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A MICRO LEVEL ANALYSIS OF VIOLENT CONFLICT

Grievance, Commodity Prices and Rainfall: A Village-level Analysis of Rebel Recruitment in Burundi

MICROCON Research Working Paper 11
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SIXTH FRAMEWORK PROGRAMME

MICRO CON

Grievance, Commodity Prices and Rainfall:

A Village-level Analysis of Rebel Recruitment in Burundi [1]

Eleonora Nillesen[2] and Philip Verwimp[3]

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Abstract: Grievance and reduced opportunity costs are two popular ideas within the civil war literature to explain participation in violent rebellion. We test both hypotheses at the village-level using data on recruitment activities during the civil war in Burundi. We use historical data on violent attacks in 1972 and 1988 as a proxy for grievance. The cross-sectional analyses report no effect of grievance on the likelihood of recruitment. By contrast, they do show tentative support for the idea that reduced opportunity costs may promote recruitment. Villages that had above mean incidents of 'insufficient rain' were more likely to have recruitment activities than others. We find similar results when we use recall information on recruitment to construct a 13-year panel. Negative income shocks through adverse weather conditions are a strong predictor of recruitment. By contrast we find no effect of commodity price shocks. These findings are consistent with a recent conclusion from literature: commodity price shocks show no robust relationship with civil war violence while weather shocks do.

Keywords: Civil war, recruitment, indiscriminate violence, coffee, rainfall

JEL codes: C23 N37, N47

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1. Introduction

The empirical analysis of the causes and consequences of civil war is the subject of study in a growing literature in economics and political science. This may in part be attributed to the empirical fact that since the end of the Second World War, intrastate violence is far more common than international wars. Of the 34 armed conflicts listed in the Armed Conflict Dataset for 2001, all but one were civil wars (Gleditsch et al., 2002). Despite increased scholarship on the topic the causes of civil warfare are still not well understood. The vast majority of the civil war research consists of cross-country analyses. While instructive, they can only explain part of the puzzle at best. They for example gloss over regional, household, or individual conditions by construction. That is, it cannot satisfactorily explain why some people actively participate in fighting war while the majority does not (Blattman and Miguel, 2009).

Theories about why people participate in violent rebellion date back to the classic sociological literature on agrarian revolutions (Scott, 1976; Gurr, 1971). These theories center on the idea that grievances such as frustrations about economic or political inequality induce rebellion. While some economists and political scientists subscribe to this view, alternative sets of mechanisms have been identified. For example, these include the idea that individuals who join warring factions are motivated by private gains that only accrue to those that actively participate in the fighting. Incentives may stem from wages, looting opportunities, promises of future rewards or to be protected from violence from the rebel group itself as well as other fighting faction(s) (e.g. Olson, 1965; Lichbach, 1994; Grossman, 1991; 1992; Besley and Persson 2008; Dal Bó and Dal Bó, 2008).

In recent years scholars have used regional or individual level data to empirically test these seemingly rival explanations. A prime example of the latter is the study by Humphreys and Weinstein (2008) on Sierra Leone. They test to what extent grievances, selective incentives**,** and (or) social sanctions can explain participation in both rebel groups as well as local militias (counterinsurgents who want to preserve the status quo). Others use regional or district-level data to investigate under which conditions people are motivated to pick up arms. Barron, Kaiser and Pradhan (2004), analyse local conflicts in Indonesia, and find a positive correlation between violence and unemployement, as well as violence and income inequality. Do and Iyer (2007)

examine determinants of the intensity of civil war in Nepal, and find that higher rates of poverty and lower levels of literacy prior to the outbreak of the Maoist insurgency are correlated with greater conflict intensity within the district. Dube and Vargas (2007) present evidence for Colombia on the mechanism linking the price of export commodities to violence. They show that a drop in the price of coffee substantially increased the incidence and intensity of the civil war in coffee-intensive areas, whereas an increase in the price of oil has a similar effect. They attribute this to the lowering of the opportunity cost of joining a rebel movement (via depressed wages) in the case of coffee and on increase in local state revenue in the case of oil.

Our paper is similar in spirit to studies of the latter type. We use cross-sectional and constructed panel data from a sample of Burundian villages to examine the determinants of recruitment activities during the Burundi civil war. Our contribution is twofold: (i) we use novel data from large-scale nationally representative household and community surveys to examine village-level determinants of recruitment; and (ii) we complement the cross-sectional findings with (recall-based) panel data analyses to examine whether variation in recruitment activities systematically varies with changing economic conditions. We consider variation in the producer price of coffee and negative rainfall shocks to probe into the issue of *when* the risk of rebellion is at its peak.

The paper is structured as follows: Section 2 reviews the course of the civil war in Burundi. Section 3 describes the data. Section 4 and Section 5 present the empirical results. Section 6 discusses and concludes.

2 The Burundi civil war

Burundi has until recently been involved in long and a brutal civil war that left hundreds of thousands of people dead, maimed or displaced. Massive bloodshed took place in 1972, where the armed forces slaughtered between 80.000 and 200.000 (mostly wealthy and intellectual) Hutu. In 1988, responding to a similar uprising where several thousand Tutsi were killed, “peace” was restored by the government forces killing 20.000 Hutu (HRW, 1998). Although no formal investigation into the 1988 massacres was allowed, president Buyoya sought for means to reconcile Hutu and Tutsi rather than controlling the Hutu majority by repression. Several Hutu were

appointed to government positions. Many Tutsi however viewed loss of ruling power and the resulting massacres in neighboring Rwanda as a warning sign for what would happen if they started sharing power with the Hutu. They therefore resisted Buyoya's reforms with unsuccessful coups in 1989 and 1992. Despite internal opposition, elections were held in 1993 and on July 1, Melchior Ndadaye became Burundi's first Hutu president. The newly installed president made important changes in the local administration and planned to reform the almost exclusively Tutsi army to increase ethnic and regional diversity (HRW, 1998). The attempts to reform however brutally ended with the killing of Ndadaye in October 1993.

The eruption of Hutu-led violence following the killing of the president was retaliated by massive indiscriminate violence by the government forces. HRW reports: "The army responded with clashes on Hutu making no distinction between communities involved in violence against Tutsi and those that were not. In a period of only a few weeks anywhere from 30.000 to 50.000 people were slain, roughly an equal number from each ethnic group." Uvin (1999) elaborates: "On October 21, 1993, low-level soldiers killed President Ndadaye and other dignitaries after only three months in office, with at least passive support from the highest levels of the army. Popular unrest then erupted throughout Burundi, and thousands of Tutsi were brutally killed, especially in the north and center. The army moved in to restore order, killing thousands of Hutu in the process. In total, it is estimated that 50,000 to 100,000 persons were murdered in the three months after the coup; one million fled the country; and hundreds of thousands were internally displaced." The scope and intensity of the violence was unprecedented and resulted in a gruesome civil war that lasted sixteen years, leaving the country and its citizens in ruins.

3 Data

3.1 Sample design and questionnaire

We use the 1998 and 2007 Burundi Priority Surveys including household and village level variables. The Burundi Priority Survey 1998 was implemented by the Burundi Institute of Statistics and Economic Studies (ISTEEBU) in cooperation with the World Bank. The research was undertaken in all provinces across the country to be

nationally representative and took place between October 1998 and March 1999.[4] In 2007, ISTEEBU collected the second wave of data for a subsample of the 1998 households in cooperation with a team of academics.

Household and individual data were collected foremost on socio-economic and demographic characteristics, income, consumption and assets, trust, social capital and subjective welfare measures. The community questionnaire entailed questions about infrastructure, violence, village-level clashes, and recruitment. For the community questionnaire we relied on a set of 'key experts' often the village level leader accompanied by elders.

The questionnaires were designed in French but interviews were conducted in Kirundi. We trained 65 interviewers during a one-week training during which we improved the questionnaire. The questionnaire was pilot tested in an out of sample village and final corrections were made. 50 interviewers were selected in a competitive exam that included a case study on household composition, consumption and production as well as a range of questions on research ethics. Each team of five interviewers was supervised by a team leader. Two out of five team members were women.

With the 2007 round we targeted 1.000 households out of the 1998 sample that encompassed 3908 rural households in sixteen provinces. We randomly selected a subsample of 100 out of the 390 clusters (villages or 'collines' in French). In each cluster we then interviewed all households that were part of the 1998 sample 10 per cluster). Of the initial 1.000 households we were able to re-interview 872, having an attrition rate of less than 13%. This is reasonable considering that a civil war was going on in the years between the survey rounds; causing many deaths and massive displacements and hence led to an increased chance that entire households had completely disappeared.[5]

[4] Due to on-going violence in the provinces of Makamba, Bubanza and Bujumbura rural no 'second wave' interviews were undertaken in these areas. In Makamba no interviews were held in 1998 either while in Bubanza and Bujumbura rural only a very small sample of households was interviewed then as a result of the war.

[5] Alderman et al., (2000) discusses the problem of attrition. They report attrition rates for Sub-Sahara African countries up to 50%. On the other hand, they find that coefficient estimates are not significantly affected by attrition. Since we use village-level data we do not worry about attrition here.

The high rate of successful tracing was influenced by at the least the following three factors; first, the team of researchers spent much time to train local interviewers in tracing household members who were interviewed in 1998. If a household was not found the interviewer would approach neighbors and other people from the colline to get information as detailed as possible about the household, i.e. why it moved, and where it moved to. Our interviewers often managed to obtain information on, sometimes distinctly different, reasons for moving of the household head and the remaining members respectively. Second, the country is very small and people live isolated at the collines, where everyone knows about each others' whereabouts. Third, in Burundi the pressure of land is extraordinary high (population density is the second highest on the African continent, after Rwanda). As a result people may have only have fled at the very last minute, if there was no other option, and return immediately after the violence as to ensure their claim to land. Most often, our survey team would find the households in the same location as in 1998.

3.2 Descriptive statistics

This section describes first how key dependent and independent variables were constructed. These variables include recruitment, grievance, rainfall shocks, and ethnic heterogeneity. The second part presents the sample means for the dependent and all independent variables.

The recruitment variable was based on information from our community survey. The survey question asked whether recruitment took place in the village in a given year and whether recruitment was voluntary or not.[6] Unfortunately, the survey did not ask for the number of recruits for each year. We thus define recruitment as a dummy variable taking the value of 1 if recruitment activities had taken place in the village in a given year and 0 otherwise. For grievance we rely on anecdotal evidence from Human Rights Watch reports (2003) and Lemarchand (1994) who describe the events and locations of violence in 1972 and 1988 respectively. Rainfall shocks are based on data from our household survey. The specific question read as follows (translated from French):

[6] In sharp contrast to for example recruitment during the civil war in Uganda, Sierra Leone or Sudan, recruitment in Burundi had a predominantly 'voluntary' character, meaning that people were rarely abducted or taken by physical or mental force.

Could you please tell me if your household experienced shocks of insufficient rainfall between 1998 and 2007?

The enumerator would write down the answer (yes or no) in a table that listed the years. Rainfall shocks can plausibly be regarded as covariate shocks in this context. Levels of rainfall do not vary within villages in Burundi because of their small size. We therefore aggregate the household responses up to the village level. To reduce measurement error, at least half of the sampled households within the village had to indicate 'yes' for a given shock in a given year, to be assigned 1 (occurrence). One may however argue that variation in reporting may reflect variation in activities. That is, households that have for example non-agricultural jobs may not consider 'insufficient rainfall' as a shock. They would hence not report it as a shock. If underreporting of rainfall shocks exhibits a systematic relation with for example village level wealth our results would be biased. The data however do not support this interpretation. More than 95 percent of the sample households rely on rain-fed agriculture for their income. Rainfall shocks would therefore plausibly affect (nearly) all households in the village.

Thus, S_{jt}=1 if $\sum_{i=1}^{n} s_{ijt} \geq 0.5n$, and 0 otherwise, where s_{ijt} is the response to a shock in year t by household i in village j. We subsequently sum over t to obtain the total number of rainfall shocks for a given village j in the period 1998-2005. We then create a binary variable that took the value 1 if the village had more than the sample mean incidents of 'too low rainfall' and 0 otherwise.

In the panel data analysis we use a continuous measure of regional rainfall shocks. The constructed variable here reflects the annual deviation from long-run average rainfall levels. Regional rainfall levels are measured at weather stations across the country (16 in total).

Given the important ethnic dimension of the conflict, we also want to control for ethnic heterogeneity. The Burundi government however had a very stringent policy after the war with respect to collecting ethnic identity information and the original questions had to be removed from the survey. We therefore cannot explicitly control

for ethnic composition here but use the percentage votes for Ndadaye as a proxy. The intuition is as follows: Melchior Ndadaye was the first elected Hutu president and likely to be most popular among Hutu-dominated villages. We therefore assume that a variation in percentage votes in favor of Ndadaye can plausibly resemble the ethnic mix within a village. We also partially tested for the validity of this assumption when we had new household data from 2009 at our disposal.

During our last visit to Burundi in spring 2009, we found that regulations were somewhat relaxed. As part of a new data collection effort we selected a subsample of 35 villages for new fieldwork applying economic games (see Voors et al, 2009). Household and community surveys now included a question on ethnic origin. We could thus check for 35 villages whether the percentage votes for Ndadaye indeed approximated the ethnic composition of a particular village. We found the correlation coefficient to be positive, reasonably high (0.48), and strongly significant at p=0.00. Although imperfect nonetheless, since there may have been changes in composition since the war started, it was the best alternative available. Besides, the 2009 data revealed that changes in composition were quite marginal (values for pre-war ethnic composition were based on recall so we could compare).

Table 2 presents the means and standard deviations of all variables.

4 Cross-section models

We start our empirical analysis by testing whether historical grievances can explain variation in recruitment activities between 1993 and 2005. Table 3 presents the results. Column (1) reports the unconditional correlation between grievances from violent attacks in the past and the probability of recruitment during the civil war. The coefficient has the 'wrong' sign (positive) and is marginally significant at 10%. The significance is clearly due to omitted variables; adding controls in (2) dramatically improves the models' fit and the coefficient for grievance is now insignificant. The coefficient for coffee growing regions is marginally significant suggesting that recruitment was more likely in these areas. Our proxy for Hutu-dominated villages is a strong significant predictor of recruitment. This is unsurprising given that Burundi's rebels were nearly all Hutu.

Column (3) and (4) present the results for the alternative idea that low opportunity costs drives farmers into the hands of rebel organizations.

Contrary to our grievances variable, we now find a positive and significant effect on the likelihood that this village had recruitment activities between 1993 and 2005. The positive effect remains when controls and our proxy variable for grievances (column 5) are included. The final column (6) includes the number of villagers that died as a result of violent attacks by the warring factions. Note that this represents the *total* number of people in the village that died as a result of the war. The coefficient is very small but significant. Cross-sectional estimates are however prone to omitted variables. We there do not draw any firm conclusions yet but proceed with the panel data analyses.

5 Panel data models

5.1. Commodity price shocks and recruitment

In this section we test the hypothesis that an exogenous shock in export commodity prices (coffee) is a positive predictor of recruitment.[7] Income from coffee is an important source of revenue for coffee farmers because they receive a substantial sum (20 to 30% of their monetary income) once a year that can be used to cover expenses on durable goods, house construction, health and education. This sum is then injected in the local economy with effects far beyond the coffee sector. For years however, Burundian farmers have received a bad deal from their government (see table 1). Compared to neighboring countries (e.g. Uganda and Rwanda), Burundian coffee producers have received very low prices for their produce. OCIBU (Burundi's state-run coffee agency) set the producer price annually and was in control of marketing and export. Figure 1 shows that the producer price of coffee set by the Burundian government is to some extent unrelated to the world market price of coffee.

The binary variable for recruitment activities may arguably be too coarse to reveal a possible systematic relation with changes in coffee prices. Nevertheless, in the absence of detailed recruitment data, it could still be informative. The producer price

[7] We cannot test grievances here as we did in our cross-section analyses with a (time-invariant) constructed variable based historical data on violent attacks in the 1907s and 1980s. In some of the panel specifications we however included "number of deaths" in the previous year as a measure of grievance. On a more speculative note, 'votes for Ndadaye' could proxy Hutu grievances towards the former Tutsi government.

is related to the farmer's opportunity costs of joining the rebels. We therefore expect high producer prices to be negatively related to recruitment activities. The negative effect may however extend to other households within the community, as low producer prices will distort the local (village) economy and hence spill over to other households that do not grow coffee. Simply put, farmers then have less money to spend on other products sold at the village market and hence affect other households as well.

The panel models allow us to include village and year fixed effects. This is commonly considered to be a powerful technique to mitigate concerns about omitted variable bias, giving it a distinct advantage over the cross-sectional analyses. We fit conditional logit models on our dataset. If t is relatively large, the conditional maximum likelihood estimator is consistent and asymptotically normal (Maddala, 1987). The model is formulated in terms of the underlying latent model:

$$r_{jt}^{*} = \gamma p_t * coffee_j + \beta X_j + \alpha_j + \lambda_t + \varepsilon_{jt} \quad (1)$$

$$r_{jt} = 1 \quad \text{if } r_{jt}^{*} > 0$$

$$r_{jt} = 0 \quad \text{if } r_{jt}^{*} \leq 0$$

The disadvantage of fixed-effect logit models is that villages that did not have any recruitment over the given period drop out in the estimation. The sample is thus consequently reduced to about half of the original sample. We report results with the random effects estimator as well. One reason for that is to compare the sensitivity of our results in the smaller sample to that of the full sample. The other reason is to shed more light on other village characteristics that could explain participation. The dependent variable r_{jt} is a binary variable taking the value 1 if village j had recruitment activities in year t, and 0 otherwise. Our key variable of interest is the interaction between the producer price p and a time-invariant variable $coffee_j$ that denotes coffee-growing areas. The latter has been constructed on altitude ranges that are most suitable for growing coffee (1800-2500m). X_j is a series of other control variables, α_j are *N* village-specific unknown parameters, λ_t are the year fixed effects and ε_{jt} is the error term. Naturally, the time-invariant controls X_j drop out in the fixed effects estimations.

In Table 4 we present the results for changes in coffee prices. Column (1) reports random effects estimates for our key variables of interest. Although the interaction is negative, it is not significant at conventional levels (p =0.163). The variable measuring the producer price of coffee drops out with year dummies. Its value does not vary across villages but is set by the central government and hence captured by the year fixed effects in our estimation.

Column (2) presents random effects estimates including the relevant village controls. Again, the coefficient on the interaction term has the 'right' sign, the p-value drops to less than 0.12 but the effect is not strong enough to reject the null hypothesis that the effect is 0. Perhaps striking, we also do not find any of the village level controls to be a significant predictor of recruitment, safe for a marginal positive correlation with distance to the nearest health center. Column (3) is essentially the same as (2) except that we now include the percentage of votes Ndadaye. This does not change the results.

Column (4) presents the preferred village fixed-effect specification. All time-invariant controls are now captured by the village dummies, hence we are left with our only variable of interest; the conditional effect of coffee prices on the probability of recruitment. Also, the sample is nearly cut in half, dropping villages that do not change over time. The coefficient, though still not significant, is however not much affected.

In Column (5) and (6) we check whether (past) violence is related to recruitment. The number of deaths as a result of violence was reported by the village leader for each year between 1993 and 2005. We can therefore use it in our panel estimates, although we of course realize that this variable is also based on recall.

We find that once we control for village and year fixed effects there is no significant effect of deaths on the likelihood of recruitment. This tentatively suggests that equating violence with recruitment may be inappropriate if one wants to explain recruitment. Given the low correlation between violence and recruitment we take the view that, in our context, violence does not proxy recruitment. We then subsequently

test whether an *increase* in the world market price of coffee (instead of the producer price) leads to more violence at the village level. The intuition is as follows: the producer price of coffee in Burundi hardly follows changes in world market prices. Therefore, positive rents would accrue to the government if the world market price would increase.[8] The growing government revenues would make it more attractive to fight, either to preserve the status quo (elites) or to change the status quo by overthrowing the government (rebels).[9] The result is presented in column (7). An increase in the world market price of coffee has no effect on violence.

5.2 Rainfall shocks and recruitment

Here we examine whether recruitment could also result from (negative) weather shocks, as implied by the literature (Miguel et al., 2004). We construct a 13-year panel for the years 1993-2005 using data on recruitment activities based on recall questions from our community survey. We focus on negative rainfall shocks. In Burundi, more than 95 percent of the rural population depends on rain-fed agriculture as an important source of income. Miguel et al., (2004) have shown that negative income shocks (instrumented by annual rainfall growth) increases the likelihood of civil war the next year. The results are presented in column 8.[10] We find that a negative income shock is associated with an increased likelihood of recruitment. This suggests that the opportunity-cost mechanism may indeed be at work.

6 Conclusions

The paper tests two popular hypotheses to explain recruitment activities during the Burundian civil. We use new village-level data to analyze the effects of historical grievances, reduced opportunity costs and rent-seeking incentives on the probability of recruitment activities. The cross-sectional analyses report no effect of grievance on the likelihood of recruitment.

By contrast, they do show tentative support for the idea that reduced opportunity costs may promote recruitment. Villages that had above mean incidents of 'insufficient

[8] An alternative explanation could be that grievances over the large divergence lead to conflict. This however would require farmers to have information about the world market price. This is unlikely to be the case for Burundian farmers.

[9] We assume that rents are not redistributed but only kept for private gains. The large divergence between the world market price and the price paid to Burundian producers offer some support for this assumption.

[10] The random effects models show similar results. They are not reported but available upon request.

rain' were more likely to have recruitment activities than others. We also use recall information on recruitment to construct a 13-year panel and assess whether exogenous drops in the producer price of coffee increase the probability of recruitment. The coefficient is negative as we would expect, and in most specifications close to being significant at 10 percent. This at the very minimum suggests that poverty or the opportunity costs of time indeed play a role in recruitment decisions, provided that recruitment is voluntary. By contrast, negative income shocks through adverse weather conditions are a very strong predictor of recruitment. This finding is consistent with a recent conclusion from the civil war literature: commodity price shocks show no robust relationship with civil war violence while weather shocks do.

However, we caution against a somewhat common routine to equate recruitment with violence. Including measures of violence in our regressions showed that they were not (robustly) associated with recruitment. This speaks against the idea that recruitment and violence are one and the same and impact on each other. In fact, several scholars have argued that joining a rebel group may be a rational choice to ensure protection from violence (Goodwin, 2001; Kalyvas and Kocher, 2007). We however cannot rule out the alternative explanation that our binary indicator for recruitment is simply too coarse to identify the true relation. Lastly, reduced opportunity costs may be one of the potentially many mechanisms at work, and that results do not necessarily extend beyond the context of Burundi. The majority of people will not risk their life, but some do. Individual data is needed to complement aggregate sub-or cross-national studies. Fortunately, several scholars are now taking up this challenging task.

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Figure 1 Annual development of (real) coffee prices (1990 BUF)

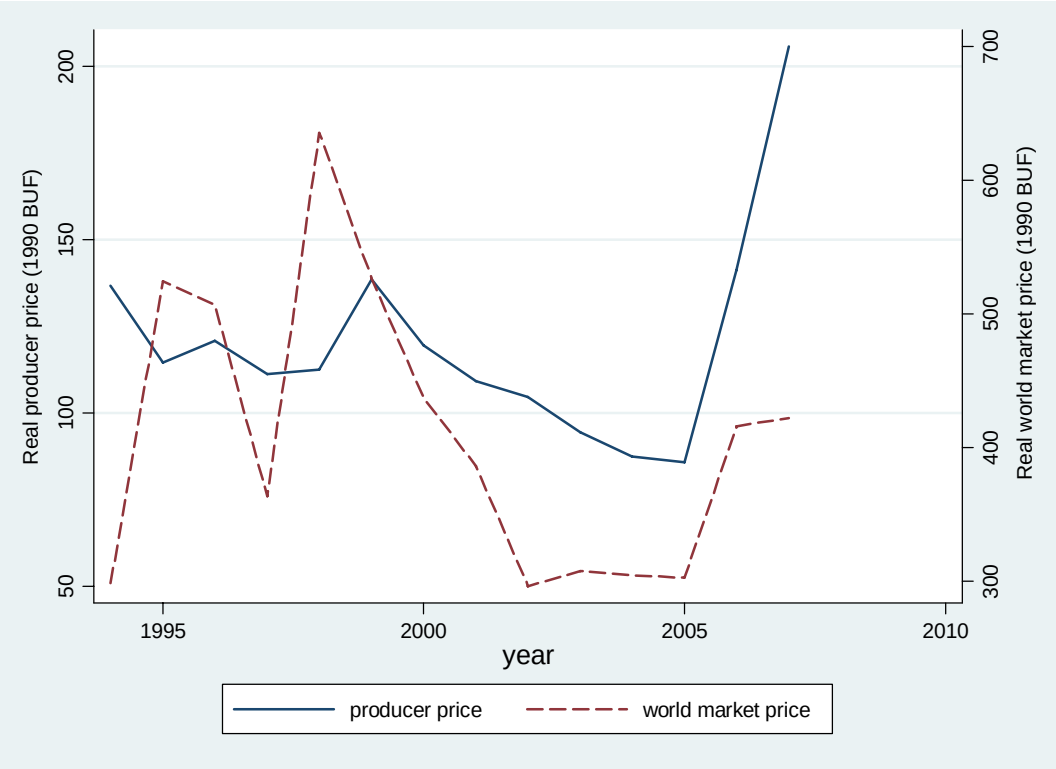

Table 1: Producer Price in Burundi and World Market Price for Arabica Coffee

Year	Nominal price	World Price in US $ per Kg	Consumer Price Index	Real Producer Price	Exchange Rate BuF-US$	Real World Price in BuF	% of World Price to Producers
1991/92	175	1,86	104,5	167,46	208,3	372,30	44,98
1992/93	175	1,40	114,6	152,71	242,8	298,49	51,16
1993/94	180	1,55	131,7	136,67	252,7	298,69	45,76
1994/95	180	3,30	157,2	114,5	249,8	524,52	21,83
1995/96	240	3,32	198,7	120,79	302,8	506,74	23,84
1996/97	290	2,68	260,6	111,28	352,4	363,57	30,61
1997/98	330	4,15	293,1	112,59	447,8	635,46	17,72
1998/99	420	2,97	303,1	138,57	536,6	526,69	26,31
99/2000	450	2,28	376,7	119,46	720,5	437,19	27,32
2000/01	450	1,91	411,8	109,28	830,4	386,27	28,29
2001/02	450	1,37	430,3	104,58	930,7	296,35	35,29
2002/03	450	1,35	476,4	94,458	1083	307,77	30,69
2003/04	450	1,41	514,5	87,464	1110	304,716	28,7
2004/05	500	1,77	583,4	85,704	998	302,84	28,3
2005/06	900	2,52	637,6	141,15	1050	416,13	33,92
2006/07	1500	3,06	729,4	205,65	1000	422,26	48,7

Source : Kimonyo and Ntiranyibagira (2007, p.9), Burundi Poverty Reduction Strategy Paper (2007) and authors own calculations.

Table 2 Descriptives

Variable	Mean	Std.dev.	Observations
***Panel A**:* Village characteristics			
Dummy for negative rainfall shocks above sample mean	0.60	0.49	100
Annual deviation from long-term average rainfall	0.01	0.28	100
% Mothers literate in village	0.34	0.18	100
log Distance to the capital (km)	4.37	0.39	100
log Population density in 1990	5.38	0.51	100
log Altitude (m)	7.37	0.75	100
Dummy for Coffee area	0.83	0.37	100
Main road in village (0/1)	0.13	0.34	100
Distance to nearest market (scale 1-7)	2.67	0.83	100
Distance to nearest drinking water facility scale (1-7)	1.62	0.61	100
Distance to health nearest health center (scale 1-7)	3.21	0.90	100
% Votes for Ndadaye	60.53	17.83	100
Panel B: Civil war variables			
Dummy for historical grievances	0.08	0.27	100
Dummy for recruitment	0.50	0.50	100
# Deaths resulting from civil war violence	56.1	114.31	100
***Panel C**:* Economic variables			
Producer price of coffee in year *t* (real 1990 Burundi Francs)	120.15	-	100
World market price of coffee in year *t* (real 1990 Burundi Francs)	409.23	-	100

Table 3 cross-section models of 'grievance and 'reduced opportunity costs' for recruitment at the village level

	Grievance	Grievance	Opportunity	Opportunity	Both	Both
Dependent: recruitment (0/1)	ML	ML	ML	ML	ML	ML
Village attacked in 1972/1988	-0.272*	-0.257			-0.240	-0.294
	(0.162)	(0.231)			(0.250)	(0.238)
Above mean drought			0.250**	0.236*	0.229*	0.298**
			(0.098)	(0.129)	(0.130)	(0.135)
Mother is literate		-0.029		-0.219	-0.133	-0.080
		(0.371)		(0.363)	(0.378)	(0.413)
Distance to capital (*log*)		0.012		-0.036	0.020	0.145
		(0.228)		(0.219)	(0.227)	(0.243)
Population (*log*)		-0.210		-0.254*	-0.233	-0.268
		(0.155)		(0.154)	(0.158)	(0.170)
Altitude (*log*)		-1.054		-0.760	-0.835	-1.019
		(0.719)		(0.747)	(0.746)	(0.790)
Coffee region		0.305*		0.236	0.263	0.387**
		(0.176)		(0.194)	(0.188)	(0.170)
Main road		-0.149		-0.226	-0.205	-0.222
		(0.206)		(0.197)	(0.201)	(0.226)
Distance to market		-0.069		-0.037	-0.047	-0.043
		(0.089)		(0.092)	(0.093)	(0.097)
Distance to drinking water facility		0.107		0.087	0.067	0.051
		(0.133)		(0.132)	(0.132)	(0.135)
Distance to health centre		-0.082		-0.088	-0.088	-0.104
		(0.081)		(0.081)	(0.082)	(0.088)
Votes for Ndadaye		0.014***		0.016***	0.015***	0.014***
		(0.005)		(0.005)	(0.005)	(0.005)
# Deaths in village						0.007**
						(0.003)
Log-likelihood	-68.18	-50.61	-66.28	-49.52	-49.13	-44.33
Pseudo-R^2	0.02	0.21	0.04	0.23	0.24	0.31
Observations	100	100	100	100	100	100

Notes: Standard errors in parentheses

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

All models are estimated using Maximum Likelihood. Marginal effects reported at mean values.

Table 4 Panel regressions for recruitment and the producer price of coffee

	(1) ML	(2) ML	(3) ML	(4) ML	(5) ML	(6) ML	(7) OLS	(8) ML
P_t*Coffee regions	-3.778	-3.926§	-3.469	-3.988§	-3.776§	-5.315§		
	(2.707)	(2.720)	(2.801)	(2.738)	(2.774)	(3.361)		
P_{wpt}* Coffee regions							3.168	
							(51.852)	
Coffee regions	18.435	19.230	17.145					
	(12.931)	(12.998)	(13.389)					
Rainfall shocks								-1.627***
								(0.483)
Population (*log*)		-0.072	-0.080					
		(0.346)	(0.339)					
Altitude (*log*)		-1.097	-0.992					
		(1.772)	(1.688)					
Distance to capital (*log*)		-0.824	-0.970*					
		(0.542)	(0.516)					
Main road		0.045	0.525					
		(0.502)	(0.516)					
Distance to market		-0.346	-0.262					
		(0.217)	(0.204)					
Distance to drinking water facility		0.333	0.303					
		(0.290)	(0.277)					
Distance to health centre		0.346*	0.239					
		(0.202)	(0.193)					
Votes for Ndadaye			0.030***					
			(0.009)					
# Deaths in village *t*					0.004			
					(0.003)			
# Deaths in village *t-1*						0.003		
						(0.003)		
Constant	-26.931***	8.969	6.758					
	(9.962)	(13.817)	(13.076)					
Village FE	No	No	No	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1188	1176	1116	660	660	572	1200	512
Log-likelihood	-412.20	-392.06	-369.58	-233.66	-232.19	-201.11		-171.26
Pseudo-R^2							0.03	0.07
χ^2	19.39***	36.06**	45.56***	40.84***	43.80***	42.42***		

Notes: Standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1 §p<0.12 Columns (1)-(3) are random effects logit models. Column (4) (5) and (6) present conditional fixed effects results. Column (7) is estimated with OLS. Dependent is 1 if recruitment activities took place in village *j* in year *t* and 0 otherwise, except in column (7) where the dependent is the # of deaths in village *j* in year *t*. The model on rainfall (column 8) includes standard errors clustered at the province level to correct for intra-group correlation.