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Unbundling Institutions at the Local Level: Conflict, Institutions and Income in Burundi

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Abstract: We use a new dataset from Burundi to analyze the role of local institutions as determinants of income, distinguishing between three distinct dimensions of the institutional framework: (i) property rights security, (ii) local political leadership and (iii) social capital. Using measures of conflict intensity during the civil war as instruments for local institutional quality, we demonstrate that property rights security is the most significant driver of long-term income. These insights extend earlier results from cross-country income regressions, and confirm the scope for institutional reform to lift African communities out of poverty.

Keywords: Property rights security, social capital, local governance, civil war, economic growth, Africa

JEL Code: B52, D23, D74, O12

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1. Introduction

The modern distribution of per capita income across countries is notoriously unequal, and has prompted a vast amount of research into the origins of income divergence. Development economists have advanced several candidate explanations to account for observed income differentials, including (differences in) capital accumulation, technical progress or the extent to which the forces of the market can work their magic. A consensus view is now emerging suggesting that the 'institutional framework' is one of the main determinants of growth and welfare, or the lack thereof. According to this view, institutions may range from rather volatile 'governance' measures to inert 'cultural' variables, and encompass both formal and informal rules of structured social interaction. Rodrik (2006) coins the phrase "institutions fundamentalism" to describe the modern economist's concern for institutional quality as a driver of development.

An apprehension for institutions dates back to the classics, and has featured prominently in a tradition of careful micro-econometric studies.[1] But the institutional perspective on (under)development has received a great impetus from a recent series of cross-country studies. In a very influential contribution, Acemoglu *et al*. (2001) tackle the endogeneity challenge that plagues econometric work on institutions and income – do good institutions raise incomes, or do high incomes result in investments in institutional quality? – by using historical data on settler mortality as an instrument for current institutional quality. They then demonstrated a causal link running from institutions (proxied by an 'expropriation risk' measure) to income.[2] A follow-up literature emerged that suggests institutions are the key driver for development, likely trumping everything else (e.g. Easterly and Levine 2003,

[1] Examples include Shaban (1987) on sharecropping models, and Townsend (1994) on insurance and risk sharing in a social context of limited formal insurance opportunities. See also Fafchamps (2004).

[2] Alternative instruments in this literature are various correlates of the extent of Western European influence around the world (Hall and Jones 1999), and measures of 'state antiquity' (Bockstette *et al*. 2002, see also Gennaioli and Rainer 2007 for work on pre-colonial centralization). Nunn (2008) compiled a dataset on African slave trades and links historic slave exports to current institutions in Africa.

Rodrik *et al*. 2004). As a result, the 'geography-as-destiny' perspective on development has been replaced by a view that emphasizes the indirect role of geography and natural endowments, co-shaping the institutional framework via its impact on the incentives and constraints of colonizers and local elites (e.g. Sokoloff and Engerman 2000, Acemoglu *et al*. 2001).

While cross-country studies have enhanced our appreciation of the key role of institutions in development processes, they are subject to several potential shortcomings (e.g., see Temple 1999 or Brock and Durlauf 2001 for critical discussions of growth regressions, for which similar arguments apply). First, the set of countries included in many regressions is quite diverse, and theories of growth are "open-ended". This means the risk of omitted variables is looming large. Second, it is an open question whether the "one-size-fits-all" approach implied by most income regressions does sufficient justice to the potential issue of parameter heterogeneity—is the link between institutions and income the same for oil-rich Nigeria as for landlocked Burkina Faso? Third, cross-national studies necessarily gloss over potentially considerable within-country heterogeneity of various relevant variables, including income and institutional measures, which renders interpretation difficult and may produce biased estimation results (Imbens and Angrist 1994). Fourth, issues have been raised about the institutional proxies commonly used, and there are concerns about the opaque way they are constructed (e.g. Pande and Udry 2005). Some of the governance data are surprisingly volatile, and it is unclear what they exactly capture (Glaeser *et al*. 2004). Fifth, using coarse institutional measures it is very difficult to infer the exact causal mechanism linking institutions to income, and as a result the studies yield little in terms of practical advice for policy makers aiming to improve the fate of their constituency. Rodrik *et al*. (2004) refer to the operational guidance that may be obtained from their results as "extremely meager." Weak macro institutions are widely recognized as a major cause of

poverty in Africa**,** and have been associated with a diverse set of factors, including social capital (Coleman 1990), ethnic fractionalization (Alesina *et al*. 2003), levels of pre-colonial organization (Englebert 2000, Gennaioli and Rainer 2007), and neo-patrimonial political strategies that may be traced back to colonial times (Lewis 1996). If the main lesson for African policy makers is to improve their institutions to Scandinavian standards, then logical follow-up questions are "which institutions should I tackle first?" and "how should I go about improving them?" (e.g. Rodrik 2006).

In this paper we revisit the institutions - income nexus using a new and extensive dataset from Burundi that we collected ourselves.[3] Our main contribution is threefold. First, we do a cross-section analysis at the *micro-level* – employing community and household data – and explore the role of institutions as a driver of *within*-country inequality as opposed to *between*-country inequality. In other words, we exploit the synergies between research based on micro data and the questions posed by the institutions and growth literature (Pande and Udry 2005). We measure institutions at the community level, and are the first study to use income, demographic and tenure data from a large number of randomly sampled households. Since the (macro) policy regime and overall institutional framework (as well as many other relevant variables) are the same throughout Burundi, many possibly influential yet unobservable variables in cross-country regressions are eliminated. Hence, omitted variable bias is attenuated (but of course not eliminated). Similarly, parameter heterogeneity issues are less likely to be relevant. To the extent that the mechanism linking institutions to income is similar in micro and macro studies, our analysis also represents a robustness check of earlier macro studies.

[3] The data collection was a collaborative effort between the Institut de Statistiques et d'Etudes Economiqeus du Burundi (ISTEEBU), Antwerp University, Brussels University and Wageningen University, and was implemented under the flag of MICROCON – an EU funded project focusing on household analysis of violent conflict in various regions of the world.

Second, we distinguish between different "*types of institutions*". Virtually all of the current literature narrowly focuses on institutions that protect individual property rights. However, this is but one dimension of the institutional framework. North (1981) distinguishes between institutions that protect individuals from 'grabbing' by the government or elites, versus institutions that facilitate contracting between individuals (reducing transaction costs). Nonetheless, this partitioning also does not exhaust the set of relevant institutional dimensions. For example, political participation and democratic rights may matter for development, and societies need to develop mechanisms to overcome coordination problems and agree on the provision of (local) public goods. We introduce two new institutional measures in addition to a conventional measure of property right protection. The first new variable is a measure of the quality of local political institutions, and the second one is a measure of social capital. Note that our focus on spatial variation in existing institutions, combined with the distinction between economic and political institutions, will produce outcomes that are better tailored to constraints of policy makers than blanket recommendations from cross-country studies.

Third, we propose a novel instrument for institutions that may be useful for future micro-level research in war-torn countries. While we do not worry much about reverse causality between income and institutional quality in our case (our income variable is measured at the household level, and institutional quality at the community level—plausibly exogenous to individual households characteristics[4]), our ability to draw causal inferences from OLS estimates is compromised by potential omitted variables affecting both income and institutions. To attenuate this problem we resort to a series of 2SLS regressions. We demonstrate that different forms of violence occur during civil wars, and that an important component of such violence is indiscriminate – affecting communities and individuals

[4] We measure institutions at the so-called *colline* level. This corresponds to communities of on average 680 households. This number is sufficiently large to justify the assumption that households take the quality of institutions as given (this assumption is supported by the data—see below).

randomly (see also Kalyvas 2006, Blattman 2008 and Lyall 2008).[5] We show that several measures of historical conflict intensity (defined below) can serve as valid instruments for property rights security, political institutions and social capital – accounting for part of the exogenous variation in local institutional quality. While income is an important determinant of conflict (e.g. Miguel *et al*. 2004), we find that the impact of conflict on income in the Burundi context is indirect, via institutions.

We are not aware of other studies that consider the impact of multiple institutions on income at the micro-level. Two earlier macro studies aim to "unbundle" institutions at the cross-country level, and use multiple instrumental variables strategies to distinguish between two distinct institutional dimensions. Bardhan (2005) includes a measure of democratic political rights in addition to the conventional private property rights protection variable. Using 'state antiquity' as an extra instrument (in addition to settler mortality), he finds support for the idea that both economic institutions as well as political rights matter for development. Acemoglu and Johnson (2005) unpack the broad cluster of institutions differently, and distinguish between property rights institutions (constraints on the executive) versus contracting institutions (capturing the cost of enforcing contracts between citizens). Using the legal origins of former European colonies as an extra instrument, they confirm earlier results that property rights institutions matter for development. However, they also find that contracting institutions have no significant impact on income or development.

Perhaps more closely related to our work are two case studies focusing on single countries (though both are situated in Asia and not in Africa, and more importantly; both are based on village or district data rather than household data). First, Grimm and Klasen (2008) revisit the "institutions-versus-geography" debate in the context of an Indonesian study. They use geography-induced migration pressure as an instrument, and consider the effect of

[5] The use of indiscriminate violence has been extensively covered in the civil war literature, for an exhaustive overview see Kalyvas (2006).

land rights (titles) on technical change in agriculture and development.[6] Their results suggest that the emergence of land rights – an institutional innovation – is an important determinant of agricultural productivity. Second, Banerjee and Iyer (2005) analyze 'institutional overhang' in India, and consider the impact of historical land revenue systems on current agricultural performance and development. So-called 'non-landlord districts', where British colonial rulers took over land tax collection from local landlords, systematically outperform areas where the landlords were able to retain or fortify their grip on power. Banerjee and Iyer point to persistent social cleavages and class-based antagonism to explain the weak performance of 'landlord districts.' Consistent with findings by Bardhan (2005) this suggests the range of institutions relevant for development may be broader than just institutions protecting (private) property rights.

We derive two sets of important results. First, and confirming results from cross-country studies, we find evidence of a strong causal link from property rights security to income. This casts new light on the existing evidence of the importance of such institutions for agricultural development in sub-Saharan Africa—which is mixed (e.g. Place and Hazell 1993, Besley 1995, Braselle *et al*. 2002, Pande and Udry 2005, Deininger and Jin 2006, Jacoby and Minten 2007, Holden *et al*. 2008). Indeed, property rights security emerges as the main determinant of income in our study. Interesting, we find no evidence of such a causal link for political institutions once property rights security is controlled for. This echoes the ambiguous evidence of macro studies exploring the relation between political institutions (including political regimes) and economic outcomes (e.g. Barro 1999, Durham 1999, Bardhan 2005, Acemoglu *et al*. 2005, 2008). We also find no evidence that social capital has a robust positive impact on income, which is not inconsistent with earlier empirical evidence either. For example, Narayan and Pritchett (1999) find a positive effect

[6] In the absence of income data, Grimm and Klasen use the percentage of houses built from brick, stone or cement as a measure of village income. Their recall-based panel set allowed them to use fixed effects estimators to identify the causal effect of institutions on development.

of social capital on economic performance, but Miguel *et al*. (2005) do not. Our findings suggest that political institutions and social capital are unlikely candidates to lift rural communities in Burundi out of poverty. Property rights security trumps alternative dimensions of the institutional environment when explaining income.

Our second result contributes to a better understanding of the multi-faceted impact of conflict on institutional development, which on balance might be positive. A reduced-form regression explaining current income in Burundi by a number of controls and the number of attacks on the community (by rebels or the army – a conflict variable made available via the Peace Research Institute in Oslo, PRIO), reveals a *positive* correlation between conflict and income (details in section 4.2). We explain this apparent paradoxical result by demonstrating a beneficial impact of conflict intensity on the quality of local institutions. There is evidence from other African regions to support this view (but of course the external validity beyond specific African countries needs to be verified). While Collier (2003) emphasizes the negative effects of civil war on development (destroying physical, human and social capital and infrastructure, as well as creating political instability), more recent evidence suggests a positive correlation between conflict and political participation or collective action.[7] Writing about Uganda, Blattman (2008) claims a positive causal effect of conflict on political participation and awareness which he explains by arguing that violence augments the inherent value placed on political expression. Similarly, Bellows and Miguel (2006) find a positive correlation between local measures of conflict intensity and political awareness and participation in Sierra Leone. Our study suggests that the effect of conflict intensity is

[7] Evidence for Uganda presented by Deininger (2003) supports Collier's dismal perspective. He finds support for the view that conflict discourages investment and promotes a subsistence lifestyle with limited opportunities for economic growth. In contrast, Bellows and Miguel (2006) do not detect negative effects of the Sierra Leone war on consumption shortly after the war. This result is possibly indicative of the low capital intensity of local production in (rural) Sierra Leone (such that losses due to destruction of physical capital were modest). For our identification strategy it is important whether conflict has a direct negative effect on income in Burundi, or not. Consistent with Bellows and Miguel (2006) we find no evidence of such an effect—see below.

institutions-specific, but we confirm that conflict can enhance the quality of local institutions, providing an impetus to economic growth.

This paper is organized as follows. In section 2 we provide background to the Burundi case and briefly sketch its recent history of bloodshed. Importantly, we distinguish between different types of conflict, and motivate why certain types of conflict may be treated as exogenous variables. This enables us to consider them as instruments for the various institutional measures in subsequent income regressions. In section 3 we introduce our data and empirical strategy. Section 4 contains our results. We start by presenting naïve OLS evidence, and proceed to present two-stage regression results, instrumenting for political and economic institutions as well as social capital to account for potential endogeneity and control for potential omitted variable bias. We present community-level and household-level evidence, and find the main results are robust across specifications. Section 5 concludes.

2. Conflict and Post-Conflict Burundi

Burundi has known several periods of civil war, characterized by strife for political dominance between the nation's two main groups: Hutu (comprising 85% of the population) and Tutsi (14%). Under Belgian colonial rule (1923-1962), Tutsi dominated the public administration and most Hutu were barred from (active) political participation. The process of Hutu exclusion intensified following independence. Nkurunziza and Ngaruko (2005) describe how Tutsi from one region – southern Bururi province – dominated the state and increasingly attempted to manage the entire nation for private gain. This process, which may be characterized as extremely weak "macro institutions" prompted several Hutu insurrections. These rebellions (mainly in 1965, 1971-72, 1987 and 1993) typically triggered drastic responses from the predominantly Tutsi army, resulting in large-scale killings and displacement of Hutu. In 1993, following the assassination of Burundi's first Hutu president

Ndadaye, Hutu rebel groups massacred thousands of Tutsi. The army responded with large scale attacks on Hutu, "…*making no distinction between communities which had been involved in violence against Tutsi and those that were not.*" (HRW 1998: 15). In a period of several weeks between 30.000 and 50.000 people were slain. In the years that followed, widespread violence by both rebel groups and the army raged across the country. Over 300.000, predominantly Hutu, have been killed as a result of this violence. Many more have been injured or displaced. It is estimated that no less than 1.2 million Burundians were internally displaced or refugees in neighboring countries, so 20% of the country's population was uprooted (Krueger and Krueger 2007).

The violence in Burundi can be divided into two types—selective and indiscriminate violence. The former type targets individuals selectively. The army, for example, singled out those individuals who could form a threat to the Tutsi government. This included Hutu with higher levels of education or leadership positions (Krueger and Krueger 2007). The rebels engaged too in selective violent acts against civilians, visiting *collines* (the French word for "hill", coinciding with the natural boundaries of most communities in rural Burundi) at night demanding supplies and shelter. Selective violence also occurred within communities, where the breakdown of the rule of law provides an opportunity to 'settle scores,' take measures to avoid repayments of outstanding debts, or to reshuffle ownership of valued property. Andre and Platteau (1998) describe such phenomena for the case of Rwanda, where especially large landholders and creditors were targeted by their fellow villagers. Experiences of non-random killings and violence have also been recorded elsewhere. Deininger (2003) for example, reports distance from infrastructure, asset inequality, appropriable wealth (cash crops) and low levels of human capital as important determinants of rebel attacks. Such non-random violence is likely endogenous to economic outcomes, and therefore not suitable for our identification strategy. For example, our dataset

contains information on variables like “assets stolen,” “kidnappings” and “torture.” Indeed, we find such variables are correlated with household characteristics such as income and education.

However, often-times violence is indiscriminate and has random elements, allowing us to exploit these data and address the nature of the causal relation between current institutions and post-war income (see below). Acts of indiscriminate violence by army and rebels are a commonly-highlighted feature of civil war (e.g. Kalyvas 2006). In the absence of sufficient information to separate rebels from the general population, armies often resort to unselective violence. Other motivations for random violence include a desire for extermination or revenge, displacement of people, and plundering. At other times there is a perceived need to demonstrate power (as part of the tactics of fear to control a population – see Kalyvas 2006). Arguably, this type of violence is exogenous to household characteristics and economic outcomes, hitting communities and civilians indiscriminate of social status, education or income. However, it likely leaves its mark on communities though loss of life and property, and via its impacts on social structures and institutions.

There is lots of evidence of such random violence in the Burundi context. Both army and rebels have arbitrarily and sweepingly targeted entire communities (HRW 1998) throughout the country. The army often assaulted *collines* where they suspected – correctly or not – that rebels were hiding, or where civilians were believed to have lent support to rebel groups. In search for rebels and arms, the army regularly killed, maimed and looted with no regard for the characteristics of their victims (Krueger and Krueger 2007). ‘*The excuse was that they were searching for weapons hidden by the bandes armées* [Hutu rebel groups], *and their mode of operations was predictable. The blindés* [armored vehicles] *went in first, firing at random and hoping to draw return fire from the houses. If a Hutu fired a weapon to*

defend himself, he then drew cannon fire or machine-gun response from the blindés.' (Krueger and Krueger 2007: 205).

A drastic example of the large scale violence by the army campaign was during the formation of '*regroupement camps'* between 1996-2001. In area's suspected of rebel activity, the army led a brutal campaign to cut rebels of supplies, support and shelter. Civilians were evicted from their homes and forcibly brought together in camps. As an incident report from Human Rights Watch reports: "*Soldiers created the camps. When they suspected that there was an area where the rebels were active, soldiers would come and order people to gather at a specific site. They killed anyone who refused*." (HRW 1998: 29). At its apex, an estimated total of 220.000 people lived in these camps.

Even though violence and intimidation have not disappeared from Burundi, the country has now known several years of relative peace. Efforts to quell the violence caught root at the turn of the century, and in 2003 an internationally-brokered peace agreement was signed between the government and all-but-one of the rebel groups. This paved the way for a transition process that led to an integrated defense force, establishment of a new constitution in 2005, and elections that resulted in a majority Hutu government. However, ravaged by three decades of civil war and conflict, Burundi is still amongst the poorest in the world. Average per capita consumption expenditures in rural areas are just under $0.50 per day (BPHS data). Most of the population is employed in agriculture, of which the majority is at least partly dependent on subsistence farming. Pressure on land is high with population densities of up to 700 people per square kilometer (MPDRN, 2006). Nearly all agriculture is rain-fed, and relies to a large extent on household labor (utilizing low levels of external inputs). The staple diet amounts to cassava, beans, banana's and sweet potatoes. Child mortality is high and undernourishment, exuberated by weather shocks, is severe, regularly resulting in stunted growth (e.g., Bundervoet *et al*. 2008). A widespread drought in 2006,

combined with a new crop disease (the mosaic disease in cassava plants) put further downward pressure on incomes for rural Burundians.

3. Empirical Strategy and Data

In this section we outline our empirical strategy and present our data. We first consider community level correlations between institutions and income and estimate a simple OLS framework:

$$Y_j = \alpha_k + \beta_1 EI_j + \beta_2 SC_j + \beta_3 PI_j + \beta_4 X_j + \beta_5 C_j + \varepsilon_j, \quad (1)$$

where Y_j refers to average per capita income in community *j*, and where *j*=1, …,100. Next, EI_j refers to the quality of economic institutions (property rights security), SC_j to the level of social capital, and PI_j to the quality of political institutions, all in community *j*. The vectors X_j and C_j refer to two vectors of control variables. The *X* variables were measured at the household level (and therefore represent community averages in equation 1), and the *C* vector captures community controls. We include province fixed effects, α_k where $k = 1,...,13$, to capture province-level differences in for instance geography, disease environment, institutions and policies.

To attenuate endogeneity problems and more fully exploit the variation in our dataset, we proceed by replicating our OLS regression using household-level income data and controls:

$$Y_i = \alpha_k + \gamma_1 EI_j + \gamma_2 SC_j + \gamma_3 PI_j + \gamma_4 X_i + \gamma_5 C_j + \mu_i \quad (2)$$

where the subscript *i* refers to household *i* (where *i*=1,…,874). We cluster our standard errors at the community level to control for potential autocorrelation between households within a *colline*. To further explore the issue of omitted variables affecting both institutions and income simultaneously, we continue our analysis by first analyzing the impacts of

various measures of past violence intensity on present institutions. Our first stage regression is specified as:

$$I_j = \alpha_k + \delta_1 V_j + \delta_2 X_i + \delta_3 C_j + \nu_j, \quad (3)$$

Where V_j refers to a vector containing our community level conflict variables (number of attacks, killings, maiming and orphan-headed households—defined below), and where I_j = EI_j, SC_j and PI_j. The next and final step of our analysis considers the impact of violence-induced institutions on current income levels in a 2SLS framework:

$$Y_i = \alpha_k + \gamma_1 EI_j^* + \gamma_2 SC_j^* + \gamma_3 PI_j^* + \gamma_4 X_i + \gamma_5 C_j + \mu_i, \quad (4)$$

where EI_j^*, SC_j^* and PI_j^* refer to our instrumented institutions variables as predicted in (3). Our identifying assumption is that violence is random at the household level and does not influence current income other then through its impact on institutions.

Next, we introduce our data. For our analysis we mainly draw from the new and extensive Burundi Priority Household Survey (BPHS) and Community Survey (BCS). The BPHS contains data from 874 households living in 100 *collines*, for which we recorded detailed information on socio-economic and farm characteristics as well as information on several dimensions of institutional quality. The BCS was aimed to collect community-level data, and for this purpose we interviewed several (typically: three) community leaders (such as local administrators) in all 100 communities. As an example of relevant community variables we have collected information on community experiences with conflict. The data was collected in 13 of the 16 Burundi provinces that have known several years of relative peace. In the three remaining provinces (Makamba, Bubanza and Bujumbura Rural) rebel activity is still persistent and data could not be collected. Interviews were conducted in Kirundi (the main local language), and recorded in French. Local enumerators, in collaboration with ISTEEBU and MICROCON researchers visited the field and collected the data in August and September 2007.

Table 1: Descriptive Statistics

Variable	obs household level	obs community level	mean	st. dev.	min	max
Panel A: Institutions variables						
Households in community with full land titles (%)	874	100	0.45	0.35	0.00	1.00
Tenure security	874	100	0.85	0.14	0.32	1.00
Cooperation	874	100	0.88	0.11	0.50	1.00
Trust in community	874	100	0.71	0.07	0.49	0.86
Political institutional quality index	874	100	0.68	0.07	0.51	0.80
Services	874	100	0.68	0.23	0.00	1.00
Panel B: Community level violence variables						
Number of times colline was attacked (PRIO)	874	100	1.36	3.18	0	17
Number of dead in community attacks	874	100	58.93	118.09	0	560
Number of wounded in community attacks	874	100	6.79	15.30	0	91
Fraction of orphan headed households	874	100	0.02	0.04	0	0.33
Panel C: Post war income variables						
Household level per capita expenditure on food and non-food (BIF)	872	100	4805.61	6367.04	0.00	62991.37
Household level per capita total expenditure (BIF)	845	100	15985.92	17103.56	0.00	160734.60
Household level per capita expenditure on food and non-food (log)	865	100	7.89	1.18	2.98	11.56
Household level per capita total expenditure (log)	871	100	9.26	1.03	3.40	12.06
Panel D: Household control variables						
Age of household head (years)	872	100	49.84	14.75	12.00	99.00
Dummy if household head is male	874	100	0.71	0.45	0.00	1.00
Dummy if household head literate	863	100	0.49	0.50	0.00	1.00
Land size per capita (ha^2)	848	100	1.34	1.61	0.00	14.03
Panel E: Community control variables						
Main road in community	874	100	0.14	0.34	0.00	1.00
Population density (MPDRN) (log, people/km^2)	874	100	5.68	0.49	4.04	6.56
Distance to market	874	100	2.93	0.81	1.00	4.90
Land Gini coefficient	874	100	0.41	0.10	0.20	0.62
NGO intervention in community	874	100	0.27	0.44	0.00	1.00

Source: BPHS and BCS, 2007 and PRIO Armed Conflict Database (available at http://www.prio.no), and MPDRN, 2006. We have deleted the two richest households (outliers), but this does not affect the main results. Note: USD 1 = 1169.55 BIF (17 July 2008)

Table 1 provides an overview of our main variables. Please refer to Appendix 1 for an overview of the specific survey questions (translated in English) and variable definitions. Panel A shows the summary statistics for the tree types of institutions we distinguish. Since institutions are best thought of as communal variables, we aggregate household responses to survey questions on local institutions. As mentioned, we distinguish between three different dimensions of he institutional framework. We use household ownership of formal land titles as a measure of the quality of economic institutions. Tenure security at the household level was captured by a dummy variable – unity for households with full titles to all their plots, and zero else – and we aggregated these numbers to arrive at a measure of average landownership in the community. The average community share of households with full title ownership is 45% in our sample. In addition to the formal title measure we also included a survey question asking people how certain they felt their plots were secure from expropriation. This alternative tenure dummy captures both formal and informal tenure arrangements (customary law at the community level), and allows a robustness check of our results. Customary law is broader than formal rights, and 85% of the households respond they feel secure about their land.

While the exact definition of social capital is subject to debate, most analysts treat it as a characteristic of communities, and describe it in terms of trust, norms and networks that enable collective action (e.g. Bowles and Gintis 2002). For example, these elements are all present in Fukuyama's (1995) view that trust is key to social capital, which he describes as 'the expectation that arises within a community of regular, honest and cooperative behavior based on commonly shared norms on the part of other members of that society'. Social norms influence people's preferences and constraints, lower transaction costs (as it precludes the necessity to write contracts that capture all contingencies), and facilitate the exchange of information. In an effort to operationalize these potent but rather imprecise ideas we focus

on two aspects of social capital by including specific questions to our household survey. We constructed two measures; one for community-level trust and one for cooperation. For our measure of trust we employed World Value Survey type questions, asking respondents to rate their level of trust in their household, extended kin and fellow *colline* members. Answers were recorded on a six point scale, and after adding the three sub-indices the score was rescaled to vary between zero and one. (Our results are robust to omitting trust in household members and only considering stated trust in extended kin and fellow *colline* members, or indeed considering just trust in *colline* members). On average, trust in kin and community members was rather high, with an average score of 0.7. For our indicator of cooperation, respondents were asked whether they agreed with the statement '*Do most of the people in your colline help each other out when help is needed?*'. On average 88% of households expressed that people in their community cooperated, reflecting only modest variation across households (but note that averaged scores across *collines* vary from 0.5 to 1.0). We employ both measures of social capital and find they produce similar results.

Our final institutional dimension is the quality of local political institutions. Local leadership is relevant not only because it provides a coordination mechanism to overcome social dilemmas, it also serves a role of arbiter in case of contested land rights and so on. As our local governance proxy we employ two distinct measures, one based on a (subjective) assessment and the other one on objective elements. For our subjective measure, the BPHS asked respondents to rate the quality of three community-level institutions on a six point scale: (i) local justice authority (whose main task is to mediate in local land conflicts), (ii) local administrator (*chef du colline*), and (iii) school teachers. The three sub-indices were added, and then rescaled so that they score between 0 and 1, to create a single community-level institutional index. Our objective measure is based on an index of the availability of local public goods, where either the provision or else the maintenance of the good is the

responsibility of local administrators.[8] Greater availability of local public goods, then, is hypothesized to reflect better local governance. Again, both measures produce similar results. Since the subjective measure is arguably less narrow than the objective one – as it captures a broader range of responsibilities of local governments – we focus our presentation on this variable.

Arguably, our measures of institutions may be correlated. We find a negative and significant correlation between the quality of political institutions and land titles ($\rho = -0.2$) as well as between political institutions and levels of cooperation ($\rho = -0.1$). Perhaps people respond to poor leadership by demanding formal land titles or developing greater levels of cooperation (ICG 2003). Another view is that NGOs, assisting communities with land distributions and sometimes stimulating the formation of land titles, have selectively targeted communities with poor local governance. We include a dummy for NGO presence to control for any NGO effects other than facilitating land titling or cooperation.

To capture community-level violence, our BCS recorded both the local number of civilians killed and injured from confrontations between army and rebels or one-sided violence in the years 1993-2007 (Panel B). In some *collines* these attacks were severe, when the army targeted whole communities in their search for rebels, arms and loyalists (Krueger and Krueger 2007, HRW 1998). Similarly, rebels often did not discriminate between individuals and targeted entire communities in search for supplies. Our data contains estimates of the number of fatalities and wounded during such events, as expressed by local administrators during the community survey. Another proxy of violence unleashed upon communities is the fraction of orphan-headed households in the community. In some *collines* this fraction extended to one-third of all households. As a final measure of violence,

[8] The variable measures the fraction of 5 public goods available in a community: primary school, health center, drinking water source, electricity and hardened road. We define "availability" as being within a two hour walking distance measured from the center of the *colline*.

we use local data from the pre-existing PRIO database. This measure identifies violence between competing rebel factions as well as violent clashes between army and rebels, and describes the total number of attacks on the *colline* (see Figure 1).[9] Throughout, we restrict community level violence to events taking place in the period 1993 – 2003 so that the PRIO data and our own data overlap.[10] We find some significant correlation between conflict variables. In places where a lot of attacks occurred, more people died ($\rho = 0.1$). Similarly, and unexpectedly, the number of orphaned households is positively correlated to the number of dead ($\rho = 0.2$) and wounded ($\rho = 0.6$).

Next, we turn to our income data. As our base measure of income we use per capita monthly expenditures on recurrent consumption goods (Panel C). This variable is labeled 'expenditures on food and nonfood items,' and sums all expenditures on food and regular non-food items (such as soap, salt and tobacco) valued at current prices prevalent at local markets.[11] We use current expenditure as a proxy for income as this is a better measure of permanent income than current income in the presence of saving and dissaving. Moreover, it is well-known that capturing full income using surveys is notoriously difficult (e.g. Deaton 1997). Per capita expenditure levels are constructed to control for family size effects. (For a discussion and analysis the impact of war shocks on consumption growth 1998-2007 which takes household split-offs into account (BPHS data), we refer to Verwimp and Bundervoet 2008).

Mean per capita expenditure in our sample is slightly over 4806 BIF per month, but there is considerable variation across households. Nine households, or about one percent of the respondents, indicated to not purchase any food or non-food items and be fully self-

[9] The PRIO dataset records the coordinates of battles, as well as one sided violence, for the period 1963-2003. We matched these coordinates to the *collines* in our dataset.
[10] Note that our income data is recorded for 2007. Limiting our violence variables to the period 1993-2003, hence greatly reduces any possible direct effects of conflict on income.
[11] Due to substantial differences in prices between markets (reflection degrees of autarky), we used 2007 district level prices for our calculation (FAO estimates).

sufficient. We use the natural log of expenditure as our dependent to (partially) control for the influence of outliers and for easy interpretation of our coefficients. We also exclude two outliers as identified via the Hadi method. As a robustness check we include a measure of total household expenditures. In addition to the expenditure categories mentioned above, this income measure also includes expenditures on durables (such as schooling, health, clothing, housing, ceremonies, travel), transfers, as well as auto-consumption of crops grown.

Finally, as controls we include sets of household-level variables (X_i) as well as community-level variables (C_j). These are summarized in Panels D and E, respectively. Household level variables include age (in years); gender (a dummy for male-headed households); literacy (a dummy variable when the household head is literate); and per capita owned land (expressed as square meters per head). Community variables are a dummy variable indicating whether a main road is present in the *colline* (as one proxy for transport costs); population density to capture both land pressure and demand for institutions (expressed as people per square kilometer); distance to an important agricultural market where food and nonfood items are traded (measured in time intervals of fifteen minutes); and a variable measuring land inequality at the local level (a Gini coefficient for land holdings). As land inequality is potentially endogenous we also ran regressions without this variable, and detected no qualitative changes to our main results. Finally, and as mentioned, we included a dummy to control for NGO intervention in the *colline*.

4. Results

4.1: Correlations

We now outline our main results and commence our exploration by presenting a series of correlations between income and institutions. Table 2 summarizes two sets of OLS regressions. In the left panel we use aggregate community data, and regress (average)

income on our three measures of institutions – property rights, political institutions and social capital – while controlling for a range of community-level variables (equation 1). The right panel of Table 2 does much the same but is based on disaggregate household data instead (equation 2).

Table 2 Institutions and Income - OLS Results

Dependent variable: expenditures on food and non-food per capita (log)	Community level						Household level					
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Title		0.500**	0.534**	0.672***				0.525***	0.561***	0.701***		
		(0.215)	(0.228)	(0.244)				(0.196)	(0.158)	(0.169)		
Cooperation		0.263	0.153		0.437			0.446	0.409		0.711**	
		(0.349)	(0.382)		(0.415)			(0.353)	(0.288)		(0.341)	
Political institutions		-1.442	-1.669			-2.483**		-1.626*	-1.495**			-2.441***
		(0.980)	(1.003)			(0.985)		(0.880)	(0.712)			(0.717)
Density (log)	-0.183		-0.095	-0.145	-0.164	-0.108	0.113		0.164	0.139	0.119	0.156
	(0.243)		(0.206)	(0.212)	(0.249)	(0.225)	(0.174)		(0.137)	(0.141)	(0.167)	(0.168)
Main road in community	0.187		0.071	0.135	0.203	0.068	-0.006		-0.061	-0.007	0.028	-0.126
	(0.188)		(0.181)	(0.202)	(0.187)	(0.161)	(0.144)		(0.142)	(0.157)	(0.139)	(0.131)
Land gini	0.314		0.420	0.265	0.291	0.542	-0.468		-0.417	-0.612	-0.491	-0.175
	(0.382)		(0.386)	(0.377)	(0.369)	(0.382)	(0.441)		(0.413)	(0.400)	(0.443)	(0.440)
Respondent is literate	0.522		0.401	0.400	0.457	0.520	0.294***		0.280***	0.282***	0.283***	0.296***
	(0.357)		(0.328)	(0.329)	(0.355)	(0.333)	(0.089)		(0.088)	(0.088)	(0.088)	(0.088)
Distance to market	-0.191*		-0.215**	-0.213**	-0.194*	-0.198**	-0.123*		-0.162***	-0.152**	-0.132**	-0.140**
	(0.099)		(0.091)	(0.094)	(0.099)	(0.093)	(0.066)		(0.059)	(0.061)	(0.063)	(0.063)
Age	0.013		0.014	0.014	0.013	0.013	0.004		0.004	0.004	0.004	0.004
	(0.011)		(0.010)	(0.011)	(0.010)	(0.010)	(0.003)		(0.003)	(0.003)	(0.003)	(0.003)
Household head is male	-0.334		-0.111	-0.106	-0.288	-0.295	0.211*		0.237**	0.233*	0.220*	0.216*
	(0.507)		(0.476)	(0.485)	(0.515)	(0.463)	(0.119)		(0.118)	(0.118)	(0.119)	(0.118)
Land size per capita	-0.114		-0.136	-0.158	-0.106	-0.097	0.105***		0.099***	0.100***	0.103***	0.103***
	(0.116)		(0.111)	(0.119)	(0.117)	(0.108)	(0.028)		(0.027)	(0.028)	(0.028)	(0.028)
NGO intervention	-0.088		-0.117	-0.136	-0.088	-0.073	-0.171		-0.218**	-0.238**	-0.175	-0.157
	(0.146)		(0.130)	(0.129)	(0.146)	(0.137)	(0.113)		(0.108)	(0.106)	(0.112)	(0.112)
Constant	9.498***	8.810***	9.689***	8.810***	9.026***	10.842***	7.942***	8.719***	8.074***	7.441***	7.357***	9.363***
	(1.695)	(0.728)	(1.891)	(1.547)	(1.853)	(1.772)	(0.970)	(0.642)	(0.948)	(0.787)	(1.044)	(0.975)
Observations	100	100	100	100	100	100	833	865	833	833	833	833
R-squared	0.28	0.29	0.38	0.36	0.29	0.34	0.14	0.09	0.17	0.16	0.14	0.15
Durbin-Wu-Hausman p-value				0.28	0.03	0.91				0.37	0.86	0.38

Robust standard errors in parentheses, clustered at community level for household-level regressions.

*** p<0.01, ** p<0.05, * p<0

The community OLS results indicate a positive correlation between property rights protection and average income, and the magnitude of the coefficient is robust across specifications. In contrast, social capital does not appear to be correlated with income. While there is some evidence suggesting that political institutions are negatively correlated with income, this result is not statistically robust.

It is well known that OLS regressions, of the type reported in columns (1)-(6), are subject to various potential endogeneity problems. In columns (7)-(12), therefore, we regress household income on community-level institutions. Community institutions are likely exogenous to household variables, so this approach should eliminate, or at least attenuate, 'reverse causality' concerns.[12] However, and as mentioned above, omitted variable bias may still plague the estimations, and these results should therefore be interpreted with caution. In particular, making causal inferences remains hazardous.

With this caveat in mind, the household OLS results in the right panel of Table 2 confirm the earlier positive correlation between property rights protection and income, and also produce coefficients of similar size. We interpret this as early and tentative evidence that property rights security may matter for income and development. The literature provides several clues as to why land tenure rights might be important for (agricultural) development and income (e.g. Besley 1995, Jacoby et al. 2002, Braselle *et al*. 2002, Grimm and Klasen 2008, and many others). Enhanced tenure security reduces the risk of losing one's property, and therefore raises the relevant discount factor when evaluating investments generating future returns. This prominently includes investments in soil and land improvement, yielding private benefits for landowners and, due to the public good nature of erosion control efforts, possibly also yielding benefits for fellow landowners – see Bouma *et al*. (2008). Moreover, titled land may be easier to sell or rent out, enabling the owner to better capture future

[12] As a check we use Durbin-Wu-Hausman test statistics (displayed at the bottom row of Table 2) which confirms that community level institutions are indeed exogenous to household income.

benefits from current investments that enhance the value of the land. Finally, (formal) land titles facilitate access to credit as it may serve as collateral for financial intermediates. Hence, property rights security should invite productive investments, and from this perspective a positive correlation between this institutional measure and income is expected.

As before, there does not appear to be a robust correlation between social capital and income (a positive correlation only ensues when we do not control for our property rights variable). However, political institutions now enter robustly and with a negative sign. This may appear puzzling, but need not be inconsistent with earlier studies. The macro literature provides little reason to assume a positive correlation between (the subjective assessment of) political leadership and income. For example, political leadership that is supported by a majority of the population may retard growth when it is based on redistributive policies (e.g., Persson and Tabellini 1992). Conversely, "strong measures" implemented with an "iron hand" that may be unpopular by community members could facilitate investments and stimulate growth (Przeworski and Limongi 1993). However, the negative correlation need not be indicative of any causal relation between the two variables. For example, ethnic relationships and intra-community balances between Hutu and Tutsi—a major "taboo" in the Burundi context about which we were unable to collect data—could be related to both the appreciation of leadership and generation of income. We need to move beyond OLS estimates to shed light on these issues.

As a final observation about our OLS estimates, please note that regression results for the control variables correspond with intuition or common sense. Specifically, there are positive correlations between income on the one hand and literacy or the available per capita land base on the other hand. Not surprisingly, distance to the market correlates negatively with income. Table 2 also suggests that households headed by men are associated with higher income.

4.2: Conflict, institutions and income

We now motivate the use of historical conflict variables as potential instruments for the quality of current institutions. Formal test-statistics supporting this approach are provided in the next sub-section, but here we will start exploring the correlation between conflict and institutions, and between conflict and income. Earlier we argued that (the intensity of) conflict in the 1990s may be viewed as an exogenous variable for the communities and households in our study—at least the dimensions of conflict included in our analysis. For our purposes, it is also important to have a strong and robust relation between conflict and institutions. Moreover, while the absence of a significant link between conflict and income is not sufficient to qualify the conflict variables as suitable instruments (the conflict variables should not be correlated with the disturbance term in the income regression), it is helpful to verify that conflict does not have a direct impact on income beyond the indirect link via institutions. In Table 3 we present OLS results.

Table 3: Violence, Institutions and Income at the Household Level - OLS Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable:	Title	Cooperation	Political institutions	Title	Cooperation	Political institutions	Title hh level	Income	Income
Number of attacks 1993-2003	0.017***	0.006***	-0.000	0.024***	0.006***	-0.000	0.021***	0.042***	0.025
	(0.003)	(0.001)	(0.001)	(0.003)	(0.001)	(0.001)	(0.005)	(0.013)	(0.016)
Number of dead 1993-2003	0.000***	0.000*	-0.000***	0.000***	0.000	-0.000***	0.000*	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of wounded 1993-2003	-0.003***	-0.001**	0.000	-0.001**	-0.001**	-0.000**	-0.000	-0.001	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.004)	(0.003)
Fraction of orphan headed Households	0.715***	0.037	0.029	0.704***	-0.068	0.108	0.894	0.764	0.428
	(0.140)	(0.108)	(0.066)	(0.148)	(0.119)	(0.071)	(0.585)	(1.245)	(1.258)
Density (log)				-0.175***	-0.004	0.016**	-0.194***	0.012	0.150
				(0.030)	(0.018)	(0.008)	(0.064)	(0.174)	(0.135)
Main road in community				-0.040***	-0.034***	-0.064***	-0.017	0.029	-0.015
				(0.014)	(0.012)	(0.006)	(0.046)	(0.137)	(0.137)
Land Gini				0.358***	0.051	0.134***	0.386**	-0.197	-0.289
				(0.075)	(0.052)	(0.023)	(0.166)	(0.462)	(0.428)
Respondent is literate				-0.003	0.013	-0.000	0.018	0.290***	0.289***
				(0.015)	(0.008)	(0.004)	(0.031)	(0.080)	(0.090)
Distance to market				0.037***	0.014**	-0.009***	0.043*	-0.114*	-0.152**
				(0.013)	(0.006)	(0.003)	(0.022)	(0.058)	(0.060)
Age				-0.000	-0.000	0.000	-0.001	0.003	0.004
				(0.001)	(0.000)	(0.000)	(0.001)	(0.003)	(0.003)
Respondent is male				-0.028*	-0.012	0.001	-0.094***	0.228**	0.250**
				(0.017)	(0.009)	(0.005)	(0.035)	(0.099)	(0.121)
Land size per capita				0.008*	0.002	-0.001	0.018*	0.108***	0.101***
				(0.004)	(0.003)	(0.001)	(0.010)	(0.028)	(0.027)
NGO intervention				0.120***	-0.003	0.003	0.143***	-0.142	-0.220**
				(0.021)	(0.012)	(0.006)	(0.042)	(0.106)	(0.111)
Title									0.240***
									(0.074)
Cooperation									0.022
									(0.037)
Political institutions									-0.085*
									(0.047)
Constant	0.778***	0.813***	12.492***	1.368***	0.791***	0.596***	1.362***	8.265***	7.544***
	(0.015)	(0.014)	(0.091)	(0.171)	(0.092)	(0.043)	(0.339)	(0.908)	(0.722)
Observations	848	848	848	812	812	812	812	808	808
Province fixed effects	yes	Yes	yes	yes	yes	yes	yes	yes	yes
R-squared	0.64	0.26	0.33	0.68	0.28	0.46	0.27	0.151	0.17
F / Partial – F	446.94***	36.03***	40.24***	35.43***	14.33***	5.28***	6.41***	.	.

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

In columns (1)-(6) we regress our institutional variables on the conflict measures as well as a number of controls. Specifically, the controls in columns (4)-(7)[13] are 'included instruments' entering in the second stage below, so that we can view these OLS estimates as the first stage of our subsequent 2SLS exercise.

We consistently find strong correlations between conflict and institutions, and find that different dimensions of conflict are correlated with different institutional proxies. While we are agnostic about the exact nature of the first stage correlations (we only want the instruments to identify an exogenous source of variation in institutional quality and do not aim to propose a new theory linking conflict to institutions—see Rodrik *et al*. 2004), we note that some of the coefficients are amenable to ready interpretation. For example, the PRIO variable captures confrontations between rebels and soldiers, inadvertently hitting communities in the process, as well as attacks on such communities. In affected communities the demand for property titles has been greater, as households sought to secure land ownership where ownership was (expected to be) contested by those who fled during the violence and subsequently returned (ICG 2003). In many cases, these communities have also disproportionally benefitted from NGO assistance in developing land titling schemes and "community re-building" (Kamungi *et al*. 2004). From this perspective the positive and highly significant coefficient in columns (1), (2), (4), (5) and (7) is not surprising. We also document a negative correlation between conflict and perceived quality of local political leadership (columns (3) and (6)). This could be due to selective killing of pre-conflict leaders (HRW 1998). Regardless, the finding provides an alternative explanation for the positive association between conflict intensity and political participation observed by Bellows and Miguel (2006) and Blattman (2008). Perhaps communities affected by violence

[13] Columns (4)-(6) regresses the community level title variable on our instruments and controls. Instead, column (7) uses title ownership measured at the household level as the dependent variable.

do indeed inherit worse leadership, so that individuals have a greater incentive to become engaged in an effort to ameliorate the situation?

Column (8) shows a reduced-form regression explaining income by our conflict variables and a number of controls, showing a strong positive correlation between past conflict and current income: communities that experienced more violence realize higher incomes. In light of earlier work (e.g. Collier 2003) this may appear puzzling, but following recent findings for Sierra Leone by Bellows and Miguel (2006) and for Uganda by Blattman (2008), we hypothesize that this relationship captures a beneficial indirect effect of violence working through institutions. This is substantiated in our 2SLS regressions below. As intuitive evidence, in column (9) we document that, after controlling for institutions, there is no direct effect of any of our (historic) conflict measures on current income.[14]

Isn't this surprising in light of the fact that war implies destruction of physical and human capital as well as assets like livestock? However, rural life in Burundi has never been capital-intensive, and there is little capital to destroy (and capital assets like livestock can be replaced within a few years). Moreover, the analysis is based on a per-capita income measure, so that the relevant capital variable is capital per worker, which is ambiguously affected by conflict. And since we use conflict exposure between 1993-2003, the potential direct effects of violence on household income are further reduced.

4.3: Property rights institutions as the driver of income

[14] These findings stand in contrast to Deininger's (2003) results for northern Uganda. Deininger (2003) finds that conflict reduces investment and non-farm startups, implying a move towards subsistence. Perhaps one crucial difference between these findings and ours and those of Bellows and Miguel is that conflict did not cease during Deininger's study period—at least not in the North and West of Uganda. Deininger (2003: 590) notes "*Surprisingly, the share of households who were affected by civil strife increased during the 1992-1999 period, from 5.9% to 9.7%. This is in line with an increase in the share of communities from which such activity is reported from 38% in 1992 to 69% in 1999/2000*." A case can therefore be made that here consumption (behavior) *during* conflict—rather than post-conflict—is measured, at least in part of the study area.

We now return to the relation between institutions and income, and will tease out the causal effect of the various institutional measures on income. To this end we use a 2SLS approach, regressing income on predicted institutions as well as a number of controls. The first stages of the analyses – where institutions are regressed on our conflict variables – are identical as the results reported in columns (4)-(6) of Table 3, and not reproduced here to economize on space.

Table 4: Institutions and Income at the household level - 2SLS Results

Dependent variable: expenditure on food and non food per capita (log)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Title	1.270**	1.330***			1.368*		2.231***
	(0.588)	(0.504)			(0.754)		(0.496)
Title household level						1.234*	
						(0.724)	
Cooperation			4.029***		0.666	1.848	
			(1.549)		(2.235)	(1.974)	
Political institutions				1.809	6.102	5.949	
				(5.195)	(5.819)	(6.075)	
Density (log)		0.287*	0.133	0.081	0.204	0.197	0.423**
		(0.174)	(0.170)	(0.185)	(0.193)	(0.203)	(0.180)
Main road in community		0.105	0.133	0.166	0.500	0.483	0.602***
		(0.137)	(0.142)	(0.350)	(0.387)	(0.406)	(0.166)
Land Gini		-0.874**	-0.643	-0.634	-1.617*	-1.608*	-1.399**
		(0.444)	(0.467)	(0.739)	(0.849)	(0.893)	(0.594)
Respondent is literate		0.297***	0.236***	0.304***	0.283***	0.246***	0.339***
		(0.078)	(0.089)	(0.080)	(0.090)	(0.094)	(0.109)
Distance to market		-0.168***	-0.165***	-0.100	-0.131*	-0.153**	-0.146**
		(0.057)	(0.058)	(0.067)	(0.069)	(0.077)	(0.073)
Age		0.004	0.004	0.004	0.004	0.004	0.004
		(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)
Household head is male		0.271***	0.278***	0.223**	0.269***	0.355***	0.272**
		(0.097)	(0.103)	(0.100)	(0.102)	(0.124)	(0.131)
Land size per capita		0.097***	0.100***	0.111***	0.101***	0.088***	0.101***
		(0.027)	(0.028)	(0.029)	(0.029)	(0.034)	(0.038)
NGO intervention		-0.282**	-0.166	-0.175	-0.324**	-0.339**	-0.446***
		(0.113)	(0.110)	(0.108)	(0.137)	(0.155)	(0.144)
Constant	7.362***	6.301***	4.656***	6.789**	2.063	1.505	5.062***
	(0.479)	(1.052)	(1.574)	(3.188)	(3.545)	(3.855)	(1.050)
Observations	840	808	808	808	808	808	521
Province fixed effects	yes	Yes	yes	yes	yes	yes	no
Hansen-J stat	0.11	0.50	0.31	0.25	0.33	0.35	0.22
First stage regression Table 3 in column:	(1)	(4)	(5)	(6)	(4)	(7)	

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

The first important observation about Table 4 is that the *p*-value of the Hansen J-test is consistently not significant – our instruments are not correlated with the disturbance terms of any of the income regressions. Combined with the earlier evidence regarding the strong correlation between conflict and institutions (see Partial F-test in Table 3), we conclude that the conflict variables are appropriate instruments for our purpose.

The most important result from Table 4 is the robust and positive effect of private property rights on income. This confirms earlier cross-country evidence presented by Acemoglu *et al*. (2001). Moreover, compared to the OLS estimates the coefficient is much larger—its size has more than doubled. This outcome has been commonly observed in cross-country studies and is usually attributed to measurement error (biasing the OLS estimates towards zero -- attenuation bias). In contrast, Pande and Udry (2005) note that IV results may produce overestimates of the true effect if the included instruments are positively correlated with omitted variables that have the same sign as the endogenous institutional variables in the income regression. Our 2SLS findings suggest that increasing the share of titled land in a community by one standard deviation (35%) raises per capita expenditure for an average household in that community by some 60%[15]. However, in light of the observation by Pande and Udry we view this result as an upper bound. Regardless, our private property rights security variable "rules", and dominates all other variables when explaining income. This is evident from a comparison of the beta coefficients of the various significant variables (beta coefficients are the regression coefficients obtained after standardizing the variables so that they have a mean of zero and a standard deviation of one). For example, the beta-coefficient of the land title variable in column (5) is 0.41. This is

[15] We calculate the difference between the per capita expenditures for an average household (variables set at their mean values) and expenditures of an average household in a community with mean plus one standard deviation in title ownership.

about three times as large as the beta-coefficients of the other significant variables in the same column.[16]

In column (6) we replace our community title variable by a household title variable—a dummy measuring whether the household in question holds formal titles to its plots, or not. Not surprising we find that the positive causal effect extends to this analysis—having titles makes households better off. One may wonder whether having titles for one's own land matters for household income, or whether the broader property rights context is also relevant. In other words, who gains when community titling efforts are undertaken – are these just the households who receive formal titles or do the benefits spread more widely?

Due to multicollinearity we cannot include both the individual and community title variable in one specification, hence we adopt a different approach to analyze whether land titling generates positive externalities. We first identified all households lacking full formal titles to their land (restricting the sample to 521 households), and then assess whether the share of households with titles in the community enters significantly in an income regression for this sub-sample. The results are reported in column (7). We find strong evidence for positive externalities of land titling. Residing in a community where more land is titled raises income for households who themselves do not hold any titles. Perhaps this reflects that titling encourages more efficient management of scarce resources and invites market development more broadly. Alternatively, land titling could invite investments in the provision of an important local public good (erosion control – see Bouma *et al*. 2008), generating biophysical external benefits for other landowners. Kamungi *et al*. (2004) argue in favor of such an explanation, noting that erosion is a major problem in Burundi and that, until recently, erosion control efforts have been minimal or absent. We revisit this issue below.

[16] The remaining beta coefficients are as follows: $\beta_{\text{land-Gini}} = -0.14$, $\beta_{\text{literacy}} = 0.12$, $\beta_{\text{distance to market}} = -0.09$, $\beta_{\text{male household head}} = 0.11$, $\beta_{\text{land base per capita}} = 0.15$, and $\beta_{\text{NGO intervention}} = -0.13$.

When controlling for property rights security, our social capital and political institutions variables remain far removed from normal significance levels. Neither a greater propensity to cooperate, nor trust in the quality and intentions of local politicians, seems to matter much for income. Of course these results do not imply political leadership and social capital are unimportant for development. Rather, we believe these results define a segment of (economic) reality within which they appear ineffective—raising incomes. Of course it is possible that political leadership and social capital play different roles in local communities. For example, political leadership in rural communities in Burundi is partly responsible for allocating land resources to community members, and arguably is concerned about the distribution of income (MPDRN 2006). Equally important, it is well known that in the absence of "formal" financial markets and insurance opportunities, many poor villagers depend on informal community structures to reduce their exposure to risk (e.g. Coate and Ravallion 1993, Townsend 1994). Redistribution and insurance are not necessarily conducive to economic growth, and from this perspective the failure of political leadership and social capital to generate income growth should perhaps come as no surprise.[17]

Finally, and consistent with the earlier OLS results, we note that many control variables enter in a manner that makes intuitive sense. Literacy is correlated with higher incomes, and so is a larger per capita land base. Male-headed households generate higher incomes than female-headed households, and distance to the market enters negatively. Finally, there is some mixed evidence that the distribution of land matters (more specifically;

[17] Consider the case of social capital in more detail. Economists have long recognized that its insurance aspect may not raise incomes—that there may exist a trade-off between the level of income and its spread. There are the usual moral hazard problems associated with the provision of (mutual) insurance against contingencies. For example, Bauer and Yamey (1957: 66) argue that informal insurance networks in Africa"*…minimize the inducement for people to improve their position because they can count on being provided with the means of subsistence at a level not very different from that of the majority of their kinsmen, including the energetic, thrifty and able*." Another avenue via which culturally determined sharing norms may harm growth is via the distorting impact on investment and spending decisions. Bauer and Yamey (1957) mention sharing obligations may *"… obstruct the spreading of the banking habit since people are unwilling to have bank accounts the content of which are likely to be divulged to kinsmen. Generally, it weights the scales against [conspicuous] investment*." Compulsory contributions to the kin pool—akin to an 'extended family tax'—would discourage individuals to work hard, accumulate assets, and distinguish themselves.

greater inequality is correlated with lower income) and that NGO involvement correlates negatively with income (presumably due to non-random placement of such interventions), but these results are not robust across specifications.

4.4: Robustness analysis

How robust are these findings to alternative model specifications? We find that our main results are robust to many alterations, and summarize a number of robustness analyses in Table 5. In column (1) we repeat an earlier analysis (Table 4, column (2)), but now use data at the community – rather than the household – level, and solve a 2SLS model. This does not affect the outcome, and we again find a positive and significant causal effect of land titling on (average) income.

Table 5a: Robustness Analysis - 2SLS Results

Dependent variable: log expenditures on…	(1) Food and non food pc	(2) Food and non food pc	(3) Food and non food pc	(4) Food and non food pc	(5) Total pc	(6) Food and non food pc mountains and hills	(7) Food and non food pc plains	(8) Food and non food pc sub-sample ex. deaths due to war	(9) Food and non food pc sub-sample no assets stolen	(10) Erosion control IV-probit	(11) Access to credit IV-probit
Title	1.939**		1.467***	1.324**	0.678*	1.366**	0.987**	1.388***	1.116**	0.712**	0.153
	(0.950)		(0.502)	(0.519)	(0.374)	(0.548)	(0.412)	(0.505)	(0.460)	(0.353)	(0.413)
Tenure		4.292***									
		(1.531)									
Services			1.110								
			(0.857)								
Trust				0.006							
				(0.119)							
Density (log)	0.130	0.145	0.227	0.288*	0.063	0.563**	0.460	0.258	0.149	-0.082	0.200
	(0.230)	(0.172)	(0.178)	(0.173)	(0.137)	(0.237)	(0.360)	(0.175)	(0.196)	(0.130)	(0.174)
Main road in community	0.154	0.336*	-0.032	0.107	-0.010	0.226	-0.066	0.098	0.093	0.433***	-0.150
	(0.193)	(0.181)	(0.168)	(0.140)	(0.100)	(0.193)	(0.202)	(0.137)	(0.153)	(0.150)	(0.203)
Land Gini	-0.253	-0.508	-0.695	-0.892*	0.206	-1.598**	0.044	-0.826*	-0.853*	0.466	0.137
	(0.464)	(0.478)	(0.457)	(0.529)	(0.370)	(0.726)	(0.787)	(0.450)	(0.512)	(0.572)	(0.720)
Respondent is literate	0.308	0.253***	0.315***	0.297***	0.319***	0.325***	0.331**	0.306***	0.335***	0.029	0.246**
	(0.310)	(0.088)	(0.079)	(0.078)	(0.066)	(0.104)	(0.159)	(0.078)	(0.084)	(0.095)	(0.123)
Distance to market	-0.259***	-0.316***	-0.216***	-0.167***	-0.015	-0.283***	-0.032	-0.157***	-0.164***	0.034	0.079
	(0.099)	(0.090)	(0.065)	(0.061)	(0.046)	(0.082)	(0.086)	(0.057)	(0.062)	(0.064)	(0.088)
Age	0.019*	0.003	0.004	0.004	0.008***	0.004	0.006	0.003	0.004	0.006*	-0.000
	(0.010)	(0.003)	(0.003)	(0.003)	(0.002)	(0.004)	(0.005)	(0.003)	(0.003)	(0.003)	(0.004)
Household head is male	0.375	0.269**	0.281***	0.271***	-0.044	0.402***	-0.084	0.302***	0.221**	0.107	0.181
	(0.521)	(0.107)	(0.098)	(0.098)	(0.080)	(0.133)	(0.166)	(0.097)	(0.103)	(0.105)	(0.141)
Land size per capita	-0.257*	0.103***	0.107***	0.097***	0.131***	0.102***	0.088**	0.106***	0.108***	-0.007	0.052
	(0.151)	(0.029)	(0.026)	(0.027)	(0.021)	(0.040)	(0.044)	(0.027)	(0.027)	(0.028)	(0.032)
NGO intervention	-0.215	-0.515***	-0.250**	-0.283**	-0.065	-0.311**	-0.289	-0.274**	-0.174	0.110	0.127
	(0.140)	(0.165)	(0.117)	(0.116)	(0.084)	(0.147)	(0.197)	(0.112)	(0.117)	(0.129)	(0.163)
Constant	6.205***	5.117***	5.806***	6.193***	8.366***	5.229***	4.997**	6.379***	7.138***	-0.658	-3.029***
	(1.826)	(1.359)	(1.106)	(2.214)	(0.818)	(1.191)	(2.136)	(1.060)	(1.108)	(0.780)	(1.058)
Observations	97	808	808	808	812	465	195	795	690	811	811
Province fixed effects	yes	yes	yes	yes	yes	yes	no	yes	yes	no	no
Hansen-J stat	0.19	0.53	0.53	0.31	0.96	0.70	0.14	0.52	0.48	.	.

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 5b: First Stage Regressions of Robustness Analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable:	Title	Tenure	Services	Trust	Title	Title	Title	Title	Title
Number of attacks 1993-2003	0.017*	0.007***	0.002	0.007	0.016***	0.026***	0.024***	0.027***	0.035***
	(0.009)	(0.001)	(0.002)	(0.011)	(0.004)	(0.003)	(0.003)	(0.003)	(0.002)
Number of dead 1993-2003	0.000	0.000	-0.000***	-0.000	0.000***	0.001***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Number of wounded 1993-2003	-0.002	-0.001**	-0.000	-0.019***	0.013***	-0.007***	-0.001**	-0.001**	-0.001
	(0.002)	(0.000)	(0.001)	(0.005)	(0.002)	(0.001)	(0.001)	(0.001)	(0.001)
Fraction of orphan Headed households	0.881	-0.000	-0.938***	10.774***	-1.519***	4.229***	0.711***	0.789***	1.179***
	(0.554)	(0.101)	(0.355)	(2.172)	(0.445)	(0.520)	(0.146)	(0.177)	(0.175)
Density (log)	-0.149	-0.010	0.084***	-0.275	-0.799***	-0.197***	-0.181***	-0.201***	-0.293***
	(0.106)	(0.015)	(0.028)	(0.167)	(0.039)	(0.026)	(0.030)	(0.033)	(0.021)
Main road in community	0.027	-0.076***	0.098***	-0.269**	-0.176***	-0.287***	-0.038***	-0.045***	-0.166***
	(0.056)	(0.018)	(0.024)	(0.120)	(0.031)	(0.023)	(0.013)	(0.015)	(0.027)
Land gini	0.230	0.029	-0.187**	3.518***	0.531***	0.600***	0.371***	0.455***	0.824***
	(0.188)	(0.051)	(0.080)	(0.520)	(0.137)	(0.166)	(0.075)	(0.086)	(0.095)
Respondent is literate	0.069	0.007	-0.021	0.037	-0.024	0.033	-0.001	-0.011	-0.004
	(0.133)	(0.008)	(0.015)	(0.092)	(0.030)	(0.026)	(0.015)	(0.016)	(0.021)
Distance to market	0.033	0.048***	0.049***	-0.296***	-0.044**	0.026	0.036***	0.038***	0.047***
	(0.040)	(0.005)	(0.010)	(0.068)	(0.019)	(0.019)	(0.013)	(0.013)	(0.014)
Age	-0.004	0.000	0.000	-0.001	0.001	-0.001	-0.000	-0.000	0.000
	(0.004)	(0.000)	(0.000)	(0.003)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Respondent is male	-0.332**	-0.008	-0.004	0.057	-0.020	0.000	-0.028*	-0.030*	0.003
	(0.160)	(0.009)	(0.016)	(0.102)	(0.032)	(0.029)	(0.017)	(0.018)	(0.023)
Land size per capita	0.237	0.001	-0.010**	0.001	0.007	0.009	0.007*	0.008*	0.004
	(0.155)	(0.002)	(0.004)	(0.029)	(0.008)	(0.008)	(0.004)	(0.005)	(0.006)
NGO intervention	0.252	0.080***	-0.066***	0.405***	0.300***	0.103***	0.117***	0.131***	0.248***
	(0.216)	(0.009)	(0.018)	(0.132)	(0.036)	(0.028)	(0.021)	(0.021)	(0.023)
Constant	1.644**	0.645***	0.213	18.269***	4.674***	1.116***	1.399***	1.460***	1.467***
	(0.636)	(0.074)	(0.147)	(0.906)	(0.253)	(0.197)	(0.168)	(0.181)	(0.151)
Observations	97	812	812	812	196	468	799	694	811
Province fixed effects	yes	yes	yes	yes	yes	no	yes	yes	no
R-squared	0.68	0.37	0.25	0.37	0.73	0.42	0.68	0.68	.
Partial - F	3.65***	18.74***	8.80***	6.09***	79.71***	22.18***	35.40***	36.27***	.
First stage of Table 5a regression in column:	(1), (3), (4), (5)	(2)	(3)	(4)	(6)	(7)	(8)	(9)	(10), (11)

Robust standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

In column (2) we use our alternative variable to capture property security. Specifically, we asked households whether they were confident about their tenure security—be it formally or informally enforced. The analysis confirms the earlier insight that tenure security matters, as the coefficient enters positively and significantly. The large size of this coefficient reflects that communities in which households lack any kind of security – a small minority in our sample – have considerably lower incomes. This finding suggests some degree of substitution between formal and informal tenure arrangements, lending credence to the insight that strengthening informal tenure systems may be an alternative for formal titling efforts.

Columns (3) and (4) report the results of regressions using our alternative measures of political institutions and social capital, respectively. In column (3) we use an index that captures the range of "local public goods" that local political leaders helped to make available (as explained in section 3). Like our earlier political institutions variable, this index is not significant, strengthening our earlier hypothesis that rural communities in Burundi do not rely on local government initiatives to lift them out of poverty. Similarly, we again find that social capital – now measured via a World Value Survey style trust question, displaying greater across-household variation than cooperation – is not a significant determinant of income. The coefficient in column (4) is tiny and far from conventional significance levels.

In column (5) we replace our dependent variable by another income proxy. Rather than just including expenditures on food and non-food, the total expenditures variable is broader and also captures expenditures on durables (obviously corresponding with less frequent purchases) and auto-consumption of home-grown food. While the significance level is compromised somewhat—perhaps due to the inclusion of subsistence consumption—

the main message goes through: private property rights matter.[18] We have also estimated a variant of this model based on individual land titles (the equivalent of column 7, Table 4), and found similar results (not shown here). As before we find sizable economic effects. Specifically, individuals going from zero property rights security to full titles will double their per capita income. Further, increasing property rights security by one standard deviation for an average household raises income by 27%.

Columns (6) and (7) provide further support for our story. We consider two geographically-based subsamples of individuals. In column (6) we focus on the mountainous area in the west of Burundi (on the east bank of Lake Tanganika, comprising the majority of the population of four western provinces), and in column (7) we separately consider the central plateau areas encompassing the bulk of ten provinces (leaving out the small and spatially fragmented sample of people living on the plains in the east, south and north of the country). In both areas we find that the main results of the paper are confirmed with hardly any modification.

To further clench our result we also explore what happens when we exclude respondents who themselves suffered from violence. By construction, this implies the excludability constraint (zero correlation between our instruments and the error term of the income regression) is almost automatically satisfied. In column (8) we exclude 13 households in which a family member has died due to the conflict and in column (9) we exclude 125 households of which assets were stolen. Again, our coefficients and significance levels are not compromised.

Finally, while it is beyond the scope of this paper to explore the exact channel through which tenure security raises rural incomes in great detail, we find positive associations between title ownership and increased investments in erosion control as well as

[18] In separate regressions (not shown) we also find that our social capital variables and political institution variables are not significant when added to the specification reported in column (5).

access to credit. Specifically, when comparing the top and bottom quartile of villages in terms of land titling reveals differences in erosion control and access to credit that are significant at the 1% and 8% level, respectively. However, additional analysis suggests that enhanced incentives for erosion control are the most likely candidate explanation for our results. Preliminary IV probit estimates (regressing erosion investments and access to credit on predicted land titles and a series of controls) suggest a significant and strong causal relation running from titles to erosion control—see column (10). This is consistent with evidence presented by Deininger and Jin (2006) and Holden *et al*. (2008) for the case of Ethiopia. As evident from column (11), the available probit evidence for a causal link via access to credit appears much weaker. While we postpone a detailed analysis of the causal mechanism for future work, these findings are consistent with Pande and Urdy (2005) who report that in the bulk of the studies on the economic impact of titling, the effect of credit on income is often limited if credit markets are poorly developed.

5. Discussion and conclusions

Recent cross-country regressions have identified protection of private property rights as a key determinant of long-term growth. In the wake of this insight, the role of institutions in development has been re-discovered and re-emphasized by development economists. However, cross-country regressions are not without problems, and it is an empirical issue to what extent macro-insights from cross-country work (based on instruments derived from persistent features of institutions such as its colonial past) are useful to inform policy makers in developing countries faced with the challenge to raise local living standards. At a more mundane level, one might wonder whether these insights hold up in a local setting—where the variation in institutions is arguably less extreme than in a global sample or in the famous

sub-sample of "former colonies," and where omitted variables issues or parameter heterogeneity play less of a role.

In this paper we extend empirical work on the role of institutions in development along three dimensions. (i) We explain the determinants of income at the micro level (i.e. a focus on communities and households in rural Burundi) rather than the macro-level, and thus focus on *within*-country income differentials—complementing the earlier focus on between-country inequality; (ii) we "unbundle" the package of institutions by distinguishing between property rights security, the subjective appreciation of local political leadership, and social capital; and (iii) we propose a novel yet powerful instrument to predict local institutional quality in war-torn countries. Specifically, we demonstrate that several – but not all! – measures of the bloody conflict between Hutu and Tutsi in the 1990s is a strong determinant of local institutional quality, yet uncorrelated with the error term of the income regression.

Our "micro results" corroborate several key findings from earlier cross-country studies, and extend them as it suggests operational principles to raise rural incomes. Most significantly, property rights security is the most important determinant of income, trumping all other explanatory variables in our analysis. We find this result to be very robust, emerging in various (geographic) subsamples and for alternative specifications of tenure security and income, at both community as well as the household level. Property rights security rules. Our results suggest that both formal and informal tenure security can act as a catalyst of income growth. A preliminary analysis of our data suggests enhanced incentives for investments in land productivity and erosion control implied by greater tenure security is the most likely candidate explanation for our results.

Our results suggest that the promotion of land titling is an effective as well as a practical means to raise rural incomes, and thereby lends support for ongoing titling

programs in countries like Burundi, Rwanda and Ethiopia.[19] Our base model predicts an economically significant improvement of income when individuals are granted secure property rights.

Interestingly, our results suggest that not all institutional dimensions are equally important when it comes to alleviating poverty, and therefore qualifies the "primacy of institutions" paradigm that is emerging in the development economics profession. Specifically, we fail to uncover any evidence that causally links the quality of local political institutions to income. The same is true for our measures of social capital—it does not appear to matter for raising local incomes in rural Burundi once property security is controlled for. We believe that political leadership and social capital – and the scope for coordination to overcome social dilemmas implied by them – may usefully contribute to rural development along alternative dimensions. For example, local leaders influence the distribution of assets and income, and social capital networks play an important insurance role in societies with abundant idiosyncratic risks and limited access to formal insurance channels. However, as a means to generate income and lift rural communities out of poverty, policy makers and development agencies better turn elsewhere.

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[19] Formalization of land rights is promoted by a variety of actors, including the Commission for Legal Empowerment of the Poor, the World Bank, UN organizations and many donor countries (Holden *et al*. 2008).

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