Local Institutions and Rural Development

Evidence from Liberia

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Theories are no more than fictions
which help us to make sense
of experience.

—Chinua Achebe

The Truth of Fiction. In *Hopes and Impediments: Selected Essays 1965-1987,* 1988. Oxford: Heinemann.

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General Introduction

1.1 Background

1.1.1 Poverty and aid in Sub-Saharan Africa

In the past five decades, there has been much attention for economic development in Africa from scientists, policy makers and development practitioners, and this attention does not waiver.[1] Since African nations gained independence in the 1960s, African economic history has been characterized by stagnation. Unlike in the rest of the southern hemisphere, in the years between 1965 and 1990 average GDP per capita in Africa did not grow (Easterly & Levine 1997). A number of countries in Sub-Saharan Africa even faced negative growth rates in this period, and were plagued by coups d'état and civil wars. In coastal West Africa, for example, violence was passed back and forth in six countries, and 'Liberia is reckoned the eye of the storm' (Richards et al. 2005, p.9). Increasingly, policy makers realise that poverty in Africa cannot be seen as separate from developments in the rest of the world. In a rapidly globalizing world, African poverty increasingly turns into a global concern. Recent crises, such as the outbreak of the Ebola virus disease in Liberia, Guinea, and Sierra Leone, as well as the countless African refugees who risk their lives in small boats to reach Europe every week, have made these global connections more visible than ever before.

Poverty has many dimensions, including undernutrition and high mortality rates as result of well-treatable diseases like malaria and diarrhoea (e.g., Sachs & Malaney 2002), the outbreak of extended civil wars (e.g., Fearon & Laitin 2003), and low school enrolment rates. Recognizing the interlinked nature of many of these poverty-related problems, 147 heads of states adopted the Millennium Development Goals (MDGs) during the United Nations Millennium Summit in 2000, to be followed up by the Sustainable Development Goals (SDGs) in 2015.[2] The MDGs, which were proposed

[1] Throughout this thesis, whereever the term Africa is used, I refer to Sub-Saharan Africa.

[2] The SDGs differ from the MDGs mainly in the sense that they are wider. Unlike the MDGs, the SDGs bring together development and climate goals, and seek to tackle global public goods problems next to national obstacles, both in poor and in rich countries (Norton & Stuart 2014).

by Jeffrey Sachs, aim to reduce extreme poverty and hunger by addressing employment, health, food security and shelter, alongside efforts to promote access to primary education, gender equality and environmental sustainability (Sachs & McArthur 2005). The motivation behind these interlinked sets of development goals is that if western countries jointly invest enough in these basic necessities of life, Africa will be able to escape the 'poverty trap' (e.g., see Sachs et al. 2004; Carter & Barrett 2006). Yet, the efforts of the West to 'save Africa' have received much critique as well, and the relationship between aid interventions and economic development is subject to a fierce debate (e.g., see Easterly 2009). One of the key elements in this discussion is the role of institutions in explaining development outcomes.

1.1.2 Institutional dimension of development

The social science literature on development in Africa has assigned a prominent role to institutional constraints as a cause for slow growth and poor governance on the national level (e.g., see Bates 1981; Bates 2008; Bayart 1993; Bayart et al. 1997; Platteau 2009; Herbst 1990). Institutions have also been taking a prominent role in macro-economic explanations of the large global income gaps. Institutions are now widely seen as key drivers for growth—mainly through the role of property rights and rule of law (North 1990). Increasingly, economists are trying to identify causal effects of institutions on economic development, and data on historical institutions have begun to play an important role in explaining modern economic outcomes. For example, Acemoglu et al. (2001) demonstrate that there is a strong correlation between colonial institutions and development of present-day African economies, relating historical data on settler mortality to current economic outcomes. Engerman and Sokoloff (1997) show that the use of slave labour is detrimental to society, which may explain the diverging growth paths of North and South America. Nunn (2008) complement these findings by demonstrating that not only the use of slave labour, but also the production of slaves—through domestic warfare, raiding and kidnapping—negatively influences present day economic development. Various cross-country studies have pointed out that the quality of institutions is much better at predicting

global income difference than geographical characteristics or openness to trade (Rodrik et al. 2004).

Poor quality of institutions, absence of accountability and weak rule of law, combined with high levels of corruption, still characterize many African states today (Bräutigam & Knack 2004). In fact, a number of indicators of institutional performance, such as government corruption, rule of law, and the quality of government bureaucracy deteriorated—both in absolute terms as well as in comparison to other developing regions (Platteau 2009). The deterioration of institutions may be related to the legacy of civil war in numerous African states, and well as ongoing conflicts for example in Congo, Somalia, South Sudan and Uganda. Whereas international war may contribute to state-building (e.g., Tilly 1975), civil conflict tends to undermine institutions (Herbst 1990). In addition, Platteau (2009) identifies the discrepancy between informal norms and institutions and the modern law system—creating a system of 'legal dualism' as a possible impediment for change. 'In SSA institutional and cultural traits such as ethnicity, kinship, redistributive norms, magical beliefs, and distrust of centralized state agencies tend to be reproduced over time and remain resistant to fundamental reshaping under conditions of decolonization and political liberalization' (*ibid*, p.670). Hence, informal, local institutions that are firmly rooted in historical norms and traditions may partially explain the slow growth that characterises large parts of Africa.

Macro-economic studies have provided important lessons about the role of institutions for development, and tried to take into account heterogeneities in institutional quality and the outcomes from it. One prominent example, positioned in between the macro and micro level, is the study from Acemoglu et al. (2014), scrutinizing the effects from the power of paramount chiefs on economic outcomes and social capital in Sierra Leone. The authors use the amount of political competition as an indicator for power: the more ruling families there are; the more restrictions a chief will face. They find that chiefdoms with fewer ruling families—a legacy from the British colonial administration—are characterised by worse development outcomes today. Autocratic rulers may have little incentive to provide secure property rights over land. Yet, these chiefdoms are also characterised by higher

levels of social capital and respect for the chief's authority. These observations form an important motivation for this thesis.

Micro-economics research is even better equipped to grasp these heterogeneities and make causal inferences (Besley & Jayaraman 2010, p.3). Increasingly, game-theoretic tools and behavioural experiments are used to understand how specific institutions shape incentives and how resulting decision-making may contribute to economic outcomes (*ibid*, p.1).

1.1.3 Local institutions and rural development

Stimulating rural development is prominently back on the international development agenda (for example, see World Bank 2007). Although urbanisation is an increasing worldwide trend, in 2013, 63 percent of the population in Sub-Saharan Africa were living in rural areas. In addition, about 70 percent of the worlds' poor depend on agriculture as their main source of income and employment. For these reasons, investing in rural development seems to be a viable way towards livelihood improvements and poverty reduction (Byerlee et al. 2009). Local institutions fulfil an important coordinating role in village economies, by regulating scarce production factors, for example through organising communal tenure rights and labour rotation systems. Fafchamps & Gubert (2007) write that 'interpersonal relationships have long been suspected of shaping agrarian institutions, probably because weak formal institutions must be supplemented by interpersonal trust. This is particularly true for informal risk sharing: a fundamental risk coping mechanism for the rural poor' (p.75).

This thesis analyses how local institutions may affect rural development in Africa in the context of local communities. More specifically, this thesis addresses how kinship networks and local governance structures might contribute to economic development, or how they could form an obstacle to it. In addition, I analyse how an external intervention may contribute to development outcomes—in interplay with the institutional environment. The core chapters in this thesis present four individual, but interrelated research articles. The chapters are based on original household level data that have been collected in Liberia. Throughout these chapters, local institutions form the thread.

This thesis is motivated by two key observations that are proposed by Matthews, one of the founders of the New Institutional Economics. The first observation is that 'institutions matter'. The second one is that 'the determinants of institutions are susceptible to analysis by the tools of economic theory' (Matthews 1986, p.903). The economics of institutions may be divided into four levels (Williamson 2000). The first level—the embeddedness level, includes informal institutions, customs, traditions, norms and religion, characterised by very slow change (up to 1000 years). This is the domain of the 'social theory'. The second and the third level are the domain of the New Institutional Economics. These levels represent the institutional environment or the formal rules of the game (i.e., economics of property rights), and the governance—or the 'play of the game' (i.e. transaction costs economics). Respectively, they may take up to 100 and 10 years to change. Finally, the fourth level is the domain of the neoclassical economics, dealing with resource allocation and employment, which is subject to continuous change. The institutions addressed in this thesis are largely part of the embeddedness level. For example, in Chapter 3 I address the implications of kinship networks. In Chapters 4 and 5 I look at the role of local leadership, which is strongly rooted in tradition and customs too, but which is also part of the 'play of the game'. Via resource allocation and employment, these institutional dimensions may affect economic outcomes.

This thesis speaks to three main literatures. In the first place, this thesis contributes to the literature on the potential adverse effects of sharing obligations that are related to kinship networks. I use a detailed measure of family network density, which I believe improves on some of the measures used in recent literature on this topic. Secondly, the research contributes to the micro-economic literature about the effects of corruption on economic outcomes. Finally, this thesis speaks to the literature on experimental evaluations of community projects aiming at improving rural development. Most impact evaluation literature focusses on projects either aiming at strengthening social cohesion in post-conflict societies, or increasing agricultural production. The project I evaluate aims to address both dimensions.

The remainder of this chapter provides an overview of the key concepts in this thesis, and the relationships between them. Subsequently, it describes the objectives of the

thesis and the research questions that will be addressed. Finally, the chapter provides a brief overview of the methodologies used and presents an outline of the thesis.

1.2 Linkages between local institutions and aid

Institutions form a 'social infrastructure' in a society, by creating the rules of the game conducive to desirable economic behaviour (Hall & Jones 1999; Rodrik et al. 2004). They are sometimes seen as part of a wider definition of 'social capital'. According to this definition, social capital includes structures such as institutions of the state and rule of law on the macro-level, and local institutions and social networks on the micro-level, as well as the governance, trust, local norms, and shared values that are produced by these structures (Grootaert & Van Bastelaer 2002).[3] In economic literature micro-level social capital is increasingly being identified as an important factor contributing to development and growth (e.g., Zak & Knack 2001). Being based on long term relationships, social networks provide secure environments to trade. Strong social preferences, such as trust and reciprocity, replace the need for formal contracts and costly monitoring. High levels of social capital are therefore related with lower transaction costs, which may enable trade.[4]

Social capital is generally divided into 'bonding' and 'bridging' social capital. Bonding social capital refers to ties within social groups, while bridging social capital connects different groups of people and individuals who are only loosely connected (e.g., see

[3] According to other definitions, institutions form the 'hardware', by providing formal or informal property rights and rule of law while social capital refers to shared norms and social values. In any definition, institutions and social capital are closely interrelated.

[4] Coleman (1988, p.S.99) provides a famous example of the diamond market in New York: 'Observation of the wholesale diamond market indicates that these close ties, through family, community, and religious affiliation, provide the insurance that is necessary to facilitate the transactions in the market. If any member of this community defected through substituting other stones or through stealing stones in his temporary possession, he would lose family, religious, and community ties. The strength of these ties makes possible transactions in which trustworthiness is taken for granted and trade can occur with ease. In the absence of these ties, elaborate and expensive bonding and insurance devices would be necessary—or else the transactions could not take place.'

Field 2003). Whereas the latter type of social capital might facilitate cooperation and innovation, the strong ties that characterise bonding social capital may form an impediment to change. Social networks characterized by strong common norms and traditions tend to be more averse to change, which could impede growth (e.g., Miguel 2005). In addition, long-standing social networks may facilitate patron-client relationships that could benefit a small elite at the expense of a poor majority. Francois et al. (2015) study this phenomenon in the context of seemingly democratic elections for local governments in Indian villages. They find that elite minorities undermine policies that would redistribute income toward the majority poor through a widespread system of vote-buying. Hence, like any social network, 'communities work because they are good at enforcing norms, and whether this is a good thing depends on what the norms are' (Bowles & Gintis 2002, p. F428).

The key themes addressed in this thesis are family networks, corruption and aid, and their relationships with social cohesion and economic development, as visualized in Figure 1. The linkages between these concepts are briefly mentioned below, and more elaborately discussed in the remainder of this section.

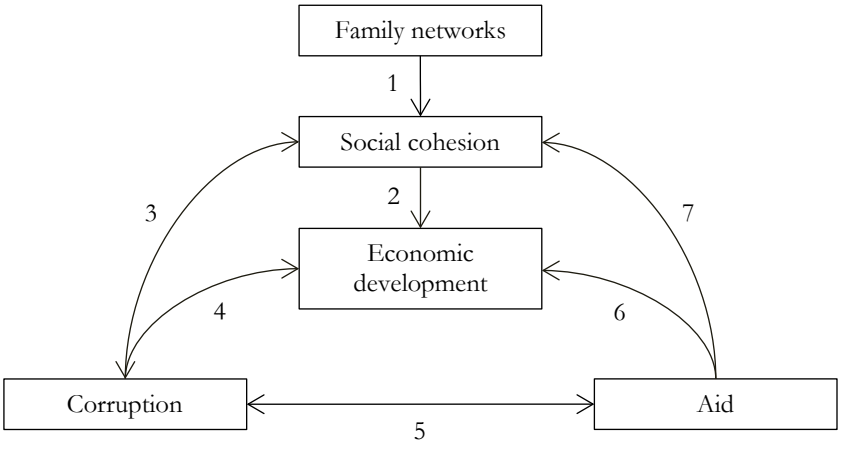

Figure 1.1: Family networks, corruption and aid

Family networks are characterized by high levels of social cohesion that reproduce shared norms [linkage **1**]. Social cohesion and its by-products, in turn, may be

conducive to economic development, but can also form an obstacle to change [linkage **2**]. Through patron-client networks, **corruption** can be one of the by-products of strong social cohesion. Reversely, networks characterised by strong social cohesion can provide monitoring mechanisms that reduce the incidence of shirking within the network. Simultaneously, corruption may affect cooperative norms in social networks [linkage **3**]. Corruption is widely believed to have direct and indirect negative effects on economic development, and economic development may in turn affect the incidence and size of corruption [linkage **4**]. Corruption (or governance quality in general) may indirectly affect economic outcomes through the impact of **aid interventions**, which is likely to be smaller in the context of corruption. In recent years, since the mediating role of institutions for development is recognised widely, aid interventions increasingly aim to improve institutional quality and to strengthen social cohesion, for example in order to combat corruption [linkages **5** and **7**]. Most aid interventions, however, are aimed at contributing to development by improving economic outcomes in the first place [linkage **6**].

1.2.1 Family networks and social cohesion

Family forms the most primitive institution in society, shaping economic outcomes, legal institutions and social preferences, even on the macro-level. Duranton et al. (2009) find that different types of medieval European family systems could explain current regional disparities in education levels, social capital, labour participation, wealth and inequality across Europe. This indicates that ancient family institutions might have been internalized in modern institutions, despite migration and economic development.[5] Family networks are characterised by strong social cohesion that reproduce shared norms [linkage **1**].

Unconditional sharing norms are among the key norms in family networks. They are rational from an evolutionary point of view, as other types of networks do not provide

[5] For example, countries characterized with strong family ties feature less generalized trust and lower civic sense (Alesina & Giuliano 2013).

this kind of unconditional support.[6] Through these shared norms, social cohesion may either contribute to economic development, or hamper it [linkage **2**]. The provision of mutual insurance is the most prominent instrument that might contribute to growth. Members of extended family networks can rely on each other in times of need: sharing obligations are a central element of typical family networks, as family networks provide informal insurance schemes and pension funds (Fafchamps 2011; Fafchamps & Gubert 2007). By acting as insurance mechanism, family networks may reduce idiosyncratic risks and risk-aversion, which could encourage investments through information sharing. Family networks also might be conducive to technology adoption (Bandiera & Rasul 2006). However, unconditional sharing obligations also provide ample opportunity for free-riding and shirking: if one can always rely on a slightly richer family member, investment incentives might well be discouraged (for example, working hard or the accumulation of resources). Sharing obligations are often enforced by strong egalitarian norms, and by fear of repercussions.[7]

A growing body of micro-economic literature provides empirical evidence for the adverse effects of sharing obligations on investments. Dense family networks are for example related with reduced savings and lower investments in education (di Falco & Bulte 2011; Di Falco & Bulte 2015), reduced investments in protection against weather shocks (Di Falco & Bulte 2013), less entrepreneurial activity (Alby et al. 2015), less productive use of resources among entrepreneurs (Grimm et al. 2013), and reduced social mobility (Nordman & Pasquier-Doumer 2015). Sharing obligations within family networks also may lead to costly income hiding strategies (e.g., Baland et al. 2011). Sharing obligations in family networks can thus also lead to additional transaction costs associated with circumventing these obligations. The results from Chapter 3 directly speak to this hypothesis.

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[6] Hamilton's rule predicts that the closer individuals are related through blood-lineage, the more altruistic they are towards each other (Hamilton 1964).

[7] Deviators from the norm may be excluded from the network, accused of witchcraft—a severe stigma throughout Sub-Saharan Africa—or even killed (Platteau 2009).

1.2.2 Corruption and the quality of governance

Research about the role of corruption in economics has a long history (e.g., see Rose-Ackerman 1975 and Shleifer and Vishny 1993). Often defined as 'the misuse of public office for private gain', corruption is a persistent feature of human societies (Aidt 2009; Aidt 2003). We may distinguish two generations of corruption research (see Lambsdorff & Schulze 2015). The first generation consists of cross-country analyses, using corruption perceptions as proxy for real corruption levels. The second generation analyses corruption on the level of individuals, households or firms, and aims to identify causal processes that underlie corruption. This second generation of corruption research mostly relies on measures of petty corruption—or the diversion of public goods for private benefit.

Is corruption really bad for development [linkage **4**]?[8] Some economists argue that corruption sometimes 'greases the wheels of commerce' (Aidt 2009, p. 273), for example by speeding up bureaucratic procedures by paying bribes (e.g., see Leff 1964; and Egger & Winner 2005 for a more recent, empirical contribution). Yet, most economists agree that corruption hampers development. Corruption is often related to a lack of political accountability and disrespect for property rights—two key predictors for economic growth (Aidt 2009). Empirical micro-economic research provides evidence that corruption may hamper development through different channels. By affecting investment decisions, corruption may for example lead to suboptimal allocation of resources or to the choice of less efficient technologies (e.g., Svensson 2003a; Murphy et al. 1991). The results from Chapters 4 and 5 contribute to this literature.

Corruption (and the quality of governance in general) may indirectly affect economic outcomes by weakening the effects from aid interventions [linkage **5**]. It is widely believed that an enabling institutional environment is conducive to the performance of development interventions (e.g., see Jones & Olken 2005 on the quality of national

[8] This relationship is endogenous: economic development might also affect the incidence and size of corruption (e.g., Treisman 2000)

leadership for economic performance).[9] Also on the micro-level it is likely that governance quality matters for village-level outcomes of development projects. Olken (2006) shows that corrupt government officials channel away a large share of the benefit from an anti-poverty program in Indonesia, offsetting the potential welfare gains from the program. Khwaja (2009) relates an objective measure of leadership quality to the upkeep of community projects in 99 rural communities in northern Pakistan. He finds that leadership presence positively affects a group's collective success and that this effect increases with the quality of the leader. Yet, the literature on this topic is limited, as both leadership quality and the outcome of development interventions might be influenced by the same, unobserved variables—'culture', for example.[10]

1.2.3 Aid and development

Whether aid interventions contribute to development is subject to a heated debate [linkage **6**]. A large number of macro-economic cross-country studies have shed light on the question to what extent international aid is contributing to economic growth (e.g. Easterly, 2003; Dalgaard et al, 2004; Burnside and Dollar, 2004; Rajan and Subramanian, 2008). None of these studies found robust evidence for positive effects of aid on growth. The key question is whether countries would have undergone a different development pathway without the aid inflows they received. Without proper counterfactual—a situation without aid—this question remains unanswerable.

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[9] Reversely, on a macro-level, 'conditional aid' is widely applied in an effort to incentivise national governments to limit corruption. The effect of conditional aid is criticised, however (e.g., see Doucouliagos & Paldam 2010; Svensson 2003b).

[10] A number of studies find evidence that a 'corruption culture' exists. Studying parking violations among diplomats in New York City, Fisman and Miguel (2007) find that parking violators are more likely to originate from highly corrupt countries. Barr and Serra (2010) play a bribery experiment with university students and find that the country of origin predicts corrupt behaviour among undergraduates, but not among graduate students, and that the association between corruption and culture may fade over time.

On the other end of the spectrum of development aid, micro-economic research allows for testing causal relationships from a wide range of different project interventions on an even wider range of outcome indicators, by relying on randomized evaluations (Imbens & Wooldridge 2009). Although these studies may often not be generalizable to other countries or contexts and underlying mechanisms that drive the effects sometimes remain underexposed, they do contribute to the puzzle of whether aid works.[11] Findings from these studies are somewhat more hopeful than the bleak picture sketched by cross-country studies: certain types of development initiatives do have promising outcomes for a set of well-defined outcome variables (the evaluation of the impact of deworming treatment among Kenyan school children by Miguel & Kremer (2004) has become a classical example).[12] Still, for many other promising projects there is no evidence of any effects (Easterly 2009).

Aid interventions are traditionally aimed at contributing to economic outcomes in the first place. In recent years, however, since the mediating role of institutions for development has become recognised widely, aid programs increasingly focus at supporting social cohesion as well as other institutional dimensions [linkage **7**]. Improved social relationships are even seen as the very goal of development itself by some economists (e.g., Sen 1999, Narayan et al. 2000).

Institutions-building forms a key element of the recently popular community-driven development approach (e.g., see World Bank 2005). Evidence of the impact from these projects on development outcomes is weak. However, some studies do suggest that development interventions sometimes do positively affect certain measures of social cohesion and collective action (e.g., Fearon et al. 2009). Whereas efforts to build institutions may pay off in the short-term, longer term impact is often absent (Casey et al. 2012). One reason for these disappointing results may be that institutions cannot be built overnight, especially when communities rely on existing institutions and strong shared norms. Sometimes newly introduced institutions might even

[11] See Deaton (2010) for a critical commentary about randomized experiments.

[12] Yet, also this study is now subject of a heathed debate.

undermine existing ones (King & Samii 2014). Alternatively, the benefits of development interventions might be captured by local elites (Mansuri and Rao 2004; Platteau 2004), and thus feed corrupt behaviour [linkage **5**]. The results from the impact evaluation presented in Chapter 6 speak to these literatures.

1.3 Objectives and research questions

The research in this thesis is based on a micro-economic perspective, aiming to establish causal relationships between institutional dimensions and economic outcomes. The objective of this thesis is to analyse how formal and informal institutions at the local level may affect rural development in Africa. More specifically, I analyse whether (1) shared norms produced by social networks and (2) the quality of local governance affect investment decisions. Secondly, the thesis addresses the question to what extent aid interventions may contribute to development, and how they are affected by local institutions—both existing and newly introduced ones.

The individual chapters in this thesis address the following research questions:

1. Do tightly-knit family networks affect economic decision making? (Chapter 3)
2. Does corruption affect public and private investment decisions? (Chapters 4 and 5)
3. Do corrupt leaders target specific social groups? (Chapter 5)
4. Does a rural community training project contribute to improved livelihood outcomes and does this depend on local institutions? (Chapter 6)

1.4 Methodology

'Problems of endogeneity and reverse causality plague any empirical researcher trying to make sense of the relationships among these causal factors [institutions and economic development]' (Rodrik et al. 2004, p. 133). Whereas macro-economists must rely on rare exogenous instruments in order to disentangle causality, randomized experiments provide micro-economists with the possibility to achieve credible causal inferences. The research presented in this thesis is based on observational data

collected through household and community questionnaires combined with a range of field experiments and instrumental variables methods, as briefly described below.

1.4.1 Field experiments

Harrison and List (2004) distinguish between different types of field experiments. The field experiments represented in this thesis are (*i*) artefactual field experiments, more widely known as 'lab-in-the-field' experiments, and (*ii*) natural field experiments (commonly referred to as 'field experiments'). Lab-in-the-field experiments take decontextualized laboratory experiments to the field, and conduct them among a 'non-standard' population. I measure behaviour in a lottery game (Chapters 3 and 4), time preference game (Chapter 3), and a public goods game (Chapters 4 and 6), and link this information to subjects' demographic characteristics and experiences.[13]

'Natural field experiments' are as realistic and unobtrusive as possible (Gerber & Green 2012). This means that subjects are observed in their natural environment, while they are unaware of the fact that they are part of an experiment. I measured the diversion of project inputs by local village leaders, in the context of an actual development intervention; the results are presented in Chapters 4 and 5. Many natural field experiments take the form of 'program evaluations', or randomized controlled trials. The evaluations are designed to gauge the impact of specific interventions on a randomly selected sample of subjects (Gerber & Green 2012). Chapter 6 presents the results from such a program evaluation.

1.4.2 Instrumental variables and observational data

When it is not possible to conduct randomized experiments, then causal inferences can be made using instrumental variables techniques. Macro-economic literature on

[13] These games measure different types of preferences. In the lottery game, I measure subjects' behaviour in a risky, but potentially profitable investment. In the time preference game, I measure subjects' discount rate, or the size of the premium that people want to receive in order to wait two weeks to receive a sum of money. The public goods game measures propensity to cooperate. In the respective chapters the games are explained in more detail.

aid and growth, for example, 'only really became meaningful when the severe problem of reverse causality was addressed with the use of instrumental variables' (Easterly 2009, p.388). Proper instrumental variables should meet two key conditions: they should be valid (i.e., exogenously determined and thus be uncorrelated with the error term) and they should strongly predict the endogenous variable (see Angrist & Pischke 2009). Finding an instrument that truly meets both criteria is difficult and requires some ingenuity. For example, in a widely cited paper Miguel et al. (2004) use variation in rainfall as a clever, exogenous instrument for changes in economic growth to show that negative growth shocks increase the likelihood of civil war outbreak.[14]

In micro-economic studies, instrumental variables techniques have moved from offering solutions to the problem of causal inference to being devices that induce quasi-randomization in project evaluation (see Deaton 2010; Imbens & Wooldridge 2009). This observation is reflected in this thesis. In Chapters 4 and 5 I predict corrupt behaviour by the village chief using characteristics of the chief as excluded instruments. In Chapter 6, I use an instrumental variables approach to estimate the effects of a community training project on project participants, using random project assignment as exogenous instrument.

The results from the field experiments are complemented by observational data. I collected extensive survey data to measure amongst others demographic characteristics, livelihood conditions, and details about social networks. The questionnaires aim to reveal as much as possible about the context subjects live in, and the experiences they have gathered. These variables often function as control variables in the regression models. In other cases, data derived from questionnaires also form key variables: the family network indicators in Chapter 3 are based on a detailed network survey, and most of the outcome variables in Chapters 5 and 6 are based on survey data, too.

[14] Although critics argue that the excludability assumption, that rainfall does not directly affect war, is debatable (Easterly 2009).

1.5 Outline of the thesis

The thesis is structured as follows. **Chapter 2** contextualizes the research that is presented in this thesis. I sketch a picture of Liberia, the country where this study is placed. The chapter presents key events in the Liberian political history, with focus on the protracted civil war that lasted until 2003—the effects of which are still visible today. The chapter also provides a description of the study areas where the data for this thesis were collected and the data collection procedure.

Chapters 3 to 6 form the core part of the thesis. These chapters present individual research articles that have been or will be published in academic peer-reviewed journals.

Chapter 3 is based on joint work with Marcel Gatto and Eleonora Nillesen. We analyse the impact of family networks on economic decision making, relating data from a detailed network survey to people's behaviour in a modified lottery experiment and a time preference game. We find that individuals with a dense family network are more likely to pay a fraction of their endowment to hide their earnings from the experiment, and that dense family networks are related with lower discount rates. These associations are driven by the male subsample. Our results are stronger for family networks characterized by members who have requested financial support before. Our results offer tentative evidence that dense family networks, under some conditions, have adverse impacts on economic decision-making.

Chapters 4 and 5 are based on joint work with Erwin Bulte and Eleonora Nillesen. In two subsequent chapters we explore the effects of the quality of local leadership, using a field experiment to obtain an objective measure of capture by the village chief—a proxy for corruption. **Chapter 4** links corrupt behaviour of the village chief to public and private investment decisions, using a public goods game and a lottery game. Our results show that corruption undermines incentives for voluntary contributions to local public goods, and may reduce private investments of individuals who are subject to rent-seeking by the chief in real life. This impact may be gender-specific and appears to vary with accessibility of communities. **Chapter 5** builds on the results of the previous chapter, and links corrupt behaviour of the village chief to economics activities of villagers. More specifically, we analyse to what extent thieving behaviour

of the chief affects rice planting and trading activities. We find that corruption leads to reduction in rice planted as well as trading activities. We also find that ethnic ties mediate the intensity of stealing pressure: the adverse effects of predation are driven by the responses of those individuals with a different ethnic identity than the chief's.

Chapter 6 assesses the impact of an agricultural community development project on livelihood outcomes and social cohesion. I find weak evidence that the project contributed to higher rice harvests. The project did not contribute to social cohesion. The analysis suggests that the project caused time allocation shifts within households: the project has a robust, positive effect on time spent on farming activities by children. These results are driven by groups where members were most closely involved in design and monitoring quality if the group leader. The results also indicate that direct project outcomes are affected by corrupt behaviour of the village chief.

Finally; **Chapter 7** provides a general discussion of the results, the limitations of the studies, as well as the broader implications of the research presented in this thesis.

2

Setting the Stage

2.1 Overview

This thesis is based on research conducted in Liberia; a small country on the coast of West Africa, bordered by Sierra Leone to the west, Guinea to the north, and Cote d'Ivoire to the east. Liberia has a population of about four million people on a surface of 111,369 square kilometres. In the 1990s the country was stage of one of the most brutal conflicts in Africa. The civil war was characterised by immense cruelty and chaos.[1] It lasted for fourteen years—until 2003.

Liberia consistently ranks among the poorest countries in the world (175/187 on the Human Development Index in 2013) (World Bank 2015). The country is also characterised by massive inequality. Richards et al. (2005, p.6) write that 'the extreme poverty of the many is masked, in per capita income statistics, by the great wealth of a few'. Social inequality has been one of the prime causes underlying the extended violent conflict. The war disrupted the lives of nearly all Liberians, eroded institutions and infrastructure and hampered economic growth. Liberia is still suffering from huge infrastructure deficit and considerable governance, institutional, and capacity constraints, and continuing risks of instability (African Development Bank, 2013).

In December 2013, the first incidences of the Ebola Virus Disease were reported close to the border area of Guinea, Liberia and Sierra Leone. In June 2014, the situation erupted into a full blown crisis that severely affected the entire region. Liberia was most heavily affected. In February 2015, disease transmissions reached 9,007 cases in Liberia alone, with 3,900 reported deaths. Apart from the huge toll on human lives, the disease formed a heavy burden for the agriculture and food sectors. The Food and Agriculture Organisation estimates that losses of paddy rice in Lofa and Margibi counties, two severely affected regions, were as high as 25 percent (FAO & WFP 2014). In addition, trading activities slowed down and food import prices increased; all contributing to a high incidence of severe food insecurity in the country.

[1] See Ellis 2006, for an ellaborate account of the nature of the war, including the key role of magic and spiritual powers.

Liberia does not represent an isolated case. It ranks among other predominantly rural post-conflict countries in Sub-Saharan Africa, such as Sierra Leone and Guinea, characterized by weak governance, high levels of corruption, poor infrastructure, and large aid inflows. The massive eruption of the Ebola crisis illustrates the fragility of the institutions combined with absence of infrastructure. On the other hand, like Sierra Leone, Liberia has made huge progress in the last decade. Coming from 15 years of civil conflict, Liberia and Sierra Leone moved up from being among the world's top-ten most fragile states to positions 24 and 35 in 2014 (although scores worsened in 2015 as result of the Ebola crisis). And after a decade of economic stagnation, the GDP in both countries tripled since the end of the war (The Fund for Peace 2015).

The remainder of this chapter sketches a picture of key events in the Liberian political history providing a context for the research presented in this thesis, and describes the data collection areas.

2.2 Settlers, war and reconstruction

2.2.1 Settlers

Liberia was founded in 1822 by the American Colonization Society to create a home for liberated slaves who, after the abolishment of slavery, were redundant on American plantations. A few thousands of them indeed moved to Africa. These 'Americo-Liberian' colonisers settled in the newly established town Monrovia—called after the American president Monroe, which would be the capital of Liberia. In 1847, the republic of Liberia was formally established.

Even though Liberia has never been colonised officially, the system very much resembled a colonial one. All ruling power was in hands of a small group of elites who did not originate from the country. Only the members of the ruling power had civil rights, and the indigenous population was involved in a system of forced labour, which is best described as slavery, or an apartheid state. By the end of the nineteenth century the Americo-Liberian settlers introduced a complex system of indirect rule, in which governance of 'officially acknowledged' ethnic tribes was organised through the

appointment of chiefs from selected leading families. This neo-patrimonial system maintained the settler rule (Bøås 2005). Under president Tubman, Liberia established partnerships with foreign investors and the presidency through a system of resource extraction (Richards et al. 2005). This 'Open Door Policy' attracted foreign investments from large rubber and mining companies, and doing business with the Liberian Presidency became much easier than with the overseas colonial powers in other African nations. For example, the Firestone Tire and Rubber Corporation provided over 60 percent of state revenues from 1950 to 1970 (Reno 2008, p.393). The labour intensive mining industry and rubber plantations also benefitted from the forced labour system. The economy of Liberia thus closely resembled a 'rentier state': depending on 'unearned' income the rulers did not need to invest in the political apparatus or in local populations, and there was no domestic economic basis (e.g., see Mahdavy 1970). A share of the revenues was channelled to the local chiefs, in turn for governing the hinterlands and to maintain the patronage system (Van der Veen 2002).

In the mid-1960s the Americo-Liberian government—still accounting for only 3 percent of the population—introduced modern hierarchal governance structures in the entire country, including the rural hinterlands. Nevertheless, traditional chiefs continued to play an important role, for example acting as judges in local courts (Baldwin & Mvukiyehe 2011). Gradually, roads and schools were built, and youths from the rural hinterlands could join the army. The years of economic prosperity ended in the 1970s, influenced by the international economic recession, which led to increasing frustration among educated, but unemployed youngsters. After more than 150 years of uninterrupted Americo-Liberian ruling, in 1980 the Americo-Liberian leadership was overthrown in a military coup lead by Samuel Doe.

2.2.2 War

Lacking a strong institutional base, the power of Samuel Doe was weak, notwithstanding his efforts to continue the Americo-Liberian patronage system. The Doe government looted the country to make up for financial shortages, and the economic situation did not improve. In 1985, he was elected as president in fraudulent elections, which triggered a series of coups, answered by a brutal invasion

of Does soldiers into the Nimba district. A group of rebels, expatriates, and exiles, led by Charles Taylor, would become the most important opposition group against the Doe government: the National Patriotic Front of Liberia (NPFL).

The Liberian civil war can be split into three successive periods of fighting: 1989-1991, 1992-1996 and 1999-2003. On Christmas Eve in 1989, the NPFL—supported by soldiers from Burkina Faso, invaded the Nimba district from Ivory Coast. During the march to Monrovia, the army grew explosively and killed thousands of people. In 1990, despite a military intervention from ECOWAS, Doe was killed. A new period of fighting started in 1992, driven by Taylor's presidential ambitions. The fighting soon erupted into chaos, fuelled by a flourishing trade in natural resources—diamonds, mainly. Although the conflict was initially structured along ethnic lines, these divisions became soon unclear and warring factions often also killed their own people (Ellis 2006). In 1995, the Abuja peace agreement was signed, leading to a fragile peace. The international community organised presidential elections two years later: in July 1997, Taylor was elected president by a large majority of the voters, probably in the most democratic elections until then (Bøås 2005; Van der Veen 2002).

Installed as president, Taylor continued his practises of corruption, repression, and exploitation of ethnic divisions, and Liberia was characterised by abject poverty. After few years of relative stability, war resumed in late 1999 (Kieh & Klay 2009). The Liberians United for Reconciliation and Democracy (LURD) started attacking and plundering villages in Lofa county—Taylor's home region, answered by attacks from Taylor's fighters. A second rebel army emerged a few years later, early 2003. This rebel army, the Movement for Democracy in Liberia (MODEL) was supported by the government of Cote d'Ivoire, aiming to end the Taylor regime. In mid-2003, Monrovia was under siege of both groups. Finally, in August 2003, Charles Taylor resigned and went into exile in Nigeria. A peace agreement was signed in Accra between government representatives and the two main rebel groups, LURD and MODEL, which formally ended the war.

2.2.3 Reconstruction

Since the end of the war in 2003 a UN Peace Mission is securing stability in the country. In 2006, Ellen Johnson Sirleaf became the first female president of the

African continent. She was re-elected for a second term of six years in 2011. The end of the civil war also was the start of a large inflow of aid initiatives from numerous non-governmental organisations (NGOs) and UN institutes. Their initiatives initially aimed at assisting the large numbers of refugees in the country. In recent years, the focus has shifted to reconstruction of the economy and society, which were left in complete disarray after the war.

Liberia's level of economic development was low even before the war and well below the average of Sub-Saharan Africa, and the outbreak of the war can at least be attributed partly to chronic poverty (Humphreys & Richards 2005).[2] Reversely, Liberia's GDP plummeted as result of the war. Between 1980 and 1997, per capita income declined by 80 percent (Figure 1 and *ibid.*, p. 11). The Liberian civil war may thus rightfully be called 'development in reverse' (Collier et al. 2003, p.13). During the war, all major infrastructures in Liberia were damaged and looted: transportation and energy provision systems were destroyed and the freefall of the GDP during the war is very likely to be related to this. Although the GDP has been steadily increasing since the end of the war, it is increasing at a much slower rate than in developing Sub-Saharan Africa as a whole. The steady growth can mainly be attributed to the rejuvenation of the rubber and mining companies after the war (FAO & WFP 2014). In 2013, ten years after the peace agreement, the per capita GDP was still below the pre-war level in 1988.

[2] Poverty is related to other factors that might jointly have contributed the outbreak of the war: 'weakness of state capacity, poor regulation of natural resource industries, ingrained corruption, alienation of populations from governmental processes, and rural disaffection, particularly among youths, arising from poor education and employment opportunities.' (Humphreys & Richards 2005, p.9)

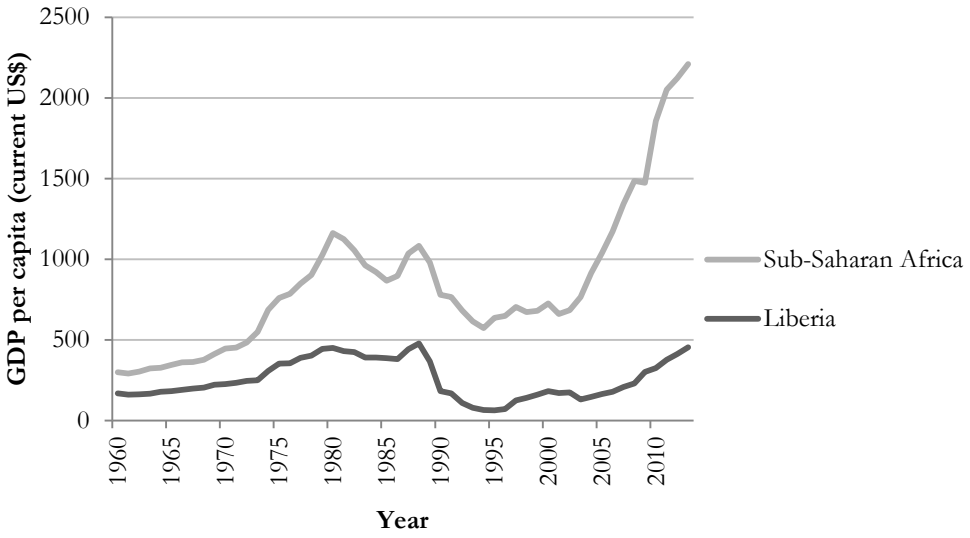


Figure 2.1: GDP per capita for Liberia and Sub-Saharan Africa

Source: World Development Indicators 2015. Available via databank.worldbank.org

Apart from the economic costs of war, the civil conflict came at huge social costs. The most notable human costs are fatalities and massive displacements. War violence has affected the entire country: hundreds of thousands of people were killed or got wounded during numerous attacks, or found their houses looted. Liberia is 'a nation displaced' (Scott 1998), with at least thirty percent of the population being displaced at least once during the protracted civil war (Richards et al. 2005). Some Liberians even recount having been displaced six or seven times during the extended war period (Humphreys & Richards 2005). Many people left their communities and found shelter in other regions of the country. Many others fled the country—mostly to Côte d'Ivoire or overseas to the Unites States.

It is widely believed that social cohesion in Liberia is low in the aftermath of the war (King 2013b). Social exclusion in the pre-war apartheid state was one of the key drivers of the conflict, and the social cohesion that was left was ruptured by the conflict (Richards et al. 2005; Ellis 2006). In a nation-wide survey that was carried out in 2010 among 4,500 Liberians, rebuilding trust is identified as major task for peace building (Vinck et al. 2011). Despite the efforts of rebuilding social cohesion, trust

seems to have been further deteriorating from 2009 to 2010, and perception of corruption of town chiefs increased (Brune et al. 2011).

More than ethnicity or poverty, Liberians perceive greed and corruption from Liberian elites as the most important root cause for the outbreak and continuation of the conflict (Vinck et al. 2011). Corruption played a key role in the pre-war settler state through the institutionalised patronage system. During the war chaos, corruption flourished, which benefitted elites. After the war, many citizens and ex-combatants still benefit from these old networks (Reno 2008). Liberia thus very well represents the 'criminalisation of the state', referring to a state where officials divert public resources for private benefit, using "existing moral and political codes of behaviour, especially those of ethnicity, kinship and even religion" (Bayart et al. 1997, p.15).

Finally, the war also affected food production. During the war years, rice production, the major staple food, declined with 70 percent—related to the large scale migration from rural to urban areas during the war. Consequently, most rice consumed in Liberia is imported and rice production is mostly used for home consumption. In 2006, rice imports—the bulk of agricultural imports—accounted for 60 percent of total rice consumption, and it is estimated that this number will be close to 70 percent in 2015. This makes the country extremely vulnerable for price fluctuations on the world market (FAO & WFP 2014).

2.3 Local institutions in present-day Liberia

Wealth differences within Liberia are generally attributed to institutions part of the Liberian settler state, which continue to affect development in present-day Liberia. Richards et al. (2005, p.28) write that Tubman's Open Door Policy 'produced growth without development. Money from interior resources drained through the Executive Mansion, into the pockets of government supporters or overseas business partners. [...] Interior communities see themselves as impoverished, neglected and not fully part of the national terrain'.

Under the pre-war Open Door Policy, customary land rights greatly suffered, as the central government sold large parts of the land that were under customary tenure. Today, land rights are still organised along a dual system of land tenure. The

government owns and administers public land and indigenous communities are permitted to maintain lineage-based communal tenure. This is a necessity for traditional rice farming rotation systems, which leaves parts of the land fallow after a farming season. However, land has increasingly been acquired, surveyed and deeded by the central government (Richards et al. 2005, p.20).[3] If young people want to gain access to land part of a family reserve, they need to be 'in good standing with the elders who rule the descent group' (*ibid*).

Liberia is divided into fifteen administrative regions, or counties, which are each represented by a county superintendent. Under the superintendent a number of chiefs operate. The paramount chief is the major figure among them; a salaried official who fulfils a mediating role between the government and citizens. Counties are further subdivided into 'clans', which are represented by district chiefs. Village chiefs are at the bottom of the bureaucratic hierarchy, and are the only officials who do not receive financial remittance for their position. State interventions are implemented at the local level through chiefdoms (Richards et al. 2005). Not everybody is sufficiently 'civilized' to qualify as a potential chief in the class-based society of northwest Liberia. Chiefs are typically land-owners, and come from an upper stratum of society. They are formally installed by the government after being appointed by local elites. Their tasks consist of preserving order, mediating in disputes, collecting taxes, and recruiting labour to maintain local infrastructure (Richards et al. 2005, p.19). Evidence suggests that chiefs sometimes (mis)use their power for private gain (Reno 2008; Richards & Bah 2005). Richards et al. (2005) for example report that 'young people frequently complained about the way village elders manipulated cases against them to levy fines, which were then commuted to "free" farm labour' (p.19).

Informal institutions such as co-operative labour associations for rotating farm labour (*kuu*) are widespread. A specific key institution in Kpelle societies—one of the major ethnic groups in Liberia—are secret societies. *Poro* for men and *Sande* for women strengthen gender roles and each come with specific initiation procedures and cults.

[3] See Alden Wily (2007) for a detailed account of historical and present-day land rights in Liberia.

The secret societies also have an important social control function, and especially the Poro fulfils a key administrative role (see Murphy 1980).

2.4 Study area and sampling strategy

The studies presented in this thesis are based on data collected in three districts in two counties adjacent to the capital city Monrovia: Careysburg and Todee district in Montserrado county and Kakata district in Margibi county (see marked area in Figure 2). These districts are—like most parts of the Liberian countryside—characterized by large-scale rubber plantations amidst subsistence farming. The plantations provide labour to many predominantly young, male contract workers. Although the area is fertile and climatic conditions are optimal for rice farming—the major staple crop in Liberia—food self-sufficiency is low. Cassava, also grown by the majority of farmers, is the major substitute for rice. The major ethnic group in these districts is the Kpelle: about 70 percent of the people in our sample belong to this group.

Data were collected between 2010 and 2012 using a stratified clustered random sample of 1500 households in 72 communities. Data were collected in the context of a project evaluation of a development intervention in these regions. We applied a two-stage block-randomization design. In the first stage we selected a total of 72 communities from detailed maps provided by the Liberian Institute of Statistics and Geo-Information Services (LISGIS), stratified on the presence of a main road in the village: half of the villages was located along a main road, the other half of the villages along smaller feeder roads, accessible by motor bike and sometimes only by foot. Note that road condition, even of main roads, is extremely poor.

Figure 2.2: Map of Liberia and study area (in red)

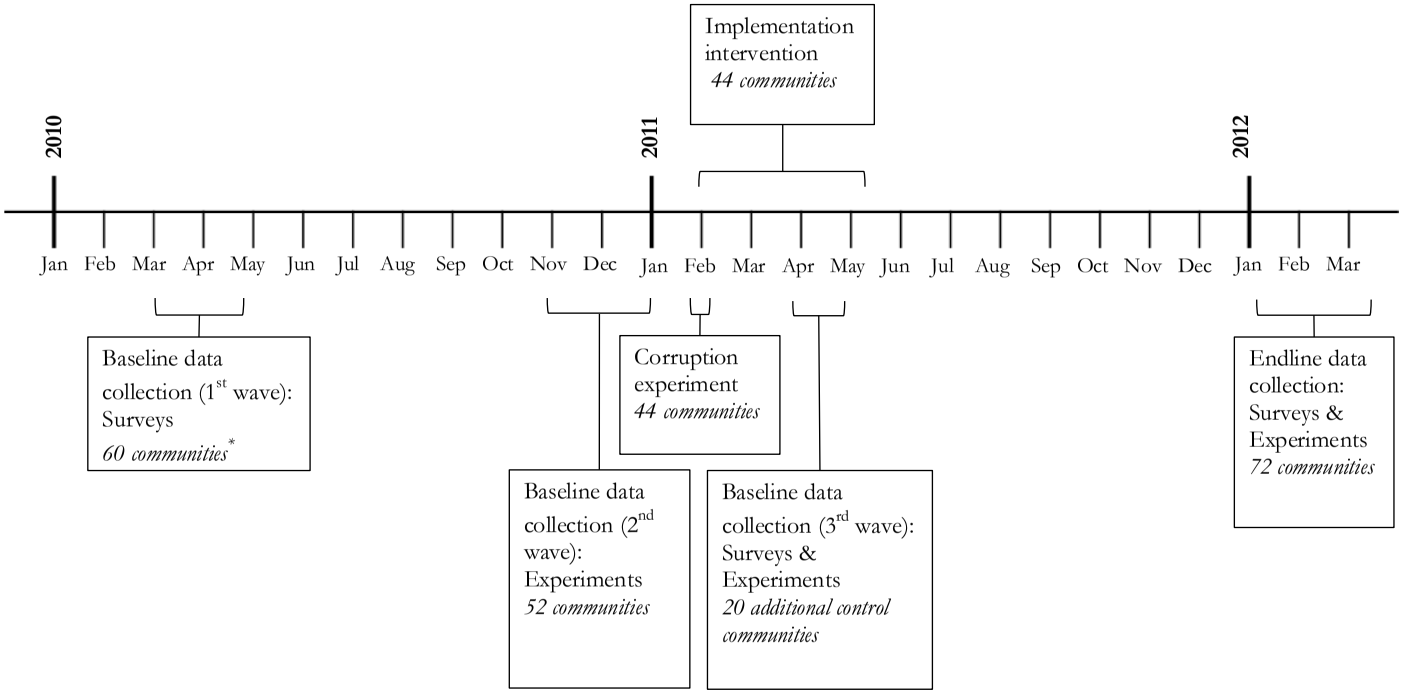
Source: PCL Map Collection. www.lib.utexas.edu/maps/liberia.html

Selection of communities was bound to four conditions: (1) Communities should not have been targeted for the development intervention before; (2) the community should be home to at least thirty households; (3) distance between selected communities should be at least five kilometres (one hour by foot, to limit spill-over effects from the development intervention between villages); (4) the community should be located in an area with farming potential. Communities were selected as follows: first, sixty grids of five square kilometres were randomly selected on detailed county-level maps and in each grid the most central village was chosen. These sixty villages were visited by the team of local experts, who assessed whether the village

passed all four criteria. If not, the village was replaced by the next suitable village along the same road.

In the second stage, sixteen households in each community were randomly selected by means of a public lottery. To this end, a team of enumerators together with the village chief numbered each house in the village and the numbers were transferred on lottery slips. Sixteen households were randomly drawn in a public lottery. Either the household head or the spouse from selected households was eligible to participate in the research activities. All research activities were conducted in 'local English' or Kpelle by a local team of trained enumerators. Whenever possible, the same enumerators were involved during all data collection phases.

Data were collected during different rounds between 2010 and 2012, as visualised in Figure 3. The first round of baseline data collection was conducted in April and May 2010 in fifty-two communities among 832 individuals. In November and December 2010, in each of the fifty-two villages about ten additional household representatives were randomly selected according to the procedure described above, and behavioural experiments were conducted among all twenty-six individuals. Among the newly selected individuals, a short version of the household survey was conducted. Hereafter, the development intervention was randomly allocated to forty-four communities and the intervention was implemented in February 2011. In April and May 20 additional communities were selected, following the same procedure as above. Finally, endline surveys and experimental data were collected between January and April 2012.



* 8 communities were dropped from the baseline sample as they had been part of the project before.

Figure 2.3: Time line of research activities

3

Family Networks and Income Hiding

Evidence from Lab-in-the-Field Experiments in Rural Liberia

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Abstract

This study investigates the relationship between family network density and income hiding in rural Liberia. We link people's behaviour in a modified lottery experiment and a time preference game to detailed information about their family networks. We find that individuals with a dense family network are more likely to pay a fraction of their endowment to hide their earnings from the experiment. This association is mainly driven by male respondents. We also find that men with dense family networks have lower discount rates than those with smaller networks. Qualitative responses suggest that these men perceive us as an alternative bank: if they have no immediate purpose for the money, they prefer the research team to keep it for two weeks. This prevents them from spending it on things other than its intended use, and may keep predatory members of the family network at bay. The negative association between family network density and investment decisions is stronger if these networks are characterised by members who sought financial support in the past. Taken together, our results offer tentative evidence that dense family networks, under some conditions, have adverse impacts on economic decision-making.

3.1 Introduction

Rural households in developing countries are exposed to a wide range of everyday risks, from adverse weather conditions to major diseases and civil conflict. In the absence of formal institutions to facilitate exchange or provide safety nets, people form groups to share risk and to provide economic or social assistance to each other. Members of such groups are often related by birth, marriage and (or) ethnic origin (Hoff & Sen 2006). People appeal to family members in times of need, instead of relying on others, such as friends, for at least two reasons. First, people may be altruistic towards members of their own family (for example, Foster & Rosenzweig, 2001).[1] The Hamilton-rule predicts that the closer individuals are related through blood-lineage, the more altruistic they are towards each other, which is rational from an evolutionary point of view (Hamilton 1964). Second, mutual obligation systems rely on repeated interaction: long-term family ties enable contract enforcement: non-compliance can easily be identified and possibly followed by sanctioning or exclusion (La Ferrara 2011). Also, Cox and Fafchamps (2008) suggest that identification with family groups facilitates guilt and shame in case of default that could account for the often large role of family in non-market exchange.

Norms of reciprocity and trustworthiness are among multiple mechanisms proposed to account for a positive correlation between family networks and economic development. Such networks may for example encourage entrepreneurship; individuals may be more inclined to undertake risky but profitable investments (for example, starting up a business) with the support of strong family relations (see Fafchamps & Minten 1999; Benedict 1968).[2] Alternatively, family networks can relax

[1] Economic literature referenced in this chapter refers to family, kinship networks or both. Kinship differs from family according to the definition of family being 'any form of blood relation' while kinship may encompass 'all socially recognised relations' within a locality. While these concepts are clearly distinct, we believe that their impact on development may be generated in the same way. In our study we focus on family networks.

[2] Although they find that family relations may help in starting up a business, they do not find evidence that these relations are a major determinant of business success.

credit constraints and provide insurance if formal institutions are absent (for example, Bentolila and Ichino, 2006; Fafchamps and Gubert, 2007; Fafchamps, 1992; Greif, 1993; Grimard, 1997; La Ferrara, 2003).

At the same time, strong family relationships may generate cooperative norms that predict negative effects on (economic) development. Scott (1976) and Platteau (1991) highlight the importance of egalitarian subsistence ethics in cultures that promote kinship-based insurance. Bauer and Yamey (1957) elaborate: '… the family system, which is largely indiscriminate in its operation, minimizes the inducement for people to improve their own position because they can count on being provided with the means of subsistence not very different from that of the majority of their kinsmen, including the more energetic, thrifty and able.' Lewis (1955) reports: 'There are many reports from Asia and Africa of able men who have refused promotion because the material benefit would accrue mostly to relatives whose moral claims they do not recognize.'[3] Under strong egalitarian norms an individual's attempt to improve his situation is not always encouraged, and sometimes even (violently) opposed by community members. Sanctions for those who shirk obligations of the kin system include economic retaliation, stigmatisation (for example, accusation of witchcraft) and social ostracism.

What then is the role of family networks in economic development? This is ultimately an empirical question. We contribute to the literature by investigating this matter for a group of rural Liberian smallholders. We use a public lottery choice experiment, wherein subjects—who are randomly assigned to one of three treatments—are given the opportunity to pay to keep their income from the experiment hidden from other people present. We link our measure of willingness-to-pay to hide income to detailed data on family networks.

[3] Geertz (1963) provides anecdotal evidence of Balinese commercial enterprises where job-and loan-seeking kinsmen constantly assaulted successful entrepreneurs. Portes and Sensenbrenner (1993) found that successful male owners of garment and leather artisan shops in Ecuador are often Protestant rather than Catholics. These owners shift religion to remove themselves from social obligations associated with the Catholic Church and its local organisations.

We find tentative evidence that family pressure in Liberia is real: individuals with a denser family network are more likely to pay a fraction of their endowment to hide the outcome of the experiment from their village members. This association is mostly driven by male respondents. We also find that men with dense family networks express lower discount rates in a complementary time preference experiment. Qualitative responses suggest that these individuals are less able to commit to saving money for future use: storing the money with us provides them with a credible claim that funds are not accessible now. As a robustness check we specify the family network in three alternative ways and find similar results.

The remainder of the chapter is organised as follows: section 3.2 reviews literature on the role of kinship in economic decision-making and development; in section 3.3 we describe our empirical strategy; section 3.4 summarises the data and econometric strategy; section 3.5 presents the results, and section 3.6 concludes.

3.2 Kinship and economic incentives

Arguments about potential perverse effects of family networks have received support from some empirical studies in the past, but evidence has remained scarce—especially within the economic domain—and is often anecdotal in nature. This has changed over the last two decades, as scholars increasingly recognise the importance of (in)formal institutions in development, resulting in a number of contributions on kinship networks to the (development) economics literature. In a theoretical article Alger and Weibull (2010) for example demonstrate why weaker family ties develop in (arguably) harsh environments and how this affects economic outcomes. A number of empirical studies demonstrate that strong family ties may lead to actual costs or reduced productivity. Fafchamps and Minten (2002) study the role of social capital on firm productivity and find that better connected traders have larger sales and value added than less connected traders. However, the type of social network matters: strong family relations are negatively correlated with firm performance, possibly due

to pressure by family members to share gains, providing disincentives to efforts. In other words, fear of predating family members is associated with lower production.[4]

Platteau (2000) lists several (costly) strategies that local entrepreneurs may adopt in order to escape demands from fellow kinsmen. These include: (*i*) holding cash (that can be hidden) instead of visible wealth like agricultural output or cattle, (*ii*) having meals outside the home, and (*iii*) taking out small loans mainly used for consumption purposes. Baland et al. (2011) find that members of credit cooperatives in Cameroon simultaneously save and take out loans at the credit cooperative and suggest that individuals behave like this to signal poverty and hence successfully oppose money requests by friends and members of the (extended) family network. Di Falco and Bulte (2011) find that larger kinship networks are associated with (*i*) increased consumption of non-shareable goods, and (*ii*) lower consumption of shareable goods. In a companion article Di Falco and Bulte (2013) find tentative evidence that traditional sharing norms in kinship networks lead to free-riding and weaker incentives to invest in protection against weather shocks.

Alesina and Guiliano (2010) show that in countries with strong family ties people have lower geographical mobility, fewer women and youngsters participate in the labour market, people have more home production and are less likely to participate in market activities. This is arguably related to the fact that in these countries people need to spend more time with their families, which may slow development. Grimm et al. (2013) explain low re-investment rates in West Africa, despite high marginal returns, with forced redistribution through kinship networks. Some studies use an experimental (lab-in-the field) setting to study the role of kinship in development. For example, Hadnes et al. (2013) play a 'real effort' experiment among tailors in Burkina Faso and find that (expectations of) solidarity obligations are associated with reduced

[4] Related, Rooks et al. (2012) write: 'if kinship and business networks overlap too much, redistributive kinship obligations are expected to act as a drain on entrepreneurial resources and an obstacle to entrepreneurial dynamism and innovation' (p. 616). Although it would have been interesting to test this for Liberia as well, our network data unfortunately do not permit us to investigate a possible correlation between kinship and business networks.

productivity. Jakiela and Ozier (2012) measure kinship pressure in rural Kenya through a modified lottery experiment, and find that women (but not men) try to hide their initial endowment, especially with kin present at the game. Our work contributes to this growing literature and is closest in spirit to the article by Jakiela and Ozier.

3.3 Empirical strategy

3.3.1 Context

The experimental design is part of the baseline phase of a larger research project that evaluates the impact of an agricultural development intervention implemented by an international NGO in Liberia.[5] The communities are located near the capital city of Monrovia (reachable within one day), and are characterised by poor infrastructure and livelihood conditions. The main livelihood activities are small-scale agriculture, petty trade and rubber tapping. None of the communities has electricity.

Communities are small (on average 43 households), and social networks are strong. Family networks are particularly dense in some places: in some communities in our sample nearly all community members are related by family. Liberia has been characterised as an over-centralised and predatory state that mainly benefits the urban elite in Monrovia (for example, Reno 1995). As a result, non-state institutional arrangements initiated by local individuals are strongly rooted in Liberian history. Communities or groups of communities are linked on the basis of traditional family ties and often have numerous voluntary groups including youth and women's groups,

[5] For the baseline phase of the impact evaluation, 52 rural communities (townships) were sampled in three provinces in Montserrado and Margibi counties, of which 44 communities were randomly assigned to receive an agricultural development project. Experiments and network surveys used in this current study were part of the baseline data collection process. The development intervention started in February 2011 and lasted until May of that same year. We did not inform participants of the surveys and experiments that they might become part of an intervention in a later stage, nor did we inform enumerators beforehand. Community members' behaviour in the experiments should hence not be influenced by anticipation on a possible future intervention.

rotating credit groups (*susu*), farmers' self-help groups (*kuu*), producers' cooperatives and social clubs. Relationships also persist outside community boundaries: Penn Handwerker (1973) demonstrates that market sellers in Monrovia are expected to spend their profits on maintaining family ties in rural areas.

3.3.2 Sampling design

Household survey data were collected in 52 randomly selected communities in Margibi and Montserrado counties in March and April 2010. Experiments and network surveys were conducted among the same respondents in November and December 2010. We used a stratified two-stage clustered sampling design, with communities as clusters and stratification based on whether the community was connected to a main road or not. We first compiled a roster of all households in the community. Within each randomly selected community we then selected 20-30 household representatives by means of a public lottery.[6] The lottery took place in an open space and could be attended by all community members, in order to assure a transparent and fair selection procedure. In total 1,266 household representatives from 52 rural communities participated.[7] A team of 20 local enumerators collected survey and experimental data. Interviews and experiments were conducted in local English (98 percent) or in Kpelle (2 percent). The household survey includes information about household composition, education and assets.

[6] In communities with 30 households or less, all household heads (or their spouses) were invited to participate, to avoid that only a few households were left out. In communities with more than 30 households, a maximum of 20 households was selected.

[7] Selected households could send either the head or the spouse (if present). Nearly all selected households participated. Note that since the experiments were only conducted some eight months after the baseline we had to retrace all households to invite them for the experiments. We retrieved almost all households: overall non-response/attrition (unfortunately we cannot distinguish between these two in our data) amounted to 7 percent.

3.3.3 Social network analysis

We measured social network structures using a detailed network survey. We used a common 'network within sample' method (see for example, De Weerdt, 2004; Krishnan and Sciubba, 2009; Maertens and Barrett, 2013), asking every person within the sample about his or her links to every other person in the sample.[8] Given that we randomly selected our participants within each community, network characteristics measured in the network survey are representative for network characteristics in the entire community.[9] Network density indicates the fraction of potential links *j* (out of [n – 1] other respondents in the community sample) that individual *i* is related to (regardless of whether this relation is confirmed by the other individual *j*) (see Jackson 2008). In addition, we propose three alternative measures to this definition. In the 'reciprocity network' we define individuals *i* and *j* related if *both* individuals state they are related to each other. In the 'maximal network' we define individuals *i* and *j* related if *either* individual *i* or individual *j* state they are related to the other. The reciprocity network is the most conservative definition of the family network and the maximal network the most liberal. Third, we introduce a measure of family networks that includes blood-linkages only, excluding in-law family members.

3.3.4 Artefactual field experiments: lotto and time preference

We performed two artefactual field experiments: a lottery experiment and a time preference experiment.[10] We played a lottery game to introduce an exogenous shock

[8] Specifically, we asked each person in the group the following set of questions about each of the other persons in the group: 1) Do you know this person? If yes: 2) How are you related to this person? (1=own family: blood relation; 2=in-law family: related through marriage; 3=neighbours; 4=friendship; 5=land rental; 6=labour transaction; 7=political group; 8=religious group; 9=commercial relationship; 10=other) 3) On a scale of 1-5, how would you value this relation? 4) How often do you meet?

[9] Note that, by design, we exclude network relations outside the community. See Santos & Barrett (2008) and Chandrasekhar & Lewis (2011) for discussions on the use of partial network data.

[10] Harrison and List (2004) define artefactual field experiments (AFEs) as lab experiments played with a 'non-standard subject pool' (Liberian farmers in our case).

of earnings to our sample of participants. Participants were randomly-assigned to one of three treatments, each involving a risky but potentially profitable choice.[11] Participants allocated to the first treatment were given a possibility to hide (a part of) the earnings they obtained in the lottery game. For this we used a modified Becker-DeGroot-Marchak (BDM) mechanism in which participants were asked to state the highest price they would be willing to pay to hide their earnings from the rest of the group—their 'willingness to pay' (WTP). If this WTP was equal to or exceeded a reservation price (set by the researchers but unknown to the participants) earnings were kept private; if their WTP was lower, the outcomes were made public. Since participants knew they could not influence the reservation price, nor would they have any inclination on how high this price might be, we would expect them to reveal the value of secrecy (hiding) truthfully.[12] This allows us to test the hypothesis that respondents with higher kinship pressure are more likely to pay for hiding than others.

The game was played after all instructions had been completed. For participants in group (1) this implied that prior to making a choice in the lottery they *(i)* disclosed their WTP to the researcher in private and *(ii)* were subsequently informed whether their WTP was sufficiently high to hide information about the investment and outcome of the game from all other participants. Each participant was then invited to make his or her investment decision in private. Subsequently, each individual was presented a bag that contained two cards: one marked, and one empty. When the participant drew the marked card, the participant won and gained 4 times the amount invested. When the empty card was drawn, the participant lost the amount invested and received nothing.

[11] See the Appendix for an extensive description of the lottery experiment and experimental instructions.

[12] As an alternative, and closer to the original BDM mechanism, we could have drawn reservation prices from a random distribution of prices, known to the group. Yet to minimize the cognitive burden on our participants we decided to use only one price. This clearly has a disadvantage as participants may try to guess at what the reservation could be rather than stating their true value. If so, however, we would not expect people with denser networks to be better at guessing the correct price. Moreover, the key objective of this paper is not to elicit exact reservation prices for people's willingness to pay for secrecy, but rather to investigate whether family networks influences people propensity to do so.

Time preferences may be associated with family networks through a similar mechanism as hypothesised for the investment game: strong family networks may be related to lower discount rates if people expect their family network to act as an insurance mechanism when facing adverse shocks in the short term. An alternative and arguably less optimistic hypothesis is that people consider the research team as a savings account or bank where they can safely store their money for two weeks to keep predatory family members at bay. We may on the other hand observe higher discount rates among participants with strong networks if people choose to spend their money as quickly as possible, before other members can lay their hands on it. As with the lottery experiment, it is difficult to predict which effect will dominate.

In the time preference experiment, participants were invited to make a set of ten choices between receiving 100 LD the next day, or 100 LD plus a premium r after two weeks (where r = 0, 20, 30, …, 100 LD). The time horizon is thus 13 days. A respondent will choose the immediate 100 LD payment if and only if

$$(1-\partial)100 \geq (1-\partial)^{14}(100+r) \Leftrightarrow \partial \geq 1-\left(\frac{100}{100+r}\right)^{\frac{1}{13}} \qquad (1)$$

where ∂ is the daily discount rate. After completing the research activities in the community, one of the ten rounds was randomly drawn to be paid out—the next day or after two weeks—according to the choices made.

3.4 Data and econometric strategy

3.4.1 Data

Panel A in Table 3.1 summarises our dependent variables—WTP and amount invested in the lottery experiment. 70 percent of the participants in the public-price treatment were willing to pay some positive amount to keep investment and outcome secret. On average, they paid 10 LD, which is below the reservation price of 15 LD. The average amount invested is 26.1 LD, which is 37 percent of the initial endowment of 70 LD.

Table 3.1: Summary statistics

Variable	Obs.*	Mean	SD	Min	Max
A. Experimental data					
Respondent paid a positive WTP (b)**	397	0.703	0.458	0	1
WTP	397	10.479	8.828	0	45
Amount invested in lottery	1236	26.137	18.384	0	70
Risk premium (r)	1222	44.157	40.792	0	100
Daily discount rate (∂)	1222	0.025	0.021	0	0.05
B. Network data					
Family density (standard)	1236	0.355	0.283	0	1
Family density (reciprocal)	1236	0.161	0.164	0	0.82
Family density (maximal)	1236	0.519	0.235	0	1
Family density (excl. in-law)	1236	0.238	0.219	0	1
C. Individual / household controls					
Interview in local English (b)	1236	0.976	0.154	0	0
Respondent is female (b)	1236	0.531	0.499	0	1
Respondent is single (b)	1236	0.063	0.243	0	1
Age in years	1236	42.375	14.993	16	94
Years of education	1236	2.461	4.048	0	16
Household size	1236	4.754	2.269	1	15
Socio-economic status (SES) index	1236	0.127	0.080	0.00	1.06
D. Network characteristics					
Financial transfer (b)	1236	0.286	0.452	0	1
Relative socio-economic status (SES)	1236	0.958	0.818	0	13.58
Food shortage in family network	1236	0.112	0.149	0	1

Notes: * The sample consists of 1,266 observations. 30 observations were disregarded because of missing observations for some control variables. For the 'discount rate' variable, 14 'multiple switchers' (people with more than one switch point) were disregarded. ** (b) indicates a binary variable.

Table 3.2: Summary statistics time preference game

Amount in two weeks (LD)	r	∂ (lower bound)	∂ (upper bound)	Frequency	Percent
100	0	-inf	0	309	25.22
120	20	0	1.4	343	28.00
130	30	1.4	2.0	55	4.49
140	40	2.0	2.6	38	3.10
150	50	2.6	3.1	43	3.51
160	60	3.1	3.6	20	1.63
170	70	3.6	4.0	14	1.14
180	80	4.0	4.4	19	1.55
190	90	4.4	4.8	12	0.98
200	100	4.8	5.2	24	1.96
>200	>100	5.2	inf	346	28.24

Table 3.2 shows summary statistics for the time preference experiment, reporting both the risk premium r for waiting for two weeks and the corresponding daily discount rate ∂. 25 percent of the participants have a zero discount rate, and prefer to receive 100 LD in two weeks instead of the next day. Over 50 percent of the participants have a very low daily discount rate (of 1.4 percent or lower). 30 percent of the participants always prefer to receive the amount offered on the next day, instead of in two weeks. These respondents have a daily discount rate of at least 5 percent. The average discount rate is 2.5 percent (but this amount may be under- or overestimated, as our risk premiums are censored at 0 and 100, while some people may prefer higher or even negative risk premiums).

We present our network variables in Panel B. According to our default definition, family networks have an average density of 36 percent (either direct family, or in-laws), with a standard deviation of 28 percent. In addition, we introduce three alternative specifications for network density. According to the more conservative 'reciprocity network' definition, 16 percent of the people are related to one another. According to the most liberal definition, 52 percent of the people are related. 24 percent of the respondent in our sample are related by blood lineage (excluding in-law family relationships).

Individual respondents' and household characteristics are tabulated in Panel C. 53 percent of our respondents are female and 6 percent are unmarried (single). The average respondent is 42 years of age, with a standard deviation of 15 years; ages range between 16 and 94. On average, respondents have had 2.5 years of education.[13] Household size ranges between 1 and 15 household members, and the average household is home to nearly 5 household members. We constructed a socio-economic status (SES) index by taking the first principle component of a set of asset indicators (including housing materials, assets and livestock) to signal the livelihood status of the respondents.

3.4.2 Hypotheses and econometric strategy

Our interest is in testing the idea that kinship pressure can induce people to adopt costly strategies to avoid sharing gains within their kin network. We hypothesise that (*i*) the willingness to pay in order to hide the investment decision and outcome from the group increases with family network density. If family networks have strong sharing norms, then people with large families would have an incentive to hide their earnings from the rest, as long as the costs are lower than benefits from hiding. The relation between (*ii*) outcomes on the discount rate and family network density may again either be positive or negative. People with large family networks may want to spend the money immediately, to avoid any future family claims, or may be more willing (able) to wait if they credibly rely on their family networks as an informal insurance mechanism, implying a positive or negative correlation between discount rates and network density. Yet, if people want to spend the money on a designated purpose in the (near) future but still wish to avoid family claims, they may opt to store the money with us for a while (thereby making a credible claim towards family that the money is not available to them now), resulting in a negative correlation.

[13] Although an average of 2.5 years of education is low, most respondents are used to make basic calculations on a weekly basis when buying and selling on the market.

We start by testing hypothesis (*i*) using two models. We estimate the correlation between family network density (value) and the *propensity* to hide investment decisions and outcomes using a probit model, where willingness to pay (*WTP)* takes a value of 1 if participants have paid a positive amount to hide their investment and 0 if they did not.

$$WTP_{ij}=\gamma_1 Fam_{ij}+\gamma_2 X_{ij}+\gamma_3 Comm_j+\varepsilon_{ij}\,, \qquad (2)$$

where

$$WTP_{ij}=\begin{cases} 1 & if\ WTP_{ij}>0 \\ 0 & if\ WTP_{ij}=0 \end{cases}$$

WTP_{ij} is explained by X_{ij}, a vector of individual and household characteristics (sex, marital status, age, education, household size and socio-economic status), community fixed effects ($Comm_j$) and error term ε_{ij}. We then proceed with a model (where WTP_{ij} in equation (2) is a continuous variable with a lower limit of 0) to estimate the impact of family network density (value) on the *amount paid* to hide using a Tobit regression model.

Hypothesis (*ii*) we test with a model as depicted in equation (3),[14] where ∂ indicates the revealed daily discount rate (the point where participants prefer to $100+r$ in two weeks instead of 100 on the next day, where r ranges from 0 to 100).

$$\partial_{ij}=\gamma_1 Fam_{ij}+\gamma_2 X_{ij}+\gamma_3 Comm_j+\varepsilon_{ij} \qquad (3)$$

3.5 Regression results

3.5.1 Main regression results

Our first set of regression results is reported in Table 3.3. Columns (1)-(4) report results from the probit regression of an individual's propensity to pay to hide earnings

[14] We use an interval regression model as observations are not continuous and censored at 0 and 1.

from the game. We present four models. We start with a parsimonious model excluding household controls and including community fixed effects in column (1); followed by the same model including all controls and community fixed effects in column 2. In column 3 and 4 we split the sample according to males and females to investigate heterogeneous effects of family pressure.

We find a positive association between family network density and hiding-behaviour in columns (1)-(2). The male subgroup seems to drive the results: we find that men in our sample are more likely to be willing to pay a positive amount to keep income hidden (columns 3-4). This could be interpreted in terms of family pressure. Perhaps men are more likely to be seen as the family spokesman, engage more in social family happenings and hence are simply more often approached for these types of demands. The average marginal effect for the family density variable is 0.18. This implies that a 1 percent increase in family network density increases the likelihood that a person will pay for hiding (positive WTP) by 0.18 percent. This implies an increase from 70 to 72 percent. If the respondent is male, a 1 percent increase in family density would increase this likelihood by 0.46 percent (from 71 to 74 percent).

Columns (5)-(8) report results for a Tobit regression model for a continuous WTP measure on family network density (lower limit set at 0). We cannot identify any correlation between family networks and the amount of money paid for hiding. Perhaps the variation within our WTP variable is not sufficiently large and (or) measured with noise. Another reason might be that WTP levels are only responsive to dense family networks with *specific* characteristics (also see section 3.5.3).

Next, we turn to our control variables. We find that paying-for-hiding appears to be less likely for individuals who are single (that is, not married or living together with a partner): these respondents are 28 percent less likely to pay for hiding than others. This suggests that aside from family-pressure outside the household, intra-household may also invoke hiding behaviour. Or, being single is another proxy for (low) family density in the community (correlation coefficient is -0.05). A higher socio-economic status is associated with less hiding: potential earnings from the experiment might be too small for better-off households to hide from their kin (a 1 percent increase in SES index is related to a decrease of 1.06 percent in willingness to pay a positive amount

for hiding). Note that these three control variables are significant in all specifications of the probit model except for the female subsample.

Our results are generally robust to our three alternative specifications of the family network although overall standard errors slightly increase and some results now become marginally insignificant (see Table A3.3 in the Appendix).

Table 3.3: Regressions of willingness to pay on family networks

	Probit (marginal effects)				Tobit			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	All	Female	Male	All	All	Female	Male
Family density	0.188*	0.184*	0.142	0.460**	2.279	1.666	-2.017	4.404
	(0.110)	(0.113)	(0.190)	(0.214)	(2.413)	(2.408)	(3.272)	(3.420)
English		0.129		0.037		1.039	-0.072	-3.019
		(0.242)		(0.427)		(4.140)	(5.454)	(6.841)
Female		-0.007				0.464		
		(0.064)				(1.370)		
Single		-0.280**	-0.108	-0.469***		-6.009**	-1.701	-9.762***
		(0.138)	(0.257)	(0.165)		(2.778)	(4.056)	(3.728)
Age		0.003	0.006	-0.002		0.278	0.329	0.146
		(0.010)	(0.020)	(0.018)		(0.212)	(0.322)	(0.307)
Age squared		-0.000	-0.000	-0.000		-0.003	-0.003	-0.002
		(0.000)	(0.000)	(0.000)		(0.002)	(0.004)	(0.003)
Years of education		0.001	-0.001	-0.004		0.041	0.048	0.073
		(0.008)	(0.022)	(0.011)		(0.163)	(0.329)	(0.187)
HH size		0.021	0.023	0.010		0.384	0.421	0.312
		(0.014)	(0.027)	(0.023)		(0.295)	(0.470)	(0.384)
SES index		-1.059**	-0.876	-1.490**		-8.540	-15.59	-1.766
		(0.458)	(0.992)	(0.741)		(9.169)	(14.52)	(11.72)
Constant					10.00***	3.860	4.258	12.46
					(3.838)	(7.324)	(9.894)	(10.77)
Village FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	362	362	144	142	397	397	206	191
Pseudo R^2	0.131	0.164	0.137	0.176	0.045	0.051	0.083	0.074

Notes: Standard errors in parentheses. * indicates significance at the 10% level; ** indicates significance at the 5% level; *** indicates significance at the 1% level. Marginal effects in columns (1)-(4) are calculated at the mean.

3.5.2 Alternative network characteristics

Next, we zoom in on network characteristics that could further explain our results. The extent of family pressure is probably correlated with the likelihood that members of the family network will actually exert pressure to share funds. We have identified a number of network characteristics that may affect the probability that family members indeed capitalise on opportunities to share in the resource wealth of their network members. These include: (*i*) whether a person has had previous demands for financial transfers by one of his network members; (*ii*) a person's relative socio-economic status within his or her family network (people with a relatively high socio-economic status within their family network are probably more likely to be approached for financial support than those with a relatively lower socio-economic status); and (*iii*) whether someone in this person's network has experienced (self-reported) food shortages in the past year (in the occurrence of food shortages, people are probably more likely to request support from family members).

Panel D in Table 3.1 shows the summary statistics of our selected network characteristics. 29 percent of the respondents have been asked for a financial transfer (loan or gift) by at least one person in their network. The relative socio-economic status (SES) is calculated as the SES index of individual *i* divided by the mean SES index of members of the family network. A relative SES-index smaller than 1 indicates that individual *i* is less well off than his family members; a relative SES-index larger than 1 indicates that individual *i* is better off. By construct, the average relative SES-index is 1.[15] On average, 11 percent of the members of the family networks experienced food shortage in the past year.

[15] Missing values (for people without family network) were replaced by 0, therefore the reported average SES-index in Table 3.1 is slightly smaller than 1 (0.96).

We re-estimate all models and now include an interaction term between family density and a network characteristic to estimate the relative importance of family network characteristics conditional on family density, on investment decisions:

$$WTP_{ik}=\gamma_1 Fam_{ik}+\gamma_2 Char_{ik}+\gamma_3 Fam_{ik}{}^{*}Char_{ik}+\gamma_4 X_{ik}+\gamma_5 Comm_{k}+\varepsilon_{ik} \quad (4)$$

Table 3.4 reports the results from our regression models including interaction terms for each of these four network characteristics. We hypothesised that people who were asked for financial support before, more strongly respond to family pressure. Indeed, we find that the willingness to pay for hiding (both the propensity to pay a positive amount and the level of payment) increases if people with dense family networks have been requested financial transfers in the previous year by members from their network (see columns 1 and 2 in Table 3.4). Columns 3 and 4 report the results for our models including the interaction term between family density and the relative SES. We find that family density in combination with the individual's relative wealth in the family network is positively correlated with the size of the willingness to pay for hiding (see column 4). Columns 5 and 6 report the results for our models including the interaction term between family density and the incidence of food shortages in the family network. Again, family networks that are characterised by a high incidence of food shortages are more strongly associated with a higher willingness (incidence) to pay for hiding. The coefficient of the amount paid is also positive but not significant ($p<0.15$) (see columns 5 and 6). Results are robust to our set of alternative specifications of the family network. Generally, results become slightly stronger for the 'reciprocity network', and remain the same for the 'maximal network' (see Table A3.4 in the Appendix).

Table 3.4: Role of mediating factors on willingness to pay for hiding

	Financial transfers		SES status		Food shortages	
	Probit	Tobit	Probit	Tobit	Probit	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)
Family density	0.039	-0.742	-0.049	-5.029	-0.068	-2.103
	(0.129)	(2.767)	(0.204)	(4.073)	(0.172)	(3.707)
Transfer	-0.107	-2.820				
	(0.108)	(2.121)				
Family density × Transfer	0.519**	7.984*				
	(0.233)	(4.559)				
Relative SES			0.008	-1.615		
			(0.088)	(1.774)		
Family density × Relative SES			0.221	7.385**		
			(0.175)	(3.545)		
Food shortage (b)					-0.558**	-13.36**
					(0.276)	(6.048)
Family density × Food shortage					2.276**	36.08
					(1.162)	(24.83)
Constant		7.313		8.985	1.507	7.081
		(8.182)		(8.208)	(1.145)	(8.125)
HH controls	Yes	Yes	Yes	Yes	Yes	Yes
Community FEs	Yes	Yes	Yes	Yes	Yes	Yes
N	362	397	362	397	362	397
Pseudo R^2	0.179	0.052	0.169	0.052	0.176	0.053

Notes: Standard errors in parentheses. * indicates significance at the 10% level; ** indicates significance at the 5% level. The probit models in columns (1), (3) and (5) report marginal effects (calculated at the mean). Included control variables: sex, marital status, age and age squared, years of education, household size, socio-economic status, and interview language.

Table 3.5: Ordered logit regression of time preference on family networks

	Interval regression (Discount rate)				Probit (zero-discount rate, marginal effects)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	All	All	Female	Male	All	All	Female	Male
Family density	-0.606	-0.545	0.293	-1.383*	0.116*	0.123**	0.047	0.241**
	(0.483)	(0.483)	(0.595)	(0.788)	(0.060)	(0.061)	(0.184)	(0.100)
English		1.630*	1.044	3.961		-0.182*	-0.011	-0.535***
		(0.854)	(0.866)	(2.571)		(0.111)	(0.117)	(0.177)
Female		-0.266				0.011		
		(0.275)				(0.035)		
Single		0.073	-0.106	0.341		0.012	0.070	-0.120
		(0.531)	(0.672)	(0.881)		(0.075)	(0.24)	(0.113)
Age		-0.068	-0.080	-0.077		0.013**	0.007	0.019*
		(0.045)	(0.058)	(0.074)		(0.006)	(0.027)	(0.011)
Age squared		0.001	0.001	0.001		-0.000**	-0.000	-0.000**
		(0.001)	(0.001)	(0.001)		(0.000)	(0.000)	(0.000)
Years educ.		-0.059*	-0.066	-0.057		0.014***	0.014	0.016***
		(0.034)	(0.058)	(0.045)		(0.004)	(0.050)	(0.006)
HH size		-0.143**	-0.075	-0.191**		0.008	0.012	0.0005
		(0.060)	(0.076)	(0.095)		(0.007)	(0.042)	(0.012)
SES index		1.997	0.871	2.932		-0.133	0.197	-0.567
		(1.704)	(1.996)	(2.966)		(0.228)	(0.739)	(0.413)
Constant	4.120***	4.830***	5.015***	2.791				
	(0.846)	(1.605)	(1.799)	(3.368)				
Village FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1222	1222	646	576	990	990	494	441
Pseudo R^2					0.142	0.158	0.192	0.197

Notes: Standard errors in parentheses. * indicates significance at the 10% level; ** indicates significance at the 5% level; *** indicates significance at the 1% level. Marginal effects in columns (5)-(8) are calculated at the mean.

3.5.3 Results from the time preference experiment

Table 3.5 presents results for our time preference experiment. We estimate the same four models as in Table 3.3. The results in columns (1)-(4) suggest that individuals with large family networks have lower discount rates than others, but results are only significant for the male subsample. Summary statistics in Table 3.2 show that the majority of the observations are clustered at the extreme ends of the distribution (over 50 percent of the respondents report either minimum or maximum possible discount rates). Columns (5)-(8) present results of a probit regression model for zero-discounters. Results are slightly stronger now. Again, this association seems to be driven by men: splitting the sample shows a strong and significant effect for males but this disappears for the female subgroup. Using alternative specifications of the family network standard errors slightly increase but overall findings remain the same (see Table A3.5).

In section 3.4.2 we hypothesised that a negative correlation between family density and discount rates could be explained by at least two potential mechanisms: (*i*) if networks act as an insurance mechanism people may be more inclined to wait for two weeks or (*ii*) leaving the money with us prevents them from spending it immediately or keep predatory family members at a distance. Qualitative responses suggest the second mechanism to be at work. People stated they are often unable to commit to save the money themselves if they were not in direct need of it, and this is arguably even more challenging with predatory family members around. They therefore gladly chose to leave the money safe with us and receive it in two weeks (see Brune et al., 2011 for empirical results on the impact of commitment savings accounts, following a similar argument). However, including family network characteristics as interaction terms does not yield results. With respect to our control variables, we find that discount rates decrease with years of education, consistent with other studies (for example, Harrison et al., 2002). We also find that discount rates decrease with household size, which may be explained by a similar argument as the negative association between family density and discount rates: individuals from larger households may have more difficulty to leave money untouched for two weeks than others.

3.6 Discussion and conclusion

In this chapter we empirically test the relationship between family networks and investment behaviour in a lottery experiment and a time preference experiment. We hypothesised that kinship pressure (*i*) increases the likelihood of people paying to hide the outcome of the experiment from the rest of the group, and increases the amount people are willing to pay to hide; and (*ii*) systematically varies with people's discount rates to receive a sum of money tomorrow or in two weeks.

We find a positive association between family density and propensity to pay a positive amount to hide their earnings from their kin. This result seems largely driven by the male subsample suggesting men are more susceptible to family pressure than women. This may be somewhat surprising, as other studies find that women are particularly affected. Yet, women arguably respond stronger to intra-household pressure than to pressure from the extended family (for example, Anderson and Baland, 2002). Only one household representative (either husband or wife) was allowed to participate in our experiment, and therefore intra-household pressure is not accounted for in our study. Possibly, members from the extended family turn to the (male) household head first with requests for financial support, which might explain why we find that men with dense family networks are more likely to hide income than women (also see David, 1996). We find no robust correlation between family density and the *level* of the WTP. The willingness-to-pay variable may display insufficient variation (and may be measured with noise) to identify a clear correlation. Results from our time preference experiment and its qualitative responses are broadly consistent with our findings on people's propensity to pay to hide earnings from the game. Again, mainly men respond to family pressure.

Family density seems to matter most when it is likely that the family network will actually exert pressure. For example, family density is more strongly associated with an individual's willingness to pay to hide the results of the game when members of the family network previously requested financial transfers, or when members of the family network experienced food shortages. Socio-economic status also matters. Family density is more strongly related with the willingness to pay to hide the results when individuals are on average better off than the members of their family network.

These individuals are arguably the ones who are mostly likely to receive requests for financial transfers.

Taken together, we present tentative evidence that the specific type of family networks can have real economic costs and that the negative impact of family pressure seems to mainly affect the males of the household. Studying under which conditions this role of the family may change we leave as an interesting avenue for future research.

Appendix

Experimental design lottery experiment

A1.1 Set-up

We use a controlled laboratory set-up to measure investments, exploring in turn the possible links between family ties and investment behaviour. Our design is related to the experimental approach of Jakiela and Ozier (2012), who use a modified lottery experiment to test the importance of kinship pressure on investments among Kenyan households. They introduce exogenous variation in the provision of information about additional earnings of community members and have two types of endowment (high and low).[16] We use the same set-up of the lottery experiment with varying levels of information; however, we only have one endowment in which participants can (partially) invest (see also section 3.3.4 in the chapter). Our design also differs with respect to how the independent variable of interest (kinship) is measured. We use a detailed network survey to measure each individual's family network, their frequency of interaction with network members, the type and value of relationship and various socio-economic characteristics (for example, requests for financial transfers in the past, the relative socio-economic status in the network, and the incidence of food shortages) to have a detailed account of what a Liberian family network comprises and to clarify potential mediating factors. We believe this to be a significant improvement over a simple count of relatives.

[16] Jakiela and Ozier have three treatments on the provision of information, and this set of treatments is public knowledge: (*i*) information about earnings in the game is made public; (*ii*) information about earnings in the game is kept private and only know to the participant and research team; (*iii*) participants were assigned a random price between 10-60 shillings and offered the opportunity to keep earnings from the game private *if* they would pay the assigned price. Our design is the same for the first two treatments yet differs from the third. Instead of assigning a random price we offered participants to state their willingness-to-pay (WTP) to keep the outcome private. Only if their WTP was equal to or larger than a non-disclosed reservation price set by the research team, the outcome would be kept private. Furthermore, in our experimental set-up, the set of treatments is not public knowledge but explained individually, as not to confuse participants.

A1.2 Procedure

For the lottery experiment all participants were invited to a public place, where the research team explained the experiments in general terms. Each household representative received an endowment of 70 Liberian Dollars (70 LD is about 1 USD, the daily wage rate for unskilled labour in rural areas at the time of the survey). Participants were informed that they could invest (part of) their endowment in a risky, but potentially profitable investment: the project paid off 4 times the amount invested with a probability of 50 percent. The research team made sure that all participants understood this procedure by using a series of six test questions (in random order). Explaining and testing participants for their comprehension of the experiment would continue until all test questions were answered correctly. After the general instructions had been explained, participants were randomly assigned to treatment groups (1), (2) or (3) of the experiment by letting them pick a folded slip of paper from a bag (see Table A3.1 and A3.2).[17] Each participant was then invited to a private space and was asked to open the slip of paper. The paper contained a number corresponding to one out of three versions of the lottery experiment (1), (2) or (3) and the experimenter explained the respective version of the game.

[17] Despite the randomised assignment of treatments to respondents, two variables significantly differ from each other across treatments. These are respondent age (43 and 44 years in treatments 1 and 2 compared to 41 years in treatment 3) and household size (4.6 in treatment 1 compared to 4.9 in treatment 3). We do control for all variables listed in Table A3.2, and do hence not expect that these cases will bias our estimations.

Table A3.1: Treatments lottery-experiment

Group	Treatment	Freq.	Percent
1	Public-Price	398	32.20
2	Public	416	33.66
3	Private	422	34.14

Table A3.2: Summary statistics for control variables per treatment group and success of randomization

	1. Public-Price		2. Public		3. Private		p-value t-test		
Variable	Mean	SD	Mean	SD	Mean	SD	1=2	1=3	2=3
A. Network data									
Family density (standard)	0.37	0.28	0.36	0.29	0.34	0.28	0.66	0.18	0.38
Family density (reciprocal)	0.17	0.16	0.16	0.17	0.15	0.16	0.59	0.21	0.48
Family density (maximal)	0.53	0.23	0.52	0.24	0.51	0.23	0.35	0.19	0.72
Family density (excl. in-law)	0.24	0.21	0.24	0.22	0.23	0.22	0.85	0.62	0.49
B. Individual / household controls									
Interview in local English (b)	0.98	0.15	0.97	0.18	0.98	0.13	0.34	0.53	0.11
Respondent is female (b)	0.52	0.50	0.52	0.50	0.55	0.50	0.93	0.32	0.27
Respondent is single (b)	0.06	0.23	0.07	0.25	0.06	0.25	0.40	0.60	0.74
Age in years	42.69	15.38	43.75	15.24	40.72	14.24	0.33	0.06	0.00
Years of education	2.57	4.10	2.39	4.06	2.43	3.99	0.54	0.62	0.89
Household size	4.60	2.19	4.72	2.19	4.94	2.40	0.42	0.03	0.17
Socio-economic status (SES) index	0.13	0.07	0.13	0.09	0.13	0.08	0.76	0.83	0.91

Table A3.3: Regressions of willingness to pay on reciprocal family networks

	Reciprocity network			Maximal network			Own family network		
	(1) All	(2) Female	(3) Male	(4) All	(5) Female	(6) Male	(7) All	(8) Female	(9) Male
Panel A – Willingness to pay (Probit, marginal effects)									
Family density	0.361	0.459	0.671*	0.223	0.315	0.392	0.251*	0.082	0.589**
	(0.224)	(0.417)	(0.380)	(0.165)	(0.299)	(0.287)	(0.143)	(0.236)	(0.279)
HH Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Community FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	362	144	142	362	144	142	362	144	142
Pseudo R^2	0.164	0.140	0.168	0.163	0.140	0.160	0.165	0.135	0.176
Panel B – Willingness to pay (Tobit)									
Family density	2.738	-2.323	7.064	3.037	0.0709	2.994	2.617	-2.436	4.835
	(4.792)	(6.940)	(6.625)	(3.533)	(5.303)	(4.731)	(3.098)	(4.113)	(4.475)
Constant	7.073	3.861	6.501	6.585	3.778	5.352	6.978	3.857	5.615
	(8.134)	(11.02)	(12.43)	(8.148)	(11.05)	(12.49)	(8.132)	(11.00)	(12.43)
HH Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Community FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	397	206	191	397	206	191	397	206	191
Pseudo R^2	0.051	0.082	0.074	0.051	0.082	0.073	0.051	0.082	0.074

Notes: Standard errors in parentheses. * indicates significance at the 10% level; ** indicates significance at the 5% level. Marginal effects in panel A are calculated at the mean. Included control variables: sex, marital status, age and age squared, years of education, household size, socio-economic status, and interview language.

Table A3.4: Role of mediating factors on willingness to pay for hiding

	Reciprocity network						Maximal network					
	Probit	Tobit	Probit	Tobit	Probit	Tobit	Probit	Tobit	Probit	Tobit	Probit	Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Family density	0.170	-0.263	-0.555	-14.55*	-0.938	-11.66	0.135	-0.186	-0.0591	-2.994	0.279	1.385
	(0.737)	(5.600)	(1.127)	(7.521)	(1.032)	(7.879)	(0.519)	(3.873)	(0.721)	(4.916)	(0.595)	(4.543)
Transfer (b)	-0.218	-1.305					-0.825*	-6.026*				
	(0.268)	(1.884)					(0.493)	(3.386)				
Family density × Transfer	2.624**	8.060					1.951**	11.32*				
	(1.239)	(7.910)					(0.864)	(5.779)				
SES index			0.022	-1.838					-0.116	-3.153		
			(0.245)	(1.714)					(0.339)	(2.286)		
Family density × SES index			1.472	17.29***					0.671	7.035*		
			(0.918)	(5.806)					(0.590)	(3.881)		
Food shortage					-1.674**	-14.84***					-1.898	-14.27
					(0.733)	(5.426)					(1.405)	(10.83)
Family density × Food shortage					15.47**	107.3**					3.511	17.84
					(6.323)	(45.04)					(3.443)	(26.52)
Constant	0.538	4.426	0.938	6.938	0.902	7.453	0.473	4.244	0.915	6.274	0.677	5.080
	(1.046)	(7.350)	(1.051)	(7.297)	(1.046)	(7.295)	(1.049)	(7.407)	(1.086)	(7.593)	(1.054)	(7.432)
HH Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Community FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	362	397	362	397	362	397	362	397	362	397	362	397
Pseudo R^2	0.177	0.051	0.171	0.054	0.180	0.054	0.177	0.052	0.166	0.052	0.167	0.052

Notes: Standard errors in parentheses. * indicates significance at the 10% level; ** indicates significance at the 5% level; *** indicates significance at the 1% level. Included control variables: sex, marital status, age and age squared, years of education, household size, socio-economic status, and interview language.

Table A3.5: Time preferences

	Reciprocity network			Maximal network			Own family network		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	Female	Male	All	Female	Male	All	Female	Male
Panel A – Interval regression (daily discount rate)									
Family density	-1.200	0.132	-2.149	-0.606	0.965	-1.695	-0.031	0.419	-0.479
	(0.985)	(1.243)	(1.575)	(0.690)	(0.876)	(1.101)	(0.615)	(0.771)	(0.991)
Constant	4.808***	5.060***	2.558	4.838***	4.920***	2.911	4.728***	5.020***	2.520
	(1.604)	(1.797)	(3.355)	(1.608)	(1.801)	(3.383)	(1.606)	(1.797)	(3.367)
HH controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Community FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1222	646	576	1222	646	576	1222	646	576
Panel B – Probit regression (minimum discounters)									
Family density	0.659*	0.245	1.042*	0.447*	0.046	0.714*	0.238	0.138	0.383
	(0.355)	(0.534)	(0.541)	(0.263)	(0.403)	(0.404)	(0.226)	(0.336)	(0.345)
Constant	-2.173***	-6.590	-0.558	-2.214***	-6.587	-0.669	-2.163***	-6.594	-0.595
	(0.634)	(194.3)	(1.097)	(0.635)	(194.2)	(1.107)	(0.633)	(194.2)	(1.097)
HH controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Community FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	990	494	441	990	494	441	990	494	441
Pseudo R^2	0.158	0.192	0.193	0.157	0.192	0.192	0.156	0.192	0.189

Notes: Standard errors in parentheses. * indicates significance at the 10% level; ** indicates significance at the 5% level; *** indicates significance at the 1% level. Included control variables: sex, marital status, age and age squared, years of education, household size, socio-economic status, and interview language.

4

Corruption, Investments and Contributions to Public Goods

Experimental Evidence from Rural Liberia

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Abstract

We analyse how corruption affects incentives to invest or contribute to public goods. We obtain a proxy for corruption among Liberian community leaders by keeping track of a flow of inputs associated with a development intervention, measuring these inputs before and after giving them in custody to the chief. We then use the "gap" between these measurements ("missing inputs") to explain variation in investment behaviour of villagers. Investment behaviour is gauged with two simple artefactual field experiments. Our main results are that corruption (i) undermines incentives for voluntary contributions to local public goods and (ii) may reduce private investments of individuals subject to rent-seeking by the chief in real life. We also provide weaker evidence that the impact of corruption on investments and contributions to public goods is heterogeneous: this impact may be gender-specific and appears to vary with accessibility of communities.

4.1 Introduction

Corruption, or the misuse of public office for private gains, is an overarching concern in many countries—especially developing ones. It has been estimated that worldwide bribery involves some $1 trillion per year, or 3% of global income (Rose-Ackerman 2004). The World Bank Institute estimates 25% of African states' GDP is lost due to corruption each year (cited in Sequeira 2012, p.145). Corruption is often considered as symptomatic for deeper-seated problems of weak governance—one of the key factors responsible for underdevelopment in large parts of the world, such as Africa.

Early analyses of the *determinants* of corruption emphasized the importance of persistent factors such as the overall level of development, religious traditions, political regimes or design of legal systems (e.g. Barr & Serra, 2010; La Porta et al., 1999; Treisman, 2000). Recent work suggests levels of corruption are also determined by more transient variables—possibly amenable to intervention by policy makers (see Olken & Pande 2012). A rapidly expanding literature investigates drivers of corruption using lab experiments and (artefactual) field experiments. Lab-experiments provide a controlled environment in which researchers test anti-corruption policies and investigate how monetary and non-monetary incentives affect subjects' propensity to engage in corruption (see Abbink & Serra 2012 for a discussion). Recent field experiments provide a complementary perspective. Using clever identification strategies, economists have scrutinized the causal effect of factors such as community-based monitoring (Björkman & Svensson 2009; Olken 2007), electoral accountability (Ferraz & Finan 2011), external audits (Olken 2007; Ferraz et al. 2012), and the interaction between wages and auditing intensity (Armantier & Boly 2011; Di Tella & Schargrodsky 2003) on the incidence and extent of corruption.

Recent work has also provided a clearer perspective on the *consequences* of corruption. Early (macro) analyses debated whether it merely "greased the wheels" of a rigid bureaucracy, or involved genuine costs to society due to distortions. A consensus has now emerged that corruption involves real costs. It may adversely affect (foreign) investment (Edgardo Campos et al. 1999; Egger & Winner 2006; Wei 2000), and it may adversely affect growth via reduced levels of human capital by impeding the

supply of public services such as education and health care Reinikka and Svensson (2004).[1]

Some analysts argue corruption can be viewed as a tax,[2] highlighting standard distortionary effects and incentives for evasion. At the margin, private agents should provide lower levels of input if a wedge exists between actual and privately appropriable levels of output. Svensson (2005, p. 37) writes "*when profits or potential profits are taken away from firms through corruption, entrepreneurs choose not to start firms or to expand less rapidly*." Bates (1981) provides evidence supporting this view, showing that many African farmers opt for subsistence farming to avoid corruption in input and output markets. Corruption may also invite the propping up of inefficient firms, and steer the allocation of talent and resources away from their most productive use (Murphy et al. 1991). Svensson (2003) demonstrates firms are inclined to produce with relatively inefficient "fly-by-night" technologies if they expect they will have to bargain over bribes in the future (as the implied reversibility of such technologies enhances their bargaining position). In other words, the shadow of corruption affects the choice of inputs, and may invite sub-optimally low levels of investment. This is the main theme we analyse.

We analyse corruption and private incentives in 44 communities in rural Liberia, using a novel dataset at the community and household level that we collected ourselves. The main objective of this chapter is twofold. First, we study the impact of corruption on private incentives to invest in local public and private goods. To gauge

[1] Corruption may also have equity implications. High-profile cases of politicians stealing hundreds of millions of dollars attract attention (Dowden 2008), but corruption usually has more subtle effects. Reinikka and Svensson (2004) document how actual transfers in Uganda are regressive, as schools in better-off communities obtain a larger share of their entitlements than other schools. Olken (2006) demonstrates how corruption raises the costs of redistribution of rice to poor Indonesian households, even to the extent that welfare benefits from redistribution may be fully eroded. Corruption thus threatens the viability of such schemes on which the poor may depend.

[2] The "corruption is a tax" perspective overlooks that corruption generates no state revenues and ignores that corruption creates uncertainty and raises transaction costs due to a lack of enforceability and need for secrecy (e.g., Shleifer & Vishny 1994).

these incentives to invest in public and private goods we carried out two so-called artefactual field experiments (AFEs) – a voluntary contribution game and an investment game (see below for details). Our main result is that corrupt community leaders erode incentives to invest. Consistent with anthropological evidence, we also find tentative support for a gender-specific impact of corruption. This may be explained by the fact that men and women are likely to be subject to different forms of rent-seeking by the leader in daily life – specifically, men may be recruited for communal labour through community self-help schemes and women may be asked for contributions in kind or cash. Second, we explore heterogeneity in terms of the communities' responses to being governed by a stealing leader. That is, we analyse which community characteristics accentuate or attenuate the impact of corruption on incentives to invest, and find accessibility of the communities may be such a factor.

We use an innovative and direct way to gauge corruption. The empirical macro literature on corruption relies heavily on subjective assessments.[3] At the micro level a wider range of corruption indicators is used. Sequeira (2012) distinguishes between various approaches to measure corruption, including "direct observation" (e.g., Bertrand et al., 2007; Olken and Barron, 2009), a "forensic economic approach" based on comparing official data and equilibrium predictions of theoretical models (e.g., Fisman, 2001), and so-called "gap measurements." Our approach falls in the latter category. The idea is to identify corruption by searching for mismatches between different data sources – "gaps" that may be indicative of diverted resources. For example, some studies compare formal entitlements and received transfers as reported at the end of a public service chain (e.g. Björkman & Svensson, 2009). Olken (2006) looks at a subsidized rice transfer program, comparing official records and household survey data about rice receipts. Olken (2007) also compares declared costs of road construction and the actual construction costs, as estimated by a team of engineers.

[3] Corruption indices published by the International Country Risk Guide (ICRG), Transparency International, and the World Bank are widely used.

In this vein, we quantify corruption using two objective field measurements. We participated in a development project that involved the provision of agricultural inputs to a sample of communities in rural Liberia. Detailed tracking of this flow of inputs enabled us to identify community leaders diverting project inputs (see below for details).[4] We find nearly half the leaders in our sample diverted seed or agricultural tools. This is the key explanatory variable in our models explaining investment behaviour of villagers.

Our results hopefully speak to three literatures. First, they extend the literature on consequences of corruption by providing detailed evidence of adverse incentive effects of being governed by a thieving chief. Second, our results contribute to the rapidly growing literature on leadership. A macro literature suggests individual characteristics of leaders matter for economic growth (Besley et al. 2011; Jones & Olken 2005). The importance of leadership in shaping aggregate behaviour is recognized and analysed in the domains of psychology (e.g. De Cremer and Van Knippenberg, 2002), political science (Ahlquist & Levi 2011) and experimental economics (van der Heijden et al. 2009). In the context of rural development in Africa, various studies have focused on bad leadership and "elite capture" in community-driven development projects (e.g. Platteau, 2004).[5] A common finding is that fair treatment by the leader motivates individuals to engage in group-oriented behaviour, and facilitates cooperation in social dilemma situations. Recent experimental evidence from the field supports these insights. For example, Kosfeld and Rustagi (2015) study leaders' motivation to punish norm violators in the context of forest management in Ethiopia, and find that leaders who care about efficiency and

[4] We believe direct measurement of corruption is preferable over data based on reported receipts by intended recipients, as respondents may have an incentive to underreport when asked about goods received, in order to qualify for additional transfers. Our own field work in West Africa suggests this is not uncommon.

[5] See Fritzen (2007) and Khwaja (2009) on leadership and project design in Indonesia and Pakistan, and Amsden et al. (2012) for a recent overview on the role of elites in economic development.

equity in an experiment are associated with better management of the commons.[6] Relatedly, Beekman et al. (2013) find a significant association between corruption of local chiefs and livelihood choices (including investments and occupational choices) of villagers. Finally, our findings speak to the literature on community isolation and the role of infrastructure in development (e.g., Porter 2002; Casaburi et al. 2013). In addition to altering transaction costs and opening up of markets, the creation of infrastructure may change the demand for institutional quality, altering the response of communities to rent seeking by elites.

The chapter is organized as follows. In section 4.2 we provide background information on life in rural Liberia, and discuss some aspects of local governance in Liberia. In section 4.3 we introduce our three field experiments and data, and outline our identification strategy. Section 4.4 presents our main regression results and robustness analysis. Section 4.5 concludes.

4.2 Context and main hypotheses

Founded as a home for former African-American slaves, Liberia resembles a traditional settler state based on a system of indirect rule. This system of governance co-opts leading members of indigenous communities as "traditional" authorities, and consolidates a ranked lineage system with a small elite and large underclass. The scant evidence that exists to characterize low-tier governance in Africa suggests chiefs can be unaccountable "despots" (e.g., Mamdani 1996). One popular explanation is that colonial systems of indirect rule, in which elites received formal authority from the colonial government, severed ties between chiefs and their constituency and reduced accountability (Boone 2003).

Richards (2010) discusses many of the challenges for modern Liberia in the domain of governance, identifying "unresolved tensions between indigenous communities and

[6] See also the complementary paper on conditional cooperation and costly monitoring (Rustagi et al., 2010).

the settler state, political competition to control an over-centralized executive, […] an unregulated scramble for rich natural resources, and a series of gender-based and age-based tensions reflecting a failure to fully emancipate former slave-based hinterland communities." We will use these gender divisions in what follows to distinguish between different demographic groups.

Liberian society is hierarchical, and evidence suggests many chiefs (mis)use their power for private gain (Reno 2008; Richards & Bah 2005). Rural communities in northwest Liberia are governed by a town chief, who is "nominated" by elders, "elected" by community members, and finally "approved" by higher levels of government. Not everybody is sufficiently "civilized" to qualify as a potential chief, and leaders tend to be local "big men" and come from an upper stratum of society (Richards et al. 2005). Class-based patterns of exploitation and marginalization characterize rural life in Liberia (and arguably shaped the most recent episodes of violence). Exploitation is embodied in informal institutions governing local justice and access to land and women (via marriage rules, see Mokuwa et al., 2011). In this context, Richards et al. (2005) refer to a "*crisis of local confidence in state institutions*" invited by "*authoritarianism and extractive rent-seeking behaviour*" (p.31).

Acemoglu et al. (2014) document for neighbouring Sierra Leone that more powerful "Paramount Chiefs" tend to provide less local public goods, so that power of the chief and rural development are negatively correlated. Paramount Chiefs are one tier up in the administrative system, compared to the category of local town chiefs that we consider. However, paramount and town chiefs have similar instruments at their disposal to extract economic rents from their underlings. First, they control and allocate communal resources. This includes communal land, but also state resources channelled down from Monrovia, and revenues from local enterprises (such as communal plantations). Chiefs have the authority to compel their subjects to communal labour in so-called community self-help schemes. This may involve clearing and brushing farmland, or rubber tapping on communal plantations. These are often physically demanding activities, so especially men are targeted to supply unpaid labour. Second, chiefs may extract surplus from their villagers directly by demanding contributions in the form of cash or food. Since especially women are engaged in trade (e.g. Fuest, 2008), we speculate that women are likely candidates for

such contributions. In what follows we probe the gender-specific implications of corruption in more detail.

Summarizing, town chiefs can be corrupt in two ways: (*i*) use communal resources for own benefit and (*ii*) appropriate private property of specific community members. We implicitly assume both variables are indicative of an underlying latent variable (propensity to engage in rent seeking or corrupt behaviour), and we will seek to obtain a proxy of this latent variable by studying the diversion of project inputs.

We hypothesize that corruption (*i*) attenuates the propensity to contribute to local public goods, and (*ii*) negatively influences investment behaviour of villagers. More speculatively, we expect that exposure to the chief's grabbing hand in daily life may be gender-specific. Specifically, communal labour supplying men have first-hand experience in the domain of public good provision, and cash-owning women have learned how the proceeds of private investments may be channelled away. Therefore, we also hypothesize (*iii*) that men are more strongly affected by corruption when it comes to public investments, and (*iv*) that women are more responsive to corruption in the domain of private investments.

We also expect that responses to thieving chiefs are heterogeneous across communities. Our communities are similar in many respects, except for the fact that only half of them are located along main roads (the remaining communities are located along poor quality dirt roads or forest tracks). Evidence suggests that road quality has implications for communities that depend on marketing of agricultural produce, such as rice and the more bulky cassava. For example, Casaburi et al. (2013) show that improvement of rural roads in Sierra Leone led to a decline in transportation costs and reduction in prices of rice and cassava. Our data reveal that villagers in road communities visit markets twice as often as villagers in off-road communities. Assuming that differences in market integration translate into differences in cash in hand for villagers (trading women), we hypothesize that (*v*) the impact of corruption on private investments is greater in road communities than in off-road communities. The relation between road quality and public goods provision is more speculative. Customary norms and patron-client relationships may be stronger in more isolated communities where villagers have limited alternatives to a traditional

livelihood. Furthermore, communal plantations that are being operated are larger in off-road communities than in road communities,[7] which may imply greater scope for communal labour mobilization and in-kind rent extraction. The potential impact of corruption on public investments may also be greater; survey-based evidence suggests that general as well as personalized trust levels are greater in off-road communities.[8] We hence hypothesize that (*vi*) the impact of corruption on public good investments is greater in off-road communities than in road communities.

Now turn to the development intervention, for which we cooperated with an international NGO. As part of the development project, 44 rural communities (townships) were randomly selected to receive an agricultural development project. These communities are spread out across three districts: Kakata district in Margibi county, and Careysburg and Todee districts in Montserrado county. These districts are located near the capital city Monrovia (reachable within one day), and are characterized by poor infrastructure and livelihood conditions. The communities in our sample are homogenous in terms of size, ethnic composition and overall development. For example, none of the communities has access to electricity. As mentioned, one factor that distinguishes communities is accessibility: half of the communities in our sample are connected to a main road, whereas the other half is inaccessible during the rainy season. The main livelihood activities are small-scale agriculture and rubber tapping. As part of the intervention, participating communities

[7] In off-road communities, plantations *farmed* are 304 acres on average, and in road communities 101 acres (p-value t-test = 0.05). The difference between plantations *owned* by communities is smaller: 235 versus 124 acres (p-value t-test = 0.20).

[8] Trust in neighbors, community leaders, co-ethnic community members, community members of other ethnic groups and strangers, is invariably higher in off-road communities than in road communities. The difference in trust levels between road and off-road communities is always significant at the 1 percent level.

received a fixed amount of inputs, consisting of vegetable seeds, rice and small hand tools.[9]

4.3 Empirical strategy

Experimental and survey data were collected in November and December of 2010, using a random subsample of 20-30 household heads per community. We collected data in 44 communities, spread out across 3 districts. Care was taken to ensure that all participants understood the artefactual field experiments (AFEs) before commencing the games (through careful instruction and multiple trial runs).

4.3.1 Voluntary contribution and investment game

We first played a standard voluntary contribution game to measure the propensity to invest in a local public good (e.g. Ledyard, 1995). All participants were invited to a public space, where the experimenter explained the experiment. Participants were randomly and anonymously matched with three fellow villagers, and were informed that the game would be played for five rounds. Participants moved simultaneously, and were informed that one of the rounds would be randomly selected for payment. Moreover, we informed them that the group composition would be changed after each round, allowing them to search for their optimal contribution strategy given the behaviour of other people in their community – excluding signalling and reputation effects (see e.g. Andreoni, 1988).

Participants received five tokens per round, each worth 10 Liberian dollars (L$70 = USD 1, or about the equivalent of one day's wages for unskilled labour in the region). In each round, participants decided how many tokens to invest in the public good ('the pot'), and how much to keep for themselves. After each round, participants were informed how many tokens they earned, after the number of tokens in the pot was

[9] Seeds included 25 kg rice, 3 kg beans and peanuts, 5 kg corn, 20 g pepper seed, and 5 g bitterball seed. The set of hand tools included 4 cutlasses, 2 shovels, 4 regular hoes, 2 files, 2 watering cans and 5 scratching hoes.

doubled and equally distributed among the four participants. In what follows we refer to contributions to the pot as a "public contribution."

After finishing the voluntary contribution game, we played a simple investment game with positive expected payoffs (e.g., Gneezy and Potters, 1997; Gneezy et al., 2009; Haigh and List, 2005) to measure the individual's proclivity to make an uncertain investment for private gain. The game was introduced to the group, framed as an investment decision, and participants were then individually called to a private space where the game was explained in detail. For logistical reasons we consistently played the voluntary contribution game first and the investment game afterwards.[10]

Each household head received an endowment of L$70. Participants could invest (part of) their endowment in a risky, but potentially profitable project. With a success probability of 50%, this project paid out four times the amount invested (and with a probability of 50% the participant lost her investment). We were careful to frame this allocation decision as an 'investment opportunity'. Nevertheless, the experiment picks up both the propensity to invest as well as individual risk preferences–an issue to which we return in section 4.4. Each participant made her investment decision in private, and afterwards was presented a bag containing two cards—one marked and one unmarked. Upon drawing the marked card, the investment paid off and the participant received four times the amount invested. In what follows we refer to the amount invested in the investment game as a "private investment."[11]

[10] While this may introduce order effects, biasing levels of point estimates of investments, we have no reason to believe the fixed order will affect the direction of the comparative static results we are interested in – the impact of corruption on investment behaviour.

[11] The investment game was actually somewhat more complex as it involved two (random) treatments with different levels of information about the winnings in the game (private or public knowledge). In what follows we include treatment-fixed effects to control for possible "level" effects stemming from these treatments.

4.3.2 The corruption experiment

Next, we discuss our tool to gauge corruption, which may be viewed as a natural field experiment (NFE). One key difference between an AFE and NFE is that participants in the latter type of experiment are unaware of the fact that their behaviour is scrutinized (Harrison & List 2004). The internal validity of NFEs is hence not compromised by socially desirable responses.[12] The standard procedure of the NGO is to give the inputs to a community leader, who then publicly distributes them to project participants. Due to logistical difficulties we were unable to transport and distribute all project inputs to 44 communities on a single day. Hence, we transported inputs on one day, and asked community leaders to store them for a period of three days in a safe place (their hut). Leaders were informed that on the third day a project worker would make a public inventory of the inputs, after which they would be distributed among the participants. However, and unknown to community leader and villagers, we also measured these inputs prior to transport, so we have two measurements. The difference between quantities transported and quantities available for distribution (the "gap" in the phrasing of Sequeira (2012) is our measure of corruption.[13]

[12] The analyst faces a trade-off when designing her experiment. While "informed consent" of participants is clearly desirable, it is obvious that one cannot measure malfeasance with consent (see List, 2006). The scrutiny effect is likely to be very large when measuring corruption. Measuring corruption is therefore among the "prime candidates for relaxation of informed consent" — informing participants about the experiment would come at minimal benefits and at huge costs (see List, 2008, p. 672). Obviously, to attenuate ethical concerns and avoid social tensions, we made sure that community leaders and villagers remained uninformed about the NFE at all times.

[13] We made sure that all communities received exactly the same information, and that neither community leaders nor villagers were informed that inputs were measured before the inputs were handed out to the leader. Moreover we made sure that neither the chief nor any villager learned about our efforts to measure input diversion.

4.3.3 Data

Table 4.1 summarizes our experimental and survey data (for a summary broken down by district, refer to Table A4.1 in the Appendix). Panel A lists the experimental data. Based on the NFE we have constructed three corruption proxies: (*i*) "*missing seed %*" is a continuous variable capturing the percentage of vegetable and corn seed diverted, (*ii*) "*missing seed*" is a dummy variable taking a value of 1 if vegetable or corn seed was missing, and (*iii*) "*missing any*" is a dummy variable indicating whether any items were missing (either rice, vegetable and corn seed, or tools). Rice is the major staple crop in Liberia, and has a special position in Liberian culture (e.g. Sawyer, 2008). Stealing rice is considered more offensive than stealing other items. Hence, we expected less theft of rice to occur than theft of other seed. This was confirmed by our data, which indicated that theft of rice was relatively rare. Overall, almost half the community leaders diverted inputs; the other half did not.

Panel A also summarizes play in the AFEs. On average, villagers shared 1.5 tokens (out of 5) in the fifth round – our measure for contributions to the public good ('*public contribution*').[14] In the investment game, villagers invested on average L$26, or 37% of the endowment ('*private investment*'). The data reveal considerable variation across communities—variation that we will seek to explain later.

Panel B summarizes household controls. About half of the sample is male, and the average age of the head of household is 43. On average, household representatives had 2.6 years of education. Earlier studies suggest key demographic characteristics like gender, age and years of education are associated with risk preferences, and therefore we include them as controls. Next, 27% of the households are involved in rubber tapping – a major source of cash income in the study region. Some 30% of the households were attacked during the civil war that ravaged Liberia between 1989 and

[14] All results that follow are robust to choosing another round, or using an aggregate measure of average play over 5 rounds. Average number of tokens shared over rounds was: 1.6 in round 1, 1.4 in round 2, 1.7 in round 3, 1.5 in round 4, and 1.5 in round 5. See Table A3.2 in the Appendix for further details.

2003. Household attacks are included because exposure to violence during the war may influence social and risk preferences (Voors et al. 2012).

Table 4.1: Summary statistics

Variable	N	Mean	SD	Min	Max
A1: Experimental results - Stealing					
Missing seed %	44	1.904	3.526	0	12.67
Missing seed (b)	44	0.364	0.487	0	1
Missing any (b)	44	0.477	0.505	0	1
A2: Experimental results – Investment decisions					
Public contribution (tokens shared)	1074	1.492	1.488	0	5
Private investment (amount invested)	729	26.310	18.680	0	70
B: Household controls					
Male (b)	1067	0.495	0.500	0	1
Age	1023	42.708	15.077	11	94
Years of education	945	2.570	4.113	0	16
Rubber tapping (b)	1069	0.269	0.444	0	1
War attack (b)	1023	0.300	0.459	0	1
C: Community controls					
Family share	44	0.348	0.140	0.117	0.726
Main road (b)	44	0.5	0.506	0	1
Ethnic diversity	44	0.353	0.219	0	0.767
Religious diversity	44	0.158	0.147	0	0.518
NGO (b)	44	0.705	0.462	0	1
Recruitment (b)	44	0.5	0.506	0	1
Plantation (acres)	44	179.000	284.370	0	1500
Share of young men†	44	1.683	0.901	1	4
Share of displaced people	44	0.731	0.132	0.364	1
D: Chief characteristics					
Tribe chief (b)	44	0.795	0.408	0	0
Acres chief	43	14.453	56.192	0	350

Notes: (b) = binary variable. † Categorical variable indicating share of young men in the community. 1 indicates 'almost nothing' and 4 indicates 'more than half'.

Community controls are provided in Panel C. The average share of family members in our sample is about 35%.[15] This may be important as evidence suggests the presence of family members may affect play in the games (e.g. Jakiela and Ozier, 2012). A main road connects half of the communities to the outside world, while off-road communities cannot be reached by car during the rainy season. This variable proxies for market integration, which in turn may be associated with both corruption and people's behaviour in the games (e.g., Henrich et al., 2001). Ethnic and religious diversity are included as they may impact on social behaviour and possibly corruption. The Herfindahl indices measure ethnic and religious diversity so that a value of zero indicates maximum homogeneity, with all villagers belonging to the same group, and greater values indicate larger degrees of diversity. Indices for ethnicity and religion are 0.353 and 0.158. About 70 percent of our sample communities have been visited by NGOs in the past. This high percentage is unsurprising given the destructive nature of the civil war, inviting considerable post-war reconstruction efforts. Controlling for NGO presence is relevant as projects may influence both corruption and social (or risk) preferences. In half of the communities, some of its members were recruited during the war. This variable may proxy for different levels of socio-economic or institutional quality. The number of acres for (rubber) plantation owned by communities varies considerably from 0-1500 with a mean of 179 acres per community. We include size of communal rubber plantations to control for different socio-economic and labour market conditions. There are relatively few young men (between 12 and 25 years old) in most of the communities: over 60% of the communities indicate to have few or almost no young men. Young men are associated with more risk-taking behaviour than other people, and may impact social preferences in the community. Some 73% of the people have been displaced during the war, which may have had an impact on social (risk) preferences and corruption through various channels (e.g. trust among co-villagers).

[15] The family share is measured as "density" in social network analysis; participants were asked to specify their relationship with all others in our sample of respondents, and we aggregated these data.

Finally, Panel D summarizes key characteristics of the community leader (ethnic identity and land ownership). Some 80% of the community leaders belong to the major ethnic group, the Kpelle, and 32% of the leaders own land—14 acres on average.[16] We will use these variables to identify exogenous variation in corruption in an instrumental variables approach outlined below.

4.3.4 Empirical strategy

Our identification strategy is simple, and consists of two components. First, we run interval regression and OLS models to explain investments (in public and private goods) by our corruption indicators ($Stealing_j$), and vectors of community ($Comm_j$) and household (X_{ij}) controls.[17] We estimate two models: (1) at community level, explaining average investment behaviour, and (2) at household level, explaining household-level investment choices:

$$Investment_j = a + \gamma_k + \beta_1 Stealing_j + \beta_2 Comm_j + \beta_3 X_j + \varepsilon_j \quad (1)$$

$$Investment_{ij} = a + \gamma_k + \beta_1 Stealing_j + \beta_2 Comm_j + \beta_3 X_{ij} + \varepsilon_{ij} \quad (2)$$

where subscript i indexes household i=1,…,1074 subscript j indexes community j=1,…,44 and subscript k indexes districts, k=1,2,3. In (1) we include a vector of household controls X_j, where household controls are averaged at the community level.[18] In all models we use district fixed effects (γ_k, k=1,2,3) to capture unobservable factors that might vary at this level of organization. Also, in all household models we

[16] We excluded an outlier of 1000 acres, which we believe represents an error. For this reason, this variable only includes 43 observations.

[17] We use interval regression to explain public contributions on the individual level (as respondents can contribute 0, 1, 2, 3, 4 or 5 tokens). To explain public contributions at the communal level, we use OLS, as average contributions can take any value between 0 and 5 here.

[18] Moreover, in some specifications we focus on subsamples (of households or communities) (testing hypotheses (*iii*) and (*iv*)), or use interaction terms ($Stealing_j \times Comm_j$ and $Stealing_j \times X_{ij}$) (testing hypotheses (*v*) and (*vi*)), to explore whether the impact of corruption is heterogeneous—varying across selected household and community characteristics.

cluster standard errors at community level, and use bootstrap-t procedures when the number of clusters is small (Cameron et al. 2008).

Equations (1-2) may suffer from endogeneity bias. The risk of reverse causality bias is limited, as individual investment decisions are unlikely to predict our community level variable: a leaders' propensity to steal. Our estimate of β_1 may however be biased due to omitted variables (unobserved factors driving both corruption and investments—think of cultural factors or average income in the community). For this reason, we also estimate an instrumental variables model. The number of communities is relatively small, which implies IV approaches may introduce small sample bias (which may or may not be worse than the endogeneity bias it seeks to address). With this caveat in mind, we use chief characteristics (see below) as instruments, and estimate the following equations in a 2SLS framework:

$$Investment_{ij} = \alpha + \gamma_k + \beta_1 Stealing^*_{ij} + \beta_2 Comm_j + \beta_3 X_{ij} + \varepsilon_{ij}, \quad (3)$$

and

$$Stealing^*_{ij} = \theta + \gamma_k + \varphi_1 Comm_j + \varphi_2 X_{ij} + \varphi_3 Chief_j + \varepsilon_j. \quad (4)$$

In (3), $Stealing^*$ is predicted with controls and chief characteristics, captured by the vector of excluded instruments, $Chief_j$.[19] We elaborate upon the IV model in section 4.4.2.

[19] Our set-up has the slightly awkward feature that predicted corruption ($Stealing^*_{ij}$) varies at the household level (because we also use X_{ij} as included instruments), even if these households are governed by the same chief. However, controlling for household level variables in the 2nd stage of the model improves the precision of our estimates. Obviously it is important to cluster standard errors at the village level for this approach to work. A similar approach was used, for example, by Edmonds (2002) and Voors et al. (2012).

4.4 Empirical results

4.4.1 Correlations between corruption and investment behaviour

We first focus on aggregate results at the community level, and report OLS results in Table 4.2. In columns (1-4) we explain variation in contributions to the local public good, and in columns (5-8) we explain variation in investments in the private investment game.[20] We find tentative support for hypothesis (*i*). In columns (1) and (2) we adopt parsimonious specifications, excluding controls and fixed effects. Our corruption indicators enter with negative signs, but only the binary corruption proxy (whether any input was stolen) enters significantly (column 2). Based on this model, in communities with a corrupt chief, on average the contribution in the game goes down by 0.27 tokens, or 19% of the mean contribution.

In columns (3) and (4) we add our vectors of household and community controls to account for possible correlations between social (risk) preferences and corruption, mitigating potential omitted variable concerns. Including these controls and district level fixed effects raises the coefficients of our corruption indicators, and increases their significance levels. Now both corruption variables enter significantly at the 1% or 5% level. Compared to the coefficient in the parsimonious model of column (1), the coefficient of our continuous corruption indicator in column (3) becomes twice as large. The coefficient of our binary corruption indicator in column (4) increases by 20 percent, compared to the parsimonious specification in column (2). In communities with a thieving chief, villagers contribute 0.36 tokens less to the common pot, which equals 24 percent of the mean contribution in this game.

In addition (but not reported), average age of household representatives is related with higher public contributions (but contributions decrease after a within-sample turning

[20] We also ran our models using an alternative binary corruption indicator: whether any seed got missing. Results are roughly the same as for the binary corruption indicator ("missing any") included in the various Tables. Regression results on community level are presented in Table A4.3. Regression results for specifications on individual level are available on request.

point). Recruitment activities during the war and a high share of young men in the community are negatively related to public contributions; religious heterogeneity is positively related to public contributions. Other included controls do not enter significantly. See Table A4.3 in the Appendix for the complete set of regression results.

Results for the investment game are reported in columns (5-8). We also find tentative support for hypothesis (*ii*): a negative correlation between the *incidence* of corruption and private investments, both in the parsimonious model in column (6) and in the full specification in column (8). Based on column (8), in communities with a thieving chief, villagers invest about 7.7 LD less in the investment game, or 29 percent of the mean contribution. Our evidence for hypothesis (*ii*) is slightly weaker than for hypothesis (*i*) – while our "missing any" variable enters highly significant in both specifications explaining private investment, the continuous corruption indicator in columns (5) and (7) does not.

Few of our control variables enter significantly (see Table A4.3 in the Appendix). The average age of respondents and the share of displaced people in the community are weakly related to higher private investments. The only covariate that is robustly correlated with investment behaviour is the presence of NGO activity in the community. In villages where NGOs have worked, private investments are lower, which suggests a "crowding out" effect of development assistance.

Two caveats are relevant. First, behaviour in the investment game may confound risk preferences and propensity to invest. Insofar as these risk preferences are 'orthogonal' to contemporary corruption levels (e.g. because they are "hard-wired," as in Netzer, 2009), they introduce noise but do not bias the estimates. However, it is possible that "malleable" risk preferences evolve in response to experiences in daily life. If such experiences vary from one community to the next then our experimental data might reflect differences in (endogenous) risk preferences, rather than differences in the propensity to invest. Moreover, if there is an omitted variable, driving risk preferences (e.g., weather patterns or war time experiences), which is also correlated

with local corruption levels, then the correlation between corruption and risk/investment would be spurious.[21]

Second, if only a subsample of the respondents experiences rent-seeking by the chief, then this group's behavioural response may be obscured when considering aggregate data. As a first step to probe this issue, we use *individual* decisions in the field experiments as dependent variables in Table 4.3. Columns (1-2) and (5-6) are, again, based on parsimonious models for public contributions and private investments, and columns (3-4) and (7-8) report on models including vectors of household and community controls as well as district level fixed effects.[22] Results are consistent with the community-level results in Table 4.2, and support hypothesis (*i*): corrupt chiefs are robustly correlated with lower public contributions. Like before, including controls increases our coefficients and their significance levels: contributions to the public good decrease by about 20% when the chief diverted any inputs. Similarly, although our "missing any" variable enters significantly at 5 percent level in both the parsimonious and the full specifications, the average response to the continuous corruption indicator in the investment game is not consistently significant (only for the public contribution in column (3)).[23]

[21] However, we are sceptical that such spurious correlation is explaining our results. First, our sample of communities is drawn from a geo-physically homogenous region in Liberia, and we control for many of the candidate factors to be correlated with risk and corruption (e.g., infrastructure, livelihoods, wealth, conflict experiences in the war). Second, the nature of the selection process of local chiefs (proposed by the elite, endorsed by the people, approved by the state), and the "stickiness" of their tenure, guarantees that the chief's identity is to a large extent independent of many shocks at the local level. Finally, results of the IV model below are consistent with the outcomes from the OLS models.

[22] As in Table 4.2, we also ran all models using an alternative binary corruption measure. Results are the same as for the model estimated on community level data.

[23] Below, we will consider subsamples of respondents to analyze the gender-specificity of the response in more detail.

Table 4.2: Public contribution and private investment, community level

	Public contribution OLS				Private investment OLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Missing seed %	-0.031		-0.068***		0.037		-0.660	
	(0.022)		(0.021)		(0.427)		(0.479)	
Missing any		-0.297*		-0.357**		-6.947**		-7.669***
		(0.147)		(0.141)		(2.779)		(2.711)
Constant	1.55***	1.64***	-5.27**	-3.56	26.44***	29.82***	-104.0*	-86.23*
	(0.08)	(0.10)	(2.39)	(2.48)	(1.70)	(1.92)	(54.16)	(47.81)
HH controls	No	No	Yes	Yes	No	No	Yes	Yes
Comm controls	No	No	Yes	Yes	No	No	Yes	Yes
District FEs	No	No	Yes	Yes	No	No	Yes	Yes
N	44	44	44	44	44	44	44	44
R^2	0.047	0.088	0.680	0.641	0.000	0.130	0.561	0.642

Notes: Clustered standard errors are in parentheses. *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level. Household controls are averaged on the community level.

Table 4.3: Public contribution and private investment, household level

	Public contribution Interval regression				Private investment OLS			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Missing seed %	-0.031		-0.056***		0.070		-0.287	
	(0.028)		(0.015)		(0.510)		(0.445)	
Missing any		-0.291**		-0.348***		-6.619**		-6.144**
		(0.143)		(0.104)		(2.776)		(2.411)
Constant	1.55***	1.63***	1.13**	1.35***	25.89***	29.26***	12.57	16.81
	(0.08)	(0.10)	(0.55)	(0.52)	(1.70)	(1.96)	(12.41)	(12.09)
HH controls	No	No	Yes	Yes	No	No	Yes	Yes
Comm controls	No	No	Yes	Yes	No	No	Yes	Yes
Treatment FEs	No	No	No	No	Yes	Yes	Yes	Yes
District FEs	No	No	Yes	Yes	No	No	Yes	Yes
N	1074	1074	914	914	729	729	603	603
R^2	0.006	0.010	0.062	0.059	0.000	0.032	0.154	0.172

Notes: Clustered standard errors are in parentheses. *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level.

4.4.2 Instrumental variables: Causal effects of corruption

While the correlations in Tables 2 and 3 are informative, it would be premature to interpret them as causal relationships. Good instruments satisfy two requirements: they should be (*i*) correlated with the endogenous regressors, and (*ii*) not be correlated with the error term in (3). We believe certain characteristics of the town chief are likely to meet these requirements, and consider his ethnic identity and land ownership as potential excluded instruments. These characteristics should explain whether or not chiefs engage in stealing, yet should not have any effect on villagers' behaviour other than via the postulated governance channel. We emphasize that the results presented below are not sensitive with respect to these exact excluded instruments—we obtain similar results when using the chief's education level and the length of his term in office as excluded instruments instead (details available on request).

First stage regression results are displayed in panel A of Table 4.4, and matching 2nd stage outcomes in panel B. Consider column (1) first. Leaders owning more land and leaders belonging to the communities' dominant ethnic tribe steal larger amounts of seed. Perhaps, leaders owning more acres of land have more opportunity to use stolen seed on their own land (or are better able to hide them). Leaders belonging to the major ethnic group may be subject to less scrutiny or retaliation by their co-ethnics, or may be better able to appease community members by redistributing part of the seed via ethnic-specific patron-client networks. Column (2) provides the result for our binary stealing indicator, which is similar. Column (1) of panel B provides the matching second stage results for the magnitude of corruption (percentage of seed stolen) and column (2) for the incidence of corruption. Predicted corruption enters significantly in both models explaining public contributions. The magnitude of the coefficients is similar as before, suggesting that small sample bias may be limited. One percentage point increase in diverted seed reduces public contributions by 4% (column 1). Being governed by a corrupt chief leads to a decrease of some 20% in

public contributions (column 2).[24] Hence, we continue to find support for hypothesis (*i*): corruption attenuates the propensity to contribute to local public goods.

Next, we use the IV set-up to analyse how corruption affects private investments. Results are reported in columns (3-4), with matching first stage results in panel A. Our results now unambiguously support hypothesis (*ii*): corruption negatively influences investment behaviour of villagers. IV results are stronger than the correlations reported earlier: both corruption variables are now statistically significant. The coefficients are also larger. This might reflect that the OLS models underestimate the true effect because of measurement error ("attenuation bias").[25] However, IV results may overestimate the true effect if the instruments are positively correlated with omitted variables that have the same sign as the endogenous institutional variables in the regression (e.g. other dimensions of the quality of local governance that matter for investments) – see Pande and Udry (2005). Therefore we prefer to refrain from speculating about which effect size is to be preferred.

[24] We also ran these models for our alternative binary corruption measure (any seed missing). Results are similar.

[25] Note that the interval and IV results for the public good experiment are not so different. Hence, accepting the attenuation bias explanation implies assuming that the public good game introduces less measurement error than the private investment game.

Table 4.4: IV model

	Public contribution		Private investment	
	(1)	(2)	(3)	(4)
Panel A: First stage				
Dependent	Missing seed %	Missing any	Missing seed %	Missing any
Acres chief	0.014**	0.0023***	0.0133**	0.0022***
	(0.005)	(0.0006)	(0.0053)	(0.0006)
Tribe chief	1.882**	0.422**	1.842**	0.414**
	(0.775)	(0.171)	(0.781)	(0.171)
Constant	-2.119	0.0835	-1.189	0.179
	(3.880)	(0.491)	(4.080)	(0.519)
Panel B: Second stage				
Missing seed %	-0.061*		-2.250*	
	(0.033)		(1.306)	
Missing any		-0.333*		-12.40**
		(0.178)		(5.588)
Constant	1.186**	1.352**	16.62	21.85*
	(0.551)	(0.540)	(13.49)	(12.55)
HH controls	Yes	Yes	Yes	Yes
Comm. contr.	Yes	Yes	Yes	Yes
Treatment FE	No	No	Yes	Yes
District FE	Yes	Yes	Yes	Yes
N	895	895	592	592
R^2 1st stage	0.340	0.430	0.334	0.429
R^2 2nd stage	0.064	0.061	0.047	0.151
Test statistics				
Partial F excl. instr.	7.69	24.69	7.03	22.66
KP LM stat	6.58	8.95	6.33	8.70
KP Wald stat	16.11	52.17	14.91	48.18
Hansen-J p-val.	0.39	0.45	0.28	0.30

Notes: Clustered standard errors are in parentheses. *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level.

4.4.3 Corruption and intra-community heterogeneity

Next, we explore whether effects of a stealing chief vary across demographic groups. Preliminary analyses suggested that corruption does not significantly explain investments and public good contributions among "youths" (defined as individuals up till 35 years of age—results not shown but available on request). Perhaps this reflects more limited exposure to corruption in daily life among youths, but it is presumably due to the fact that the number of youths in our sample is relatively low, implying low power. We therefore limit our attention to a possible differential impact of corruption on males and females over 35 years of age, and report OLS (interval regressions in case of public contributions) and IV evidence in Table 4.5 in panels A and B.

Columns (1) and (2) report results for the public contribution game for male and female subsamples, and provide tentative support for hypothesis (*iii*): men seem to be more strongly affected by corruption when it comes to public investments. Both the OLS and IV evidence suggests the aggregate results reported earlier were mainly driven by the subsample of males. Men respond strongly to corruption by lowering their voluntary contributions to the public good but women do not. This is consistent with the hypothesis that men in particular have learned providing communal labour – contributing to local public goods – does not pay off in a corrupt environment. They are prime targets for requests for unpaid provision of communal labour. However, we should interpret these results with caution. While point estimates of the adverse effect of corruption on investment are greater for men than for women, the difference is not statistically significant (p=0.27 in the interval regression model). Also, when estimating a model with pooled data and an interaction term (column 3), this interaction term has the right (negative) sign, but does not enter significantly. We believe the low statistical power of our tests may prevent us from identifying a significant difference between men and women.

Table 4.5: Contributions, Corruption and Gender (non-youths)

	Public contribution			Private investment		
	Female > 35	Male > 35	Pooled >35	Female > 35	Male > 35	Pooled >35
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:	Interval regression			OLS		
Missing any	-0.163	-0.449***	-0.169	-9.409***	-5.815	-9.430***
	(0.195)	(0.163)	(0.187)	(3.188)	(3.539)	(3.483)
Male			-0.132			65.52
			(2.481)			(39.48)
Missing any × Male			-0.280			3.525
			(0.230)			(4.482)
Constant	1.675	1.384	1.559	-16.94	48.41	-16.75
	(1.546)	(1.674)	(1.582)	(26.54)	(30.15)	(26.80)
N	258	318	576	160	184	344
R^2	0.114	0.092	0.109	0.222	0.180	0.225
Panel B: IV model						
Missing any	-0.273	-0.517**	-0.406	-20.79***	-14.47**	-23.16**
	(0.350)	(0.235)	(0.372)	(8.029)	(6.447)	(9.476)
Male			-0.398			64.78*
			(2.485)			(36.09)
Missing any × Male			-0.064			9.003
			(0.419)			(9.127)
Constant	1.801	1.351	1.781	-15.88	49.75*	-15.48
	(1.530)	(1.711)	(1.535)	(23.95)	(28.33)	(24.77)
HH + Comm. controls	Yes	Yes	Yes	Yes	Yes	Yes
Treatment FE	No	No	No	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes	Yes	Yes
N	251	312	563	155	181	336
R^2 2nd stage	0.121	0.106	0.118	0.170	0.146	0.174
Hansen-J	0.20	0.40	0.40	0.35	0.50	0.95

Notes: Clustered standard errors are in parentheses. *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level.

The results regarding private investments (columns 4 and 5) are more robust across the sexes. According to the IV model both men and women invest less when governed by a corrupt chief. The results for both women and men are similar to the OLS model, (although the results for men are only significant at the 11% level in the OLS model). Point estimates of the adverse effect of corruption on investment are also greater for women than for men, but the differences are insignificant (p=0.36 in the OLS model). The interaction term in the pooled model (column 6) is of the right sign but also insignificant. Taken together, this implies only very weak evidence for hypothesis (*iv*), or that that especially women – a cash-owning but socially vulnerable group – are subject to rent-seeking by the chief in daily life and respond accordingly in an experimental setting.

4.4.4 Corruption and inter-community heterogeneity

We also consider inter-community heterogeneity. Communities in our sample were selected on the basis of a stratified random sample, and stratification was based on whether communities are located along an all-weather road, or not. We split our sample into two equal-sized groups: communities on the main road (22 *road* communities) and communities not on the main road (22 *off-road* communities), and also use a pooled model with an interaction term. Table 4.6 reports results for OLS (Panel A) and IV models (Panel B).[26] All models include the standard vector of household and community controls and district fixed effects. Standard errors are clustered at the community level.[27]

Columns (1) and (2) report results for public investments in the *off-road* and *road* community subsamples. In both the OLS and IV models we find a strong negative effect of corruption on public investments in *off-road* communities — and a much

[26] To economize on space we only report 2nd stage results. Corresponding first stage results are available in Table A4.4 in the Appendix.

[27] Cameron et al. (2008) advise to use cluster bootstrap-t procedures when the number of clusters is small (5-30). We therefore used wild bootstrap to calculate unbiased p-values in the road subsamples (22 clusters in each subsample).

smaller (and insignificant) effect in *road* communities (difference between the corruption indicators in the subgroups is significant at p=0.003). The pooled model (with interaction term) supports hypothesis (*vi*): the impact of corruption on public good investments is greater in off-road communities than in road communities. Results are reversed for the case of private investments, summarized in Columns (4) and (5). We find a strong negative effect of corruption on private investment in *road* communities, and a somewhat smaller effect in *off-road* communities, but the difference is not statistically significant (p=0.387). The coefficient for the interaction term in the private investment model, is of the right sign but not significant. This implies no support for hypothesis (*v*); or the idea that the impact of corruption on private investments is greater in road communities than in off-road communities.

Table 4.6: Contributions, Corruption, and Infrastructure

	Public contribution			Private investment		
	Off-road	Road	Pooled	Off-road	Road	Pooled
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A:	Interval regression			OLS		
Missing any	-0.600***	-0.039	-0.549***	-5.125*	-8.848*	-5.033*
	(0.153)	(0.115)	(0.143)	(2.920)	(3.418)	(2.923)
Road			-1.078			13.40
			(0.866)			(30.89)
Missing any × Road			0.477***			-4.103
			(0.179)			(4.654)
Constant	1.722**	0.647	1.711**	14.71	24.05	15.20
	(0.763)	(0.590)	(0.696)	(15.56)	(33.56)	(13.78)
N	445	469	914	296	307	603
R^2	0.10	0.06	0.06	0.20	0.20	0.21
Panel B: IV model						
Missing any	-1.040***	-0.321***	-0.736***	-10.27*	-15.75***	-10.11*
	(0.260)	(0.137)	(0.211)	(5.834)	(3.925)	(5.174)
Road			-0.897			18.76
			(0.875)			(31.01)
Missing any × Road			0.509**			-3.785
			(0.240)			(6.318)
Constant	1.966***	1.199*	1.810***	17.63	37.35	18.10
	(0.631)	(0.631)	(0.629)	(13.94)	(32.97)	(13.27)
HH controls	Yes	Yes	Yes	Yes	Yes	Yes
Comm. Controls	Yes	Yes	Yes	Yes	Yes	Yes
Treatment FE	No	No	No	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes	Yes	Yes
N	445	450	895	296	296	592
R^2 2nd stage	0.082	0.053	0.077	0.178	0.192	0.200
Test statistics						
Hansen-J	0.575	0.371	0.489	0.317	0.284	0.355

Notes: Clustered standard errors are in parentheses. *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level. P-values for 'Missing any' in columns (1) and (2) were calculated using cluster bootstrap-t procedures because of small number of clusters.

4.5 Discussion and conclusions

In recent years, bad governance has been identified as a leading factor of slow growth and underdevelopment. Our main contributions fit in an emerging micro literature, and are twofold. First, corrupt leadership attenuates individual investment incentives. Reflecting the reach of chiefs in rural Liberia, we find that corruption strongly undermines incentives to provide local public goods (creating goods or services amenable to confiscation by the leader), and has a similar, albeit less robust, effect on aggregate investments in private goods. Zooming in on subsamples of community members, we find weak evidence that responses to exposure to rent seeking by the chief may be gender-specific, consistent with anecdotal and observational data provided by anthropologists. Second, accessibility may matter. We find that corruption translates into reduced levels of contributions for public goods in isolated (off-road) communities, and we find weak evidence that corruption translates into lower levels of private investment in connected (road) communities. We speculate these patterns in the data reflect spatial differences in exposure to different forms of corruption in daily life. Analysing the determinants and consequences of intra- and inter-community heterogeneity in more detail is left for future work.

One obvious policy recommendation can be gleaned from this research. NGOs that aim to improve local livelihoods via (agricultural) development projects should try to target communities with good chiefs if they want to maximize the impact of their interventions. Insofar as the success of interventions depends on combining project inputs and effort or private inputs supplied by community members, projects will be more successful when the chief is not corrupt. Of course it may not be straightforward to learn about the "type" of the chief, but many NGOs like to build their activities on prior interventions—revisiting the same communities again (and perhaps again). Information about the type of the chief may be gradually revealed during such forms of repeated interaction.

Why do people invest less when their leader is corrupt? Our data do not allow us to identify the channel linking diversion to reduced investments, but two candidate explanations leap to mind. First, corruption might work like a distortive tax. While there is no "taxation" in the experiment, such an effect might work via

internalization—people are used to the fact that the chief has a finger in the allocation of (communal) resources, and bring their life-time experience into the lab. Another sort of effect might be at play as well. Fehr and Falk (1999) write that when subordinates are treated with respect, they respond with loyalty and greater productivity. Insofar as we can equate the incidence of corruption with a lack of respect, the loyalty channel could also explain the behavioural patterns in our data. Additional research will be necessary to untangle the mechanism.

Finally, some caveats are relevant. First, while we interpret the diversion of inputs as a measure of corruption, we acknowledge that we do not know what happened to these inputs. It is possible that the chief stole them for private gain, or used them to fortify his position in existing patron-client networks. However, he may also have used a fraction of these inputs to improve the livelihoods of the poorest in his community – using his knowledge to improve upon the distribution of benefits as proposed by the implementing NGO. Second, while our IV strategy goes a long way to address endogeneity concerns, it does not eradicate them completely. We believe the chief-selection mechanism in place in rural Liberia implies that certain characteristics of the chief are orthogonal to the investment behaviour we study, and the test statistics give no cause for alarm. But it is well-known that these tests have low power, and we consider alternative approaches to identifying exogenous variation in levels of corruption a priority for future research.

Appendix

Table A4.1: Community characteristics by district and road

Variable	Careysburg district (N=6)	Todee districts (N=17)	Kakata district (N=21)	ANOVA Test (p-value)	Off-road (N=22)	Road (N=22)	P-value of t-test Road = Off-road
Family share	0.35 (0.12)	0.36 (0.17)	0.34 (0.12)	0.89	0.31 (0.11)	0.39 (0.15)	0.04
Main road	0.67 (0.52)	0.82 (0.39)	0.19 (0.40)	0.00			
Ethnic diversity	0.43 (0.20)	0.40 (0.22)	0.29 (0.21)	0.19	0.33 (0.22)	0.38 (0.22)	0.47
Religious diversity	0.17 (0.14)	0.12 (0.11)	0.18 (0.17)	0.40	0.21 (0.16)	0.10 (0.11)	0.01
NGO (b)	0.67 (0.52)	0.76 (0.44)	0.67 (0.48)	0.80	0.68 (0.48)	0.73 (0.46)	0.75
Recruitment (b)	0.33 (0.52)	0.53 (0.51)	0.52 (0.51)	0.70	0.41 (0.50)	0.59 (0.50)	0.24
Plantation	266.67 (364.29)	232.65 (364.76)	111.10 (157.01)	0.31	234.91 (353.59)	123.64 (184.59)	0.20
Share of young men	1.83 (0.98)	1.88 (0.93)	1.48 (0.85)	0.36	1.23 (0.50)	2.13 (0.99)	0.00
Share of displaced people	0.70 (0.09)	0.81 (0.12)	0.68 (0.13)	0.00	0.68 (0.13)	0.78 (0.12)	0.01

Table A4.2: Frequency distribution of numbers of tokens shared in each round

Tokens	Round 1		Round 2		Round 3		Round 4		Round 5	
shared	Freq.	Perc.	Freq.	Perc.	Freq.	Perc.	Freq.	Perc.	Freq.	Perc.
0	373	34.99	351	32.83	323	30.94	382	36.07	382	35.6
1	186	17.45	219	20.49	233	22.32	224	21.15	212	19.76
2	282	26.45	254	23.76	228	21.84	236	22.29	232	21.62
3	108	10.13	133	12.44	130	12.45	93	8.78	108	10.07
4	76	7.13	71	6.64	81	7.76	83	7.84	91	8.48
5	41	3.85	41	3.84	49	4.69	41	3.87	48	4.47
Total †	1066	100	1069	100	1044††	100	1059	100	1073	100

† Some observations were dropped due to erroneous ID codes.

†† There are fewer observations in round 3 because the data sheet from this round was lost for one community. Note that the results in the text are based on play in round 5 (and are robust to picking another round, or using average play across the rounds).

Table A4.3: Public contribution and private investment, community level

	Public contribution			Private investment		
	(1)	(2)	(3)	(4)	(5)	(6)
Missing seed %	-0.068***			-0.660		
	(0.021)			(0.479)		
Missing seed		-0.425***			-3.757	
		(0.147)			(3.268)	
Missing any			-0.357**			-7.669***
			(0.141)			(2.711)
Male	-0.105	-0.218	-0.174	-0.225	-1.370	0.261
	(0.515)	(0.526)	(0.544)	(11.65)	(11.72)	(10.49)
Age	0.360***	0.297**	0.287**	4.659*	4.027	4.019*
	(0.112)	(0.113)	(0.116)	(2.537)	(2.505)	(2.235)
Age squared	-0.004***	-0.003**	-0.003**	-0.040	-0.033	-0.033
	(0.001)	(0.001)	(0.001)	(0.026)	(0.026)	(0.023)
Years of education	-0.101	-0.126	-0.150*	2.863	2.599	2.330
	(0.078)	(0.080)	(0.082)	(1.775)	(1.771)	(1.577)
Rubber tapping	0.093	0.319	0.397	-7.487	-5.178	-4.981
	(0.532)	(0.537)	(0.552)	(12.04)	(11.96)	(10.65)
War attack	-0.053	-0.405	-0.707	3.989	0.578	-6.154
	(0.785)	(0.800)	(0.834)	(17.77)	(17.80)	(16.09)
Family share	0.465	0.313	0.0521	11.60	10.24	2.849
	(0.558)	(0.575)	(0.612)	(12.62)	(12.81)	(11.80)
NGO	0.170	0.167	0.173	-8.536*	-8.568*	-8.453**
	(0.188)	(0.193)	(0.199)	(4.245)	(4.291)	(3.832)
Recruitment	-0.261*	-0.273*	-0.251*	-1.805	-1.904	-1.722
	(0.138)	(0.142)	(0.146)	(3.130)	(3.168)	(2.824)
Main road	0.234	0.420	0.380	0.941	2.653	3.213
	(0.250)	(0.259)	(0.265)	(5.659)	(5.762)	(5.117)
Plantation	-0.0005	-0.0003	-0.0004	-0.0004	0.001	0.0002
	(0.0003)	(0.0003)	(0.0003)	(0.007)	(0.007)	(0.006)
Share of young men	-0.114	-0.193**	-0.163*	3.029	2.269	2.767
	(0.087)	(0.086)	(0.089)	(1.960)	(1.923)	(1.721)
Ethnic diversity	-0.503	-0.387	-0.504	-12.23	-11.05	-14.27
	(0.480)	(0.490)	(0.510)	(10.86)	(10.91)	(9.827)
Religious diversity	1.668**	1.868**	2.002***	-3.537	-1.666	2.279
	(0.656)	(0.673)	(0.698)	(14.83)	(14.98)	(13.47)
Share of displaced people	0.397	0.423	0.388	23.32	23.38	25.32*
	(0.649)	(0.668)	(0.688)	(14.68)	(14.87)	(13.27)
Constant	-5.266**	-3.956	-3.557	-104.0*	-90.99	-86.23*
	(2.394)	(2.407)	(2.479)	(54.16)	(53.58)	(47.81)
N	44	44	44	44	44	44
R^2	0.680	0.663	0.641	0.561	0.552	0.642
District FEs	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *** indicates significance at the 1% level, ** at the 5% level, and * at the 10% level.

Table A4.4: First stage results for subgroups

	Columns refer to public contribution in Table 5				Columns refer to public contribution in Table 6			
	Female >35	Male >35	Pooled >35		Off-road	Road	Pooled	
			Any miss	Any miss × Male			Any miss	Any miss × Road
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Acres Chief	0.002***	0.002***	0.002**	0.0005	-0.005	0.003**	-0.0006	0.002*
	(0.0008)	(0.0006)	(0.0008)	(0.0003)	(0.0038)	(0.0007)	(0.0023)	(0.001)
Tribe Chief	0.331*	0.502***	0.348*	-0.034	1.219**	0.367	0.756**	-0.191
	(0.168)	(0.174)	(0.167)	(0.040)	(0.247)	(0.176)	(0.276)	(0.124)
Acres × Male			0.0004	0.002**				
			(0.0006)	(0.0006)				
Tribe × Male			0.132	0.598***				
			(0.076)	(0.168)				
Acres × Road							0.003	-0.0007
							(0.002)	(0.0012)
Tribe × Road							-0.448	0.501**
							(0.307)	(0.182)
Constant	0.055	-0.271	0.198	-0.401	-0.011	1.199	0.011	0.027
	(0.535)	(0.624)	(0.540)	(0.207)	(0.622)	(0.631)	(0.684)	(0.104)
N	251	312	563	563	445	450	895	895
R^2	0.435	0.468	0.460	0.543	0.478	0.678	0.526	0.707
Partial F excl. instr.	9.51	30.26	16.70	15.83	14.58	17.05	14.26	21.25
HH controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Comm. contr.	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
District FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Clustered standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5

Corruption and Economic Activity

Micro Level Evidence from Rural Liberia

Gonne Beekman, Erwin Bulte and Eleonora Nillesen

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Oxford Development Studies, 2015 43(3), pp.349-360 [1]

[1] Results concerning the relationship between corruption and ethnic identity are published in a follow-up article, entitled 'A Note on Targeting by Predatory Leaders: Evidence from Rural Liberia,' authored by Gonne Beekman and Erwin Bulte. Results from both articles are combined in this chapter.

Abstract

We study how corruption affects economic activities of households in rural Liberia. A proxy of corruption of community leaders is obtained by directly monitoring the diversion of inputs associated with a development project. We measure quantities of these inputs twice; before and after the chief stored them, and interpret any 'gaps' between these measurements as indicative of diversion by the chief (or corruption). We use this 'gap' proxy to explain variation in economic behaviour across respondents, and find that corrupt community leaders cause reduced levels of income generating activities that are economically important: corruption leads to a 50% reduction in rice planted and to nearly equally large reductions in trade activity. We also find that ethnic ties mediate the intensity of stealing pressure. The adverse effects of predation are largely or exclusively driven by the responses of those individuals with a different ethnic identity than the chief's.

5.1 Introduction

An extensive and rapidly growing literature examines the multi-faceted relation between corruption and economic performance. This literature has gradually shifted from analyses based on aggregate data and perception indices (see, for example, Gupta & Abed 2002) to micro-based research, occasionally including experimental methods (see Serra & Wantchekon 2012 for a recent overview). Part of the literature considers the *determinants* of corruption, and probes the scope for limiting the incidence of corruption via various policy measures. The other part focuses on the complementary question: how does corruption affect economic performance? This literature increasingly reaches the conclusion that corruption is bad for growth and development. For example, while early literature on the effects of corruption produced rather mixed evidence (e.g. Mauro 1995) and occasionally argued that corruption may 'grease the wheels' of a rigid bureaucracy,[2] most recent articles are rather more critical about the consequences of corruption. Aidt (2003) warns that the notion that efficient corruption may offset government failures is based on 'second-best reasoning.' He argues corruption often *creates* government failure, rather than repairs it (see also Rowley 2000, on the difference between rent seeking and rent extraction). Aidt (2009) finds a strong negative correlation between growth of per capita wealth and corruption, and concludes that "corruption is much more likely to sand than to grease the wheels" (p. 276). Similarly, a recent review by (Olken & Pande 2012) concludes corruption is widespread and pervasive, and induces efficiency costs. According to an estimate by the World Bank Institute (cited in Sequeira 2012, p. 145), some 25% of African states' GDP is lost to corruption each year. Hence, the costs of rent extraction ('grabbing') due to corruption will in most circumstances outweigh the benefits of rent sharing ('greasing').

[2] See, for example, Shleifer and Vishny (1994), and the discussion about 'efficient corruption' and side payments in Aidt (2003). In line with such reasoning, greater corruptibility may increase investments in pollution abatement technology (Fredriksson & Wollscheid 2008), and offer a 'helping hand' for FDI provision by multi-national enterprises (Barassi & Zhou 2012; Egger & Winner 2005).

Corruption can undermine growth and development via various channels. Macro studies suggest it tends to hamper international trade (de Jong & Bogmans 2011) and impede country-level FDI inflow (Busse & Hefeker 2007). In addition, corruption may have adverse distributional consequences (Olken 2006), and could have long-term consequences (e.g., by undermining the supply of education and health care services, see, for example, Reinikka and Svensson, 2004). A micro-oriented literature considers the direct consequences of corruption for firms, exploring consequences for investment choices (Egger & Winner 2006; Wei 2000). If corruption acts as a tax, or leads to uncertainty and high transaction costs (Fisman & Miguel 2007), it drives a wedge between actual and privately appropriable levels of output—discouraging private levels of input supply (see also Edgardo Campos et al., 1999). In addition to such under-investment, corruption may invite evasive yet costly behaviour (Sequeira & Djankov 2010) and affect the direction of investments. In a setting where corruption pressure is endogenous, firms may rationally invest in inefficient but malleable 'fly-by-night' technologies to improve their bargaining position vis-à-vis corrupt bureaucrats (Svensson 2003a).

The objectives of this chapter are twofold. First, we contribute to the debate on the economic consequences of corruption by analysing how corruption affects economic choices for a sample of smallholder farmers in rural Liberia. We analyse the causal effect of local corruption on certain economic activities that are at the heart of Liberian policies to reduce poverty and achieve food security (e.g., Hilson & Van Bockstael 2012). While strengthening governance, at various levels including the local one, is widely perceived as a precondition for agrarian development (World Bank 2007), we are not aware of empirical research analysing the consequences of corruption on production decisions of smallholder farmers in Africa. Bates (1981) argued African farmers may opt for subsistence farming to avoid corruption in input and output markets, but this hypothesis remains to be rigorously tested using micro data. Such testing is important as it could, for example, inform NGOs and multilateral agencies about whether or not the quality of local governance should play a role in the design of agricultural development strategies. The urgency of these questions is now more pressing than ever. The majority of the world's poor continue to live in rural areas and their livelihoods tend to be intimately linked to agriculture.

Moreover, agricultural development – intensification and commercialisation of farming – is prominently back on the international development agenda as a strategy to pursue sustainable and pro-poor development (World Bank 2007; Christiaensen et al. 2011).

Second, we address the question about corruption is targeted at specific individuals or social groups. Little is known about who is targeted by corrupt leaders. Insofar as corruption is a symptom for failing leadership and governance more broadly, one may expect people to be affected in the same way. Alternatively, corrupt officials may target specific social groups more intensely than others. Based on what criteria, if any, will corrupt officials select their victims? We investigate whether ethnic ties between chief and villager are a factor explaining such targeting.

The importance of ethnic identity as a determinant of policy making and public good provision is well understood. Some studies focus on ethnic fractionalisation as a factor impeding effective public good provision (e.g. Alesina and La Ferrara, 2005; Habyarimana et al., 2007). Others consider the ethnic identity of political incumbents, and examine the extent of ethnic favouritism in spending decisions by the state (e.g., Burgess et al., 2013; Hodler and Raschky, 2014). Economists have also examined the role of ethnic identity as a driver of economic performance at the micro level, highlighting that ethnic ties may reduce transaction costs, lower costs of contractual enforcement, and facilitate screening of (business) partners (e.g. Ali & Peerlings 2011). Bowles and Gintis (2004) analyse the persistence of ethnic-based networks based on the assumption that ethnic identity promotes within-network trust (facilitating coordination), and argue that the resulting parochial sentiments may imply "social distinctions and intolerance of strangers" (p.3).

Starting from these premises, we ask whether ethnic ties matter for corruption targeting as well. This fits in a larger literature focusing on 'clientelism' and the role of ethnic identity therein. For example, Posner (2005, p.3) writes "people want resources from the state [and] believe that somebody from their ethnic group in power will facilitate their access to those resources" (see also Bratton et al. 2012). The latter perspective implies that ethnic identity plays a key role in the context of coalition building or patron-client networks: chiefs seeking to garner support by taking

resources from the community at large and from non-co-ethnics, and channelling them towards co-ethnics. However, this issue has not been explored empirically at the micro level. The contribution of this chapter is that we probe whether ethnic identity (relative to that of the chief) is a factor determining susceptibility to predation. Specifically, we ask whether aggregate patterns in the data by Beekman et al. (i.e., villagers governed by a corrupt chief invest less) are especially driven by the behavioural responses of the sub-group of non-co-ethnics.

We use an objective approach to gauge corruption. Building on a small number of recent articles we obtain two measurements of a flow of agricultural inputs allocated to the community – before and after these inputs have been given in custody to the local chief. We employ the 'gap,' if any, between these measurements as the basis for several corruption proxies, and then seek to explain key economic variables by these corruption variables. Our main finding is that corruption undermines productive private investments and the propensity to engage in trade. This, in turn, could sustain poverty. Furthermore, our findings support the perspective that ethnicity matters—while co-ethnics of the chief appear relatively unresponsive to a context of corruption, non-co-ethnics reduce investment levels when their chief diverts community resources. The behaviour of non-co-ethnics explains most of the variation in our data.

This chapter is organized as follows. In section 5.2 we briefly summarize some key recent micro studies on corruption. In section 5.3 we outline our data and identification strategy, and section 5.4 contains our results. Section 5.5 analyses mediating effects of ethnic identity on the effects from corruption. Finally, section 5.6 concludes.

5.2 Micro-based corruption studies: minding the gap

This chapter seeks to contribute to the relatively small set of corruption studies based on micro data. While aggregate cross-country studies tend to be based on perception-

based corruption measures, a strong point of much of the micro work is objective measurement of the incidence and extent of corruption. Sequeira (2012) provides an extensive discussion of recent advances in measuring corruption in the field.[3] In addition to efforts to directly observe corruption and bribing in the field (e.g., Bertrand et al., 2007; Olken and Barron, 2009), corruption may be measured via a 'forensic economic' approach based on a comparison of official data and the equilibrium predictions of theoretical models (e.g., Fisman, 2001). However, the most common approach to measuring corruption in the field is by 'minding gaps in the data,' suggesting corrupt behaviour. Such gaps might be identified in case of mismatches between different data sources; mismatches between administrative data and results from an independent household study; or simply because two primary sources of data do not add up (as in our measurement strategy, outlined below).

An early and influential article based on gaps in the data is Reinikka and Svensson (2004). They analysed diversion of national grant money allocated to primary schools. On average, approximately 80 cents from every dollar disbursed by the national government was diverted. Money thus stolen was perhaps used by local level officials to strengthen their patronage network or finance political activities. In a follow-up article, the authors investigate the impact of a possible solution: a newspaper campaign publishing data on monthly transfers (when and how much) to the various districts, facilitating monitoring at the grassroots level (Reinikka & Svensson 2011). In this context, such newspaper campaigns are cost-effective in reducing capture of public funds.

Another well-known study based on an analysis of gaps in the data is Olken (2007), who investigates corruption of a national road-construction project in Indonesia, using 'missing expenditures' as a measure of corruption. Missing expenditures are defined as the difference between reported and actual costs of constructing the road (as estimated by a team of engineers). 'Missing expenditures', thus defined, account for about 24% of the total costs of road construction. Moreover, upon combining the

[3] See also Olken and Pande (2012) for a review of other approaches measuring corruption.

gap analysis with random assignment of communities into various anti-corruption interventions, Olken found that external audits are more effective in reducing corruption than grassroots monitoring. In another article, examining the workings of a subsidized rice transfer program in Indonesia, Olken (2006) identifies gaps based on a comparison of official records and household survey data. A considerable share of the rice (18%) could not be accounted for, suggesting the presence of corruption.

Niehaus and Sukhtankar (2013) use a similar approach to study corruption in India. They compare official figures from the National Rural Employment Guarantee programme (days worked and wages paid) to survey responses obtained from alleged beneficiaries. By measuring corruption as the gap between reported and actual payments, they observed that an exogenous policy change in program wages makes agents cautious about future extractable rents, resulting in a dampening effect on corruption.

5.3 Data and identification

In this study, we explore how corruption affects two economically important activities in Liberia: rice production and petty trade. Farmers mainly produce for subsistence, as reflected in the dominant crops: cassava, (paddy) rice and sugar cane. 'Making a market' (petty trade) is considered an important source of (extra) household income. With peace being maintained by UNMIL since 2003, the country has seen a revival of petty-trade in Liberia (Richards et al. 2005). We selected rice for its special status in Liberia: it is the key staple food – a meal without rice is not considered a true meal – and it is commonly used as payment to labourers in the diamond mines (Hilson & Van Bockstael 2012) and to feed *kuu*-labourers (rotational agricultural labour schemes).

5.3.1 Sample selection

We randomly selected 44 communities, part of an agricultural development project by an international NGO. All communities are located within a day drive of the capital Monrovia, but road conditions are poor. Of these communities, 22 are connected by a main road, and 22 communities have access to clean water. None of them has

electricity. The most important sources of household income are subsistence agriculture, petty trade and off-farm labour (rubber tapping, coal burning and preparing local gin from sugar cane). Communities are typically very small – 45 households on average. Household surveys were carried out in October-December 2010, among a random selected subsample of 16-20 households in each community. We also conducted a community survey among community leaders. We tracked the diversion of inputs in February 2011 (see below). Survey data, including our dependent variables, were collected prior to disbursement of the inputs, and there is no direct, mechanical link between inputs received and production decisions.

5.3.2 Measuring corruption

As discussed in section 2, analysts increasingly use corruption measurements based on 'gaps' in the data. Our 'gap measure' is based on two distinct measurements of a flow of project inputs – rice seeds, vegetable seeds, and agricultural tools.[4] Specifically, we carefully measured all inputs prior to distributing them to the field (measure 1). We then asked the chief to store these inputs in his private house for two days, after which a project facilitator would make an inventory and publicly distribute the inputs to beneficiaries in the community (measure 2). The difference between these two measures is the basis for our corruption proxies. Obviously we did not disclose information about missing inputs to either the chief or community members. Advantages of our gap measure include the fact that it does not pick up errors due to incompetent bookkeeping (a possible source of bias in official data), and is not based on potentially biased survey responses (see Sequeira 2012 for a discussion).

We construct several measures of corruption. First, we create a binary variable indicating whether any vegetable or corn seed was missing. We excluded rice from this variable because of its special status in Liberia. Stealing rice would be considered highly inappropriate and the data reflect this: rice was only diverted in eight

[4] Seeds: 25 kg upland or paddy rice (depending on land type); 3 kg beans and peanuts; 5 kg corn; 20 gram pepper seed; 5 gram bitterball seed. Tools: cutlasses and regular hoes (4 each); shovels, files and watering cans (2 each).

communities (but including these observations does not alter any of our results). As a robustness check we also use a continuous measure of diverted inputs ('percentage missing') and another binary variable capturing any missing inputs – that is, also including rice and (or) tools.

In 36% of our communities we observed missing seed, and in 48% of them we observed missing inputs. On average, 0.19 kg (about 2%) seed was diverted (0.53 kg or nearly 5%, considering the sub-sample with missing inputs). To the extent that missing inputs are a reasonable (albeit noisy) proxy for corruption among chiefs, these results suggest a non-negligible amount of corruption. Creative chiefs have access to multiple avenues to misuse their public role for private gain.[5] We assume that theft of project inputs is correlated with these alternative opportunities for corruption and misuse of office.

5.3.3 Indicators of economic behaviour

Does the shadow of corruption influence economic behaviour of community members? We collected household data on two economic activities: (*i*) amount of rice seed planted, representing a labour investment in the rotational fallow system practiced in these communities, and (*ii*) engagement in petty trading activities.

Planting rice is among the most labour intensive activities among small-scale Liberian farmers. The land first needs to be manually cleared from shrubs and bushes with machetes and other small hand tools, which is physically demanding and takes up a considerable amount of time that could have been spent on more economically-productive activities, such as tapping rubber. Corrupt local authorities may provide a disincentive for people to commit to such laborious (often communally undertaken) tasks. We hypothesise that, if the chief is corrupt, community members may fear that

[5] They control and allocate communal resources, including communal land and revenues from local enterprises (communal plantations). They may also ask for contributions, either in cash or in kind, for example to feed workers on communal plantations (the revenues of which will be controlled by the chief).

the output from agricultural production may also be appropriated. Petty trade provides cash income to the household, enabling households to buy basic necessities. Given the high level of subsistence farming in most Liberian communities, trading activities provide a unique source of cash inflow. However, cash is potentially subject to direct (e.g. stealing) or indirect (e.g. asking for monetary contributions) appropriation by corrupt chiefs, so again disincentive effects may matter.

These, and other data, are summarised in Table 5.1. Our dependent variables and corruption indicators are provided in panels A and B. Note that our dependent variables are only weakly correlated to one another. The amount of rice planted is marginally negatively correlated to involvement in trading activities (correlation coefficient is -0.07; p=0.105). Characteristics of the chief are summarized in Panel C (ethnic identity and land ownership). Household controls are summarized in Panel D (gender of respondent, age, education, history of exposure to violence, assets) and community controls in Panel E (community size, infrastructure, availability of local markets, ethnic and religious diversity, NGO activities, communal resources, share of the population displaced).

Table 5.1: Summary statistics

Variable	N	Mean	SD	Min	Max
A: Main variables					
Rice planted in 2009 (tins)	519	1.975	2.836	0	23
Involved in trading activities? (b)	518	0.407	0.492	0	1
Same tribe (b)	497	0.680	0.467	0	1
B: Corruption indicator					
Missing vegetable and corn seed (%)	44	0.190	0.353	0	12.67
Missing vegetable and corn seed (b)	44	0.364	0.487	0	1
Any input missing (b)	44	0.477	0.505	0	1
C: Characteristics of the chief					
Years in office	44	5.705	6.504	0	32
Chief belongs to major ethnic group (b)	44	0.795	0.408	0	1
Acres of land owned	43	14.453	56.192	0	350
Years of education	44	4.771	4.314	0	12
D: Household controls					
Male (d)	518	0.479	0.500	0	1
Age	519	43.064	14.260	11	90
Involved in rubber tapping (b)	519	0.250	0.434	0	1
Household attack during war (b)	518	0.301	0.459	0	1
Years of education	519	2.440	3.593	0	16
Household size	519	6.784	2.966	3	15
Attacks per household *(vill. mean)*	44	0.3	0.124	0.091	0.654
Share of family in the community (vill. mean)	44	0.348	0.140	0.117	0.726
E: Community controls					
NGO (b)	44	0.705	0.461	0	1
Number of households in community	44	45.38	36.141	3	145
Rebel recruitment during war (b)	44	0.5	0.506	0	1
Main road (b)	44	0.5	0.506	0	1
(Weekly) market in the community (b)	44	0.091	0.291	0	1
Plantation owned by community (acres)	44	179.273	284.370	0	1500
Share of young men (12-25 years)	44	1.683	0.901	1	4
% of people displaced during war	44	0.731	0.132	0.364	1
Ethnic heterogeneity (Herfindahl index)	44	0.349	0.216	0	0.767
Religious heterogeneity (Herfindahl index)	44	0.098	0.123	0	0.498

(b) = binary variable

5.3.4 Identification

We first estimate an OLS model to explain variation in behaviour across households

$$Economic\ behaviour_{ij} = \alpha + \beta_k + \beta_1 Corrupt_j + \beta_2 Comm_j + \beta_3 X_{ij} + \varepsilon_{ij}, \quad (1)$$

where subscript i indexes household i=1,…,N and subscript j indexes community j=1,…,44. We also use province fixed effects (β_k, k=1,2,3) to capture unobservable factors that might vary at this higher level of organization. Our coefficient of interest is β_1, which should be negative if corruption discourages private economic activities.

Equation (1) may suffer from endogeneity bias. In particular, our estimate of β_1 may be biased due to reverse causality (if corruption responds to economic activities chosen by community members) or omitted variables (unobserved factors driving both corruption and investment decisions). We therefore also estimate an IV model, using a vector of characteristics of the chief as excluded instruments, Z_j. Specifically, for our base model we use the chief's ethnic identity, land ownership, and the interaction between these variables. Ethnic identity is measured as a binary variable, with value 1 if the chief belongs to the dominant ethnic group in the region (the Kpelle), and zero otherwise. Ethnic identity might affect corruptibility because of three reasons. Kpelle chiefs may (expect to) be subject to less scrutiny by co-ethnic community members, reflecting higher co-ethnic trust or aligned preferences, so that the costs of corruption are lower. In addition, the presence of many co-ethnics may imply a higher redistributive burden within the co-ethnic network, resulting in higher pressure to divert communal resources. Potentially offsetting these effects, co-ethnicity may invite more altruistic behaviour of the chief, and reduce stealing for private consumption.

Our second excluded instrument is land ownership, which is measured as the total number of acres of land owned by the chief. This variable may drive corruption because land holdings may affect the value marginal product of agricultural inputs (assuming the chief would use stolen inputs on his own land, rather than sell them or redistribute him within patron-client networks), and the size of these landholdings

affects the chief's ability to use these inputs without being noticed.[6] Possibly offsetting these effects is a simple income effect – large landowning chiefs may have less need to be corrupt because they have sufficient alternative means to support themselves and their social role. We believe land is sufficiently exogenous in the Liberian context to be considered as an instrument. Land is traditionally inherited, so landholdings should be unrelated to current economic behaviour of community members (recall our discussion of the election of chiefs in rural Liberia, in section 3 above).

Finally, we also interact land ownership and ethnic identity, based on the assumption that the conditional effect of ethnicity is 'more exogenous' than either of the two variables alone.[7] The model we estimate reads as follows. In the first stage we estimate

$$Corrupt_{ij} = \mu + \gamma k + \gamma Comm_j + \gamma X_{ij} + \gamma Z_j + \upsilon_{ij}, \qquad (2)$$

where υ denotes the error term. In the second stage of the model we estimate equation (1), and replace observed corruption by predicted corruption, based on equation (2). We always cluster our standard errors at the community level. [8]

[6] This may be relevant for all inputs, but especially for stolen rice seed, because the distributed rice were of a special improved type (a NERICA variety) that has a different growing cycle and looks somewhat different so that villagers can easily distinguish this type from the regular type.

[7] We thank one of the reviewers for this suggestion.

[8] We have also probed alternative instruments. For example, we also tried 'years in office' as part of our set of instruments. This variable passed all relevant test statistics, and the 2nd stage results were fully consistent with the ones reported below. However, one referee was concerned about whether the exclusion restriction was satisfied and argued that 'years in office' may also influence which economic activities are undertaken in the village. We agree, and decided to remove this variable from the set of preferred instruments. We also probed 'education of the chief' as an additional instrument (alone and interacted with ethnicity). However, the partial F-value was modest, and the exclusion restriction was not robustly satisfied (in a Hansen-J test, p-values ranged from 0.01 to 0.15 depending on the specification used). The level of education of the chief may of course affect economic possibilities in the village through various channels.

5.4 Empirical results

Table 5.2 presents the results of a number of OLS regressions explaining economic activities: rice planted in the previous season (columns 1-3) and involvement in trading activities (columns 4-7). Columns (1) and (4) present the results of our most parsimonious specification that does not include controls or fixed effects. While we find no effect for rice, there is a significant and negative relation between corruption and trading activities. Since underlying regional or household level differences may obscure the relation between corruption and economic activities, we estimate more comprehensive models in what follows. In Columns (2) and (5), we added a set of standard household demographics (gender of the household head, education, age), community controls (presence of a main road, and ethnic heterogeneity), and province level fixed effects.[9] For both variables we now find a (marginally) significant negative correlation. Columns (3) and (6) present a 'full' specification that includes other control variables relevant in the post-war context of rural Liberia. These include, for example, proxies for displacement, violent attacks, NGO presence, and involvement in rubber tapping. Both rice planting and trading are now significantly and negatively correlated with corruption. In column (7) we report the results of a probit model (marginal effects) explaining whether or not the household is engaged in trading. These results are very similar to the ones of the linear probability model in column (6).[10]

With respect to the controls we find that male household heads are more likely to plant rice, and that education reduces this probability (e.g. more educated people may

[9] Since we have a large proportion of zero's in our dependent we re-estimated column (1)-(3) running a Tobit regression. Results are qualitatively the same.

[10] The reason to favour OLS over probit or logit models in columns (4)-(6) is that we want to be able to compare the OLS results to the outcomes of the IV estimates. Yet, since IV probit only produces valid estimates with a continuous endogenous variable (and ours is binary) we need to resort to a 2SLS when using the instruments. Hence we prefer to compare our 2SLS estimates to a normal OLS with a binary dependent. All outcomes fall neatly within the predicted 0-1 interval except for column (6). Here 0.11% of the estimates are predicted just below 0. We therefore re-estimate the model using Maximum Likelihood. Results are very similar.

have alternative income sources). The amount of rice planted increases with age, but only up to age 48. Significant community level variables include belonging to an ethnically diverse community, owning a communal plantation, and having a large percentage of displaced people. All these variables are positively associated with rice planting. Turning to the trade models, we see that being older (more experienced) is positively associated with trading, but only up to age 47. It also appears as if rubber tapping is a substitute activity for trading. In communities owning a plantation, people are less likely to be involved in trading.

Summarizing the main insights of these OLS results, we find a significant negative correlation between corrupt chiefs and economic activities. Farmers in communities with a corrupt chief plant 1 tin of rice less, which amounts to nearly a 50% reduction in rice planted. Corrupt chiefs are also associated with a smaller probability that community members choose to engage in trade (a reduction of 13 percentage points). This, too, is an economically significant effect as, according to our data, some 40% of all households are engaged in trade. Yet, these findings may suffer from endogeneity bias and need to be interpreted with caution. We therefore proceed with our preferred IV model.

Table 5.2: Corruption and economic activities, OLS models

	Rice			Trade			
	OLS	OLS	OLS	OLS	OLS	OLS	Probit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Seed missing	-0.415	-0.786*	-0.856**	-0.108**	-0.103*	-0.131**	-0.137**
	(0.366)	(0.473)	(0.376)	(0.049)	(0.058)	(0.058)	(0.059)
HH is male		0.770**	0.793***		-0.043	-0.084	-0.089
		(0.296)	(0.284)		(0.057)	(0.058)	(0.060)
Education		-0.114**	-0.118**		0.003	0.006	0.007
(yrs)		(0.048)	(0.044)		(0.007)	(0.008)	(0.008)
Age		-0.002	0.112**		-0.001	0.027***	0.030***
		(0.011)	(0.042)		(0.002)	(0.008)	(0.010)
Age^2			-0.001**			-0.0003***	-0.0003***
			(0.001)			(0.0001)	(0.0001)
Road		0.375	0.845		-0.084	-0.047	-0.066
		(0.325)	(0.540)		(0.061)	(0.062)	(0.067)
Ethnic		1.894**	2.125**		0.066	-0.126	-0.141
heterogeneity		(0.786)	(1.007)		(0.135)	(0.138)	(0.140)
Rubber			0.028			-0.194***	-0.200***
tapping			(0.244)			(0.057)	(0.057)
Household			0.250			-0.048	-0.054
Attack			(0.294)			(0.052)	(0.056)
Family share			0.148			0.025	0.026
			(0.519)			(0.090)	(0.096)
NGO			0.221			-0.078	-0.081
			(0.340)			(0.058)	(0.062)
Recruitment			-0.164			0.007	0.009
			(0.339)			(0.051)	(0.052)
Market			-0.023			-0.003	-0.007
			(0.589)			(0.125)	(0.126)
Plantation			0.002***			-0.0002*	-0.0002*
			(0.001)			(0.0001)	(0.0001)
Young men			0.121			-0.034	-0.034
			(0.201)			(0.036)	(0.038)
Religious			0.307			0.350	0.363
heterogeneity			(1.533)			(0.220)	(0.233)
Displaced			2.461**			0.077	0.074
			(1.204)			(0.241)	(0.253)
Constant	2.114***	1.110	-4.562***	0.449***	0.606***	0.196	
	(0.254)	(0.872)	(1.106)	(0.0350)	(0.145)	(0.266)	
Province FE	No	Yes	Yes	No	Yes	Yes	Yes
N	526	430	427	518	419	403	403
R^2 (Pseudo R^2)	0.005	0.060	0.127	0.011	0.036	0.102	(0.080)

Notes: Standard errors are clustered at the community level and in parentheses. * indicates significance level at the 90 percent level, ** indicates significance level at the 95 percent level, and *** indicates significance level at the 99 percent level. Column (7) reports marginal effects, calculated at the mean.

Table 5.3 reports the results of our IV model. While all models were estimated with the full set of controls, we only report results for the corruption variable (other results are available on request). Panel A contains the results of the first stage. Columns (1) and (2) report the results for 'planting rice' and columns (3) and (4) for 'involvement in trading activities'. The first stage shows that chiefs belonging to the majority ethnic group and chiefs owning more land are more likely to be corrupt across all specifications. The interaction effect between ethnicity and land ownership in columns (2) and (4) also matters.[11] The partial F's of our first stage regressions exceed the common threshold value of 10, suggesting our instruments are sufficiently relevant.

The 2nd stage IV results are summarised in panel B. Columns (1) and (3) are based on the two excluded instruments, and columns (2) and (4) also include the relevant interaction term (which does not matter for our results). Overall, we find the 2SLS results are consistent with the OLS results above. However, the coefficients are larger, and even more robust to changes in the set of control variables (recall that endogeneity and measurement error may bias OLS estimates towards zero).[12] These findings suggest that the adverse associations between corruption and investment and trading may be interpreted as causal relationships: if the chief is corrupt, community members respond by lowering (labour) investments and are more likely to refrain from starting commercial activities.

Finally, in Table 5.4, we further probe the robustness of our results by using two alternative measures of corruption (based on the same data). Specifically, we use a variable capturing whether any inputs (rice, vegetable seed, or tools) were stolen, and

[11] It may be difficult to interpret the interaction term in the first stage. We therefore report results both with and without an interaction between ethnicity and land ownership.

[12] Our IV results provide an upper bound of the true effect if the instruments are positively correlated with omitted variables with the same sign as corruption in the economic activities models (Pande & Udry 2005). Part of the increase in the coefficient is due to the drop in observations in the IV model, as chief landholding is missing for one community. The drop in observations accounts for 49% of the coefficient increase for rice planting, and for 18% for trading activities.

we use a variable indicating the share of inputs taken. Our results are robust with respect to this extension: across all specifications, and for both dependents, we find that corruption undermines economic behaviour.

Table 5.3: Corruption and economic activities, IV models

Panel A: First stage				
Dependent (binary)	Seed missing			
	(1)	(2)	(3)	(4)
Land ownership (chief)	0.003***	0.027**	0.003***	0.026*
	(0.0005)	(0.013)	(0.0005)	(0.013)
Ethnicity (chief)	0.216*	0.309**	0.217*	0.312**
	(0.122)	(0.136)	(0.125)	(0.140)
Land * Ethnic		-0.024*		-0.023*
		(0.013)		(0.013)
Panel B: Second stage				
Dependent	Rice planted		Trading activities	
Seed missing	-1.435***	-1.599***	-0.252**	-0.257**
	(0.548)	(0.554)	(0.123)	(0.124)
Controls				
Household controls	Yes	Yes	Yes	Yes
Community controls	Yes	Yes	Yes	Yes
Province FE	Yes	Yes	Yes	Yes
Test statistics				
N	417	417	394	394
Partial F excl. instr.	32.79	19.25	34.82	20.42
R^2	0.121	0.118	0.097	0.097
Hansen-J	0.891	0.483	0.491	0.776

Notes: Standard errors clustered at the community level in parentheses; * indicates significance level at the 90 percent level, ** indicates significance level at the 95 percent level, and *** indicates significance level at the 99 percent level. All specifications include the full set of household and community controls listed in column (3) of Table 5.2.

Table 5.4: Robustness - different measures of corruption (binary and continuous), IV models (second stage)

	Rice planted				Trading activities			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Missing any inputs	-1.205** (0.589)		-1.333*** (0.491)		-0.242** (0.122)		-0.221* (0.122)	
% Missing seed		-24.98** (12.62)		-27.59** (12.98)		-4.652** (2.188)		-4.779** (2.281)
Controls								
HH controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Comm. controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Test statistics								
N	417	417	417	417	394	394	394	394
R^2	0.130	0.066	0.129	0.054	0.078	0.054	0.083	0.051
Partial F excl. instr.	26.67	8.53	12.86	5.44	27.96	10.44	13.19	6.54
Hansen-J	0.263	0.598	0.493	0.496	0.845	0.791	0.828	0.915

Notes: Standard errors clustered at the community level in parentheses; * indicates significance level at the 90 percent level, ** indicates significance level at the 95 percent level, and *** indicates significance level at the 99 percent level. All specifications include the full set of household and community controls listed in column (3) of Table 5.3.

5.5 Corruption and ethnic identity

A key (conditioning) variable in the analysis that follows is the ethnic identity of our respondents and of the chief. While there are 14 ethnic groups in the sample, the ethnic majority are the Kpelle (74 percent). We are especially interested in whether village respondents share the same ethnic identity as their chief. We speculate co-ethnics are relatively immune to predation and informal taxation by the chief—indeed; they may benefit from it, via ethnic-based patronage networks.

We augment model (1) by introducing the ethnic identity of respondents. Specifically, we introduce a new variable and consider whether the respondent has the same ethnic identity as the chief. If so, the binary variable E_{ij} takes a value of 1 (else it is zero). We estimate the following model:

$$Farm_{ij} = \alpha + \beta_k + \beta_1 Corrupt_j + \gamma_1 E_{ij} + \gamma_2 Corrupt_j \times E_{ij} + \beta_2 Comm_j + \beta_3 X_{ij} + \varepsilon_{ij}. \quad (2)$$

If corrupt leaders prefer to target individuals with another ethnic identity, then the response to whether or not the chief is a thief should vary across ethnic identities. Specifically, the impact of a corrupt chief on farm size for non-co-ethnics is simply β_1.

For co-ethnics, this effect is given by $\beta_1 + \gamma_2$. If co-ethnics are not targeted by predatory chiefs (and are immune to a context of predation), then we would expect $\beta_1 \approx -\gamma_2$. Finally, any differences in farm size due to co-ethnicity of villager and chief (and unrelated to targeting by a thieving chief) are captured by coefficient γ_1. Since we have no reason to expect such co-ethnicity to matter beyond the corruption channel, we expect $\gamma_1=0$.

Results are presented in columns (1) and (2) in Table 5.5. While ethnic identity per se does not affect farm size (as expected we cannot reject the hypothesis that $\gamma_1=0$), we observe that the interaction between a thieving chief and co-ethnicity is significant—both in the OLS and IV model. Consider the OLS model first. The coefficients of interest (i.e. the coefficients associated with the variable *Predation* and with the interaction term *Predation x Same ethnic*) are of opposite sign, and roughly of equal size. A Wald test confirms that $\beta_1 + \gamma_2$ cannot be distinguished from zero (p-value = 0.28). This suggests co-ethnics do not respond by reducing farm size to being governed by a corrupt chief.

While we do not find that $\beta_1 = -\gamma_2$ in the 2SLS model (p-value = 0.01), here too the response to a thieving chief is attenuated considerably for co-ethnics (by more than 50 percent). The first stage results of the new models are also presented in Table 5.3. As one of the endogenous variables is an interaction term, we include two additional interaction terms in the first stage model. As before, the test statistics associated with relevance and over-identification tests are no cause for concern (even if the partial F value is now slightly below the "conventional threshold" value of 10), and the Stock and Yogo weak identification test gives no reason for concern.

As a robustness test we have split the sample into subsamples of co-ethnics and non-co-ethnics. OLS results are reported in columns (3) and (4) of Table 5.5. As expected, the predation variable only enters significantly in the subsample of non-co-ethnics. We also estimated 2SLS models for the two subsamples, and find similar results (columns 5 and 6). The predatory variable is only weakly significant in the co-ethnics subsample, and strongly so in the non-co-ethnics subsample. In both the OLS and the 2SLS models, the coefficient size of the predatory variable is much larger in the non-co-ethnics subsample. We note that the result for the non-co-ethnic subsample in the 2SLS model may be overestimated for reason mentioned above (it is almost twice as large as in the corresponding OLS model), and is perhaps better interpreted as an upper bound.

The overall lesson from these regression models is that the adverse effects of predation are largely or exclusively driven by the responses of non-co-ethnics.

Table 5.5: Regression of rice planted on predation and ethnic identity

	Rice planted in 2009					
	OLS	2SLS	OLS Co-ethnics	OLS Other ethnics	2SLS Co-ethnics	2SLS Other ethnics
	(1)	(2)	(3)	(4)	(5)	(6)
Predation (b)	-2.075***	-2.681***	-0.626	-1.969**	-0.626*	-3.711***
	(0.468)	(0.777)	(0.376)	(0.931)	(0.369)	(1.370)
Predation × Same tribe	1.449***	1.501**				
	(0.497)	(0.739)				
Same tribe (b)	-0.035	-0.017				
	(0.536)	(0.510)				
Constant	-4.524***	-4.535***	-3.162**	-8.525**	-3.119**	-9.613***
	(0.985)	(0.954)	(1.336)	(3.438)	(1.333)	(3.604)
HH controls	Yes	Yes	Yes	Yes	Yes	Yes
Comm. controls	Yes	Yes	Yes	Yes	Yes	Yes
Province Fes	Yes	Yes	Yes	Yes	Yes	Yes
N	496	485	337	159	326	159
R^2	0.135	0.128	0.152	0.226	0.143	0.195
Sargan chi^2 (p-value)		0.95			0.90	0.81
Stock Yogo test statistics		18.58			42.02	47.40
Critical val. (5% bias)		13.97			13.91	13.91

Notes: Clustered standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

5.6 Conclusions and implications

Corrupt chiefs undermine key economic activities in the communities they govern. Community members plant less rice, and are less likely to engage in trading activities, if their chief steals community resources. While we do not test the mechanism linking corruption to production, our results are consistent with an explanation based on the assumption that corruption acts as a tax, reducing incentives to invest and produce. We speculate therefore that corruption may perpetuate poverty – not only because resources are transferred from community members to the chief, but also because of the distortive effect induced by a stealing chief. Our data suggest these distortive effects are both statistically and economically significant. Our findings are consistent with the interpretation that ethnic ties mediate the intensity of stealing pressure. We find that ethnic identity of the villagers (relative to that of the chief) determines whether predation affects investment levels, and that co-ethnics of the chiefs are much less responsive to a context of theft than non-co-ethnics.

We use corruption in the strict sense of the word (i.e., as the misuse of public office for private gain), and focus on the diversion of project inputs by the local chief (see below). Our interpretation of the data is consistent with a 'political culture of corruption' (e.g. Rowley 2000), exemplified by dominant elites and 'big men'. We acknowledge, however, that this argument may be simplistic since we do not know *how* diverted inputs are allocated. The chief might convert these inputs into private wealth, but he may also use them to cement his position in local patron-client networks[13] or even to support the neediest households in 'his community.'[14] Lacking detailed information on the allocation of diverted inputs, the 'corruption label' may not be equally appropriate for all cases of diverted inputs. However, regardless of

[13] Reno (2008) points out that corruption in Liberia is essentially organized to promote networks of patronage.

[14] If diversion would indeed result in transfers to the neediest households, there is the possibility that corruption could even be welfare-enhancing at the community level. On the other hand, any potential welfare gains could be offset by the negative consequences of reduced economic activity in the community.

whether diversion of inputs implies corruption, or not, we find it undermines productive private investments and the propensity to engage in trade.

We cannot explain why local chiefs target non-co-ethnics. Indeed, our corruption proxy does not distinguish between co-ethnics and non-co-ethnics – by diverting project resources the chief is "hurting" all project beneficiaries; co-ethnics and non-co-ethnics alike. However, our data fit a story where corrupt chiefs convert communal resources into resources supporting their patronage network — benefitting especially co-ethnics. If chiefs willing to convert communal resources into patronage networks are also more likely to "tax" individuals outside their patronage network, then our results make perfect sense. But alternative explanations do exist. For example, targeting of non-co-ethnics may be based on preferences, perhaps invited by post-war tensions across ethnic groups. Opening this black box remains a priority for future research.

Finally, we return to the implications for agricultural development strategies and development interventions. A large literature explores the two-sided relation between aid and corruption, and we believe our results on the disincentive effects of corruption speak to this literature as well.[15] Specifically, insofar as the success of development interventions varies with the provision of complementary private inputs, we expect

[15] The nature of the relation between aid and corruption, or governance more broadly, is disputed. Dalgaard and Olsson (2008), Bräutigam & Knack (2004) and Congdon Fors & Olsson (2007) analyse how institutional change responds to incentives, and demonstrate that institutional erosion can be a rational response to changing economic conditions (including specific income shocks). The availability of windfalls may fuel and sustain corruption (Rowley 2000; Djankov et al. 2008). Voors et al. (2011) find that positive income shocks induce corruption, especially in societies where corruption is already widespread. However, and in contrast, Tavares (2003) and Okada and Samreth (2012) find that foreign aid decreases corruption, possibly because of aid conditionality (requirements to improve governance). The debate about the effects of aid on institutions extends to the effect of aid on growth. For recent meta studies on this issue, refer to Doucouliagos and Paldam (2008; 2011). Among other things, and supporting some insights obtained in the current paper, they conclude that many development projects benefitted local elites rather than the targeted beneficiaries, which may have contributed to the rather bleak effects of development aid on economic growth.

projects to be more successful in less corrupt settings.[16] The main implications are twofold. First, to enhance the direct effect of interventions, projects may try to focus on the provision of inputs that cannot easily be stolen or arrange projects such that the flow of inputs is transparent to community members (thus enhancing local scrutiny). However, to the extent that project outputs can be appropriated by the chief, and the success of the project depends on the supply of effort (labour) of community members, we should still expect projects to be less successful in a corrupt environment (even if inputs themselves cannot be stolen). Therefore, and second, insofar as intervening agencies focus on promoting economic activity based on the combination of an outside project and private effort, they may decide to target communities with chiefs that are less corrupt.

[16] This is confirmed by additional work, linking the local governance setting to choices ('play') in two standard behavioural experiments (see Chapter 4). In communities with corrupt chiefs, villagers contribute less to public goods in public goods games. Moreover, and consistent with the evidence on these pages, they invest less in privately-profitable 'investment games' (lotteries). The governance environment thus affects the response of villagers to economic opportunities, as potentially provided by development interventions.

6

Does Agricultural Community Training Benefit Rural Development?

Experimental Evidence from Liberia

Gonne Beekman

Abstract

Stimulating rural development is prominently back on the international development agenda. This chapter analyses the impact from a community training and input provision project in rural Liberia on households' livelihood status and social cohesion. We present findings from a randomized controlled trial and complement them with results from a matching procedure among a larger, non-experimental sample. We vary group institutions among the treatment group in four sub-treatments, and relate the project outcome to the incidence of local corruption. We find weak evidence that the project contributed to higher rice harvests and lower dietary diversity. The project did not contribute to social cohesion. Our results suggest that the project causes unintended shifts of activities within households: the project leads to an increase of time spent on farming activities by children in targeted households. We find that most of our results are driven by the subgroup that received a direct democracy and leadership accountability treatments. Finally, we find suggestive evidence that local corruption undermines the project impact. Our results imply that, in order to measure the full impact of a project intervention, allocative efficiency of production factors must be taken into account.

6.1 Introduction

Stimulating rural development in conflict-affected areas is a key objective for many development organisations. Civil war is sometimes called 'development in reverse' (Collier et al. 2003, p.13). War is generally believed to be destructive for both infrastructure and markets and disrupts human capital accumulation, particularly on the short term (see Collier 1999; Blattman & Miguel 2010). In addition to (temporarily) hampering economic growth, civil war is believed to erode social capital (e.g., see Colletta & Cullen 2000), and the consequences of war can last for decades.[1] Many development organisations have therefore shifted their focus from short term emergency aid to more sustainable 'reconstruction' programs. These programs are often following an 'integrated' development approach in rural areas, focussing on an array of activities that target different needs at the same time (e.g., King 2013).

We evaluate a community training project in rural post-war Liberia, implemented by an internationally operating non-governmental organisation (NGO). Many rural development programs aim to stimulate rural development either through agricultural extension services or through 'community-driven' development programs by introducing new institutions or supporting existing ones. This project aims to do both. It combines elements from a 'farmer field school' (FFS) and a 'community-driven reconstruction' (CDR) program in order to improve food-security and livelihoods, as well as strengthening social cohesion. We briefly describe both approaches below.

Farmer field schools (FFS) are a popular avenue to rural livelihood development. A number of recent papers on the livelihood impact of FFS suggest that the approach may contribute to poverty alleviation and productivity growth through improved farming techniques (e.g., Davis et al. 2012). Where the FFS approach initially aimed at increasing the adoption of specific agricultural technologies such as better pest

[1] Recent empirical evidence shows that war can strengthen parochial altruism—trust and cooperation among in-group members (Gneezy & Fessler 2012; Voors et al. 2012; Bauer et al. 2014; Gilligan et al. 2014). Social cohesion between groups, however, is more likely to be weakened by civil conflict.

management or the reduction fertilizer use, advocates of the FFS approach motivate that the approach should also contribute to 'educational, social and political capabilities' (Van den Berg et al. 2007). FFS could therefore rather be seen as a model for general adult learning, focussing on farmers' empowerment than as an extension service with focus on technological outcomes alone (Van den Berg et al. 2007; Friis-Hansen and Duveskog 2012). A key characteristic of FFS is the self-selection of participants. The likelihood that FFS will be successful is largest if they are based on existing farmers groups, supported by trusted lead-farmers. For this reason, experimental evaluations of FFS are scarce. Instead, some studies employ matching techniques combined with a difference-in-differences (DD) set-up. For example, Todo and Takahashi (2013) find a large effect on agricultural income from a FFS in Ethiopia, and Larsen and Lilleør (2014) find positive effects on food security among participating households, but not on their poverty status, which may be caused by intra-household shifts of labour or consumption smoothing over time. The only randomized controlled trial on FFS we are aware of is conducted among rice farmers in China by Guo et al. (2015). The authors find mixed evidence for increase of knowledge acquisition, and effects are smaller for female and older participants.

Community-driven reconstruction (CDR) projects have increasingly gained popularity in recent years. The CDR approach is developed specifically for post-war contexts, and aims to build or reshape institutions and contribute to social reconciliation. The approach is rooted in the more general 'community-driven development' (CDD) approach, which was introduced in response to some of the shortcomings of traditional development aid programs. Mansuri and Rao (2013) estimated that the World Bank—the largest supporter of CDD programs—invested 85 billion USD on participatory development programs since the early 1990s. In the past decade the World Bank approved more than 600 CDD programs in 110 countries. These programs should be better-tailored to local needs than traditional development programs, allow for more ownership and autonomy of project participants, and welcome institutional transformation. New institutions might be adopted when local communities are exposed to democratic procedures and accountability practises. This could especially be relevant in contexts characterized by weak institutional quality. Main elements that characterize CDR projects are democratic elections of village

development councils and block grants in order to carry out commity projects (see King & Samii 2014).

Notable experimental evaluations of CDR projects were carried out in the Democratic Republic of Congo (Humphreys et al. 2015), Liberia (Fearon et al. 2009), and Sierra Leone (Casey et al. 2012). Despite great expectations, none of these studies find robust evidence that institution building contributes to social cohesion or improved development outcomes. In fact, it is questionable whether 'new' institutions can really improve on existing ones within the brief timeline of the project activities. Newly introduced institutions are found to be ineffective if communities rely on traditional (informal) institutions (Fearon et al. 2013), and impacts of CDD programs may even be harmful if they undermine existing local institutions (King and Samii 2014).

We are aware of two studies that are related to the project we evaluate—both in terms of intervention and geographically. Casey et al. (2012) study the impact of a CDR program in Sierra Leone using a randomized experimental design. The program aims at stimulating local democracy and institutional quality through financial assistance for different types of community projects, combined with the organisation of structures that facilitate collective action (such as village development committees). Common village projects included the construction of local public goods (education, water and sanitation, etcetera), communal farming, livestock and fishing, and small business development. After completion of the project activities, the authors implemented 'structured community activities' to experimentally measure collective action and elite capture. The authors find no evidence for elite capture, perhaps because the gifts that were distributed in this context had a highly 'public' nature, and could therefore not easily be diverted. Although the authors find some positive (short term) effects on local public goods and economic outcomes, they find no evidence for (longer term) impact on collective action. A second study that is related to ours is the evaluation of the Kokoyah Millennium Villages Project in Liberia (King 2013b). This project includes a large number of different interventions, the most notable being agricultural training and inputs provision and the interventions in the realm of health. The author evaluates the impact of the program on social cohesion—other outcome variables are not taken into account. She applies a DD research design, matching treatment and control groups on the village level. The results indicate that the projects slightly

improved social cohesion, but that from the onset, social cohesion was not as weak as initially feared.

The project evaluation will be of particular interest to development economists and practitioners. We use a randomized controlled trial (RCT), which generates evidence on causal impacts from the project in a sample of 52 villages. As our sample is small, we complemented our analysis with quasi-experimental evidence including additional control communities to form a larger sample of 72 villages. Our analysis deploys a range of methodologies. We use a public goods game to measure cooperation and we measure leadership quality using a 'natural' field experiment which allows us to directly observe capture of project inputs by the village chief. Finally, our study rests for a large deal on detailed household questionnaires measuring livelihood indicators and time use patterns for individual household members.

Our study hopefully speaks to three literatures. First, our research speaks to the literature on rural development interventions, aiming to improve food security. We analyse whether agricultural community training contributed to rural development, and measure the project impact on livelihood indicators as well as on social cohesion. We then expand our analysis beyond assessing the mere project impact on intended outcome variables, and analyse how the intervention influences intra-household labour allocations. It is a general misconception among development practitioners that African villagers are not time-constrained. Although many villagers may not be formally employed, labour often forms a constraining factor for rural households (e.g., see Ellis 1993 for an analysis of family labour in peasant farms). Most rural households are close to full-time involved in labour-intensive farming activities. We suspect that the introduction of a development intervention that heavily relies on labour input will increase the burden on household labour. This will either happen at the expense of other activities, or lead to shifting activities to the less time-constrained individuals in the household: children. To test this hypothesis, we exploit a detailed time-use survey in order to map labour allocations among individual household members.

Second, our study speaks to the literature on community-based development. We ask to what extent newly introduced institutions on the group level mediates project

performance. To this end, we introduce two institutional sub-treatments—a direct-democracy treatment and a leadership accountability treatment—in a two-by-two design. The direct democracy treatment involves project participants in the choice of training modules in a very early stage of the project. It is expected that participating in direct democracy induces democratic norms within communities (Casey et al. 2012) and as villagers are truly involved in the selection procedure, the final outcome from the election could gain legitimacy (Olken 2010). For example, Beath et al. (2012) find that in villages where projects are selected through consultation meetings, final project selection is more likely to accord to the preferences of elites than projects selected by means of a secret ballot referendum. Olken (2010) finds that under a general voting procedure, selected projects are more in favour of poor women, who might not have been involved as much in a general meeting process. More importantly, though, direct democracy leads to much more overall satisfaction among villagers with the selected project. Yet, some authors contend that involving inexperienced villagers in decisions making processes leads to better development outcomes. The argument is that established leaders can provide more technical expertise, drive, and continuity, which could lead to more productive outcomes (Bernard et al. 2010). Giving villagers means to keep their leaders accountable and the transparency of decision making processes increases. This could, in turn, lead to better collective outcomes. Previous studies have found that a higher level of monitoring indeed increase the leader's effort as well as public goods provision (e.g., Grossman and Hanlon 2014; Olken 2007; Björkman and Svensson 2009).

Finally, our study offers a modest contribution to the literature on elite capture of project benefits and the impact from local governance quality on project outcomes in general. Few studies have empirically investigated the effects from leadership quality on project outcomes. The underlying reason is that leadership quality and project outcomes (or: economic growth) are interrelated. One notable exception on the macro-level is the cross-country analysis by Jones and Olken (2005), who demonstrate

that (powerful) national leaders have a large impact on GDP in their country.[2] On the micro-level, Khwaja (2009) relates the upkeep of community projects to the project leader's quality in 99 rural communities in northern Pakistan. He finds that leadership presence positively affects a group's collective success and that this effect increases with the quality of the leader.[3] We use a direct measure of corruption, by tracking the amount project inputs captured by the village chief, which may signal local governance quality in daily life. Our measure of capture entirely coincides with the project intervention. Hence, we cannot measure the mediating effects from capture on household level project impact. Instead, we relate the incidence of capture to direct project performance indicators, which could be a prediction for the impact of the project.

We find suggestive evidence that the project contributed to higher rice harvests on farmers' private farms. The project did not contribute to social cohesion. Instead, our results suggest that social cohesion slightly decreased. Both results are driven by the subgroup that received the combination of direct-democracy (DEM) and leadership accountability (LA) treatments. The project has a robust, positive effect on time spent on farming activities by children in targeted households. We hypothesise that these children compensate for the time spent on additional farming activities by adults in the household. This effect is smallest for the groups assigned to the DEM/LA treatment combination. Finally, we find that the incidence of capture of project inputs is related with lower harvests of the project groups, controlling for the actual amount of inputs captured. This indicates that capture of public goods may negatively affect project outcomes beyond the direct negative effects from inputs diversion.

[2] They evaluate a sample of countries where a new national leader was installed after the sudden death (due to natural consequences or an accident) of the incumbent leader. The timing of leader replacement is thus unrelated to political factors and economic performance.

[3] Leadership quality is measured as the average of the evaluations of five community individuals (good or bad) of the project leader's quality.

The remainder of this chapter is organised as follows. Section 6.2 presents the intervention, the research design, information about attrition and non-compliance and summary statistics of key variables. Section 6.3 describes the empirical strategy, and Section 6.4 reports results. In Section 6.5 we present the effect from the intervention on the allocation of time within households. Section 6.6 speculates how project impact may be undermined by capture of project inputs. Finally, Section 7 concludes.

6.2 Training farmers' groups in rural Liberia

6.2.1 The project

Our experiment evaluates an agricultural community training program targeting households in rural Liberia. The program has been implemented in two rural provinces not far from the capital city Monrovia. Infrastructure in these areas is in extremely poor condition or entirely absent. None of the rural communities, for example, is connected to the electric grid. Major livelihood activities in this region are subsistence farming and contract labour on rubber plantations, which are ubiquitous in this region. The implementing organisation is one of the many international development organisations aiming at reconstruction of Liberian society after the 14-year civil war. Most of their programs aim at stimulating food production and improving education, health care and water and sanitation. The main objective of the training project currently evaluated is to improve rural livelihoods and to stimulate food self-sufficiency through a combination of communal training and farming activities.[4] In addition, through the communal set-up the program is expected to strengthen social cohesion within communities.

These elements are deemed important in the Liberian post-war context. After the civil war that lasted for fourteen years, all major infrastructures—roads and bridges, telecommunications, power, transportation, water and sanitation systems, schools, and

[4] In a next stage of the project, beyond the scope of this study, the most successful training groups will be selected to continue as actual FFS.

health facilities—had been destroyed or were neglected for years (IMF 2008). In combination with massive displacement of the Liberian population, this negatively affected income activities and undermined food security. The war is believed to have ruptured social cohesion and undermined trust (Richards et al. 2005; Ellis 2006). In this context, corruption thrives, which provides additional challenges for interventions to succeed (e.g., see Reno 1995).

The community training project is based on a participatory approach. Groups of about twenty farmers select their own leader from their community, who is then trained by a local development organisation. This group leader is chairing all activities that are part of the program (see the Appendix for an overview). The project contains both theoretical and experiential learning elements. In the course of four months, the group gathers in weekly meetings to discuss training modules. In addition, the group brings the newly acquired knowledge into practise on a plot of communal land, which is designated to the group by the village chief for the duration of the project. Each group receives a selection of seeds and tools for the experiential part of the training.[5] A team of six local project facilitators keeps track of the activities and each of the facilitators is expected to weekly visit eight to nine training groups, which is an intensive task. They are, however, not actively involved in the training process. The training modules and field activities last for four months, but the group is supposed to continue to tend their communal farm until crops can be harvested. After the harvest, participants can decide to continue the farming group by themselves – as long as the land is available for the group.

To test whether direct involvement of project participants matters for the project outcomes, we add two institutional sub-treatments to the existing project design, implemented in a two-by-two design (see Table 6.1). In the first sub-treatment, we vary the selection procedure of an additional training module, following Olken (2010). Groups were either assigned to a secret-ballot referendum procedure, wherein

[5] Inputs include: seeds – 25 kg rice, 5 kg corn, 3 kg beans and peanuts, 20 g pepper seed, 5 g bitter ball seed; tools – 4 cutlasses, 2 files, 4 hoes, 2 shovels, 2 watering cans.

participants select their preferred training module through an anonymous majority vote system—referred to as 'direct democracy' (DEM), or to a traditional consensus meeting (CON). Under the consensus treatment the training module is chosen in a group meeting. Even though in the latter treatment each group member could potentially speak out, it is possible that the final outcome of the referendum is captured by a few powerful group members. Other benefits of direct democracy could be that participants can override decisions that would be in the best interest of the village elite, and the final choice outcome will generally be closer to the preference of the median voter (Matsusaka 2005).

The second sub-treatment varies whether the elected group leader can be held accountable for his performance. Half of the groups (including the group leaders) are informed that the group can replace their group leader in case they are unhappy about the leader's performance, whereas the other half of the groups does not receive this possibility. We refer to this treatment as 'leadership accountability' (LA). We expect that, whether or not groups actually use this opportunity, the possibility alone may increase the leaders' effort. Also, as groups know they have the possibility of holding their group leader accountable, they may better monitor his performance throughout the duration of the project.

Table 6.1: Two-by-two design of sub-treatments

	Leader Accountability (LA)		*Total*
	Yes	No	
Direct democracy (DEM)	11	11	*22*
Consensus (CON)	11	11	*22*
Total	*22*	*22*	*44*

6.2.2 Research set-up

Our field experiment evaluates the impact of the training program on agricultural production, expenditures, and food. To establish a proper counterfactual, we applied a two-stage randomization design. In the first stage, sixty communities in Montserrado and Margibi—the counties were the development organisation is active—were selected. In the second stage, sixteen households in each community

were randomly selected by means of a public lottery (see section 2.4 in Chapter 2 for more details).[6]

Data were collected in multiple stages. The first round of baseline data collection was conducted in April and May 2010 in fifty-two communities among 832 individuals.[7] In November and December 2010 behavioural experiments were conducted. Hereafter, the intervention was randomly allocated to forty-four communities.[8] We randomized treatment and control with two blocks (road and no road).[9] The project was rolled out in February 2011. In each village, twenty project participants were randomly selected from our baseline household sample. Endline surveys and experiments were conducted between January and April 2012.

The implementation of the experiment was subject to some challenges. First, we dropped eight villages from our sample as they had received the treatment in earlier stages of the project. This left us with a very small control group of only eight

[6] Letting farmers self-select into the project would probably have led to lower non-compliance rates and a more 'efficient' selection procedure. This is how the implementing development organisation normally works. However, this would not allow us to get unbiased estimates in an RCT framework. For this reason, and because the majority of the households are involved in agriculture, we deemed this choice defendable. In addition, selected farmers were free to decide whether they participated or not. In our analysis, the random treatment assignment is used as instrument for actual participation in the project.

[7] After the first round of baseline data collection it turned out that despite our careful selection procedure eight communities had already been targeted for a program before. These communities were dropped from the sample. So instead of 60 communities and 960 individuals we remained with 52 communities and 832 individuals.

[8] The remaining eight communities serve as control communities. The research team was careful not to raise expectations about the intervention that would be rolled out in a selection of the villages at any time. The link between the research team and the implementing organization was never mentioned during the baseline research activities.

[9] Road quality is an indicator for many other village-level characteristics, such as transportation costs and food prices (Casaburi et al. 2013) and rural service delivery (Porter 2002).

communities, which increases the risk of type II errors.[10] In order to increase the sample size, we randomly selected sixteen household representatives in twenty additional control villages in April 2011, following the same selection procedure used for the other communities in the sample. These additional twenty villages were not part of the random assignment procedure, and differ significantly from the control villages in the random sample with respect to a number of key variables (see Table 6.3). Hence, we present RCT results based on the randomized sample of fifty-two communities and we use PSM to reduce selection bias for results from the full sample (see Rosenbaum and Rubin 1983). Second, partly due to the many different moments of data collection, non-compliance and drop-out are high. We argue that drop-out is partly random (due to technical problems in the process of recoding identity codes between the first and second rounds of data collection), but non-compliance is not. This implies that despite of random assignment of the treatment, the sample might be subject to selection effects. We control for this using random treatment assignment as instrument for actual treatment take-up. Another implication is that the drop of observations leads to reduction of statistical power. These attrition and non-compliance are further specified in the next section.

6.2.3 Attrition and non-compliance

Table 6.2 shows the sampling frame. Panel 1 lists the initial target sample, the actual sample after baseline data collection and the difference between the two. Differences between the targeted and actual samples for treatment and control groups are small (two percent on average). As planned, exactly twenty respondents were selected for treatment in each village and attrition in the control villages is low (five percent).

[10] In an early phase of the research design we opted for a treatment group of 44 villages and a much smaller control group of 16 villages in order to allow for sub-treatments. In each treatment bin there are 11 villages, and after dropping 8 control villages, there are 8 villages in the control group.

Table 6.2: Sampling frame

		Treated	Control	Additional control	Total
	# Villages	44	8	20	72
	# HH per village	20	26	16	
1)	Target sample	880	208	320	1,408
	Actual sample	880	201	303	1,384
	Attrition (count)	*0*	*7*	*17*	*80*
	Attrition (percent)	*0*	*3*	*5*	*5*
2)	Participated	387*	0	0	387
	Did not participate	246**	201*	303*	750
3)	Total sample at endline	633	201	303	1,137
	Attrition (count)	*247*	*0*	*0*	*334*
	Attrition (percent)	*28*	*0*	*0*	*21*

Notes: * Compliers: treated according to treatment assignment (60%). ** Non-compliers: not treated according to treatment assignment (40%).

Panel 2 shows participation information. This information is based on self-reported data collected among respondents in treated communities, in a survey evaluating the training group. This survey was conducted directly after the endline household survey. The data show that in treated communities both drop-out and non-compliance rates are high. First, treatment information on 28 percent of the treated sample is missing (247 observations in total). In one village, treatment information has not been collected, which explains nine percent of the attrition rate. The remaining attrition is caused by missing data from individual households in treated villages. Non-compliance is high also. Non-compliance is defined as not complying with initial allocation to treatment or control groups. From the households allocated to the treatment, 387 indeed participated in the training group (compliers), and 246 did not (non-compliers), corresponding with a high non-compliance rate of 39 percent. High attrition and non-compliance rates have several implications.

6.2.4 Balance and data

We test whether random treatment assignment was successful using a vector of baseline-level community and household variables. Table 6.3 reports averages for

treatment and control groups, both for the restricted RCT sample as well as for the full sample. We report averages for the treatment group, as well as eventual differences between treatment and restricted and full control groups. The last two columns report test statistics for a t-test, testing for differences between treatment and control groups.

Panel A presents our community level variables. The average community is very small, and consists of 43 households. The majority of households are involved in agriculture (68 percent). There are 22 mobile phones present in the village; hence on average, every other household owns a mobile phone (note however, that none of the villages is connected to the grid). Presence of development organizations is high: 70 percent of the villages have been targeted for development projects in the past. The size of plantations in the villages varies widely across communities. The average plantation is 154 acres whereas the largest one is 1500 acres. Finally, by design, half of the communities are located along the main road.

Panel B reports our set of household controls. Households count 4.7 members, and 13 percent of the households are female headed. Household heads are 43 years of age; compared to 39 years in the control group (the difference is statistically significant at 1 percent). 6 percent of the household heads are single and they had 2.4 years of education. Nearly all respondents are protestant (90 percent). 74 percent belong to the Kpelle tribe—the most prevalent tribe in our study region. Respondents indicate that their household owns six different assets and experienced one shock in the previous year. Finally, the war clearly had a large impact on many of our respondents. A large majority of 73 percent of the households in our sample have been displaced during the war and 30 of the households have experienced an attack.

We conclude that the randomization has been successful with respect of the restricted RCT sample, with only one out of seventeen control variables being statistically different between treatment and control groups. We must note, however, that the lack of significant differences between treatment and control groups may be caused by low power, due to the small number of observations. The full sample, including twenty additional control villages, is not balanced with respect to two community level variables, and a large number of household level variables. Villages selected into

treatment have a larger share of households involved in agriculture and more NGO activity. In addition, treated households are larger, household heads are less often single, had fewer years of education and more often belong to the Kpelle tribe. Furthermore, treated households own fewer assets and experienced fewer shocks. In order to minimize the selection bias in the full sample, we turn to a PSM framework (Rosenbaum and Rubin 1983). We estimate the propensity score based on this set of unbalanced control variables, using nearest neighbour matching with replacement. Next, we estimate our regression models including frequency weights based on the weights assigned to the control variables in the PSM procedure, as well as a set of household and community level covariates.

We measure the impact of the project on harvest of the two major staple crops: rice and cassava. These crops are grown by the majority of farmers. In order to measure the effect on daily farm practise, we only measure the harvest on farmers' private farms (thus not taking into account the harvest from the communal project farm). Rice seed was the most important input provided by the project. It is expected that rice harvest increases after learning more about better rice farming techniques, as farmers can directly apply the newly acquired knowledge on their private farms. We include effects on cassava harvest to test for potential indirect effects from the intervention. Next, we measure the impact from the project on various income indicators: (*i*) the household's two-weekly expenditures on food and non-food items and (*ii*) a household dietary diversity score (HDDS).[11] Panel A in Table 6.4 provides descriptive statistics of our set of key outcome variables for the treatment group and for both the restricted and full controls groups separately.

We also measure the effects from the program on cooperation and trust. The project aims to improve cooperation and trust by letting farmers work together. It is hoped and expected that farmers will recognise the benefits of cooperation in the training

[11] The HDDS is defined by the number of items a household consumed from twelve different food categories in a certain reference period (two weeks, in our case). This measure is seen as a good predictor for nutritional status, especially for children (see Swindale and Bilinsky 2006; Arimond and Ruel 2004).

group, and that cooperation and trust among community members is supported, also in daily life. We measure social cooperation using the results from a simple public goods game (PGG) that we conducted in a random subset of the village sample. In the PGG participants were grouped in groups of four players, and asked to allocate five tokens to a public or to their private account. Each token kept in the private account was worth 10 LD to the individual player, and each token shared in the public account was worth 5 LD to each of the players (so 20 LD in total). They played the game for five rounds, in changing group compositions, to allow for learning (see Chapter 4 for a detailed description of the experiment). Households assigned to the treatment group contributed 1.5 tokens in the fifth round, which is slightly more than the contribution in the control group in the RCT sample, but not different from the full control group in the PSM sample. Our trust variable is measured in the household survey on a scale from 1 to 5, where 1 refers to very little, and 5 refers to a lot of trust in fellow community members (see Panel A in Table 6.4).

Table 6.3: Balance test key variables (at baseline)

	TREATMENT GROUP					CONTROL RCT SAMPLE					CONTROL PSM SAMPLE					RCT	PSM
	Obs	Mean	SE	Min	Max	Obs	Mean	SE	Min	Max	Obs	Mean	SE	Min	Max	p-value t-test	p-value t-test
Panel A: Community variables																	
# Households	41	43.34	5.43	3	145	8	41	13.29	8	127	28	31.93	5.42	8	127	0.86	0.16
Share of agri hh's	42	0.68	0.03	0.15	1	8	0.71	0.04	0.5	0.85	28	0.56	0.04	0.5	0.85	0.59	0.04
# GSMs	41	21.78	5.47	2	200	7	8.57	1.81	3	18	20	17.5	5.19	2	95	0.33	0.62
NGO activity (b)	44	0.70	0.07	0	1	8	0.63	0.18	0	1	27	0.44	0.10	0	1	0.66	0.03
Plantation farmed (acre)	44	154.14	42.41	0	1500	8	32.75	16.92	0	110	27	54.15	36.94	0	1000	0.23	0.11
Main road (b)	44	0.5	0.08	0	1	8	0.5	0.19	0	1	28	0.57	0.10	0	1	1.00	0.56
Panel B: HH variables																	
Hh size	856	4.74	0.07	1	15	172	4.44	0.17	1	13	475	3.67	0.09	1	13	0.10	0.00
Female head (b)	739	0.13	0.01	0	1	154	0.08	0.02	0	1	452	0.12	0.02	0	1	0.11	0.71
Age	850	42.50	0.51	16	90	171	39.48	1.11	17	86	471	41.13	0.66	17	86	0.01	0.10
Single (b)	880	0.06	0.01	0	1	173	0.08	0.02	0	1	476	0.11	0.02	0	1	0.62	0.00
Years of education	877	2.38	0.14	0	16	173	2.53	0.30	0	12	475	3.80	0.22	0	19	0.65	0.00
Protestant (b)	845	0.90	0.01	0	1	170	0.93	0.02	0	1	470	0.90	0.02	0	1	0.28	0.68
Kpelle (b)	851	0.74	0.01	0	1	170	0.75	0.03	0	1	470	0.69	0.03	0	1	0.78	0.03
Assets	871	6.00	0.09	0	13	175	6.02	0.20	0	12	469	6.55	0.13	0	15	0.93	0.00
Displaced (b)	839	0.73	0.02	0	1	168	0.73	0.03	0	1	469	0.74	0.02	0	1	0.96	0.89
Shocks	880	1.03	0.04	0	4	168	1.08	0.08	0	4	476	1.24	0.08	0	4	0.57	0.00
War attack	822	0.30	0.02	0	1	169	0.26	0.03	0	1	447	0.33	0.03	0	1	0.31	0.31

Notes: The bold variables are included as matching variables in the PSM procedure. All variables that significantly differ between treatment and control groups are included as control variables in the PSM regression models.

Table 6.4: Descriptive statistics outcome variables (at endline)

	TREATMENT GROUP					CONTROL RCT SAMPLE					CONTROL PSM SAMPLE					RCT p-value t-test	PSM p-value t-test
	Obs	Mean	SE	Min	Max	Obs	Mean	SE	Min	Max	Obs	Mean	SE	Min	Max		
Panel A: Key outcome variables																	
Rice harvest	181	874.54	178.51	0	3750	43	242.47	113.23	0	20000	119	658.34	162.17	0	12500	0.09	0.40
Cassava harvest	93	3.94	1.49	0	100	23	2.26	1.38	0	30	49	1.53	0.73	0	30	0.59	0.26
Food expenditures	662	2563.10	61.01	0	6710	151	2562.76	120.47	0	12550	445	2481.18	65.91	0	6710	1.00	0.37
Non-food expenditures	661	2236.70	130.61	0	15150	151	2008.80	210.46	0	25425	444	1757.35	106.66	0	16350	0.43	0.01
Dietary diversity score	658	4.99	0.07	0	10	151	5.16	0.15	0	9	445	5.45	1.94	0	10	0.30	0.00
PGG contribution	339	2.72	0.09	0	5	119	2.73	0.16	0	5	237	3.36	0.11	0	5	0.95	0.00
Trust comm. members	647	3.64	0.04	1	5	148	3.76	0.07	1	5	440	3.85	0.04	1	5	0.16	0.00
Panel B: Time use variables																	
Farming head	588	3.22	0.19	0	24	133	3.20	0.37	0	11.75	414	3.05	0.22	0	18.5	0.97	0.57
Tapping head	588	1.16	0.13	0	12	133	1.76	0.29	0	10.50	414	1.56	0.16	0	12	0.05	0.05
Housework head	588	1.53	0.11	0	24	133	1.25	0.18	0	9.5	414	0.94	0.09	0	11	0.26	0.00
Recreation head	588	0.38	0.05	0	10.5	133	0.41	0.10	0	9	414	0.27	0.05	0	10	0.78	0.17
Farming spouse	282	2.31	4.18	0	19.75	61	2.40	0.53	0	12.75	211	1.33	0.23	0	12.75	0.87	0.01
Tapping spouse	282	0.50	0.13	0	15	61	0.71	0.29	0	10.75	211	0.20	0.09	0	10.75	0.51	0.09
Housework spouse	282	2.97	0.15	0	16.25	61	3.05	0.35	0	12	211	3.16	0.17	0	13.5	0.83	0.41
Recreation spouse	282	0.50	0.09	0	13	61	0.34	0.15	0	7	211	0.28	0.08	0	8.5	0.45	0.09
Farming child	145	1.36	0.25	0	16	23	0	0	0	0	56	0.58	0.30	0	10	0.03	0.08
Schooling child	145	4.1	0.37	0	24	23	4.25	0.80	0	0	56	4.30	0.45	0	10	0.88	0.76
Recreation child	145	1.89	0.21	0	12.5	23	2.88	0.70	0	0	56	2.50	0.42	0	11.75	0.10	0.16

6.3 Empirical strategy

To probe the impact of training program on food security and social cohesion, we do two things. First, we calculate intent-to-treat effects (ITT) from the overall intervention, as well as from each of our four individual sub-treatments (DEM/LA, DEM, CON/LA and CON). To this end, we measure the effect of the treatment assignment z regardless of actual treatment uptake $d(z)$.[12] ITT is defined as follows:

$$ITT_{i,D} = Y_i\big(d(1)\big) - Y_i\big(d(0)\big), \qquad (1)$$

where the *ITT* for individual i is the difference between outcome Y for individuals assigned to the treatment $(d(1))$ and outcome Y for individuals assigned to the control group $(d(0))$. Because the treatment was randomly assigned to subjects, treatment assignment is exogenous, as confirmed by balance tests in Table 6.3. Any observed effect on Y can hence be attributed to the treatment. To calculate the ITT, we estimate a simple OLS regression model:

$$Y_{ij} = a + \beta_{ij} * z_{ij} + \varepsilon_{ij} \qquad (2)$$

where Y_{ij} is the ITT of the outcome variable, a is the intercept $E[Y_{ij}(d(0)]$, estimating the outcome for untreated individuals i in community j, β is the treatment effect of treatment assignment z, and standard errors ε are clustered on the community level (j=1,...,52).

The sampling frame in Table 6.2 shows that treatment assignment often did not coincide with actual treatment take-up. Non-compliance amounts to about forty percent, indicating that forty percent of the individuals in our sample either did not participate in the training group although they were selected for treatment. Hence, the ITT effects likely underestimate actual treatment effects (assuming that treatment effects are larger for participants than for non-participants). To estimate the effects of

[12] The treatment allocation has three potential outcomes. Households assigned to the treatment are actually treated ($d(1)=1$), and households assigned to the control group are untreated ($d(0)=0$). These are the compliers. Then, households assigned to the treatment may opt-out ($d(1)=0$). These are non-compliers.

the program on those individuals who were actually treated, we also estimate the local average treatment effects (LATE), the effect of the treatment on compliers:

$$LATE_{i,\,D}=Y_i(d(1)=1)-Y_i\big(d(0)\big)\,, \quad (3)$$

where the *LATE* for individual *i* is the difference between outcome *Y* for treated individuals *(d(1)=1)* and outcome *Y* for non-treated individuals *(d(0))*.

As actual participation in the program is endogenously determined (the likelihood that someone will decide to join is affected by certain personal characteristics) we run an instrumental variable (IV) model:

$$Y_{ij}=a+\beta_1 d_{ij}^{*}+\varepsilon_{ij} \quad (4a)$$

$$d_{ij}^{*}=\theta+\gamma_1 z_{ij}+\epsilon_{ij} \quad (4b)$$

where the endogenous treatment status d^* is instrumented by the exogenous treatment assignment status z.

We estimate the ITT and LATE of the treatment on harvest, livelihood status and food security, and cooperation and trust. As the results from the RCT analysis are based on a small sample, which increases the risk of type II errors, we also consider the full sample including the twenty additional control villages that were selected after the implementation of the project. In what follows, we present results for the restricted RCT sample alongside results from the full sample, using a PSM framework.

6.4 Empirical results

Table 6.5 presents the ITT (Panel A) and LATE (Panel B) on harvest of the two major staple crops: rice and cassava. The ITT on rice harvest, presented in column (1) is large and highly significant (an increase of 632 rice bundles harvested per household in the treatment group compared to an average of 242 bundles in the control group). As expected, the LATE are even larger (834 bundles). This implies that the project had a large, positive effect on rice harvest, regardless whether people actually

participated in the project (an increase of 260 percent among villagers assigned to the treatment, and an increase of 320 percent among actual project participants).[13] The results are driven by a small number of relatively large observations. We suspect that some of these observations may be outliers, are these farmers are not characterized by other specific household characteristics, such as higher expenditures or consumption patterns. In columns (2) and (3) we present results from regression models cropping the top 1 and 5 percent of the observations (represented by only 2 and 11 observations). The effect is much smaller in column (2), and disappears in column (3). The results for the full sample in column (5) are also positive but insignificant. Finally, the treatment has no effect on cassava harvest [see columns (4) and (6)].

Zooming in on the effects from our sub-treatment in Table 6.6, it seems that the ITT effect is entirely driven by groups assigned to the direct-democracy and leadership accountability treatment (DEM/LA). We perform a Wald-test to test whether coefficients from sub-treatments are equal; corresponding p-values are reported in the bottom panel. The effect sizes do not significantly differ across the sub-treatments. The DEM/LA treatment combination only has a significantly larger effect on rice harvest than the DEM treatment alone in column (2), excluding the top 1 percent of observations. As before, the result disappears when cropping the top 5 percent of observations.

[13] Note that the size of these figures should be interpreted with caution: crop harvest has been measured with some of measurement error due to use of many different units that were ex-post transposed to a single unit—i.e. bundles in the case of rice, and bags in the case of cassava.

Table 6.5: Harvest

	RCT				PSM	
	Rice harvest *All obs.*	Rice harvest *Top 1% obs. excluded*	Rice harvest *Top 5% obs. excluded*	Cassava harvest	Rice harvest	Cassava harvest
	(1)	(2)	(3)	(4)	(5)	(6)
ITT	632.1***	446.31**	131.31	1.675	79.29	2.128
	(215.2)	(172.61)	103.13	(1.936)	(272.4)	(1.959)
N	224	222	213	116	347	188
R^2	0.013	0.014	0.004	0.003	0.031	0.058
LATE	870.02***	605.09***	163.38	1.304	128.1	2.679
	(288.80)	(226.53)	(140.54)	(2.470)	(360.1)	(2.568)
N	217	215	206	111	341	184
R^2					0.030	0.060

Notes: Standard errors are clustered at the village level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Regression models in columns (5) and (6) include household controls (household size, marital status, years of education, ethnicity, assets, shocks, displacement during war) and community controls (share of households involved in agriculture, NGO activity). Rice harvest is measured in bundles, cassava harvest in bags.

Table 6.6: Sub-treatment effects on harvest (ITT)

	Rice harvest *All obs.*	Rice harvest *Top 1% obs. excluded*	Rice harvest *Top 5% obs. excluded*	Cassava harvest
	(1)	(2)	(3)	(4)
1. DEM/LA	1002.2**	1002.2**	226.4	4.822
	(420.1)	(420.1)	(159.9)	(4.729)
2. DEM	312.4	23.47	23.47	-0.511
	(360.0)	(129.8)	(129.9)	(1.511)
3. CON/LA	322.0	322.0	187.6	1.906
	(200.6)	(200.7)	(172.8)	(3.759)
4. CON	828.5*	442.2	125.6	1.160
	(447.9)	(273.7)	(147.5)	(2.415)
N	224	222	213	116
R^2	0.028	0.062	0.012	0.022
1=2	0.21	0.03	0.24	0.26
1=3	0.14	0.14	0.85	0.62
1=4	0.77	0.26	0.59	0.47
2=3	0.48	0.16	0.37	0.51
2=4	0.36	0.14	0.52	0.45
3=4	0.29	0.71	0.75	0.86

Notes: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The bottom panel reports p-values for tests that test whether parameters are equal.

Our results suggest that participation in the training group may have a positive effect on rice harvest on people's private plots, as people might directly apply newly acquired knowledge about farming strategies on their own land, too. The effect is driven by groups where members were directly involved in the project design and who could keep their group leader accountable for his performance. The size of the effect should be interpreted with caution, however, as it is possibly partly driven by a small number of outliers. The absence of an effect on cassava harvest indicates that this result is not driven indirect mechanisms, other than participation in the training.

In Table 6.7 we estimate the treatment effects (ITT and LATE) of the training program on expenditures and dietary diversity. We find no statistically significant effects from the program on any of our livelihood indicators in the restricted sample in columns (1)-(3). The lack of results is due to the large standard errors combined with a limited number of observations. In the full PSM sample in columns (4)-(6) we only find a marginally significant, negative LATE for the household dietary diversity score (the ITT is also negative, but not statistically significant). Our results provide no more than suggestive evidence that the project might lead to a slightly lower dietary diversity score. Perhaps, by relying on few nutritious food items, households free up resources for expenditures on schooling, clothing and other non-food items.

Table 6.8 reports the effects of the intervention on social cohesion. We find no effects from the intervention on contributions in the public goods game, or for reported trust in the restricted sample [columns (1)-(2)]. However, nearly all coefficients are negative. Considering the full PSM sample, we find a marginally significant negative ITT for contributions in the PGG in column (3): contributions in the public goods game decrease by 0.6 tokens (equivalent to 18 percent relative to the control group). Zooming in on our sub-treatments in Table 6.9, we find that the negative effect is driven by the DEM/LA treatment combinations: assignment to this sub-treatment reduces contributions in the public goods game by 0.8 tokens (equivalent to a reduction of 30 percent relative to the control group). The ITT is significantly different from the other tree sub-treatments. Our results indicate that the project did not manage to stimulate social cohesion in the best case and that in the worst scenario the intervention might have undermined social cohesion, especially in those groups where participants are most intensively involved in decision making and

monitoring. We speculate that members in these groups may have had higher expectations from the project impact than group members in other sub-treatments, and 'learned' that cooperation with the group does not pay-off as much as expected.

Table 6.7: Expenditures and dietary diversity

	RCT			PSM		
	Food expenditures	Non-food expenditures	HDDS	Food expenditures	Non-food expenditures	HDDS
	(1)	(2)	(3)	(4)	(5)	(6)
ITT	0.338	227.902	-0.173	74.826	166.163	-0.401
	(190.744)	(263.878)	(0.242)	(142.825)	(282.995)	(0.253)
N	813	812	809	1276	1274	1272
R^2	0.000	0.001	0.001	0.087	0.071	0.054
LATE	-26.933	417.683	-0.285	71.996	283.421	-0.672*
	(304.959)	(437.798)	(0.391)	(222.493)	(460.923)	(0.402)
N	779	778	775	1247	1245	1243
R^2				0.095	0.071	0.046

Notes: Standard errors are clustered at the village level and reported in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Regression models in columns (4)-(6) include household controls (household size, marital status, years of education, ethnicity, assets, shocks, displacement during war) and community controls (share of households involved in agriculture, NGO activity).

Table 6.8: Social cohesion

	RCT		PSM	
	PGG contribution (Round 5)	Trust in community members	PGG contribution (Round 5)	Trust in community members
	(1)	(2)	(3)	(4)
ITT	-0.011	-0.075	-0.613*	-0.145
	(0.439)	(0.127)	(0.357)	(0.121)
N	458	790	789	1250
R^2	0.000	0.001	0.090	0.052
LATE	0.049	-0.062	-1.050	-0.184
	(0.812)	(0.202)	(0.666)	(0.189)
N	424	759	761	1223
R^2			0.070	0.046

Notes: Standard errors are clustered at the village level and reported in parentheses. * $p < 0.10$. Regression models in columns (4)-(6) include household controls (household size, marital status, years of education, ethnicity, assets, shocks, displacement during war) and community controls (share of households involved in agriculture, NGO activity).

Table 6.9: Sub-treatment effects on social cohesion (ITT)

ITT	(1) PGG contribution (Round 5)	(2) Trust in community members
1. DEM/LA	-0.836*	-0.099
	(0.470)	(0.196)
2. DEM	0.232	-0.011
	(0.504)	(0.187)
3. CON/LA	-0.068	-0.229
	(0.497)	(0.168)
4. CON	0.694	0.016
	(0.422)	(0.145)
N	458	790
R^2	0.082	0.011
1=2	0.01	
1=3	0.05	
1=4	0.00	

Notes: Standard errors in parentheses. * $p < 0.10$. The bottom panel reports p-values for Wald-tests (H_0: coefficients are equal).

6.5 Allocation of time

Farming in Liberia is, like in much of sub-Saharan Africa, a particularly physically demanding activity. It involves clearing land of shrubs and bushes and 'digging up the soil' with a hand hoe to make the land suitable for cultivation (Wairimu et al. 2014). Land is usually used for one or more farming seasons, after which it is left fallow for the next farming season. As farming activities are mostly performed using not more than small hand tools, farming—the major livelihood activity of most rural households—is extremely labour intensive. This is at odds with the increasing focus of community based programmes, which heavily rely on precisely the scarcest resources in communities: labour (*ibid*). Activities on the communal project farm are additional to the private farming activities that most households undertake. Households that are not involved in farming are either not able to perform physically demanding labour, or they are involved in other, non-farm income activities. This means that involving households in labour intensive community-projects will increase the strain on available labour in the household. Consequently, project participants may shift labour input from their private farm to the communal project farm, reduce time spent on other activities, or involve other household members in farming activities. We asked project participants how they compensated for the time they spent on the training modules and field work. Nearly half of the project participants respond that they abandoned their regular activities. Only 14 percent of the respondents indicate that someone else took over their activities, and 35 percent respond that they worked more or had less time for leisure (see Table 6.10).

Table 6.10: Compensation for time spent on SGP activities

	Obs	Mean	S.D.	Min	Max
Abandoned regular activities	424	0.47	0.50	0	1
Someone took over my work	424	0.13	0.35	0	1
Worked more	424	0.22	0.42	0	1
Hired labour	424	0.02	0.18	0	1
Less leisure time	424	0.13	0.38	0	1

To test whether the intervention caused shifts in time use between activities or between household members, we collected detailed information on the amount of time spent on various activities by all household members using a time use survey measuring all activities performed on a single day.[14] We assess the treatment effects of the intervention on time allocation to major daily activities for the household head, spouse, and children: farming, rubber tapping, house work and recreation for household head and spouse, and farming, schooling, and recreation for children. Panel A in Table 6.11 reports the ITT and LATE for the restricted RCT sample. We find no effect from the intervention on the amount of time spent on any of these daily activities by the household head or spouse. However, the treatment leads to children spending significantly more time on the farm than their peers in the control group. The ITT is 1.4 and the LATE 2.1, compared to an average of zero in the control group.[15] This implies that the treatment leads to an increase of 1.4 hours spent on the farm by children in households assigned to the treatment, and by 2.1 hours by children in households that were actually treated. This result is replicated in the full PSM sample in Panel B: the ITT is 0.7 and the LATE 1.1, which corresponds to a huge increase of 120 and 190 percent, respectively. Note that the effect sizes should be interpreted with caution given the small number of observations both in the restricted sample, as well as in the full sample. We suspect that increased time spent on the farm by children is related to less time spent on other major activities. Coefficients for time spent on schooling and recreation are indeed negative, but not significant. Perhaps the effects are not picked up as time spent on the farm is compensated by less time spent on a range of many different activities.

[14] In the time use survey respondents were asked to meticulously report each activity performed by each household member in the course of the last regular working day (mostly 'yesterday'), including starting and ending time, beginning from 4 a.m. If present, both head and spouse were interviewed separately. If not, the interviewee would estimate time use activities for the other household members.

[15] The number of observations in the control group is small. None of the 23 households in the control group indicated that their children worked in the farm on the last working day.

Zooming in on the mediating effects from our sub-treatments, we find that the increase in time spent on the farm is significantly smaller in training groups assigned to the DEM/LA sub-treatment (see Table 6.12). Children from households in this sub-treatment spend only 0.4 hours more on the farm, compared to an increase of 1.5 and 2 hours in groups assigned to the DEM or COM treatments. The ITT in the other three sub-treatments do not statistically differ from each other. We speculate that a larger involvement of group participants in the DEM/LA treatment may raise their awareness of the importance of children's education, which might weaken the effect of reallocation of labour to children. The coefficient for time spent on schooling is positive (but insignificant) for the DEM/LA sub-treatment, while under the CON sub-treatment, the increase of time spent on farming by children seems to be compensated by a similar reduction of time spent on schooling. Our results indicate that net time spent on farming by adults remains unchanged. Most people probably shift some of their farming activities from their private farm to the project farm. If people spend more time on the communal plot, communal production might increase, whereas production on the private plot would probably decrease. If this assumption holds, then we can only expect that the project is successful in terms of increasing household level productivity if per-household productivity on the communal farm is greater than productivity on the private farm. Unfortunately, we lack the data to assess land productivity of private versus project plots, so we cannot test this hypothesis. One caveat should be taken in mind. The time use data were collected a couple of months after the end of the main farming season, meaning that we measure the shifts in time use that *persist* after a couple of months. By the time the endline survey data were collected many farmers had probably reverted to their regular farming activities. Had we performed the time use survey during the main farming season, we might have found larger effects on labour input and labour shifts among household members.

Table 6.11: Time use

	Head				**Spouse**				**Children**		
	Farming	Tapping	Housework	Recreation	Farming	Tapping	Housework	Recreation	Farming	Schooling	Recreation
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Panel A: RCT Sample											
ITT	0.014	-0.593	0.275	-0.034	-0.0949	-0.210	-0.079	0.158	1.357***	-0.150	-0.994
	(0.531)	(0.552)	(0.297)	(0.142)	(0.857)	(0.445)	(0.389)	(0.250)	(0.214)	(1.269)	(0.874)
N	721	721	721	721	343	343	343	343	168	168	168
R^2	0.000	0.005	0.002	0.000	0.000	0.001	0.000	0.002	0.027	0.000	0.016
LATE	0.028	-0.931	0.510	-0.063	-0.007	-0.329	-0.170	0.189	2.116***	-0.349	-1.559
	(0.857)	(0.890)	(0.483)	(0.230)	(1.438)	(0.743)	(0.657)	(0.415)	(0.378)	(1.866)	(1.286)
N	691	691	691	691	325	325	325	325	161	161	161
Panel B: PSM Sample											
ITT	-0.097	-0.483	0.358	-0.017	0.366	-0.018	0.273	0.222	0.734*	0.257	-0.503
	(0.574)	(0.513)	(0.238)	(0.195)	(0.590)	(0.306)	(0.265)	(0.227)	(0.414)	(0.922)	(0.645)
N	1116	1116	1116	1116	589	589	589	589	218	218	218
R^2	0.019	0.031	0.044	0.021	0.086	0.080	0.049	0.024	0.085	0.083	0.076
LATE	-0.188	-0.762	0.633*	-0.039	0.838	-0.001	0.434	0.311	1.132**	0.319	-0.881
	(0.915)	(0.819)	(0.380)	(0.308)	(1.006)	(0.540)	(0.464)	(0.379)	(0.565)	(1.249)	(0.864)
N	1091	1091	1091	1091	572	572	572	572	212	212	212
R^2	0.018	0.025	0.033	0.021	0.088	0.080	0.042	0.026	0.066	0.080	0.059

Notes: Clustered standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regression models include household controls (household size, marital status, years of education, ethnicity, assets, shocks, displacement during war) and community controls (share of households involved in agriculture, NGO activity).

Table 6.12: Sub-treatment effects on children's time-use (ITT)

	(1) Farming	(2) Schooling	(3) Recreation
1. DEM/LA	0.398**	1.234	-0.763
	(0.183)	(1.417)	(1.000)
2. DEM	1.494***	-0.415	-0.399
	(0.465)	(1.276)	(0.961)
3. CONS/LA	1.278**	2.241	-1.667*
	(0.497)	(1.369)	(0.908)
4. CONS	1.961***	-2.328*	-1.297
	(0.278)	(1.294)	(0.917)
N	168	168	168
R^2	0.062	0.138	0.043
1=2	0.03	0.07	0.60
1=3	0.10	0.32	0.14
1=4	0.00	0.00	0.39
2=3	0.75	0.00	0.02
2=4	0.39	0.01	0.11
3=4	0.24	0.00	0.42

Notes: Standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The bottom panel reports p-values for Wald-tests (H_0: coefficients are equal).

6.6 Elite capture

Community leaders can play a mediating role in the failure or success of development interventions via two main channels. First, community leaders may play a direct mediating effect by channelling away project inputs for their own benefit (e.g. Platteau and Gaspart, 2003; Platteau, 2004). Second, good leadership may (indirectly) create an enabling environment for new initiatives to succeed. As community leaders fulfil a key position in many African societies, often based on traditional authority, their support is crucial for the potential success of development interventions (e.g., see Kyamusugulwa & Hilhorst 2015).

We measured the quality of leadership by the amount of 'capture' of program inputs by local leaders. Just before the start of the training program, project inputs (seeds and tools) were delivered to the 44 treated communities, and community leaders were asked to store them in their private hut for three days. After three days, a field worker

would publicly weigh the seeds and officially hand over the inputs to the project beneficiaries.[16] If any of the project inputs was missing after these three days, we define the leader as 'corrupt'. According to this (rather narrow) definition, 22 of the community leaders are corrupt, and 22 are not. As our corruption measure coincides with the treatment (we have no measure for corruption in control communities), we cannot analyse the interaction between the project implementation and the incidence of capture. Instead, we relate capture by the village chief to the performance of the training groups. We define 'group performance' in terms of two different variables: the number of active members and whether the group harvested rice at the end of the growing season.[17] We argue that both participation in the group, as well as whether the group actually harvested, could function as predictors for eventual project impact. Questions about group performance were asked to all project participants, and then averaged at the community level. Groups were reported to have sixteen active members on average (ranging between four and twenty-five members). Also rice harvest rates vary greatly: only 50 percent of the group members indicate they harvested rice (see Table 6.13).[18]

Table 6.13: Elite capture and SGP group performance

	Obs	Mean	S.D.	Min	Max
Capture (b)	44	0.48	0.51	0	1
Active members	43	15.85	4.44	4.33	25.19
Rice harvested (b)	43	0.50	0.37	0	1

Notes: (b)=binary variable

[16] See Chapter 4 for a detailed description of the corruption experiment.

[17] Rice is the major staple crop in Liberia, and the most important provided to all groups. Apart from rice, groups also received a variety of vegetable seeds, but these seeds were provided in much smaller quantities and were often not harvested as seeds did not always germinate.

[18] We also have data for the actual quantity of the harvest. However, these variables are measured with a lot of measurement error due to conversion from a multitude of local units to one standard unit.

We run the following regression model:

$P_j=\beta_1 Q_j+\gamma X_j+\varepsilon_j$,

where project performance P in community j is explained by the quality of local leadership Q, a binary variable taking the value 1 if the village leader diverted some of the project inputs (j=1,...,44). X is a vector of community level controls and ε is the error term.

In Table 6.14 we report results of regressions of group performance on capture of project inputs. We find no evidence that leadership quality matters for the number of active members in column (1). However, our result in column (2) indicates that input diversion is negatively correlated with rice harvest. We control for the direct effects of input diversion (if project inputs are stolen there is obviously less seed to plant and the change of harvesting the crop is smaller) by including the amount of rice seed diverted. The coefficient indicates a decrease of 28 percentage points. Our results show that the decreased likelihood of rice harvesting vis-a-vis a corrupt chief is not a direct consequence of input diversion (it does not matter how much rice seed was diverted; in fact, rice seeds were not stolen much at all). Instead, we hypothesize that input diversion by the chief signals his general support for 'community-goods': in communities with a supportive chief, projects may stand a better chance to succeed. This relation may be influenced by unobserved effects, such as shared village norms, that both affect group performance and leadership quality. Nevertheless, our result suggests that 'good' leadership may indeed provide an enabling environment for projects to succeed—beyond the direct effect of input provision. Our result is in line with the results in Beekman et al. (2013), who find that 'corrupt' leadership also leads to lower rice harvests on people's private farms.

Table 6.14: Elite capture and group performance

	Active members	Rice harvest
	(1)	(2)
Panel A: Corruption		
Inputs missing (b)	-1.598	-0.264**
	(1.473)	(0.127)
Rice missing (grams)		0.00004
		(0.00004)
Constant	11.153	0.382
	(14.212)	(1.100)
Controls	Yes	Yes
N	41	41
R^2	0.309	0.424

6.7 Conclusions and discussion

Community participation, especially in the setting of rural reconstruction after a period of civil war, is seen as a viable way to escape poverty by many development agencies (e.g., Burde 2004). In this chapter, we evaluated a rural community-based training program in post-war Liberia that combines elements from farmer field schools and community-driven reconstruction. We apply a randomized controlled trial, complemented with results from a larger, non-random sample, based on a matching procedure.

Our results indicate that the project has contributed to higher rice harvest—the main staple crop in Liberia. However, the effect size needs to be interpreted with caution. The treatment effect estimator is imprecise as it is measured with relatively much noise. Furthermore, our study is based on a relatively small number of observations: the project was carried out in forty-four communities, and the number of control communities is much smaller. This increases the risk of type II errors due to low power. We re-estimated our models using a larger, non-random sample of villages. Although we reduced the estimation bias using a propensity score matching procedure, this may not rule out selection bias based on unobservable effects (see Angrist & Pischke 2008). The effects we do find, seem to be driven by training groups allocated to a direct-democracy and leadership-accountability treatment. We

also find that this sub-treatment leads to lower willingness to cooperate in the aftermath of the project. Perhaps, members in these groups had high expectations from the project impact, and experienced that cooperation with the group does not pay-off as expected. Finally, results suggest that an 'enabling institutional environment' may matter for direct project outcomes: group in villages with a stealing chief are less likely to harvest their crops.

One mechanism that may explain the limited impact is the possible reallocation of labour within the household. Like all labour-intensive project interventions for the rural poor—'cash-for-work' is another example—this project is based on the assumption that labour is an abundant production factor. This assumption is probably wrong. We observed that farming is highly labour intensive, and people are spending most of their time on the farm or on other income activities. Those people who are not working are not fit for physical labour. As the community project targets exactly the scarcest production factor—labour—it is likely that households shifted labour from their private to the public farm, while total farming time remained unchanged. This is exactly what we find, based on results from a time-use survey. In addition, we find evidence that the intervention leads to higher involvement of children in farming activities, probably partly at expense of their time spent on schooling. This does, however, not lead to higher harvests or improved livelihood status. A relevant question to further explore in the context of this project is whether communal farming is more (or less) efficient than farming on a private plot. Very few studies look at yields based on a distinction between collective and individual plots. Guirkinger et al. (2015) show that for care-intensive crops and cash crops, male private plots are much more productive than common family plots. Whereas there may be scale-advantages—sharing inputs and knowledge, these are probably overruled allocative inefficiencies. Even if each farmer would contribute maximally, we may ask ourselves whether the resulting communal output would be higher than the sum of individuals' output from each of their private farms. Unfortunately, we lack detailed data on productivity of the communal farms vis-a-vis the private plots, so this assumption remains untested. The key lesson, however, is that if households are labour constrained, then introducing new activities will inevitably lead to shifts in

labour allocation within the household. Hence, a newly introduced intervention will only pay off when these shifts lead to more productive allocation of labour.

Our dataset is subject to a high attrition rate. A significant share of control villages was dropped as they had received the treatment before. We selected additional control villages, but only after the random treatment assignment. They are thus not part of the experimental framework. The sample is also subject to high attrition on the household level. The implication is that the sample size in the experimental framework is small, which may lead to increased chance of Type-II errors, or 'false negatives'. In other words, we might fail to reject the null hypothesis when it is actually false. Our results thus probably underestimate the actual effects from the program. We expanded our control group with the additionally collected control villages, using a matching strategy. Yet, propensity score matching is no panacea either, as it remains a control strategy (Angrist & Pischke 2009). A larger sample from the onset as well as a more careful assignment of identification codes to our subjects would have prevented these flaws, which is an important lesson for future impact evaluation work. Yet, regardless of these limitations, we expect that a larger sample size would not lead to changing signs of the relationships we found.

Two final remarks should be made. First, the 'community-driven' elements in the project under evaluation are limited, and so are our results. It would be worthwhile to study whether more intensive involvement of project participants and more salient monitoring mechanisms would benefit the project impact, using a larger sample of communities. Second, our data do not allow measuring project impact on allocative efficiencies of production factors on private and communal farms. This remains a key avenue for future research.

7

General Discussion

7.1 Introduction

In rural village economies, local institutions such as kinship networks and village rules function as efficient coordination instruments. Belonging to an in-group provides trust, enables trade and lowers transaction costs within this group (Sindzingre 2006). Simultaneously, strong shared norms that are reproduced by local institutions may sometimes form an obstacle to change. This thesis has contributed to the understanding of the channels through which a variety of local institutions may hamper development in rural village economies.

This thesis is in line with a widely embraced reappraisal of the role of institutions in development. Since the past decade or so, institutions—defined by 'the rules of the game'—have been taking a prominent role in economic thinking about global development. Initially, development economists and policy makers were mainly focussing on the role of formal and informal market institutions (World Bank 2001). More recently, this focus has shifted also to the role of human behaviour and related local coordinating institutions (World Bank 2014). The broadly shared interest among economists for the role of institutions in economic development is also signalled by a continuous flow of publications in the field of 'institutional development economics'. Recent contributions include cross-country analyses (Francois, Rainer, et al. 2015) as well as studies of institutions on the micro-level (Kosfeld & Rustagi 2015; Burgess et al. 2015; Francois, Anderson, et al. 2015), and a review article on the two-way causal effects between institutions and culture (Alesina & Giuliano 2015).

One of the advantages of analysing institutions on the micro-level is that it allows for analysing *de jure* (formal) institutions as well as *de facto* (informal) institutions on a disaggregated level. Especially the latter may be relevant in developing economies. In contexts where the state is largely absent and where formal institutions fail—such as in the rural villages that were mentioned above—informal institutions as well as the shared norms they may reproduce, can be of key importance for the functioning of society.

This thesis aimed to capture the implications for development from an array of local institutions. To this end, I exploited a rich dataset from rural Liberia. These data were collected in the context of a randomized evaluation of a community

development project. I shed light on how every-day decisions made by rural villagers are affected by the institutional environment, and how informal institutions may form obstacles to rural development. In the coherent context of a single randomized development intervention, I aimed to analyse the implications of local institutions on development, including family networks (Chapter 3), the quality of local leadership (Chapters 4 and 5), ethnic dimensions of corruption (Chapter 5), and the impact of a agricultural community development program in relationship with local institutions (Chapter 6).

In what follows, I present an overview of the key lessons from the various analyses in this thesis and their limitations. I proceed with a discussion of the policy implications from this study, and in conclusion I provide a number of avenues for future research.

7.2 Key lessons

7.2.1 Kinship networks as egalitarian poverty trap

Kinship forms the most fundamental institution in society. Kinship networks function as important redistributional institutions and are a source of public goods provision (Cox & Fafchamps 2008). Yet, although the sharing obligations that are reproduced by family networks may benefit the poorest members in the network, they could create an 'egalitarian poverty trap' for the more successful or ambitious members (Lewis 1955; Bauer & Yamey 1957; Hoff & Sen 2006). As these members will be forced to share their resources with the poorer members in their networks, they will find themselves unable or may lack the incentives to move on. This might lead to a suboptimal allocation of resources.

In Chapter 3 we asked whether tightly-knit family networks affect economic decision making. We learned that people with denser family networks are more likely to revert to costly strategies to hide their incomes, especially those individuals who were affected by kinship pressure in daily life. Our study fits in a small empirical micro-literature that tests the effects from kinship networks on investment behaviour. Based on this literature, we already knew that dense family networks are associated with lower investments in, for example, human capital and risk management strategies (Di

Falco & Bulte 2013; Di Falco & Bulte 2015). Although we found no evidence that kinship networks directly affect investment behaviour, we did find support for the hypothesis that kinship networks are related with costly strategies to evade sharing obligations, and hence, to inefficient allocations of resources.

Measuring the causal effects from family networks on development is challenging as it is intrinsically difficult to instrument for family ties. Some studies, like ours, try to allow for identification of causation by conducting lab-in-the-field experiments (e.g., Jakiela & Ozier 2012 implemented an information treatment, which we replicated in our study). However, we suspect that in the context of dense family ties in villages, the information treatment may not be salient enough (information might find its way, regardless of the treatment). Despite of possible endogeneity problems caused by reverse causality or selection issues, family networks seem mostly exogenous—especially in African village societies where family ties are difficult to escape.

7.2.2 Local governance and investment incentives

In many African societies, village rules are rooted in complex patron-client relationships, wherein 'Big Men', or local elites, play a key role (Rowley 2000). Village chiefs allocate power positions and rights—referred to as 'elite contol' (Acemoglu et al. 2014). An important function in the context of agricultural economies is the allocation of tenure rights and the organisation of scarce resources, for example by organising communal labour and by deciding about rules of sharing the output through sharecropping and tenancy rules (Sindzingre 2006). Yet, evidence suggests that local 'Big Men' sometimes misuse their power for private rent-seeking (Acemoglu et al. 2014; Rowley 2000; Richards & Bah 2005). Elite control may then turn into elite capture.

In Chapters 4 and 5 we turned to the implications of elite capture (or: 'corruption') for public and private investment behaviour. We learned that corruption discourages public and private investment decisions; both measured experimentally using a lab-in-the-field experiment, as well as using survey data measuring livelihood activities. These findings fit in a small but expanding experimental literature on the costs of corruption (see Serra & Wantchekon 2012 for an overview). Corruption does not only come at direct costs (through the diversion of project benefits, for example), but

also leads to indirect costs by attenuating investment incentives and cooperation. If the negative effects from elite capture surpass the beneficial efficiency effects from elite control, then local patron-client systems may lead to suboptimal allocation of resources—a similar mechanism as for the effects from kinship networks.

Two methodological remarks are in place. First, due to its nature, corruption is notoriously difficult to observe. Various measures for corruption are used in the literature, yet, 'the best way to measure corruption is often to observe it directly' (Olken & Pande 2012, p.483). For reasons of observability, most corruption studies look at the diversion of public goods, or 'petty corruption'. Our study is one of the few in this field that uses an objective measure for corruption. Yet, Lambsdorff and Schulze (2015) write that 'grand corruption', or 'corruption between the political elite and the business community in form of cronyism, preferential access to government contracts or freedom from prosecution may be fundamentally more important and much harder to measure.' Whereas the diversion of public goods is relatively easy to measure, 'grand corruption is extremely hard to quantify in all of its consequences'. In our study, we argue that our measure of petty corruption may signal other forms of (grand) corruption in daily life. Yet, in future work we may enrich our current measure by combining objective corruption measures with perceived corruption.

7.2.3 Opening the black box of corruption

In patron-client relationships, rent-seeking behaviour might be a rational strategy. Especially those rulers who obtained their position through rent-seeking with help of their network tend to use public funds to give material rewards for the loyalty of their network in order to sustain their position and to keep other rent-seekers at bay (Rowley 2000; Bayart 1993). The ruler 'must also shore up support within his preferred ethnic or tribal group by providing such groups with rents, sometimes in the form of direct subsidies, but often through complex and costly networks of economic regulation' (Rowley 2000, p.143). In a recent paper, Francois et al. (2015) convincingly show that African autocratic leaders maintain their position in two ways. In the first place, they seek support from the population by proportionally assigning power positions in the ministry across ethnic groups. Secondly, they buy support from their own in-group by rewarding them through patronage allocations.

Little empirical research has been conducted to test whether these systems also hold on the micro-level, and to what extent local corruption may have heterogeneous impacts on different social subgroups. In Chapter 5 we addressed this question. We learned that corruption is targeted at specific social groups, along ethnic lines. More specifically, we observed that only those people from a different ethnic group than the chief's respond to corruption by investing less (in rice production), whereas the chief's co-ethnics are unaffected. Our study provides a little step towards more understanding of the winners and losers from corruption in the context of local villages. To better understand how these mechanisms work, further micro-economic research is needed to 'penetrate the black box of corruption' (Bertrand et al. 2007, p.1672)

7.2.4 Development interventions, institutions and the allocation of resources

The nature of the relationship between aid, institutions and growth is subject to debate. Institutional change may be driven by economic incentives. For example, aid flows may contribute to democratisation in transition countries (Askarov & Doucouliagos 2015) and decrease corruption (Tavares 2003; Okada & Samreth 2012). Vice versa, windfall gains from development interventions may sustain corrupt behaviour (e.g., Rowley 2000; Djankov et al. 2008; Voors et al. 2011) and aid flows may undermine institutional quality (Djankov et al. 2008), especially when governance quality is low already. A weak institutional environment may thus explain the weak impact from aid on economic development.

On the micro-level, development interventions benefit from an enabling institutional environment: projects are more likely to succeed if the conditions are right (Khwaja 2009). Community-driven development programs have been aiming to contribute to development outcomes by strengthening existing institution, or to building new ones. The programs are mostly implemented in post-conflict countries that are characterised by weak institutional quality. It is questionable, however, whether 'new' institutions can really improve on existing ones within the brief period of time that is typical for project interventions. Recent evidence on the impact of community-driven development programs is rather bleak. None of the recent evaluations of these programs find robust evidence that institution building contributes to social cohesion

or improved development outcomes (Fearon et al. 2009; Humphreys et al. 2015; Casey et al. 2012; King 2013a; Beath et al. 2012; King & Samii 2014).

The analysis in Chapter 6 fits within this context. I asked to what extent an agricultural community training project contributed to improved livelihood outcomes and social cohesion. I also asked whether effects from the intervention are driven by institutional sub-treatments. The impact from the project is limited, and so are the effects from the newly introduced institutions. One notable observation is that project participation may lead to intra-household shifts in labour allocation, and specifically to more involvement of children on the farm—an unintended side-effect from the project. This is related to the view that the allocation of aid projects may have positive or negative externalities via the reallocation of resources (Lee & Izama 2015). On the micro-level, resource allocations may shift within households and within communities, both among treated and untreated individuals. Measuring project impact on the community level would capture this latter type of reallocations (Angelucci & Giorgi 2009). Finally, the results suggest that the existing institutional environment might matter: training groups perform less well in terms of productivity in the presence of elite capture, which is in line with our findings from Chapter 5. In future work, I hope to directly test the effects of local governance on project impact and the interplay with newly introduced democratic and monitoring institutions.

7.3 The persistence of institutions

Institutions persist over time; also dysfunctional ones. The reason for this persistence is that institutions are far from distribution neutral—even if institutional change is widely considered as a change for the better (Besley & Jayaraman 2010; Acemoglu & Robinson 2008). Institutional change will therefore always produce winners and losers. For example, in Chapter 3 we observed that the sharing obligations that are part of kinship networks may lead to suboptimal allocation of resources by some individuals. Yet, poorer individuals in the network may benefit from the status quo. Similarly, corruption as part of patron-client networks may form an obstacle for development, but the system is sustained because powerful elites and their in-groups may benefit from it, as we suggest in Chapters 4 and 5.

The creation of institutions is strongly rooted in history. The description of the historical background in Liberia in Chapter 2 illustrates how historical institutions may still affect development outcomes today. This observation is reflected in other parts of the world, too. The description of historical roots of institutions in South-America by Sokoloff and Engerman (2000), for example, very well reflects the Liberian situation. Like Liberia, colonial South-America was characterized by an extractive economy with large plantations, where the large indigenous population was working under slavery. This led to a situation where a small elite was holding on to power whereas the majority of the population was not given access to property rights or the possibility to political participation. Both in South America and in Liberia, as well as in other extractive economies, this situation lead to extreme inequality. The extreme distributional effects from institutions in combination with their history-dependent nature might lead to an 'institutional poverty trap': an equilibrium wherein resources remain unequally distributed among social groups. 'Although one could imagine that extreme inequality could take generations to dissipate even in a free and even-handed society, such biases in the paths of institutional development likely go far in explaining the persistence of inequality over the long run in Latin America and elsewhere in the New World' (Sokoloff & Engerman 2000, p.230).[1]

Institutions may adapt if changing external conditions provide new incentives leading to altering norms (e.g., Platteau & Seki 2001). If they do not, poverty traps may occur when traditional norms meet the market during a period of economic transition (Sindzingre 2006). What is the effect from increasing market integration or migration on local norms, and what are the implications for development? Institutions matter, but it remains an empirical questions whether they will function as 'vehicle of progress' or 'instrument of stagnation' (Hoff & Sen 2006). (Formal) institutions are strongly interrelated with culture, and develop alongside each other. One implication is that 'even when new institutions are introduced to increase economic growth, their effect depends on whether the appropriate cultural trait develops to support the new

[1] See Acemoglu et al. (2001) and Banerjee and Iyer (2005) for similar arguments.

institution' (Alesina & Giuliano 2015, p.50). In Chapter 6, I hypothesise that the more the current status quo benefits local elites, the less willing they will be to act as 'vehicle of progress' and the less successful initiatives aiming to incur change, will be.

7.4 Local institutions as vehicles of change?

What are the implications from this study from a policy perspective? The discussion about the persistence of institutions illustrates that it is difficult to alter local institutions. Nor does it seem sensible to work around them in an attempt to avoid the negative consequences from some informal institutions. For example, to what extent should policy makers try to combat local corruption? Would it be useful to invest in economic conditions (resource allocation and employment) and governance structures? To the extent that corruption is determined by social norms, ethnic ties, and custom, this might not be an effective thing to do. Instead, a greater awareness of the way how existing governance structures may be rooted in long-term tradition could help policy makers (and development workers) shaping more suitable ambitions. Deeper knowledge of the functioning of local institutions and shared norms on the micro-level may help policy makers to design those institutions that might provide new incentives and contribute to altering norms. For example, knowing that tight family networks might form an obstacle to save could be a motivation for governments or development institutions to invest in alternative saving opportunities for the rural poor.[2] And the finding that corrupt elites may not only directly channel away community resources, but also indirectly influence project outcomes, may motivate development institutions to better target future interventions. Simultaneously, local institutions are often rooted in strong social norms, including moral obligation and interpersonal accountability, organised along specific social networks. Such existing structures may form a foundation for new development strategies. Hence, it may be sensible to investigate ways to using existing local

[2] For example, a recent study by Dupas et al. (2015) shows that access to saving accounts decreases dependence on the family network, while supporting non-family relationships with co-villagers.

institutions as 'vehicles of change'. This will require an adapted approach per locality, depending on the specific institutional context.

Any institutional change needs a sufficiently large critical mass that may induce the adaptation of norms. Policy makers could contribute to institutional change by supporting the growth of a critical mass, and by helping to build specific institutions that could function as complements or alternatives to existing ones. Yet, this is not without risk. Just like increasing market integration may interrupt traditional norms at the expense of some; project interventions sometimes undermine social preferences, and create new or exacerbate existing tensions. For example, the benefits of development can be diverted to local elites (Mansuri & Rao 2004; Platteau 2004), or interventions themselves can foster discord (Labonne & Chase 2011; Anderson 1999). The 'Do not harm' principle is central to development policy, and it is crucial to check whether this principle is met during all stages of an intervention, both during design and implementation phases. To this end, carefully designed impact evaluations of development interventions could directly inform development policy.

This discussion highlights the need for a close cooperation between policy makers and researchers, especially in the design phase of development projects and new policy interventions. Testing underlying project assumptions in a pilot project before implementing it on a larger scale would increase the chance that projects would have a positive impact. In addition, and perhaps more importantly, this cooperation might help to in an early stage address unforeseen side-effects from the intervention, such as allocative inefficiencies and potential exacerbation of social tension.

7.5 Avenues for future research

One key avenue for future research is how policy interventions may contribute to development—despite of local institutions, or through them. Literature on community-driven development has mainly focussed on building institutions around accountability and democracy, and their effects on development. Yet, in the current micro-economic literature systematic research on the effects from newly introduced institutions and infrastructure on social norms, networks, and perceptions towards corruption, is underrepresented. The most promising way of measuring such effect

on a micro-level would be using randomized controlled trials, in order to move from mere correlations to causal inference. In order to measure the effects from institutional change on 'culture' on a higher level of analysis, we could search for suitable field-experimental settings of sudden institutional change that is reasonably unrelated to culture and social norms (Alesina & Giuliano 2015).

The line of research proposed above might provide insight in ways how to combat corruption at the local level, which is particularly relevant from a policy perspective. We already know quite a bit about the consequences of corruption. This thesis has contributed to the understanding of effects of corruption on investment incentives in the context of local villages. Yet, a third question—perhaps the most relevant one, from a research as well as from a policy perspective—has largely remained unaddressed in micro-economic literature: What are drivers of corruption? Most evidence on the 'causes of corruption' is based on cross-country studies (e.g. Treisman 2007), whereas little is known about drivers of corruption on the micro-level. A recent flow of experiments has tried to elicit drivers of corruption in the lab (Abbink & Serra 2012). Although lab-experiments on corruption may have empirical relevance (Armantier & Boly 2013), field experiments may be better suited to answer these questions: To what extent is the incidence and size of corruption predicted by the economic or institutional environment, and to what extent is corruption determined by culture and shared norms? Disentangling the causal effects from environmental factors on corruption remains a challenge, given the endogenous nature of corruption. Exploiting controlled field experiments in combination with detailed survey data might contribute to solving this puzzle.

Finally, this thesis demonstrated how different types of local institutions may simultaneously affect development through various channels. Yet, we do not know much about the interplay between these institutions; nor do we fully understand how local institutions interact with development interventions. Are dense kinship networks invoking the incidence of corruption, or do they provide such clear monitoring mechanisms that shirking becomes merely impossible? Related to this, it would be relevant to closer scrutinize the heterogeneity of local institutions and their heterogeneous impacts on development outcomes. One promising research agenda would be to distinguish between social networks as source of social immobility, versus

the informative and safety networks functions of social networks. In a similar vein, local 'corruption' does not necessarily signal 'bad governance'. Capture of public resources by local elites may be driven by many different motivations, including benevolent ones. Recognizing different types of 'corrupt' behaviour would be a first step towards a deeper understanding of the micro-level consequences from corruption. As institutional change is not 'distribution neutral', it will be of key interest to identify winners and losers, in addition to measuring aggregate development impacts. One way to go about this would be to move beyond the household as prime unit of investigation, and scrutinize effects from local institutions on the individual level.

7.6 Final reflections

I conclude this thesis with three final reflections: they refer to validity, methodology, and practise. One, local institutions are highly context-specific, as they are firmly rooted in specific culture and norms, the local stage of development, and institutional setup (Lambsdorff & Schulze 2015). To what extent then are my findings, based on research in a single locality in Liberia, representative for other geographical areas? In other words, what can we say about the external validity of this study? The historical and institutional context of Liberia, as described in Chapter 2, is certainly a specific one. Yet, the Liberian economy has its roots in a typical rentier state, like multiple other African nations as well as much of the 'New World'. In addition, the local institutions that took a central position in this thesis; kinships networks and the central position of local 'Big Men', play a key role in much of Sub-Saharan Africa (e.g., Platteau 2000). Hence, I believe that the lessons derived from this research are applicable to many similar contexts in Africa.

Two, lab-in-the-field experiments remain an abstraction of reality. So, why do we play 'games' in villages in the African countryside? To the extent that subjects' behaviour in an experimental setting predicts their behaviour in real life (e.g., Armantier & Boly 2012), they contribute to a better understanding of how decision making occurs in real life. By conducting lab-in-the-field experiments I contributed to more understanding about how informal institutional dimension influence decision making at the

household level. In addition, by using rigorous quantitative methods, I hope to make credible causal claims about the impact of local institutions on development.

Three, the cooperation between academic researchers and development practitioners can be a fruitful marriage. Development practitioners may derive directly applicable lessons from project evaluations. At the same time, evaluating an actual development intervention provides researchers with an exquisite opportunity to look into the mechanisms that could explain the outcome of the project. By adding additional sub-treatments to existing interventions, by conducting behavioural experiments and by asking numerous survey questions, we can move beyond the mere question whether projects work or not (e.g., see Deaton 2010; Barrett & Carter 2010 for critical reflections). If we use impact evaluations to attempt to uncover the mechanisms that drive development, this cooperation allows us to deepen our understanding of the dynamics between institutions and development.

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Summary

In this thesis I investigate the role of local institutions in the context of rural development. This research fits in a wider literature on the relationships between formal and informal institutions and economic development. Yet, there is still a lot to be understood about the dynamics between local institutions and economic decision making on the micro-level. I present evidence from rural Liberia based on micro-economic research, using a range of experimental methods.

In Chapter 1, I present an overview of the role of institutions in development, and the interrelations between the key concepts in this thesis: kinship networks, local governance, and development interventions. This discussion leads towards a set of research questions that guide the core chapters in this thesis. Specifically, the research questions break down into: Do tightly-knit family networks affect economic decision making (Chapter 3)? Does corruption affect public and private investment decisions (Chapters 4 and 5)? Do corrupt leaders target specific social groups (Chapter 5)? Does a rural community training project contribute to improved livelihood outcomes and does this depend on local institutions (Chapter 6)?

In Chapter 2, I set the stage of the research. The chapter presents key events in the Liberian political history, focussing on the settler history and the civil war events. These historical events have important implications for institutions and development in present-day Liberia. I also provide a description of the study areas where the data for this thesis were collected as well as the data collection procedures.

In Chapter 3, I analyse the impact of family networks on investment decisions, relating data from a detailed network survey to people's behaviour in a modified lottery experiment and a time preference game. I find that dense family networks are related with costly income hiding and lower discount rates. These associations are driven by the people who have been confronted with sharing obligations before. The results offer tentative evidence that dense family networks, under some conditions, have adverse impacts on economic decision-making.

In Chapters 4 and 5, I explore the effects of the quality of local leadership, using a field experiment to obtain an objective measure of thieving behaviour of the village chief—a proxy for corruption. Chapter 4 links corrupt behaviour of the village chief to public and private investment decisions, as revealed in a public goods game and a lottery game. The results show that corruption undermines incentives for voluntary contributions to local public goods, and may reduce private investments of individuals who are subject to rent-seeking by the chief in real life. The impact of corruption on investments and contributions to public goods seems to be gender-specific and appears to vary with accessibility of communities. Chapter 5 builds on these results, and links corrupt behaviour of the village chief to economics activities of villagers. I find that thieving behaviour of the chief reduces rice planting and trading activities and that the adverse effects of predation are driven by the responses of those individuals with a different ethnic identity than the chief's.

In Chapter 6, I assess the impact of an agricultural community development project on livelihood outcomes and social cohesion. I find weak evidence that the project contributed to higher rice harvests. The project did not contribute to social cohesion. The analysis suggests that the project caused time allocation shifts within households: the project has a robust, positive effect on time spent on farming activities by children. The study also shows that the involvement of group members in project design and monitoring the quality of the group leader can lead to better results, and that corrupt behaviour of the village chief can negatively affect direct project outcomes.

Finally; Chapter 7 provides a synthesis and discusses the broader implications from the research findings from this thesis. Local institutions do sometimes form an obstacle to development, and they are not easily changed. Yet, they may also function as vehicle for change. Under both scenarios, a thorough understanding of the role of local institutions in society is of key importance.

Samenvatting

In deze thesis onderzoek ik de rol van lokale instituties in de context van rurale ontwikkeling. Dit onderzoek past in een bredere literatuur over de relatie tussen formele en informele instituties en economische ontwikkeling. Er echter is nog veel onbekend over de dynamiek tussen lokale instituties en economische besluitvorming op microniveau. Ik presenteer bevindingen uit ruraal Liberia gebaseerd op micro-economisch onderzoek, aan de hand van verschillende experimentele methodes.

In Hoofdstuk 1 geef ik een overzicht van de rol van instituties in ontwikkeling en de wisselwerking tussen de sleutelbegrippen in deze thesis: familienetwerken, lokaal bestuur, en ontwikkelingsinterventies. Deze uiteenzetting leidt tot een aantal onderzoeksvragen welke de basis vormen voor de kernhoofdstukken in deze thesis. De onderzoeksvragen zijn: Beïnvloeden nauw-verweven familienetwerken economische besluitvorming (Hoofstuk 3)? Beïnvloedt corruptie publieke en private investeringsbeslissingen (Hoofstukken 4 en 5)? Richten corrupte leiders zich op specifieke sociale groepen (Hoofdstuk 5)? Draagt een gemeenschappelijk ruraal trainingsproject bij aan verbeteringen levensonderhoud en is dit afhankelijk van lokale instituties (Hoofdstuk 6)?

In Hoofdstuk 2 beschrijf ik de achtergrond van het onderzoek. Het hoofdstuk geeft een overzicht van sleutelmomenten in de Liberiaanse politieke geschiedenis: de gebeurtenissen rondom de 'kolonisatie' van Liberia en de burgeroorlog. Deze historische gebeurtenissen hebben belangrijke implicaties voor instituties en ontwikkeling in hedendaags Liberia. Ook beschrijf ik het onderzoeksgebied waar de data voor deze thesis zijn verzameld en de procedures rondom de dataverzameling.

In Hoofdstuk 3 analyseer ik de relatie tussen familienetwerken en investeringsbeslissingen, waarbij ik data van een gedetailleerde netwerksurvey relateer aan het gedrag van mensen in een lotto-experiment en een tijdvoorkeurexperiment. Ik vind dat nauw-verweven familienetwerken samengaan met kostbare strategieën om inkomen te verbergen en met een lagere discontovoet. Deze verbanden worden gedreven door mensen die eerder in aanraking zijn geweest met de plicht om inkomen

te delen. De resultaten laten zien dat economische besluitvorming onder bepaalde omstandigheden negatief beïnvloed wordt door nauw-verweven familienetwerken.

In Hoofdstukken 4 en 5 onderzoek ik de effecten van lokaal bestuur. Aan de hand van een veldexperiment verkrijg ik een objectieve maatstaf voor diefstal door de dorpsleider—een proxy voor corruptie. Hoofdstuk 4 relateert corrupt gedrag door de dorpsleider aan publieke en private investeringsbeslissingen, gemeten in een publieke-goederenexperiment en een lotto-experiment. De resultaten laten zien dat corruptie de drijfveer om vrijwillig bij te dragen aan lokale publieke goederen ondermijnt. Corruptie lijkt ook private investeringen te verkleinen van individuen die in het dagelijks leven te maken hebben met corrupt gedrag van de dorpsleider. Het lijkt erop dat de invloed van corruptie op investeringen sekse-specifiek is en varieert afhankelijk van de toegankelijkheid van de dorpen. Hoofdstuk 5 bouwt voort op deze resultaten, en relateert corrupt gedrag van de dorpsleider aan economische activiteiten van dorpelingen. Ik vind dat diefstal door het dorpshoofd het planten van rijst en lokale handel beïnvloedt, en dat vooral de dorpelingen met een andere etnische identiteit dan die van de dorpsleider, hebben te lijden onder corrupt gedrag van de dorpsleider.

In Hoofdstuk 6 analyseer ik de effecten van een gemeenschappelijk landbouw-ontwikkelingsproject op de status van levensonderhoud en sociale cohesie. Ik vind zwak bewijs dat het project heeft bijgedragen aan hogere rijstoogst. Het project heeft niet bijgedragen aan sociale cohesie. De resultaten suggereren dat het project heeft geleid tot verschuivingen in tijdsbesteding binnen huishoudens: het project heeft een positief effect op de hoeveelheid tijd die kinderen besteden aan landbouwactiviteiten. Verder laat het onderzoek laat zien dat het betrekken van deelnemers bij het projectontwerp en het monitoren van de kwaliteit van de groepsleider kan leiden tot betere resultaten, en dat corrupt gedrag door het dorpshoofd een negatieve invloed kan hebben op directe projectuitkomsten.

Hoofdstuk 7 geeft tenslotte een synthese en bediscussieert de bredere implicaties van de bevindingen in deze thesis. Lokale instituties kunnen ontwikkeling soms belemmeren en zijn niet eenvoudig te veranderen. Echter, ze kunnen soms ook een instrument voor verandering vormen. In beide gevallen is een gedegen begrip van de rol van lokale instituties in de samenleving van groot belang.

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Publications

Beekman, G., Gatto, M. & Nillesen, E., 2015. Family Networks and Income Hiding: Evidence from Lab-in-the-Field Experiments in Rural Liberia. *Journal of African Economies*, 24(3), pp.453-469.

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Wageningen School of Social Sciences (WASS)
Completed Training and Supervision Plan

Wageningen School of Social Sciences

Name of the activity	Department/Institute	Year	ECTS*
A) Project related competences			
Advanced Econometrics (APL**) (AEP 50806)	WUR	2009	6
Advanced Micro Economics (ECH 32306)	WUR	2010	6
Behavioural Economics (Prof. J. Potters)	NAKE	2010	6
Program evaluation methods (Prof. G. Imbens)	Foundation of the Swiss National Bank	2010	1.4
Intensive course in evolution of social preferences	University of Trento	2011	2.9
B) General research related competences			
'Kinship networks and investment in post-war Liberia'	CSAE (Oxford) & WASS PhD Day	2011	2
'Corruption, investments and contributions to public goods: experimental evidence from rural Liberia'	CSAE (Oxford) & Humboldt University (Berlin) & SEEDEC (Bergen)	2012 2013	1
'The effect of local corruption on investment behaviour: experimental evidence from rural Liberia'	Shanghai University of Finance and Economics & Zhejiang University	2012	1
'Rural development, community participation and leadership: Evidence from an RCT in rural Liberia'	UNU-MERIT (Maastricht)	2015	1
'Sanctioning regimes and chief quality. Evidence from rural Liberia'	Wageningen University	2015	1
C) Career related competences/personal development			
Creative Thinking	NWO	2010	0.3
Competence Assessment	WGS	2011	0.3
Project and time management	WGS	2011	1.5
Writing and presenting a scientific paper	WGS	2011	1.2
Career Assessment	WGS	2012	0.3
Mobilising your scientific network	WGS	2012	1
Career perspectives	WGS	2013	1.6
D) Teaching assistance and students' supervision			
Introduction Development Economics		2012-2014	1
Field Research Practical		2012	1
Methodology for field research in the social sciences		2011, 2012	0.4
Methods, Techniques and Data Analysis for Field Research		2012	0.4
Supervision 1 BSc and 2 MSc students		2011-2015	1.2
TOTAL			**38.5**

Notes: * 1 ECTS is equivalent to 28 hours of study load; ** APL = Accreditation of prior learning

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Cover

Farmers clearing land in preparation of a community farmer field school in Montserrado county, Liberia, in December 2009. Picture and cover design by author.