changes in the data correspond precisely to commonly known historical events. Stationary bandits emerge and substitute for the state in villages in 1996 and 1997 with the AFDL rebellion, but particularly in 1998 with the RCD conquests. The data also coincides with known dates of RCD occupation: it emerged in 1998 with the second Congo War and retreats in in 2003/2004 following the 2002 peace agreements of Sun City. The timing of the attacks also corresponds precisely to well known historical rebellions. Figure A.3 shows that the data maps precisely to the known phases of the war: the AFDL led its rebellion in 1996 into 1997, the RCD between 1998 and 2003, and the CNDP in 2004. Finally, survey-based recall prices closely track the international world price. Overall the data on prices, armed groups, and attacks benchmarks extremely well to well-known historical events.

To verify the data quality a step further I also assigned violent events geo-coded by an external dataset (ACLED) to circles around the survey villages. The ACLED dataset contains 3,500 violence events since 1997, coded by perpetrator and type of event. As opposed to the data collected in this survey, these data are based on reports. When an event falls in circles assigned to more than one village, I allocated the event to all village circles in which it

fell. I then compare this data to my source that contains the number of attacks on villages.[28] Figure A.4 shows that the ACLED dataset systematically reports less violence data around the selected villages than what the current survey. While it is possible that this difference is due to villagers over-reports of violence events, under-reporting by villagers in the survey is more likely to be a concern, due to memory loss and fear of retaliation. If data from this survey is more likely to under-report violent events, this suggests that my survey perfoms well, possibly improving upon the ACLED dataset for the specific locations. To cross-validate this survey data econometrically, I correlated the data to ACLED's data in an OLS framework. Regression estimates of the correlation between ACLED and survey conquest attacks in the neighborhood of the village are positive, significant, but never larger than 10% across all distances (Table A.1). In addition, attacks reported by this survey are less likely to be reported in ACLED when the villages were under RCD occupation (suggesting that the source of under-reporting in ACLED could be obstruction of information), and when the attacks were pillages (ACLED may specifically under-estimate the number pillages).[29] The data collected in this survey also captures the well-known evolution in conflict intensity in the region. Figure A.4 shows the increase in violence during the Second Congo War, especially during the coltan shock, its decrease after 2003, and its drastic increase as a result of the Kimia II operations, which consists mostly of FDLR pillages.

[28]This source contains larger numbers of violent events than the other survey source because villagers could not remember the details of some attacks.

[29]This result applies specifically to RCD perpetrated violent events, consistent with reporting bias.

2.9 Conclusion

While the the state has attracted scholars of all social sciences, a fundamental fact in all disciplines is that prior to the state, there are no statistics. There is therefore no formal statistical evidence of states before they have developed a bureaucracy capable of collecting data. This absence of data on preliminary states has shaped our conceptualization of the state, and biased it towards governments as most of us know them: stable, bureaucratic, capable of taxing income, subject to the rule of law and enjoying (often) legitimacy.

I question the view of the state as a "monopoly of violence" and provide econometric evidence linking organized crime to the actions that characterize states. To observe nascent states, I managed a team of data collectors in stateless and war-torn areas of Eastern Congo and collect a village-level panel dataset of yearly activity of armed groups. I develop a theory, similar to Olson (1993) in which the ability to hold a "monopoly of violence" in order to expropriate induces armed groups to develop stable, reasonable taxation, and promote economic activity such as medieval states did (Ardant, 1975, Tilly, 1985).

Consistent with this theory, I find that armed groups, in villages where they hold a monopoly of violence, develop elaborate taxation plans that are consistent with optimal taxation and similar to medieval European states, provide public goods, and enjoy some popular support. This result challenges normative and sensationalist accounts of armed groups' behavior by providing a rational explanation for their choices, and grounds it on a unique dataset of armed groups' behavior.

I take advantage of two natural experiments during the time period observed to go a step further. First, I exploit a political agreement that resulted in the withdrawal of a settled armed group, I show that the presence of criminal "monopolies of violence" is associated with higher economic activity. Second, I show that a large military intervention aimed

against a large number of village criminal "monopolies of violence", and which reduced their time-horizon, induced the "stationary bandits" to revert from state-like stable taxation and protection, to arbitrary full expropriation - violent pillage on populations they formerly protected.

This paper's results contribute to a large literature in economics (Acemoglu and Robinson, 2006, Acemoglu, Ticchi, and Vindigni, 2006, Bates, Greif, and Singh, 2002, Besley and Persson, 2009, Grossman, 1997, North and Weingast, 1989), but also to disciplines outside of economics. The conceptual similarity between states and organized crime has been established by historians, anthropologists and sociologists (Hintze, 1906, Tilly, 1985). Olson (1993) endowed this analogy with theoretical foundations. However, none of these studies was able to provide systematic evidence to establish this link, or to demonstrate empirically the causes that lead criminals to engage in state-like behavior instead of pillaging, or to demonstrate their palpable effects on economic activity when they choose to do so. This paper is the first to provide empirical support this conceptualization of the state - by showing that even criminal "monopolies of violence" collect regular taxes, provide public goods, enjoy legitimacy and can be beneficial to growth. However, I go a step further and show that this not alone sufficient: the state collapses if its time horizon is short. This refines existing scholarship that reduces states to "monopolies of violence".

This paper has implications for policy on armed groups, weak states, and development aid.

Armed groups are a topic of interest among current governments. This study is one of the first to provide econometric evidence of the relationship between conflict and mineral endowments in the DRC. While there are vivid debates and speculations around the motives of armed groups, I establish a systematic link between the mining sector and armed groups' taxation. This result is in line with other reports based on qualitative data, such UN (2002).

Furthermore, I show that the benefits that armed groups can extract from the mineral trade depend on physical properties of the mineral that affect their ability to tax: while coltan is easy to tax, gold is not. This result should help refine mineral specific policies aimed at targeting armed groups. Second, I find that interventions aimed at constraining armed groups' revenues *increase* violence against civilians in the short-run. Interventions that attempt to weaken armed groups finances have become dominant among policy circles. In particular, the United States issued the Dodd-Frank legislation in 2012, aimed at constraining purchases of minerals whose trade is a source of finance to armed groups. Governments interested in "cleaning" the mineral chain, thus, may need to protect civilians in the aftermath of these interventions, and provide alternative occupations to combatants who lose access to revenues from taxes as a result of these interventions.

Weak states are a major threat to global stability and a major concern for the security of Western governments. First, developing the Congolese state capacity to collect taxes can be a powerful deterrent of violence against civilians by the state. The findings illustrate the role played by expected taxation revenues on government stability, reflecting existing academic scholarship on the determinants of state predation (Bates, Greif, and Singh, 2002, Besley and Persson, 2008). Second, interventions aimed at improving the Congolese state's capacity to pay salaries to armed state officials are a promising avenue to reduce violence and pillaging. Armed state officials lacking a stable source of revenue may engage in violent expropriations in order to secure revenue.

With 5,532 Million USD received in 2011, the Democratic Republic of the Congo was the second largest receipient of international aid flows in 2011, and the fifth in 2012 (World Bank, 2013). First, this paper suggests that development aid disbursed in this area provides a potential source of revenue to armed groups. This paper's findings demonstrate that armed groups often settle in individual villages, and systematically raise taxes on a large number of

economic activities, not only on the mineral trade, contrary to what is commonly believed. Therefore, delivery of aid that does not take into consideration the revenues it can generate to armed groups may counteract policies aimed at weakening armed groups. Second, the findings of this paper also suggest *how* to design aid in order to reduce this risk. Aid that translates into unobservable or intangible assets, may be harder to tax. This is the case of education and health. Attention needs to be paid to the process in which these services are delivered. This also implies that the returns from aid delivery will be larger if armed groups are not taxing in targeted villages, suggesting that aid delivery and interventions that eradicate armed groups are complementary. Third, aid may increase civilian exposure to risk of violence if wealth can be converted to portable assets. The findings of this study indeed suggest that pillages target specifically portable assets. This finding is consistent with the findings of Nunn and Qian (2012).

The "stationary bandits" I am able to observe are a mixed blessing. On the one hand, they challenge the authority of the state by holding alternative "monopolies of violence" and abuse civilians. On the other hand, they develop relationships of mutual dependency with civilians, provide services that the state is unable to provide, and may even have positive economic impacts if their territorial monopoly is not under threat. In addition, through the period I examine, armed public servants, like criminal groups, engage also in abuse of civilians, protection, and public goods provision. This suggests that academic and policy thinking that distinguishes stationary bandits from the state might lack a fundamental similarity between them: having comparative advantage in violence, they will choose among various forms of expropriation. These strategies might benefit the population when there is an economic surplus that can be taxed and when the criminal is not under threat, which may in some cases lead to behavior that characterizes states as most of us know them.

2.10 Tables

Table 2.1: Summary statistics

	No minerals	Coltan	Gold
	Mean	Mean	Mean
	(sd)	(sd)	(sd)
Number of external attacks at the mining center	.	0.64	0.02
	.	(2.21)	(0.21)
Number of internal attacks at the mining center	.	0.22	0.01
	.	(0.95)	(0.11)
Number of external attacks at village	0.18	0.13	0.22
	(0.38)	(0.34)	(0.42)
Number of internal attacks at village	0.02	0.04	0.03
	(0.14)	(0.20)	(0.17)
Armed organization stationed at the village	0.40	0.37	0.30
	(0.49)	(0.48)	(0.46)
Armed organization living inside the village	0.37	0.32	0.27
	(0.48)	(0.47)	(0.44)
Armed organization sharing power with the village authorities*	0.26	0.52	0.47
	(0.44)	(0.50)	(0.50)
Armed organization administering the village	0.07	0.19	0.07
	(0.25)	(0.40)	(0.25)
Size of the village (Number of households in 2012)	187.15	172.13	102.56
	(116.39)	(101.38)	(44.07)
Number of individuals immigrating per year	24.4	21.37	16.19
	(69.67)	(76.19)	(60.10)
Number of individuals emigrating per year	15.22	15.20	13.57
	(49.67)	(59.42)	(52.49)
Observations	460	368	207

Notes: Standard errors in parentheses. This table presents the mean and standard deviation of the main outcomes, classifying villages by whether they were endowed with coltan, gold or no minerals. * indicates that the variable was added after survey start.

Table 2.2: Frequency of tax collection, by type of tax

	Heavy minerals	Gold only	Agriculture
Mining: per mineral output	0.30	0.00	0.00
Mining: per labor input	0.30	0.25	0.00
Poll taxes	0.67	0.70	0.33
Transit of persons	0.36	0.25	0.53
Tax on cassava sales	0.24	0.25	0.16
Village mill	0.11	0.18	0.03

Notes: This table presents the taxation results. Since I am interested in the behavior of existing stationary bandits, I condition the sample to observations where a stationary bandit is present in the village. The numbers indicate the proportion of villages in which any of the following taxes was collected by a stationary bandit: tax on mineral output (first line), tax on mining labor inputs (second line), poll tax on households (third line), tax on transit of persons (fourth line), tax on sale of cassava at the local market (fifth line), tax on village mill (sixth line).

Table 2.3: Test of the fiscal externality channel

	(1)	(2)
VARIABLES	model	sigma
Agriculture tax index	1.937e+06**	
	(859,293)	
Constant	-609,841***	482,854***
	(124,753)	(92,741)
Observations	3,033	3,033

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table presents the results of a tobit regression of the index of taxes on one day of labor in the mining sector of the index of daily taxes on cassava sales at the local market. The indexes are the estimated day equivalent tax in dollars in each sector. For the mining sector, I use taxes at the mine of the village, while for the agricultural sector I use taxes at the market on cassava sales. I then computed a daily sector i tax index for each sector as follows: $I_i = \left(\hat{Y}_i \tau_i + t_i\right) p_i$ where $\hat{Y}_i$ is the estimated average volume product per worker for one day of work, τ_i is sector i tax on output, t_i is sector i tax levied on one day of work, and p_i is the local output price in sector i. Since I do not observe taxes on output for gold and since the tax on cassava is charged on one day of sales at the market, $\tau_i = 0$ in gold and agriculture. I therefore estimate production for heavy minerals, coltan and cassiterite. Based on the average production reported in all year-village observations, I imputed 20 kilograms both for cassiterite and coltan. Standard errors are clustered at the village level.

Table 2.4: Price shocks and taxes

VARIABLES	(1) Output tax roadblocks	(2) Entry tax	(3) Food sales tax	(4) Poll tax	(5) Transit tax	(6) Mill tax
Coltan X Pc	0.515***	0.393***	-0.008	0.337**	0.080	0.057
	(0.105)	(0.093)	(0.095)	(0.138)	(0.092)	(0.054)
Gold X Pg		0.001	0.001	0.006*	-0.006**	0.002
		(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
Coltan	0.110**	-0.083	-0.038	0.068	-0.070	-0.010
	(0.043)	(0.069)	(0.057)	(0.076)	(0.069)	(0.035)
Constant	-0.026*	0.106**	0.118***	0.237***	0.189***	0.051**
	(0.015)	(0.052)	(0.039)	(0.050)	(0.047)	(0.023)
Observations	1,586	1,586	1,342	1,586	1,586	1,586
R-squared	0.132	0.017	0.014	0.052	0.026	0.014

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table presents the results on the baseline specification for dummies indicating whether armed groups collect a particular tax in the village as dependent variables. Standard errors are clustered at the village level. The dependent variable in column (1) is a dummy indicating whether an armed group collects a tax on output at roadblocks outside the mine. The dependent variable in column (2) is a dummy indicating whether an armed group collects a tax on labor at the mine (entry tax). The dependent variable in column (3) is a dummy indicating whether an armed group collects a tax cassava sales at the local market.The dependent variable in column (4) is a dummy indicating whether an armed group collects a tax on households (poll tax). The dependent variable in column (5) is a dummy indicating whether an armed group collects a tax on transit of persons in and out of the village. Finally, the dependent variable in column (6) is a dummy indicating whether an armed group collects a tax on a local mill in the village.

Table 2.5: Price shocks and tax levels

VARIABLES	(1) Output tax roadblocks	(2) Entry tax	(3) Food sales tax	(4) Poll tax	(5) Transit tax	(6) Mill tax
Coltan X Pc	3.08***	2.29	0.01	7.50***	8.60**	5.03
	(0.75)	(2.56)	(0.20)	(2.06)	(3.81)	(5.99)
Gold X Pg	-0.14**	-0.02	0.00	0.06	0.10	0.06
	(0.06)	(0.10)	(0.01)	(0.08)	(0.17)	(0.27)
Constant	-1.64***	-2.14**	-0.37***	-0.29	-7.22***	-14.15***
	(0.45)	(0.92)	(0.08)	(0.60)	(1.64)	(0.53)
Observations	805	506	829	837	846	848

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table presents the results on the baseline tobit specification for left-truncated variables indicating the level of different taxes dependent variables. Standard errors are clustered at the village level. The dependent variable in column (1) is the tax on output at roadblocks outside the mine, in US$ per kilogram. The dependent variable in column (2) is the tax on labor at the mine (US$ per entry to the mine). The dependent variable in column (3) is the tax cassava sales at the local market, in US$ per day of sale. The dependent variable in column (4) is the tax on households (US$ per week). The dependent variable in column (5) the tax on transit of persons in and out of the village, in US$ per passage. Finally, the dependent variable in column (6) a tax on a local mill in the village, in US$ per day of activity.

Table 2.6: Economic impact of stationary bandits: IV estimates of RCD impact

VARIABLES	(1) Output	(2) Output	(3) Output	(4) Output	(5) Output
RCD	0.220**	0.174*	0.202**	0.169*	0.162
	(0.0877)	(0.0899)	(0.0972)	(0.0884)	(0.111)
Coltan X Coltan Price		-10.70***	-10.64***	-9.723**	-13.48***
		(3.815)	(3.846)	(3.869)	(4.418)
Gold X Gold Price			0.0129		
			(0.00992)		
Observations	436	436	436	436	400
R-squared	0.024	0.068	0.067	0.183	0.083
Number of panel units (mines)	109	109	109	109	100
Year FE	YES	YES	YES	YES	YES
Village FE	YES	YES	YES	YES	YES
Region*Year FE	NO	NO	NO	YES	NO
Distance to airport	NO	NO	NO	NO	YES
Mineral controls	NO	YES	YES	YES	YES

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table presents the results on the 2SLS regression of the main welfare outcomes on presence of the Rassemblement Congolais pour la Démocratie. The endogenous regressor RCD is a dummy variable indicating whether the RCD controlled the village in a given year. The first stage regresses RCD on an interaction of a variable indicating whether the observation is posterior to the year 2003, and a dummy indicating whether the observation is a village that was under the control of the RCD in the year 2002, the year prior to the Sun-City agreement. All regressions include year fixed effects and village fixed effects. Standard errors are clustered at the level of the village. Column (1) presents the baseline 2SLS regression. Column (2) includes as control control the interaction between a dummy indicating whether the village is endowed with coltan and the US price of coltan in US$ per kilogram (Coltan X Coltan Price). Column (3) adds to this control the interaction between a dummy indicating whether the village is endowed with gold and the US price of coltan in US$ per kilogram (Gold X Gold Price). Column (4) includes region*year fixed effects to the baseline specification. Column (5) includes as control the distance to the airport, interacted with year dummies.

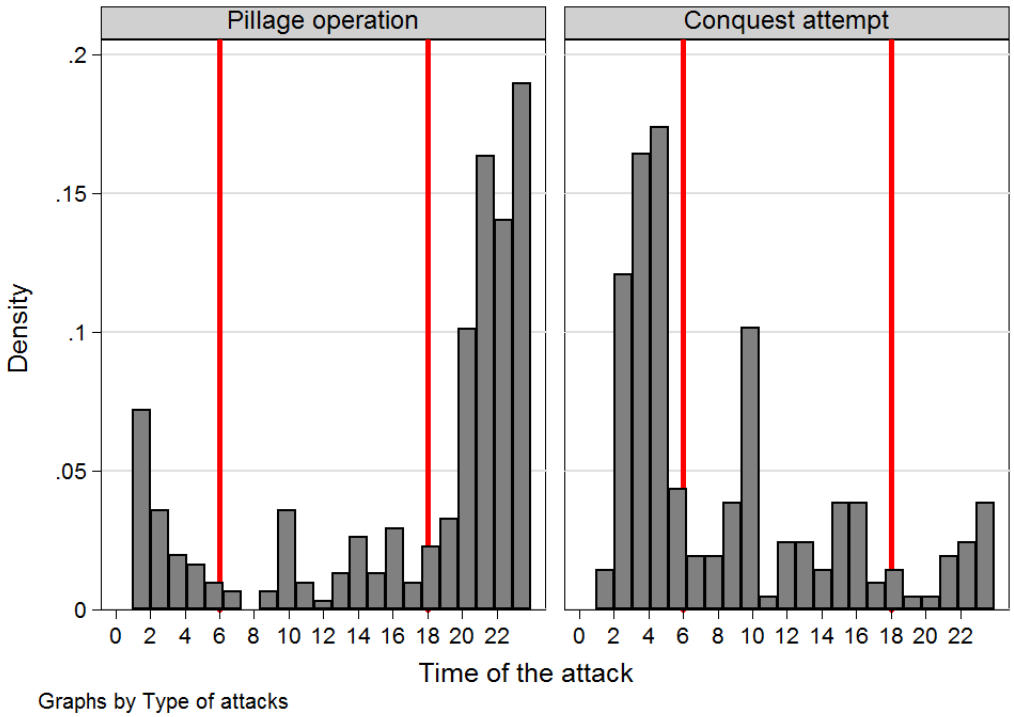
Table 2.7: Stationary bandits and the time horizon

VARIABLES	(1) FDLR taxbase	(2) Village taxes	(3) FDLR Attack	(4) Pillage	(5) Pillage	(6) Pillage	(7) Pillage	(8) Pillage
KIMIA2	-0.741***	-0.389***	0.278***	0.259***	0.250***	0.228**	0.196**	0.259***
	(0.0936)	(0.116)	(0.0827)	(0.0792)	(0.0759)	(0.0893)	(0.0952)	(0.0632)
Constant	0.140***	0.494***	0.0763***	0.0903***	0.0916***	0.0992***	0.120***	0.0903***
	(0.00643)	(0.00959)	(0.00568)	(0.00544)	(0.0250)	(0.0259)	(0.0311)	(0.0125)
Observations	786	654	786	786	786	726	600	262
R-squared	0.788	0.703	0.335	0.279	0.320	0.292	0.290	0.601
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Village FE	YES	YES	YES	YES	YES	YES	YES	YES
Region*Year FE	NO	NO	NO	NO	YES	NO	NO	NO
Distance to road	NO	NO	NO	NO	NO	YES	NO	NO
Accessibility	NO	NO	NO	NO	NO	NO	YES	NO

Notes: *** p<0.01, ** p<0.05, * p<0.1. This table presents the results on the OLS regression of FDLR behavior. The regressor Post*FDLR interacts Post and FDLR. Post is a dummy variable indicating whether the observation is after 2008, and FDLR is a dummy variable indicating whether the village was controlled by the FDLR in 2008, and hence a target of the Kimia II operation. All regressions include year and village fixed effects. Standard errors are clustered at the village level The regressors Post and FDLR are thus absorbed by the fixed effects. Columns (1)-(3) use the following variables as dependent variables, respectively: FDLR tax base (a dummy indicating whether the FDLR collects taxes in the village in that given year); Village taxes (a dummy indicating whether any group collects taxes in the village in that given year); FDLR attack (a dummy indicating whether the village received an attack by the FDLR in that given year). Columns (4)-(8) present variations to the baseline specification, using as dependent variable a dummy indicating whether the village was subject to pillages in that given year. Pillages are defined as a violent event in which an armed group enters the village in order to acquire household assets and leaves the village after. Pillages tend to be very violent and are straightforward to identify. Column (4) presents the results on pillage for the main specification; column (5) adds region*year fixed effects, column (6) adds distance to the road, interacted with year dummies; column (7) adds a time-varying control dummy indicating whether the village has access to the road; column (8) collapses all observations by treatment status and by time period (pre and post), thus ignoring the time-series information.

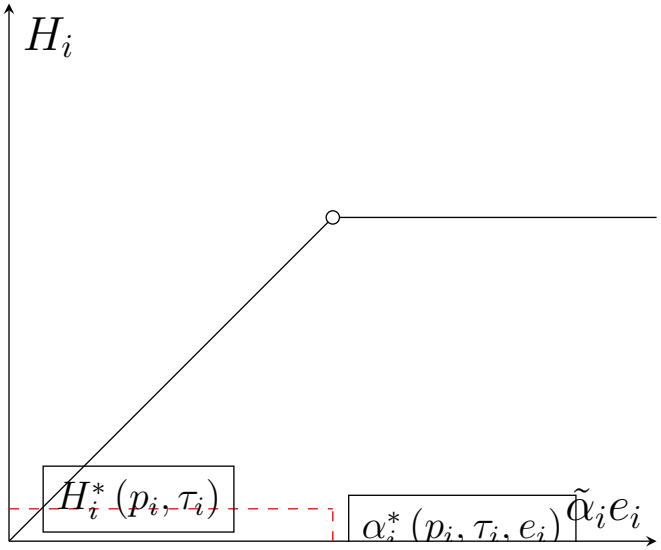
2.11 Figures

Figure 2.1: Timing of pillages and conquest attempts



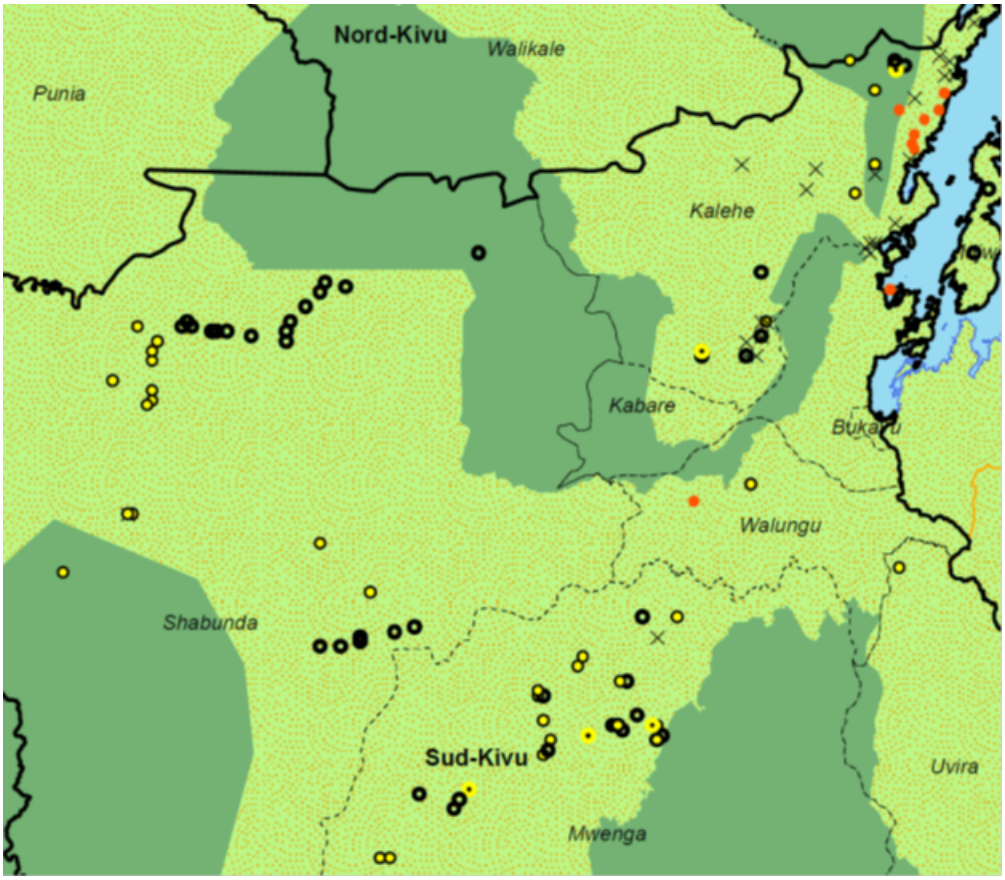
Notes: This figure draws on the attacks level dataset collected in this survey. In total, there are 600 attacks. The figure plots the distribution of the recorded *hour* of the attack. The horizontal axes indicate the hour of the attack in 24h format. The vertical axes indicate the mass of observations. The left quadrant plots the distribution for recored pillages, and the right quadrant plots the distribution for recorded conquest attempts. The red, vertical lines indicate the hours at which the sun rises (left line) and sets (right line). In the outer of these lines the village is in the dark.

Figure 2.2: Labor real productivity and volume hidden by households



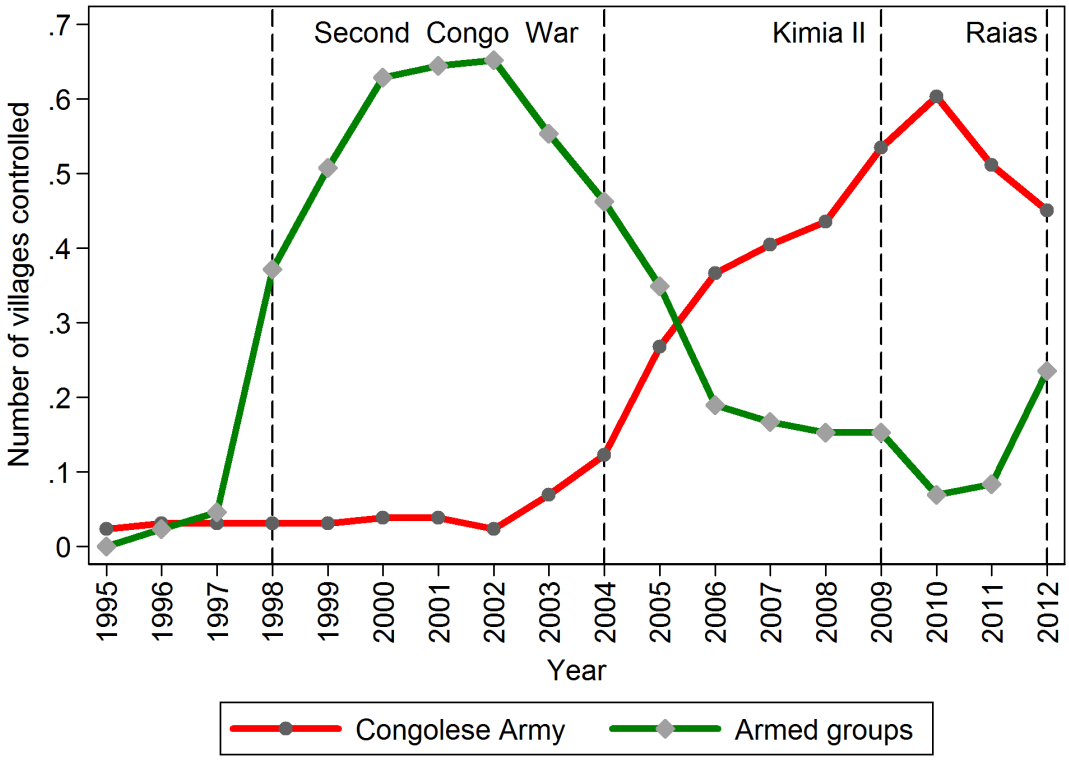
Notes: This figure represents the relationship between the volume of output hidden in sector i, H_i, and the realized output in sector i, $\tilde{\alpha}_i e_i$ derived from the household's first order conditions. If the realized labor volume productivity is below a certain threshold, the household is at a corner solution and hides all output. If the realized labor volume productivity is above the threshold, the volume hidden is an interior solution. Any additions to the realized volume productivity do not increase the volume hidden by the household.

Figure 2.3: Villages in sample



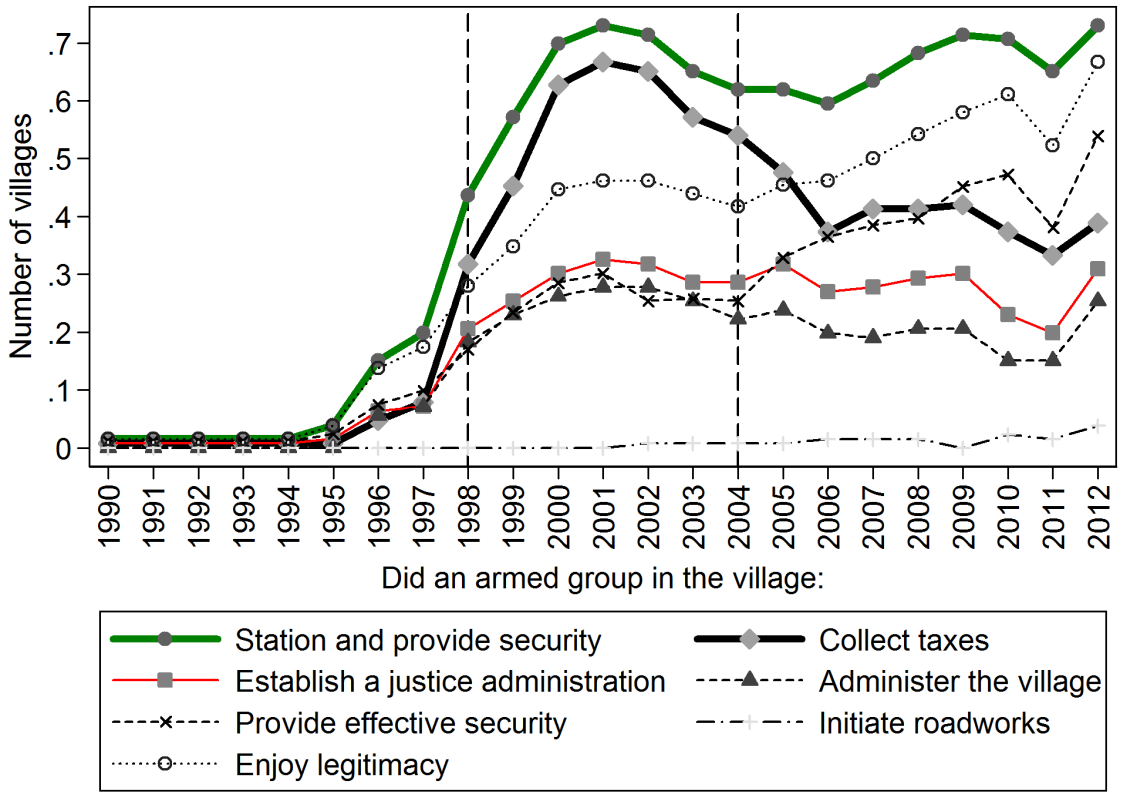
Notes: This map presents the spatial location of the survey villages across Sud Kivu. Thick lines indicate frontiers between Provinces, dashed lines indicate fronteers between Territoires. Dark green indicates a natural reserve (forest). Blue indicates the lake Kivu. The villages endowed with coltan only are indicated in black circles. The villages endowed with coltan and gold are indicated as black dots with golden circles. Villages endowed with cassiterite only are indicated with orange dots, and agricultural villages are indicated as crosses. In each Territoire (Shabunda, Mwenga, Walungu, Kabare, Kalehe) the survey visited the population of coltan villages that were accessible to the survey teams. In addition, it visited a sample of gold villages randomly selected within the Territoire. In addition to this, in the Territoire of Kalehe, the survey visited all mining villages, as well as a matched sample of agricultural villages. To match agricultural villages to the population of mining villages in Kalehe prior to the survey, I matched multiple data sources, including SAESSCAM (Congolese institute of mining), the Institute of Peace (Belgium), and data I collected with my own qualitative fiedlwork in May-July 2012 with mining traders. After compiling the population of mining villages of Kalehe, I then used their geographic coordinates to assign to each a distance to: the lake, the natural reserve (forest), Bukavu, Goma, Rwanda, and the road. Since mining villages are likely to be more remote by the fact that they are more valuable and populations self select into locations, I matched the resulting population of mining villages on the population of villages based on the Mahalanobis metric on these variables. To obtain geo-locations of the population of villages prior to the survey, I used publicly available data from Référentiel Géographique Commun (2010), that I compiled with a dataset collected by the International Rescue Committee and Humphreys, Sánchez de la Sierra, and Van der Windt (2012, 2013a), who mapped all villages in most Territoires of Sud Kivu and collected geo-locations for each.

Figure 2.4: Group affiliation of the village monopoly of violence



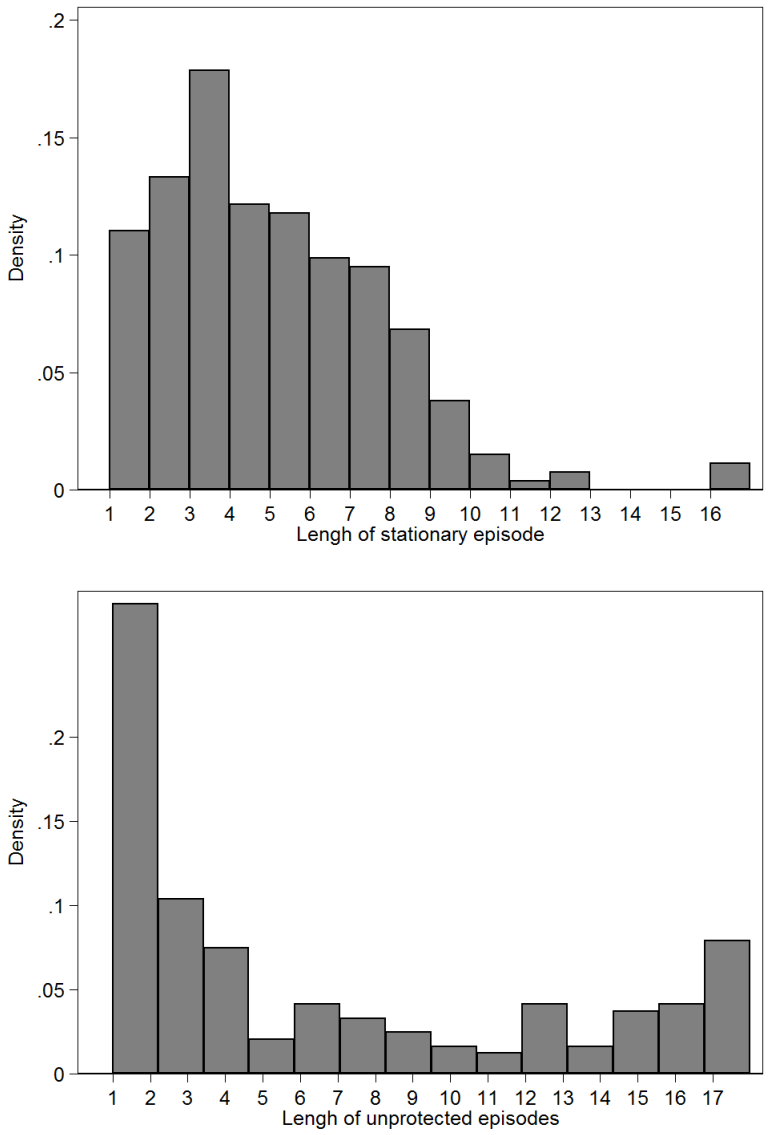
Notes: This figure graphs the share of villages in the sample under the control of a given armed group. The vertical axis represents the number of villages, and the horizontal axis represents the years. The green (light grey) line indicates the proportion of villages in the sample controlled by armed groups. The red (dark) line indicates the proportion of villages in the sample controlled by the Congolese army. The data fits well-known historical phases of the conflict and post-conflict periods. The dashed vertical lines indicate the start and end of the rebellion by the Rassemblement Congolais pour la Démocratie. The thick vertical line on the left indicates the timing of the Kimia II operations, and the thick vertical line on the right indicates the timing of the emergence of the Raia Mutombokis (Raias). The Congolese Army progressively replaces armed groups after 2004, the end of the second Congo War. The Congolese Army progressively replaces armed groups after 2004, the end of the second Congo War. However, local armed groups were integrated into the national army after 2003, without relocation. Armed groups structures of command and network remained intact. The distinction between the Congolese Army and armed groups is thus blurred.

Figure 2.5: Stationary bandits taxes, public goods, and popular support



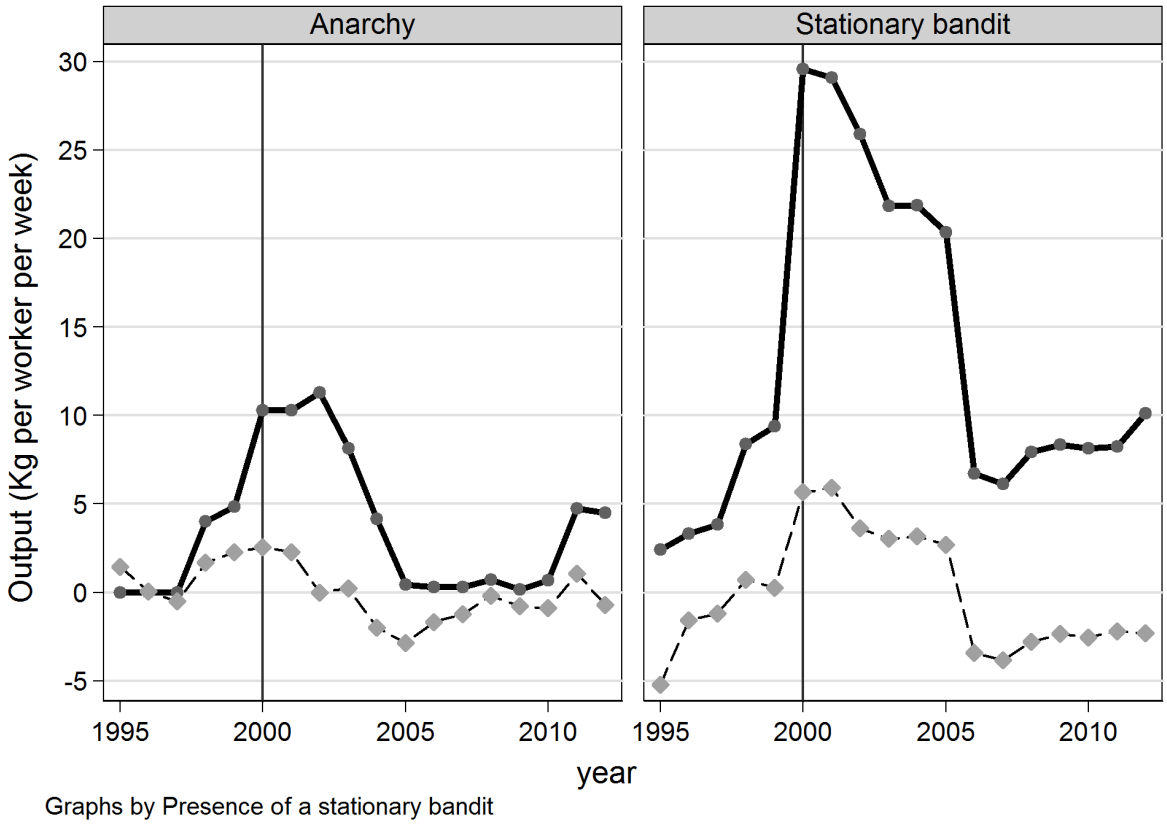
Notes: This figure graphs the share of villages in the sample in which an armed group coded as stationary bandit implements the following activities: is a stationary bandit and secures the village, collects taxes, administers justice, administers the village, his security provision is perceived to be effective, initiates road works, and enjoys legitimacy. I measured legitimacy by whether there were any oppositions to the group inside the village. Armed groups were coded as stationary bandits when they had the monopoly of violence at the village for at least 6 months. The vertical axis represents the share of villages, and the horizontal axis represents the years. Labels for activities are included in the legend.

Figure 2.6: Duration of stationary bandits



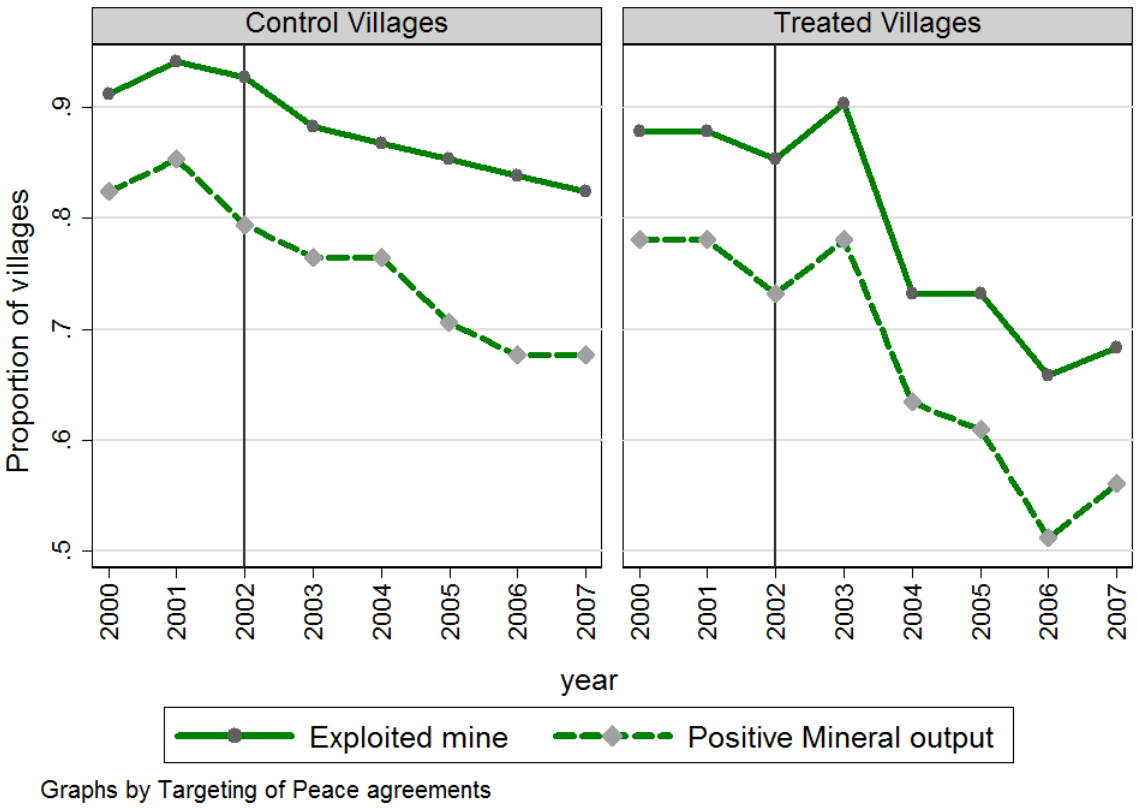
Notes: The upper figure plots the distribution of the duration for all episodes in the sample in which a village was under the control of a given stationary bandit. The lower figure plots the distribution of the duration for all episodes in the sample in which a village was under the control of *no* stationary bandit. For both figures, the vertical axes represent the density and the horizontal axes indicate the number of years.

Figure 2.7: Economic impact of stationary bandits: using the coltan price shock



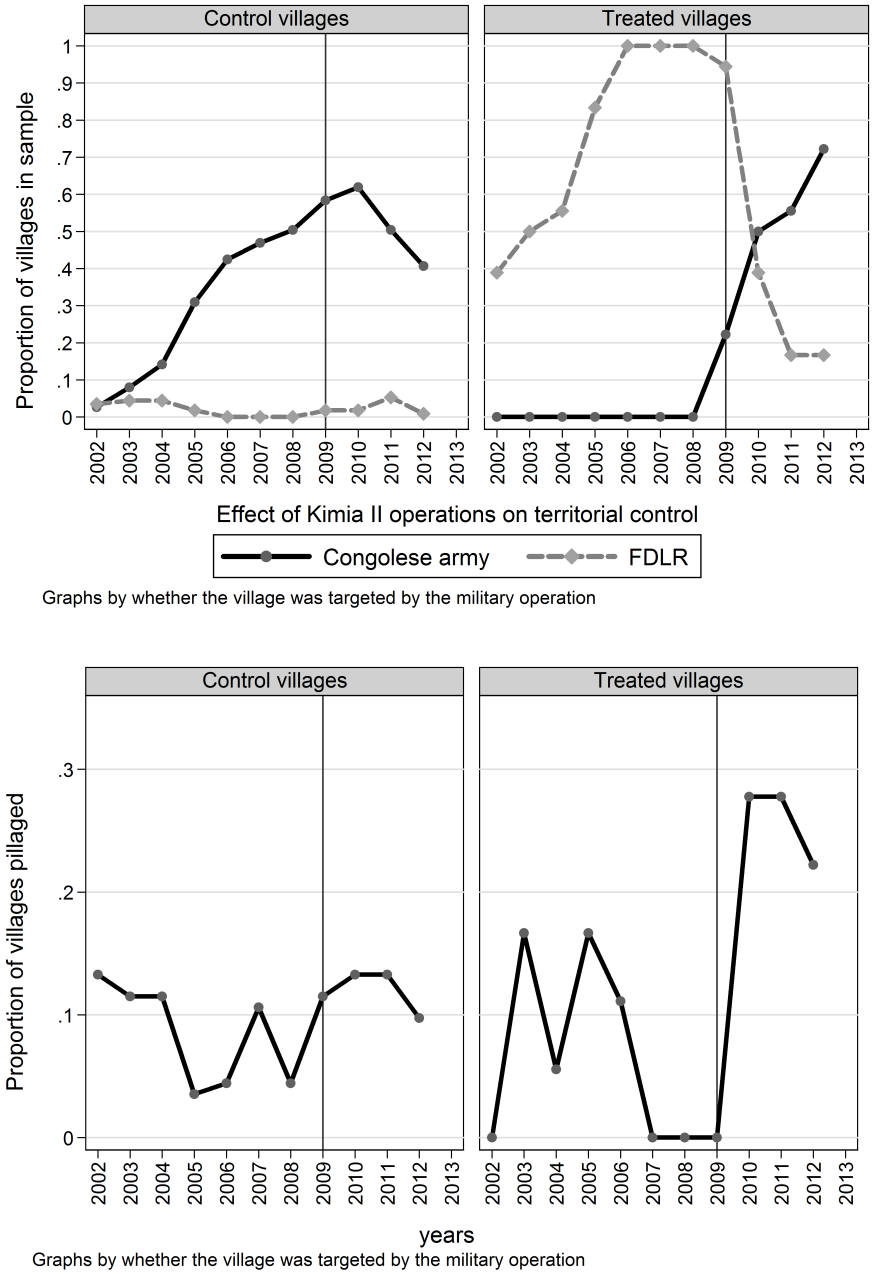
Notes: This figure presents estimates of output collected from the survey for villages endowed with a coltan mine. The thick black line shows the average output produced by one miner per day of work, for villages in which a stationary bandit was not present in 2000 (Left) and villages in which a stationary bandit was present in 2000. To control for coltan village potential value, I then include in the regression controls for the value of a coltan village identified in Sánchez de la Sierra (2014b). The dashed line shows the residuals, after regressing output on village fixed effects, year fixed effects, and year dummies interacted with distance to the nearest airport. Conditional on selection on observables, stationary bandits have a positive impact on the effect of the coltan price shock on output.

Figure 2.8: Economic impact of stationary bandits: using the timing of a peace agreement



Notes: These figures present the evolution of economic indicators by year. The left figure presents the villages that were not under the control of the RCD in 2002, and the right figure presents the evolution for villages that were under the control of the RCD in 2002. The solid line indicates the proportion of villages in which mining is active, and the dashed line indicates whether there is evidence of positive mining output.

Figure 2.9: Stationary bandits and the time horizon: effect of the Kimia II operation



Notes: The first two figures (upper-level) show the proportion of villages controlled by the FDLR and the Congolese Army between 2002 and 2012. I split the sample into villages targeted by the Kimia II operations (left) and villages that were not targeted (right). Kimia II targeted all FDLR villages. I define a *Treated* village as to whether it was under the control of the FDLR in 2008. The proportion of these villages is indicated on the left axis. The red (dark) line, marked with squares, indicates the proportion of villages controlled by the Congolese Army; the grey line, marked with diamonds, indicates the proportion of villages under the control of the Congolese army. In the bottom figures, I use the same classification (left, right) to show the proportion of villages pillaged by the FDLR. The proportion is on the left axis.

Chapter 3

On the Impact of the State: Contract Enforceability and Ethnic Institutions in Eastern Congo

Raúl Sánchez de la Sierra

3.1 Introduction

What is the impact of the state on economic exchange? Absent states, societies may develop group-based mechanisms to generate trade when commitment problems prevail (Greif, 1993). However, groups are often fragmented, resulting potentially in large inefficiencies. As a monopoly of violence, the state can enforce contracts, and thus induce trade that would otherwise not occur.

In this paper, I provide evidence of the impact of expanding the state system of formal contract enforcement. To do so, I consider the existing social groups. I create a home delivery sector with commitment problems that replicate the holdup problem that is frequent in trade (Williamson, 1983), in Eastern Democratic Republic of Congo. Taking as given the existing social divisions, I deploy traders of different groups to offer trades to households of different salient groups. I randomly assign access to state-backed contracts to a subset of the sales offered. In state-backed contracts, drafted by a local lawyer and certified by the Provincial Administration, the trader who promises delivery in the future exposes himself to the existing sanctions applicable by the Congolese legal system in the case that he does not deliver as promised. I find that social groups (shared ethnicity) and state-contracts increase trade in the presence of commitment problems. Furthermore, using randomization of the time in which the trader will deliver the good offered (immediate or future delivery), I find that contracts increase trade by improving the expectations about the behavior of the agent - the likelihood that the trader will deliver the good if he promised it for the future. But are contracts really enforceable, or is this result driven by, for instance, signaling effects? I go a step further, and using a design à-la Karlan and Zinman (2009), I demonstrate that, even in one of the world's weakest states, contracts improve the expectations about the behavior of the agent because they are effective at changing his incentives: they are enforceable. How-

ever, contracts discipline incentives only if the claimant belongs to a group that has access to the state. When contracts are used to protect claimants of marginalized minorities, they do not decrease the incentives to cheat on them.

This paper's findings advance the existing literature by testing a solution to the welfare losses associated with social divisions and proposing a new explanation for why social divisions affect trade. There is a large literature documenting how individuals and groups solve commitment problems (Alesina, Baqir, and Easterly, 1999, Greif, 1993, Habyarimana, Humphreys, Posner, and Weinstein, 2007, Hjort, 2013, Miguel and Gugerty, 2005). However, this literature focuses on analyzing how groups solve commitment problems, and how they are unable to do so across groups, but does little to explore how commitment problems across groups can be addressed by expanding access to the state, and how this might affect the group mechanisms.

A first mechanism stressed by the literature is reputation and monitoring. Greif (1993) describes how the threat of collective punishment and concern with reputation sustained trust and trade among Maghrebi traders in XI^{th} century Mediterranean trade. Related, McMillan and Woodruff (1999) document the extent to which past relations predict current supply of risky trade credit among firms in Vietnam. If repeated interaction within groups can sustain cooperation, distance between group members can unravel this mechanism (Dixit, 2003). Distance among group members could be especially large when groups are fragmented into separate networks, for instance along ethnic lines. The effect of heterogeneity on public goods provision has indeed been extensively documented in the literature: Alesina and Ferrara (2004) show, using data on US neighborhoods, that ethnic heterogeneity depresses public goods provision. Similarly, Miguel and Gugerty (2005) describe how participatory public goods provision in Kenyan villages decreases with ethnic diversity. They suggest and document that ethnic divisions undermine public goods provision due to the difficulty to

sanction defectors across groups. If cooperation and trust within groups is sustained by repeated interaction and group sanctions, state contracts should increase trade across groups by providing a commitment device based on the threat of legal sanctions. However, by providing an exit option, entry of third-party contract enforcement can reduce the threat of losing business opportunities in the future. With contracts available, past defectors are no more likely to defect. This can weaken group-based mechanisms of contract enforcement.

A second mechanism identified by the literature are preferences. If individuals' payoffs are allowed to be interdependent, group members may be more altruistic towards groups, and value more the payoff of group members. In the extreme, individuals may even value the dis-utility of out-group members in contexts of inter-group hostility. Alesina, Baqir, and Easterly (1999) argue that ethnically heterogeneous neighborhoods are less effective at providing public goods because group members have different preferences for public goods. Chen and Li (2009) draw on a large literature in social psychology (Tajfel and Turner, 1979) and find evidence in a laboratory setting that artificially created group identities generate strong in-group altruism. However, by signaling lack of trust, requesting to sign a contract could generate negative reciprocity, crowding-out the preference-based mechanism through social preferences.

A third mechanism documented in the literature is internalized group level norms and conventions. Fehr and Gaechter (1999) show that individuals are willing to forgo their own consumption in order to sanction members of their own group who have defected. However, in this framework, the introduction of incentives can potentially crowd-out social norms or information and thus pre-existing equilibria (Bénabou and Tirole, 2003, Bowles and Polania-Reyes, 2012, Gneezy and Rustichini, 2000). Thus, contracts could even decrease trade if the incentive effects are outweighed by crowding-out effects.

My findings contribute to these literature by demonstrating that a low-cost interven-

tion that provides access to state-backed contracts can increase trade in situations where commitment problems prevail across social groups. Furthermore, I am able to identify that state-backed contracts improve trade only by affecting trust (expectations that the seller will deliver), and that they are enforceable, even when the state is weak.

However, I go a step further and introduce and test a new mechanism: groups have different bargaining power that allows them to enforce contracts. In doing so I place this question back in a political economy context. Most scholarship on contracts and trade ignores that the state legal enforcement system can be influenced by certain groups. However, if the legal enforcement system is captured, providing access to state-backed contracts increases the bargaining power of the groups who have captured it. In this context, expanding access to state contract enforcement can worsen pre-existing imbalances. My findings provide support for this ethnic institution view. I find that state-backed contracts only protect from defection claimants belonging to groups which have captured the state. State-backed contracts are ineffective at decreasing cheating on members of marginalized groups.

The remainder of the paper is organized as follows. Section 3.2 presents the social and institutional context. Section 3.3 presents the experiment design. Section 3.4 introduces the behavioral predictions and Section 3.5 presents the results. Finally 3.6 provides a brief discussion of the experiment and Section 3.7 concludes.

3.2 Context

I implement the experiments in the Province of Sud-Kivu, Eastern Democratic Republic of the Congo (DRC). The DRC state collapsed in the 1990's, which left economic governance to informal networks. Ethnicity is a major factor of these informal networks, and has been

an important vector of mobilization and conflict.[1] Indeed, since the state collapse in the 1990's, a myriad of armed groups has emerged. Many of these groups use ethnic networks to mobilize recruits.[2]

The population of Sud Kivu is composed of the following ethnic groups, who speak different languages: Bashis, Bahavus, Balegas, Batembos, Bafuliros, Pygmies, and Rwandophone populations (mostly Banyamulengues and Banyarwandas). Bashis and Bahavus' languages (Shi and Havu) belong to the Shi-Havu family. They have 70% lexical similarity, and their speakers perceive each other as members of the same group.[3] Historically, they have been politically active in similar groups. Unsurprisingly, Bashis and Bahavus is not a salient social division. Balegas have good relationships with the Shi-Havus. Balegas are mostly present in the Territories of Mwenga and Shabunda. Batembos' language (Kitembo) also belongs to the Shi-Havu family. Batembos, however, are mostly found in remote areas of Sud-Kivu, mainly in Shabunda and Western parts of Kalehe (Bunyakiri). Batembos have been affected by conflict, there is therefore a large population of displaced Batembos in other villages of Sud Kivu. Historically, Batembo's grievances are opposed to Shi-Havus and they have been a major force in local rebellions known as Mayi-Mayi groups. The divide between Shi-Havus and Batembos is salient. While they recognize their distinct ethnic affiliation, they perceive each other as close neighbors culturally and historically.[4] Bafuliros originate mostly from Uvira. Like the Balegas, they consider the Shi-Havus as close to them and this is not a

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[1]See the following accounts of recent history: Clark (2002), Nest (2011), Nest, Grignon, and Kisangani (2011), Ngonzola-Ntalaja (2002), Stearns (2011), Vlassenroot and Huggins (2005), Vlassenroot and Raeymaekers (2004), Van Reybrouck (2008), Autesserre (2006), Autesserre (2007), Autesserre (2008), Verweijen (2013).

[2]This is the case of the AFDL, RCD, CNDP, M23 in particular, but also the FDLR and a large number of ethnic based self-defense groups.

[3]https://www.ethnologue.com/language/shr

[4]Surveyors in this project further report considering them as their "brothers".

salient division.

Rwandophones (language Kinyarwanda) are historically cattle herders from Rwanda who live in the highlands of the Kivus and belong to both Tutsi, Hutu, and Twa groups. In contrast with the other groups, Rwandophone populations are sharply marginalized and their affiliation is very salient. Rwandophones first recorded migration wave took place well before the colonial period, in the seventeenth century, fleeing taxes in Rwanda (today these are called Banyarwanda - the people of Rwanda). A second migration wave took place in 1880 (today these are called Banyamulengue - the people of the Mulengue mountain). The Belgian colonial administration orchestrated subsequent waves in the 1930's and 1940's, sending workers to coffee plantations in DRC. In the second half of the twentieth century, large numbers of Rwandophones fled conflicts in Rwanda to the Congo, culminating in the migration of 2 Million refugees in the 1994 Rwanda crisis. This continuous influx exacerbated the ethnic tensions with Rwandophones.

Historically, the Rwandophones have been discriminated against, and are a minority that tends to be excluded from the benefits provided by the state. They were a major mobilizing force in armed conflict in the region, and Congolese populations perceive the Rwandophone populations with hostility. While Banyarwandas are mostly Tutsi and Hutu, Banyamulengues are mostly Tutsi.[5]

In this paper, I first focus on the major divides: Shi-Havus and Batembos, Shi-Havus and Rwandophones, Batembos and Rwandophones. In the first part of the paper I refer to coethnic interactions those interactions between Shi-Havus, Regas, and Fuulirus. I refer to non-coethnic interactions those interactions between Shi-Havus and Rwandophones and Shi-

[5]See Stearns (2011), Ngonzola-Ntalaja (2002), and Newbury (1992) for accounts of current ethnic relations in Sud-Kivu and their historical foundations.

Havus and Batembos.[6] I abuse the term ethnicity, since I focus on existing salient divides and do not aim to provide an abstract effect of ethnicity removed from its social context. This is consistent with the aim of this paper. I take the social structure as given, focus on the major divisions, and examine the impact of contracts. As I show in sections below, this classification is consistent with the empirical results.

3.3 Experimental Design and Procedures

This section describes the field experiment design.

3.3.1 The home delivery sector

In order to shape the design of a real economic activity, I designed and managed a business of home delivery of household basic consumption goods. After three months of qualitative fieldwork, I hired a group of ten young men to deliver soaps in randomly selected households in 140 villages in Sud Kivu (and cell phone minutes in the experiment extension). Soaps are particularly attractive because they are highly demanded. The focus on a real world economic activity is important, because the context in artificial environments can alter behavior in unknown ways.[7] There are four important features of this economic activity.

First, in the baseline experiment, the sales were implemented on *credit* (sale on credit, henceforth). Traders visit randomly selected households and offer 5 soaps at a discount, promising to deliver the soaps in the new future (two days after purchase). However, traders require immediate payment. The household can choose to accept or reject the offer. If he

[6]Since I am unable to observe interactions between Batembos and Rwandophones.

[7]Haley K. J. (2005) show that cooperation in anonymous games was significantly affected by the addition of payoff-irrelevant cues (earplugs and images of eyes).

accepts, he must pay immediately. The trader then may deliver the soaps as he promised. While traders' defection does not affect the main outcome (whether the household accepts) this could affect the viability of the project. I thus designed the project to prevent traders' defection. Based on a monitoring system I developed, I record no occurrence of deception by traders. I provide more information on traders' incentives in this section. An important fact is that households did not know if traders would deliver the soaps with certainty. Therefore, this design allows me to introduce a commitment problem: the holdup problem (Greif, 1993, Williamson, 1983). Absent informal or formal mechanisms that provide incentives to the trader to deliver the soaps, the household would proceed by backward induction and should refuse the offer. He would anticipate that it is in the trader's best interest not to deliver the soap. Since the exchange is socially desirable, this commitment problem can lead to inefficiencies if left unsolved. In the second experiment, I added sales on the spot. In sales on the spot, the trader offers to provide the soaps at the same time as he receives the payment. In the third experiment, I added sales on *debt*. In sales on *debt*, the trader first provided the good, requiring the household to pay by cellphone within a few days. I describe each experiment in detail in Section 3.3.2.

Second, soaps (and cell phone minutes) were sold at a discount. Traders offered 5 soaps for the price of 3 in each household (and so for cell phone minutes) in the last experiment.

Third, after the offer was introduced, households were invited to answer questions of an exit survey. The survey contains information on demographics, ethnicity, and measures of perceptions.

Fourth, traders selected households randomly within every village. In their first day in the village, traders established a village census with the assistance of the village authorities, aimed for a separate research project. Using this census, traders selected households,

as well as their treatment assignment, randomly.[8] Households main economic activities is agriculture and mining. The total sample of the main two experiments contains 2,684 households composed of 20 ethnic groups. The ethnic composition of the selected households is as follows: Regas (n=1,208), Shi-Havus (n=993), Batembos (n=188), Rwandophones (n=161), Other groups (n=134.2). To minimize variation induced by gender bias, all respondents were randomly selected males within the selected households. Another 2,000 households were part of a third experiment, in which traders offered cell phone credit.

I now turn to the traders. I hired 10 traders of various ethnicities for the first two experiments, and 20 for the third experiment. Traders were recruited and trained prior to implementation.[9] The traders in the first experiments are mostly Shi-Havus. In the last experiment, I added an equal number of Rwandophones - resulting in a team composed of ten non-Rwandophones and ten-Rwandophones.[10] When I refer to the coethnicity effect in Experiments I and II, thus, I identify the household ethnic group effect. I designed the economic activity so that traders are residual claimants. Traders' incentives are thus a central element of the design.

Traders may have incentives to cheat. For instance, traders had incentives to defect on their promises of delivery. To prevent trader defection, I implemented the following measures. First, traders provided households with a project cell phone number, and instructions for complaints. Furthermore, to allow monitoring, traders collected their cell phone number at the exit survey. Traders were informed that the supervisor would contact a random

[8]For each village, a list of randomly selected numbers was previously drawn using a statistical package, conditional on village size. Traders then selected the households based on these numbers. Each household was then allocated to a treatment status randomly.

[9]Traders in the first experiment were also hired as surveyors for a related projects in the villages in which they sold soaps. See Sánchez de la Sierra (2014a).

[10]In most of the analysis of the first two experiments, I focus on Shi-Havu traders to minimize biases arising from individual effects among Batembos, Regas and Fulirus. The results are qualitatively identical.

sample of respondents to check whether the trade was implemented as planned.[11] Furthermore, the supervisor planned to visit randomly selected villages and verify the traders' activity.[12] More importantly, traders work in a long-term basis for various research projects for Columbia University and attribute a strong value of implementing their work without cheating. Despite this monitoring system, I designed the project so that the research was incentive compatible.

In what follows, I describe the strategies to make the design incentive compatible for traders. A first concern is that traders may not have incentives to sell the good, decreasing the realism of the observed transactions. To provide incentives to the traders to sell the soaps, I made traders residual claimants on all sales, in addition to providing them with a fixed daily payment.[13] Traders had therefore real profits to make from successfully selling the soaps, and they considered the revenues from the sales to be a non-negligible part of their income.[14] A second concern is that traders, in an attempt to increase profits, could be tempted to violate the random allocation of households and extract higher surplus from richer households. However, a village contains hundreds of households, which makes discovering the wealth distribution in the village a costly exercise.[15] Furthermore, I informed

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[11]This threat was only implemented in experiment III, but was present in all three versions. I recorded no instance of fraud or cheating by traders, among all households in which the Supervisor implemented the verification.

[12]Although the project functioned with this threat, the project Supervisor ultimately never implemented this back-checking in rural areas. However, every team had a team Supervisor that monitored the activity of the other team in real time.

[13]The fixed component was set after discussions with traders and surveyors, so that their profit, adjusted for uncertainty, was equal to the market wage they would otherwise obtain as surveyors, or as school teachers.

[14]In experiments I, II and III, if all trades were successful during the selling day, they would generate 3USD of profits per trader.

[15]See Sánchez de la Sierra (2014a) for a description of how even armed groups settled in the village struggle to know the wealth distribution.

traders during training that the researcher could use statistical techniques to verify violations of implementation, such as randomization.[16] A third concern is that, traders may may be tempted to accept payments below the price set by the research project, hence extracting strictly positive surplus from households that would otherwise have refused a purchase at the price set by the research. To avoid this, I imposed "supply marginal costs" on traders. For each soap that traders claimed to have sold, they were required to pay a cost, lower than the sale price. The supervisor verified the stock of soaps and traders had thus to pay for the missing soaps at the cost established by the project. This reduced the margin that traders would get by selling at prices below the price set by the research, and thus the range of potential households in which cutting prices would be a viable strategy to increase their profits at the cost of the quality of the research. Based on the monitoring structure just described, I recorded no downward violations of this price policy. Finally, traders may be tempted to sell above the price set by the research project, and extract additional surplus from the visited households. I implemented a number of measures to prevent this from affecting the quality of the research. To start with, traders were allowed to sell above the price set by the project if a particular household would accept to pay above the price. This would not have affected the main outcome - whether the household accepts the deal - since a household that accepts to pay at their potential bargained price would also accept to pay at the lower price set by the research. However, the option to sell above the price set by the research could induce traders to limit sales in households unable to pay high prices and choose to sell only in rich households instead, in which they would extract a disproportionately higher price. To reduce risks that traders would choose reallocate soaps to households that offered high prices, I endowed traders with soaps for all households that they had to visit.

[16]Traders report that they believed violations of random sampling would be identified was very high.

3.3.2 The three experiments

In this section I describe the three versions of the field experiment.

Experiment I

Experiment I is the baseline field experiment. Figure 3.1 provides a graphical representation of the experiment setup. Each trader is allocated to randomly selected households and offers the soaps at a discount. In exchange for immediate payment, he promises to deliver the soaps in the next few days, after he is done taking the orders. Households decide to accept or reject the deal. A few days after, the traders deliver the soaps, although households are uncertain that they will. While the sale is made on a randomly selected individual of the household, in what follows, I refer to them as households. I randomly assign two treatments.

Within the ethnic families of households, I randomize traders to households. Traders identified the ethnicity of the households during the village census.[17] This allows me to create sales in which the trader and the household are coethnics, and sales in which they are not co-ethnics.

I also randomly allocated access to state-backed contracts, stratifying by trader and ethnicity of the allocated household. I randomized both treatments at the household level within village and within trader. The original contract was drafted by a local lawyer and was backed and stamped by the Ministry of Interior of Sud Kivu. Traders carried copies of the original contract. The contract stipulates the following:

"I, the undersigned... , recognize to have received... Congolese Francs from... in anticipation of ... soaps of type... , for a value of 200 Congolese Francs per unit. I hereby commit

[17]Village authorities were informed that in order to better implement the research project, surveyors needed to know which language the households were most familiar with. Since language is almost identical to ethnicity, this provided an ethnic based census.

to deliver... soaps of the type... and of value 200 Congolese Francs per unit to... in the interval of TWO days at most. I am ready to bring this contract, if necessary, to a legal representative. I recognize that in case of no delivery, I am exposed to the prosecutions and sanctions that the Congolese law considers for these cases. Done in... . Date... . Signature of debtor... Signature of creditor... Signature of witness... ."

Figure 3.4 shows the original contract. Based on preparatory fieldwork, the stamp was important to signal the legal validity of the contract. In what follows I refer to state-backed contracts and contracts interchangeably. The factorial design is presented in Table 3.1.

This design allows me to identify the marginal effect of contracts among coethnics and non-coethnics interactions on trade, in the presence of commitment problems. Furthermore, it allows me to identify how coethnicity affects trade. However if contracts or coethnicity increase trade, even upon finding that contracts improve trust on the trader, one may worry they do so through other channels than by affecting expectations of delivery. In particular, it could be that households value more trading with coethnics, because of social preferences; or there may be a norm of cooperation among coethnics. In a similar fashion, households may prefer to trade with traders who have access to contracts because access to contracts may signal the type of the trader, and this may affect the private valuation that the household derives from accepting the transaction. To rule out these interpretations, and separate the effect of contracts and coethnicity on trust, I designed Experiment II.

Experiment II

In Experiment I, traders always offered the sale on credit. To disentangle whether contracts and coethncity increase households' trust, I introduced sales in which traders provided the soaps immediately during payment in a random sample of households (sales on the spot, henceforth). This allows me to identify the effect of contracts and coethnicity for sales with

and without commitment problems. Figure 3.1 provides a graphical representation of the experiment setup. As in Experiment I, I randomized at the household level within village and within trader. The design in Experiment II is a factorial design as shown in Table 3.2. If contracts and coethnicity improve expectations about trader delivery in the sale on credit, then their marginal effect on trade should be significantly larger for sale on credit than for sale on the spot.

Experiments I and II allow me to estimate the marginal effect of contracts and coethnicity and whether this effect is larger for sales on credit. Such a result would suggest they are effective at improving trust (expectations of trader delivery). But are contracts enforceable? One may indeed worry that even if contracts improve trust, they are not enforceable. Similarly, coethnicity may not produce credible sanctions upon traders' defection. In particular, since traders are unknown to the households, households may just update their beliefs about the type of trader upon observing a contract. This belief effect can confound the incentive effect of contracts and coethnicity. For instance households could have better priors about traders who demonstrate to have access to contracts, even when contracts are not enforceable. This would increase trust in traders who demonstrate to have access to a contract, through a signaling channel.[18] To identify whether contracts and coethnicity do reduce the incentives to defect, I designed Experiment III. Experiment III allows me to capture the causal effect of signing a contract on actual cheating.

Experiment III

Experiments I and II allow to identify whether contracts, or coethnicity, are effective at increasing trade. To identify whether contracts and coethnicity work through signaling

[18]This is plausible if trader types select into occupations of different proximity with the state institutions, and if proximity to the state is associated with other factors that motivate good reputation.

(households believe that traders with contracts and coethnics are different types) or incentives (contracts and coethnicity are effective at disciplining traders' incentives), Experiment III focuses on cheating behavior in a similar setting. Using a design à-la Karlan and Zinman (2009) I am able to causally identify the impact of state-backed contracts, and coethnicity, on cheating behavior.

I next describe the experiment design. Figure 3.2 provides a graphical representation of the experiment setup. A team of twenty traders (ten Rwandophones, ten non-Rwandophones) visits 2,000 households in a semi-urban neighborhood (Nguba) of the town where the traders of Experiments I and II were selected. They offer a household consumption good. Traders deliver cell phone minutes, which is in high demand in this population.[19] I randomly allocated traders of the two groups to households. In each household traders offered a sale at a discount (cell phone minutes), providing the good on the spot, but required that the household pays by cellphone within a few days (henceforth, sales on *debt*). To identify the incentive effects of contracts, I used a design à-la Karlan and Zinman (2009). If I required only some households to sign a contract, the pool of households who accept the deal with a contract condition would differ from the pool of households who refuse the deal without a contract condition. Thus, to separate these selection effects from incentive effects, traders offered the sale with the condition to sign a contract to all households. However, upon noting whether the household accepted or refused, the traders proceeded to ask the household signature in the contract only to randomly selected households. In the rest of households, randomly selected, the traders communicated that the stock of contracts available did not allow them to ask the contract to be signed in this household. The differences

[19]In this experiment, I changed the household good from soaps to cell phone minutes because of logistical limitations. Experiment III is implemented in semi-urban neighborhood, because there is a larger concentration of households, and because Rwandophone traders cannot travel across villages for security reasons.

in payment rates of households in which traders ultimately required to sign the contract and households in which they did not identifies the incentive effect of contracts.

There are three important features to the design of Experiment III.

First, traders implement the sales on the household's *debt*: traders offer 5 cell phone cards (with a market value of .6 USD each) at a discount, but require the households to pay by cell phone within two days. In the first step, the trader presents the offer. The household can then choose to accept or reject the deal. In the second step, the households that accepted the deal may choose to implement the payment by cell phone or to defect. This design allows me to introduce a commitment problem. Absent informal or formal mechanisms that provide incentives to the household to pay, it is in the household's best interest not to pay. Second, after the offer was introduced, traders implemented an exit survey. Third, within street of each neighborhood, households were randomly assigned to different treatments. I allocated teams to randomly selected streets (henceforth avenues), which were easy to identify. I randomly allocated households (and treatments) in each avenue to each trader, using a statistical package prior to implementation.

To identify the impact of contracts on households' incentives, I executed the request to sign the contract only in a random sample of households. The contract was drafted by the same lawyer as in Experiments I and II and was also backed and stamped by the Ministry of the Interior. The text of the contract reads as follows:

" I, the undersigned... , recognize to have received ... cell phone units of the company ... from ... , for a value of 500 Congolese Francs per unit. I hereby commit to pay ... in exchange of these cell phone units to ... in the interval of TWO days at most. I am ready to bring this contract, if necessary, to a legal representative. I recognize that in case of no payment, I am exposed to the prosecutions and sanctions that the Congolese law considers for these cases. Done in... . Date ... . Signature of debtor... Signature of creditor... Signature

of witness... ."

In what follows, I describe the steps of the transaction:

Step 1: In all households, traders offer the deal with the request that households sign a contract, in which households commit to pay by cellphone in a few days. Households can then accept ($B_{t=1} = 1$) or reject ($B_{t=1} = 0$) the deal, and the trader coded the response of the households.

Step 2a: After noting the answer, in a random sample of households traders withdraw the requirement to sign a contract. Since they had initially requested a contract, their script specified that they did not have a sufficient number of contracts to give to the household, and that the deal would now hence require no contract.[20] This creates two random samples: those who had accepted the offer with the contract and sign the contract (F=1), and those that had accepted the offer with the contract and do not sign the contract (F=0).

Step 2b: Now that traders have withdrawn the contract requirement for a subset of customers, they allow the households who refused the offer initially ($B_{t=1} = 0$) to reconsider their decision. This step allows to identify screening effects, and leaves Step2a unaffected. To analyze the incentive effect of contracts, I condition on the households who accepted the offer initially ($B_{t=1} = 1$) and compare their payment rates. To analyze the screening effect of contracts, I condition on the households who accepted the offer after Step 2a ($B_{t=2} = 2$) and that were not requested to sign a contract (F=0), and compare payment rates by who those

[20] "Forgiving" the requirement to sign a contract could induce reciprocity, and bias behavior. To avoid this, the script specified that traders did not have sufficient contracts to pursue with the signature of the contract. Since the traders had already committed to sell the good, this reduces concerns about the information that this decision can convey and the biases it can generate.

that had initially accepted ($B_{t=1} = 1$) to payment rates by those that had not ($B_{t=1} = 0$).

Step 3: Traders provide the cell phone cards on the spot to the households who accept the deal. Traders provide instructions to the household on how to execute payment. These explanations are also written in a payment coupon that they give to each household. I present the payment coupon in Figure 3.3.

In addition to sales on *debt*, I implemented sales on the spot for all households prior to the debt sale in order to isolate ethnic preferences from expectations about future behavior.

3.4 Behavioral predictions

Allow the household's utility to depend on the monetary payoff of the trader, with a, separable, social preference parameter à-la Charness and Rabin (2002). I assume separability of consumption and social preferences for clarity, but this assumption does not affect the main predictions. This social preference weight may indicate that the household values the trader's payoff, but it could also indicate dis-utility from violating a social norm of surplus-sharing with the trader. Let $E \in R$, $F \in \{0; 1\}$, $C \in \{0; 1\}$ indicate respectively the degree of ethnic proximity, whether the transaction is formalized by a contract, and whether the sale is on credit.

3.4.1 Experiments I and II:

In this section, I derive testable predictions for Experiments I and II.

Sales on the spot:

With on the spot sales, the buyers utility is:

$$U_B = B(v - \wp + \lambda(E, F, C)(\wp - \wp_a))$$

where $B = \{0; 1\}$ is 1 if he buys the good and 0 otherwise. The term v is the private valuation of acquiring the good and $\wp$ is the sale price. The term $\lambda(E, F, C)$ is the weight that the buyer assigns to the monetary payoff of the trader. I allow the social preference weight to depend on the trader's ethnic proximity, E, whether the sale is formalized, F, and whether the sale is on credit, C. Finally, $\wp_a$ is the alternative monetary revenue to the trader in case the household rejects the purchase. I assume $\wp > \wp_a$, so that the trader makes positive profit if he sales the good. I normalize the household utility of not purchasing to 0. The marginal effect of ethnic proximity when a sale is not formalized and on the spot is given by:

$$\begin{aligned} \frac{\partial U_B}{\partial E}|_{F=0,C=0} &= \frac{\partial U_B}{\partial \lambda(E, F, C)} \frac{\partial \lambda(E, F, C)}{\partial E}|_{F=0,C=0} \\ &= (\wp - \wp_a) \frac{\partial \lambda(E, F, C)}{\partial E}|_{F=0,C=0} \end{aligned}$$

When the buyer has ethnic biased social preferences, $\frac{\partial \lambda(E,F,C)}{\partial E}|_{F=0,C=0} > 0$, coethnicity in sales on the spot increases the buyer valuation of the transaction - since the trader is making positive profit. The marginal effect of contracts when a sale is with a non-coethnic and on the spot is:

$$\begin{aligned} \frac{\partial U_B}{\partial F}|_{E=0,C=0} &= \frac{\partial U_B}{\partial \lambda(E, F, C)} \frac{\partial \lambda(E, F, C)}{\partial F}|_{E=0,C=0} \\ &= (\wp - \wp_a) \frac{\partial \lambda(E, F, C)}{\partial F}|_{E=0,C=0} \end{aligned}$$

If the household has a preference for the payoff of traders who formalize transactions, $\frac{\partial \lambda(E,F,C)}{\partial F}|_{E=0,C=0} > 0$, the use of contracts in sales on the spot increases the buyer's valuation of the purchase. If contracts crowd out the existing ethnic preferences, $-\frac{\partial \lambda(E,F,C)}{\partial F}|_{E=1,C=0} > -\frac{\partial \lambda(E,F,C)}{\partial F}|_{E=0,C=0}$. This effect would be larger among coethnics if contracts signal deviation from in-groups norms of behavior, which are weaker or absent across groups.

Sales on credit

When sales are made on credit, households discount the future value of consumption by their subjective probability that the trader will deliver the good $\eta(E,F,C)$ and by their discount factor, $\beta(E,F,C)$. I have allowed both the subjective probability of delivery and the discount factor to depend on the ethnic proximity and formalization. For instance, households may be more impatient with delivery lags by non-coethnics traders and when the trader signed a contract; in addition, households may assign a different probability of delivery to coethnics and to sales that were formalized with a contract. In sales on the spot, there is no uncertainty of whether the trader will provide the good, $\eta(E,F,C=0)=1,\ \forall E,F$ and since the delivery is immediate $\beta(E,F,C=0)=1,\ \forall E,F$. In what follows, for both functions, I omit the argument C, and omit them altogether whenever the sale is on the spot, $C=0$ (they are equal to 1). The general formulation of the household's utility is:

$$U_B = B(\beta(E,F)\eta(E,F,C)\,v - \wp + \lambda(E,F,C)(\wp - \wp_a))$$

The effect of credit on the buyer's utility:

$$U_B(E,F,C=1) - U_B(E,F,C=0) = -v(1-\beta(E,F)\eta(E,F))$$

Unambiguously, introducing sale on credit reduces the expected payoff households derive from the deal. This is a function of both the discount factor and the subjective probability that the trader will deliver the good as promised.

I now compare this quantity for coethnics and non-coethnics, and for formalized sales and not formalized sales. If trust is higher among coethnics, ie, if $\eta(E=1,F) > \eta(E=0,F)$, then it must be that, whenever $v > 0$:

$$\begin{aligned}
\Delta^C(E,F) &= \left| \frac{\partial U_B}{\partial C}|_{E=0,F=0} - \frac{\partial U_B}{\partial C}|_{E=1,F=0} \right| \\
&= v\left(\beta(E=1,F)\eta(E=1,F) - \beta(E=0,F)\eta(E=0,F)\right) \\
&> 0
\end{aligned}$$

The marginal effect of the credit variation decreases the utility of purchasing less among coethnics than among non-coethnics. Assuming the discount factor is independent on the ethnic proximity of the trader, E, and on formalization, F, this is equivalent to $\eta(E=1,F=0) > \eta(E=0,F=0)$. Comparing the effect of ethnicity among sales on the spot to the effect of ethnicity among sales on credit, thus, allows to isolate the effect of ethnic ties on preferences from its effect on trust, $\eta(E,F)$.

If contracts are effective at improving expectations about trader's delivery (increasing η), then it must be that $\eta(E,F=1) > \eta(E,F=0),\ \forall E$. Thus:

$$\begin{aligned}
\Delta(E) = \Delta^C(E,F=1) - \Delta^C(E,F=0) &= v\beta(\eta(E=0,F=0) - \eta(E=0,F=1)) \\
&< 0
\end{aligned}$$

Furthermore $\Delta(E=0) > \Delta(E=1)$ if and only if:

$$\eta(E=0,F=0) - \eta(E=0,F=1) - (\eta(E=1,F=0) - \eta(E=1,F=1)) < 0$$

This will be the case if $\eta(E=1,F=0)$ is already 1, for instance, or whenever a larger mass of households believe that $\eta(E=1,F=0)=1$. In words, if this condition is true, among coethnics contracts affect *less* the impact of whether the sale is implemented on credit.

If the effect of ethnicity on trust is larger when contracts are absent, then it must be that $\frac{\partial \Delta(E)}{\partial E} > 0$. The effect of contracts on the decrease in trust provoked by making transactions on credit would thus be smaller among co-ethnics.

Econometric strategy for Experiments I and II

The main outcome is whether household i visited by trader j buys the soaps, $Trade_{i,j} \in \{0;1\}$. I refer to this event as "trade occurs". To identify the effect of individual treatments, I compare trading rates in each cell of Tables 3.1 and 3.2. While the traders offer prices below market prices, I expect that households will reject the offer for multiple reasons, including liquidity constraints. Since treatments are randomized, the unobservables affecting rejection are orthogonal to the treatments, so OLS is an unbiased estimate of the treatment effect.

I estimate the average treatment effect using a linear probability model in all regressions below. Let $E_{i,j} \in \{0;1\}$ denote whether the interaction is coethnic, $F_{i,j} \in \{0;1\}$ whether formal contracts are used, and $C_{i,j} \in \{0;1\}$ whether the sale is made on credit, all of which are randomized at the dyad level. In addition, let T_v be village fixed effects. The regression specification is:

$$Trade_{i,j} = a_0 + a_1 E_{i,j} + a_2 F_{i,j} + a_3 C_{i,j} + a_4 E_{i,j} F_{i,j} + a_5 E_{i,j} C_{i,j} + a_6 F_{i,j} C_{i,j} + a_7 E_{i,j} F_{i,j} C_{i,j} + T_v + e_{i,j}$$

Table 3.3 presents the testable implications.

3.4.2 Experiment III

In this section, I derive testable predictions for Experiment III.

Analytical framework

In Experiment III, the seller first provides the good (cell phone minutes), and the buyer can choose whether to buy it, as well as whether to implement the payment. The buyer's utility is therefore now indexed by $B = \{0; 1\}$ as previously and $P = \{0; 1\}$. The choice variable P indicates whether the buyer implements the payment. Finally $F = \{0; 1\}$ indicates whether a contract was used to formalize the transaction and $L \in R$ denotes the expected loss of cheating stemming from the legal system. The buyer's utility is now:

$$u_{BP}|\theta, v = B(v - P\wp - (1 - P)(\theta + FL))$$

I normalize utility to 0 if he does not purchase the good. If he purchases the good, $B = 1$, he derives v from the consumption of the good. If he has chosen $B = 1$, he then chooses whether to pay, $P = 1$, or not, $P = 0$. If he pays, he incurs the monetary loss $\wp$. However, if he does not pay, he can save $\wp$, but this comes at a cost: θ indicates the cost incurred by the buyer upon non-payment. This cost could be internalized, such as utility losses arising from guilt or from social preferences as in Section 3.4. It could also be generated by extrinsic motivations in his social context: he could suffer shame, or he could suffer other sorts of social sanctions. Whichever is the source of the dis-utility from not payment, θ indicates the losses incurred upon cheating ($P = 0$) if he chooses $B = 1$. Table 3.4 provides a mapping of the parameter space onto the two-dimensional optimal choice of the household. There are 3 possible strategies: $(B = 1, P = 1)$, $(B = 1, P = 0)$, $(B = 0, P = .)$, divided in four cells.

Table 3.4 maps the parameter space onto the strategy set. Figure 3.5 provides a graphical representation.

The terms $\alpha_i \in \{1, 2, 3, 4\}$ denote the mass of agents in each of the relevant cells. I consider the following partition of the parameter space. In each set, buyers' strategies are identical, with and without contracts. For instance, while α_3 and α_4 display the same behavior in the absence of contracts, α_3 values the good enough that he would be willing to accept a purchase in which he is required to sign an enforceable contract. In contrast, α_4 would not not. Finally, I refer to each set according to two dimensions. First, if the cost of not paying θ is sufficiently high (this depends on the individual v) I refer to them as honest. This reflects that they have a *relatively* high θ given v so that they would never defect (forgo payment). Second, if the valuation of the good purchased is higher than the monetary cost, I refer to them as peach (lemons otherwise). The peach/lemon terminology captures that trade is only socially optimal when the customer has a sufficiently high private valuation, which is unobserved. It does not capture whether customers are of high quality, in contrast to the standard adverse selection literature (Akerlof, 1970). I use the peach/lemon terminology from the lens of a social planner deciding where to implement trades.

Assume first that the state contract is sufficient to motivate payment: $\wp < \theta + L$ for all households. In that case, α_2 and α_4 reject the offer, since they don't value the good enough to pay for it. Once customers have accepted or rejected the offer, the trader announces in a random sample of households that he cannot offer a contract because he does not have enough contracts ($F = 0$). The remaining are still required to sign the contract. Among the households in which the contract requirement was withdrawn randomly, α_3 will prefer to avoid payment. Table 3.5 presents the selection and incentive effects.

Testable implications in Experiment III

I now describe the main testable implications of Experiment III. I allow v and θ to depend on the ethnicity of the trader (Rwandophone or non-Rwandophone) and whether a contract was signed. Therefore, I consider the functions $v(E, F)$ and $\theta(E, F)$. These determine the mass of customers in each cell: $\alpha_1(E, F)$, $\alpha_2(E, F)$, $\alpha_3(E, F)$, $\alpha_4(E, F)$.

First, within each ethnic group of traders (Rwandophones and Shi-Havus), contracts have incentive effects if $\alpha_3(E) > 0$. Indeed $\alpha_3(E)$ is the mass of customers who would like to forgo payment but pay if they were requested to sign a contract. I allow $\theta(E, F)$ to depend on F, so that social norms/sanctions can be crowded out or complemented by contracts. I also allow the expected judiciary sanctions to depend on the ethnicity of the trader $L(E)$. This is important if Rwandophone traders have different ability to enforce contracts than non-Rwandophone traders. Once I allow the expected legal sanctions, L, to depend on the ethnicity of the trader, E, the effect of ethnicity, and the comparison of the marginal effects of contracts across ethnicities, are ambiguous. While coethnics may share norms of no defection, which would decrease the effect of contracts among co-ethnics, non-coethnics (Rwandophones) may have weaker access to the judiciary, decreasing the effect of the contract when the trader is Rwandophone.

Second, contracts have screening effects. In particular, the quality of the pool of customers that accepts the offer when the contract is not requested, $\alpha_1(E) + \alpha_3(E) + \alpha_4(E)$ contains a larger fraction of customers who would not pay than the pool who accepted when the contract is requested, $\alpha_1(E) + \alpha_3(E)$. Indeed $\alpha_4(E)$ is the mass of customers who value the good and would like to forgo payment but for whom purchasing is not worth if they were requested to sign a contract. If contracts have a screening effect, it must be that, conditioning on the sample of households in which the contract requirement was withdrawn randomly -and hence among which the α_i's are present in the same proportions as in the

main population-, payment rates are higher among those who initially accepted the offer when the contract was requested $\alpha_1(E) + \alpha_3(E)$, than among those who had rejected, but accepted once they found out that the contract was not requested, $\alpha_4(E)$.

Third, the pool of households who accept the deal is likely to be different for Rwandophone traders and non-Rwandophone traders. Indeed, the screening effect is likely to be different across ethnic groups. I can identify the *incentive* effects of contracts within each group. However, *selection* of households into $B = 1$ confounds any comparison of the effect of contracts across groups. To see this, note that the choice of accepting or rejecting the sale is affected by $\theta(E, F)$, $v(E, F)$, and $L(E)$. Ethnicity of the trader has thus an ambiguous effect on the quality of the pool of households who accept the sale. These effects affect the average θ of the customers who accept the sale in ambiguous directions.

To separate pure social preference channels $\left(\frac{\partial v(E,F)}{\partial E} < 0\right)$ from changes due to other parameters $\left(\frac{\partial \theta(E,F)}{\partial E} < 0\right)$, I use the sub-sample of sales on the spot. In sales on the spot, the decision to buy or not, $B = 1$ or $B = 0$, is only affected by $v(E)$ and $\wp$ since there are no contracts involved. Therefore, if $\frac{\partial v(E,F)}{\partial E} < 0$, the mass of customers who accept the sale when the trader is Rwandophone will be lower than when the trader is not Rwandophone, in sales on the spot. In the absence of ethnic preference bias, but if Rwandophones are less able to activate legal sanctions among the main ethnic groups, Rwandophone traders may be associated with lower θ (group disciplining technology) and lower L (group access to the judiciary). For constant L, lower θ increases the screening effect of contracts - contracts lead to a larger increase in pool quality, since in the absence of contracts, there are less informal mechanisms. For constant θ, lower L decreases the screening effect of contracts - contracts lead to a smaller increase in pool quality, since they are less likely to be enforced. Therefore, in the absence of evidence suggesting ethnic preference bias, if the increase in customer quality resulting from the request to sign a contract is smaller for Rwandophone traders, it

must be that L is smaller for Rwandophones - since θ can only be smaller for Rwandophone traders and affects the quality of the pool of households who accept the sale in the opposite direction.

Econometric strategy for Experiment III

Let $B_{i,j} \in \{0,1\}$ indicate whether the customer accepts to buy. For sales on debt, $B(t = 1)_{i,j}$ indicates whether the customer accepted the sale initially (when signing the contract was requested) and $B(t = 2)_{i,j}$ indicates whether the customer accepted the sale after the randomization was implemented and they were asked to reconsider their choice. Let $P_{i,j} \in \{0,1\}$ indicate whether the customer pays for the transaction. This is only observed if the customer accepted the purchase. Let $R_j \in \{0,1\}$ denote whether the trader is Rwandophone, $F_{i,j} \in \{0,1\}$ whether formal contracts are used, and $D_{i,j} \in \{0,1\}$ whether the sale is made on debt. In sale on debt, the trader provides the good immediately, and asks the customer to implement payment in the future by cell phone. In addition, let $T_a \in \{0,1\}$ be avenue fixed effects and $T_e \in \{0,1\}$ denote team fixed effects.[21] Trader teams of two are randomly assigned to avenues. Within each avenue, traders are randomly assigned to households. Finally the contract treatment is randomly assigned within avenue for each trader.

To capture the incentive effects of contracts, I run the following linear probability model in the subsample of sales on debt:

$$P_{i,j} = c_0 + c_1 F_{i,j} + c_2 R_{i,j} + c_3 F_{i,j} R_{i,j} + A_a + T_e + e_{i,j}$$

[21] To differentiate it from the time index, t, team fixed effects are indexed by e, which stands for team in French (equipe).

where I condition the sample on $D_{i,j} = 1$ and $B(t = 1)_{i,j} = 1$.

To capture ethnic preference bias, I run the following linear probability model in the subsample of sales on the spot:

$$B_{i,j} = b_0 + b_1 R_{i,j} + A_a + T_e + e_{i,j}$$

where I condition the sample on $D_{i,j} = 0$.

Let $S_{i,j} \in \{0, 1\}$ indicate whether the customer was initially screened by the request to sign a contract ($S_{i,j} = B(t = 1)_{i,j}$). To capture the screening effects of contracts, I run the following linear probability model in the subsample of sales on credit:

$$P_{i,j} = d_0 + d_1 S_{i,j} + d_2 R_{i,j} + d_3 R_{i,j} S_{i,j} + A_a + T_e + e_{i,j}$$

where I condition the sample on $D_{i,j} = 1$ and $F_{i,j} = 0$. Table 3.6 presents the testable implications of Experiment III.

3.5 Results

In this section I present and discuss my results. The analysis of the data of the main experiment in Section 3.5.1 reveals that, in a sector where the holdup problem prevails, households who are of the same ethnicity as the trader are more willing to engage in trade than traders who are not. Contracts also increase trade among non-coethnic up to the coethnic level, which are unaffected by contracts. In Section 3.5.2, I demonstrate that the increase in trade stemming from contracts and shared ethnicity is driven by exclusively by their effect on trust. This confirms that both coethnicity and state-contracts can solve hold-up commitment problems leading to welfare gains. Finally, Section 3.5.4 exploits a field

experiment design à-la Karlan and Zinman (2009) to reveal that signing a contract reduces cheating, but only when the claimant is of an ethnic group that has captured the state.

3.5.1 Main result: Experiment I

This section presents the results in Experiment I. I first present the behavioral results. I then report the effects of contracts on self reported beliefs about social sanctions, in order to confirm that the main effect on trade is not confounded by crowding out of social norms.

Effect of contracts on trade: experimental evidence

Figure 3.6 presents the main result. Non coethnic households trade in 48% of the cases. However, when households are coethnics, 62% accept the deal. If the trader uses a state-backed contract to commit to future delivery, the coethnic bias disappears: the proportion of non-coethnics who accepts the sale rises from 48% to 69%, while the proportion of coethnic households who accepts the deal remains unchanged.

Table 3.7 replicates Figure 3.6 in a linear probability model with the $Trade_{i,j}$ dummy as dependent variable. When trade occurs, I coded the variable as 1, and 0 otherwise. All specifications in Experiment I include village fixed effects, because I implemented the randomization at the household level within village. Column (1) presents the baseline specification, Column (2) adds trader fixed effects, Column (3) includes controls for age, Column (4) includes controls for wealth (family size), and Column (5) includes a wealth index, available only for a sub-sample of households. The results are identical accross columns. As can be seen from Column (1), the coefficient on Contract is .19 and significant, suggesting that contracts increase trade by 19% among non-coethnics. Furthermore, the coefficient on Coethnic is .19 and significant, suggesting that coethnics are 19% more likely to accept the deal when a contract is absent than non-coethnics. Finally, the interaction Contract X Coethnic

is $-.17$ and significant. This shows that state-backed contracts increase trade when traders and buyers are of different ethnic groups, that coethnicity is equally effective as state-backed contracts among non-coethnics, and that state-backed contracts are less effective among co-ethnics. When I test the null hypothesis $\beta_{ContractXCoethnic} + \beta_{Contract} = 0$, I cannot reject that contracts have no effect on trade among coethnics. Figure 3.7 replicates Figure 3.6, disaggregating sales by all ethnic groups.

Effect of contracts on social norms: survey evidence

This section shows that the main effect of contracts is not dampened by crowding-out of social norms among in-groups. As a measure of beliefs about prevailing social norms, I asked the households at the end of the offer which informal sanctions will be activated if the trader does not deliver as promised. The question was open-ended, but the traders collected the response by coding it in a list of predetermined options. I identified informal sanctions in preparatory activities and consist of: Shame, Loss of friends, Physical violence. If the option was not available, traders wrote the answer literally. Traders were able to classify the universe of answers using these categories.

Table 3.8 presents the results of a linear probability model of the measure of prevailing social sanctions on whether the contract was used. Column (1) presents the effect of contracts on perceived occurrence of any informal sanction, and columns (2)-(4) disaggregate informal sanctions by the classification presented above. Among all households visited, 50% believe that informal sanctions apply. The magnitude of the coefficient on Contract has insignificant magnitudes across columns. Furthermore, except for column (3), the coefficient on the Contract dummy is statistically insignificant. Table 3.8 suggests that contracts do not crowd out beliefs about social sanctions. If contracts improve the incentive of traders but crowd-out social norms, the main effect identified in Section 3.5.1 is an underestimate of the incentive

effect of contracts.

Fehr and Gaechter (1999) show that in-groups are more likely to sanction defectors. If state-backed contracts crowd out norms of cheating, then households would be less willing to expend resources to sanction the trader upon no delivery. In order to capture the willingness to sanction traders who defect, I asked the following question at the end of the offer: "Imagine the trader will not deliver the good as promised. Would you be willing to pay so that he gets sanctioned? If so, how much would you be willing to pay?". To capture the willingness to incur private losses in order to sanction, I coded strictly positive answers as 1 and the rest as 0. In Table 3.9, I report the results of a linear probability model of this variable on the contract treatment. The coefficient on the Contract dummy is positive and significant across specifications, suggesting that contracts do not crowd-out social norms.

3.5.2 Why do contracts work?

Experiment II: disentangling trust from preferences

The results in Section 3.5.1 did not establish why coethnicity and contracts improve trade. For instance, some households could have social preferences that are biased in favor of co-ethnics ($\frac{\partial \lambda}{\partial E} > 0$) or in favor of traders who reveal to have state-backed contracts ($\frac{\partial \lambda}{\partial F} > 0$). This alone could explain the result, but would have different implications than if contracts or coethnicity solve the holdup problem ($\frac{\partial \eta}{\partial E} > 0$).

To elicit whether contracts and coethnicity affect the *expectations* of traders' delivery, this section reports the results of Experiment II. In this experiment, I randomly assigned whether the sale was made on credit or on the spot, in addition to contracts and coethnicity. This allows me to identify the effect that state-backed contracts or coethnicity have on sales on the spot, and their effect on sales on credit separately. If coethnicity or state-backed

contracts affect trade on the spot as much as they affect trade on credit, this would suggest they do not affect trust.

Table 3.10 reports the effect of contracts, sale on credit, and coethnicity on trade. I use a linear probability model with village fixed effects, since I randomized within village. Column (1) presents the main effect of *sale on credit* on trade in the whole sample. Column (2) restricts the sample to sales made on the spot and reports the main effect of Contract. Column (3) restricts the sample to sales made on credit and reports the main effect of Contract. In columns (4) and (5), I restrict the sample similarly, but focus on the effect of Coethnicity. Column (6) presents the coefficients in the fully saturated model and Column (7) adds household level controls to the fully saturated model (age, age squared, and number of children in the household as a proxy for household wealth).

The coefficient on Sale on Credit is $-.16$ in the whole sample, validating the randomization of sale on credit. This suggests that households were 16% less likely to accept the trade when delivery was postponed to a future date. Columns (2) and (3) demonstrate that contracts increase trade when delivery is in the future, and not when delivery is on the spot. While the coefficient on Contract is insignificant for trades on the spot, it is .1 and significant for trades on credit.

Scrutiny of columns (4) and (5) reveals that coethnicity increases trade only when delivery is in the future (sale on credit). The coefficient on Coethnic is positive significant for the subsample of sales on credit, neither so in the subsample of sales on the spot.

Column (6) shows the fully specified model. Contract and coethnicity increase trade, only when trade is on credit. Indeed, the coefficients on Sale on Credit X Contract and Sale on Credit X Coethnic are respectively .28 and .29 and are statistically significant. However, the coefficients on Contract and Coethnic are negative and insignificant, suggesting they do not affect trade when sale is on the spot. Finally the coefficient on the triple interaction Sale

on Credit X Contract X Coethnic is negative marginally significant, and equal to -.28. This suggests that contracts have a weaker effect on expectations of delivery among coethnics. Furthermore, inspection of the coefficient magnitude suggests that contracts do not change expectations of delivery among coethnics: the magnitude is exactly the inverse of the Sale on Credit X Contract coefficient. Results are unchanged when I add household level controls in Column (7).

Section 3.5.1 showed that coethnicity, or alternativey, state-backed contracts, can increase trade. In Section 3.5.2, I go a step further. I establish that contracts and coethnicity work *exclusively* through their impact on expectations of delivery, trust. This suggests that there can be welfare gains from expanding access to state-backed contracts among members of different ethnic groups. By solving a holdup problem that coethnicity already solves, contracts could indeed spur trade among non-coethnics. However, the offer to sign a state-backed contract may improve expectations of behavior through a signaling channel instead of disciplining the incentives of the trader. Indeed, this result could be the result of pure signaling, if access to state-backed contracts signals that the trader is a better type. Do contracts provide incentives to deliver or do they just signal better traders? In the next Section, I provide support to the relevance of the findings in this section, and demonstrate that contracts provide incentives not to defect.

3.5.3 Why do contracts improve trust?

Measuring incentive effects of contracts in survey data

Section 3.5.1 showed that contracts and shared ethnic affiliation increase trade by improving expectations that the trader (the agent) will deliver as promised. This section uses survey data to show that contracts and coethnicity reduce incentives to cheat. In Section 3.5.4 I

confirm this finding using real behavior in Experiment III.

To measure the sanctions that would prevail if the trader does not deliver, I exploit survey responses. At the end of the offer, I ask the households what they think will happen to the trader if he does not deliver as promised. I coded the answers as Informal sanctions (Shame, Loss of friends, and Physical violence) and judicial sanctions. The question on judicial sanctions was asked as follows: "Do you believe that this contract can lead to judicial sanctions if needed?". Hence, the question captures whether judicial sanctions are a threat *if* the contract was signed, and is also asked to households who did not sign the contract.

Table 3.11 reports results of a linear probability model of this measure regressed on Coethnicity or Contract. To capture how coethnicity produces informal sanctions, Columns (1)-(4) report the effects of coethnicity on informal sanctions. To capture if households believe that judicial sanctions are a real threat, Column (5) regresses the measure of beliefs about the judiciary on Contract. The coefficient on Coethnic in column (1) is .10 and statistically significant, suggesting that households whose ethnic affiliation coincides with the traders' are 10% more likely to expect that informal sanctions are a real threat. Overall, 37.5% of non-coethnic households expect informal sanctions upon defection, against 47% of coethnic households. As shown in columns (2)-(4) show, this is driven by concerns about reputation (shame). Column (5) shows that 13% of households believe that judicial sanctions are a real threat *if* the trader would sign the contract. This proportion rises by 5% if the contract was offered.

Therefore, coethnicity produces beliefs about the threat of informal sanctions and a significant fraction of the population believes in the judicial system. Do they change the beliefs about the trader type? To asses whether contracts change the beliefs about the trader's type, I measured self-reported belief about the motive of the trader at the end of

the offer. In particular, I asked the household who they think the trader is working for. I regress this outcome on Coethnicity and Contract in a fully saturated linear probability model in Table 3.12. The dependent variables in Columns (1)-(3) are three institutions that the trader could potentially work for, from the perspective of households. These are, respectively, a University, a private company, an NGO. Accross columns, Coethnicity and Contract do not affect the belief about who is employing the trader. Furthermore, beliefs are widely dispersed across categories: 25% of households believe that the trader works for an NGO, 34% of households believe that the trader works for a University, and 15% of households believe that the trader works for a private company. This suggests that showing a contract is unlikely to affect the beliefs about the trader's type.

This section has shown, using self-reported survey data on expectations that a significant portion of households believe contracts are enforceable, and that contracts do not affect households' beliefs about the trader's type. Self-reported data, however, is subject to social desirability bias. For instance, households may report what they think the trader wants to hear.[22] Furthermore, social desirability bias may correlate with the willingness to engage in trade for other reasons. To avoid social desirability biases, the next Section, examines real behavior to capture whether self-reported beliefs are mirrored in real behavior. In particular, I examine whether the act of signing a state-backed contract decreases the likelihood to cheat, in order to provide support to the interpretation that contracts solve a holdup problem because they are a credible commitment device.

[22]This concern is plausible: in Humphreys, Sánchez de la Sierra, and Van der Windt (2013c), I demonstrate the presence of strong social desirability bias in this population using a randomized experiment.

3.5.4 Why do contracts improve trust?

Observing incentive effects of contracts in Experiment III

Section 3.5.1 established that state-backed contracts increase trade among non-coethnics. Access to contracts may reveal information about the traders' types. If beliefs about the trader's type correlate with the belief that the trader will deliver the good, signaling alone can lead contracts to increase trade. To disentangle signaling from the incentives effect of contracts, I analyzed survey-based evidence in Section 3.5.3. The results suggest that contracts are enforceable and that beliefs about the trader's type do not change when traders show a contract. In this section, I present the results of Experiment III, which confirm the survey-based results. I find that signing a state-backed contract reduces incentives to cheat on claimants that are part of the dominant social group. However, I also find that contracts are ineffective for claimants that have low representation in the state administration.

Incentive effects of contracts

Table 3.13 presents the main result of Experiment III. To capture whether contracts decrease incentives to cheat, I consider the pool of households who accepted the sale on *debt*. That is, I consider households who accepted to receive the good immediately and promised to pay in the future, under the condition to sign a state-backed contract in which they commit to pay. In a random sample of these households, traders withdrew the requirement to sign a contract. Since households selected into the accepting the trade prior to this randomization (other households rejected the deal), I can obtain the incentive effects of contracts by comparing payments among households who ultimately signed the contract to payments of households in which traders withdrew the requirement to sign a contract. This contrasts to the analysis of the screening effects of contracts in Section 3.5.4, where I focus on households who *ultimately*

accept the trade (after the trader announced that the requirement to sign the contract was withdrawn).

Among the households who accepted the offer, I regress a dummy indicating whether the payment was actually executed on the contract dummy. To identify the effect of contracts, I implement a fully saturated linear probability model on the dummies Rwandophone and Contracts, including team and avenue fixed effects.[23] Column (1) reports the regression on Contract, column (2) reports the regression on Rwandophone trader, and column (3) reports the fully saturated model. Column (1) shows that the average effect of Contract is insignificant, and column (2) shows that the average effect of a Rwandophone trader is negative but is marginally significant. Turning to column (3) Contract has a strong effect for all sales when the trader is not Rwandophone. Indeed, the coefficient on contract is .9 and significant, suggesting that contracts increase payment by almost 10% when the trader is not Rwandophone. Furthermore, the coefficient on Rwandophone is insignificant and small, suggesting that the average effect of Rwandophone traders identified in column (2) does not arise from sales where the traders withdrew to the requirement to sign the contract. As confirmed by the coefficient on Contract X Rwandophone, contracts do not have a positive effect on payment when the trader is Rwandophone. Scrutiny of the coefficients suggests that the average effect of contracts is negative when the trader is Rwandophone, but not significant.

This result supports that contracts provide incentives to respect the promise ($L > 0$). However, this is only true if traders (the claimants) belong to the dominant groups. The marginal effect of contracts is identified for each subgroup of traders, since I randomized

[23] Avenues were randomly assigned to trader teams. Teams were composed of two traders, one Rwandophone, one not. Within each avenue, households were randomly assigned to traders, and treatments were randomly assigned within trader. I therefore use avenue and team fixed effects in all specifications. Since trader ethnicity is fixed within trader, I cannot use trader fixed effects.

the requirement to sign the contract within trader. But is this evidence that Rwandophones have less leverage to enforce contracts ($L(E=1) > L(E=0)$, where $E=0$ if and only if the trader is Rwandophone)? Since the pool of households considered in Table 3.13 is restricted to households who have accepted the sale, I am potentially conditioning to different pools of households when the trader is Rwandophone than when the trader is not Rwandophone. Differential selection into trade by trader ethnicity could be due to preferences ($\frac{\partial v}{\partial E} \neq 0$ households may value differently Rwandophone payoffs), social sanctions ($\frac{\partial v}{\partial E} \neq 0$, households may believe that social sanctions upon defecting a Rwandophone are different), or beliefs about the relative ability of Rwandophones to enforce contracts ($\frac{\partial L}{\partial E} \neq 0$). In what follows, I show that there is no evidence for $\frac{\partial v}{\partial E} \neq 0$ or $\frac{\partial v}{\partial E} \neq 0$.

Heterogeneous incentive effects of contracts by ethnic group: mechanisms

In this section, I identify why contracts fail to work if the claimant is a member of the Rwandophone community. Rwandophones may have be less able to activate legal sanctions ($\frac{\partial L}{\partial E} < 0$), or the screening effects of contracts may be different for Rwandophones, generating a different pool of households under the contract condition. This will be the case if $\frac{\partial \theta}{\partial E} < 0$ (social sanctions/internalized sanctions are endogenous to the identity of the trader) or if $\frac{\partial \theta}{\partial E} < 0$ (private valuation of the good is endogenous to the identity of the trader). I start with a measure of the preference against Rwandophones.

Preferences

To capture preferences against trading with Rwandophones, I use the subset of sales that were implemented on the spot. If there is a preference-based bias against Rwandophones, sales on the spot should be less likely if the trader is Rwandophone. I implement a linear probability model of whether trade occurs $Trade_{i,j}$ on whether the trader was Rwandophone ($Rwandophone_j$). I also include the variable contract as a balance test. Since contract was

randomized at the household level, each household that participated to a sale on the spot later participated to a sale on debt, where the contract randomization was implemented. Thus, contract should not be related to whether the trade on the spot occurs. I include team and avenue fixed effects in all specifications.

Table 3.14 presents the results. Columns (1)-(3) show the results respectively on: Contract, Rwandophone, and the fully saturated model. As expected, the coefficient on contract in column (1) is negligible. As seen in column (2), households who receive the offer from a Rwandophone trader are 4% less likely to accept the deal, but this difference is not statistically significant. This suggests that preference-based discrimination against Rwandophone traders is absent in this population. I then turn to the analysis of screening in the sale on debit sample.

Screening: pool size

I now analyze the screening effects defined in Table 3.6. To capture screening, I focus on the sample of households who ultimately accepted the trade. This contrasts to the analysis of the incentive effects of contracts in Section 3.5.4, where I focused on households who *initially* accepted the trade. Focusing on households who initially accepted the trade allowed me to exclude selection effects, by comparing payment behavior among households who ultimately had to sign a contract and those in which the traders withdrew the requirement to sign a contract. Here, I focus on households who accepted the deal ultimately, $B(t = 2)$, and are subject to a common incentive schemes (those households in which the contract was ultimately not required). After implementing the contract randomization, traders allowed households who rejected the initial offer (with the requirement to sign a contract) to change their decision. This is especially important for the households in which traders ultimately withdrew the requirement to sign the contract: some households may now find it profitable to accept. I first provide evidence of screening, by showing that the decision to ultimately

accept the trade, $B(t = 2)$, depends on whether the contract has to be signed or not.

To show that the requirement to sign a contract affects the household decision to accept the offer, I regress acceptance of the trade post-randomization on the treatment, contract. I use a linear probability model to regress a dummy indicating whether the household ultimately accepts the deal ($B(t = 2)$) on the following dummy variables: Contract ultimately requested (Contract), whether the trader is Rwandophone (Rwandophone), and their interaction (Contract X Rwandophone). Table 3.15 presents the results. Columns (1) and (2) report the average effects of Contract and Rwandophone respectively and column (3) reports the full specification. As expected, column (1) households are 6% more likely to accept to trade at this second step if the contract requirement is withdrawn. This difference is statistically significant. Furthermore, column (2) shows that households visited by a Rwandophone trader are 5% less likely to accept the deal, but this difference is marginally significant. I then turn to the fully saturated model in column (3). The coefficient on Contract reveals that contracts have a strong screening effect for traders who are not Rwandophones: households are 9% more likely to accept the purchase if the contract requirement is withdrawn. However, contracts have no effect on acceptance when the trader is Rwandophone. Indeed, the linear combination Contract + Contract X Rwandophone, which captures the effect of contracts among the Rwandophone traders, is not significant (and negative).

Screening: pool quality

I have now established that contracts affect the decision to purchase goods from non-Rwandophone traders, and they do not affect this decision when the sale is offered by a Rwandophone trader. I now establish that the screening effect of contracts I identified for non-Rwandophone traders is positive. Namely, the households who were screened by initially requiring a contract are a pool of higher quality. To do this, I now focus on households who accepted the offer ultimately, and in which the contract requirement was withdrawn.

Among these households, I compare the payment rates of households who initially accepted to households who initially rejected the offer: this obtains the screening effect. Table 3.16 presents the results of this comparison, estimated in a linear probability model. I include avenue and team fixed effects in all columns. Column (1) focuses on the entire sample, column (2) restricts the sample to sales by non-Rwandophone traders, and column (3) restricts the sample to sales by Rwandophone traders. The variable *Screened* indicates whether the household accepted the initial offer, in which signing the contract was a requirement. Since in this sample, the requirement to sign a contract was withdrawn, the coefficient on *Screened* indicates how much more likely to pay are households who were initially screened, holding constant their current contractual conditions. The coefficient in column (1) is positive and significant. Its magnitude, .35 suggests very strong screening effects: households who accepted the offer initially are 35% more likely to pay. Columns (2) and (3) reveal that this effect is entirely driven by sales by non Rwandophones: the coefficient in the non Rwandophone sample is almost identical, while the coefficient on the Rwandopnone sample is zero.

Heterogeneous ability to enforce contracts

This result suggests contracts have a screening effect, only for the group of traders who has access to the judiciary. In Section 3.5.4, I showed that contracts have incentive effects, only if used by the group of traders who has access to the judiciary. Furthermore, in Section 3.5.4, I showed that Rwandophone traders are no less associated with trades on the spot, suggesting there is no preference-based discrimination against Rwandophone traders. Absent preference-based ethnic bias, $\frac{\partial v}{\partial E} = 0$. If contracts are less effective for Rwandophone traders and they have a lower screening effect, it must be that θ, social sanctions, is higher when cheating on a Rwandophone trader, or that L, the judiciary sanctions, is lower for Rwandophones. But if θ was higher for Rwandophones, payment rates in sales on debt would likely be higher for Rwandophone traders in the absence of contracts. Furthermore, as shown in

Table 3.15, purchase is no less likely for Rwandophone traders. It is therefore unlikely that θ is higher among Rwandophones. As shown in Section 3.4.2, for constant θ and v, lower L decreases the screening effects of contracts (consistent with the results in Tables 3.15 and 3.16) and reduces the incentive effects of contracts (consistent with Table 3.13). Therefore, the evidence presented in this section is strongly supportive that contracts are enforceable, but only when the claimant is member of a group that has access to the state administration, which is not the case of Rwandophones.

I go a step further to validate this result. I asked at the end of the trade in each household whether they believed the contract would have real consequences. Table 3.17 presents these results using a linear probability model. This time, 70% of households who signed a contract believed that the contract was enforceable. This proportion is 11% smaller if the contract is claimed by a Rwandophone.

Figure 3.8 replicates the results using the answer to the question "If you do not pay, what consequences do you think are likely?".[24] In this figure, I present the proportion of households who believe that there will be legal sanctions if they do not pay. I separate matches by whether the household is of the majority ethnic groups (above, labelled NR) and whether the household is of the Rwandophone minority (below, labelled R). For each type of household, I include the proportion who believe that there will be legal consequences when the trader that visited the household is not Rwandophone (NR), and when the trader that visited the household is Rwandophone (R). Clearly, non-Rwandophone traders are consistently better able to activate legal threats. Furthermore, non-Rwandophone traders are better able to activate legal threats on Rwandophone households than vice-versa. Both these differences

[24]Results using both questions are qualitatively identical. However, the wording of the question used in Table 3.17 is ambiguous. It asks the household to identify if there were legal consequences if he would sign a contract. In order to better capture the impact of the dyadic relationship between trader and household on the legal threat, I focus on the second question.The analysis pools all households, including those that did not sign a contract.

are statistically signifcant. This is consistent with the interpretation that it is the ability to enforce contracts that explains why Rwandophones are not better able to increase payment rates when they use a contract to protect themselves. Results in Table 3.17 and Figure 3.8 confirm that contracts are enforceable, but only if they are drafted to protect the claims of certain groups: those which have captured the state.

3.6 Discussion

The previous sections have shown that contracts and coethnicity are substitutes that can solve the holdup problem in Eastern Congo. This section discusses external validity.

One may worry that the household delivery sector considered in this paper is artificial, and created by the researcher.

First, the findings of this paper improve upon a laboratory setting. Haley K. J. (2005) show that cooperation levels in a public goods game increase in a laboratory setting when the experimenter displays an eye on the screen, and decrease when subjects are asked to wear earplugs. This finding revealed controlled environments can generate biases in unknown ways. In this experiment observe behavior in a natural context. To do so, I created a real economic activity of household delivery.

Second, the sales I implement are plausible. The design reflects months of field experimentation. These preparatory activities showed that there was a large demand for soaps in rural households, and for cellphone credit in semi-rural households. Following these observations, I designed a sector of home delivery that provided households with goods that ambulant traders would plausibly deliver.

Third, one may worry that sales on credit (or on debt) are unusual. Households reacted to the trade on credit (or on debt) without suspicion. To assess perceptions, I included a

question at the end of the survey that asked respondents to describe what they thought was the purpose of the traders. Figure 3.9 provides a graphical representation of the language used in this answer. Most households describe economic and research related goals and there is no evidence that households knew what the purpose of the experiment was.

Finally, I included a closed form question at the end of the survey in order to capture whether the respondent finds the activity unusual. Less than 10% did.

3.7 Conclusion

While the impact of the state on economic development has attracted scholars of all social sciences (Bates, 2011), there is little statistical evidence of economic activity in the absence of the state. As a result, the empirical bases of existing scholarship are weak. To improve upon this, I implemented a field experiment in East Congo, where the state is relatively absent, and I am able to estimate the impact of expanding the state, by providing access to its formal system of contract enforcement.

I provide econometric evidence on the economic consequences of extending access to the state judiciary. To observe expansion of the judiciary to areas where there penetration is limited or absent, I managed a team of twenty traders in Eastern Congo. I developed an economic activity of household delivery, based on prior qualitative fieldwork. I allocated the traders to more than 4,000 households, in which they offered to sell a domestic good in high demand at a discount. In randomly selected households, they offered a state-backed contract to guarantee delivery. This contract was drafted by a lawyer and validated by the Provincial administration. Exploiting the mixed ethnicities of this population, I examine whether expanding access to state-backed contracts can lead to gains in trade in where the holdup problem prevails. Furthermore, I can compare it to the mechanisms that groups already

have established in the absence of the state, such as coethnicity. I am able to ask whether access to state-backed contracts increases trade, crowds-out the prevailing social norms, and why.

In Experiment I, I provided the main result. I exploit a feature of the design of the household delivery activity: traders requested immediate payment and promised future delivery. I randomly assigned whether the trader was required to back this promise with a state-backed contract. I find evidence that contracts and coethnicity are equally effective at increasing trade. Furthermore, I find that contracts are ineffective among coethnics. Finally, using self-reported survey data, I find no evidence that contracts crowd-out social norms.

In Experiment II, I extended the main result. Contracts and coethnicity may improve trade because of other channels than households' expectations of the traders' behavior. In particular, coethnics may be more willing to trade because they attach a higher value to the payoff of their in-groups than to the payoff of out-groups (preference-based discrimination/social preferences). To disentangle preference-based families of explanations from expectations of trader's behavior, I randomly assigned whether the sale was offered on the spot or on credit. This allows me to separate the effects of contracts and of coethnicity on expectations of trader behavior from other effects such as preferences. I can identify the trust effect of contracts and coethnicity by comparing their effects in sales are made on credit to their effects in sales that are made on the spot. I demonstrated that both coethnicity and contracts affect trade exclusively through expectations of future behavior.

Finally, using survey data as well as a third Experiment, I showed that contracts have have strong incentive effects. This finding provides confidence in the main result. However, I find that contracts are only enforceable when the potential claimant is member of an ethnic group that has captured the state. This suggests that groups unconnected to the state will struggle to extract the rents that the legal system provides.

This paper has implications for policy. First, my results suggest that expanding access to the state judicial system by providing contracts has potentially large welfare gains, especially in populations where social groups are fragmented. Second, my findings suggest that the design of interventions that ignore the allocation of material power and the political equilibrium may be misguided, as suggested in Acemoglu and Robinson (2013). In Experiment III, state-backed contracts activate threats of judiciary sanctions enforced by the state. However, the state is embedded in a network of social relations and some groups have captured the state. Ignoring the political equilibrium underlying the state can lead to misled policies. In particular, expanding access to state backed contracts where one group has limited access to the state can increase the gap in bargaining power by providing more leverage to those with pre-existing access to the state.

The sales I am able to observe in this field experiment are only a small fraction of the potentially larger number of foregone trade opportunities stemming from poor access to the state-endorsed, formal system of contract enforcement. Future research and policy interventions should consider solutions to the holdup problem that take into account the social and political equilibrium in place.

3.8 Tables

Table 3.1: Design of Experiment I

	Coethnic trader	**non-Coethnic trader**
Contract	I	II
No contract	III	IV

Notes: This table presents the factorial design of Experiment I. There are four treatment cells, according to whether the trader was coethnic, and whether the trader was instructed to offer a state-backed contract to guarantee the sale. In Cell I, the trader is coethnic and offers a contract. In cell II, the trader is non-coethnic and offers a contract. In cell III, the trader is coethnic and does not offer a contract. In cell IV, the trader is non-coethnic and does not offer a contract. In Experiment I, all sales are made on *credit*. In sale on credit, the trader promises to deliver the good in two days, in exchange of immediate payment by the household.

Table 3.2: Design of Experiment II

	Coethnic trader	**non-Coethnic trader**
Contract	I	II
No contract	III	IV

Sales on the Spot

	Coethnic trader	**non-Coethnic trader**
Contract	V	VI
No contract	VII	VIII

Sales on Credit

Notes: This table presents the factorial design of Experiment II. There are eight treatment cells, according to whether the trader was coethnic, whether the trader was instructed to offer a state-backed contract to guarantee the sale, and also whether the trade was implemented on the spot or on credit. In sales on the spot, the trader provides the good on the spot immediately upon receiving payment. In sales on credit, the trader promises to deliver the good in two days, in exchange of immediate payment by the household. In cells I to IV are the households in which trade is implemented on the spot. In Cell I, the trader is coethnic and offers a contract. In cell II, the trader is non-coethnic and offers a contract. In cell III, the trader is coethnic and does not offer a contract. In cell IV, the trader is non-coethnic and does not offer a contract. In cells V to VIII are the households in which trade is implemented on credit. In Cell V, the trader is coethnic and offers a contract. In cell VI, the trader is non-coethnic and offers a contract. In cell VII, the trader is coethnic and does not offer a contract. In cell VIII, the trader is non-coethnic and does not offer a contract.

Table 3.3: Testable Implications: Experiments I and II

Hypothesis	Testable implication
Ethnic preferences: Households prefer to trade with traders of their ethnic group	$a_1 > 0$
Contract preferences: Households prefer to trade with traders that have revealed to have access to state-backed contracts (when there is no risk involved)	$a_2 < 0$
Contracts preferences/ethnic preferences substitutes: Contract distaste is larger among coethnics	$a_4 < 0$
Trust/Discount: Households value more immediate delivery than future delivery	$a_3 < 0$
Ethnic trust: Households trust more traders of their ethnic group	$a_5 < 0$
Contract trust: Households trust more traders who use contracts to back their delivery promise	$a_6 < 0$
Contracts trust/ethnic trust substitutes: Contracts improve trust less among coethnics	$a_7 < 0$
Contracts crowding out/in of ethnic trust: Contracts reduce/increase trust among coethnics	$a_6 + a_7 < 0$

Notes: This table presents the testable implications from Experiments I and II. The left column describes the hypothesis. The right column describes the testable implication in the framework of the econometric specification. It indicates the implied sign of the parameter in the corresponding specification. Let $E_{i,j} \in \{0;1\}$ denote whether the interaction is coethnic, $F_{i,j} \in \{0;1\}$ whether formal contracts are used, and $C_{i,j} \in \{0;1\}$ whether the sale is made on credit. In addition, let T_v be village fixed effects. The regression specification is: $Trade_{i,j} = a_0 + a_1 E_{i,j} + a_2 F_{i,j} + a_3 C_{i,j} + a_4 E_{i,j} F_{i,j} + a_5 E_{i,j} C_{i,j} + a_6 F_{i,j} C_{i,j} + a_7 E_{i,j} F_{i,j} C_{i,j} + T_v + e_{i,j}$. In Experiments I and II, sales are either on credit (both experiments I and II) or on the spot (experiment II only). In sales on credit, the trader promises to deliver the good in two days, in exchange of immediate payment by the household. In sales on the spot, the trader provides the good on the spot immediately upon receiving payment.

Table 3.4: Characterization of the four types of buyers

Strategies	$B=1,P=1$	$B=0,P=.$	$B=1,P=0$	
Preference ordering	$u_{11}>.$	$u_{00}>.$	$u_{10}>u_{11}>u_{00}$	$u_{10}>u_{00}>u_{11}$
conditions	$\theta>\wp$ $v>\wp$	$v<\theta$ $v<\wp$	$\theta<\wp$ $v>\wp$	$v>\theta$ $v<\wp$
Share	α_1	α_2	α_3	α_4
Label	Honest peach	Honest lemon	Moral Hazards	Dishonest lemon

Notes: This table characterizes the strategies in the parameter space. The first line maps the strategy set. The second line presents the conditions associated to the corresponding observable behavior. I present the implied parameter relationships in line three. The terms in line four, $\alpha_i \in 1,2,3,4$ are the mass of agents in each of the relevant cells. Cell relevance is defined by observable behavior: I consider the cells in which buyers make identical choices with and without contracts. For instance, while α_3 and α_4 display the same behavior in the absence of contracts, α_3 values the good enough that he would be willing to accept a purchase requiring enforceable contract while α_4 not. Finally, I label each mass according to two dimensions. First, if the cost of not paying, θ, is sufficiently high (this depends on the individual v) I label them honest. This reflects that they have a *relatively* high θ given v so that they would never defect (forego payment). Second, if the valuation of the good purchased is higher than the monetary cost, I label them peach. If the valuation of the good purchased is lower than the monetary cost, I label them lemon. The peach/lemon terminology captures that trade is only socially optimal when the customer has a sufficiently high private valuation, which is unobserved.

Table 3.5: Screening and Incentives in Experiment III

Step1 *selection*	**C=1**	
	$B=0$	$B=1$
Who buys?	$\alpha_2+\alpha_4$	$\alpha_1+\alpha_3$

Step2 *incentives*	**C=0**	**C=1**
Who pays?	α_1	$\alpha_1+\alpha_3^P$

Notes: This table presents the predicted selection and incentives effects in Experiment III. To disentangle both effects, all households are offered to purchase the good (with immediate delivery) with the condition to sign a contract guaranteeing that they will pay for that good within 2 days by cellphone. If $L>\wp$, the contract is sufficient to motivate payment upon purchase, where $L \in R$ is the cost associated with breaking the contract. In that case, $\alpha_3^P=\alpha_3$. Individuals in the groups α_2 and α_4 reject the offer, since they don't value the good enough to pay for it. Once customers have self-selected, at the time of the transaction, the trader announces in a randomly selected subset of households that he cannot offer a contract because he does not have enough contracts ($C=0$) while the remaining are still required to sign the contrct. The last column reports who pays: among the households in which the contract requirement was withdrawn, α_3 will now prefer to avoid payment.

Table 3.6: Testable Implications: Experiment III

Hypothesis	Testable implication
Ethnic preferences: There is a preferences-based ethnic bias	$b_1 < 0$
Contract enforceable, non-Rwandophones: Contracts have positive incentive effects when they are used to protect a non-Rwandophone trader	$c_1 > 0$
Contract enforceable, Rwandophones: Contracts have positive incentive effects when they are used to protect a Rwandophone trader	$c_1 + c_3 > 0$
Contract screening, non-Rwandophones: Contracts have positive screening effects when they are used to protect a non-Rwandophone trader	$d_1 > 0$
Contract screening, Rwandophones: Contracts have positive screening effects when they are used to protect a Rwandophone trader	$d_1 + d_3 > 0$
Contract heterogeneous incentive effects: Contracts used to protect the trade are more effective for non-Rwandophone traders. This does not identify the difference in access to the judiciary because θ and v are functions of E and hence affect the pool of customers	$d_1 + d_3 > 0$
Contract screening vs. access to the state: If the screening effect is smaller for Rwandophone traders, it must be that Rwandophones have weaker access to justice	$d_3 < 0$

Notes: This table presents the testable implications from Experiment III. In experiment III, sales are either *on debt* or on the spot. In sales on debt, the trader first provides the good, and asks the household to pay in the future by cellphone. In sales on the spot, the trader provides the good on the spot immediately upon receiving payment. The left column describes the hypothesis. The right column describes the testable implication in the framework of the econometric specification. In particular, it indicates the implied sign of the parameter in the corresponding specification. Let $B_{i,j} \in \{0,1\}$ indicate whether the customer accepts to buy. For sales on debt, $B(t=1)_{i,j}$ indicates whether the customer accepted the sale initially (when signing the contract was requested) and $B(t=2)_{i,j}$ indicates whether the customer accepted the sale after the randomization was implemented and they were asked to reconsider their choice. Let $P_{i,j} \in \{0,1\}$ indicate whether the customer pays for the transaction. This is only observed if the customer accepted the purchase. Let $R_j \in \{0,1\}$ denote whether the trader is Rwandophone, $F_{i,j} \in \{0,1\}$ whether formal contracts are used, and $D_{i,j} \in \{0,1\}$ whether the sale is made on debt. In addition, let $T_a \in \{0,1\}$ be avenue fixed effects and $T_e \in \{0,1\}$ denote team fixed effects. Trader teams of two are randomly assigned to avenues. Within each avenue, traders are randomly assigned to households. Finally the contract treatment is randomly assigned within avenue for each trader. The linear probability model specifications are as follows. To capture the incentive effects: $P_{i,j} = c_0 + c_1 F_{i,j} + c_2 R_{i,j} + c_3 F_{i,j} R_{i,j} + A_a + T_e + e_{i,j}$ where I condition the sample on $D_{i,j} = 1$ and $B(t=1)_{i,j} = 1$. To capture ethnic preferences: $B_{i,j} = b_0 + b_1 R_{i,j} + A_a + T_e + e_{i,j}$ where I condition the sample on $D_{i,j} = 0$. Let $S_{i,j} \in \{0,1\}$ indicate whether the customer was initially screened by the request to sign a contract ($S_{i,j} = B(t=1)_{i,j}$). To capture the screening effects of contracts: $P_{i,j} = d_0 + d_1 S_{i,j} + d_2 R_{i,j} + d_3 R_{i,j} S_{i,j} + A_a + T_e + e_{i,j}$ where I condition the sample on $D_{i,j} = 1$ and $F_{i,j} = 0$.

Table 3.7: Effect of contracts and coethnicity on trade (Experiment I)

VARIABLES	(1) Trade	(2) Trade	(3) Trade	(4) Trade	(5) Trade
Contract	0.191***	0.182***	0.194***	0.185***	0.184**
	(0.0594)	(0.0593)	(0.0596)	(0.0617)	(0.0809)
Coethnic	0.190***	0.171***	0.203***	0.170***	0.209***
	(0.0518)	(0.0518)	(0.0523)	(0.0540)	(0.0755)
Contract X Coethnic	-0.168***	-0.158**	-0.172***	-0.148**	-0.190**
	(0.0647)	(0.0646)	(0.0649)	(0.0672)	(0.0884)
Age			0.0178***		
			(0.00679)		
Age squared			-0.000223**		
			(9.35e-05)		
Wealth Index (Family Size)				0.0102**	
				(0.00424)	
Wealth Index (Assets)					0.152***
					(0.0413)
Constant	0.448***	0.351**	0.116	0.427***	0.473***
	(0.0471)	(0.153)	(0.127)	(0.0532)	(0.0675)
Observations	1,685	1,685	1,650	1,563	870
R-squared	0.113	0.126	0.125	0.123	0.213
Village FE	YES	YES	YES	YES	YES
Household controls	NO	NO	YES	YES	YES
Sample	CREDIT	CREDIT	CREDIT	CREDIT	CREDIT

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table presents the results of the main effect. Within villages, traders are randomly assigned to households. In Experiments I and II, sales are either on credit (both experiments I and II) or on the spot (experiment II only). In sales on credit, the trader promises to deliver the good in two days, in exchange of immediate payment by the household. In sales on the spot, the trader provides the good on the spot immediately upon receiving payment. In a random subset of households for each trader, traders were instructed to sign a contract in which the trader commits to deliver the good within two days. The contract was drafted by a local lawyer and stamped by the Provincial authorities (Ministry of the Interior of Sud Kivu). I regress a dummy (Trade) indicating whether the trade took place on a dummy indicating whether the trader used a contract (Contract), a dummy indicating whether the trader and the household were coethnics (Coethnic) and their interaction. Each column presents the results of a linear probability model with village fixed effect. All regressions include only sales made on credit. Column (1) presents the baseline specification, and Column (2) adds trader fixed effects. Columns (3)-(5) include household level controls.

Table 3.8: Effect of contracts on beliefs about social norms (Experiment I)

VARIABLES	(1) Informal sanctions	(2) Shame	(3) Loss of friends	(4) Physical violence
Contract	-0.0156	0.00583	-0.0281**	0.00660
	(0.0227)	(0.0228)	(0.0130)	(0.00747)
Constant	0.492***	0.381***	0.0899***	0.0208***
	(0.0162)	(0.0162)	(0.00923)	(0.00531)
Observations	1,654	1,654	1,654	1,654
R-squared	0.204	0.154	0.073	0.090
Village FE	YES	YES	YES	YES
Sample	CREDIT	CREDIT	CREDIT	CREDIT

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table presents the analysis of the impact of contracts on informal social sanctions. All columns present the results of a linear probability model of the dependent variable on a dummy (Contract) indicating whether a contract was shown by the trader. The dependent variable is the response to the following survey question, asked after the transaction was made "If I did not deliver the good as promised, what do you think will happen to me?". Answers were open, but the traders coded answers into the three most frequent ansers: Shame, Loss of friends, Physical violence. Shame captures reputational losses. Each of these dependent variables is a dummy taking value 1 if it was part of the answer. The dependent variable in column (1) is a dummy variable taking value 1 if any of the three informal sanctions above was part of the answer. All regressions include village fixed effects, since randomization is within village. In Experiments I and II, sales are either on credit (both experiments I and II) or on the spot (experiment II only). In sales on credit, the trader promises to deliver the good in two days, in exchange of immediate payment by the household. In sales on the spot, the trader provides the good on the spot immediately upon receiving payment.

Table 3.9: Effect of contracts on willingness to sanction defectors (Experiment I)

VARIABLES	(1) Willing to pay to sanction	(2) Willing to pay to sanction	(3) Willing to pay to sanction
Contract	0.0252**	0.0250**	0.0301**
	(0.0124)	(0.0119)	(0.0131)
Coethnic	0.00241	-0.0158	0.00102
	(0.0246)	(0.0237)	(0.0258)
Constant	0.125***	-0.00678	0.0285
	(0.0237)	(0.110)	(0.0717)
Observations	3,012	3,012	2,768
R-squared	0.076	0.153	0.081
Village FE	YES	YES	YES
Sample	CREDIT	CREDIT	CREDIT
Trader FE	NO	YES	NO
Household controls	NO	NO	YES

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table presents the effect of contracts on the answer to the following survey question: "If I did not deliver the good as promised, would you be willing to pay so that I receive a sanction?". The regression model is a linear probability model with village fixed effects in all Columns. All regressions include village fixed effects, since randomization is within village. In Column (1) I report the baseline specification, in Column (2) I include trader fixed effects. In Column (3) I include household controls instead (age, age squared, number of children as a proxy of wealth). In Experiments I and II, sales are either on credit (both experiments I and II) or on the spot (experiment II only). In sales on credit, the trader promises to deliver the good in two days, in exchange of immediate payment by the household. In sales on the spot, the trader provides the good on the spot immediately upon receiving payment. The sample is restricted to sales on credit.

Table 3.10: Effect of contracts and coethnicity. Preferences or trust? (Experiment II)

VARIABLES	(1) Trade	(2) Trade	(3) Trade	(4) Trade	(5) Trade	(6) Trade	(7) Trade	(8) Trade
Credit sale	-0.161***					-0.443***	-0.442***	-0.441***
	(0.0313)					(0.131)	(0.135)	(0.130)
Contract		0.0591	0.0953***			-0.00975	-0.0283	-0.0229
		(0.0430)	(0.0320)			(0.146)	(0.151)	(0.145)
Coethnic				-0.0897	0.106**	-0.0913	-0.0840	-0.119
				(0.0879)	(0.0536)	(0.118)	(0.120)	(0.117)
Contract X Coethnic						0.0775	0.0891	0.0937
						(0.154)	(0.158)	(0.153)
Credit sale X Contract						0.284*	0.301*	0.283*
						(0.172)	(0.178)	(0.171)
Credit sale X Coethnic						0.293**	0.283**	0.293**
						(0.136)	(0.140)	(0.136)
Credit sale X Contract X Coethnic						-0.281	-0.263	-0.281
						(0.182)	(0.188)	(0.181)
Constant	0.752***	0.675***	0.569***	0.786***	0.524***	0.808***	0.518**	0.971***
	(0.0243)	(0.0304)	(0.0226)	(0.0826)	(0.0495)	(0.113)	(0.214)	(0.218)
Observations	1,308	450	858	450	858	1,308	1,179	1,308
R-squared	0.069	0.084	0.131	0.082	0.126	0.083	0.095	0.098
Trader FE	NO	NO	NO	NO	NO	NO	NO	YES
Household controls	NO	NO	NO	NO	NO	NO	YES	YES
Sample	ALL	SPOT	CREDIT	SPOT	CREDIT	ALL	ALL	ALL

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table presents the results from a linear probability model of the dummy Trade (indicating whether the trade was successful) on the following dummies: Sale on Credit, Contract, Coethnic. In Experiments I and II, sales are either on credit or on the spot. In sales on credit, the trader promises to deliver the good in two days, in exchange of immediate payment by the household. In sales on the spot, the trader provides the good on the spot immediately upon receiving payment. Sale on Credit is a dummy takes value 1 if the sale was on credit and 0 if the delivery was implemented on the spot.Column (1) presents the average effect of Sale on Credit in the whole sample. Columns (2) and (3) present respectively the average effect of Contract, in the sample of sales on the spot and the sample of sale on credit. Columns (4) and (5) present respectively the average effect of Coethnic, in the sample of sales on the spot and the sample of sale on credit. Column (6) presents the fully saturated model. Column (7) includes household level controls (age, age squared, and number of children) and Column (8) includes trader fixed effects. All regressions include village fixed effects.

Table 3.11: Self-reported expectations about sanctions on the trader (Experiment II)

VARIABLES	(1) Informal sanctions	(2) Shame	(3) Loss of Friends	(4) Physical violence	(5) Legal sanctions
Coethnic	0.0981*	0.0880*	0.0256	-0.0156	0.0231
	(0.0509)	(0.0503)	(0.0271)	(0.0176)	(0.0376)
Contract	-0.0211	-0.00450	-0.0235	0.00688	0.0499**
	(0.0275)	(0.0272)	(0.0146)	(0.00953)	(0.0203)
Constant	0.386***	0.286***	0.0611**	0.0392**	0.112***
	(0.0500)	(0.0494)	(0.0266)	(0.0173)	(0.0370)
Observations	1,219	1,219	1,219	1,219	1,219
R-squared	0.156	0.115	0.113	0.097	0.139
Village FE	YES	YES	YES	YES	YES
Sample	CREDIT	CREDIT	CREDIT	CREDIT	CREDIT

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table presents the results of regressing survey measures of expectations about sanction upon defection of the trader on a dummy indicating whether the household and trader are coethnics (Coethnic) and a dummy indicating whether the trader was instructed to show a contract (Contract). The survey measures of expectations in columns (1)-(4) are the answer to the following question, asked at the end of the sale: "If I did not deliver the good as promised, what do you think will happen to me?". Answers were left open, but the traders coded answers into the three most frequent ansers: Shame, Loss of friends, Physical violence. Shame captures reputational losses. Each of these dependent variables is a dummy taking value 1 if it was part of the answer. The dependent variable in column (1) is a dummy variable taking value 1 if any of the three informal sanctions above was part of the answer. The survey measures of expectations in column (5) is the answer to the following question: "If I signed this contract to guarantee delivery and I did not comply, would there be judiciary consquences for me?". The dependent variable in column (1) is a dummy variable taking value 1 if any of the sanctions in columns (2)-(4) was part of the answer (Shame, Loss of friends, Physical violence). All regressions include village fixed effects, since randomization is within village. In Experiments I and II, sales are either on credit (both experiments I and II) or on the spot (experiment II only). In sales on credit, the trader promises to deliver the good in two days, in exchange of immediate payment by the household. In sales on the spot, the trader provides the good on the spot immediately upon receiving payment.

Table 3.12: Self-reported beliefs about traders' types (Experiment II)

VARIABLES	(1) University	(2) Soap company	(3) NGO
Coethnic	0.00771	0.00319	-0.0328
	(0.0485)	(0.0367)	(0.0451)
Contract	-0.0340	0.0131	0.0209
	(0.0609)	(0.0460)	(0.0566)
Contract X Coethnic	0.0382	-0.00816	-0.0127
	(0.0636)	(0.0480)	(0.0591)
Constant	0.337***	0.151***	0.254***
	(0.0463)	(0.0350)	(0.0431)
Observations	2,591	2,591	2,591
R-squared	0.173	0.194	0.085
Village FE	YES	YES	YES
Sample	ALL	ALL	ALL

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table presents the results of the fully saturated regression model of a dummy variable indicating, respectively, whether the household believes that the trader works for a University, for a Soap company, or an NGO. This variable is the answer to the following survey question, implemented after the sale: "Who do you think I work for?". All regressions include village fixed effects, since randomization is within village.

Table 3.13: Incentive effects of contracts, by ethnic group (Experiment III)

VARIABLES	(1) Pay	(2) Pay	(3) Pay
Contract	0.0183		0.0877**
	(0.0319)		(0.0420)
Rwandophone		-0.0629*	0.0107
		(0.0325)	(0.0440)
Contract X Rwandophone			-0.158**
			(0.0636)
Constant	0.460**	0.477**	0.439*
	(0.226)	(0.225)	(0.225)
Observations	668	668	668
R-squared	0.204	0.208	0.216
Team FE	YES	YES	YES
Avenue FE	YES	YES	YES
Sample	ALL TRADERS	ALL TRADERS	ALL TRADERS

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table presents Experiment III results on incentives. All regressions consider only sales on debt, in which the trader provided the good first, and asked the household to pay by cell phone. I regress a dummy variable indicating whether the household paid by cell phone (Pay) on the dummy Contract, Rwandophone, and their interaction. Contract indicates whether the household ultimately was requested to sign the contract in which he commits to pay by cell phone. Randomization was implemented by randomly withdrawing the requirement to sign the contract in some households after they had accepted the deal (the script specifies that the trader does not have enough contracts to allow him to request a contract here). Rwandophone indicates whether the trader is Rwandophone, and Contract X Rwandophone is their interaction. In column (1) I regress Pay on Contract only. Column (2) I regress Pay on Rwandophone only. Incolumn (3) I report the fully saturated regression model. All regressions are a linear probability model with avenue and team fixed effects. Mixed ethnic two-person trader teams were randomly allocated to avenues and households within avenues were randomly allocated to each trader. The contract randomization was implemented within trader. Since traders' ethnicity is fixed within trader, I do not include traders' fixed effects. An F test of H_0: Contract + Rwanda X Contract =0 cannot reject the null hypothesis, suggesting contracts have no effect on payment incentives when the trader is Rwandophone.

Table 3.14: Preference against Rwandophones, sales on the spot (Experiment III)

VARIABLES	(1) Trade	(2) Trade	(3) Trade
Contract	-0.0328		-0.0514
	(0.0271)		(0.0369)
Rwandophone		-0.0426	-0.0621
		(0.0272)	(0.0379)
Contract X Rwandophone			0.0391
			(0.0541)
Constant	0.0962	0.101	0.127
	(0.152)	(0.152)	(0.153)
Observations	1,009	1,009	1,009
R-squared	0.201	0.202	0.203

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table presents Experiment III preferences results. All regressions consider only sales on the spot, in which the trader provided the good on the spot in exchange of immediate payment. I regress a dummy variable indicating whether the trade occurred (Trade) on the dummy Contract, Rwandophone, and their interaction. Contract indicates whether the household ultimately was requested to sign the contract in which he commits to pay by cell phone. Randomization was implemented as randomly withdrawing the requirement to sign the contract in some households after they had accepted the deal (the script specifies that the trader does not have enough contracts to allow him to request a contract here). Rwandophone indicates whether the trader is Rwandophone, and Contract X Rwandophone is their interaction. In column (1) I regress Trade on Contract only. Column (2) I regress Trade on Rwandophone only. Incolumn (3) I report the fully saturated regression model. All regressions are a linear probability model with avenue and team fixed effects. Mixed ethnic two-person trader teams were randomly allocated to avenues and households within avenues were randomly allocated to each trader. The contract randomization was implemented within trader. Since traders' ethnicity is fixed within trader, I do not include traders' fixed effects. The variable Rwandophone does not decrease Trade when sales are on the spot, at conventional levels of statistical significance. This suggests absence of preference-based ethnic bias against Rwandophones.

Table 3.15: Screening. Effect of contract on trade, in sales on debt (Experiment III)

VARIABLES	(1) Trade	(2) Trade	(3) Trade
Contract	-0.0568**		-0.0848**
	(0.0271)		(0.0365)
Rwandophone		-0.0463*	-0.0770**
		(0.0275)	(0.0383)
Contract X Rwandophone			0.0622
			(0.0543)
Constant	1.185***	1.181***	1.224***
	(0.156)	(0.156)	(0.157)
Observations	960	960	960
R-squared	0.192	0.191	0.196
Team FE	YES	YES	YES
Avenue FE	YES	YES	YES
Sample	ALL TRADERS	ALL TRADERS	ALL TRADERS

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table presents Experiment III screening results. All regressions consider only sales on debt, in which the trader delivered the good first, and asked the household to pay by cell phone. I regress a dummy variable indicating whether the household accepted the sale (Trade) after being asked again once the contract randomization took place. Traders offered the sale with the request to sign a contract to all households. Once they have accepted or rejected, the contract requirement was randomly withdrawn in a subset of households. At this point, households were asked to reconsider their choice. Trade indicates whether at this second step, the household accepted the trade. I regress Trade on the dummy Contract, Rwandophone, and their interaction. Contract indicates whether the household ultimately was requested to sign the contract in which he commits to pay by cell phone. Randomization was implemented as randomly withdrawing the requirement to sign a contract in a random sample of households, after the household has accepted the offer. Rwandophone indicates whether the trader is Rwandophone, and Contract X Rwandophone is their interaction. In column (1) I regress Pay on Contract only. Column (2) I regress Pay on Rwandophone only. Incolumn (3) I report the fully saturated regression model. All regressions are a linear probability model with avenue and team fixed effects. Mixed ethnic two-person trader teams were randomly allocated to avenues and households within avenues were randomly allocated to each trader. The contract randomization was implemented within trader. Since traders' ethnicity is fixed within trader, I do not include traders' fixed effects.

Table 3.16: Screening. Effect of contract on customers' pool quality, in sales on debt (Experiment III)

VARIABLES	(1) Pay	(2) Pay	(3) Pay
Screened	0.346*	0.386*	0
	(0.181)	(0.213)	(0.409)
Constant	-0.0621	-0.0353	0.429
	(0.207)	(0.219)	(0.419)
Observations	355	201	154
R-squared	0.268	0.377	0.375
Team FE	YES	YES	YES
Avenue FE	YES	YES	YES
Sample	ALL TRADERS	NON RWANDOPHONES	RWANDOPHONES

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table presents Experiment III screening results. All regressions consider only sales on debt, in which the trader delivered the good first, and asked the household to pay by cell phone. I regress a dummy variable indicating whether the household implemented the payment by cell phone (Pay). Traders offered the sale with the request to sign a contract to all households. Once households have accepted or rejected the offer, traders withdraw the requirement to sign a contract in a random sample of households. At this point, households were asked to reconsider their choice. In this table, I consider only households in which the contract requirement was withdrawn. I compare those that accepted in the first step (when they were required to sign a contract) to those that accepted only in the second step (when the requirement was withdrawn). Pay indicates whether at this second step, the household implemented the payment. I regress Pay on the dummy Screened. Screened takes value 1 if the household had initially accepted the offer, with the contract requirement, and 0 if it had not (but accepts once the requirement is withdrawn). Rwandophone indicates whether the trader is Rwandophone, and Contract X Rwandophone is their interaction. In column (1) I regress Pay on Contract only. Column (2) I regress Pay on Rwandophone only. Incolumn (3) I report the fully saturated regression model. All regressions are a linear probability model with avenue and team fixed effects. Mixed ethnic two-person trader teams were randomly allocated to avenues and households within avenues were randomly allocated to each trader. The contract randomization was implemented within trader. Since traders' ethnicity is fixed within trader, I do not include traders' fixed effects.

Table 3.17: Belief about contract enforceability (Experiment III)

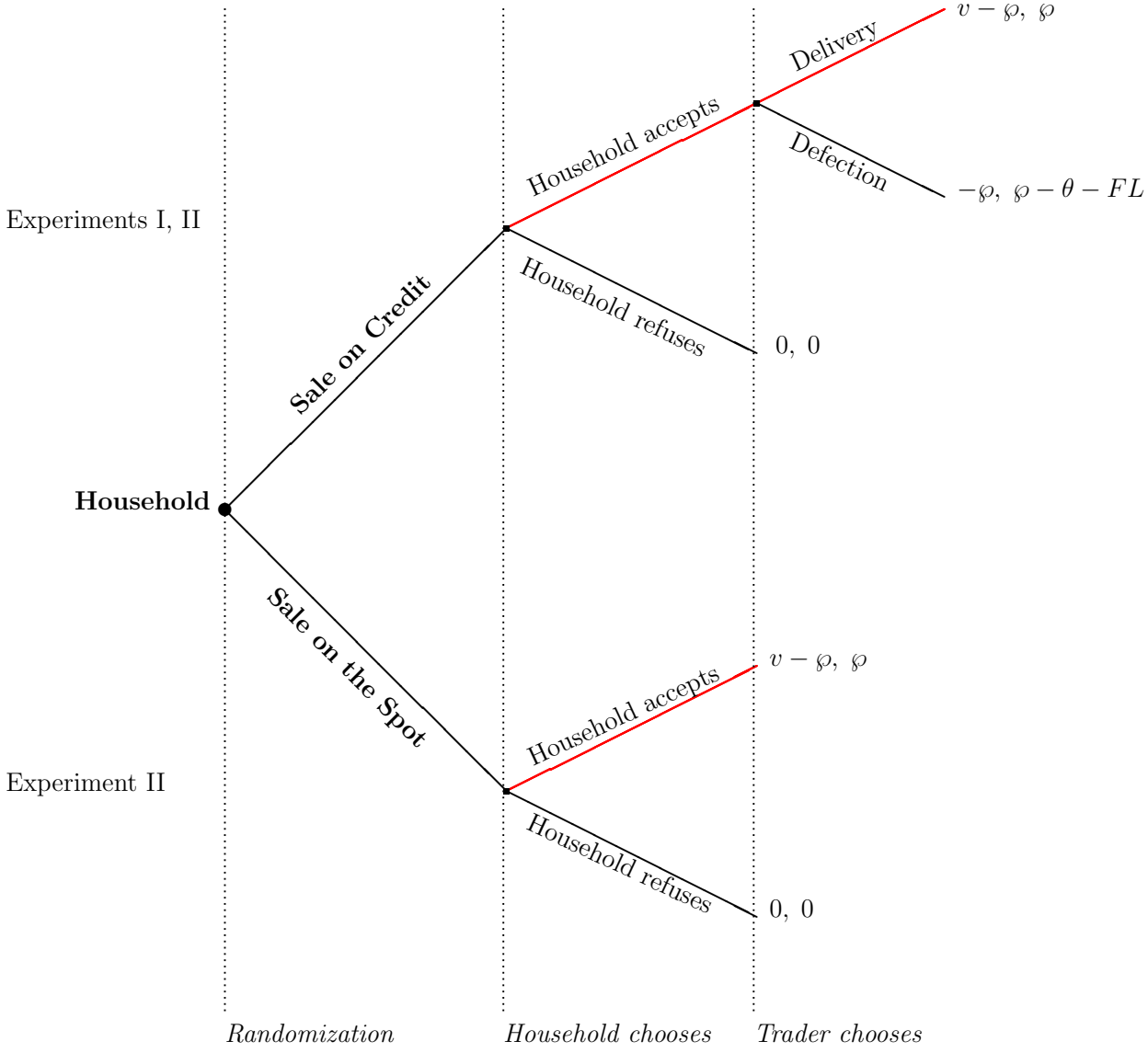
	(1)
VARIABLES	Legal sanctions
Rwandophone	-0.114***
	(0.0351)
Constant	0.673***
	(0.0236)
Observations	764
R-squared	0.014
Team FE	YES
Avenue FE	YES
Sample	ALL TRADERS

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notes: This table presents Experiment III main results using survey based data. I regress a dummy variable indicating whether the household believes that the contract is enforceable (Legal sanctions) on the dummy Rwandophone. Rwandophone indicates whether the trader is Rwandophone. Legal sanction takes value 1 if the household answered yes to the following questio: "Do you think that the contract you just signed can have judicial consequences if you don't pay?". The econometric model is a linear probability model with avenue and team fixed effects. Mixed ethnic two-person trader teams were randomly allocated to avenues and households within avenues were randomly allocated to each trader. The contract randomization was implemented within trader. Since traders' ethnicity is fixed within trader, I do not include traders' fixed effects. 62% of households believe that contracts are enforceable, when they are used to protect a non-Rwandophone trader. This percentage decreases by 12% and the difference is significant at 0.001 significance level.

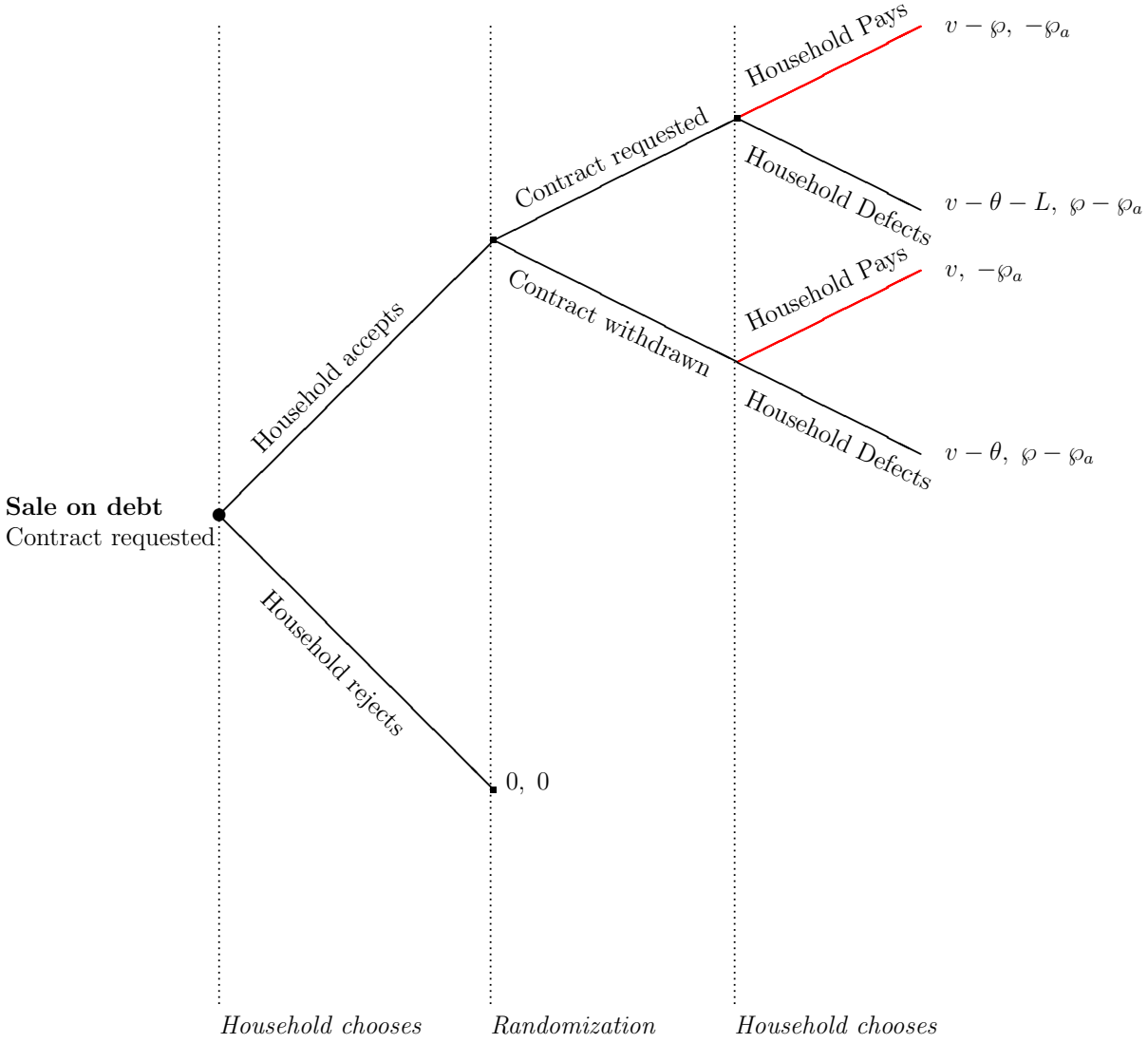
3.9 Figures

Figure 3.1: Design of Experiments I and II



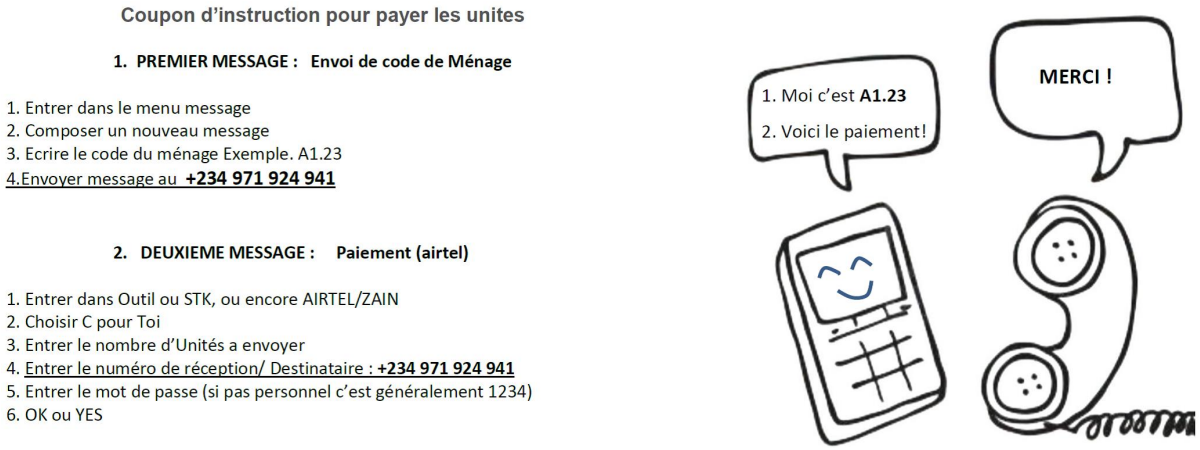
Notes: This graph illustrates the structure of experiments I and II. Once households have been sampled, I randomly allocate households to either Sale on Credit or Sale on the Spot. In sale on credit, the trader makes the offer and requests payment immediately, in exchange for the promise of delivering the good to the household in the near future (in a few days). In sale on the spot, the trader makes the offer and requests payment immediately, but makes the good available to the household immediately upon payment. Once the offer has been made, the household can choose to accept it, in which case trade occurs, or reject it. In sales on credit, if the household rejects, the sale ends, and if the household accepts, then the household makes the payment to the trader. Later, the trader may deliver the good, or may defect. In sales on the spot, if the household rejects, the sale ends, and if the household accepts, then the household makes the payment to the trader and the trader immediately provides the good. I have included the payoffs that the trader and the household would obtain, assuming that the utility function is linear and separable.

Figure 3.2: Design of Experiment III



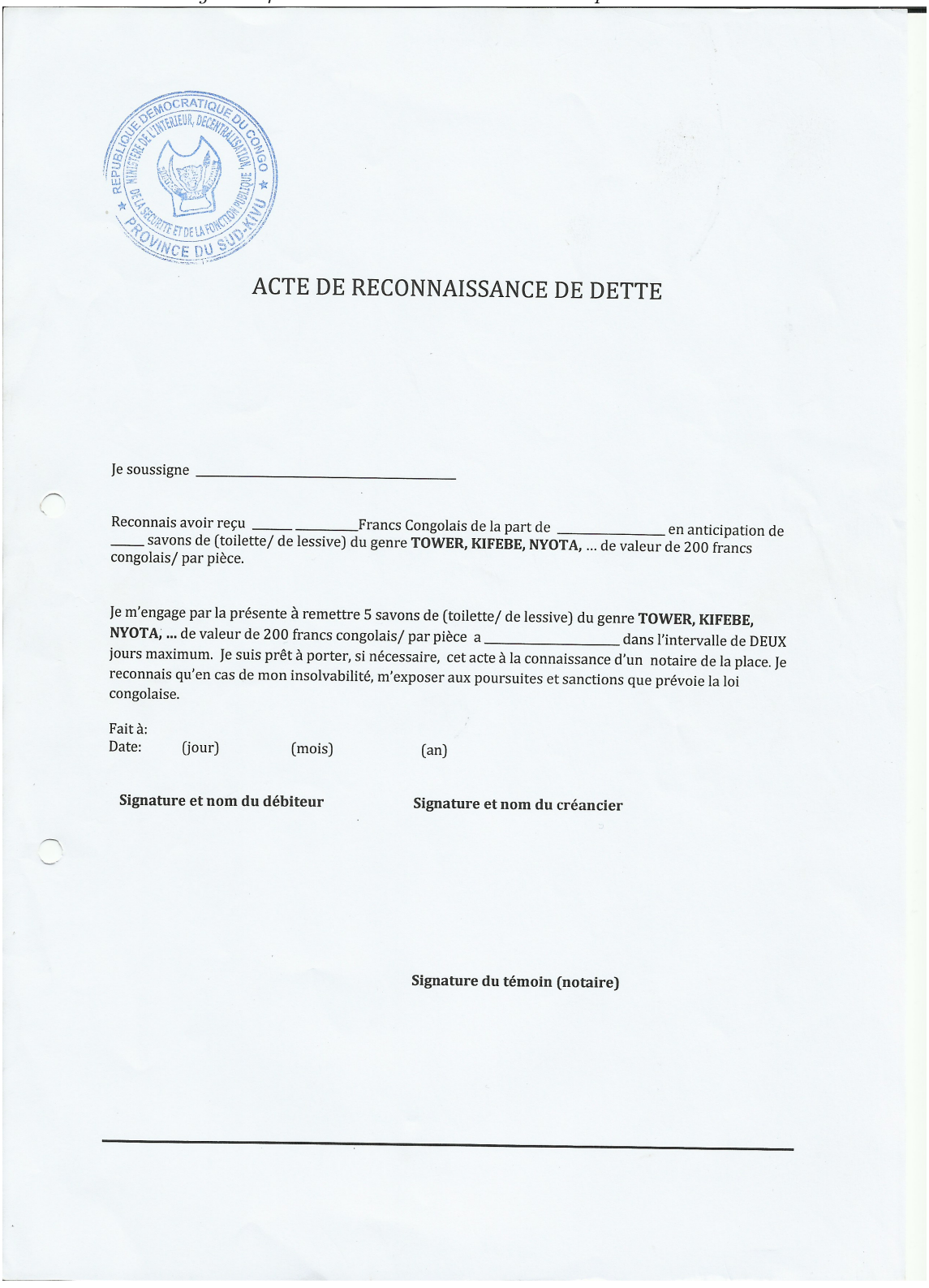
Notes: This graph illustrates the structure of experiment III. Once households have been sampled, the traders implement the visits to the households. In each household, they present the offer to the household. The trader informs the household that he will make the good immediately available to the household, if the household promises to pays by cell phone in the near future (two days). However, all households are informed that in order for the sale to be possible, the household needs to sign a state-backed contract in which he commits to pays within two days. This is the sale *debt*. The household then accepts or rejects. Once the decision has been recorded, a random sample of households is selected in which the requirement to sign the contract is withdrawn. In these households, the trader announces "I do not have a sufficient number of contracts today that would allow me to have you sign a contract. Since we have decided to make the deal, I must go ahead without having you sign a contract". In the remaining households, the requirement to sign the contract is maintained. The trader then leaves the household and the household can pays or defects. I included the payoffs that the trader and the household would obtain, with linear and separable utility.

Figure 3.3: Instructions for payment by cellphone (Experiment III)



Notes: This figure shows the payment instructions given to the household in Experiment III.

Figure 3.4: State-backed contract used in Experiments I and II



REPUBLIQUE DEMOCRATIQUE DU CONGO
MINISTERE DE L'INTERIEUR, DECENTRALISATION, DE LA SECURITE ET DE LA FONCTION PUBLIQUE
PROVINCE DU SUD-KIVU

ACTE DE RECONNAISSANCE DE DETTE

Je soussigne _____

Reconnais avoir reçu _____ _____Francs Congolais de la part de _____ en anticipation de _____ savons de (toilette/ de lessive) du genre **TOWER, KIFEBE, NYOTA,** ... de valeur de 200 francs congolais/ par pièce.

Je m'engage par la présente à remettre 5 savons de (toilette/ de lessive) du genre **TOWER, KIFEBE, NYOTA, ...** de valeur de 200 francs congolais/ par pièce a _____ dans l'intervalle de DEUX jours maximum. Je suis prêt à porter, si nécessaire, cet acte à la connaissance d'un notaire de la place. Je reconnais qu'en cas de mon insolvabilité, m'exposer aux poursuites et sanctions que prévoie la loi congolaise.

Fait à:
Date: (jour) (mois) (an)

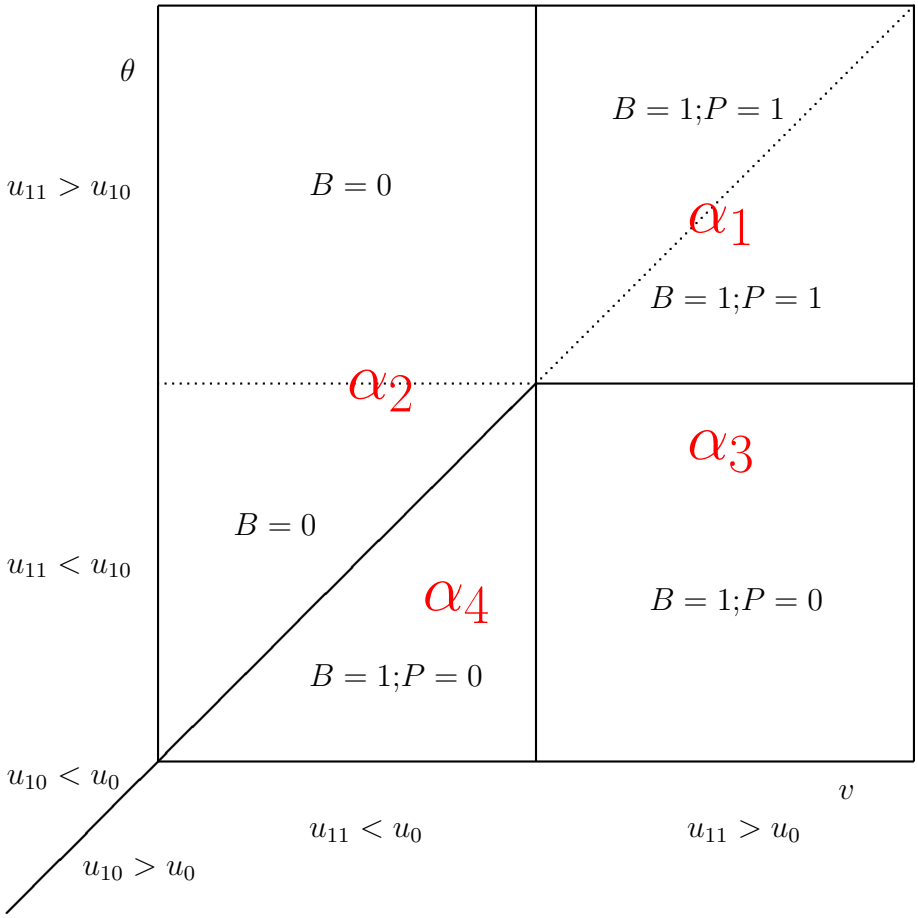
Signature et nom du débiteur

Signature et nom du créancier

Signature du témoin (notaire)

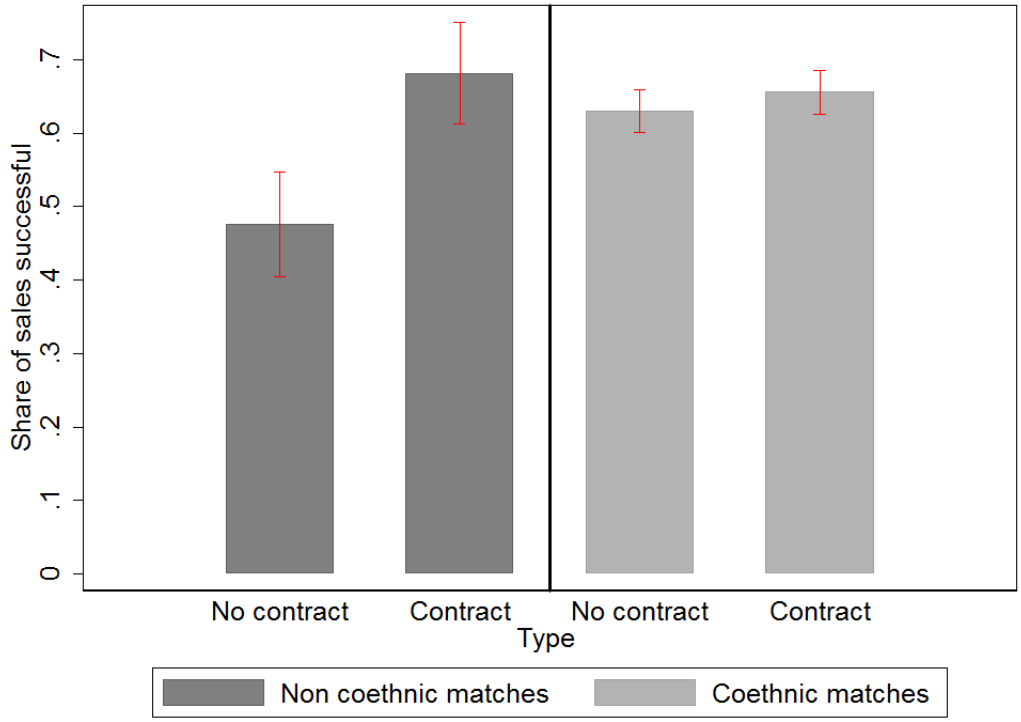
Notes: This figure shows the contract used in Experiments I and II.

Figure 3.5: Household best responses in the parameter space (Experiment III)



Notes: This figure maps the the parameters to the best responses of the household. Thick lines delinate areas where the observed strategies are different. Dotted lines delineate areas where the prefrence ordering changes, but leads to no change in observed behavior whether contracts are required or not. $\alpha_i \in 1, 2, 3, 4$ are the mass of agents in each of the cells. For instance, while α_3 and α_4 display the same behavior in the absence of contracts, α_3 values the good enough that he would be willing to accept a purchase requiring enforceable contract while α_4 not.

Figure 3.6: Effect of contracts and coethnicity on trade (Experiment I)

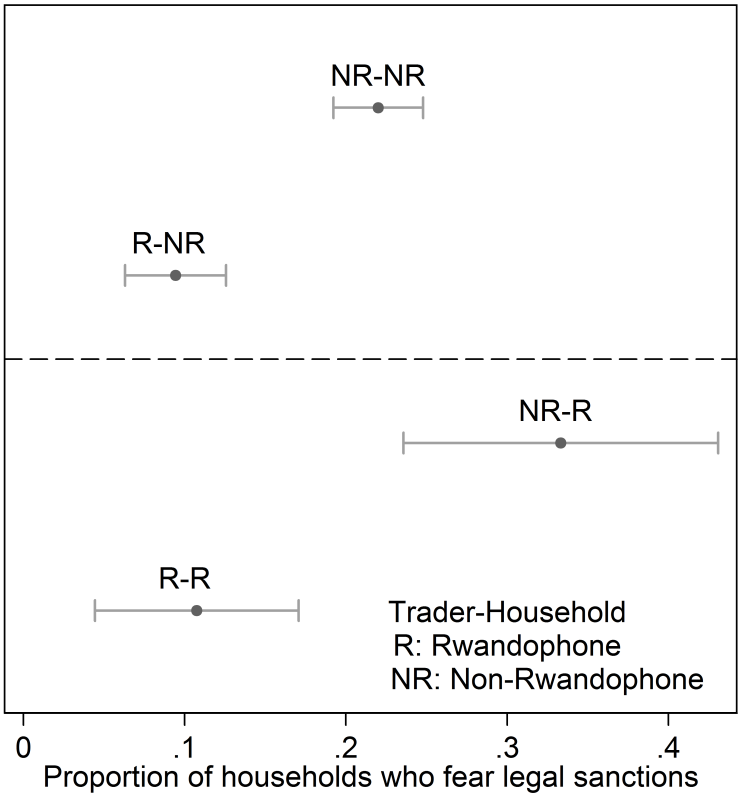


Notes: This figure presents the main result in Experiment I. In Experiment I, sales are implemented on credit. In sales on credit, the trader promises to deliver the good in two days, in exchange of immediate payment by the household. The vertical axis indicates the share of attempted sales that were successful. There are four columns. The first two columns from the left indicate the share of successful sales among households that are non-coethnics of the traders. Among these households, the first column reports the share for households in which traders did not show a contract, and the secnd, the share for households in which the traders showed a contract. Columns 3 and 4 have the same interpretation, albeit for sales in which traders and households are coethnics. Red intervals indicate the 95% confidence interval.

Figure 3.7: Disaggregation of the main effect by ethnic sub-group (Experiment I)

Notes: This figure presents the main result in Experiment I, disaggregated by households' and traders' ethnicity. In Experiment I, sales are implemented on credit. In sales on credit, the trader promises to deliver the good in two days, in exchange of immediate payment by the household. The vertical axis indicates the share of attempted sales that were successful. There are two groups of columns. The first group columns on the left (dark columns) indicate the share of successful sales among households visited by a Shi trader. Columns are grouped in two for each ethnic group of the household: the first column reports the share for households in which traders did not show a contract, and the secnd, the share for households in which the traders showed a contract. The second group columns on the left (light columns) indicate the share of successful sales among households visited by a Havu trader and its interpretation is identical. Red intervals indicate the 95% confidence interval.

Figure 3.8: Belief about contract enforceability (Experiment III)



Notes: This figure presents the beliefs result in Experiment III, disaggregated by households' and traders' ethnicity. In Experiment III, traders are implemented on debit. In sales on debit, traders provide the good, and ask the household to pay within two days by cellphone. At the end of the transaction, the trader asks the household what sanctions he thinks he incurrs if he does not pay as promised. In this figure, I present the proportion of households who believe that there will be legal sanctions if they do not pay. I separate matches by whether the household is of the majority ethnic groups (above, labelled NR) and whether the household is of the Rwandophone minority (below, labelled R). For each type of household, I include the proportion who believe that there will be legal consequences when the trader that visited the household is not Rwandophone (NR), and when the trader that visited the household is Rwandophone (R). I label each of the four interactions according to the ethnicity of the trader, followed the ethnicity of the household.

Figure 3.9: Language used to describe the intention of the traders (Experiments I and II)

Notes: This figure presents the language used by respondents as answer to the following question: "According to you, what am I trying to find out with the reseach component?" As can be seen, households overwhelmingly thought that the traders, through their survey, want to know about their life and economic conditions.

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Appendices

Appendix A

Appendix for Chapter 1

A.1 Mathematical Appendix

A.1.1 Households first order conditions

There are N First order conditions for e_i, N First order conditions for e_i^H, N first order conditions for H_i conditioned on $\tilde{\alpha}_i e_i$ plus the $N+1$ first order conditions from the constraints. The Kuhn-Tucker conditions with respect to H_i, $i = 1, \ldots, N$ give:

$$\forall\ \tilde{\alpha}_i,\ u'[\tau_i p_i - G'(H_i, p_i)]\mu_i = 0,\ i = 1, \ldots, N$$

and

$$\tau_i p_i - G_1(H_i, p_i) \geq 0\ i = 1, \ldots, N$$

An interior solution is therefore given by $H_i < \tilde{\alpha}_i e_i$ and $H_i > 0$, hence $\mu_i = 0$ and:

$$\tau_i p_i = G_1(H_i, p_i)$$

Similarly, an interior solution for $e_i^H$is given by $e_i^H < e_i$ and $e_i^H > 0$:

$$t_i = E_1(e^H, p_i)$$

The FOC with respect to e_i, $i = 1, \ldots, N$ give:

$$\int (\mathrm{u}'[(1-\tau_\mathrm{i})\mathrm{p_i}\tilde{\alpha}_\mathrm{i} - \mathrm{t_i}] + \mu_\mathrm{i})dF(\alpha) - \lambda - c'(e_i) = 0,\ i = 1, \ldots, N$$

for sectors where the hiding constraint is binding, and:

$$\int (\mathrm{u}'[(1-\tau_\mathrm{i})\mathrm{p_i}\tilde{\alpha}_\mathrm{i} - \mathrm{t_i}])dF(\alpha) - \lambda - c'(e_i) = 0,\ i = 1, \ldots, N$$

for sectors where it is not.

A.1.2 Proof of proposition 1

Suppose there is only one sector, so I drop the sector identifiers i. Let $G(H,p) = k\frac{H^2}{2}$. The parameter k captures the cost of hiding output. Let $c(e) = \frac{e^2}{2}$and the utility function be linear in consumption. Labor supply in this form is isoelastic, and the optimal tax is:

$$\tau^* = \frac{1}{2}\frac{\alpha^2}{\alpha^2 + \frac{1}{k}}$$

The optimal tax is larger the higher is the cost to conceal an additional unit of output. At the limit, if output cannot be concealed, and $k = +\infty$, $\tau^* = \frac{1}{2}$. This is because labor supply is isoelastic and always equal to 1, which can be seen from the inverse elasticities rule. If $k \in R^+$, $0 < \tau^* < \frac{1}{2}$. When α is large τ^* approaches its upper bound, and when α is small, τ^* tends to 0. Since $\overline{\alpha}_g \approx 0$, the optimal tax in a village where the only sector is

gold will be low. Furthermore, if $\overline{\alpha}_g$ is sufficiently low so that $\overline{\alpha}_g L < \underline{H}$ (assumption G1), the choice of tax is irrelevant and always raises no revenues, since output is always hidden. Turning to assumption G2, the tax revenue can be written as:

$$\begin{aligned} R &= \frac{1}{2}\frac{\alpha^2 p^2}{\alpha^2 + \frac{1}{k}}\left(\alpha^2(1-\tau) - \frac{\tau}{k}\right) \\ &= \frac{1}{4}\frac{\alpha^4 p^2}{\alpha^2 + \frac{1}{k}} \end{aligned}$$

which is strictly increasing in α. Therefore, $\exists \underline{\alpha}$ s.t. $\forall \alpha < \underline{\alpha}$,

$$R(\alpha) \leq F_g,\ \tau^* = 0$$

and $\forall \alpha > \underline{\alpha}$,

$$R(\alpha) > F_g,\ \tau^* > 0$$

where F_g was defined as the fixed cost of levying an output tax in the gold sector.

A.1.3 Proof of proposition 2

The household's and the bandit's programs are now, respectively:

$$\max_{e, e_H, H} \quad (1-\tau)\, p\alpha e + \tau p H - t\,(e - e_H) - \tfrac{1}{2}e^2 - \tfrac{c}{2}e_H^2 - \tfrac{ph}{2}H^2$$

$$\max_{t, \tau} \quad \tau p\,(\alpha e - H) + t\,(e - e_H)$$

The optimal taxes are $\tau^* = \frac{1}{2}\frac{1}{1+s}$ and $t^* = \frac{\alpha p}{2}\frac{c}{1+c}\frac{s}{1+s}$, where $s = \frac{1+c}{\alpha^2 h}$. Applying the Envelope theorem to the bandit's objective function, it follows that:

$$\frac{\partial V}{\partial p} = \frac{\alpha^2 p}{(1+s)^2}\left(\frac{1}{2} + s + \frac{s^2}{2}\frac{c}{1+c}\right)$$

For gold, there is no tax on output, we can assume $s = 0$, and therefore:

$$\frac{\partial V}{\partial p_g} = \frac{\alpha^2 p}{2}\frac{c}{1+c}$$

We then have:

$$\begin{aligned}\frac{\partial V}{\partial p_c} - \frac{\partial V}{\partial p_g} &= \frac{\alpha_c^2 p_c}{(1+s)^2}\left(\frac{1}{2} + s + \frac{s^2}{2}\frac{c}{1+c}\right) - \frac{\alpha_g p}{2}\frac{c}{1+c} \\ &= \frac{1}{2}\frac{\alpha_c^2 p_c}{(1+s)^2}(1+2s)\left(1 - \frac{c}{1+c}\frac{\alpha_g^2 p_g}{\alpha_c^2 p_c}\right)\end{aligned}$$

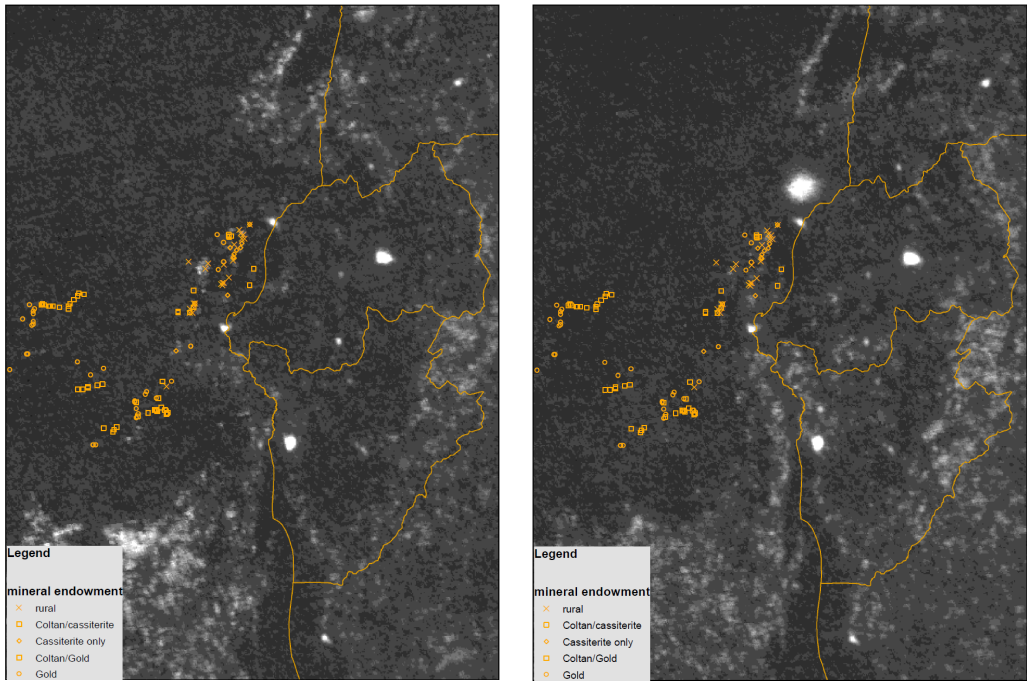
This is positive whenever $\alpha_g = \alpha_c$and $p_g = p_c$. It follows that:

$$\begin{aligned}\frac{\frac{\partial V}{\partial p_c}}{\alpha_c^2 p_c} - \frac{\frac{\partial V}{\partial p_g}}{\alpha_g^2 p_g} &> \frac{1}{2(1+s)^2}\frac{1+2s}{1+c} \\ &> 0\end{aligned}$$

A.2 Data Appendix

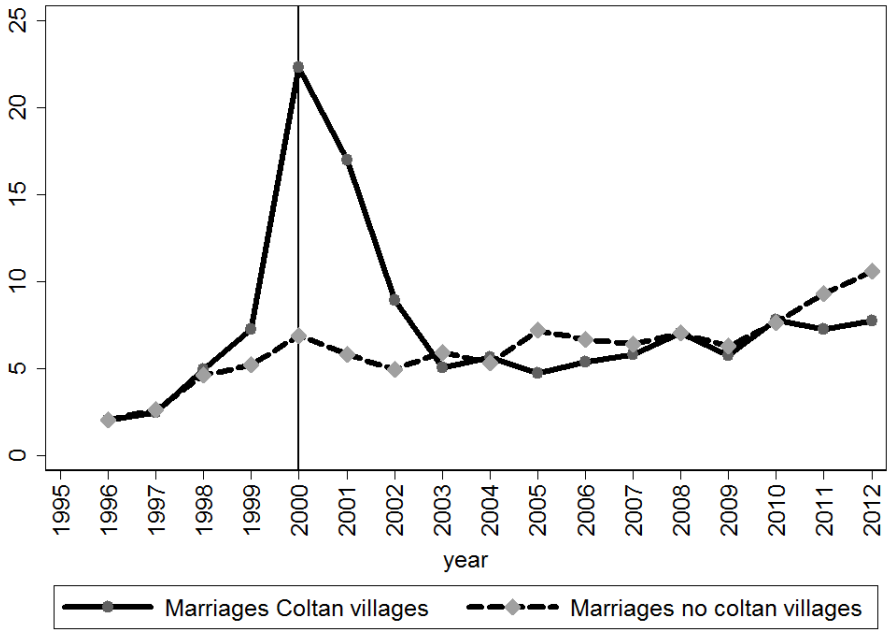
A.2.1 Validation of the coltan shock

Figure A.1: The coltan price shock from satellite



Notes: This figure shows two images of the survey area from satellite at night. The left figure shows the average cloud free lights captured by NASA-NOAA satellites from the survey area in 1999. The right figure is the equivalent figure for 2000. The orange lines indicate international borders. On the left is the Democratic Republic of Congo. On the right, and from North to South: Uganda, Rwanda, Burundi, Tanzania. The endowments of the villages are as indicated in the figure legend. In the year 2000, a very large town emerges in the northern part of the picture. This town is close to Goma, the provincial capital of North Kivu, which is the richest region in coltan. As the price of coltan skyrocketed, miners, traders and individuals whose activities related to mining had an income shock, and consumption increased in the main towns in the proximity of the provincial capitals.

Figure A.2: The coltan shock, number of marriages and occupational choice



Notes: This figure plots the number of recorder marriages on years. The left vertical axis indicates the average number of marriages and the horizontal axis indicates the year. The solid line indicates the average number of marriages in coltan villages, by year. The dashed line indicates the average number of marriages in villages not endowed with coltan, by year.

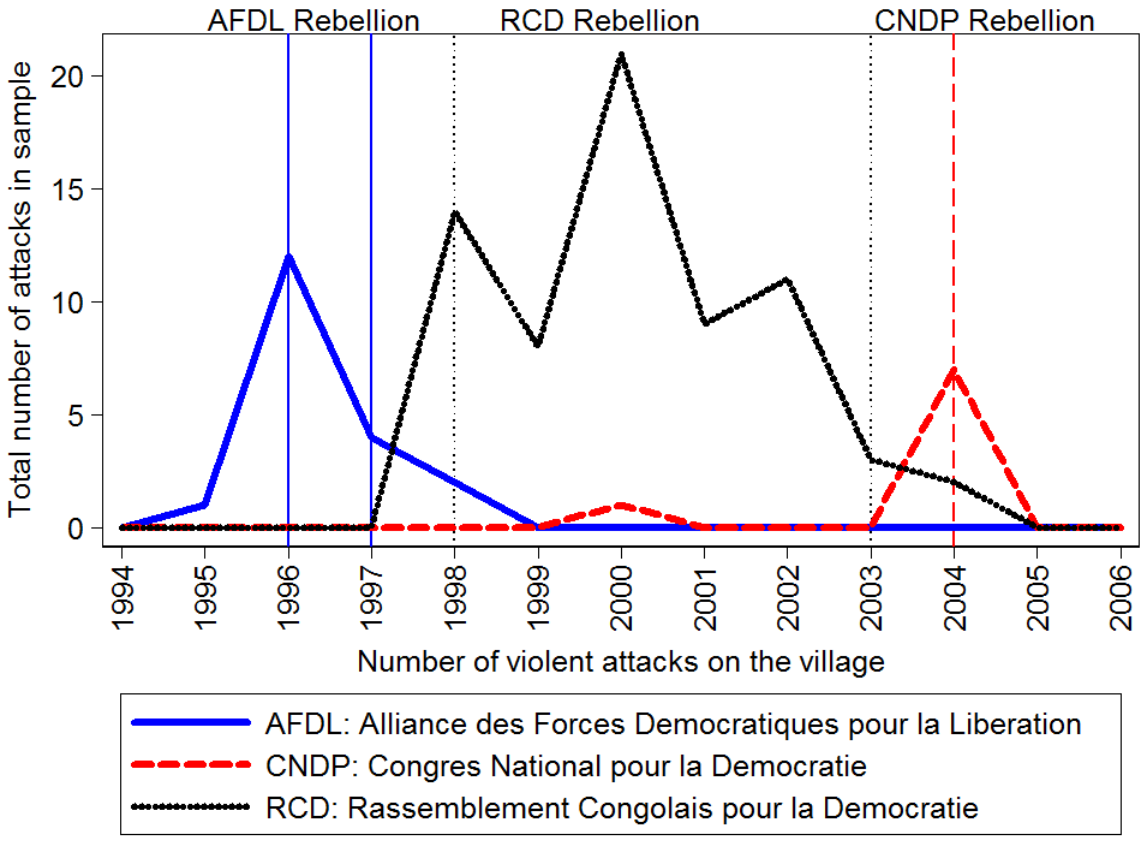
A.2.2 Validation of the data

Table A.1: Survey and ACLED battles

VARIABLES	(1) 1 km	(2) 2 km	(3) 5 km	(4) 10 km	(5) 15 km	(6) 20 km	(7) 25 km	(8) 50 km
Conquest attempts, survey	0.0138***	0.0298***	0.0565***	0.0679***	0.100***	0.0872***	0.122***	0.102***
	(0.00396)	(0.00674)	(0.0107)	(0.0167)	(0.0233)	(0.0268)	(0.0291)	(0.0359)
Constant	0.00718*	0.0295***	0.0515***	0.0817***	0.126***	0.203***	0.248***	0.614***
	(0.00373)	(0.00636)	(0.0101)	(0.0158)	(0.0220)	(0.0253)	(0.0274)	(0.0339)
Observations	2,102	2,102	2,102	2,102	2,102	2,102	2,102	2,102
R-squared	0.107	0.133	0.146	0.172	0.232	0.271	0.281	0.396

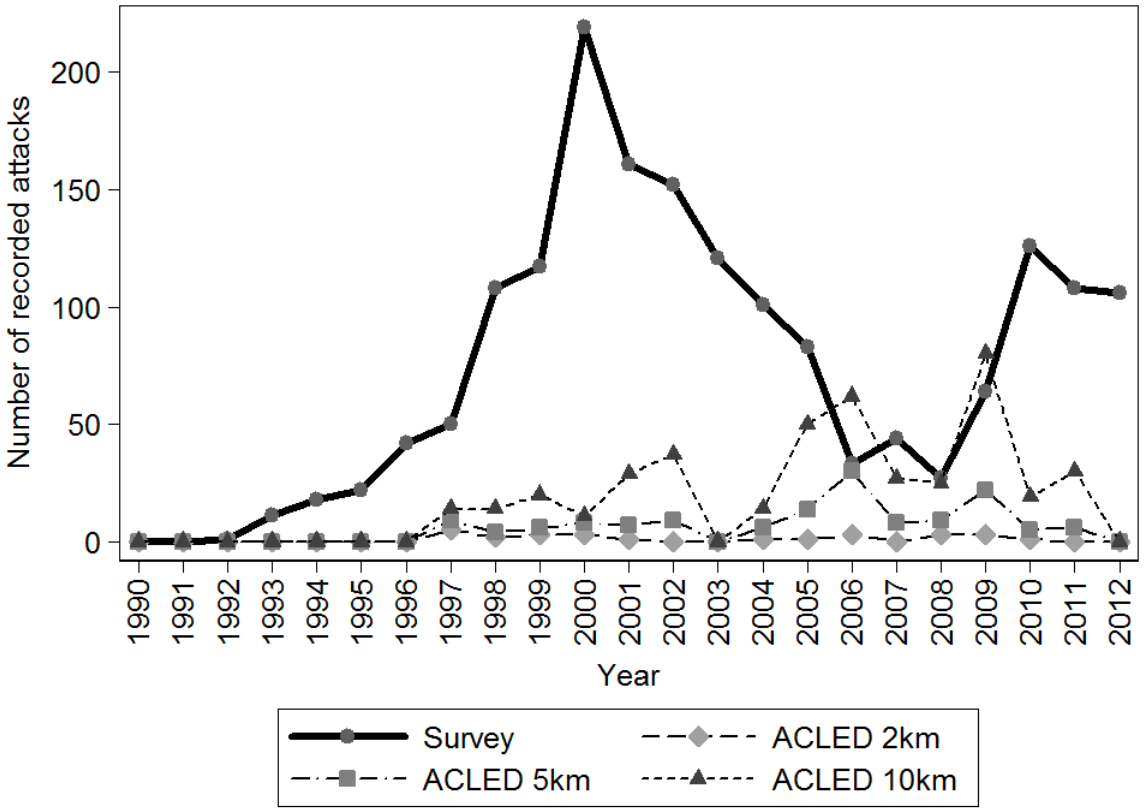
Notes: *** p<0.01, ** p<0.05, * p<0.1. This table presents the results on the OLS regression of battles recored by ACLED on battles recorded by my survey. To assign battles recorded by ACLED to the survey villages, I derived circles of varying circumferences to each of my survey villages. For each circle, I computed the number of geo-located ACLED battles that fall inside the circle. Thus, for each village, the assigned number of ACLED battles is the number that is closer than the radius of the selected circle. Across columns, I present the results for circles of the following radius: 1km (column 1), 2km (column 2), 5km (column 3), 10km (column 4), 15km (column 5), 20km (column 6), 25km (column 7), 50km (column 8). Standard errors are clustered at the village*year level.

Figure A.3: Recorded violent Events and known historical rebellions



Notes: This figure plots the number of attacks on the sample villages by different armed groups recorded in my survey, by year and compares them to well known dates for known historical rebellions. The left axis indicates the number of attacks recorded in the sample, and the horizontal axis indicates the year. The blue line indicates attacks by the AFDL as recorded in the survey. The black line indicates attacks by the RCD as recorded in the survey. The red, dashed line indicates the number of attacks by the CNDP group as recorded in the survey. Correspondingly, the vertical blue lines at 1996 and 1997 mark the well known dates for the launch and end of the AFDL rebellion as known from historical accounts; the vertical dotted lines at 1998 and 2003 mark the well known dates for the launch and end of the RCD rebellion as known from historical accounts; the vertical dashed line at 2004 mark the well known date for the launch the CNDP rebellion as known from historical accounts. The dates of the attacks recorded from the survey coincide exactly with well known historical rebellions. The source for this graph is the attacks dataset, which contains less, more detailed information on attacks.

Figure A.4: Recorded violent Events and ACLED violent events



Notes: This figure plots the total number of attacks on the sample villages recorded in my survey and recorded by ACLED in the neighborhood of my survey villages, by year. The solid line represents the total number of attacks recorded in my survey, and the dotted and dashed lines represent the number of attacks recorded from ACLED, in the neighborhood of my survey villages. To assign attacks recorded by ACLED to the survey villages, I derived circles of varying circumferences to each of my survey villages. For each circle, I computed the number of geo-located ACLED violent events that fall inside the circle. Thus, for each village, the assigned number of ACLED battles is the number that is closer than the radius of the selected circle. The dotted line indicates the resulting number of violent events in a circle of 10km radius. The dashed and dotted line, 5 km radius. The dashed line, 2 km radius. The solid line captures the well-known phases of the Congo Conflict. The number of attacks rises in 1998 drastically, with the Second Congo War, an in 2000, with the coltan shock. It then decreases with the post-conflict period, and rises again in 2009, 2010. This last rise is the rise in attacks by the FDLR as a result of the Kimia II operation that led them to pillage. The ACLED dataset does not capture these trends. This provides additional confidence in my survey attacks data. The source I used for villages in my survey for this graph is the village dataset, which contains more, less detailed information on attacks.

Appendix B

Appendix for Chapter 2

B.1 Mathematical Appendix

B.1.1 Households first order conditions

There are N First order conditions for e_i, N First order conditions for e_i^H, N first order conditions for H_i conditioned on $\tilde{\alpha}_i e_i$ plus the $N+1$ first order conditions from the constraints. The Kuhn-Tucker conditions with respect to H_i, $i = 1, \ldots, N$ give:

$$\forall\ \tilde{\alpha}_i,\ u'[\tau_i p_i - G'(H_i, p_i)]\mu_i = 0,\ i = 1, \ldots, N$$

and

$$\tau_i p_i - G_1(H_i, p_i) \geq 0\ i = 1, \ldots, N$$

An interior solution is therefore given by $H_i < \tilde{\alpha}_i e_i$ and $H_i > 0$, hence $\mu_i = 0$ and:

$$\tau_i p_i = G_1(H_i, p_i)$$

Similarly, an interior solution for $e_i^H$is given by $e_i^H < e_i$ and $e_i^H > 0$:

$$t_i = E_1(e^H, p_i)$$

The FOC with respect to e_i, $i = 1, \ldots, N$ give:

$$\int (\mathrm{u}'[(1-\tau_\mathrm{i})\mathrm{p_i}\tilde{\alpha}_\mathrm{i} - \mathrm{t_i}] + \mu_\mathrm{i})dF(\alpha) - \lambda - c'(e_i) = 0,\ i = 1, \ldots, N$$

for sectors where the hiding constraint is binding, and:

$$\int (\mathrm{u}'[(1-\tau_\mathrm{i})\mathrm{p_i}\tilde{\alpha}_\mathrm{i} - \mathrm{t_i}])dF(\alpha) - \lambda - c'(e_i) = 0,\ i = 1, \ldots, N$$

for sectors where it is not.

B.1.2 Optimal tax properties

This section derives some of the results introduced in Section 2.3.3.

Price shocks and the optimal tax

If labor supply is isoelastic, a price shock has no effect on the optimal tax. To see when price changes increase the optimal tax, suppose there is only one sector, that there are no labor hiding or output concealing activities (for instance, when the cost of both activities is infinite), that the utility function is separable in consumption, $(1-\tau)p\alpha e$, and labor disutility, $c(e)$. From the household's problem, the optimal labor supply is given by $c'(e^*) = (1-\tau)p\alpha$. Let $g(e) = c'(e)$, the optimal tax formula can be rewritten as:

$$\tau^* = \frac{g^{-1}\left(\left(1-\tau\right)p\alpha\right)}{g^{-1'}\left(\left(1-\tau\right)p\alpha\right)p\alpha}$$

An increase in the output price p increases the optimal tax if and only if:

$$\frac{g^{-1'}}{g^{-1}}\left(1-\tau\right)p\alpha > 1 + \frac{g^{-1''}}{g^{-1'}}p$$

Hence a price increases the optimal tax when $g^{-1'}$ is sufficiently large relative to g^{-1}. This will be the case when $c''(e)$ is sufficiently small. In this case, the elasticity of labor supply decreases with the level of the price and the optimal tax increases.

Poll tax

The poll tax is a tax on households, where the activity being taxed is living permanently in the village. Since migration is costly, the elasticity of living permanently in the village is low, and the optimal tax on living in the village may be positive. This can be seen directly from the optimal tax formula.

Incentives to increase the cost of concealing output

Suppose there is only one sector, I hence drop the sector identifiers. Let $G(H,p) = k\frac{H^2}{2}$. The parameter k captures the sector cost of hiding output. Let $c(e) = \frac{e^2}{2}$ and the utility function be linear in consumption. Labor supply in this form is isoelastic, and the optimal tax is:

$$\tau^* = \frac{1}{2}\frac{\alpha^2}{\alpha^2 + \frac{1}{k}}$$

The optimal tax increases in k. Hence the costlier it is to conceal output, the higher the optimal tax. Furthermore, the stationary bandit's revenue at the optimum is:

$$\begin{aligned} R &= \frac{1}{2}\frac{\alpha^2 p^2}{\alpha^2 + \frac{1}{k}}\left(\alpha^2(1-\tau) - \frac{\tau}{k}\right) \\ &= \frac{1}{4}\frac{\alpha^4 p^2}{\alpha^2 + \frac{1}{k}} \end{aligned}$$

Hence, R is increasing in k. If the bandit can manipulate k by using power to increase the cost of concealing output, he will do so since this raises his tax revenue.

Mix of input and output taxes

Proposition: If either both the bandit and households are risk averse, or if both output and input can be hidden, the optimal contract at extraction sites will be characterized by a mix of input and output taxes. Furthermore, in the absence of risk aversion, but with imperfect monitoring of inputs and outputs, the ratio of output to input taxes will be characterized by the following relationship:

$$\tau_i^* = t_i^*\left(\alpha_i e_i - H_i\right)\frac{G_{11}^i}{p_i E_{11}^i}$$

Under either risk aversion or imperfect observability (or both), if stationary bandits are offering optimal contracts, they should provide a mix of input and output taxes. First, because the optimal contract is an insurance contract. Indeed, by levying a tax on output, the bandit is absorbing the risk stemming from output realizations. On the contrary, there is no risk transfer associated with a task on labor inputs, since labor supply is certain. The optimal

contract will thus be characterized by a contract in which risk is bared disproportionately by the actor with lower risk aversion. Second, even in the absence of risk aversion, when either outputs or inputs are imperfectly monitored, the optimal contract will be characterized by a mix of input and output taxes. Sheltering activities generate losses through two channels: since they consist of hiding, they decrease the surplus available to tax; in addition, since they are costly, they decrease the overall surplus.

Proof that the optimal tax on output is positive when households are risk averse. Consider the one sector problem without sheltering activities:

$$\begin{aligned} \max_{t,\tau} \quad & (t + p\overline{\alpha}\tau)\, e = R \\ \text{s.t.} \quad & e = argmax\{Eu\left((1-\tau)\, p\tilde{\alpha}e - te\right) - c(e)\} \end{aligned}$$

where $\overline{\alpha}$ is the mean of the volume productivity distribution. I use the fact that the constraint is a first order condition on the producer's objective function. From the Lagrangian first order conditions:

$$\overline{\alpha} = \frac{E\left[u''\left((1-\tau)\, p\tilde{\alpha}e - te\right)\left((1-\tau)\, p\tilde{\alpha} - t\right)\tilde{\alpha}e + \tilde{\alpha}u'\left((1-\tau)\, p\tilde{\alpha}e - te\right)\right]}{E\left[u''\left((1-\tau)\, p\tilde{\alpha}e - te\right)\left((1-\tau)\, p\tilde{\alpha} - t\right)e + u'\left((1-\tau)\, p\tilde{\alpha}e - te\right)\right]}$$

Exploiting the definition of the Coefficient of relative risk aversion, this expression is equivalent to:

$$E\left[u'\left((1-\tau)\, p\tilde{\alpha}e - t\right)\left(\tilde{\alpha} - \overline{\alpha}\right)\right] = 0$$

Which implies:

$$cov\left(u'\left(\left(1-\tau\right)p\tilde{\alpha}e-t\right)\left(\tilde{\alpha}\right)\right)=0$$

This obtains only for $\tau = 1$. Obviously, the optimal contract is characterized by the bandit (principal) absorbing all the risk from production. Inserting $\tau = 1$ on the program leads to

$$u''\left(-te\right)=\frac{-1}{\lambda}$$

where λ is the lagrange multiplier on the producer maximization incentive compatibility constraint. This implies $t < 0$. The optimal contract if the bandit was risk neutral is one in which the bandit provides payment to producers in order to work that does not depend on output (a wage), and keeps all output. This assumes that the bandit is risk neutral. It is straightforward to show in the same framework that when the bandit is risk averse, the optimal contract will be a mix of input and ouptut taxes/payments.

I now consider sheltering activities exclusively and ignore risk. Consider the dual problem. To avoid dealing with interactions between sheltering and labor supply, I focus on a problem with infinitely elastic labor supply. This allows me to extract the role of sheltering activities, while leaving the qualitative conclusions unchanged. The proof exploits the stationary bandit's revenue neutral change in τ_i and t_i around the optimal choice of e_i and H_i for the household. The stationary bandit's revenue neutral infinitesimal change in taxes in sector i for a given e_i is characterized by:

$$dR=0=d\tau_i\left[p_i\left(\alpha_i e_i-H_i\right)-p_i\frac{\partial H_i}{\partial\tau_i}\right]+e_i dt_i$$

Total differentiation of the household's objective function yields:

$$\begin{aligned} \text{dEU} &= \tfrac{\partial EU}{\partial \tau_i} d\tau_i + \tfrac{\partial EU}{\partial t_i} dt_i \\ &= e_i p_i d\tau_i E\left[-u(\tilde{\alpha}_i - \overline{\alpha}_i)\right] - e_i dt_i E u' \end{aligned}$$

subsituting for dt_i from the last equation:

$$\begin{aligned} &= e_i p_i d\tau_i E\left[-u(\tilde{\alpha}_i - \overline{\alpha}_i)\right] - p_i \tau_i \tfrac{\partial H_i}{\partial \tau_i} E u' d\tau_i \\ &= p_i d\tau_i \left[-\tau_i \tfrac{\partial H_i}{\partial \tau_i} E u' - cov(u', \tilde{\alpha}_i e_i)\right] \end{aligned}$$

It is immediate to see that $dEU > 0$ if $\tau_i = 0$ which implies that conditional on a tax on inputs being optimal, it must be that there are also taxes on outputs in the case of uncertainty.

I have already shown that $dEU > 0$ if τ_i. Let's now show that $dEU < 0$ at $\tau_i = 1$.

$$\begin{aligned} \text{dEU} &= p_i d\tau_i \left[-cov(u', \tilde{\alpha}_i e_i) - \tfrac{\partial H_i}{\partial \tau_i} E u'\right] \\ &= -p_i d\tau_i \tfrac{\partial H_i}{\partial \tau_i} E u' \\ &= -p_i E u' \tfrac{\partial H_i}{\partial \tau_i} < 0 \end{aligned}$$

The first equality follows from the fact that in an interior solution, when $\tau_i = 1$, $cov(u', \tilde{\alpha}_i) = 0$ (since at interior solutions for the hiding choice, $\tilde{\alpha}_i$ where $H_i = \tilde{\alpha}_i e_i$ are of measure zero.)

The maximum τ_i therefore must be interior. A necessary condition for the maximum is therefore $dEU = 0$, which obtains by setting the expression for $dEU = 0$, ie, whenever:

$$\tau_i^* = \frac{-cov(u', e_i \tilde{\alpha}_i)}{\tfrac{\partial H_i}{\partial \tau_i} E u'}$$

Derivation of the closed form formula for input and input taxes when both can be hidden. The proof of the optimal tax with imperfectly observable inputs and outputs exploits

a revenue neutral change in t_i and τ_i and selects amount the sect of revenue constant combinations the one that maximizes household welfare. As explained above, I focus on the problem, *given* e_i. The bandit's revenue in sector i can be written as:

$$R_i = \tau_i p_i \left(\alpha_i e_i - H_i\right) + t_i \left(e_i - e_i^H\right)$$

Total differentiation of the bandit's revenue in sector i gives:

$$dR = d\tau_i \left[p_i \left(\alpha_i e_i - H_i\right) - \tau_i p_i \frac{\partial H_i}{\partial \tau_i}\right] + dt_i \left[e_i - e_i^H - t_i \frac{\partial e_i^H}{\partial t_i}\right]$$

The infinitesimal change in t_i and τ_i is revenue neutral, given e_i, by setting $dR_i = 0$. Total differentiation of the household's utility given e_i gives:

$$dU = p_i \left(-\alpha_i e_i + H_i\right) d\tau_i - e_i dt_i$$

Expressing dt_i by the equivalent revenue neutral $d\tau_i$ from the previous equation gives:

$$dU = \frac{p_i d\tau_i}{e_i - e_i^H - t_i \frac{\partial e_i^H}{\partial t_i}} \left[t_i \left(\alpha_i e_i - H_i\right) \frac{\partial e_i^H}{\partial t_i} - \tau_i \frac{\partial H_i}{\partial \tau_i}\right]$$

Setting $dU = 0$ gives the revenue neutral combination that minimizes distortions:

$$\begin{aligned} \tau_i^* &= t_i^* \left(\alpha_i e_i - H_i\right) \frac{\frac{\partial e_i^H}{\partial t_i}}{\frac{\partial H_i}{\partial \tau_i}} \\ &= t_i^* \left(\alpha_i e_i - H_i\right) \frac{G_{11}}{p_i E_{11}} \end{aligned}$$

Proceeding similarly, it is straightforward to derive the set of contracts as a function of the distribution of risk aversion in this population and the observability of inputs and outputs. Table B.1 characterizes the optimal contracts.

Table B.1: Optimal contracts

Sheltering activities:

None	$\tau = 1$ $t < 0$	$0 < \tau < 1$ $0 < t < 1$	$0 \leq \tau \leq 1$ $0 \leq t \leq 1$	$\tau = 0$ $0 < t < 1$
Outputs	$0 < \tau < 1$ $0 < t < 1$	$0 < \tau < 1$ $0 < t < 1$	$\tau = 0$ $0 < t < 1$	$\tau = 0$ $0 < t < 1$
Inputs	$\tau = 1$ $t < 0$	$0 < \tau < 1$ $0 < t < 1$	$0 < \tau < 1$ $t = 0$	$0 < \tau < 1$ $t = 0$
Inputs and outputs	$0 < \tau < 1$ $0 < t < 1$	$0 < \tau < 1$ $0 < t < 1$	$0 < \tau < 1$ $0 < t < 1$	$0 < \tau < 1$ $0 < t < 1$
Bandits	Risk neutral	Risk averse	Risk neutral	Risk averse
Households	**Risk averse**		**Risk neutral**	