H i C N Households in Conflict Network

Civil War and the Welfare of Extended Households: Evidence from Longitudinal Data from Burundi

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Abstract: We analyse the effect of violence and rebellion on the evolution of household welfare. We collected new panel data for Burundi (1999-2007) in which we re-interviewed original as well as newly formed households (split-offs). We use several definitions of the household as unit of analysis and test for resource pooling between parental and split-off households. Focusing on the effect of civil war, we find that village-level violence, measured as the number of battle-related deaths or wounded reduces consumption growth by 9% for every 25 casualties. Joining an armed rebel group was a lucrative livelihood strategy: households of which at least one member joined an armed group experienced 41% higher growth in welfare over the study period. Results are robust to alternative variables of civil war shocks and model specifications, including household fixed effects and initial household fixed effects.

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1   Introduction

A growing body of empirical literature uses longitudinal data to examine the impact of household specific and village level shocks on household consumption. Since the livelihoods of the majority of rural populations in poor developing countries depend on rain-fed agriculture, most studies have focused on the welfare-impact of climatic shocks. The literature, which is now matured, shows that climatic shocks have negative and persistent effects on households' welfare levels. Dercon and Krishnan (2000) examine the evolution of welfare among a panel of Ethiopian households and find that rural households' welfare levels are affected by climatic shocks and livestock diseases, testifying for the importance of uninsured risk in their setting. In another study on Ethiopia, Dercon, Hoddinott and Woldehanna (2005) find that climatic shocks and severe illness adversely impact household consumption levels. The lack of insurance against adverse shocks may set a poverty trap in itself because social, human and physical capital may be lost reducing access to profitable opportunities. Such losses add to the ex-ante behavioural response in a risky environment, to wit the avoidance of risky but high-yielding economic activities for the sake of a lower but more secure return.

Several empirical studies (Rosenzweig and Binswanger (1993), Alderman et al (2001), Hoddinott and Kinsey (2001)) find evidence consistent with persistent effects of temporary events such as drought. Using the same Ethiopian data to study the effects of rainfall shocks on household consumption growth, Dercon (2004) finds that consumption levels are not only impacted by recent rainfall shocks, but also by such shocks occurring several years (four to five years) before. Moreover, he finds evidence that a severe famine that happened a *decade* before still negatively influences consumption growth of households that were affected by it.

Micro-level research on the welfare impact of civil war in general and violence in particular is much scarcer. On the one hand this is surprising given the pervasiveness of civil war and violence in Africa in the past two decades, on the other hand it is not surprising given the difficulties of data collection. Deininger and Okidi (1999) examine the evolution of welfare in Uganda and find a strong negative impact of initial civil strife on subsequent consumption growth. In another study on Uganda, Deininger (2003) finds that presence of civil strife at the community level reduces subsequent household level investment. Rural households that experienced civil strife were also less likely to start a small non-farm enterprise or, if they already had a small enterprise, were more likely to shut it down.

Deininger interprets the results by arguing that households who have been confronted with civil strife refrain from productive investments since these are too risky and vulnerable to war-related destruction. Bundervoet et al (2008), using variation in fighting over time and space in Burundi's civil war, show that children who are exposed to the civil war are smaller compared to those who are not exposed.

Cross-country time-series research on civil war is more readily available. It shows that long and bloody civil wars are usually followed by a period (of about five years) of sharp increases in prosperity and growth, commonly called the peace dividend (Collier, 1999; Collier et al, 2003). While this can be explained by the macro-economic recovery of a country returning to its pre-conflict growth path, a recent literature is emerging that suggests a micro-level mechanism for such growth. Work by Blattman (2008) in Uganda and Bellows and Miguel (2006) in Sierra Leone suggests that political participation is greater in areas that have experienced violence, and that community networks are more extensive there. Stronger social capital after war may offer a new foundation for growth and development.

In this paper, we study the evolution of household welfare in Burundi between 1999 and 2007. We focus on the role of the civil war as a covariate village level shock and on the decision to join an armed rebel group as an idiosyncratic event in explaining household consumption paths. We use household private consumption per month per adult equivalent as a measure of welfare in a reduced-form econometric approach. Thanks to the panel nature of our data, we can study the determinants that explain welfare dynamics over the period under consideration. We find that village level violence, in terms of number of people wounded and killed substantially decreases consumption growth. Membership of rebel groups on the other hand pays-off.

Our paper contributes to the debate on the welfare impact of violence and civil war in Africa in five ways. *First*, we show that initial households can be tracked and re-interviewed in a very poor country even after a prolonged period of civil war. We show that the civil war did not affect attrition in our sample. *Second*, we account for the split-off households in our consumption measure. These are members of the original households in the first round of the survey who in the meantime started their own household. Most panel data studies, in particular in Africa, do not track nor interview newly formed households in between the two survey rounds.[1] Using the first panel data survey collected in rural Burundi through the Burundi Priority Household Survey (BPHS) we show that neglecting to account for split-off

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[1] We refer to Foster and Rosenzweig (2002) for a detailed argument in favor of tracking and interviewing newly formed households between two survey rounds in a panel.

households in panel data studies may bias poverty results. We provide evidence for the direction and magnitude of the bias. *Third*, we use different definitions of the household, depending on the question of autonomy and resource pooling between parental and split-off households. We show how our results for the civil war effects depend on the definition used and we test for resource pooling between the split-off and the parental household. As for as we know such test has not yet been performed with Burundese data. *Fourth*, we use detailed community measures of the intensity of the war as indicators of violence next to subjective appraisals. In the Deininger and Okidi (1999) study for example, the violence indicator is a first-period dummy variable that takes on the value one if the household reported to have been affected by the civil war. We use much more fine-grained measures of violence. And *fifth*, we present results on the welfare impact of war-related violence and rebellion. Despite the salience of civil war in Africa in the past two decades, we are not aware of other studies using a household panel to study the impact of war on consumption growth. In the case of Burundi, we present the first analysis of changes in welfare over time which includes both split-off households as well as measures of the intensity of violence in the civil war.

Our findings are three-fold. <u>First</u>, we show that the inclusion of the newly formed households in our analysis of welfare changes over time decreases headcount poverty by 3.5% instead of 1%. This sheds light on the extent of the potential bias in studies which do not interview split-off households. While split-offs on average have higher welfare compared to their parental household, the magnitude of the bias remains relatively small for two reasons: (i) not all households (in our sample 1/3) have split-offs and; (ii) parental households with a split-off have experienced increases in consumption growth themselves after the departure of the split-off. <u>Second</u>, we find that 25 war-related deaths or wounded at the village level reduce consumption growth by 9%. However difficult a peace settlement may be to achieve, the policy relevant prescription of our finding is clear: failing to find a settlement has direct negative implications for household welfare. <u>Third</u>, we find that membership of rebel groups substantially increases household welfare, by 41% compared to non-member households. War thus has winners and losers, which we are able to profile with our data. Our results are robust for different household and initial household fixed effects specifications.

The paper is structured as follows. In section 2, we describe the civil war in Burundi and the data we use in this study. We analyse drop out during the second wave of the survey and potential selection bias. Section 3 offers descriptive statistics on the evolution of welfare in our sample and details the calculation of the welfare measure and the procedure we used to account for the welfare of the split-off households. In this section, we also show difference-in-

differences evidence on the effect of violence and rebellion on welfare dynamics. Section 4 starts with the econometric models that we estimate and continuous with the presentation of our main empirical results. We discuss potential biases resulting from endogeneity. This section also includes a test for resource pooling and thus for the 'correct' definition of the household as unit of analysis. We end the section with robustness checks using alternative measures for the intensity of civil war. Section 5 concludes.

2   Setting the stage

2.1 Burundi's Civil War

The latest episode of civil war in Burundi began in October 1993, when the first democratically elected president – and for the first time a Hutu president – was assassinated by paratroopers from the Tutsi-dominated army in a failed coup d'etat. This was followed by large-scale massacres in the countryside, with peasant-supporters of the president killing Tutsis and *UPRONA*-Hutus, and the army killing all Hutus in sight in an operation to 'restore order'[2]. In a matter of days, 100,000 people lost their lives in what the UN calls a genocide (UN, 1996). The massacres were followed by the spread of violence and warfare throughout the country, with several Hutu rebel factions opposing the regular government (Tutsi) army. This marked the beginning of one of the most brutal and bloody civil wars in recent history (Uvin, 1999)

In August 2000, some minor rebel groups signed the Arusha peace agreements with the still Tutsi dominated Burundian government. This had little effect on the security situation in the field since the two major rebel groups, CNDD-FDD and FNL, were not involved in the peace talks. In 2003, the new president (Hutu) announced a one-sided cease fire and allowed the largest rebel group CNDD-FDD to descend from the hills and march victoriously on Bujumbura. Rebel leader Nkurunziza was incorporated in the government and rebel combatants were integrated in army and police forces. The intensity of the civil war decreased dramatically and in 2005 Nkurunziza was elected as the new president. One rebel group (FNL) remained outside the peace process and continued murdering and pillaging, as a result of which pockets of insecurity still exists throughout the country.

Human rights organizations describe the Burundian war as a war against civilians (Human Rights Watch, 1998; 2003). Civilians were widely used as proxy targets, with both

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[2] UPRONA-Hutus are Hutus loyal to the Tutsi-dominated political party UPRONA, and are therefore seen as traitors to the Hutu cause.

sides (rebel groups and the regular army) targeting civilians deemed supportive of the other group. Direct battles between the army and the rebel forces were relatively rare despite the duration of the war. Both sides of the conflict engaged in massive looting of civilian property and massive human rights violations. The forced regroupment of the rural Hutu population between 1996 and 2001 was associated with a lot of brutality by the regular army, resulting in thousands of deaths and rapes (Human Rights Watch, 2000). According to a UNFPA (2002) survey, 50% of the rural population has been displaced at least once during the war. Between the onset of the war in 1993 and 2002, 250,000 people were killed (UNFPA, 2002). Given the low-intensity of the war after 2002, the widely claimed mortality figure of 300,000 deaths between 1993 and 2007 seems plausible. These figures only concern the persons who were directly killed in the violence. The indirect mortality resulting from the breakdown of the economy and the health-care system is bound to be a lot higher.

The war had devastating effects on the economy. Income per capita halved from USD 162.7 in 1993 to USD 82.6 in 2003. Rural poverty headcount increased from 39.6% in 1993 to just over 70% in 2003, making Burundi the world's poorest country (IMF, 2007). Social indicators also worsened: life expectancy fell from 51.1 in 1993 to 46.3 in 2003, and the prevalence of undernourishment increased with 20 percentage points (67% in 2003). Although real GDP per capita in Burundi increased since the ceasing of major hostilities (in 2003), it has done so at a moderate rate of per 1.6% per annum (IMF, 2008). Moreover, economic growth in the post-war period was fuelled almost entirely by the reconstruction of major infrastructure that was destroyed during the war. Growth in agriculture, which provides the livelihood for over 90% of the population in Burundi, has faltered. In 2005, agricultural growth amounted to -6.6%. The peace dividend seems to be absent.[3]

2.2 The Burundi Household Priority Survey 1998-2007

The data we use in this paper consist of a nine-year panel with two data points, 1998 and 2007. In 1998, the World Bank and the Burundi Institute of Statistics and Economic Studies (BISES) conducted a nationally representative general-purpose household survey to analyse living standards. For this survey, 3908 rural households were interviewed (Republic of Burundi, 1998). We designed the 2007 Priority Survey (henceforth PS07) as a follow-up to the 1998 Priority Survey (henceforth PS98). Due to budget limitations, it was impossible to

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[3] For an analysis of the longer-term effects of Burundi's civil war, we refer to Voors, M.J. and E.H. Bulte. (2008). Unbundling Institutions at the Micro Level: Conflict, Institutions and Income in Burundi, Wageningen University, HiCN Working Paper 49

try to track and re-survey all 3908 rural households (391 survey sites) included in the PS98. Therefore, we decided to randomly draw 100 of the 391 baseline sites with the purpose to track and re-survey all 1000 original (1998) rural households in these sites.[4] 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 5 interviewers was supervised by a team leader. Two out of five team members were women. The interviewers were instructed to track and re-interview, within each hill, the 10 original households.

Besides tracking and re-surveying 1000 original households, we decided also to track and interview the new households that have been formed between 1998 and 2007 by members of the selected 1000 original households[5]. For instance, if a son or daughter of a household that was interviewed in 1998 got married in the meantime and constituted his/her own household (between the two waves of the survey), this new household was tracked and surveyed. Overall we managed to locate and re-interview 874 of the 1000 selected household and trace and interview 534 splits. In sum, we interviewed 1408 households during the 2007 survey. The supervisor of each team of interviewers undertook a village level community survey in which (s)he asked questions on infrastructure, history, population, attacks and war-related violence.

2.3 An analysis of attrition in the sample

In the 2007 survey, we managed to track and re-interview 87.4% of the original households and 82% of all individuals interviewed in 1998. These are reasonable figures after a period of 9 years in between the survey rounds, a period characterised by civil war. Well-known panels in developing countries such as the Kenya Life Panel Survey 1998-2003/2005 (84%), the Indonesian Family Life Survey 1993,1998, 2000 (88%); and the KwaZulu-Natal Income Dynamics Study 1993-1998 (84%) have similar attrition rates. Baird, Hamory and Miguel (2008) signal the lower re-contact rates for panels who do not track individual members who moved between survey rounds.

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[4] To choose which hills to revisit and to end up with the same ratio of selected to unselected sites in each province as in the 1998 survey, we listed, per province, all hills surveyed in 1998 and picked each fourth hill until we selected 100 hills. In the 1998 survey, 10 households were chosen randomly in each survey site. The survey site was the hill (*sous colline*), the smallest administrative unit.
[5] These are so-called "split-off" households.

For 113 of the 126 of the drop out households we were able to find out the reason why they dropped out, by asking neighbours and village authorities about their whereabouts. The drop outs in the second round can be divided into four groups: (1) all original household members deceased (24.8%). This mostly concerns small households with old members, (2) the household moved to another place, but we were unable to find where (22.1%), (3) the household disappeared all of a sudden and nobody knows its current whereabouts (21.2%) or (4) the household dissolved (31.9%), mostly parents deceased and children went their own way.

This kind of drop out is not different from drop out in other panel surveys. Important is to find out whether or not the drop out households differ from the tracked households, which would bias subsequent estimation results. In Appendix 1 we explore potential selection bias in two ways. First, we examine mean differences on observables between tracked and drop out households and second we perform a probit analysis of attrition similar to Fitzgerald, Gottschalk, and Moffit (1998). We find that drop out households were on average smaller, were more likely to have an older, female or less educated head of the household and had lower total income and less cattle. In terms of poverty, measured as consumption per adult equivalent, the drop outs do not differ from the tracked households. We do not observe a difference for the intensity of violence, measured as the number of deaths and wounded at the village level. The drop out households did have a slightly worse access to markets in 1998.

The fact that the violence in the civil war did not influence or, worse jeopardize our tracking of households is an important finding in itself. It means that panel surveys can be conducted safely and with low drop-out rates in a war-affected country, with selection effects not different from those in other panel surveys and with no selection, at least not in our Burundi sample, on violence in civil war. The results of the FGM-method imply that we use Heckman's method to correct for sample selectivity in subsequent analysis using the variables that are statistically significant in explaining attrition.

3   Household Welfare in Rural Burundi, 1999-2007

3.1   Welfare Evolution of the Original Households

Table 1 shows the 2007 consumption expenditures per adult equivalent of the 872 original households with complete consumption data who were interviewed in both rounds. The average consumption per adult equivalent of the 872 panel households amounted to BIF

16,789.85 per month during the 2007 survey.[6] This translates in USD 14.47 using the October 2007 official exchange rate. We find considerable differences across provinces: households in Bururi had an average consumption level that exceeded three times the consumption of households in Ruyigi. As the figures in the first column of Table 1 are not comparable with the 1998 consumption levels in column two, the third column expresses the 2007 consumption levels in constant 1998 prices. Average consumption per adult equivalent of the panel households increased by over 13 percent between 1998 and 2007 (from BIF 7,084 in 1998 to BIF 8,033 in 2007). However, the 2007 average consumption level of BIF 8,033.42 (in 1998 prices) still falls short of the poverty line (BIF 8,173.15). A salient feature of Table 1 is that despite the 13% increase in average consumption, the incidence of poverty decreased by only one percentage point. While in some provinces poverty levels among the panel households declined drastically (for instance in Bururi and Muyinga), panel households in other provinces have been confronted with a substantial increase in poverty between 1998 and 2007 (for instance Kirundo, Ngozi and Muramvya). Higher average consumption levels do not necessarily translate into lower poverty levels: although on average the panel households in Cankuzo have increased their consumption levels by almost 31%, poverty incidence has *increased* with over two percentage points. This suggests a greater inequality in the distribution of consumption.

Figure 1 shows the change in the distribution of consumption levels between 1998 and 2007. While there are more households with high or very high consumption levels in 2007 than in 1998, there are also more households that have to manage with lower levels of consumption. The mode of the distribution is lower in 2007 than in 1998. The overall picture is one of a 13% increase in average consumption levels but a 16% *decrease* in the median consumption level: while the median consumption level of the 872 panel households amounted to BIF 6,119.89 in 1998, it fell to BIF 5,151.16 in 2007. The pattern in Figure 1 is mirrored by the evolution of the Gini-coefficient of consumption per adult equivalent: in 1998, the Gini-coefficient among the panel households amounted to *0.33*. In 2007, this had risen to *0.44*. This confirms the sharp increase in inequality of the 1998-2007 period. Overall, 54.1% of panel households had a lower consumption level in 2007 than in 1998. The shift in the distribution of consumption levels translates in a rising incidence of extreme poverty or food poverty. While the overall poverty headcount dropped one percentage point between 1998 and 2007 (see Table 1), the incidence of extreme poverty increased from 56.0% in 1998

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[6] We had to drop 2 households from the analysis because of large inconsistencies in the data.

to 59.2% in 2007. The poor households in 2007 are worse off in terms of consumption than the poor households in 1998, with an average consumption level of BIF 3,682.54 in 2007 and BIF 4,570.64 in 1998. In contrast, the mean consumption expenditure of the non-poor in 2007 (BIF 17,184.21) is much higher than that of the non-poor in 1998 (BIF 12,569.69).

There is however an important caveat to this, the poor and non-poor are not necessarily the same in both years. As Table 2 shows, economic mobility among the panel households is considerable. 35% of household switched from one economic category (poor or non-poor) to another between 1998 and 2007. Nevertheless, poverty is persistent: 74% of households that were poor in 1998 still are in 2007. In contrast, prosperity is less persistent, with over half of households (54.4%) that were non-poor in 1998 falling into poverty by 2007.

3.2 Accounting for Split-off Households

Most studies that use panel data to examine welfare and poverty dynamics do not take account of the new households that are formed between the survey rounds by members of an original (first period) household. If the new households (split-off households) are systematically poorer or richer than their original household, a comparison between the original household's welfare level in period 1 and period 2 will over- or underestimate the second-period welfare of the original household.[7] To see this, imagine a household that consisted of four members in 1998. Consumption per adult equivalent of the household was, say, BIF 6000. Between 1998 and 2007, the two sons of the household got married and constituted their own households. Meanwhile, the mother and father of the original household adopted a war orphan. Consumption per adult equivalent in 2007 of the original household, which now consists of three members, amounts to BIF 5000 (in 1998 prices). Naïve comparison of the two consumption figures suggests that the original household became poorer between 1998 and 2007. However, imagine that the new households of the two sons who got married have expenditures per adult equivalent of BIF 10,000 and BIF 7000 BIF. When *comparing* the level of welfare of the original household over time, one needs to take into account the current (2007) welfare level of *all* individuals who were member of the household in 1998. Proceeding in this way, the welfare level of the *reconstituted* original

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[7] While we study the welfare of split-off households, the formation process of new households as such is not the topic of this paper.

household in 2007 is calculated as $\frac{2}{4}*5000+\frac{1}{4}*10000+\frac{1}{4}*7000=6750$ BIF per adult equivalent. Using this method, the original household is *better-off* in 2007 than in 1998.

To capture this in a stylized fashion, consider a simple two-period model where HM_{1ij} denotes the number of household members in household i in period 1. J is the index for the household's status and ranges from 0 to N, with N being the number of new households that are formed between period 1 and period 2 by members of the original household i. Note that j=0 is the index for the original household. Let HM_{2ij} be the number of original household members in the j-th split-off of household i (j=0,…,N) in period 2, so that $\sum_{j=0}^{N} HM_{2ij} + number\_of\_deaths = HM_{1ij}$. Finally, C_{2ij} denotes the consumption expenditure per adult equivalent in period 2 of household j (j=0,…,N). The welfare measure of the original household in time period 2 is calculated as

$$W_{2i0} = \sum_{j=0}^{N} \frac{HM_{2ij}}{HM_{1i0} - Number\_of\_Deaths} C_{2ij} \qquad (1)$$

When we include all original households in the analysis irrespective of having an interviewed split-off (N=872), we find that the mean consumption level in 2007 of the *reconstituted* households, calculated according to equation (1), does not differ with (BIF 16,771.69) or without (BIF 16,789.85) the splits. However, the median consumption level increases with 3.6% (BIF 5,334.95 (in 1998 prices) with split-offs vs. BIF 5,151.16 without split-offs. Poverty headcount in 2007 is 65.4% for the reconstituted households compared to 67.8% when splits are not taken into account. The incidence of extreme poverty also lowers (59.2% without split-offs vs. 56.9% with split-offs) but remains higher than in 1998.

3.3 Violence, Rebellion and Welfare: Descriptive Statistics

Our description and subsequent analysis focuses on the impact of two specific events on household welfare: the impact of village-level violence and the impact of joining an armed rebel group. The former is defined as the number of battle-related deaths and wounded in the village between 1999 and 2007. The latter variable takes on 1 if at least one member of the household joined an armed rebel group between the two survey rounds.

The first panel of Table 3 shows average household consumption expenditures in 1999 and 2007 by the incidence of village-level violence between 1999 and 2007. The first important observation is that the villages that were affected by violence and the villages that were not affected do not differ in terms of initial (1999) average household welfare (7159.8 BIF for households in villages not exposed to violence and 6725.5 BIF for households exposed to the violence). This is reassuring in terms of a potential endogeneity bias: if villages that were exposed to the violence between 1999 and 2007 were poorer to begin with, any effect we would find of violence on household welfare might be due to this selection rather than to a genuine welfare effect of violence. The descriptives in Table 3 offer a first hint that this is not likely to be a problem in our data. We will test this later formally in Table 7. The figures in Table 3 further suggest a welfare decreasing effect of village-level violence: While average household consumption in the villages without violence *increased* by 19 percent between 1999 and 2007 (from 7159.8 BIF to 8511.2 BIF), consumption in villages exposed to violence *decreased* by 14 percent (from 6725.5 BIF to 5770.2 BIF). The difference-in-differences estimator is substantial (2306.8) and statistically significant at the one percent level: The welfare evolution in the war villages was substantially different than that in the non-war villages.

The second panel of Table 3 shows the evolution of household welfare for "rebel households" (defined as a household of which at least one member voluntarily joined an armed rebel group between 1999 and 2007) and other households. Again, the first important observation is that "rebel households" and other households do not differ in terms of initial (1999) welfare levels: Joining a rebellion does not seem to be the prerogative of poorer households (the difference in initial average welfare between rebel and non-rebel households is only *19.1* BIF). Again, we test this formally in Table 7. The second important observation is the substantial welfare gain of rebel households between 1999 and 2007: Average consumption of rebel households increased from *7102.7* BIF in 1999 to *10919.5* BIF in 2007, an increase of more than 50 percent. For non-rebel households, the welfare increase was only 12 percent (from 7083.6 BIF to 7962.2 BIF). However, based on this observation we cannot claim that rebel households had a higher welfare growth than non-rebel households: Although the size of the difference-in-differences estimator is substantial (2938.2), there is a lack of precision in the estimate, mainly due to the small sample of rebel households (only 19 households in our data had one or more members who joined the rebellion).

4 Violent Shocks and the Evolution of Welfare

4.1 The Econometric Models

In this section we examine the impact of war-related violence on the evolution of households' welfare levels**.** Panel households have been confronted with various types of shocks between the two survey rounds. To examine whether and to what extent these shocks have affected consumption growth, we estimate standard micro-level growth models augmented for the presence of specific violence-induced shocks during the 1998-2007 period. The dependent variable in the analysis is the change in real consumption expenditure per adult equivalent (expressed in BIF 1998) between 1998 and 2007. The first model explains consumption growth between 1998 and 2007 based on initial household characteristics and shocks that happened between 1998 and 2007. This model can be written as:

$$\ln(\exp\_ad)_{ij,2007} - \ln(\exp\_ad)_{ij,1998} = \ln(\exp\_ad)_{ij,1998} + \alpha.H_{ij,1998} + \beta.S_{ij,98-07} + \eta + \varepsilon_{ij,2007} \qquad (2)$$

With $\ln(\exp\_ad)_{ij,1998}$ being the natural log of the expenditures per adult equivalent for household i in village j in 1998, $H_{ij,1998}$ being initial household characteristics of household i in village j, $S_{ij,98-07}$ the violent shocks that happened to household i in village j between 1998 and 2007 and η are the province fixed effects and $\varepsilon_{ij,2007}$ is the white noise error term. Initial household characteristics or the household level control variables ($H_{ij,1998}$) are the size of the household, the age and the sex of the head of the household and the proportion of educated household members. The shocks ($S_{ij,98-07}$) include the number of death and wounded at the village level between 1998 and 2007 as a result of civil war confrontations between the army and one of the rebel factions. We also include a second war-related variable, to wit whether or not the household has at least one member who joined the rebellion between 1998 and 2007.[8] While we expect that the number of death and wounded has a negative impact on consumption growth, this is not necessarily the case for the membership of a rebel faction. In effect, the integration in the government of former rebel leader Pierre Nkurunziza in 2003 and the integration of his troops in police and army forces (and in thriving private security companies) meant the accession to a steady and relatively high-paying job for most former rebel fighters. The remaining combatants who had to be demobilized received considerable

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[8] We recognize that this behavior is the result of choice (at least to a large degree) and cannot be considered as a shock. We will address potential sources of endogeneity further on.

reinsertion payments in the framework of the World Bank's Multi-Country Demobilization and Reintegration Program (MDRP)[9]. These jobs and payments may have a positive effect on consumption growth of the rebel's household.

The results produced by econometric model (2) may be suspect if there are unobservable factors that simultaneously influence the dependent variable and the right-hand-side variables. In this case, the relation between the right-hand-side variables and consumption growth is spurious. Panel data offer a powerful tool to remove this omitted variable bias. This is known as fixed-effects regression (or the within-estimator) and exploits *only* the within household variation over time (the variation across households is not used to estimate the regression coefficients). First differencing the variables sweeps out all time-invariant effects, both observable as well as the unobservable:

$$\ln(\exp\_ad)_{ij,2007} - \ln(\exp\_ad)_{ij,1998} = \alpha.(H_{ij,2007} - H_{ij,1998}) + \beta.(S_{ij,98-07}) + (\varepsilon_{ij,2007} - \varepsilon_{ij,1998}) \qquad (3)$$

H_{ij} is a vector of household controls used in equation (2) that can change over time. $S_{ij,98-07}$ is the vector of shocks between 1999 and 2007 and contains the variables used in the previous analyses. Equation (3) resolves a number of potential sources of endogeneity such as ability or risk aversion, but it does not address concerns about heterogeneity among households affecting consumption growth and choices made during the civil war, such as joining a rebel movement.

Next to first-differencing we use three other avenues to address potential endogeneity. (i) we include household level control variables which may affect consumption growth as well as joining a rebel movement; (ii) we test with a Probit model whether or not consumption, intensity of civil war at the village level together with household level control variables predict membership of a rebel group and; (iii) we use an *initial* household fixed effect specification. Points (i) and (ii) will be further discussed in section (4.2) below.

In Equation (4) we implement point (iii) by adding an initial household fixed effect θ_{ij} to the specification. The difference between an (old and new) household fixed effect and an *initial* household fixed effect is that the latter controls for the fact that the split-off households originate from the old household. Using data from multiple households originating from the same initial household allows us to control for any initial household heterogeneity θ_{ij} that may

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[9] This reinsertion payment amounted to at least 556,000 BIF and increases with rank. Anyone above the rank of corporal received 600,000 BIF on minimum (Uvin, 2007).

affect consumption growth. As a result, the effect of the civil war variables is identified using variation *within* initial households.

$$\ln(\exp\_ad)_{ij,2007} - \ln(\exp\_ad)_{ij,1998} = \alpha.(H_{ij,2007} - H_{ij,1998}) + \beta.(S_{ij,98-07}) + \theta_{ij} + (\varepsilon_{ij,2007} - \varepsilon_{ij,1998}) \qquad (4)$$

Estimating equation (4) only makes sense when split-off and original households do not pool resources. When they do, then there is in fact only one household and we cannot use the data from multiple households to identify the within household variation. Therefore we need to test whether or not original and split-off households pool resources. We test resource pooling using the methods developed in Altonji et al (1992) and Witoelar (2005). These authors use the following model for panel data:

$$\ln(\exp\_ad)_{ij,2007} - \ln(\exp\_ad)_{ij,1998} = \alpha.(H_{ij,2007} - H_{ij,1998}) + \beta.(Y_{ij,2007} - Y_{ij,1998}) + \theta_{ij} + (u_{ij,2007} - u_{ij,1998}) \qquad (5)$$

Where Y_{ij} is the own income of the household i in village j. The reasoning behind the model is as follows[10]: When the parental and the split-off household pool resources, then we can regard household decision making as a unitary actor (representing the extended household) constrained by the pooled income. Hence, the marginal utility of income is the same for all household members. This marginal utility can be interpreted as the initial household fixed effect θ_{ij}. Controlling for this fixed effect, the own income of the parental and split-off household (Y_{ij}) should not affect their consumption. Under the null-hypothesis of resource pooling (also called extended-family altruism in the literature), the coefficient of Y_{ij} should be zero. Meaning that after controlling for the own household characteristics and the initial household fixed effects, household's *own* income should not affect its consumption. Or, in dynamic form of the model, changes in household *own* income should not affect changes in household consumption.

There may be omitted variables, ending up in the error term that correlate with income in equation (5). One way to deal with this is to use IV estimation. This would also deal with potential measurement error in the income variable. In an agricultural economy such as Burundi, the capital stock allowing the farm household to produce revenue seems to be an adequate instrument for income. Indeed, as we only performed our survey in the rural areas (we did not trace nor re-interview the households who lived in the capital Bujumbura in 1998)

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[10] For a formal derivation we refer to Altonij et al (1992) and Witoelar (2005)

and as even city dwellers with on full-time off-farm job (5% of the population) have a farm, the agricultural capital stock seems to be a good candidate to serve as instrument. We have two variables in our data capturing farm capital, to wit the size of the household's land and the number of cattle. Land is a necessary input to produce crops, and cattle delivers manure and traction for cultivation. Both determine the volume of crop output and the latter constitutes the most important part of household income. Farm size as well as cattle are thus likely to affect consumption only through the income channel. In contrast to *products from livestock* such as milk or – very occasionally – meat (both responsible for a small part of farm household income in Burundi), cattle are *not* considered part of income. It is a capital stock variable. We do not include smaller animals such goats, chicken and pigs in our instrument as these are likely to have a direct effect on consumption.

We use changes in the capital stock over time to instrument changes in income over time. This strategy may potentially induce additional endogeneity into the model: households with high income may purchase more land or cattle allowing them to further increase their income. Therefore, we worked out a way to purge all income-induced acquisitions of land and cattle between 1998 and 2007. Our data allow us to know exactly in what way these two capital stock elements were acquired in the 1998-2007 period. Two important features reduce the endogeneity of our instruments. First, we know the date the farmer started cultivating each plot: 91% of all land and cattle transactions occurred prior to 2007, and thus cannot be the result of income in 2007. Second, for land, our questionnaire allows us to distinguish - for each plot of the farm - between inheritance, gift, purchase, lending, state distribution and temporal occupation as means of acquisition. 70% of all plots were acquired through inheritance, while 16% was purchased. For cattle, we distinguish between natural growth of the herd (birth and death), theft, purchase/sales and gift. Purchases and sales account for 25% of all cattle transactions in our data. Since we have all land and all cattle transactions for each year between 1998 and 2007 we are able to leave out all *purchases* of land and all *purchases and sales* of cattle in order to purge our instruments as much as possible from all income-induced changes. We thus arrive at two capital stock variables, land and cattle, which determine income in 2007 through the channel of crop production and which are not the result of prior income-induced transactions. We will perform a set of tests to determine if our two instruments obey the exclusion restriction. The next section presents our empirical results.

4.2   Empirical Results

In our analysis we use three different definitions of the household as our unit of analysis, capturing different kinds of relationships between original and split-off households. We start with the *reconstituted* household as unit of analysis where it is assumed that split-off and the parental household pool resources. We then test this assumption. Afterwards we consider split-of and original households as autonomous, independent households and perform our analysis with and without using the feature of multiple households originating from the same initial household in our data.

In column 1 of Table 4 we estimate equation (2), presenting a specification that includes the 1998 consumption level, the number of deaths and wounded at the village level (i.e. our measure of the intensity of violence) and our set of household controls (age, sex, size and education). We also control for all effects that are fixed at the level of the province. In line with the new growth literature, we find strong conditional convergence: poorer household grow faster. We find that exposure to violence significantly lowers household consumption growth. The number of deaths and wounded has a strong negative effect on consumption growth: 25 war-related deaths or wounded in the village between 1998 and 2007 lowered households' consumption growth by 7.5%. Households with more splits see their consumption grown faster.

Specification two in column 2 adds the membership of a rebel faction as a regressor. When a member of the household joined the rebellion between 1999 and 2007, household consumption growth is increased by 34% and the effect of the deaths and wounded variable has not changed. This result should not surprise: other research on ex-combatants in Burundi (Uvin, 2007, p. 13) has shown that many of them now belong to the highest economic category in rural Burundi, and that there is "no reason to assume that this is the result of their better initial conditions"[11]. Using Heckman's sample selection model (1979) does not change the magnitude nor the level of statistical significance of our variables of interest (results not shown).

In the third column of Table 4, we use a household fixed effects specification (thus eliminating all time invariant effects of the reconstituted household). We find that the magnitude of the intensity of violence variable increases while that of rebel group membership remains the same. The level of statistical significance of the latter however decreases to 15%.

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[11] There seems however to be a difference between the demobilized in the North and the South of the country. In the South, where many homes were destroyed during the war, demobilized soldiers had to use their reinsertion payments to rebuild their houses. As such they could not use this money for more profitable investments (Uvin, 2007).

We also ran regressions including a large number of other shocks and control variables at the individual, household and village level, such as illness, death of a member, household composition, migration of a member, harvest failures, crop disease, and the presence of a road and health center. We do not show these results in order not to distract attention away from our core topic, the effect of the civil war on consumption. The inclusion of these other shocks does not alter our findings for the civil war variables. A high number of explanatory variables in the analysis may result in multi-collinearity in the data. Collinearity does not bias the estimates but inflates the standard errors, resulting in smaller t-statistics and hence a higher probability of accepting the null hypothesis.

As mentioned in section (4.1), the results in Table 4 may be criticized because they use the reconstituted household as unit of analysis. While that definition of the household is useful in order to observe the evolution of poverty from the perspective of the initial household, it has a major deficiency: the approach assumes that the split-off and the original household pool resources (see equation (1)). Whether or not this is the case is an empirical question. Table 5 presents the results of estimating model (5) testing the assumption of resource pooling. Columns 1 and 2 estimate initial household fixed effects explaining changes in monthly household consumption per adult equivalent. We find that the coefficient of own household income (in log) is 0.47 and 0.46 respectively, meaning that the own income elasticity of consumption is large. Column 3 presents an IV initial household fixed effects regression whereby income is instrumented with land size and the number of cattle. Results remain the same. The Sargan test for overidentification fails to reject the null-hypothesis, which means that over instruments are valid. Depending on specification, we find that the coefficient of household own income, is between 0.45 and 0.50 and statistically significant at the 1% level.

Table 6 shows that our instruments obey the exclusion restriction. In column 1, it is shown that land size and cattle do not determine consumption once income is controlled for in an initial household fixed effects specification. Column 2 shows that land size and cattle determine income. Taken together with results in table 5, this offers strong evidence that Burundese extended households do not pool resources and that are instruments to deal with the potential endogeneity of income are valid.

Building on the finding that our households do not pool resources, we now consider split-off households as independent households. Column 1 in Table 7 is a household fixed effect specification. It considers the split-off and the original household as independent households and controls for all time-invariant effects specific to each (parental and split-off)

household. It is *not* an initial household fixed effect, it *does not* consider the fact that the split-off and the parental household originate from the same initial household. Consequently this specification does not use the multiple household feature of our data. Nevertheless, it is instructive to show the results of this specification in order to compare them with the results of the initial household fixed effects. The latter only sweeps away those unobservables that are common to the split-off and the old household, while unobservables that are household-specific and vary across the households remain. By comparing the results for different specifications and different definitions of the household, each with their strengths and shortcomings, we want to show the robustness of our results on the civil war variables. For the specification in column 1 of Table 7, the magnitude of the coefficients of the variables of interest (intensity of civil war and rebel movement membership) is similar to the ones in the earlier specifications in table 4. We include a dummy variable for new households (split-offs) whose coefficient is large and statistically significant at the 1% level, showing that split-offs do better compared to parental households.

The specification in column 2 is an *initial* household fixed effects specification. It considers the fact that the split-off once was a member of the original household, applying an initial household fixed effect to the original as well as to the split-off household.[12] The magnitude of the variables of interest is similar to the ones in earlier specifications: 0.0036 for the intensity of violence and 0.41 for membership of a rebel group. The level of statistical significance of their coefficients is 98% and 96% respectively, an improvement compared to the other specifications. This means that 25 deaths or wounded at the village level decrease consumption growth by 9% and rebel membership increases it by 41%. Column 2 in Table 7 is our preferred specification, it controls for all time-invariant effects of the initial household, thereby taking into account that the split-off was a member of the original household in 1998.

Column 3 in Table 7 uses a similar initial household fixed effect specification, but with alternative variables measuring the intensity of civil war. In the survey, we asked all our respondents to evaluate the level of violence and security in the village (hill) in 1998 as well as in 2007. We used a scale from 1 to 4, whereby 1 is 'very peaceful' and 4 is 'very violent'. The variables used in the specification represent changes in this subjective evaluation of security. A change for the better of 1 unit is defined as 'somewhat improved' and a change by

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[12] We remind that, while Table 7 presents initial household fixed effects regressions, these regressions should not be mistaken for resource pooling tests (as in table 5). In these tables, we present the results for our variables of interest, i.e. the civil war variables. The IHHFE controls for all time-invariant effects of the initial household.

2 or 3 units is defined as 'improved a lot'. Baseline is no improvement. [13] Results on the rebel membership variable remain the same in this robustness check and the outcome for the subjective measures of violence and security confirm the earlier result attained with the variable measuring the deaths and wounded in the civil war. An improvement of two units or more on a scale of four, capturing subjective evaluation of security on the hill increases consumption growth by 20%.

One might argue that there is an endogeneity problem with the variable that captures village-level intensity of the war (number of war-related deaths and wounded between 1998 and 2007): the negative effect of village-level violence on household consumption growth may reflect the possibility that villages where household consumption growth has faltered were more likely to experience war related violence. This is however unlikely to be the case given our measure of violence: in the survey, we asked for the number of deaths and wounded in the village resulting from (1) confrontations between army and rebels that took place in or near the village, (2) rebel attacks on the village (because the villagers were believed to support the government) or (3) army attacks on the village (because the army believed the villagers provided shelter and support to the rebels). This variable does not include other types of violence crimes, such as theft, disputes between neighbours or families that resulted into violence and deaths or poverty-induced violence. The latter types maybe determined by consumption, but it is unlikely that *household consumption or household consumption growth* in the village determines whether the village will become the scene of battles between army and rebel forces

In order to check this, we ran logit province fixed-effect regressions (not shown) with the occurrence of confrontations and with the number of deaths and wounded (OLS; shown) as dependents and with average expenditures at the village level as explanatory variables, including consumption in 1998. As shown in column 1 of Table 8, the variables have no statistically significant effect on the number of deaths and wounded at the village level. Hence, endogeneity of violence is not likely to pose a serious threat.

In columns 2 and 3 of Table 8 we test a similar problem of endogeneity at the household level: maybe the persons who joined the rebel movement come from poor households or from households residing in villages with a high intensity of civil war violence. Results show that none of these hypotheses are confirmed. Neither of the variables predicts the joining of a rebel faction in a province fixed effect probit specification.

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[13] There was only one village where security had deteriorated (by 1 unit). We included it with the 'no improvement' group.

5   Conclusions

The paper is concerned with the impact of war-related violence on household consumption growth. We argue that in order to study the evolution of households' welfare throughout time in a methodologically sound fashion, one has to take account of the welfare levels of the newly formed or split-off households. We find that accounting for the welfare levels of the split-off households positively influences the welfare levels of the reconstituted households in 2007: failing to account for the split-off households results in an underestimation of panel households' welfare in 2007.

The effect of civil war on welfare is under-researched, and if researched, the war or violence indicators that are used are at best dummy variables capturing whether a household has been affected by war. Using the number of war-related deaths and wounded in the village between 1999 and 2007 as an indicator for village-level intensity of the civil war, we find a considerable and statistically significant negative effect on household consumption growth. In our preferred household fixed effects specification, we find that 25 war-related deaths and wounded diminished household consumption growth by 9%. Consumption growth of a household living in the most violent village was reduced by over 50%.

We also found that not everybody's welfare is negatively affected by the war. Households who have at least one member joining a rebel group fared far better than the average Burundese household in our sample. We showed that this improvement cannot be explained by more favourable initial conditions. Correction for sample attrition using the Heckman procedure does not change our results. Our results do not seem to be biased by potential problems of endogeneity of the civil war variables.

The results are obtained for different definitions of the household as unit of analysis, characterising different levels of autonomy and independence between parental and split-of households. Whether are not the households of an extended family pool resources is an empirical question. When we test for it, these households seem not to pool their resources, hence we prefer the specification where we consider parental and split-ofs as independent households but where we control for initial household fixed effects.

To test for robustness of the civil war shock, we use a self-reported ordinal variable capturing the violence level in the village in 1998 and 2007. Consumption growth is positively affected in villages where the ordinal measure improved a lot. Together, these results provide strong micro-level evidence on the on the adverse welfare-effects of violent conflict.

While we measured consumption in the second wave just a few years after the end of the civil war, our study suggests to be cautious about recent findings in the literature on the positive effects of civil war. In terms of social capital, cooperation, reconstruction and so such effects may be there, but in our study we clearly found negative effects, except for the rebels themselves.

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Table 1: Household Consumption per Month per Adult Equivalent and Poverty Incidence of

Original Households in 1998 and 2007, by Province of Residence

Province	Consumption Level 1998 (1998 BIF)	Consumption Level 2007 (2007 BIF)	Consumption Level 2007 (1998 BIF)	Poverty Incidence 1998 (%)	Poverty Incidence 2007 (%)
Bururi	9479.83	29720.93	14220.54	49.4	30.4
Cankuzo	9329.83	25531.64	12216.10	50.0	52.3
Cibitoke	6571.40	20218.24	9673.80	71.1	57.8
Gitega	4443.31	14121.24	6617.08	90.2	81.2
Karuzi	5604.18	13829.71	6617.08	76.9	76.9
Kayanza	7271.63	15121.84	7235.33	63.2	72.6
Kirundo	7604.21	13055.82	6246.80	63.6	78.2
Muramvya	9619.66	18628.88	8913.34	52.5	61.6
Muyinga	6563.87	20287.74	9707.05	77.2	48.1
Ngozi	8093.07	9856.72	4716.13	55.6	86.7
Rutana	5970.48	14542.94	6958.34	81.8	70.5
Ruyigi	4161.91	9051.02	4330.63	96.2	90.4
Mean	7084.10	16789.85	8033.42	68.8	67.8
N	872	872	872	872	872

Notes: In columns one and two, consumption per adult equivalent is expressed in actual prices. The poverty incidence in 1998 is based on the rural poverty line of BIF 8,173.15. The poverty incidence in 2007 is based on the same poverty line but updated with product-specific 2007 prices. This updated 2007 poverty line amounts to BIF 16,560.64. Data source: ISTEEBU (2007) and Republic of Burundi and World Bank (1998), Bundervoet (2006) and author's own calculations.

Table 2: Economic Mobility of the Original Households, 1998-2007

1998	2007		Total
	Poor	Non-Poor	
Poor	442 (73.9%)	156 (26.1%)	598
Non-Poor	149 (54.4%)	125 (45.6%)	274
Total	591	281	872

Table 3: Difference-in-Differences for Consumption and Civil War Casualties

Panel A	No War-Related Casualties in Village (1999-2007)	War-Related Casualties in Village (1999-2007)	Difference
Consumption 1999	7159.8 (179.9)	6725.5 (416.2)	434.3 (452.6)
Consumption 2007	8511.2 (362.3)	5770.2 (437.6)	2741.0*** (813.9)
Difference	-1351.4*** (365.5)	955.3* (561.9)	-2306.8*** (725.8)

Panel B	Joined Rebellion 1999-2007	Did Not Join Rebellion 1999-2007	
Consumption 1999	7102.7 (918.3)	7083.6 (167.9)	-19.1 (912.8)
Consumption 2007	10919.5	7962.2	-2957.3 (2024.7)
Mean Difference	-3816.8* (1945.5)	-878.6** (356.2)	-2938.2 (2242.6)

Notes: The Table shows results of difference-in-differences regression with household consumption expenditures (expressed in 1999 prices) as dependent variable. Robust standard errors in parentheses. . ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. Data source: ISTEEBU (2007) and Republic of Burundi and World Bank (1998).

Table 4: Determinants of Consumption Growth (1999-2007), Province and
Household Fixed Effects Regressions, *Reconstituted Households*

Dependent Variable: Consumption Growth	(1) Province Fixed Effects	(2) Province Fixed Effects	(3) Household Fixed Effect
Consumption 1998	-0.74*** (0.052)	-0.74*** (0.052)	-0.01** (0.006)
Deaths & Wounded in Village (99-07)	-0.003** (0.001)	-0.003*** (0.001)	-0.005*** (0.002)
Member Joined Rebellion (1 if yes)		0.34** (0.15)	0.35[+] (0.25)
Number of Split-Off Households	0.16*** (0.034)	0.16*** (0.035)	0.10** (0.04)
Household Control Variables	Yes	Yes	
Changes in HH Control Variables			Yes
Province FE	Yes	Yes	No
Household FE	No	No	Yes
constant	6.52*** (0.55)	6.52*** (0.55)	8.93*** (0.18)
R²	0.35	0.35	0.04
F-Statistic Chi2-Statistic	39.48***	38.21***	13.64***
N of observations	866	866	1728
N of groups			866

Notes: The first two regressions use province fixed effects. The third regression includes a household fixed effect. Standard errors are robust and clustered at the level of the hill. ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. Data source: ISTEEBU (2007) and Republic of Burundi and World Bank (1998).

Table 5: Testing for resource pooling between parental and split-of households (altruism)

Dependent variable: Consumption Growth	(1) Initial HHFE	(2) Initial HH FE	(3) IV- Initial HH FE
Household Own Income Growth	0.47*** (0.011)	0.46*** (0.011)	0.45*** (0.11)
Changes in HH Control Variables	No	Yes	Yes
Constant	4.72*** (0.09)	5.16*** (0.11)	5.27*** (0.60)
R²	0.55	0.55	0.64
F-Statistic	1705.5***	371.7***	38.1***
Overidentification test Sargan statistic Chi-sq (p-value)			0.11 (0.7)
N of observations	2750	2704	2434
N of groups	895	895	824

Notes: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. Data source: ISTEEBU (2007) and Republic of Burundi and World Bank (1998). Because split-offs are included in the analysis and about half of the split-ofs do not live on the hill of their parents (where the 1998 survey was implemented), it does not make much sense to cluster at the level of the hill. When we do estimate robust standard errors clustered at the level of the hill, results do not change.

Table 6: Testing the exclusion restriction for land size and cattle

Dependent variable	(1) (log) Changes in Own Consumption	(2) (log) changes in Own Income
(log) Own Income	0.46*** (0.011)	
Farm size	-0.03 (0.15)	1.12*** (0.31)
Number of Cattle	0.003 (0.14)	0.06** (0.03)
Changes in Household Control Variables	Yes	Yes
Initial Household Fixed Effects	Yes	Yes
Constant	5.1*** (0.11)	8.1*** (0.12)
R² - overall	0.57	0.07
F-Statistic	22.98***	3.9***
N of observations	2541	2541
No of Groups	850	850

Notes: ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. Data source: ISTEEBU (2007) and Republic of Burundi and World Bank (1998). Because split-offs are included in the analysis and about half of the split-ofs do not live on the hill of their parents (where the 1998 survey was implemented), it does not make much sense to cluster at the level of the hill. When we do estimate robust standard errors clustered at the level of the hill, results do not change.

Table 7: Determinants of Consumption Growth (1999-2007), Province and Household Fixed Effects Regressions, *Original and Split-off Households*

| Dependent Variable: (log) Consumption Growth	(1) (old and new) Household Fixed Effects	(2) Initial Household Fixed Effects	(3) Initial Household Fixed Effects (*robustness*)
	HHFE	IHHFE	IHHFE
(log) Consumption 1998	-0.014*** (0.005)	-0.011*** (0.004)	-0.017*** (0.005)
Deaths & Wounded in Village (99-07)	-0.004* (0.002)	-0.004** (0.002)	
Member Joined Rebellion (1 if yes)	0.48** (0.24)	0.41** (0.19)	0.39** (0.19)
Security situation at the village level (99-07) improved <u>somewhat</u>			-0.03 (0.06)
Security situation at the village level (99-07) improved <u>a lot</u>			0.20*** (0.064)
HH is Split-off (1 if yes)	0.86*** (0.09)	0.78*** (0.07)	0.77*** (0.07)
Changes in HH Control Variables	Yes	Yes	Yes
constant	8.9*** (0.13)	9.03*** (0.11)	9.05*** (0.11)
R²	0.16	0.17	0.15
F-Statistic	72.11***	102.0***	91.86***
N of observations	2795	2795	2795
N of groups	1400	896	896

Notes: Following Angrist and Pischke (2009), we do not cluster standard errors at the level of the hill as several split-offs have moved outside the hill. ***: significant at 1% level; **: significant at 5% level; *: significant at 10% level. Data source: ISTEEBU (2007) and Republic of Burundi and World Bank (1998).

Table 8: Testing for Potential Endogeneity of Civil War Variables,

Original and Split-off Households

	(1) Endogeneity Village-Level Violence (OLS)[a]	(2) Endogeneity Rebellion (Probit)	(3) Endogeneity Rebellion (Probit)
Ln(Consumption 1998)	-4.11 (4.82)	0.104 (0.121)	0.106 (0.121)
Deaths & Wounded in Village (99-07)			0.003 (0.007)
Number of Split-Off Households		-0.88 (0.126)	-0.09 (0.127)
HH is a split-off		0.283 (0.245)	0.275 (0.240)
HH Control Variables	Yes	Yes	Yes
Province Fixed Effect	Yes	Yes	Yes
R²	0.18	0.06	0.06
Chi2-Statistic	0.82	15.84	17.35
N	100	1397	1397

[a]: left-censored Tobit regression gives similar results, none of the regressors is statistically significant.

Figure 1: Kernel Density Estimation of Consumption Levels
per Adult Equivalent, 1998 and 2007
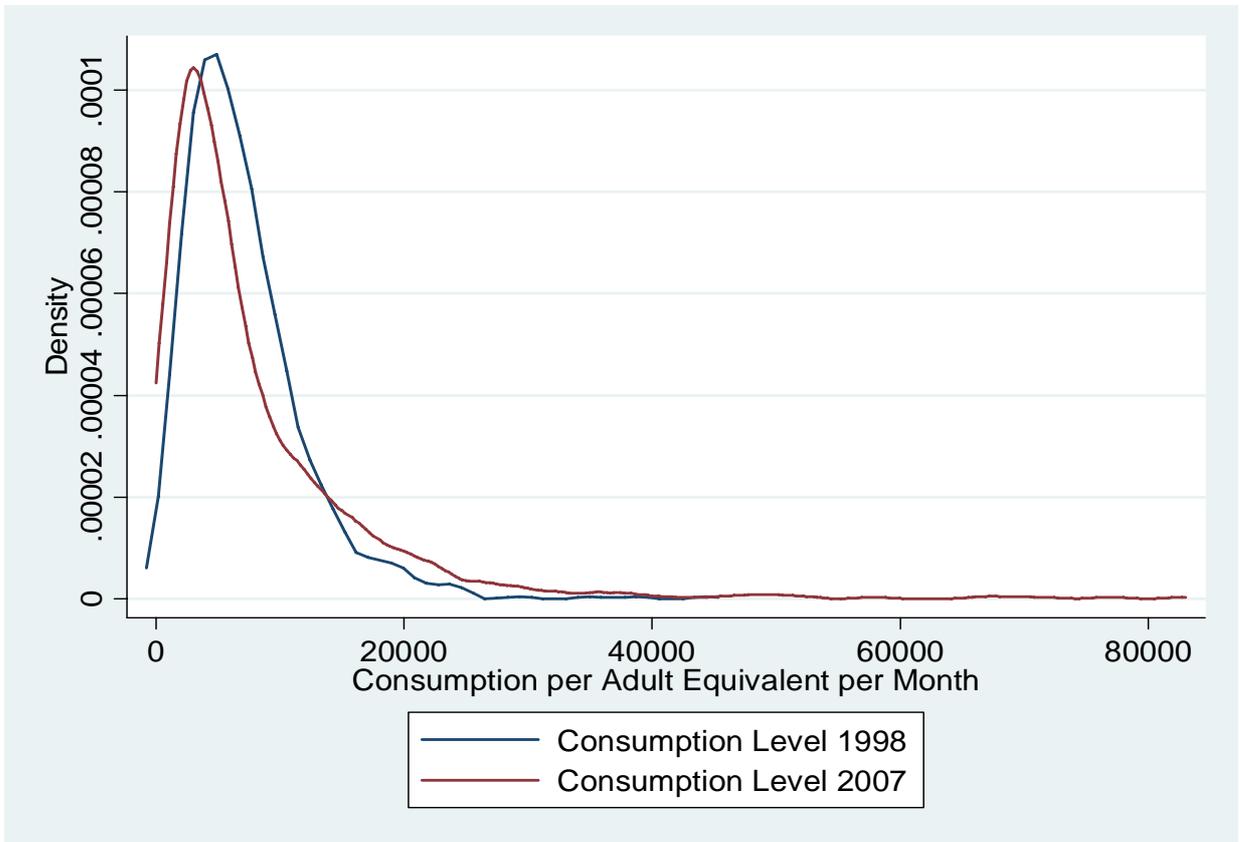
Notes: Kernel density estimation using Epanechnikov kernel. Data source: ISTEEBU (2007) and Republic of Burundi and World Bank (1998).

Appendix

Table A1: Testing for selective attrition among original households (+)

Household characteristics in first round	Sampled in both rounds	Sampled in first round only	Difference in means with t-test	Interviewed in both rounds	Drop out in second round	Difference in means with t-test
Household size	4.90	4.94	0.039	5.09	3.57	-1.52***
Adult equivalents	3.88	3.89	0.19	4.03	2.85	-1.18***
Age of the head	44.09	43.18	-0.9*	43.05	51.12	8.06***
% head educated	0.32	0.31	-0.0	0.34	0.19	-0.15***
% female head	0.25	0.25	0.0	0.22	0.44	0.22***
Total income	42673	40012	-2260	44248	31941	-12307**
Consumption per ae	7115	6689	-425**	7089	7295	206
Number of cattle	0.46	0.38	-0.08*	0.5	0.19	-0.3**
% poor	0.69	0.71	0.03*	0.69	0.70	0.02
village level						
time to market	2.67	2.69	0.02	2.65	2.8	0.16*
Intensity of violence				4.07	3.62	-0.45
N	1000	2908		872	128	

Table A2: Probit models testing for selective attrition, using FGM method (+)

Household characteristics in first round	Sampled in both rounds (1)	Sampled in both rounds (2)	Interviewed in both rounds (3)	Interviewed in both rounds (4)
Household size	-0.01	-0.013	0.129***	0.126***
Age of the head	0.002	0.002*	-0.01***	-0.01***
head educated	0.016	0.007	0.167	0.177
female head	-0.001	-0.008	-0.287**	-0.346**
Log total income	0.036	0.054*	0.036	0.069
Number of cattle	0.018	0.025	0.09	0.08*
Village level				
time to market	-0.012	-0.011	-0.054	-0.054
Intensity of violence			0.001	-0.001
constant	-1.05***	-6.59	0.56	0.68
Province FE	No	Yes	No	Yes
N	3908	3908	1000	1000

(+) Source : Burundi Priority Household Survey 1998 and 2007. The consumption data of two tracked households were found to be unusable. Probit specifications where the selection variable is 1 for selection into the sample or the interview and 0 otherwise. All models are clustered around survey cites to obtain robust standard errors; Fitzgerald, Gottschalk and Moffitt (1998).