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Violence exposure and deprivation: Evidence from the Burundi civil war*

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Abstract

We investigate the relationship between exposure to conflict and household deprivation, using original three-wave household-level panel data for Burundi which report local-level violence exposure. *First*, the data reveal that aggregate poverty has not changed between 1998 and 2012, while food poverty has increased and we observe multiple household-level transitions into and out of poverty. *Second*, households living in localities exposed to the war since 1993 subsequently exhibit a significantly higher level of deprivation than non-exposed households, this difference being persistent years after the conflict termination. Moreover, the correlation between violence and household deprivation is robust to *within-household* estimations. *Third*, the analysis of the household-level poverty dynamics following the most recent period of violence reveals that the likelihood to pull through of poor households is hampered by exposure to high-intensity violence, while the risk to fall into poverty of non-poor households is amplified by exposure to low-intensity violence. We discuss a mechanism based on the nature of violence which could explain this result, and derive some policy implications regarding poverty alleviation in the aftermath of civil wars.

JEL classification: C81, I32, O12, 015, N47.
Keywords: Deprivation; Poverty dynamics; Civil war; Panel data; Africa; Burundi.

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1 Introduction and historical background

After a peak in the early nineties following the end of the Cold War and a decline thereafter, global conflict numbers have increased again since 2010 (see ACLED data (Raleigh et al., 2010), 2015 update). From 1946 to 2012, the world has witnessed more than 250 civil wars, civil conflicts have kicked off every year in 1 to 2% of countries, and have lasted 4 years on average (Simon Fraser University, 2013). Civil wars can affect outcomes as varied as poverty, institutions, human capital and economic growth, which also determine potential factors of risk for the reoccurrence of conflict. Improving knowledge on the consequences of wars, identifying and understanding the various effects of conflicts and their persistence over time, thus appears as a key research challenge to enlighten post-conflict policy making and the management of conflict risk.

This paper investigates the legacies of violence on deprivation in Burundi adopting a micro-level approach. The Burundi civil war started in 1993, following the assassination of the country's first democratically-elected president, Melchior Ndadaye, three months after his arrival in power. This murder triggered brutal attacks by Hutu groups followed by violent retaliation by the Tutsi-dominated army. The conflict, starting from the north-western parts of the country, soon spread toward the northeastern, central and southern provinces. Until 2005, the whole country was concerned by violence, though at different degrees and periods (United Nations, 1996; Chrétien and Mukuri, 2002; Bundervoet, Verwimp, and Akresh, 2009). Between 1993 and 2005, the conflict eventually left over 300,000 Burundians dead, over a population of around 5.7 million in 1993 (Ngaruko and Nkurunziza, 2000). It also led to a sharp reduction in the GDP and GDP per capita, and while the former started to increase again in the beginning of the years 2000, the latter has globally stagnated until now.[1]

The consequences of civil wars have received increasing attention from economists over the recent years. On the macro side, the literature notably shows that civil wars cause steep short-run falls in output (Cerra and Saxena, 2008), massive destruction of capital and outflows of mobile capital (Annan et al., 2006; Collier, Hoeffler, and Pattillo, 2004), and prevents poverty and hunger reduction (Gates et al., 2012). However, it is unclear how such effects persist over time. In particular, how

[1] World Development Indicators.

the disruption of institutions and technology as well as the economic and political uncertainty that follow a conflict endanger the post-war recovery predicted by the neoclassical growth model remains understudied (Blattman and Miguel, 2010). In a recent paper, closely related to ours, Bircan, Brück, and Vothknecht (2016) investigate the distributive impacts of internal violent conflicts and their persistence over time. Using country-level panel data, they find that periods of violence, as well as early post-conflict, are associated with rising levels of inequality, but that this effect is not permanent, and they show evidence of catching-up in the longer run.

A nascent literature, in economics but also in other fields of social sciences, investigates the micro-level consequences of civil wars. Justino (2012) provides a thorough overview of the various effects of war on poverty, and Martin-Shields and Stojetz (2017) summarize the results of the literature on the interactions between violent conflict and food security. We do not claim to be as complete as these two papers here. To enlighten the empirical investigation that follows, and that focuses on poverty and food poverty, we simply highlight four main (non-exclusive) micro-level mechanisms through which violence exposure is expected to affect deprivation. We do not test directly whether one or more of these mechanisms have been at play in Burundi in what follows, but we use this review of the mechanisms identified by the literature as a framework to think about the link between violence and the dynamics of poverty and to discuss our empirical results.

First, war-related deaths and casualties affect households' composition and labour endowment. This demographic effect is likely to affect deprivation. Direct excess mortality has notably been shown to target young men (De Walque and Verwimp, 2010; De Walque, 2006). Labour being the main input in the production function of many households, this could result into a shrink in the ratio of net producers to net consumers within the household, and eventually translate into higher poverty in war-affected areas. In addition to the direct deaths, referred to as the "missing males", Guha-Sapir and D'Aoust (2011) underline the indirect deaths caused by disease, hunger or lack of care. Injuries, disability and post-war psychological trauma may also impede the working ability of the surviving household members, and eventually boost (food-)poverty incidence.

Second, violence might reduce the accumulation of human capital in exposed areas. Early-life exposure to civil wars has notably been shown to impede children's nutritional status and health

outcomes, as well as schooling outcomes.[2] Martin-Shields and Stojetz (2017) provide a rich overview of the findings of this literature, and highlight that adverse effects may even develop *in utero*. All these effects might have long-run post-war consequences, notably on human capital, labour market outcomes and income during adulthood (Smith, 2009; Alderman, Hoddinott, and Kinsey, 2006; Ichino and Winter-Ebmer, 2004), and, *in fine*, translate into more (food-)poverty.

Third, conflict often causes the loss or destruction of productive assets such as land or cattle, in areas where many households are smallholder farmers (Brück, 2001; González and Lopez, 2007; Shemyakina, 2011). This impedes on important sources of households' livelihood, and can affect critically their productive capacity. The literature suggests that the micro-level impact of conflict-related destruction of physical capital depends on the type of violence and on households' characteristics (Justino, 2012; Martin-Shields and Stojetz, 2017). In particular, asset endowment might shape households' vulnerability to conflict as some assets may be more or less easily looted (Bundervoet, 2010). These works consistently emphasize the destruction of physical capital as another channel through which violence might affect deprivation, probably heterogeneously across household characteristics.

Fourth, beyond physical and human capital, civil conflicts affect social capital and attitudes, which might have durable effects on household-level outcomes – in particular, poverty. In a theoretical paper, Rohner, Thoenig, and Zilibotti (2013b) show that violence erodes trust and social capital, a result which they empirically confirm in the case of Uganda (Rohner, Thoenig, and Zilibotti, 2013a). In different settings, Bellows and Miguel (2009) find that more victimized people during the Sierra Leone conflict became more likely to attend community meetings and to join social and political groups in the aftermath of the war; and Voors et al. (2012) show that individuals exposed to the civil war in Burundi subsequently displayed more altruism and risk-seeking behaviors. The recent paper by De Luca and Verpoorten (2015) could reconcile these mixed findings, as

[2] A rich literature studies such effects. See notably Bundervoet, Verwimp, and Akresh (2009), Akresh, Verwimp, and Bundervoet (2011), Minoiu and Shemyakina (2014), Akresh, Lucchetti, and Thirumurthy (2012), Akresh et al. (2012), Domingues and Barre (2013), Shemyakina (2017) on children's nutrition and health; and Akresh and de Walque (2008), Shemyakina (2011), Chamarbagwala and Moran (2011), Swee (2015) on education. Note that the literature on the consequences of violence on human capital accumulation is not completely univocal: Pivovarova and Swee (2015) find no effect of war intensity on schooling attainment in Nepal once unobserved individual heterogeneity is accounted for.

it shows that short-term negative effects on trust in Uganda are followed by recovery. Although non-univocal yet, the literature thus suggests that violence exposure can influence attitudes – for instance, toward risk and cooperation – and social capital, which could eventually affect poverty and food poverty.

In addition to these four micro-level channels, the destruction of infrastructures at the meso-economic level, such as health or transportation facilities, might also affect the development of human and social capital and participate in increasing household deprivation (Ghobarah, Huth, and Russett, 2004; Lai and Thyne, 2007).

According to Justino (2012), "although there is a large body of evidence on the destructive effects of war, we are still far from understanding how these effects may or may not persist across time". This paper aims at contributing to the micro-economic literature on the consequences of civil wars, by documenting how violence exposure correlates with households' deprivation over time, relying on three waves of panel data collected in Burundi between 1998 and 2012. In the next section, we present our empirical approach and discuss the potential endogeneity which could threaten it. Section 3 presents the survey and data at hand, and, based on these data, Section 4 draws an overview of poverty, food poverty and violence exposure over the period of analysis. Sections 5 and 6 then display the empirical results. In Section 5, we put forward a persistent correlation between violence exposure and household deprivation over time. In Section 6, we investigate which households have been particularly vulnerable to violence in terms of poverty status, by looking at poverty switches, and emphasize a heterogeneous relationship between poverty transitions and violence exposure during the second period of the war (1999 – 2007). Section 7 discusses the interpretation and implications of the results, and Section 8 concludes.

2 Empirical approach and the risk of endogeneity

Two major challenges arise when trying to assess the consequences of violence over time. *First*, violence is not random. Both at the macro and at the micro level, poor post-war macroeconomic performances and micro-level outcomes could reflect the prevailing conditions that triggered violence in the first place. Poverty is notably among the factors that boost the likelihood of conflict onset

while they are also likely to be exacerbated by violence (Fearon and Laitin, 2003; Brück and d'Errico, 2017). *Second*, observing longitudinal data is rare in post-conflict settings. In a micro-economic perspective, this involves the ability to track individuals and their outcomes over time.

The data that we rely on have major advantages regarding these methodological concerns. Their main value-added is their longitudinal dimension. While the Burundi civil war broke out in 1993 and lasted until 2005, three waves of a nationwide survey were conducted in 1998, 2007 and 2012. A large effort of tracking was implemented, in particular toward households who migrated inside Burundi and toward split-off households created by members of the original households after 1998. Moreover, retrospective data on violence exposure from the very beginning of the civil war were collected at the level of the localities.[3] Local exposure is thus observed during the whole period of the war. Thanks to the panel dimension of the data, we implement three empirical strategies to measure as properly as we can the correlation between violence exposure and subsequent household deprivation: (i) first, we introduce time trends, province-specific time trends, and province fixed effects, to purge the estimates from time patterns and province-level unobservable heterogeneity, (ii) second, *within-household* estimations are performed, thus ruling out all the time-invariant household-level heterogeneity affecting the correlation between violence and deprivation over time, and (iii) third, we control for household-level time varying characteristics.

Their panel dimension and the local measure of violence that they provide are very interesting features of the data, which are also very rare in the context of a poor economy undergoing long-lasting conflict. This provides us with a great opportunity to question the legacies of local exposure to the war on household-level poverty. Yet, our empirical exercise remains subject to two important limits, which we want to acknowledge and discuss here.

The first limitation is that the data do not contain pre-war household-level information, which could allow us to investigate the potential endogeneity of conflict spatial location to household poverty. In the absence of baseline information, the longitudinal dimension of our data is very helpful to track the evolution of household poverty after violence exposure, but does not help us

[3]The territory of Burundi is composed of four administrative levels: *provinces*, *communes*, *collines*, and *sous-collines*. Our unit of observation of violence, which comes from the survey design and which we call *locality* for simplicity, is not an administrative cluster. It is an intermediary between the *colline* and the *sous-colline*: a *locality* can cover multiple *sous-collines* but is always linked to a unique *colline*.

dealing with the non-randomness of war. External sources of information can help us to evaluate the potential impact of this first limitation on our empirical results. In particular, previous literature shows that standard factors of greed and grievance are poorly explanatory of the pattern of violence over the course of the Burundi civil war (Voors et al., 2012). Militia attacks, either from the army or from the rebels, indiscriminately brutal and random, affecting the entire country and causing profound fear among the whole population, reflect more accurately what happened according to the dedicated qualitative literature (Uvin, 1999; Krueger and Krueger, 2007; Longman, 1998).

In addition to these elements from the literature, we run two empirical exercises based on external data in Appendix 9.1. They aim at investigating, respectively, the risk of locality-level and household-level selection into violence. *First*, we rely on pre-violence province-level data from the national statistics institute (ISTEEBU, 1993) and locality-level data from Voors et al. (2012), to analyze the determinants of violence exposure at the level of the localities (see Table 12).[4] The dependent variable, which comes from our panel data, is a dummy equal to one if at least one war-related death or casualty was reported in the locality, over each sub-period of the war. The vector of explanatory variables gathers geographic, demographic and socio-economic characteristics of the localities. No systematic correlation appears between, on the one hand, violence incidence per period and, on the other hand, the pre-violence characteristics of the localities taken from ISTEEBU (1993) and Voors et al. (2012).

Second, we rely on a survey conducted in 2002 to provide insights on the potential endogeneity of conflict location to household poverty. In 2002, the United Nations Population Fund conducted a nationally representative survey called 'Enquête socio-démographique et de santé de la reproduction' (ESDSR), which objective was to fill in the information gap since the previously collected census data in 1990. This dataset is not a panel but it provides one household-level pre-exposure information, based on a recall question, which we can use to investigate household-level selection into violence (though on a different sample of households), namely the amount of livestock owned in 1993. While there is no information on poverty, our prior is that livestock ownership is a proxy for households' wealth, and thus very correlated with (non-)poverty. In Table 13 in Appendix 9.1, we

[4] Among the 100 localities of our data, only 94 can be matched to these two external sources of data.

compare households' mean livestock in 1993 across future exposure status. The exposure dummy, which comes from our panel data, is available at the locality-level. Unfortunately, the smallest geographic unit in the 2002 survey is the *commune*, which is larger than the locality.[5] Thus, we aggregate our locality-level data to compute a *commune*-level exposure dummy equal to one if at least one war-related death or casualty was reported in the *commune* (between 1993 and 1998 and between 1998 and 2007), and we compare the average livestock owned in 1993 by households of the 2002 survey living in *communes* which are going to be exposed to the war, to the average livestock owned in 1993 by households of the 2002 survey living in *communes* which are not going to be exposed to the war. The differences of means are not significant, which suggests that the average household-level wealth prior to violence exposure was not different between subsequently exposed and non-exposed *commune*.

None of these two empirical exercises, based on external data, perfectly deals with the potential endogeneity of conflict spatial location to household deprivation. However, together with the quantitative and qualitative results of the existing literature, they provide suggestive evidence which is reassuring regarding the potential importance of locality- and household-level selection into violence during the Burundi civil war. In the absence of baseline data allowing us to bracket the period of violence, but given that our data allow us to control for time trends and household fixed effects, this makes us more confident in exploiting the timing of local exposure to violence to investigate the correlation between conflict exposure and household poverty, though we do not interpret our estimates as causal.

The second main limit that our empirical approach suffers from is that, although the longitudinal dimension of the data allows us to develop a *within-household* analysis ruling out any bias introduced by time-invariant household-level characteristics, we cannot do anything about time-varying unobserved household-level characteristics. As stated earlier, we mobilize household-level time varying variables in order to control for as much characteristics as possible. In particular, we introduce household size, a number of characteristics of the household's head (age, gender, education and occupation) and dummies indicating whether the household migrated and whether it is

[5] Our data cover 100 localities spread across 76 *communes*, among which 45 are in the 2002 survey.

a split-off or an original household. Yet, non-observed time-varying characteristics simultaneously correlated with violence exposure and poverty could still introduce omitted variable biases, which we need to keep in mind when interpreting the results.

3 Data

3.1 The Three-round Panel Priority Survey

We rely on a fifteen-year panel with three rounds of data collection, 1998, 2007 and 2012. In 1998, with support from the World Bank, the Government of Burundi undertook a study named 'Enquête Nationale sur les Conditions de Vie de la Population'. The national statistics institute conducted this so-called Priority Survey over more than 6,000 households living in 391 randomly selected rural and urban survey sites (we use the generic term 'locality' to refer to these survey sites). In 2007, a Panel Priority Survey was designed as the second wave of the 1998 survey and targeted to re-interview 1,000 households from 100 randomly selected rural localities of the original sample, as well as the so-called 'split-off' households formed between 1998 and 2007 by members of the original households. A total of 874 original households and 534 split-offs were re-interviewed. The third round was implemented in 2012, allowing to follow the evolution of households' consumption patterns relying on fully comparable questions. Out of the 1,408 households of 2007, 1,263 were re-interviewed in 2012, i.e. a resurvey rate of 89.7%.[6]

3.2 Measuring deprivation

3.2.1 Valuing household consumption

In each round of the survey, a module asked interviewees about their consumption of a range of food and non-food items. For each food item, respondents declared the quantity consumed by their household over the last seven days in the unit of their choice,[7] and the price per unit on the local market. To compute the nominal value of households' food consumption, we first express all the

[6] While the 2007 survey targeted both 1,000 households interviewed in 1998 and their split-offs, the 2012 round focused on re-interviewing households who had already been interviewed in 2007. New split-offs, potentially emerging between 2007 and 2012, are thus absent from the sample.

[7] Including quantity bought, produced, and received as gift.

quantities consumed in kilograms (or liters). We then compute household-level prices per kilograms (or liters) for each item, which allow us to calculate country-level median prices. We eventually use these prices to compute the total value of households' food consumption per month, that we express per adult equivalent. In the absence of market price data, taking the median of declared prices aims at mitigating the measurement errors that might characterize declared prices. The total value of households' consumption is finally the sum between households' food consumption, valued with the vector of country-level median prices, and non-food consumption. The value of the latter is directly derived from declared prices: as opposed to food items, it is not possible to derive country-level median prices for non-food items such as 'clothing' or 'cell phones', for which households declare the total amount spent without specifying a quantity.

3.2.2 Poverty line

We compute the poverty line relying on the 'cost of basic needs' method (Ravallion, 1994, 1998; Ravallion and Bidani, 1994), according to which poverty is "a lack of command over basic consumption needs, and the poverty line the cost of those needs" (Ravallion and Bidani, 1994). This minimal level of consumption must encompass food and non-food consumption.

To estimate the food component of the basic consumption needs, we specify a consumption bundle deemed adequate to satisfy physical needs in terms of caloric value, and then estimate its cost. The basket of goods is derived from the food consumption of the 50% households who have the lowest consumption per month and adult equivalent. We calculate the caloric value of the average basket of goods that these households consume, and re-scale it so as to reach the caloric requirement considered as minimum (namely, 2,500 calories per day and adult equivalent (Minecofin, 2002)[8]), without changing the proportions of the different items. This allows to derive a basket of goods which exactly corresponds to 2,500 calories per day per adult equivalent, and which is consistent with the consumption habits of the poorest. The food component of the poverty line is the value of this basket based on country-level median prices, and households' food poverty status depends on whether their food consumption per adult equivalent reaches this threshold.

[8]Minimum caloric requirement in the case of Rwanda, which is a very similar context.

Estimating the minimal non-food requirements is trickier than estimating the minimal food requirements in the absence of an equivalent to caloric intake for non-food consumption. To compute the non-food component of the poverty line, we follow Bundervoet (2006) and Verwimp and Bundervoet (2009). Based on the previously cited literature, and using the first two waves of the panel data that we rely on, they estimate the share of non-food spending of Burundian households whose total level of consumption is very close from the food poverty line. These households could exactly fit their caloric needs, but instead they decide to sacrifice part of their food consumption in favor of non-food consumption. Eventually they do not fulfil their minimal caloric requirements, thus it is arguable that their non-food expenditures correspond to what they consider as absolutely necessary. We use the estimated food share for Burundi (namely, 82%) to derive the non-food component of the poverty line. In the end, the poverty line equals the addition between the food and non-food components, and households' poverty status is determined by comparing their monthly consumption per adult equivalent to the poverty line.

3.3 Sample and attrition

Most of the empirical analysis displayed hereafter relies on the sample of households for whom data on consumption, as well as socioeconomic and demographic characteristics introduced as control variables, are available. Since this information is not available for all the households of the dataset, we work on a sample of it, which consists of 943 original households in 1998, 751 in 2007 and 595 in 2012.[9] Additionally, we observe 327 split-offs in 2012[10] and 287 in 2007, and we can impute the data of their household of origin for 279 of them in 1998. For sake of consistency with the empirical analysis, and given that they are found to be very similar if the whole sample is considered, the descriptive statistics that follow are displayed over this core sample.

Considerable efforts were done to re-contact the members of original households, and the overall re-contact rate of the survey was 87.4% between 1998 and 2007 and 89.7% between 2007 and 2012. This can be considered as a success given the time interval between each round and context of

[9] Among the 751 original households of 2007, 741 are tracked from 1998 and 10 are unobserved in 1998 because of missing data. Among the 595 original households of 2012, 542 are tracked from 2007 and 53 are unobserved in 2007 because of missing data.

[10] Of which 188 are tracked from 2007 and 139 are unobserved in 2007 because of missing data.

violence. Still, some households are lost from one wave to the other. In that case, qualitative information was collected to document the reasons for dropping out. Four main cases emerge, in comparable proportions: (i) the household members died, (ii) the household migrated and neighbors could not inform of the whereabouts, (iii) the household disappeared without leaving a trace, and (iv) the household dissolved.

This attrition could bias the results if non-tracked households have specific characteristics. To document this issue, we look at the differences of means of the main characteristics of tracked and non-tracked households of our sample of analysis in Table 14 (Appendix 9.2). We document households' socioeconomic and demographic characteristics, average monthly consumption (in logarithm), (food-)poverty status, and violence exposure measured through a dummy equal to one if the locality recorded at least one war-related death or casualty.

Some characteristics are different between households who stay in the sample and households who drop out. Households who were larger at time t are significantly more likely to remain in the sample at time $t+1$. To a lesser extent, the occupation, age and gender of the head turn out to be slightly different between tracked and non-tracked households. These results urge us to check the robustness of our empirical results to controlling for socioeconomic and demographic characteristics. The *t-tests* do not reveal any significant difference in terms of consumption and (food-)poverty status between households who remain in the sample and households who do not, neither in 2007 nor in 2012. On the other hand, households who drop out were living in localities that had been on average less exposed to violence between 1993 and 1998, but not significantly differently exposed between 1999 and 2007. One potential reason for dropouts to be more frequent in localities which were less exposed during the first period of the war might be linked to the migration of households who decided to flee before being exposed to violence. This could lead us to over- or underestimate the link between violence and poverty depending on whether these households got poorer or richer after dropping out, and it is thus impossible to conjecture the direction of the potential related bias. In spite of very good resurvey rates, the non-random selection into the sample of analysis with respect to violence exposure over the first period of the war thus has to be kept in mind, along with the two previously discussed potential endogeneity threats.

3.4 Violence data

In the two first rounds of the survey (1998 and 2007), local leaders were asked about the past incidence of violence at the locality level. Specifically, in 1998 (respectively, 2007), the number of war-related deaths and casualties suffered in each locality between 1993 and 1998 (respectively, between 1999 and 2007) is reported. We construct our variables of interest on violence exposure based on this information. Given their declarative and retrospective nature, we are very cautious about the precision of the data. In this perspective, our main results are based on a dichotomic variable for violence incidence, which equals one if at least one war-related death or casualty is reported. While the exact number of war fatalities might be difficult to precisely capture through the survey protocol, we are confident over the fact that the distinction between 'at least one fatality' and 'no fatality at all' is much less subject to recall bias and measurement error. Between 1993 and 1998, 527 of the 943 households of the sample (55.89%) were exposed to violence. Between 1999 and 2007, violence was much less spread, with 192 of the 1,038 households of the sample exposed (18.50%). After 2007, no violence was to deplore anymore. The temporal variation which we exploit is thus a generally decreasing trend in violence incidence over time, although some localities which were not exposed during the first period did report violence exposure between 1999 and 2007.

While we argue that the dichotomous measure of violence exposure is less subject to recall and measurement errors, it also implies treating in the same way localities with one and with one hundred fatalities, which is questionable. Thus, in addition to the main results based on violence incidence, we also exploit the information we have on violence intensity, which might correlate differently with poverty. The intensity of violence was much more important over the first period, as compared to the second. Between 1993 and 1998, the maximum number of reported deaths and casualties in the sample is equal to 560, and the average (respectively, median) among exposed localities is equal to 113 (respectively, 68). Between 1999 and 2007, the maximum number of reported deaths and casualties in the sample is equal to 93, and the average (respectively, median) among exposed localities is equal to 20 (respectively, 9). Given (i) the potential measurement errors in the reported number of deaths and casualties, and (ii) the important difference in violence intensity between the two periods of observation; we use the logarithm of the number of war-

related deaths and casualties as a proxy for violence intensity.[11] In the last step of the analysis, which focuses on post-1998 violence, we also mobilize a categorical variable to compare non-exposed households to households living in localities exposed to lower-or-equal-to-median violence intensity and households living in localities exposed to higher-than-median violence intensity.

4 Descriptive statistics

4.1 An overview of poverty in Burundi between 1998 and 2012

Figure 1 displays the level of households' deflated monthly expenditures per adult equivalent in 1998, 2007 and 2012, distinguishing original and split-off households. Monthly expenditures have undergone an upward trend across the period, which was more pronounced after 2007 for original households. Note that the average monthly consumption of split-offs' original households in 1998 equals 5,374 BIF: split-offs thus emanate from relatively poorer households.

Figure 1: **Monthly expenditures in BIF, deflated.**

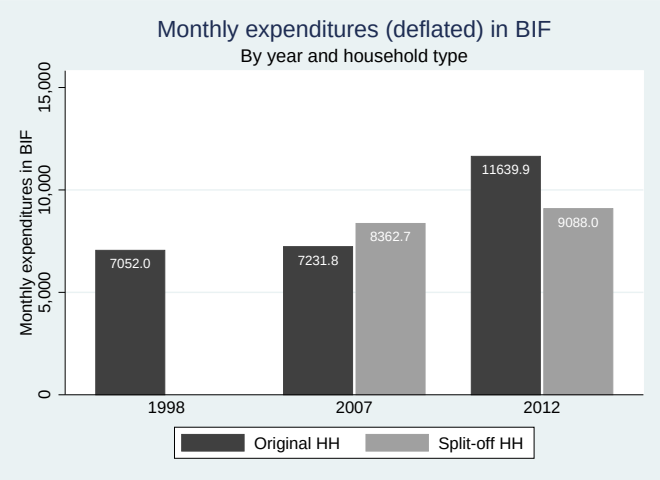


Table 1 presents the first–, second– and third–degree Foster–Greer–Thorbecke poverty measures

[11] More precisely, this intensity measure is equal to the logarithm of one plus the number of deaths and casualties.

(Foster, Greer, and Thorbecke, 1984), by household type and over time.[12] Around 69% of the households of the sample are poor in 1998. The headcount ratio only slightly decreases afterward. While we observe almost constant headcount poverty over fifteen years, there is a clear upward increase in the poverty gap (from 31% in 1998 to 39% in 2012), which corresponds to an increased depth of poverty, and in the poverty severity (from 18% to more than 26%), which corresponds to increased inequalities. The Gini index consistently grows over time, for both original and split-off households (see Table 15 in Appendix 9.3). At the same time, food poverty increases from 57% in 1998 to 61% in 2012.

Table 1: **Poverty by household type and year.**

Type of HH	**Statistics**	**Year**		
		1998	**2007**	**2012**
All	Number of HH	943	1,038	922
	Poverty headcount ratio	69.14	68.98	67.68
	Food Poverty headcount ratio	57.05	60.60	61.06
	Poverty gap	30.91	36.14	38.89
	Poverty severity	18.00	23.29	26.49
Original	Number of HH		751	595
	Poverty Headcount ratio		70.57	69.41
	Food Poverty headcount ratio		61.78	62.69
	Poverty gap		37.84	40.10
	Poverty severity		24.81	27.50
Split-off	Number of HH		287	327
	Poverty Headcount ratio		64.81	64.53
	Food Poverty headcount ratio		57.49	58.10
	Poverty gap		31.67	36.71
	Poverty severity		19.33	24.66

The poverty profile is different by household type: (food-)poverty, poverty gap and poverty severity are higher among original than split-off households across the whole period. However, the difference between the two groups decreases between 2007 and 2012 (except regarding food poverty). Combined with Figure 1, these results are consistent with inequality having increased relatively more among original households than among split-off households (see Table 15 in Appendix 9.3).

[12] For a continuous expenditure distribution, the FGT–index is given by:

$$P_\alpha = \int_{i=1}^{z} \{((z - y_i)/z)^\alpha f(y)\} \mathrm{d}y$$

where z and y are, respectively, the poverty line and amount of expenditures. For $\alpha = 0$ and 1, the index measures, respectively, the prevalence of poverty (poverty headcount) and the intensity of poverty (poverty gap), while for $\alpha = 2$ it provides a measure of the poverty severity (Ravallion, 1992; Bigsten et al., 2003).

In the course of the panel, some households fall into (food-)poverty whereas others escape it. Table 2 maps these movements into and out of (food-)poverty across time.[13] Panel A is the transition matrix between 1998 and 2007. Nearly 59% of the non-poor households in 1998 fall into poverty by 2007, while less than 28% of the poor households of 1998 escape poverty in 2007. Moreover, more than 51% of the non-food-poor households of 1998 suffer food poverty in 2007, while less than 34% of the 1998 food-poor escape food poverty nine years later. The results are very similar between 2007 and 2012 (Panel B). In the end, relatively few of the non-(food-)poor households of 1998 remain so by 2012 (namely, 38% of the non-poor, and 45% of the non-food-poor), and a relatively large share of the (food-)poor of 1998 remain in the same condition in 2012 (namely, 71% of the poor, and 66% of the food-poor). Two conclusions can be drawn from these figures. First, at the household level, there is high instability over the period, with multiple transitions into and out of (food-)poverty. Second, regardless of the time period and of whether poverty or food poverty is considered, the share of poor households who manage to escape their condition is systematically smaller than the share of non-poor who fall into poverty. These results hold when only split-off households are considered (see Table 16 in Appendix 9.4), but comparing Tables 2 and 16 shows that transitions toward non-(food-)poverty are relatively more frequent for split-off households, regardless of the considered time period.

Another angle to view poverty dynamics is to look at the distribution of households at the lens of a threefold poverty status. Table 3 gives the shares of households who are permanently non-poor (i.e., non-poor in 1998, 2007 and 2012), chronically poor (i.e., poor in 1998, 2007 and 2012), and 'switchers' (i.e., one time poor (non-poor) and two times non-poor (poor)), for both poverty and food poverty. Computing these shares requires to observe households' poverty status across the three points of the panel. There are 1,223 households whose poverty status is observed three times (Column (1)). In the second part of the empirical analysis (Section 6), we investigate the determinants of poverty status switches focusing on households whose poverty status is observed three times but also whose socioeconomic and demographic characteristics are observed in 1998. There are 996 such households. To have an idea of their degree of representativeness, Column (2)

[13] The summary statistics are displayed below over the complete analytical sample. We display the same results over the sub-sample of split-off households in Appendix 9.4

Table 2: **(Food-)Poverty transition matrices between 1998, 2007 and 2012.**

Panel A. 1998-2007.

	2007	
1998	Non Poor	Poor
Non Poor	41.26	58.74
Poor	27.54	72.46

	2007	
1998	Non Food-Poor	Food-Poor
Non Food-Poor	48.84	51.16
Food-Poor	33.76	66.24

Panel B. 2007-2012.

	2012	
2007	Non Poor	Poor
Non Poor	43.91	56.09
Poor	24.80	75.20

	2012	
2007	Non Food-Poor	Food-Poor
Non Food-Poor	50.35	49.65
Food-Poor	30.09	69.91

Panel C. 1998-2012.

	2012	
1998	Non Poor	Poor
Non Poor	37.93	62.07
Poor	29.16	70.84

	2012	
1998	Non Food-Poor	Food-Poor
Non Food-Poor	44.75	55.25
Food-Poor	34.48	65.52

of Table 3 displays their distribution by threefold poverty status. The distribution is very similar between the two samples. Over the period, only 6% of the households have never been poor, while around 39% are in chronic poverty and the remaining 55% have switched poverty status at least once. Moreover, around 27% of households suffer food poverty over the whole period and 62% episodically, while around 11% have never been food-poor.[14]

Table 3: **Threefold (food-)poverty status in 1998, 2007 and 2012.**

	(1)	(2)	(3)	(4)
	Number of HH (%)			
	Poverty		Food poverty	
	Whole sample	Sub-sample	Whole sample	Sub-sample
Permanent non poor	76 (6.21)	58 (5.82)	138 (11.28)	106 (10.64)
Switchers	676 (55.27)	546 (54.82)	757 (61.90)	612 (61.45)
Chronic poor	471 (38.51)	392 (39.36)	328 (26.82)	278 (27.91)
Total	1,223 (100)	996 (100)	1,223 (100)	996 (100)

Table 17 in Appendix 9.4 computes the same figures over the sub-sample of split-off households. Consistently with our previous findings, the share of 'switchers' is larger among split-offs than in the complete sample (while split-offs count less permanent non-poor and, to a lesser extent, chronic poor).

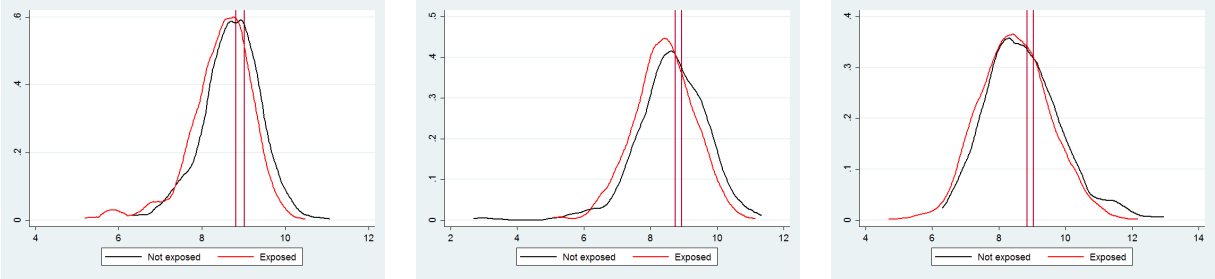
[14] While the share of "switchers" is very high, which could rise doubts regarding our measure of poverty, it is very much in line with the previous literature on comparable contexts (Baulch and Hoddinott, 2000). See in particular Justino and Verwimp (2013) on Rwanda and Hoddinott and Kinsey (1998) on Zimbabwe.

Last, in Table 18 of Appendix 9.4, we compute the differences of means in consumption and (food-)poverty between original and split-off households. While split-offs emerge from significantly poorer households, the difference between the two categories of households changes sign after splitting off took place, in 2007. Finally, no significant difference appears any more in 2012.

4.2 Deprivation by exposure status

Although the aggregate incidence of poverty did not change much over the period, its severity has deepened and household-level transitions into and out of (food-)poverty have been numerous. Figure 2 displays the density function of households' monthly consumption (in logarithm) in 1998, 2007 and 2012, distinguishing households exposed and non-exposed to violence over the past. Exposure to violence is defined at the locality level as a dummy variable, equal to one if at least one war-related death or casualty was reported over the past in the locality where the household lives and zero otherwise. The two vertical lines represent the (food-)poverty lines. The distribution is systematically shifted toward the left-hand side in the group of exposed households, the gap between the two lines being seemingly smaller but persistent in 2012.

Figure 2: **Distribution of consumption among exposed and non-exposed households.**



1998 2007 2012

Table 4 displays the differences of means in consumption and prevalence of (food-)poverty in each survey wave, between households who have been exposed to violence over the past and households who have not. Exposed households exhibit a significantly lower average consumption and higher incidence of (food-)poverty in the subsequent years, the difference between exposed and non-exposed households being similar in 1998 and 2007. Although no violence occurs between 2007 and 2012,

households exposed to violence before 2007 still exhibit a significantly lower average consumption and gather a significantly higher share of (food-)poor in 2012. This suggests that the increased deprivation associated with violence exposure persists over time.[15] Table 19 in Appendix 9.4 runs the same exercise over the sub-sample of split-off households. Differences in average deprivation between split-offs in exposed and non-exposed localities are also highly statistically significant in 1998 and 2007, while they seem to be less persistent in 2012 than for the complete sample.

Table 4: **Violence exposure and deprivation – Differences of means.**

		Not exposed to violence	Exposed to violence	Difference
Panel A: 1998	Consumption (in log)	8.743	8.515	0.228*** (0.049)
	Poverty	0.627	0.742	-0.115*** (0.030)
	Food Poverty	0.502	0.624	-0.122*** (0.032)
	Number of households	*416*	*527*	*943*
Panel B: 2007	Consumption (in log)	8.635	8.400	0.235*** (0.062)
	Poverty	0.618	0.733	-0.115*** (0.029)
	Food Poverty	0.541	0.645	-0.104*** (0.031)
	Number of households	*390*	*648*	*1,038*
Panel C: 2012	Consumption (in log)	8.725	8.478	0.246*** (0.077)
	Poverty	0.628	0.706	-0.078** (0.032)
	Food Poverty	0.566	0.637	-0.071** (0.033)
	Number of households	*341*	*581*	*922*

Significance of the difference between exposed and non-exposed households using a paired *t-test*.
***p<0.01, **p<0.05, *p<0.1. Standard errors in parentheses.
Violence exposure is computed over the past, i.e. between 1993 and 1998 for Panel A, and between 1993 and 2007 for Panels B and C.

From 2007 to 2012, the difference of means between exposed and non-exposed households in terms of (food-)poverty incidence decreases; and the standard error of the difference in terms of both (food-)poverty incidence and consumption increases. Based on these uncontrolled differences of means, we run a simple simulation exercise aiming at estimating the date at which the gap between exposed and non-exposed households would close, i.e. at which the differences between these two groups would no longer be significantly different from zero, based on the trend observed

[15] Running the same comparison at the locality- rather than at the household-level yields similarly persistent differences between exposed and non-exposed localities in terms of average consumption and (food-)poverty rates (results not shown for brevity, available upon request). Moreover, comparing the socio-demographic and economic characteristics of exposed and non-exposed localities suggests that exposed localities are, on average, younger and counting smaller shares of non-farmers after exposure than non-exposed households. This is consistent with increased poverty, these two differences being persistent across the three survey waves.

between 2007 and 2012. We do so by assuming that, for each measure of deprivation, the yearly growth rates of the difference of means and of its standard error after 2012 equal their observed values between 2007 and 2012, i.e. once violence has stopped. The results of this simulation are displayed in Table 5. If the recently observed trends continue, the convergence between households who were exposed to the civil war and households who were not is predicted in 2015 in terms of food poverty, in 2017 in terms of poverty incidence, and not before 2034 in terms of consumption.

Table 5: **Simulated convergence speed.**

		Year	Difference	Std error
Panel A: Consumption (in log)	Observed	1998	0.228***	0.049
		2007	0.235***	0.062
		2012	0.246***	0.077
	Simulated:	...	...	...
		2033	*0.303**	*0.183*
		2034	*0.306*	*0.191*
Panel B: Poverty	Observed	1998	-0.115***	0.030
		2007	-0.115***	0.029
		2012	-0.078**	0.032
	Simulated:	...	...	...
		2016	*-0.057**	*0.034*
		2017	*-0.053*	*0.034*
Panel C: Food Poverty	Observed	1998	-0.122***	0.032
		2007	-0.104***	0.031
		2012	-0.071**	0.033
	Simulated:	*2013*	*-0.066**	*0.034*
		2014	*-0.061**	*0.034*
		2015	*-0.056*	*0.035*

Differences of means (and their standard errors) between households exposed and not exposed to violence as displayed in Table 4 for years 1998, 2007 and 2012. *Simulated* figures for the subsequent years assume that the yearly growth rates of the differences and of their standard errors after 2012 are equal to those observed between 2007 and 2012. ***p<0.01, **p<0.05, *p<0.1.

These results should not be pushed too much given the simplistic assumptions on which they rely. Still, they highlight the potentially long persistence of increased deprivation for people living in areas subject to violence. In terms of policy implications, this suggests that the targeting of these areas should be maintained sufficiently long after the end of violence – not only in the post-conflict recovery policies implemented in the short run, but also in the pro-poor policy tools designed in times of peace.

5 Violence and deprivation over time

As discussed in Section 1, the data at hand allow us to implement three empirical strategies in order to mitigate the endogeneity biases in our measurement of the correlation between household deprivation and exposure to violence over time.

First, we control simultaneously for (i) *year* dummies to account for the general trend in poverty in Burundi, (ii) *province* dummies to account for time-invariant unobserved heterogeneity across provinces, and (iii) *province* x *year* dummies to account for province-level trends. We estimate:

$$Y_{i,j,p,t} = \alpha + \beta ViolenceExposure_{j,t-1,t} + \delta_t + \lambda_p + \gamma_{p,t} + \varepsilon_{i,j,p,t} \quad (1)$$

with i indexing households, j indexing localities, p indexing provinces and t indexing time. Index t can take three values: 1998, 2007 and 2012. Note that since $ViolenceExposure_{j,t-1,t}$ is measured at the locality-level, there is intra-province variation in exposure to violence, which enables us to control for *province* and *province* x *year* fixed effects.

We explore three different dependent variables $Y_{i,j,p,t}$ to capture deprivation: *Poverty status*, *Food Poverty status*, and *Consumption (in log)*. $ViolenceExposure_{j,t-1,t}$ is the dummy variable equal to one if at least one war-related death or casualty was reported in locality j between $t-1$ and t.[16] Two periods of violence are observed: 1993 – 1998 and 1999 – 2007. $ViolenceExposure_{j,2008-2012}$ is equal to zero for all j.

Second, we introduce household-level instead of province-level fixed effects while still controlling for time and province-specific time trends. This allows to rule out the time-invariant household-level characteristics that affect consumption patterns, and leads us to measure the *within-household* correlation between violence exposure and deprivation over time by estimating:

$$Y_{i,j,p,t} = \alpha + \beta ViolenceExposure_{j,t-1,t} + \delta_t + \gamma_{p,t} + \eta_i + \varepsilon_{i,j,p,t}. \quad (2)$$

Last, we re-estimate Equation 2 adding a vector $X_{i,t}$ of household-level time-varying controls.

[16] The same specifications are then run using a continuous measure of the intensity of violence – namely, the logarithm of one plus the number of war-related deaths and casualties – in Appendix 9.6.

We control for the number of members, the age and gender of the household's head. We also introduce a dummy indicating whether the head of the household has some education (versus no education whatsoever) and two dummies capturing her occupation (agriculture with at least one export crop and non agricultural occupation, agriculture with no export crop being the omitted category). We finally control for a dummy indicating whether the household migrated between two survey waves. Migration is defined based on the *commune* of residence, i.e. a household is defined as migrant if it changes of *commune* between two waves.[17] Around 10% of the sample migrate between each wave (see Table 20 in Appendix 9.5). To investigate how migration is associated with poverty dynamics and exposure to violence, Tables 21 and 22 in Appendix 9.5 display, respectively, the average differences in deprivation and in exposure to violence between migrants and non-migrants, before and after migration takes place. They suggest poor correlations between household migration across *communes* and the pattern of violence, as well as poverty dynamics, over the period of study.

Although *Poverty status* and *Food Poverty status* are binary variables, we display the results of linear probability models so as to keep all the observations in the sample, but conditional Logit regressions yield comparable results.[18] Finally, for each dependent variable and specification, we first estimate Equations 1, 2 and 2 with controls over the sub-sample of original households (Table 6), and second over the complete sample gathering original and split-off households (Table 7). In the latter case, we allocate to split-offs the consumption, poverty and food poverty statutes of their household of origin in 1998, and we additionally control for a split-off dummy.[19]

Focusing on original households, Table 6 reveals a significant, robust correlation between violence exposure and deprivation. The results of Column (1) first show that, within each province, households living in exposed localities are characterized by a subsequently higher chance of being poor than households living in non-exposed localities, when controlling for national and province-specific time trends. This result remains stable when we introduce household-level fixed effects in Column (2): the *within-household* variation in locality-level exposure to violence is also signifi-

[17] The territory is composed of *sous-collines*, *collines*, *communes*, and *provinces*. The *Migrant* dummy thus does not consider as migrants households who changed of *sous-colline* or *colline* but stayed in the same *commune*. This choice is based on the context under study, movements within the same *commune* being very frequent, notably on a temporary basis.

[18] Not shown for brevity, available upon request.

[19] Note that this variable is always equal to zero in 1998.

cantly correlated with poverty. We thus cannot suspect that the result of Column (1) only stems from exposed households having unobserved (time-invariant) characteristics that also make them more likely to be poor. The source of heterogeneity between households which remains potentially confounding is time-varying. In order to partially account for this aspect, Column (3) introduces household-level controls. This does not turn out to affect the coefficient of interest. More specifically, the point estimates in Columns (1) to (3) of Table 6 suggest that the switch of a locality from the non-exposed to the exposed status is associated with a 6 to 11 percentage points increase in households' likelihood to be poor in average.

The results on food poverty status and consumption are very much in line with those on poverty. Specifically, the point estimates suggest that the switch of a locality from the non-exposed to the exposed status is associated, in average, with a 5 to 9 percentage points increase in a household's likelihood to live below the food poverty line (Columns (4) to (6)), and with a 11.5 to 15.5% decrease in a household's level of consumption (Columns (7) to (9)).

The coefficients associated with the control variables in Columns (3), (6) and (9) are consistent with the previous literature. In particular, deprivation is significantly correlated with the number of members: larger households tend to consume less and are more likely to be (food-)poor. Households headed by a woman are worse off on average, while the opposite holds for households whose head is older, has some education, and works in the non-agricultural sector or in agriculture with at least one culture of exportation (as opposed to agriculture without any export crop).

The results on the complete sample displayed in Table 7 are consistent with those of Table 6, with a positive correlation between deprivation and violence, but they are smaller in magnitude and less robustly significant. In particular, the correlation between violence exposure and consumption is not significantly different from zero as soon as household-level heterogeneity is accounted for. Adding split-off households to the sample thus makes the correlation between violence exposure and deprivation shrink, which suggests that splitting-off from the original households could be a strategy to mitigate the adverse consequences of violence exposure.[20]

[20] See Verwimp (2017) for a companion paper on the determinants of household splitting in Burundi.

Table 6: **Violence exposure and deprivation over time – Original households.**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Poverty status			Food Poverty status			Consumption (in log)		
Violence exposure (d)	0.0613**	0.0910**	0.109***	0.0484*	0.0724*	0.0902**	-0.115**	-0.116†	-0.154**
	(0.0254)	(0.0355)	(0.0336)	(0.0292)	(0.0406)	(0.0394)	(0.0437)	(0.0708)	(0.0712)
Number of members			0.0534***			0.0512***			-0.110***
			(0.00737)			(0.00797)			(0.0150)
Head – Age			-0.00153			-0.00198			0.00722**
			(0.00198)			(0.00196)			(0.00360)
Head – Female			0.123**			0.0885			-0.225**
			(0.0530)			(0.0536)			(0.108)
Head – Educ			-0.0673**			-0.0527			0.105
			(0.0306)			(0.0332)			(0.0747)
Head – AgrExp			0.00193			-0.00478			0.114*
			(0.0302)			(0.0315)			(0.0682)
Head – NonFarm			-0.0287			-0.00932			0.243**
			(0.0636)			(0.0603)			(0.117)
Migrant			-0.00519			0.0210			-0.0490
			(0.0452)			(0.0515)			(0.101)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province dummies	Yes	No	No	Yes	No	No	Yes	No	No
Year x Province dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HH fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Observations	2,289	2,289	2,289	2,289	2,289	2,289	2,289	2,289	2,289
R-squared	0.104	0.081	0.127	0.107	0.093	0.130	0.113	0.084	0.137

Robust standard errors clustered at the level of localities in parentheses. ***p<0.01, **p<0.05, *p<0.1, †p=0.103.
Head – Educ equals one if the household head ever went to school, zero otherwise. Head – AgrExp equals one if the household head's main occupation is agriculture with at least one export crop, zero otherwise. Head – NonFarm equals one if the household head's main occupation is not related to agriculture, zero otherwise. The omitted occupation is agriculture without export crop. Migrant equals one if the household changed *commune* of residence between two waves, zero otherwise.

Table 7: **Violence exposure and deprivation over time – Complete sample.**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Poverty status			Food Poverty status			Consumption (in log)		
Violence Exposure (d)	0.0496**	0.0511†	0.0717**	0.0508*	0.0558	0.0764**	-0.108**	-0.0477	-0.0856
	(0.0234)	(0.0316)	(0.0294)	(0.0292)	(0.0392)	(0.0373)	(0.0436)	(0.0584)	(0.0593)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province dummies	Yes	No	No	Yes	No	No	Yes	No	No
Year x Province dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HH fixed effects	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
HH controls	No	No	Yes	No	No	Yes	No	No	Yes
Observations	3,182	3,182	3,182	3,182	3,182	3,182	3,182	3,182	3,182
R-squared	0.095	0.068	0.118	0.094	0.069	0.115	0.100	0.067	0.132

Robust standard errors clustered at the level of localities in parentheses. ***p<0.01, **p<0.05, *p<0.1, †p=0.109.
HH controls are the same as in Table 6, namely: Head – Age, Head – Female, Head – Educ, Head – AgrExp, Head – NonFarm, and Migrant. In addition we control for a dummy for split-off households.

We develop two robustness tests in Appendix 9.6. *First*, in Table 23 we employ a measure of violence intensity (the logarithm of one plus the number of war-related deaths and casualties), rather than incidence. For brevity, we only display the results of our most demanding specification (i.e., with household-level fixed effects and time-varying controls), both on the sub-sample of original households and on the complete sample. They are highly consistent with those of Tables 6 and 7 and confirm the correlation between exposure to violence and poverty, food poverty, and consumption.

Second, Table 24 mobilizes the ESDSR survey data collected by the UNPF in 2002 to check whether comparable results emerge from this alternative sample of households. As explained in Section 2, the 2002 survey is not a panel. Yet, households were both asked their current livestock and their livestock in 1993, which allows us to observe the evolution of this variable between two points in time, the major advantage of this external data source being that it provides pre-exposure information. Table 24 displays the results of the estimation of the following equation:

$$Livestock_{i,c,p,t} = \alpha + \beta ViolenceExposure_{c,t} + \theta X_{i,t} + \delta_t + \gamma_{p,t} + \eta_i + \varepsilon_{i,c,p,t}, \tag{3}$$

with i indexing households, c indexing *communes*, p indexing provinces and t indexing time. Index t can take two values here: 1993 and 2002. The 2002 survey provides information on households' mobility over the 1993 – 2002 period. This allows us to control for a vector $X_{i,t}$, which gathers two variables: a dummy indicating whether household i migrated between 1993 and 2002 (as in our benchmark data, migration is defined as a change in the *commune* of residence), and a dummy indicating whether household i spent time in a displacement camp over the period. Both variables are always equal to zero in 1993.

In 1993, $ViolenceExposure_{c,t}$ is always equal to zero, as the war had not started yet. In 2002, $ViolenceExposure_{c,t}$ takes the value one if at least one death or casualty was reported in *commune* c after 1993. As noticed in Section 2, the 2002 survey can only be matched to our violence data at the *commune* level. Moreover, as our data on violence exposure are at the period level, in 2002 $ViolenceExposure_{c,t}$ is equal to one if there was at least one death or casualty over the whole 1993 – 2007 period. Thus, in this test our measurement of violence exposure looses in terms of both geographic and historical preciseness. In particular, one of the 45 *communes* of the sample

was not exposed before 1998 but exposed over the 1999 – 2007 period. If violence occurred after 2002 in this *commune*, we wrongly consider it as exposed in this specification. For robustness, we drop the households living in this *commune* in Column (3) of Table 24. The results of Table 24 corroborate our benchmark findings, pointing out to a strong *within-household* negative correlation between violence exposure and livestock ownership. As in our benchmark estimations, the variable $Migrant$ is never significantly correlated with the dependent. On the other hand, the variable $Camp$, which is not available in our panel, does happen to be negatively correlated with livestock ownership in Column (2). While this correlation is not surprising, it is reassuring to see that including this additional control does not affect the coefficient associated with violence exposure.

6 The dynamics of poverty

We aim here at going further in the analysis of the interaction between violence exposure and the household-level dynamics of poverty by investigating how violence correlates with the patterns of transition into and out of (food-)poverty. Specifically, we document how households' exposure to violence is related to their subsequent likelihood to switch (food-)poverty status, i.e. become (food-)poor while they used to be non-(food-)poor in 1998 and *vice versa*. We estimate the following equations:

$$Switch_{i,j,p} = \alpha + \beta ViolenceExposure_{j,1999-2007} + \lambda_p + \varepsilon_{i,j,p}, \tag{4}$$

and

$$Switch_{i,j,p} = \alpha + \beta ViolenceExposure_{j,1999-2007} + \mu X_{i,1998} + \lambda_p + \varepsilon_{i,j,p}, \tag{5}$$

with i indexing households, j indexing localities, and p indexing provinces.

The dependent variable, $Switch_{i,j,p}$, is a dummy equal to one if household i from locality j in province p has seen its (food-)poverty status switch after 1998, as compared to its status in 1998. Said differently, $Switch_{i,j,p}$ equals one for households who were (food-)poor in 1998 and became non-(food-)poor in 2007 and/or 2012, and for households who were non-(food-)poor in 1998 and became (food-)poor in 2007 and/or 2012. It is equal to zero for the chronic (food-)poor and for the permanent non-(food-)poor.

Since transitions into and out of poverty are likely to follow different patterns, and since chronic poor and permanent non-poor do not form a homogenous group for comparison, we estimate Equations 4 and 5 separately over the sub-samples of initially poor and initially non-poor households.[21] *First*, we analyze the likelihood to escape poverty of households who were poor in 1998, by comparing those who remained in their initial poverty status to those who subsequently became non-poor (regardless of whether it happened in 2007 or in 2012). Over the 724 households of the sample who were poor in 1998, 332 (45.86%) became non-poor afterwards. *Second*, we investigate the determinants of the risk to fall into poverty by comparing households who were non-poor in 1998 and remained so in 2007 and 2012, to households who were non-poor but fell into poverty in 2007 or 2012. Over the 272 households of the sample who were non-poor in 1998, 214 (78.68%) became poor afterward. The variable on which the sample is split being measured in 1998, we focus on the correlation between poverty switches and post-1998 violence. In Equation 5, we additionally control for the vector $X_{i,1998}$ of household characteristics measured in 1998 (except the *Split-off* and *Migrant* dummies which are only revealed afterwards).

As in the panel analysis, we consider successively poverty and food poverty. We also first estimate Equations 4 and 5 over original households (Table 8), and then over the complete sample (Table 9).[22] In both tables, Columns (1) and (2) ((5) and (6)) focus on transitions out of (food-)poverty and Columns (3) and (4) ((7) and (8)) on transitions into (food-)poverty. We display the results of linear probability models but Logit regressions yield virtually similar results.[23]

Households who were poor in 1998 and exposed to violence between 1999 and 2007 appear to be less likely to escape poverty, while households who were non-poor in 1998 and exposed to violence between 1999 and 2007 are more likely to fall into poverty, but the latter correlation is statistically significant while the former is not. The same pattern is observed for food poverty.

[21] The endogeneity of households' poverty status in 1998 to exposure to violence over the 1993 – 1998 period is discussed below.

[22] In each case, the sample of analysis gathers households whose poverty status is available in 1998, 2007 and 2012. There are 996 such households, among whom 760 are original households.

[23] Not shown for brevity, available upon request.

Table 8: **Violence and poverty status switches – Original households.**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Out of poverty		Into poverty		Out of food poverty		Into food poverty	
Violence exposure, 1999 – 2007 (d)	-0.0796	-0.0723	0.213***	0.185***	-0.0159	-0.0110	0.132**	0.123**
	(0.0510)	(0.0550)	(0.0560)	(0.0577)	(0.0724)	(0.0722)	(0.0637)	(0.0575)
Number of members		-0.0133		0.0117		-0.0219*		0.00539
		(0.00992)		(0.0119)		(0.0121)		(0.0110)
Head – Age		0.00387**		-0.00100		0.00367*		-0.00121
		(0.00166)		(0.00202)		(0.00191)		(0.00165)
Head – Female		0.00879		-0.00606		-0.000492		-0.0496
		(0.0621)		(0.0737)		(0.0651)		(0.0674)
Head – Educ		0.120**		-0.101*		0.151***		-0.166***
		(0.0469)		(0.0579)		(0.0529)		(0.0560)
Head – AgrExp		0.0111		-0.0544		0.0188		-0.104
		(0.0569)		(0.0785)		(0.0664)		(0.0691)
Head – NonFarm		0.0667		-0.240**		0.0631		-0.297***
		(0.0920)		(0.109)		(0.118)		(0.0916)
Migrant		-0.00645		-0.0975		0.0463		-0.0607
		(0.0613)		(0.0793)		(0.0700)		(0.0670)
Province dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	526	526	234	234	406	406	334	334
R-squared	0.054	0.078	0.163	0.224	0.080	0.110	0.078	0.165

Robust standard errors clustered at the level of localities in parentheses. ***p<0.01, **p<0.05, *p<0.1.

HH controls are measured in 1998. Head – Educ equals one if the household head ever went to school, zero otherwise. Head – AgrExp equals one if the household head's main occupation is agriculture with at least one export crop, zero otherwise. Head – NonFarm equals one if the household head's main occupation is not related to agriculture, zero otherwise. The omitted occupation is agriculture without export crop. Migrant equals one if the household changed *commune* of residence between two waves, zero otherwise.

Columns (1) and (2) (respectively, (5) and (6)) estimate the determinants of the probability of transition out of poverty (respectively, food poverty) over the sample of households who were poor (respectively, food-poor) in 1998. Columns (3) and (4) (respectively, (7) and (8)) estimate the determinants of the probability of transition into poverty (respectively, food poverty) over the sample of households who were non poor (respectively, non food-poor) in 1998.

Table 9: **Violence and poverty status switches – Complete sample.**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Out of poverty		Into poverty		Out of food poverty		Into food poverty	
Violence exposure, 1999 – 2007 (d)	-0.0820*	-0.0835*	0.186***	0.164***	-0.0620	-0.0660	0.109**	0.106**
	(0.0423)	(0.0457)	(0.0506)	(0.0524)	(0.0582)	(0.0616)	(0.0533)	(0.0489)
Province dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HH controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	724	724	272	272	570	570	397	397
R-squared	0.056	0.081	0.141	0.193	0.075	0.113	0.074	0.151

Robust standard errors clustered at the level of localities in parentheses. ***p<0.01, **p<0.05, *p<0.1.
HH controls are measured in 1998. They are the same as in Table 8, namely: Head – Age, Head – Female, Head – Educ, Head – AgrExp, Head – NonFarm, and Migrant. In addition we control for a dummy for split-off households.
Columns (1) and (2) (respectively, (5) and (6)) estimate the determinants of the probability of transition out of poverty (respectively, food poverty) over the sample of households who were poor (respectively, food-poor) in 1998. Columns (3) and (4) (respectively, (7) and (8)) estimate the determinants of the probability of transition into poverty (respectively, food poverty) over the sample of households who were non poor (respectively, non food-poor) in 1998.

The vector of households' characteristics performs less well in explaining poverty status switches than in explaining the deprivation variables in Table 6. This might stem from the fact that we are only controlling for initial characteristics, while Table 6 documents the joint evolution of socioeconomic and demographic characteristics and deprivation. Still, the coefficients that are significantly correlated with the likelihood of poverty transitions are consistent with Table 6. In particular, poor households whose head is educated, or older, are more likely to escape poverty, and non-poor households whose head has a non-agricultural occupation are less likely to fall into poverty.

Table 9 displays the same specifications over the complete sample, i.e. pooling together original and split-off households. The results are very consistent with those of Table 8. Violence exposure is negatively correlated with the chances of (food-)poor households to pull through, and positively correlated with the chances of non-(food-)poor households to fall into poverty, but the latter is more robustly significant than the former.

As compared to the benchmark panel specification, these transition analyzes suffer from additional endogeneity threats. Indeed, while time-varying unobservable household characteristics could still play as confounders, it is also the case of time-invariant locality- and household-level unobservables which, here, cannot be accounted for by fixed effects. Moreover, the (food-)poverty status of 1998, on which we split the sample, is likely to be affected by pre-exposure to violence, which would generate an endogeneity bias and make it difficult to draw conclusions from our empirical results. The major concern here is that households who were (food-)poor in 1998 are more likely to have been exposed to violence before 1998, on average, than households who were non-(food-)poor in 1998. This generates asymmetry in violence exposure across the two sub-samples: post-1998 exposure is more likely to be a violence *re*-exposure for households who are (food-)poor in 1998, and more likely to be a violence *first* exposure for households who are not (food-)poor in 1998. Indeed, the results of Tables 8 and 9 are compatible with an interpretation distinguishing first and re-exposure to violence. In particular, the low correlation between post-1998 exposure to violence and the chances to escape (food-)poverty (Columns (1)-(2)-(5)-(6)) could be interpreted in terms of capacity of resilience: if those who were (food-)poor in 1998 are very likely to have already been exposed to violence, the cumulative violence that they suffer over the second period does not signif-

icantly hamper their chances to escape poverty. On the other hand, the strong correlation between post-1998 exposure to violence and the risk to fall into (food-)poverty (Columns (3)-(4)-(7)-(8)) could reflect the impact of a first violence shock.

To document this point, we check how the results are affected if we separate households who were not exposed to violence over the 1993 – 1998 period and households who were. In the first case, regardless of the poverty status in 1998, violence exposure over the 1999 – 2007 period is a first exposure, while in the second one it is a cumulative exposure. The results are displayed in Table 25 in Appendix 9.6. They are very similar to those of Tables 8 and 9, which does not point to an important differential in the correlation between poverty transitions and first or cumulative violence exposure. Although we fully acknowledge the endogeneity of the (food-)poverty status in 1998 to pre-1998 exposure, our results thus do not seem to be fully explained by this weakness of the specification.

As in Section 5, Table 26 in Appendix 9.6 focuses on the intensity of violence proxied by the logarithm of one plus the number of war-related deaths and casualties (for brevity, and since the results are very similar, we only display the specification controlling for households' initial characteristics). As in the case of violence incidence, the results on original households and those on the complete sample are very much alike. On the other hand, while the signs of the coefficients of interest remain the same as in Tables 8 and 9, their statistical significance does not. *First*, while escaping food poverty is still non significantly correlated with violence, escaping poverty is significantly correlated with exposure measured through the intensity of violence. *Second*, the coefficient associated with violence intensity is less significant than the coefficient associated with violence incidence when descents into (food-)poverty are considered. The intensity of violence exposure remains statistically significantly linked to descents into poverty (though to a lesser extent), but this is not the case of descents into food-poverty. Since violence intensity is likely to be more subject to measurement errors than violence incidence, we are not surprised that the coefficients are less precisely estimated when this measure is used, which can explain the observed lower level of statistical significance. However, the fact that escaping from poverty is significantly correlated with violence intensity while it is not significantly associated with violence incidence is surprising.

To shed light on this difference between the results on incidence and the results on intensity captured by the logged number of war-related deaths and casualties, Tables 10 and 11 use a categorical measure of violence intensity. Namely, we distinguish lower-or-equal-to-median and higher-than-median violence intensity, households in unexposed localities being the reference category. Between 1999 and 2007, the median number of war-related deaths and casualties in exposed localities is equal to 9. The results explain the differences observed between Tables 8-9 and Table 26 by pointing out the fact that the correlation between violence intensity and poverty switches is non-linear, and that this non-linearity is heterogenous across initial poverty statutes. Namely, the capacity of initially (food-)poor households to escape (food-)poverty appears to be hampered by exposure to high-intensity violence. On the other hand, the likelihood that initially non-(food-)poor households fall into (food-)poverty is linked to exposure to relatively low levels of violence intensity. Consistently with Table 26, the correlations between violence and poverty transitions are generally more strongly significant than those between violence and food-poverty transitions.[24]

Last, although households' (food-)poverty status is endogenous to pre-exposure status, the results of Tables 10 and 11 do not seem to call for an interpretation in terms of first versus cumulative exposure to violence. Focusing on the chances to pull through for initially (food-)poor households (Columns (1)-(2)-(5)-(6)), it is arguable that households who have already been exposed once can more easily absorb a low-intensity shock than a high-intensity shock. However, we do not see any theoretical reason to expect that first exposure to violence is very strongly correlated with initially non-(food-)poor households' risk to fall into (food-)poverty mostly when it is of relatively low (versus relatively high) intensity.

[24]The results of Tables 10 and 11 are robust to implementing Logit instead of OLS regressions. Results available upon request.

Table 10: **Violence intensity categories and poverty status switches – Original households.**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Out of poverty		Into poverty		Out of food poverty		Into food poverty	
Violence exposure, 1999 – 2007 ≤ median	0.0123	0.0136	0.263***	0.240***	0.0899	0.0841	0.175**	0.167**
	(0.0626)	(0.0645)	(0.0563)	(0.0612)	(0.101)	(0.102)	(0.0702)	(0.0636)
Violence exposure, 1999 – 2007 > median	-0.198***	-0.184**	-0.0733	-0.0962	-0.142*	-0.125*	-0.0592	-0.0574
	(0.0667)	(0.0740)	(0.120)	(0.127)	(0.0723)	(0.0718)	(0.112)	(0.112)
Province dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HH controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	526	526	234	234	406	406	334	334
R-squared	0.062	0.084	0.178	0.238	0.089	0.118	0.085	0.171

Robust standard errors clustered at the level of localities in parentheses. ***p<0.01, **p<0.05, *p<0.1.
HH controls are measured in 1998. They are the same as in Table 8, namely: Head – Age, Head – Female, Head – Educ, Head – AgrExp, Head – NonFarm, and Migrant.
Columns (1) and (2) (respectively, (5) and (6)) estimate the determinants of the probability of transition out of poverty (respectively, food poverty) over the sample of households who were poor (respectively, food-poor) in 1998. Columns (3) and (4) (respectively, (7) and (8)) estimate the determinants of the probability of transition into poverty (respectively, food poverty) over the sample of households who were non poor (respectively, non food-poor) in 1998.

Table 11: **Violence intensity categories and poverty status switches – Complete sample.**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Out of poverty		Into poverty		Out of food poverty		Into food poverty	
Violence exposure, 1999 – 2007 ≤ median	-0.0309	-0.0276	0.226***	0.201***	0.00579	0.00286	0.136**	0.128**
	(0.0492)	(0.0513)	(0.0524)	(0.0574)	(0.0813)	(0.0848)	(0.0590)	(0.0536)
Violence exposure, 1999 – 2007 > median	-0.140**	-0.148**	-0.0466	-0.0321	-0.133**	-0.140**	-0.0149	0.0136
	(0.0559)	(0.0606)	(0.101)	(0.0993)	(0.0544)	(0.0604)	(0.0902)	(0.0866)
Province dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HH controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	724	724	272	272	570	570	397	397
R-squared	0.058	0.083	0.150	0.199	0.078	0.117	0.077	0.152

Robust standard errors clustered at the level of localities in parentheses. ***p<0.01, **p<0.05, *p<0.1.
HH controls are measured in 1998. They are the same as in Table 8, namely: Head – Age, Head – Female, Head – Educ, Head – AgrExp, Head – NonFarm, and Migrant. In addition we control for a dummy for split-off households.
Columns (1) and (2) (respectively, (5) and (6)) estimate the determinants of the probability of transition out of poverty (respectively, food poverty) over the sample of households who were poor (respectively, food-poor) in 1998. Columns (3) and (4) (respectively, (7) and (8)) estimate the determinants of the probability of transition into poverty (respectively, food poverty) over the sample of households who were non poor (respectively, non food-poor) in 1998.

7 Discussion

Two main empirical results emerge:

(i) Poverty and food poverty (respectively, consumption) are positively (respectively, negatively) correlated with violence exposure and intensity. This correlation, which persists years after the end of exposure, is robust to *within-household* estimations.

Although our empirical analysis does not rely on a causal identification strategy, the observed strong correlation between deprivation and conflict is very important to emphasize. As discussed in Section 1, quantitative evidence on the micro-level economic legacy of civil wars is relatively scarce. The data mobilized here allow to shed light on this issue and point to a strong and lasting correlation between exposure to violence and household deprivation. In terms of policy implications, if this result was to be confirmed by analyzes of other contexts, it would motivate an enlargement of the scope of post-conflict recovery policies. In particular, in addition to short-term policies focusing notably on disarmament, reconciliation, and the reconstruction of institutions and physical infrastructures, the targeting of households living in localities which were exposed to violence over the past would be recommended to become a component of any pro-poor policy in the longer run.

(ii) The household-level poverty dynamics following the recent period of violence (1999 – 2007) reveals that the likelihood to pull through of (food-)poor households is hampered by exposure to high-intensity violence, while the risk to fall into (food-)poverty of non-(food-)poor households is fostered by exposure to low-intensity violence.

While result (ii) is based on a weaker specification than result (i), in particular because the initial (food-)poverty status is affected by pre-1998 violence exposure, this does not seem to be sufficient to explain the empirical results. On the other hand, we argue that one mechanism which could explain the heterogeneity of the consequences of violence on poverty transitions across initial level of deprivation and intensity of violence is the destruction of physical capital. As discussed in the literature, multiple channels link violence exposure to household poverty, and the micro-level effects of violence on households' deprivation should depend on household characteristics and on the nature of violence. If labour is more destroyed (meaning, people killed), it is likely that poor households suffer relatively more in terms of deprivation than non-poor households, as the former only have

their labour force to live off. If physical capital is more affected (assets destroyed), the non-poor are likely to suffer relatively more. Our data do not allow us to measure the intensity of physical capital destruction entailed by the war. However, one could argue that our proxies for low-intensity and high-intensity violence somehow differentiate events which primarily involved the destruction of physical capital (with less than 9 war-related deaths and casualties) and events which were very labour-destructive (with 10 or more deaths or casualties). In that sense, the results would point to the heterogenous vulnerability of households to different types of violence, poorer households being less sensitive to the destruction of physical capital than richer households. Such an interpretation brings support to the idea expressed in previous literature that households' endowment shapes their vulnerability to conflict.

In the case of Burundi, the relative intensity of the destruction of labour versus physical capital seems to have changed over the course of the war, from more destructive in terms of labour to more destructive in terms of physical capital. The UN Security Council reports on Burundi of 2004, 2005 and 2006 document such a shift (United Nations, 2004, 2005, 2006). The report of 2004, while signaling a general decrease in hostilities during the preceding years and significant progresses in achieving peace, underlines a rise in criminality. The year after, it emphasizes the persistence of banditry and looting, and in 2006 it highlights that "criminality has risen throughout Burundi, with widespread incidents of armed banditry, intimidation, looting". Consistently, Cazenave-Piarrot (2004) explains that the destruction of cattle significantly intensified after 1996 in comparison to what had been observed between 1993 and 1996. He underlines that cattle has constituted one of the main resources of the rebels, while, taking advantage of the climate of fear and impunity, criminal bands did ally with the rebels to loot cattle, food stocks, or shops. In the perspective of such a changing nature of violence, our results on the correlation between violence intensity and poverty dynamics after the second period of the war would allow to suppose that the poor have suffered relatively more in the first period of the conflict and the non-poor in the second period. Given that no pre-violence information on poverty is available for the first period of the war, and that the change in the relative intensity of labour and capital destruction cannot be directly captured in the data, this interpretation is purely tentative. Still, the asymmetry in households' economic

vulnerability to conflict depending on their initial characteristics that the data reveal does call for further investigation in other contexts, in order to confirm or infirm the mechanism based on the nature of violence that we suggest.

8 Conclusion

Locality-level data on war exposure during the whole conflict, combined with three survey waves with household tracking from 1998 to 2012, allow us analyze the joint evolution of violence and households' deprivation in Burundi. While the rate of poverty has been almost constant from 1998 to 2012, we observe that its severity – and, at the same time, food poverty and inequalities – have increased, and that numerous households experienced transitions into and out of poverty. This makes it crucial to understand the dynamics of poverty in order to enlighten post-conflict policy-making.

We find that households who suffered violence exposure are characterized by significantly higher deprivation. This difference persists over time and is still visible in 2012, more than seven years after the end of the war. Panel estimations controlling for national and province-specific time trends, as well as households' time-invariant unobservable characteristics and time-varying characteristics, confirm the adverse correlation between violence and deprivation over time.

Finally, the investigation of the determinants of descents into and escapes from (food-)poverty in the aftermath of the second period of the war suggests that the risk to fall into (food-)poverty for initially non-(food-)poor households was significantly boosted by low-intensity violence, while (food-)poor households' chances to pull through were mostly correlated with high-intensity violence exposure. One mechanism that may explain this asymmetric result is related to the nature of violence, relatively intense labour destruction amplifying predominantly the vulnerability of poor households and relatively intense physical capital destruction being likely to mostly affect initially non-poor households.

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9 Appendix

9.1 Investigating potential endogeneity with external data

Table 12: **Determinants of locality-level violence exposure.**

Dependent variable:	(1)	(2)	(3)
Violence exposure dummy	1993 – 1998	1999 – 2007	
Distance to Bujumbura	0.0782	0.0555	0.0909
	(0.194)	(0.207)	(0.207)
Altitude	0.643	0.370	0.371
	(0.491)	(0.488)	(0.472)
Literacy HHH – 1990 (prov)	0.0229*	0.00969	0.00425
	(0.0134)	(0.0152)	(0.0155)
Resources (log) – 1990 (prov)	-0.693	-0.665*	-0.568
	(0.494)	(0.386)	(0.406)
Population density – 1990	0.0450	-0.0703	-0.0688
	(0.125)	(0.122)	(0.117)
% Farmer HHH – 1990 (prov)	0.0320	0.0224	0.0225
	(0.0202)	(0.0149)	(0.0151)
% HHH under 30 – 1990 (prov)	0.0185	0.0162	0.0188
	(0.0175)	(0.0157)	(0.0154)
% HHH above 50 – 1990 (prov)	-0.0188	0.0117	0.0161
	(0.0179)	(0.0237)	(0.0227)
Share votes Ndadaye – 1993	0.00385	0.000427	-0.000559
	(0.00337)	(0.00317)	(0.00317)
% HHH women – 1998			-0.00246
			(0.00252)
Socioeconomic homogeneity – 1998			0.0832*
			(0.0441)
Observations	94	94	93
R-squared	0.250	0.195	0.230

OLS estimations. Robust standard errors in parentheses. ***p<0.01, **p<0.05, *p<0.1
Literacy HHH measures the share of household heads who are literate. Resources (log) is the average yearly amount of resources per household. % HHH under 30 – 1990 (respectively, % HHH above 50 – 1990) is the share of household heads aged under 30 (respectively, aged above 50). Share votes Ndadaye – 1993 is the share of votes obtained by Melchior Ndadaye, whose assassination a few months after his arrival in power triggered the beginning of the war in 1993.
Distance to Bujumbura, Altitude, Population density – 1990, Share votes Ndadaye – 1993, % HHH women – 1998 and Socioeconomic homogeneity – 1998 are provided by Voors et al. (2012) at the level of the localities. Literacy HHH – 1990, Resources (log) – 1990, % HHH farmer – 1990, % HHH under 30 – 1990 and % HHH above 50 – 1990 are provided by ISTEEBU (1993) at the province level.

Table 13: **Household-level pre-violence livestock, difference of means by future exposure status of the commune of residence.**

Status of the commune of residence 1993 – 1998:	Not exposed (15 communes)	Exposed (30 communes)	Difference
Number of livestock heads in 1993	9.275	9.095	0.180 (0.612)
Number of households	*950*	*2,211*	*3,161*
Status of the commune of residence 1999 – 2007:	Not exposed (33 communes)	Exposed (12 communes)	Difference
Number of livestock heads in 1993	9.097	9.333	-0.236 (0.679)
Number of households	*2,471*	*690*	*3,161*

Significance of the difference between households living in exposed and non-exposed *communes* using a paired *t-test*. ***p<0.01, **p<0.05, *p<0.1. Standard errors in parentheses.
Violence exposure is computed at the *commune*-level based on our data. Number of livestock heads in 1993 measures the number of heads of cattle, sheep, poultry owned by households in 1993 and is provided by the ESDSR survey (UNPF, 2002).

9.2 Testing for selective attrition

Table 14: **Differences of means between tracked and non-tracked households.**

Characteristics in 1998	Tracked in 2007	Drop out in 2007	Difference
Number of members	5.115	4.371	0.743***
Head – Age	40.36	50.97	-10.60***
Head – Female	0.220	0.347	-0.127***
Head – Educ	0.343	0.282	0.061
Head – AgrNoExp	0.312	0.361	-0.050
Head – AgrExp	0.605	0.520	0.085**
Head – NonFarm	0.084	0.119	-0.035
Consumption (in log)	8.611	8.633	-0.022
Poverty status	0.696	0.673	0.023
Food Poverty status	0.571	0.569	0.002
Violence exposure, 1993 – 1998 (d)	0.588	0.450	0.138***
Violence exposure, 1999 – 2007 (d)	0.181	0.144	0.037
Number of households	*741*	*202*	*943*
Characteristics in 2007	Tracked in 2012	Drop out in 2012	Difference
Number of members	5.314	4.870	0.444***
Head – Age	42.25	42.78	-0.529
Head – Female	0.225	0.169	0.056**
Head – Educ	0.441	0.455	-0.013
Head – AgrNoExp	0.452	0.497	-0.045
Head – AgrExp	0.455	0.370	0.085**
Head – NonFarm	0.093	0.133	-0.040*
Migrant	0.092	0.101	-0.009
Consumption (in log)	8.487	8.493	-0.006
Poverty status	0.685	0.701	-0.016
Food Poverty status	0.605	0.607	-0.002
Violence exposure, 1993 – 1998 (d)	0.595	0.519	0.075**
Violence exposure, 1999 – 2007 (d)	0.184	0.188	-0.005
Number of households	*730*	*308*	*1,038*

Significance of the differences using a paired t-test. ***p<0.01, **p<0.05, *p<0.1.
Head – Age is the age of the household head, Head – Female is a dummy equal to one is the head is a woman, Head – Educ is a dummy equal to one if the household head ever went to school, Head – AgrNoExp is a dummy equal to one if the household head's main occupation is agriculture with no export crop, Head – AgrExp is a dummy equal to one if the household head's main occupation is agriculture with at least one export crop, Head – NonFarm is a dummy equal to one if the household head's main occupation is not related to agriculture.

9.3 Inequality

Table 15: **Gini coefficients by household type and year.**

Type of household	**Year**		
	1998	**2007**	**2012**
All	0.360	0.485	0.613
Original households		0.488	0.651
Split-offs		0.473	0.521

9.4 Split-off households

Table 16: **(Food-)Poverty transition matrices: split-offs only.**

Panel A. 1998-2007.

	2007	
1998	Non Poor	Poor
Non Poor	43.18	56.82
Poor	34.05	65.95

	2007	
1998	Non Food-Poor	Food-Poor
Non Food-Poor	47.89	52.11
Food-Poor	40.98	59.02

Panel B. 2007-2012.

	2012	
2007	Non Poor	Poor
Non Poor	50.75	49.25
Poor	26.45	73.55

	2012	
2007	Non Food-Poor	Food-Poor
Non Food-Poor	52.44	47.56
Food-Poor	34.91	65.09

Panel C. 1998-2012.

	2012	
1998	Non Poor	Poor
Non Poor	44.44	55.56
Poor	34.62	65.38

	2012	
1998	Non Food-Poor	Food-Poor
Non Food-Poor	41.30	58.70
Food-Poor	43.80	56.20

Table 17: **Threefold (food-)poverty status in 1998, 2007 and 2012: split-offs only.**

	(1)	(2)	(3)	(4)
	Number of HH (%)			
	Poverty		Food poverty	
	Whole sample	Sub-sample	Whole sample	Sub-sample
Permanent non poor	23 (5.24)	9 (3.81)	37 (8.43)	11 (4.66)
Switchers	254 (57.86)	136 (57.63)	283 (66.74)	160 (67.80)
Chronic poor	162 (36.90)	91 (38.56)	109 (24.83)	65 (27.54)
Total	439 (100)	236 (100)	439 (100)	236 (100)

Table 18: **Differences in deprivation: original households versus split-offs.**

		Original HH	Original HH with future split-offs	Difference
Panel A: 1998	Consumption (in log)	8.616	8.382	0.234*** (0.051)
	Poverty	0.691	0.839	-0.147*** (0.030)
	Food Poverty	0.571	0.738	-0.168*** (0.033)
	Number of households	*943*	*279*	*1,222*
		Original HH	Split-offs	Difference
Panel B: 2007	Consumption (in log)	8.433	8.632	-0.199*** (0.068)
	Poverty	0.706	0.648	0.058* (0.032)
	Food Poverty	0.618	0.575	0.043 (0.034)
	Number of households	*751*	*287*	*1,038*
Panel C: 2012	Consumption (in log)	8.546	8.611	-0.065 (0.078)
	Poverty	0.694	0.645	0.049 (0.032)
	Food Poverty	0.627	0.581	0.046 (0.034)
	Number of households	*595*	*327*	*922*

Significance of the difference between original households and original households who are going to have split-off(s) in 2007 in Panel A, and of the difference between original and split-off households in Panels B and C, using a paired *t-test*. ***p<0.01, **p<0.05, *p<0.1. Standard errors in parentheses.

Table 19: **Violence exposure and deprivation: Split-offs only.**

		Not exposed to violence	Exposed to violence	Difference
Panel A: 1998	Consumption (in log)	8.559	8.234	0.325*** (0.082)
	Poverty	0.772	0.895	-0.123*** (0.044)
	Food Poverty	0.654	0.809	-0.156*** (0.052)
	Number of households	*127*	*152*	*279*
Panel B: 2007	Consumption (in log)	8.895	8.457	0.438*** (0.105)
	Poverty	0.548	0.715	-0.167*** (0.057)
	Food Poverty	0.470	0.645	-0.176*** (0.059)
	Number of households	*115*	*172*	*287*
Panel C: 2012	Consumption (in log)	8.739	8.532	0.207* (0.115)
	Poverty	0.576	0.688	-0.112** (0.054)
	Food Poverty	0.536	0.609	-0.073 (0.056)
	Number of households	*125*	*202*	*327*

Significance of the difference between exposed and non-exposed split-off households using a paired *t-test*. ***p<0.01, **p<0.05, *p<0.1. Standard errors in parentheses.

Violence exposure is computed over the past, i.e. between 1993 and 1998 for Panel A, and between 1993 and 2007 for Panels B and C.

9.5 Migrant households

Table 20: **Number of migrant households.**

	# Migrant HH (%)	Non-migrant HH (%)	Total (%)
1998	0 (0)	943 (100)	943 (100)
2007	98 (9.44)	940 (90.56)	1,038 (100)
2012	89 (9.65)	833 (90.35)	922 (100)

Table 21: **Differences in deprivation: migrant versus non-migrant households.**

Migration between 1998 and 2007		Non migrant	Migrant	Difference
Panel A: 1998 (pre-migration)	Consumption (in log)	8.625	8.511	0.114 (0.091)
	Poverty	0.688	0.733	-0.046 (0.056)
	Food Poverty	0.570	0.573	-0.003 (0.060)
	Number of households	*868*	*75*	*943*
Panel B: 2007 (post-migration)	Consumption (in log)	8.493	8.442	0.052 (0.104)
	Poverty	0.689	0.694	-0.005 (0.049)
	Food Poverty	0.605	0.612	-0.007 (0.052)
	Number of households	*940*	*98*	*1,038*
Migration between 2007 and 2012		Non migrant	Migrant	Difference
Panel C: 2007 (pre-migration)	Consumption (in log)	8.534	8.519	0.015 (0.109)
	Poverty	0.668	0.640	0.027 (0.053)
	Food Poverty	0.588	0.539	0.049 (0.055)
	Number of households	*828*	*89*	*917*
Panel D: 2012 (post-migration)	Consumption (in log)	8.576	8.509	0.067 (0.126)
	Poverty	0.676	0.685	-0.010 (0.052)
	Food Poverty	0.606	0.652	-0.045 (0.054)
	Number of households	*833*	*89*	*922*

Significance of the difference between migrant and non-migrant households using a paired *t-test.* ***p<0.01, **p<0.05, *p<0.1. Standard errors in parentheses.

Table 22: **Differences in violence exposure: migrant versus non-migrant households.**

Migration between 1998 and 2007		Non migrant	Migrant	Difference
Panel A: 1993 – 1998 (pre-migration)	Violence exposure (d)	0.574	0.551	0.023 (0.053)
	Violence exposure (level)	66.479	70.194	-3.715 (12.909)
	Violence exposure (log)	2.296	2.290	0.006 (0.237)
	Number of households	*940*	*98*	*1,038*
Panel B: 1999 – 2007 (post-migration)	Violence exposure (d)	0.176	0.197	-0.022 (0.046)
	Violence exposure (level)	3.159	2.895	0.264 (1.473)
	Violence exposure (log)	0.410	0.457	-0.046 (0.118)
	Number of households	*940*	*76*	*1,016*
Migration between 2007 and 2012		Non migrant	Migrant	Difference
Panel C: 1999 – 2007 (pre-migration)	Violence exposure (d)	0.164	0.225	-0.060 (0.042)
	Violence exposure (level)	3.425	2.427	0.998 (1.393)
	Violence exposure (log)	0.408	0.482	-0.074 (0.113)
	Number of households	*833*	*89*	*922*

Significance of the difference between migrant and non-migrant households using a paired *t-test.* ***p<0.01, **p<0.05, *p<0.1. Standard errors in parentheses.

9.6 Robustness tests

Table 23: **Violence exposure intensity and deprivation over time.**

	(1)	(2)	(3)	(4)	(5)	(6)
	Original households			Complete sample		
	Poverty	Food Poverty	Cons. (in log)	Poverty	Food Poverty	Cons. (in log)
Violence exposure (log)	0.0263***	0.0271***	-0.0456**	0.0148*	0.0201*	-0.0309*
	(0.00987)	(0.0102)	(0.0189)	(0.00886)	(0.0107)	(0.0172)
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Year x Province dummies	Yes	Yes	Yes	Yes	Yes	Yes
HH fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
HH controls	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,289	2,289	2,289	3,182	3,182	3,182
R-squared	0.126	0.131	0.138	0.117	0.115	0.133

Robust standard errors clustered at the level of localities in parentheses. ***p<0.01, **p<0.05, *p<0.1.
HH controls are the same as in Table 6, namely: Head – Age, Head – Female, Head – Educ, Head – AgrExp, Head – NonFarm, and Migrant.

Table 24: **Violence exposure and livestock ownership over time – external data source.**

	(1)	(2)	(3)
	Livestock ownership		
Violence exposure (d)	-2.293***	-2.393***	-2.302***
	(0.798)	(0.819)	(0.846)
Migrant (d)		1.957	1.888
		(1.479)	(1.524)
Camp (d)		-2.211*	-2.025
		(1.226)	(1.282)
2002 dummy	Yes	Yes	Yes
2002 x Province dummies	Yes	Yes	Yes
HH fixed effects	Yes	Yes	Yes
Cendajuru	Yes	Yes	No
Observations	6,286	6,286	6,026
R-squared	0.134	0.135	0.134

Weighted OLS estimations with household fixed effects (based on the sampling weights provided by the ESDSR survey). Robust standard errors clustered at the level of the *communes* in parentheses. ***p<0.01, **p<0.05, *p<0.1.
Violence exposure (d) is computed at the level of the *communes*. Number of livestock heads, Migrant (d) and Camp (d) are provided by the ESDSR survey (UNPF, 2002).
Column (3) runs the same specification as Column (2) excluding households from the *commune* of Cendajuru from the sample, as this *commune* was only exposed after 1998 (and thus, potentially, after 2002).

Table 25: **Violence and poverty status switches: distinguishing households by pre-exposure status**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Out of poverty		Into poverty		Out of food poverty		Into food poverty	
Panel 1: Not pre-exposed – Original households.								
Violence exposure, 1999 – 2007 (d)	-0.0292	0.0465	0.500***	0.452***	0.0113	0.0815	0.471**	0.481***
	(0.106)	(0.145)	(0.143)	(0.0939)	(0.0789)	(0.142)	(0.180)	(0.0860)
Province dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HH controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	201	201	114	114	146	146	162	162
R-squared	0.101	0.171	0.154	0.289	0.083	0.175	0.091	0.276
Panel 2: Not pre-exposed – Complete sample.								
Violence exposure, 1999 – 2007 (d)	-0.0157	0.0411	0.244*	0.275***	0.0506	0.0897	0.304*	0.354***
	(0.0610)	(0.0809)	(0.130)	(0.0940)	(0.0344)	(0.0755)	(0.162)	(0.0864)
Province dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HH controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	281	281	138	138	209	209	198	198
R-squared	0.059	0.158	0.123	0.277	0.055	0.162	0.077	0.266
Panel 3: Pre-exposed – Original households.								
Violence exposure, 1999 – 2007 (d)	-0.0434	-0.0414	0.161***	0.176***	0.0111	0.00756	0.101	0.0867
	(0.0489)	(0.0477)	(0.0507)	(0.0537)	(0.0820)	(0.0751)	(0.0655)	(0.0610)
Province dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HH controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	325	325	120	120	260	260	172	172
R-squared	0.087	0.104	0.095	0.136	0.142	0.171	0.078	0.142
Panel 4: Pre-exposed – Complete sample.								
Violence exposure, 1999 – 2007 (d)	-0.0613	-0.0715	0.155***	0.183***	-0.0466	-0.0601	0.0926*	0.0860*
	(0.0475)	(0.0468)	(0.0473)	(0.0510)	(0.0666)	(0.0681)	(0.0527)	(0.0503)
Province dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HH controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	443	443	134	134	361	361	199	199
R-squared	0.084	0.104	0.084	0.133	0.125	0.162	0.071	0.126

Robust standard errors clustered at the level of localities in parentheses. ***p<0.01, **p<0.05, *p<0.1.
HH controls are measured in 1998. They are the same as in Table 8, namely: Head – Age, Head – Female, Head – Educ, Head – AgrExp, Head – NonFarm, and Migrant. In addition we control for a dummy for split-off households in Panels 2 and 4.
Columns (1) and (2) (respectively, (5) and (6)) estimate the determinants of the probability of transition out of poverty (respectively, food poverty) over the sample of households who were poor (respectively, food-poor) in 1998. Columns (3) and (4) (respectively, (7) and (8)) estimate the determinants of the probability of transition into poverty (respectively, food poverty) over the sample of households who were non poor (respectively, non food-poor) in 1998.

Table 26: **Violence intensity and poverty status switches.**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Original households				Complete sample			
	Out of poverty	Into poverty	Out of food poverty	Into food poverty	Out of poverty	Into poverty	Out of food poverty	Into food poverty
Violence exposure,	-0.0433**	0.0598*	-0.0250	0.0298	-0.0319*	0.0592**	-0.0315	0.0328
1999 – 2007 (log)	(0.0215)	(0.0352)	(0.0244)	(0.0278)	(0.0174)	(0.0289)	(0.0197)	(0.0218)
Province dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
HH controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	526	234	406	334	724	272	570	397
R-squared	0.081	0.212	0.112	0.159	0.081	0.186	0.115	0.147

Robust standard errors clustered at the level of localities in parentheses. ***p<0.01, **p<0.05, *p<0.1.
HH controls are measured in 1998. They are the same as in Table 8, namely: Head – Age, Head – Female, Head – Educ, Head – AgrExp, Head – NonFarm, and Migrant.
Columns (1) and (5) (respectively, (3) and (7)) estimate the determinants of the probability of transition out of poverty (respectively, food poverty) over the sample of households who were poor (respectively, food-poor) in 1998. Columns (2) and (6) (respectively, (4) and (8)) estimate the determinants of the probability of transition into poverty (respectively, food poverty) over the sample of households who were non poor (respectively, non food-poor) in 1998.