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

Marion Mercier[†], Rama Lionel Ngenzebuke[‡], Philip Verwimp[§]

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We investigate the relationship between exposure to the Burundi Civil War and household (food) poverty, using a three-wave household-level panel matched with data on local-level violence. We find that households living in localities exposed to the war have been subsequently more likely to be poor than non-exposed households. *Within-household* estimations, controlling for time-varying heterogeneity at the province level, confirm the positive impact of violence exposure on household poverty. We investigate some of the potential mechanisms at play in the violence – poverty nexus, and the role of violence exposure in household poverty dynamics over time. Our results notably suggest that the destruction of physical capital, as well as a shift of exposed households out of non-farm activities, shape poverty dynamics and lower their chances of durably remaining out of poverty.

JEL classification: O12, I32, H56, O55.

Keywords: Civil war; Poverty; Panel data; Burundi.

1 Introduction

From 1946 to 2012, the world witnessed more than 250 civil wars (Simon Fraser University, 2013). After a peak following the end of the Cold War and a decline thereafter, the global

number of conflicts has increased again since 2010. Civil wars can affect outcomes as varied as poverty, institutions, human capital, and economic growth, which also determine the risk of conflict reoccurrence. Improving knowledge on the various consequences of wars and on their persistence over time is thus a key research challenge to enlighten post-conflict policy making and the management of conflict risk.

The Burundi Civil War started in 1993, following the assassination of Melchior Ndadaye, the first democratically elected president, three months after he came to power. This triggered brutal attacks by Hutu groups, followed by violent retaliation from the Tutsi-dominated army. Starting in the Northwest, violence soon spread toward the northeastern, central, and southern provinces. From 1993 to 2005, the whole country was affected, though at different degrees and periods (United Nations, 1996; Chrétien and Mukuri, 2002). The conflict eventually left over 300,000 Burundians dead, for a population of around 5.8 million in 1993 (Ngaruko and Nkurunziza, 2000).

We adopt a micro-level approach to analyze the legacy of violence on household poverty dynamics in Burundi. While a strand of economic research documents the poverty impact of a range of shocks (e.g. weather- or trade-related) and the socioeconomic characteristics that move individuals and households into and out of poverty, there is little evidence on the impact of violent events on long-term poverty (Justino and Verwimp, 2013). This is mostly due to the lack of micro-level panel data allowing to track households over time in violent contexts. We exploit a three-wave panel dataset in which we observe households in 1998, 2007, and 2012. We match these household-level data with information on violence, which is available at the locality level and covers the whole period of war. To do so, we aggregate yearly exposure data over two subperiods: [1993,1998] and [1999,2007], no violence being observed after 2007. The evolution of household poverty over time can thus be related to the time-varying intensity of violence in the locality. Based on these data, we contribute to the literature on the micro-level consequences of civil wars through three empirical exercises.

First, we analyze in a panel setting how changes in locality-level violence from one subperiod to the other translate into variations in household poverty over time. The richness of the data allows us to account for household fixed effects, as well as for national and subnational trends. We find a positive relationship between exposure to violence and poverty, which we argue reveals causality. This is notably consistent with cross-sectional evidence from Serneels and Verpoorten (2015) that households exposed to civil war and genocide in Rwanda had significantly lower consumption than non-exposed households six years after the end of the violence.

The second part of our empirical analysis relies on the panel dimension of the data to document some of the mechanisms at play in the violence – poverty nexus. The literature on the micro-level consequences of civil wars emphasizes four main channels through which violence exposure is expected to affect poverty.¹ *First*, war-induced deaths, injuries, and disabilities affect households’ demographic structure and labor endowment. In some cases, direct excess mortality has notably been shown to impact young men (De Walque and Verwimp, 2010; De Walque, 2006), which could result in a shrinking ratio of net producers to net consumers among households, and in higher poverty. *Second*, conflict causes the loss or destruction of productive assets such as land and cattle (Brück, 2001; González and Lopez, 2007; Shemyakina, 2011).² This impedes important sources of households’ livelihood and can critically affect their productive capacity. The vulnerability to such loss is likely to depend on households’ asset endowment, some assets being more easily looted than others (Bundervoet, 2010; Justino, 2012; Martin-Shields and Stojetz, 2019). *Third*, violence disrupts the accumulation of human capital: exposure to civil wars has been shown to impede children’s nutritional status, health, and schooling outcomes.³ All these effects might have long-run

¹Justino (2012) provides a thorough overview of the various effects of war on poverty. Martin-Shields and Stojetz (2019) summarize the literature on the interactions between violent conflict and food security.

²Households may also decide to sell cattle to smooth their consumption, as shown by Verpoorten (2009) in the case of Rwanda.

³A rich literature studies such effects. Martin-Shields and Stojetz (2019) provide a thorough overview of

consequences on income (Smith, 2009; Alderman, Hoddinott, and Kinsey, 2006; Ichino and Winter-Ebmer, 2004; Islam et al., 2016). *Fourth*, the literature suggests that conflicts influence various dimensions of social capital, such as trust and altruistic attitudes. Though the nature of this influence is uncertain, it is likely that war-induced changes in social capital also affect poverty.⁴

We seek to uncover some of the mechanisms at play in the violence – poverty nexus in Burundi. In particular, we show that violence affects the demographic structure of exposed households, as well as their physical capital and economic activities. We notably find a negative effect of violence exposure on cattle ownership, and evidence of a war-induced shift of exposed households toward subsistence farming. With the advantage of being based on household fixed effect specifications, these results are consistent with the evidence from other conflicts provided by the literature – in particular, Brück (2001) on Mozambique and Annan, Blattman, and Horton (2006) on Uganda – and in line with the available qualitative evidence on Burundi. The impact of violence on human capital, which is the object of a very rich literature (see in particular Bundervoet, Verwimp, and Akresh (2009) in the case of Burundi), is left aside, while the social capital channel cannot be dealt with based on our data.

The third part of the analysis investigates how violence shapes households’ poverty dynamics over time. While very few databases cover the same households over multiple waves in a poor, conflict-ridden setting, the panel at hand allows us to document how violence exposure relates to households’ long-run poverty status. In particular, we underline a negative correlation between violence exposure and the likelihood that a household remains non-poor

its findings, and highlight that adverse effects may even develop *in utero*. 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), and Domingues and Barre (2013) on children’s nutrition and health; and Akresh and de Walque (2008), Shemyakina (2011), Chamarbagwala and Moran (2011), Swee (2015), and Islam et al. (2016) on education.

⁴In Uganda, Rohner, Thoenig, and Zilibotti (2013) find that violence eroded social capital and De Luca and Verpoorten (2015) that short-term negative effects were followed by recovery. In Sierra Leone, Bellows and Miguel (2009) show that victimized people became more likely to join community meetings. In Burundi, Voors et al. (2012) observe that violence exposure increased altruism and risk-seeking behaviors.

over the whole period of observation. Although this result emanates from a weaker specification (as household fixed effects and time trends cannot be accounted for), it is highly consistent with the observed war-induced destruction of assets, which is likely to primarily affect non-poor households. It is also in line with the results of Justino and Verwimp (2013) which suggest that non-poor households were the most affected by exposure to violence in Rwanda, and that initially better-off provinces suffered disproportionately from the destruction of productive factors.

The following section describes the data and provides an overview of poverty in Burundi over the period of analysis. In Section 3, we analyze the link between violence exposure and household poverty, and discuss the threats to our empirical approach. Section 4 investigates some of the potential mechanisms at work. Section 5 explores the relationship between violence exposure and the dynamics of poverty, and Section 6 concludes.

2 Data

2.1 Measuring Poverty

We exploit a rich 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’ on a random sample of over 6,000 households, selected from a random sample of 391 rural and urban survey sites.⁵ In 2007, a second survey wave aimed at re-interviewing 1,000 households from 100 randomly selected rural localities from 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 872 original

⁵We use the generic term ‘locality’ to refer to these survey sites. The territory of Burundi is composed of four administrative levels: *provinces*, *communes*, *collines*, and *sous-collines*. The *localities* of the survey design are not administrative clusters. They are an intermediary between the *colline* and the *sous-colline*: a *locality* can cover multiple *sous-collines*, but is always linked to a single *colline*.

households and 530 split-offs were covered. The third round was implemented in 2012, and tracked 1,223 households out of the 1,402 households interviewed in 2007.⁶

In each round, a module focuses on the consumption of a range of food and non-food items. For each food item, households declared the quantity consumed over the last week and the price per unit on the local market.⁷ To calculate the total value of food consumption, we use the median of the prices declared by respondents for each item. In the absence of market price data, this aims at mitigating the measurement errors that might characterize declared prices. Households' consumption is computed as the sum of food consumption, valued with the vector of country-level median prices, and non-food consumption.⁸ We finally deflate the monetary value of consumption relying on consumer price index data from the Burundi central bank.

We then compute the poverty line relying on the 'cost of basic needs' method (Ravallion, 1994, 1998; Ravallion and Bidani, 1994). Ravallion and Bidani (1994) define poverty as "a lack of command over basic consumption needs, and the poverty line [as] the cost of those needs," which 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 estimate its cost. The basket of goods is derived from the food consumption of the 50% poorest households. We calculate the caloric value of the average basket of goods that they consume, and re-scale it to reach the caloric requirement

⁶While 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.

⁷Note that in 1998 and 2007 (unlike in 2012), households did not directly declare the total quantity they consumed, but rather: (i) the quantity they produced, (ii) the quantity they sold and income they got from it (from which we can compute a price), and (iii) the expenditure they made to buy the considered item on the local market. We derive the total quantity of each item consumed by adding up (i) the quantity produced and kept for consumption, and (ii) the quantity bought, inferred from the amount of expenditure and price (to limit measurement issues in the absence of market price data, we use the median price across respondents).

⁸The value of the latter is directly derived from the declared prices: as opposed to food items, it is not relevant to compute median prices for non-food items such as 'clothing' or 'cell phones,' for which households declare the total amount spent without specifying a quantity.

considered as the minimum,⁹ without changing the proportions of the various items. We thus obtain a basket of goods which corresponds to the minimum caloric intake 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 the vector of median prices), and households' food poverty status depends on whether their food consumption per adult equivalent reaches this threshold.

Estimating the minimum non-food requirements is trickier, in the absence of an equivalent to caloric intake for non-food consumption. We follow Bundervoet (2006) and Verwimp and Bundervoet (2009) who, based on the first two waves of the data that we use, estimate the share of non-food spending of Burundian households whose total level of consumption is very close to the food poverty line. These households could exactly meet their caloric needs, but instead they sacrifice part of their food consumption in favor of non-food consumption. Since they do not fulfil their minimum caloric requirements, it is arguable that their non-food expenditures correspond to what they consider as absolutely necessary. We use the estimated food share for Burundi to derive the non-food component of the poverty line. The poverty line is the sum of the food and non-food components, and a household is deemed poor if their monthly consumption per adult equivalent is below the poverty line.

2.2 Violence Data

In the 2007 survey round, local leaders were asked about the annual number of war-related casualties, defined as the dead and wounded, suffered by the locality from the beginning of the conflict (1993) to the time of the survey. Our variable of interest, which aims at capturing local violence exposure, is constructed based on this information.

The declarative and retrospective nature of the data collected can cast doubt on the pre-

⁹Namely, 2,500 calories per day and adult equivalent, based on the case of Rwanda which is a very similar context (Minecofin, 2002).

cision of the reported (i) dates of casualty-generating events, and (ii) number of casualties. The way our main explanatory variable is constructed should however mitigate the consequences of such imprecision. We use the subperiod level, rather than yearly information, which should limit measurement errors related to dating mistakes. We calculate the total number of war-related casualties (dead and wounded) over 1993 – 1998 for the first data point in the panel (1998) and over 1999 – 2007 for the second data point of the panel (2007). Our main explanatory variable is then computed as a dichotomic variable for violence incidence, which equals one if at least one war-related casualty was reported in the locality over the preceding subperiod. While the exact number of war casualties might be difficult to precisely capture through the survey protocol, we are confident that the distinction between ‘at least one casualty’ and ‘no casualty at all’ is much less subject to recall bias and measurement error.

Between 1993 and 1998, 550 of the 1,000 households in the sample (55%) were exposed to violence, i.e. living in a locality which had at least one casualty. After 1999, the incidence of violence decreases, with 234 of the 1,402 households in the sample exposed (16.69%), most of them before 2005. The incidence of exposure at the locality level is very similar to the incidence of exposure at the household level for both subperiods: out of the 100 localities in the sample, 55 were exposed between 1993 and 1998, and 17 between 1999 and 2007.

The temporal variation which we exploit is thus a generally decreasing trend in local violence incidence: most households in our sample experienced a shift from violence to no violence, be it between the first and second points, or between the second and third points of the panel. Still, five localities did not report any casualty during the first subperiod, but did experience violence between 1999 and 2007. This corresponds to 43 households exposed only over the second subperiod.¹⁰

¹⁰Our benchmark results are robust to dropping these households from the sample. Results not shown for brevity, available upon request.

The dichotomous measure of violence exposure is arguably much less subject to recall and measurement errors. Yet, identically treating localities with one and with one hundred casualties is questionable. In addition to the main results based on violence incidence, we exploit information on violence intensity, which might correlate differently with poverty. Violence was much more intense over the first subperiod. Between 1993 and 1998, the maximum number of casualties reported in the sample is 560, and the average (respectively, median) among exposed localities is equal to 112 (respectively, 68). By contrast, between 1999 and 2007, the maximum number of reported casualties in the sample is 93, and the average (respectively, median) among exposed localities is equal to 19 (respectively, 9). Given the potential measurement errors in the reported number of casualties, and the substantial difference in violence intensity between the two subperiods, we use the logarithm of one plus the number of war-related casualties as a proxy for violence intensity.¹¹

2.3 An Overview of Poverty in Burundi between 1998 and 2012

Table A1 in the Appendix presents the first-, second- and third-degree Foster-Greer-Thorbecke poverty measures, food poverty headcount and Gini index by year.¹² Around

¹¹Bundervoet, Verwimp, and Akresh (2009) exploit historical records from Chrétien and Mukuri (2002) to measure violence exposure. It is difficult to directly compare our data to theirs, as Bundervoet, Verwimp, and Akresh (2009): (i) provide the timing of war exposure between 1994 and the end of 1998, so that they only partially cover the war; (ii) distinguish six regions (defined as sets of provinces), so that their level of geographic aggregation is large; and (iii) do not have any measure of violence intensity. Still, if the households in our sample are allocated the exposure status of their province as built by Bundervoet, Verwimp, and Akresh (2009), we reassuringly see that all the households living in localities flagged as exposed in our data over our first subperiod (1993 – 1998) were indeed living in provinces considered as exposed by Bundervoet, Verwimp, and Akresh (2009). Moreover, going back to the year-level locality exposure data, we find that the correlation between our locality-level and their province-level exposure dummies over the period of common coverage is highly significant and of reasonable size, given that many localities which were not exposed according to our data are located in exposed provinces (coefficient of correlation of 0.25, p-value of 0.0000).

¹²For a continuous expenditure distribution, the FGT-index is given by: $P_\alpha = \int_{y=1}^z \{((z - y)/z)^\alpha f(y)\} dy$, where z and y are, respectively, the poverty line and amount of expenditures (Foster, Greer, and Thorbecke, 1984). For $\alpha = 0$ and 1, the index respectively measures the prevalence of poverty (poverty headcount) and the intensity of poverty (poverty gap). For $\alpha = 2$, it provides a measure of poverty severity (Ravallion, 1992; Bigsten et al., 2003).

69% of households were poor in 1998. The headcount slightly decreases afterward, while the poverty gap rises, pointing out an increased depth of poverty, which is consistent with the increasing share of food-poor households. At the same time, poverty severity increases, which corresponds to growing inequalities. The increase in the Gini index reflects this trend.

Over the course of the panel, some households fell into (food) poverty, while others escaped it. Table A2 in the Appendix displays the distribution of the sample through the lens of a threefold poverty status, distinguishing between households who were 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. once poor (non-poor) and twice non-poor (poor)), for both poverty and food poverty. Over the period, only 6% of households were never poor, while nearly 39% were in chronic poverty, and 55% switched poverty status at least once. Moreover, 27% suffered from food poverty over the whole period and 62% episodically, while 11% were never food poor.¹³

Table 1 displays the differences of means in the prevalence of (food) poverty in each survey wave, between households who were exposed to violence in the past and households who were not. In this table, and unlike the rest of the paper, the whole exposure to violence in the past is considered. The exposure dummy is thus equal to one if at least one war-related casualty was reported in the locality between 1993 and 1998 for Panel A, and between 1993 and 2007 for Panels B and C.

Exposed households exhibit a significantly higher incidence of (food) poverty in subsequent years. It was still the case in 2012, which suggests that the increased poverty associated with violence exposure persists over time. This makes it crucial to understand which households are most at risk in the long run. In terms of policy implications, it also implies that violence-affected areas should still be targeted long after the end of violence, not only as part

¹³The high share of ‘switchers’ is very much in line with the existing evidence on comparable contexts (Baulch and Hoddinott, 2000). See for instance Justino and Verwimp (2013) on Rwanda and Hoddinott and Kinsey (1998) on Zimbabwe.

of the short-run post-conflict recovery policies, but also as part of the pro-poor policy tools designed in times of peace.¹⁴

3 Violence and Poverty over Time

3.1 Benchmark Estimations

Our benchmark estimations relate households' (food) poverty to time-varying locality-level violence. The households in our sample are spread across 100 localities and 13 provinces, each province comprising 4 to 13 localities. *First*, we estimate the correlation between (food) poverty and violence, controlling simultaneously for *year* dummies, to account for the general trend in poverty in Burundi, and *province* \times *year* dummies, to account for unobserved heterogeneity across provinces. The estimated equation is thus:

$$Y_{i,j,p,t} = \alpha + \beta \text{ViolenceExposure}_{j,t-N,t} + \delta_t + \gamma_{p,t} + \varepsilon_{i,j,p,t} \quad (1)$$

with i indexing households, j localities, p provinces, and t time. Index t can take three values: 1998, 2007, and 2012. The dependent variable $Y_{i,j,p,t}$ successively measures poverty and food poverty. $\text{ViolenceExposure}_{j,t-N,t}$ is the dummy variable equal to one if at least one war-related casualty is reported in locality j between $t - N$ and t . Violence is observed over 1993 – 1998 and 1999 – 2007. $\text{ViolenceExposure}_{j,2008-2012}$ is equal to zero for all j .

The *year*- and *province* \times *year*-level fixed effects insure that we account for all sources of unobserved heterogeneity related to the country and province trends. In such a specification,

¹⁴After 2007, the difference between exposed and non-exposed households in terms of (food) poverty incidence decreases. Based on these uncontrolled differences of means, one can extrapolate when the gap between the two groups would no longer be statistically significant if the trend observed between 2007 and 2012 continued. If, for both measures of poverty, the yearly growth rates of the difference of means and of its standard error remained the same as over the 2007 – 2012 period, the convergence between households who were exposed to the war and households who were not should occur in 2020 in terms of poverty, and in 2021 in terms of food poverty – roughly speaking, 15 years after the conflict termination. These figures are however based on a far-fetched simulation exercise, relying on simplistic assumptions.

potential remaining confounding factors can relate to either locality-level or household-level unobserved heterogeneity. We (partially) deal with these potential sources of bias in our second and third specifications.

Second, we additionally control for locality fixed effects λ_j , which allows us to purge the coefficient of interest from unobserved time-invariant local heterogeneity:

$$Y_{i,j,p,t} = \alpha + \beta \text{ViolenceExposure}_{j,t-N,t} + \delta_t + \gamma_{p,t} + \lambda_j + \varepsilon_{i,j,p,t}. \quad (2)$$

Third, the data allow us to introduce household fixed effects, while still controlling for national and province-specific time trends. This is our preferred specification, which makes sure that all the time-invariant household-level characteristics are ruled out, and measures the *within-household* correlation between violence exposure and poverty by estimating:

$$Y_{i,j,p,t} = \alpha + \beta \text{ViolenceExposure}_{j,t-N,t} + \delta_t + \gamma_{p,t} + \eta_i + \varepsilon_{i,j,p,t}. \quad (3)$$

Each of the three equations is estimated on the sub-sample of original households, and the preferred specification (Equation 3) is also run over the whole sample, which gathers original and split-off households. We successively display the results without and with split-off households because the latter could behave differently: it is reasonable to think of splitting-off as a coping strategy, which could mitigate the consequences of violence exposure on poverty in the long run. Observing the correlation between violence and poverty for both the sub-sample of original households and the whole sample will help to shed light on this question. Finally, we estimate all the models using our continuous measure of violence intensity – namely, the logarithm of one plus the number of war-related casualties – rather than the dummy for violence incidence.

The results are displayed in Part 1 of Table 2. Panel A focuses on violence incidence, and Panel B on violence intensity. Panel A shows a significant correlation between vio-

lence exposure and poverty, which resists the introduction of province-specific time trends, locality fixed effects, and household fixed effects, and the inclusion of split-off households in the sample.¹⁵ The *within-household* variation in locality-level exposure to violence being significantly correlated with poverty, we cannot suspect that the correlation 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 here is time varying. Non-observed time-varying characteristics simultaneously correlated with violence exposure and poverty could introduce omitted variable biases, a limitation which must be kept in mind.¹⁶ The point estimates suggest that a locality switching from the non-exposed to the exposed status is associated, on average, with a 6 to 8 percentage point increase in households' likelihood of being poor, and with a 5.5 to 7 percentage point increase in their likelihood of being food poor. Moreover, the coefficient associated with violence exposure is smaller in columns (4) and (8), where the whole sample is exploited, than in the comparable specifications relying on original households only in columns (3) and (7). Although the difference between the two coefficients is poorly significant in statistical terms (notably in the case of food poverty), it suggests that splitting-off could indeed help newly created households to mitigate the negative consequences of violence exposure.

The results of Panel B are very much in line with the benchmark specifications relying on the exposure dummy. Although statistically weaker, they consistently point to a positive correlation between violence intensity and poverty, including when household fixed effects are

¹⁵One could be concerned by the risk that our results do not hold when accounting for spatial dependence. Unfortunately, we cannot properly check that because our localities are not geolocalized. Still, we can collapse our data at the level of the *collines*, which are geolocalized, and adjust the standard errors for spatial correlation following Conley (1999) and Hsiang (2010). The results prove robust to this correction (available upon request).

¹⁶A strategy to mitigate this potential bias is to include household-level time-varying control variables. This yields virtually similar results (available upon request). However, it is arguable that most observable household-level characteristics are themselves endogenous to violence exposure, which is why we only display a specification with no controls but the rich set of fixed effects allowed by the data.

introduced. The point estimates of the preferred specifications suggest that a one percent increase in violence intensity is associated, on average, with a 1.5 to 2 percentage point increase in households' likelihood of being (food) poor. Given the distribution of violence in the sample, this means that the average estimated effect of moving from the 25th to the 75th percentile of violence intensity is a 3.5 to 5.2 percentage point increase in the probability to be (food) poor, and that a one standard-deviation increase in violence intensity is estimated to trigger a 2.1 to 3 percentage point increase in the risk of (food) poverty.

3.2 Attrition

Considerable efforts were made to track the members of the original households, and the overall recontact rate of the survey exceeded 87% for both rounds. This can be considered a success given the time intervals between each wave and the ongoing violence between the first two. Still, some households were lost from one round to the other. This attrition could bias the results if non-tracked households have specific characteristics. To document this issue, we look at the differences of means in terms of poverty and violence exposure between tracked and non-tracked households in Table 3.

The *t-tests* do not show any significant difference in (food) poverty prevalence between households who remained in the sample and households who did not, either in 2007 or in 2012. Households who dropped out in 2007 and 2012 were not differently exposed to violence over the 1999 – 2007 period, but we do find a difference in exposure during the 1993 – 1998 period, for which we do not have a clear explanation. Table A3 in the Appendix additionally displays the differences of means regarding the main economic and demographic characteristics of the households. Some of these characteristics significantly differ across the two groups.¹⁷ Although the attrition rates are small, the observed differences between

¹⁷In particular, larger and younger households at time t were significantly more likely to remain in the sample at $t + N$. It also appears that households whose head worked in the non-farm sector were more likely to drop out, both in 2007 and 2012. On average, households who dropped out in 2007 were likely

tracked and non-tracked households, in particular regarding exposure to violence before 1998, make it difficult to conjecture the direction of the bias potentially induced in our estimations.

We implement two robustness tests to mitigate this concern. *First*, we rely on the inverse probability weighting (IPW) method developed by Wooldridge (2002) and implemented, among others, by Yamano and Jayne (2005). Specifically, we first estimate three reinterview models to predict: (i) the probability of original households being reinterviewed in 2007, (ii) the probability of original households tracked in 2007 being reinterviewed in 2012, and (iii) the probability of split-off households being reinterviewed in 2012.¹⁸ In the three cases, the vector of explanatory variables gathers the poverty and food poverty statuses, the locality-level violence exposure dummies over the two subperiods of the war, the province dummies, and the demographic and economic characteristics documented in Table A3 measured over the previous survey wave.¹⁹ Then, we use the inverse probabilities of being reinterviewed as weights and re-estimate our benchmark panel estimates. The results are displayed in Part 2 of Table 2. They attest to a great stability, in terms of both coefficient sizes and statistical significance, in the estimated relation between violence exposure and poverty. The same is true when violence intensity is considered, as displayed in Panel B.

Second, in Table 4 we calculate bounds for the coefficient of interest accounting for attrition. First, following Horowitz and Manski (2000), we impute missing (food) poverty data, assuming that all the exposed (respectively, non-exposed) households who drop out are poor (respectively, non-poor) and vice versa. We then re-run our benchmark fixed effects estimations (columns (3), (4), (7) and (8) of the first part of Panel A, Table 2) on the sample combining real and imputed observations. This yields the smallest and largest coefficients

to own fewer cattle heads and cultivate fewer crop types over the previous period. This is not the case for households who dropped out in 2012, although they were likely to own fewer fields in 2007 (a variable which is not available in 1998). Finally, households who dropped out in 2012 were more likely to have migrated internally between 1998 and 2007.

¹⁸Results not shown for brevity, available upon request.

¹⁹Namely, *Average age*, *Dependency ratio*, *Number of members*, *Head - AgrExp*, *Head - AgrNoExp*, *Head - NonFarm*, *# cattle*, *# crops* and, for the 2012 equations, *# fields* and *Migrant*.

compatible with the data effectively observed. They are displayed in the top part of Table 4. The width of the bounds is large, which is not surprising given the extreme assumptions on which these worst-case scenarios calculations rely, but, importantly, the lower bounds are never significantly negative. In order to narrow the bounds, our second strategy consists, in the spirit of Lee (2009), to tackle differential attrition by trimming the sample until we reach a balance between the exposed and non-exposed groups. Lee (2009) recommends to alternately remove the observations with the highest and lowest values of the dependent variable, which yields lower and upper bounds under the assumption that the effect of the treatment on attrition is monotonic.²⁰ Our dependent variable being binary, we follow Merlino, Steinhardt, and Wren-Lewis (2019) and calculate a lower bound by dropping, among exposed households, those that contribute most to a positive correlation between violence and poverty, i.e. those that have the smallest residual when we regress the dependent variable on the control variables (household fixed effects, national and province-level trends). The results are displayed in column (1) of the bottom part of Table 4. In column (3), we instead drop exposed observations with the highest such residual. As expected, the resulting bounds are tighter. The lower bound is now always positive, though not always statistically significant. Overall, these results are reassuring over the risk that attrition is compromising our benchmark findings, since relatively strong assumptions on the nature of attrition still indicate a strong likelihood that the coefficient of interest is significantly positive.

²⁰Since households who are tracked (both in 2007 and in 2012) are more exposed than households who are not, the assumption here is that there are households who would drop out if they are not exposed to violence but not if they are exposed, but not vice versa.

3.3 Endogeneity

3.3.1 Threats to Identification

The main value added of our data is their longitudinal dimension, combined with the large effort of tracking which was implemented during the two follow-up rounds. Such data are very rare in the context of a poor economy undergoing long-lasting conflict. They allow us to measure the correlation between violence exposure and subsequent household deprivation purging the estimates from a time trend, from province-level time-varying heterogeneity, and from household-level time-invariant heterogeneity through *within-household* estimations.

Our identification assumption is then that there are no time-variant locality- or household-level characteristics simultaneously related to violence exposure and to poverty. Locality- and household-level potential selection into violence is thus a concern. Indeed, violence is not random: poor post-war outcomes could notably reflect characteristics that triggered violence in the first place. Unfortunately, the data do not contain pre-war household-level information which could allow us to investigate the potential endogeneity of conflict location. 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 deal with the non-randomness of war.

In what follows, we mobilize external data and implement a test on our data to provide suggestive evidence that our results are not driven by selection into violence or unobserved heterogeneity, and thus reveal causality.

3.3.2 External Evidence

External sources of information can help us to evaluate the potential impact of the endogeneity of violence on our results. The previous quantitative 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, which are indiscriminately brutal, affecting the entire country and causing profound fear among the whole population, more accurately reflect 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, to investigate the risk of locality- and household-level selection into violence. *First*, we rely on pre-violence locality-level data from Voors et al. (2012) and province-level data from the national statistics institute (ISTEEBU, 1993) to analyze the determinants of violence exposure at the locality level.²¹ The dependent variable, coming from our panel, is the dummy equal to one if at least one war-related casualty was reported in the locality, over each subperiod of the war. The vector of explanatory variables gathers several of the geographic, demographic, and socio-economic characteristics of the localities. The results are displayed in Table 5. No systematic correlation appears between violence incidence by subperiod and the pre-violence local characteristics taken from ISTEEBU (1993) and Voors et al. (2012).

Second, we rely on a survey conducted in 2002 to provide insights into the potential endogeneity of conflict location to an indicator of households' assets, which is assumed to be highly correlated with poverty. In 2002, the United Nations Population Fund conducted a nationally representative survey called 'Enquête socio-démographique et de santé de la reproduction' (ESDSR). This dataset is not a panel, but provides household-level pre-exposure information based on a recall question on the amount of livestock owned in 1993, which we can use to investigate household-level selection into violence, though on a different sample of households. In Table 6, we compare households' mean livestock in 1993 across future exposure status. The exposure dummy, which comes from our panel, is available at the locality

²¹Among the 100 localities in our data, 94 can be matched with these two external sources of data.

level. Unfortunately, the smallest geographic unit in the 2002 survey is the *commune*, which is larger than the locality.²² Thus, we aggregate our locality-level data to compute a *commune*-level exposure dummy equal to one if at least one war-related casualty was reported in the *commune* between 1993 and 2007, and we compare the average livestock owned in 1993 by households in the 2002 survey living in *communes* which were going to be exposed to the war to the average livestock owned in 1993 by households in the 2002 survey living in *communes* which were not going to be exposed to the war.²³ 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 *communes*.

None of these two empirical exercises, based on external data, perfectly deals with the potential endogeneity of conflict location to poverty. However, together with the quantitative and qualitative results from the existing literature, these tests provide suggestive evidence which is reassuring regarding the potential importance of locality- and household-level selection into violence for our empirical results.

3.3.3 Internal Evidence

Since our panel gathers two subperiods of war, we observe households exposed to violence at different periods in time. We can thus further investigate the issue of household-level time-varying heterogeneity by restricting the sample to ever-exposed households, and exploiting only the *within-household* time variation in exposure. While unobserved time-varying heterogeneity is still likely to exist, it is reasonable to believe that it is much less problematic when comparing households exposed over different time periods than when comparing exposed to never-exposed households.

Part 3 of Table 2 displays the results of the benchmark specifications run over the subsample of ever-exposed households. They are remarkably comparable to our benchmark

²²Our data cover 100 localities spread across 76 *communes*, among which 45 are in the 2002 survey sample.

²³Note that zooming out geographically implies a loss of precision in the measurement of violence exposure.

estimates. Although not proving causality, these results provide additional suggestive evidence that household-level time-varying heterogeneity is not driving our results, and that our identification assumption is plausible.

4 Potential Mechanisms

The benchmark results suggest the existence of a positive impact of violence exposure on household poverty. As discussed in Section 1, the literature highlights multiple mechanisms through which exposure to violence could translate into increased poverty. We investigate two of the potential channels which may be at work in Burundi.

4.1 Demographic Changes

War exposure is likely to transform the demographic structure of exposed households facing current or expected shocks, through violence-induced deaths, but also mobility and household recomposition. This can eventually affect household poverty. To document the relation between violence and the structure of households, the top part of columns (1) to (4) of Table 7 estimates Equation 3 using successively four demography-related variables as dependent. In particular, we compute the total number of household members, the dependency ratio (equal to the number of members under 15 and over 65 over the total number of members), and the number of household members under 15 and over 65. For each of these dependent variables, both the results on the sub-sample of original households and on the whole sample are displayed. Different from Table 2, here the two alternative measures of violence appear in columns: columns (1) and (2) focus on the exposure dummy as explanatory variable of interest, while columns (3) and (4) explore violence intensity. The various dependent variables appear in rows.

Columns (1) and (2) point to a significantly lower average number of household members

and a significantly lower dependency ratio in exposed localities. This is due to a lower average number of members under 15, while there is no significant difference regarding the number of members over 65. The results on violence intensity in columns (3) and (4) confirm this trend, which could be explained by three (non-exclusive) phenomena: (i) a decreased fertility in exposed areas, translating into fewer births; (ii) an increased mortality of youngsters in exposed areas; and/or (iii) an increased mobility of youngsters out of exposed areas.

The rest of columns (1) to (4) of Table 7 aims at investigating these three potential mechanisms, exploiting another set of dependent variables. We first focus on the number of births observed in the household over the years preceding each survey round.²⁴ The results suggest that, while there is no significant fertility differential between exposed and non-exposed areas, households in localities exposed to more intense violence are characterized by a smaller number of births. We then turn to mortality, with the dependent variable capturing the number of household members who died between each survey round.²⁵ Violence incidence and intensity appear to translate into significantly increased mortality. Finally, the bottom part of the table exploits information on household members' mobility between each survey round. In particular, the questionnaires of 2007 and 2012 allow to identify the number of household members who were not present during the previous survey but came back in the meantime, as well as the number of members who used to be present, but who left since the previous survey wave, for work-related motives or because they got married.²⁶ The results show that violence exposure and intensity do not affect the number of members who left their households for labor motives or in order to get married. On the contrary, there is clear evidence that exposed households and households exposed to more intense violence are more likely to have members returning, after having temporarily left the household. Violence exposure and intensity are thus associated with households

²⁴Specifically, it is the number of births over [1993,1998], [1999,2007], and [2008,2012].

²⁵Note that, contrary to fertility, mortality over the period preceding the 1998 survey is not available.

²⁶As mortality, mobility-related variables are only observed in 2007 and 2012.

sending away some of their members, who eventually come home, while the information available in the survey questionnaire allows us to exclude labor- and marriage-related reasons to explain this increased mobility. Child fostering during the war period, as well as fighters returning from the battlefield, could be responsible for this. Columns (1) to (4) of Table A4 in the Appendix display the same estimations implementing the IPW method to correct for attrition. Although slightly less significant, the results globally confirm those in Table 7.

Our results are compatible with all three mechanisms discussed above. Although weak, there is evidence of decreased fertility in intensely exposed areas, which can partly explain the observed smaller household size and number of young members. At the same time, mortality is significantly higher, and this is particularly the case among young males.²⁷ Finally, the members of households from exposed localities are characterized by a higher temporary mobility.

Consistent with the literature, we find that violence affects the demographic structure of households. Both direct consequences of violence (such as battle-related mortality and mobility) and indirect consequences, notably through coping mechanism (for instance, child fostering and reduced fertility) could explain these changes in households structure, which can participate in shaping long-run poverty dynamics. In particular, while the observed reduced household size could mitigate the risk of poverty (as the number of household members is positively correlated with poverty), the change in the composition of the household, notably with an increased mortality of young men, could on the contrary boost poverty.²⁸ The net responsibility held by such demographic changes in the war-induced poverty increase thus remains unclear.

²⁷The results differentiating mortality across gender and age groups are not shown for brevity, but available upon request.

²⁸In the case of Rwanda, Justino and Verwimp (2013) show that violence-related deaths can affect household welfare differently depending on the characteristics of the deceased: while poverty decreases in the aftermath of the death of an older member, it is not the case when the deceased is an adult at working age, who would otherwise contribute to the household income.

4.2 Economic Activity and Physical Capital

As discussed in Section 1, another mechanism likely to contribute to the spread of poverty in war-affected areas is linked to households' assets and economic activity. In columns (5) to (8) of Table 7, we start by estimating Equation 3 using as dependent variable three dummies indicating whether the household head works in the agricultural sector with at least one export crop, without any export crop, or works in the non-farm sector. The results of columns (5) and (6) suggest that, while violence exposure does not affect the likelihood to cultivate export crops, household heads living in exposed localities become significantly less likely to participate in non-farm activities, and more likely to live off subsistence agriculture. The rest of the table then focuses on agricultural households, and uses as dependent variables, successively, the number of cattle heads that they own, the number of crop types that they cultivate, and their number of fields (the latter being available only in the 2007 and 2012 questionnaires). The results suggest that agricultural households do not cultivate a smaller number of crops or own fewer fields in exposed localities, but that they own significantly less cattle. While, contrary to violence exposure, the intensity of violence is not significantly related to the household head's activity, columns (7) and (8) also show a strong negative association between violence intensity and cattle ownership. Columns (5) to (8) of Table A4 in the Appendix run the same estimations implementing the IPW method to correct for attrition. The results prove to be very robust.

We use the ESDSR survey data presented in Section 3.3 to check whether comparable results on assets emerge from this alternative sample of households. As previously discussed, the ESDSR survey is not a panel, yet households were both asked their livestock in 2002 (at the time of the survey) and in 1993, before the start of the war. This allows us to observe the evolution of this variable between two points in time, and thus to run a household fixed-effect regression, the major advantage of this external data source being that it provides pre-exposure information. In Table A5 in the Appendix, we estimate the following equation:

$$Livestock_{i,c,p,t} = \alpha + \beta ViolenceExposure_{c,t} + \delta_t + \gamma_{p,t} + \eta_i + \varepsilon_{i,c,p,t}, \quad (4)$$

with i indexing households, c *communes*, p provinces, and t time. Index t can take two values here: 1993 and 2002. $Livestock_{i,c,p,t}$ measures the number of livestock heads owned by household i , living in commune c of province p , at time t . This information comes from the ESDSR data. $ViolenceExposure_{c,t}$ is the violence exposure dummy from our data. In 1993, it is equal to zero everywhere as the war had not started yet. In 2002, it takes the value one if at least one casualty was reported in *commune* c between 1993 and 2002, and zero otherwise. As already noticed (see Section 3.3), the 2002 survey can only be matched to our violence data at the *commune* level, thus our measurement of violence exposure loses geographic precision. Still, the results corroborate the findings of Table 7, pointing to a significantly negative *within-household* correlation between violence exposure and livestock ownership.

In the end, a set of converging evidence suggests the existence of two simultaneous mechanisms participating in the impact of violence on poverty: a decrease in agricultural households' assets – which is all the more important as violence is intense – and a switch in economic activity back to subsistence farming in exposed localities.

A few other papers describe a comparable war-induced destruction of physical capital in other countries. Based on figures from the Ministry of Agriculture, Brück (2001) reports that the number of cattle in Mozambique declined from over 1.3 million in 1982 to 0.25 million in 1992 due mostly to rebels looting and killing households' livestock. Weinstein (2006) consistently shows that rebel fighters were remunerated notably through looting civilian property in Mozambique, Sierra Leone, and Peru. In northern Uganda, Annan, Blattman, and Horton (2006) explain that many households lost all of their cattle, homes, and assets because of violence. Last, in Gaza, Brück, d'Errico, and Pietrelli (2019) put forward a

negative impact of the 2014 conflict on agricultural asset ownership. As underlined by Blattman and Miguel (2010), these papers have started building a set of consistent evidence on war-related households' asset loss. They still need to be complemented by analyses of different contexts, in particular based on panel data.

Our results add to this set of quantitative evidence, relying on household fixed-effect specifications. They are also consistent with the existing qualitative literature on Burundi. In particular, Cazenave-Piarrot (2004) describes massive looting phenomena, and underlines the importance of cattle destruction, which constituted one of the rebels' main resources. Based on figures from the UNDP, he reports that the total number of cattle heads in the country, estimated around 1.5 million before the war, decreased by around 430,000 (or nearly 30%) between 1993 and 1996. The destruction of assets continued after 1996, as criminal bands took advantage of the climate of fear and impunity to ally with the rebels in their looting activities. Cazenave-Piarrot (2004) documents the case study of the commune of Vugizo (Bururi Province), where the number of cattle heads fell from 3,413 in 1996 to 1,657 in 1999, i.e. a collapse of more than 50%. Uvin (2009), analyzing 388 in-depth interviews of Burundians, also insists on the omnipresence of criminality during the conflict. He reports that "the issue that surfaces constantly when people discuss the war years is pillage: it was hard in our conversations to find any family, any person, who was not deeply marked by the theft of their animals, their money, their roofs, their bike, their clothes." Consistently, the UN Security Council annual reports on Burundi document the widespread banditry, looting, and destruction of physical capital during the civil war (United Nations, 2004, 2005, 2006).

In addition to the reduction of assets, we find suggestive evidence of a war-induced shift in households' economic activity. This is consistent with the fact that the weight of subsistence agriculture in the Burundi GDP increased during the conflict, while the production of coffee, tea, cotton, tobacco, and sugar, largely for exportation, as well as the related transformation industries, shrank (Ngayimpenda, 2000; Cazenave-Piarrot, 2004; San Pedro, 2011). In his

analysis of the earlier economic impacts of the war, Lecompte (1997) also reports the massive destruction and pillage of shops and small craft units. He illustrates the collapse of the sector of small businesses with one figure from the capital, Bujumbura, where, aside from the central market which was completely paralyzed, the market fees collected in the city were divided by 3.5 between 1992 and 1994. In addition, he explains that the operations of the main factories across the country were largely hindered, notably by substantial electricity shortages. All of this led people to adopt “survival strategies,” mainly relying on subsistence agriculture, even in urban settings (Lecompte, 1997).

In different contexts, the literature also provides quantitative evidence of war-induced shifts in households’ economic activities: Bozzoli and Brück (2009) find that violence in Mozambique pushed people into on-farm, subsistence production activities; Arias, Ibáñez, and Zambrano (2019) observe that farmers confronted to violence in Colombia have tended to shift to activities with short-term yields, low investments, and low profitability, in particular subsistence activities; and Brück, d’Errico, and Pietrelli (2019) emphasize the negative impact of the 2014 Gaza conflict on households’ income diversification.

5 The Dynamics of Poverty

Non-farm activities as well as cattle ownership are negatively correlated with poverty in our sample, over the three years of data. Said differently, non-poor households are more likely to own cattle and to participate in the non-agricultural sector as compared to poor households. If, as suggested by the results in Section 4.2, the switch from non-agricultural to agricultural activities and the destruction of physical capital are important mechanisms to explain the impact of violence exposure on poverty, violence should particularly affect non-poor households, who are more likely to own assets and to work in the non-farm sector in the first place. To further document this question, we follow the previous literature by

estimating multinomial logit models allowing to analyze the dynamics of household poverty.²⁹

The dependent variable can take three values: 0 for households who are permanently non-poor, 1 for switchers, who are poor in one or two of the three data waves, and 2 for households who are poor in all three years. The multinomial logit determines the probability of a household experiencing one of these three outcomes. It yields coefficient values for two groups relative to the third omitted group. The results are however easier to interpret in terms of the average marginal effects, which reflect the impact of each explanatory variable on the likelihood of a household being in each of the three groups. Our set of explanatory variables gathers the province dummies and the variables of interest capturing violence over the first and second subperiods of the war.

The results are displayed in Table 8. Panel A relies on the violence exposure dummies for 1993 – 1998 and 1999 – 2007. Focusing on original households, column (1) shows a negative relationship between the two violence dummies and the probability of staying permanently non-poor over 1998 – 2012. A locality switching from the non-exposed to the exposed status over the first subperiod of the war is estimated to be associated with a decrease in households' probability of being permanently non-poor of 5%, and a locality switching to the exposed status over the second subperiod of the war with a decrease in households' probability of being permanently non-poor of 7.5%. On the other hand, the risk of being a switcher is increased by exposure to the first subperiod of the war (column (2)), while there is no significant evidence that exposure affects the likelihood that a household is chronically poor in column (3). Columns (4) to (6) exploit the whole sample, gathering both original and split-off households. By definition, split-off households are not present in 1998, thus in these estimations split-offs are allocated the poverty and exposure statuses of their households of

²⁹In particular, Lawson, McKay, and Okidi (2006), Kedir and McKay (2005), Justino and Verwimp (2013), Glewwe, Zaman, and Gragnolati (2000), and Baulch and Dat (2010) estimate multinomial models to document the factors of poverty transition and persistence respectively in Uganda, Ethiopia, Rwanda, and Vietnam for the last two.

origin in 1998. The results prove to be very stable.

Panel B exploits the intensive margin of the violence data. The results are very much in line with those of Panel A in highlighting a strong association between violence and a lower chance of being permanently non-poor. They also provide evidence that households living in more intensely affected localities (notably during the second subperiod of the war) are more likely to be trapped in chronic poverty. Finally, the results of columns (7) to (12) show roughly consistent but statistically weaker results on food poverty.

As compared to the benchmark specifications, the multinomial logit models suffer from additional endogeneity threats. In particular, while time-varying unobserved household characteristics could still act as confounders, it is also the case of time-invariant locality- and household-level unobservables which, here, cannot be accounted for by fixed effects.

Although based on a weaker specification than the panel estimates, the results of Table 8 bring insights on the distribution of the effect of violence on poverty which are very complementary to our results on the mechanisms. In particular, Section 4.2 suggests that the shift from non-farm to subsistence activities, and the destruction of physical capital, play important roles in the relationship between violence and poverty. Both channels are more likely to concern initially non-poor households (as poorer households are less likely to work in the non-farm sector and to hold assets in the first place), which is very consistent with the result of Table 8 that violence is particularly related to a lower probability to remain permanently non-poor. Such an interpretation also supports the idea put forward in the previous literature that households' endowment shapes their vulnerability to conflict.

6 Conclusion

Locality-level data on violence during the whole conflict, combined with three waves of a survey tracking households from 1998 to 2012, allow us to analyze the joint evolution of

violence exposure and household poverty in Burundi. While poverty only slightly decreased in the country from 1998 to 2012, its severity deepened, and 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 first show that households who suffered violence exposure are characterized by significantly higher subsequent poverty. This was still the case in 2012, seven years after the end of the war. Panel estimations controlling for national and province-specific time trends, as well as household fixed effects, confirm the impact of violence on poverty. We then shed light on the potential mechanisms at play in the violence – poverty nexus. We find evidence of important war-induced changes in households’ demographic structure, but their impact on poverty is unclear. We also highlight the impact of violence on the destruction of households’ physical capital and on their chances of shifting to in-farm, subsistence activities. Finally, and consistently with the latter channel, our analysis of the link between violence exposure and poverty dynamics suggests that violence prevents households from remaining durably non-poor.

Our results advocate for a conception of post-conflict policies adopting a broad scope. In addition to short-term policies of disarmament, reconciliation, and reconstruction of institutions and infrastructures, targeting households who live in previously exposed localities should be a component of any pro-poor policy in the long run. Helping households to diversify their portfolio of productive assets and their economic activities notably seems to be a promising route to fight war-induced poverty.

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7 Appendix

Table A1: **Household poverty over time.**

	1998	2007	2012
Poverty headcount ratio	68.80	65.62	66.23
Poverty gap	30.58	34.02	38.18
Poverty severity	17.79	21.83	26.19
Food Poverty headcount ratio	56.60	57.20	59.28
Gini index	0.359	0.485	0.620
<i>Number of households</i>	<i>1,000</i>	<i>1,402</i>	<i>1,223</i>

Table A2: **Threefold (food) poverty status in 1998, 2007, and 2012.**

	(1)	(2)
	Number of households (%)	
	Poverty	Food poverty
Permanent non-poor	76 (6.21)	138 (11.28)
Switchers	676 (55.27)	757 (61.90)
Chronic poor	471 (38.51)	328 (26.82)

Table A3: **Attrition: differences of means (continued).**

<i>Characteristics in 1998</i>	Tracked in 2007	Drop out in 2007	Difference
Average age	22.44	33.93	-11.49***
Dependency ratio	0.477	0.488	-0.011
Number of members	5.097	3.570	1.527***
Head – AgrExp	0.028	0.008	0.020
Head – AgrNoExp	0.850	0.813	0.037
Head – NonFarm	0.123	0.180	-0.057*
# cattle	2.150	0.891	1.258***
# crops	6.805	5.414	1.391***
<i>Number of households</i>	<i>872</i>	<i>128</i>	<i>1,000</i>
<i>Characteristics in 2007</i>	Tracked in 2012	Drop out in 2012	Difference
Average age	22.50	24.83	-2.33**
Dependency ratio	0.428	0.436	-0.008
Number of members	4.964	4.184	0.780***
Migrant	0.109	0.156	-0.048*
Head – AgrExp	0.395	0.257	0.138***
Head – AgrNoExp	0.4129	0.4134	0.0005
Head – NonFarm	0.204	0.346	-0.142***
# cattle	0.714	0.800	-0.086
# crops	5.485	5.190	0.295
# fields	3.452	3.106	0.346**
<i>Number of households</i>	<i>1,223</i>	<i>179</i>	<i>1,402</i>

Average age is the average age of the members, *Dependency ratio* is the number of members under 15 and over 65 over the total number of members, *Head – AgrExp* is a dummy equal to one if the household head works in the agriculture sector and cultivates at least one export crop, *Head – AgrNoExp* is equal to one if she works in the agriculture sector and cultivates no export crop, *Head – NonFarm* is equal to one if she works in the non-farm sector, *# cattle* is the number of livestock heads owned by the household, *# crops* is the number of crop types cultivated by the household, *# fields* is the number of fields owned by the household, and *Migrant* is a dummy variable equal to one if the household changed *communes* of residency since the previous survey wave.

Significance of the differences using a paired *t-test*. ***p<0.01, **p<0.05, *p<0.1.

Table A4: Potential mechanisms. Results corrected for attrition.

	(1)	(2)	(3)	(4)		(5)	(6)	(7)	(8)
<i>Dependent:</i>	Demographic changes				<i>Dependent:</i>	Economic activity and physical capital			
<i>Nb of members</i>	Violence (d)		Violence (log)		<i>Head:</i>	Violence (d)		Violence (log)	
	-0.244*	-0.220*	-0.0561	-0.0578	<i>AgrExp</i>	-0.00649	-0.0114	0.00324	0.00166
	(0.130)	(0.121)	(0.0437)	(0.0421)		(0.0321)	(0.0323)	(0.00720)	(0.00702)
R-squared	0.052	0.047	0.051	0.046		0.365	0.374	0.365	0.374
Observations	2,656	3,625	2,656	3,625		2,626	3,574	2,626	3,574
Number of HH	1,000	1,530	1,000	1,530		1,000	1,530	1,000	1,530
Mean dep. var.	5.238	4.990	5.238	4.990		0.168	0.175	0.168	0.175
<i>Dep. ratio</i>	-0.0432*	-0.0410**	-0.0132**	-0.0132**	<i>Head:</i>	0.0781*	0.0789*	0.00641	0.0101
	(0.0221)	(0.0201)	(0.00646)	(0.00609)	<i>AgrNoExp</i>	(0.0444)	(0.0417)	(0.0103)	(0.0101)
R-squared	0.043	0.053	0.044	0.054		0.450	0.431	0.449	0.429
Observations	2,656	3,625	2,656	3,625		2,626	3,574	2,626	3,574
Number of HH	1,000	1,530	1,000	1,530		1,000	1,530	1,000	1,530
Mean dep. var.	0.458	0.461	0.458	0.461		0.505	0.444	0.505	0.444
<i>Nb <15 y.o.</i>	-0.226**	-0.205**	-0.0672*	-0.0685*	<i>Head:</i>	-0.0732**	-0.0682**	-0.00943	-0.0109
	(0.108)	(0.0982)	(0.0372)	(0.0357)	<i>Non Farm</i>	(0.0344)	(0.0321)	(0.00832)	(0.00838)
R-squared	0.061	0.052	0.061	0.053		0.432	0.498	0.430	0.497
Observations	2,656	3,625	2,656	3,625		2,626	3,574	2,626	3,574
Number of HH	1,000	1,530	1,000	1,530		1,000	1,530	1,000	1,530
Mean dep. var.	2.337	2.299	2.337	2.299		0.331	0.386	0.331	0.386
<i>Nb >65 y.o.</i>	0.00417	-0.00116	0.00486	0.00409	<i># cattle</i>	-0.691***	-0.676***	-0.157***	-0.156***
	(0.0310)	(0.0273)	(0.00818)	(0.00764)		(0.232)	(0.231)	(0.0497)	(0.0494)
R-squared	0.050	0.043	0.050	0.043		0.135	0.136	0.133	0.134
Observations	2,656	3,625	2,656	3,625		1,766	2,211	1,766	2,211
Number of HH	1,000	1,530	1,000	1,530		927	1,348	927	1,348
Mean dep. var.	0.192	0.146	0.192	0.146		1.384	1.178	1.384	1.178
<i>Nb of births</i>	-0.0689	-0.0623	-0.0259	-0.0265†	<i># crops</i>	-0.154	-0.155	-0.0410	-0.0412
	(0.0600)	(0.0570)	(0.0181)	(0.0171)		(0.175)	(0.174)	(0.0476)	(0.0473)
R-squared	0.200	0.118	0.200	0.118		0.237	0.238	0.237	0.238
Observations	2,656	3,625	2,656	3,625		1,767	2,214	1,767	2,214
Number of HH	1,000	1,530	1,000	1,530		927	1,350	927	1,350
Mean dep. var.	0.944	1.075	0.944	1.075		6.130	5.975	6.130	5.975
<i>Nb of deaths</i>	0.116*	0.0938**	0.0746***	0.0500***	<i># fields</i>	0.0297	0.0327	-0.186	-0.218
	(0.0692)	(0.0423)	(0.0208)	(0.0139)		(0.474)	(0.459)	(0.170)	(0.167)
R-squared	0.070	0.033	0.074	0.035		0.375	0.385	0.378	0.389
Observations	1,656	2,625	1,656	2,625		897	1,344	897	1,344
Number of HH	872	1,402	872	1,402		716	1,139	716	1,139
Mean dep. var.	0.214	0.147	0.214	0.147		3.301	3.252	3.301	3.252
<i>Nb left for work</i>	0.0156	0.0134	0.000823	0.00214	OHH only	×		×	
	(0.0483)	(0.0314)	(0.0197)	(0.0125)	Year (d)	×	×	×	×
R-squared	0.032	0.033	0.032	0.032	Year × Prov. (d)	×	×	×	×
Observations	1,656	2,625	1,656	2,625	HH FE	×	×	×	×
Number of HH	872	1,402	872	1,402					
Mean dep. var.	0.0900	0.0651	0.0900	0.0651					
<i>Nb left for marriage</i>	-0.147	-0.0747	-0.0536	-0.0324					
	(0.132)	(0.0875)	(0.0437)	(0.0281)					
R-squared	0.044	0.027	0.044	0.027					
Observations	1,656	2,625	1,656	2,625					
Number of HH	872	1,402	872	1,402					
Mean dep. var.	0.572	0.368	0.572	0.368					
<i>Nb returns</i>	0.0606**	0.0463**	0.0304***	0.0202***					
	(0.0236)	(0.0208)	(0.00732)	(0.00702)					
R-squared	0.020	0.012	0.023	0.013					
Observations	1,656	2,625	1,656	2,625					
Number of HH	872	1,402	872	1,402					
Mean dep. var.	0.0248	0.0198	0.0248	0.0198					
OHH only	×		×						
Year (d)	×	×	×	×					
Year × Prov. (d)	×	×	×	×					
HH FE	×	×	×	×					

The unit of observation is the household (HH). The various dependent variables are described in the text. *Violence (d)* is a dummy equal to one if the household's locality reported at least one war-related casualty over the previous subperiod, and zero otherwise. *Violence (log)* is equal to the logarithm of one plus the number of war-related casualties reported in the household's locality over the previous subperiod. OHH stands for original households.

The smaller number of observations in the bottom part of columns (1) to (4) is explained by the fact that the number of deaths and mobility variables are only observed in 2007 (with 1998 as a reference) and 2012 (with 2007 as a reference). The smaller number of observations in the bottom part of columns (5) to (8) is due to the fact that only agricultural households are considered when measuring the number of cattle owned, crops cultivated, and fields. Moreover, and different from the two former variables, the number of fields is only available in 2007 and 2012, not in 1998.

Linear probability models. Robust standard errors clustered at the locality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1, †p=0.125.

Table A5: **Violence exposure and livestock.**

	Number of livestock heads
Violence (d)	-2.293*** (0.798)
Observations	6,286
Mean dep. var.	5.650
R-squared	0.134
2002 dummy	Yes
2002 x Province dummies	Yes
Household FE	Yes
Number of households	3,143

The unit of observation is the household. The dependent variable is the number of livestock heads owned in 1993 and 2002, from the ESDSR survey (UNPFA, 2002). *Violence (d)* is a dummy equal to one in 2002 if the household's commune reported at least one war-related casualty over 1993 – 2002, and to zero otherwise. It is equal to zero for all households in 1993.

Linear probability models. Weighted based on the sampling weights of the ESDSR survey. Robust standard errors clustered at the *commune* level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Tables

Table 1: **Violence exposure and poverty – Differences of means.**

		Not exposed to violence	Exposed to violence	Difference
<i>Panel A: 1998</i>	Poverty	0.622	0.742	-0.120*** (0.029)
	Food Poverty	0.496	0.624	-0.128*** (0.031)
	<i>Number of households</i>	<i>450</i>	<i>550</i>	<i>1,000</i>
<i>Panel B: 2007</i>	Poverty	0.566	0.716	-0.149*** (0.026)
	Food Poverty	0.489	0.627	-0.138*** (0.027)
	<i>Number of households</i>	<i>558</i>	<i>844</i>	<i>1,402</i>
<i>Panel C: 2012</i>	Poverty	0.602	0.700	-0.098*** (0.028)
	Food Poverty	0.533	0.630	-0.096*** (0.029)
	<i>Number of households</i>	<i>467</i>	<i>756</i>	<i>1,223</i>

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.

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

Table 2: Violence exposure and poverty over time.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Poverty				Food Poverty			
Part 1: Benchmark results								
<i>Panel A</i>								
Violence (d)	0.0736*** (0.0239)	0.0776** (0.0317)	0.0767** (0.0339)	0.0597* (0.0330)	0.0550* (0.0278)	0.0615* (0.0351)	0.0690* (0.0372)	0.0679* (0.0348)
R-squared	0.099	0.153	0.063	0.052	0.106	0.162	0.076	0.058
<i>Panel B</i>								
Violence (log)	0.0205*** (0.00704)	0.0164* (0.00901)	0.0169* (0.00985)	0.0151 [†] (0.00967)	0.0179** (0.00825)	0.0144 [‡] (0.00950)	0.0174* (0.0103)	0.0203** (0.00985)
R-squared	0.099	0.152	0.062	0.051	0.107	0.161	0.076	0.058
Observations	2,656	2,656	2,656	3,625	2,656	2,656	2,656	3,625
Number of HH	1,000	1,000	1,000	1,530	1,000	1,000	1,000	1,530
Mean dep. var.	0.685	0.685	0.685	0.667	0.591	0.591	0.591	0.577
Part 2: Benchmark results corrected for attrition.								
<i>Panel A</i>								
Violence (d)	0.0859*** (0.0251)	0.0852** (0.0343)	0.0819** (0.0359)	0.0661* (0.0344)	0.0659** (0.0288)	0.0676* (0.0372)	0.0695* (0.0385)	0.0693* (0.0361)
R-squared	0.099	0.155	0.058	0.047	0.110	0.167	0.075	0.057
<i>Panel B</i>								
Violence (log)	0.0237*** (0.00738)	0.0174* (0.00963)	0.0174* (0.0102)	0.0160 [#] (0.00993)	0.0216** (0.00862)	0.0167 [‡] (0.0101)	0.0184* (0.0106)	0.0213** (0.0102)
R-squared	0.099	0.154	0.057	0.046	0.111	0.167	0.075	0.057
Observations	2,656	2,656	2,656	3,625	2,656	2,656	2,656	3,625
Number of HH	1,000	1,000	1,000	1,530	1,000	1,000	1,000	1,530
Mean dep. var.	0.685	0.685	0.685	0.667	0.591	0.591	0.591	0.577
Part 3: Ever-exposed households only.								
<i>Panel A</i>								
Violence (d)	0.114*** (0.0414)	0.118** (0.0534)	0.131** (0.0528)	0.0787* (0.0451)	0.0866** (0.0425)	0.103* (0.0577)	0.108* (0.0562)	0.0960** (0.0471)
R-squared	0.090	0.132	0.096	0.086	0.087	0.122	0.097	0.081
<i>Panel B</i>								
Violence (log)	0.0344*** (0.0118)	0.0190 ^b (0.0128)	0.0247* (0.0139)	0.0212 [§] (0.0130)	0.0367*** (0.0126)	0.0245* (0.0125)	0.0297** (0.0130)	0.0374*** (0.0116)
R-squared	0.091	0.131	0.094	0.086	0.090	0.122	0.098	0.083
Observations	1,572	1,572	1,572	2,150	1,572	1,572	1,572	2,150
Number of HH	593	593	593	905	593	593	593	905
Mean dep. var.	0.731	0.731	0.731	0.717	0.637	0.637	0.637	0.627
Original HH only	×	×	×		×	×	×	
Year (d)	×	×	×	×	×	×	×	×
Year × Province (d)	×	×	×	×	×	×	×	×
Locality FE		×				×		
HH FE			×	×			×	×

The unit of observation is the household (HH). The dependent variable is equal to one for (food-)poor households, zero for non (food-)poor households. *Violence (d)* is a dummy equal to one if the household's locality reported at least one war-related casualty over the previous subperiod, and zero otherwise. *Violence (log)* is equal to the logarithm of one plus the number of war-related casualties reported in the household's locality over the previous subperiod.

Linear probability models. Robust standard errors clustered at the locality level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$, [†] $p = 0.123$, [‡] $p = 0.133$, [#] $p = 0.110$, ^b $p = 0.102$, [§] $p = 0.109$.

Table 3: Attrition: differences of means.

<i>Characteristics in 1998</i>	Tracked in 2007	Drop out in 2007	Difference
Poverty status	0.686 (0.0157)	0.703 (0.0405)	-0.017 (0.0439)
Food Poverty status	0.560 (0.0168)	0.609 (0.0433)	-0.050 (0.0469)
Violence exposure, 1999 – 2007 (d)	0.174 (0.0129)	0.141 (0.0308)	0.034 (0.0356)
Violence exposure, 1993 – 1998 (d)	0.561 (0.0168)	0.477 (0.0443)	0.084* (0.0471)
<i>Number of households</i>	<i>872</i>	<i>128</i>	<i>1,000</i>
<i>Characteristics in 2007</i>	Tracked in 2012	Drop out in 2012	Difference
Poverty status	0.663 (0.0135)	0.609 (0.0366)	0.054 (0.0380)
Food Poverty status	0.580 (0.0141)	0.520 (0.0374)	0.060 (0.0396)
Violence exposure, 1999 – 2007 (d)	0.171 (0.0108)	0.140 (0.0260)	0.031 (0.0299)
<i>Number of households</i>	<i>1,223</i>	<i>179</i>	<i>1,402</i>
Violence exposure, 1993 – 1998 (d)	0.578 (0.0177)	0.409 (0.0527)	0.169*** (0.0556)
<i>Number of households</i>	<i>784</i>	<i>88</i>	<i>1,000</i>

Significance of the differences using a paired *t-test*. Standard errors in parenthesis. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4: Attrition: bounds calculations.

		(1)	(2)	(3)	
		Lower bound	Benchmark	Upper bound	
<i>Imputation</i>	Original HH	Poverty	-0.0405 (0.0394)	0.0767** (0.0339)	0.116*** (0.0320)
		Food Poverty	-0.0460 (0.0397)	0.0690* (0.0372)	0.110*** (0.0360)
		Observations	3,000	2,656	3,000
	All	Poverty	-0.0421 (0.0376)	0.0597* (0.0330)	0.102*** (0.0305)
		Food Poverty	-0.0352 (0.0369)	0.0679* (0.0348)	0.109*** (0.0338)
		Observations	4,060	3,625	4,060
<i>Trimming</i>	Original HH	Poverty	0.0453 (0.0362)	0.0767** (0.0339)	0.0898** (0.0353)
		Food Poverty	0.0411 (0.0383)	0.0690* (0.0372)	0.0830** (0.0386)
		Observations	2,604	2,656	2,604
	All	Poverty	0.0389 (0.0344)	0.0597* (0.0330)	0.0762** (0.0343)
		Food Poverty	0.0535 [†] (0.0355)	0.0679* (0.0348)	0.0815** (0.0360)
		Observations	3,553	3,625	3,553

Column (2) reproduces the coefficient for *Violence (d)* in columns (3), (4), (7) and (8) of the first part of Panel A, Table 2. Columns (1) and (3) respectively report lower and upper bounds for this coefficient of interest under different assumptions.

The first part of the table combines real and imputed data, assuming alternately that all the exposed (non-exposed) households who drop out are poor (non-poor) and the opposite.

The second part of the table trims the sample to reach a balance in terms of attrition between the exposed and non-exposed groups. We calculate the lower (upper) bound by dropping, among exposed households, those that contribute most (least) to a positive correlation between violence and poverty, i.e. those that have the smallest (lowest) residual when we regress the dependent variable on the control variables (household fixed effects, national and province-level trends). Since we only have dichotomous variables in these regressions, multiple observations have the same residual value, and dropping the required number of observations with the lowest/highest residual implies a random draw. We re-iterate this procedure 1,000 times for each estimation, and report the average coefficients and standard-errors estimated over these 1,000 regressions.

Standard errors in parenthesis. ***p<0.01, **p<0.05, *p<0.1, [†] p=0.132.

Table 5: **Determinants of locality-level exposure.**

<i>Dependent variable:</i>	(1)	(2)	(3)
<i>Violence exposure dummy</i>	1993 – 1998	1999 – 2007	
Distance to Bujumbura	0.0809 (0.194)	0.0546 (0.207)	0.0896 (0.207)
Altitude	0.646 (0.491)	0.368 (0.487)	0.369 (0.472)
Population density – 1990	0.0479 (0.125)	-0.0704 (0.121)	-0.0694 (0.117)
Share votes Ndadaye – 1993	0.00382 (0.00337)	0.000448 (0.00316)	-0.000529 (0.00317)
% HHH women – 1998			-0.00245 (0.00252)
Socioeconomic homogeneity – 1998			0.0833* (0.0442)
Literacy HHH – 1990 (prov)	0.0232* (0.0134)	0.00951 (0.0152)	0.00400 (0.0155)
Resources (log) – 1990 (prov)	-0.696 (0.494)	-0.665* (0.386)	-0.568 (0.406)
% Farmer HHH – 1990 (prov)	0.0322 (0.0202)	0.0221 (0.0148)	0.0222 (0.0150)
% HHH under 30 – 1990 (prov)	0.0186 (0.0175)	0.0160 (0.0157)	0.0186 (0.0154)
% HHH above 50 – 1990 (prov)	-0.0186 (0.0178)	0.0115 (0.0237)	0.0159 (0.0226)
Observations	94	94	93
Mean dep. var.	0.543	0.160	0.161
R-squared	0.250	0.195	0.230

The unit of observation is the locality. The dependent variable is equal to one if the locality reported at least one war-related casualty over the specified subperiod (1993 – 1998 in column (1), 1999 – 2007 in columns (2) and (3)), and zero otherwise.

Distance to Bujumbura, *Altitude*, *Population density – 1990*, *Share votes Ndadaye – 1993*, *% HHH women – 1998*, *Socioeconomic homogeneity – 1998* are provided by Voors et al. (2012) at the locality level. *Literacy HHH – 1990*, *Resources (log) – 1990*, *% HHH farmer – 1990*, *% HHH under 30 – 1990*, *% HHH above 50 – 1990* are provided by ISTEEBU (1993) at the province level. *Share votes Ndadaye* is the share of votes obtained by Melchior Ndadaye in 1993. *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* (respectively, *% HHH above 50*) is the share of household heads under 30 (respectively, above 50).

Linear probability models. Robust standard errors in parentheses. ***p<0.01, **p<0.05, *p<0.1

Table 6: **Household-level pre-violence livestock.**

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)
<i>Number of households</i>	<i>950</i>	<i>2,211</i>	<i>3,161</i>
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)
<i>Number of households</i>	<i>2,471</i>	<i>690</i>	<i>3,161</i>

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, and/or poultry owned by households in 1993, and is provided by the ESDSR survey (UNPFA, 2002).

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

Table 7: Potential mechanisms.

	(1)	(2)	(3)	(4)		(5)	(6)	(7)	(8)
	Demographic changes					Economic activity and physical capital			
<i>Dependent:</i>	Violence (d)		Violence (log)		<i>Dependent:</i>	Violence (d)		Violence (log)	
<i>Nb of members</i>	-0.290** (0.125)	-0.248** (0.118)	-0.0658 [†] (0.0439)	-0.0647 [‡] (0.0423)	<i>Head:</i>	-0.00740 (0.0338)	-0.0123 (0.0338)	0.00301 (0.00739)	0.00134 (0.00717)
R-squared	0.053	0.046	0.052	0.045	<i>AgrExp</i>	0.380	0.387	0.380	0.387
Observations	2,656	3,625	2,656	3,625		2,626	3,574	2,626	3,574
Number of HH	1,000	1,530	1,000	1,530		1,000	1,530	1,000	1,530
Mean dep. var.	5.238	4.990	5.238	4.990		0.168	0.175	0.168	0.175
<i>Dep. ratio</i>	-0.0397** (0.0195)	-0.0360** (0.0178)	-0.0120** (0.00566)	-0.0118** (0.00533)	<i>Head:</i>	0.0856** (0.0427)	0.0836** (0.0403)	0.00840 (0.00987)	0.0117 (0.00963)
R-squared	0.048	0.056	0.049	0.057	<i>AgrNoExp</i>	0.466	0.444	0.464	0.443
Observations	2,656	3,625	2,656	3,625		2,626	3,574	2,626	3,574
Number of HH	1,000	1,530	1,000	1,530		1,000	1,530	1,000	1,530
Mean dep. var.	0.458	0.461	0.458	0.461		0.505	0.444	0.505	0.444
<i>Nb < 15 y.o.</i>	-0.268** (0.106)	-0.228** (0.0973)	-0.0796** (0.0371)	-0.0777** (0.0356)	<i>Head:</i>	-0.0799** (0.0305)	-0.0722** (0.0285)	-0.0111 [‡] (0.00729)	-0.0122 [‡] (0.00747)
R-squared	0.066	0.055	0.066	0.056	<i>Non Farm</i>	0.453	0.524	0.451	0.523
Observations	2,656	3,625	2,656	3,625		2,626	3,574	2,626	3,574
Number of HH	1,000	1,530	1,000	1,530		0.331	0.386	0.331	0.386
Mean dep. var.	2.337	2.299	2.337	2.299		1,000	1,530	1,000	1,530
<i>Nb > 65 y.o.</i>	0.00352 (0.0286)	-0.00204 (0.0249)	0.00581 (0.00727)	0.00494 (0.00676)	<i># cattle</i>	-0.780*** (0.251)	-0.764*** (0.249)	-0.177*** (0.0539)	-0.176*** (0.0536)
R-squared	0.050	0.043	0.051	0.043		0.144	0.144	0.141	0.142
Observations	2,656	3,625	2,656	3,625		1,766	2,211	1,766	2,211
Number of HH	1,000	1,530	1,000	1,530		927	1,348	927	1,348
Mean dep. var.	0.192	0.146	0.192	0.146		1.384	1.178	1.384	1.178
<i>Nb of births</i>	-0.0886 (0.0622)	-0.0725 (0.0588)	-0.0316* (0.0188)	-0.0304* (0.0176)	<i># crops</i>	-0.142 (0.177)	-0.146 (0.176)	-0.0427 (0.0483)	-0.0430 (0.0480)
R-squared	0.212	0.122	0.213	0.123		0.246	0.248	0.246	0.248
Observations	2,656	3,625	2,656	3,625		1,767	2,214	1,767	2,214
Number of HH	1,000	1,530	1,000	1,530		927	1,350	927	1,350
Mean dep. var.	0.944	1.075	0.944	1.075		6.130	5.975	6.130	5.975
<i>Nb of deaths</i>	0.120* (0.0683)	0.0997** (0.0424)	0.0737*** (0.0213)	0.0503*** (0.0140)	<i># fields</i>	0.0480 (0.490)	0.0314 (0.475)	-0.191 (0.179)	-0.227 (0.175)
R-squared	0.068	0.031	0.072	0.033		0.383	0.394	0.386	0.399
Observations	1,656	2,625	1,656	2,625		897	1,344	897	1,344
Number of HH	872	1,402	872	1,402		716	1,129	716	1,129
Mean dep. var.	0.214	0.147	0.214	0.147		3.301	3.252	3.301	3.252
<i>Nb left for work</i>	0.0136 (0.0513)	0.0114 (0.0336)	-0.000659 (0.0212)	0.000932 (0.0131)	OHH only	×	×	×	×
R-squared	0.034	0.033	0.034	0.033	Year (d)	×	×	×	×
Observations	1,656	2,625	1,656	2,625	Year × Prov. (d)	×	×	×	×
Number of HH	872	1,402	872	1,402	HH FE	×	×	×	×
Mean dep. var.	0.0900	0.0651	0.0900	0.0651					
<i>Nb left for marriage</i>	-0.157 (0.135)	-0.0813 (0.0909)	-0.0500 (0.0446)	-0.0302 (0.0286)					
R-squared	0.042	0.026	0.041	0.025					
Observations	1,656	2,625	1,656	2,625					
Number of HH	872	1,402	872	1,402					
Mean dep. var.	0.572	0.368	0.572	0.368					
<i>Nb returns</i>	0.0628*** (0.0237)	0.0481** (0.0208)	0.0321*** (0.00731)	0.0214*** (0.00687)					
R-squared	0.021	0.013	0.025	0.015					
Observations	1,656	2,625	1,656	2,625					
Number of HH	872	1,402	872	1,402					
Mean dep. var.	0.0248	0.0198	0.0248	0.0198					
OHH only	×	×	×	×					
Year (d)	×	×	×	×					
Year × Prov. (d)	×	×	×	×					
HH FE	×	×	×	×					

The unit of observation is the household (HH). The various dependent variables are described in the text. *Violence (d)* is a dummy equal to one if the household's locality reported at least one war-related casualty over the previous subperiod, and zero otherwise. *Violence (log)* is equal to the logarithm of one plus the number of war-related casualties reported in the household's locality over the previous subperiod. OHH stands for original households.

The smaller number of observations in the bottom part of columns (1) to (4) is explained by the fact that the number of deaths and mobility variables are only observed in 2007 (with 1998 as a reference) and 2012 (with 2007 as a reference). The smaller number of observations in the bottom part of columns (5) to (8) is due to the fact that only agricultural households are considered when measuring the number of cattle owned, crops cultivated, and fields. Moreover, and different from the two former variables, the number of fields is only available in 2007 and 2012, not in 1998.

Linear probability models. Robust standard errors clustered at the locality level in parentheses. *** p < 0.01, ** p < 0.05, * p < 0.1, [†]p = 0.137, [‡]p = 0.130, [§]p = 0.104.

Table 8: Violence exposure and long-run poverty status.

	(1)	(2)	(3)	(4)	(5)	(6)
	Poverty					
	Permanent non-poor	Switching	Chronic poor	Permanent non-poor	Switching	Chronic poor
<i>Panel A</i>						
Violence 1993 – 1998 (d)	-0.0492** (0.0195)	0.0833** (0.0415)	-0.0341 (0.0412)	-0.0398** (0.0154)	0.0308 (0.0344)	0.00906 (0.0341)
Violence 1999 – 2007 (d)	-0.0757*** (0.0261)	0.0300 (0.0405)	0.0457 (0.0382)	-0.0589*** (0.0151)	0.0302 (0.0327)	0.0287 (0.0346)
<i>Panel B</i>						
Violence 1993 – 1998 (log)	-0.0112** (0.00506)	0.00656 (0.00986)	0.00468 (0.0100)	-0.0104*** (0.00367)	-0.00271 (0.00831)	0.0131 [†] (0.00809)
Violence 1999 – 2007 (log)	-0.0242** (0.0118)	-0.0142 (0.0206)	0.0384** (0.0173)	-0.0232*** (0.00627)	0.000482 (0.0122)	0.0227* (0.0129)
Observations	784	784	784	1,223	1,223	1,223
Original HH only	×	×	×			
Province (d)	×	×	×	×	×	×
	(7)	(8)	(9)	(10)	(11)	(12)
	Food poverty					
	Permanent non-poor	Switching	Chronic poor	Permanent non-poor	Switching	Chronic poor
<i>Panel A</i>						
Violence 1993 – 1998 (d)	-0.0247 (0.0265)	0.0438 (0.0403)	-0.0192 (0.0386)	-0.0416* (0.0221)	0.0342 (0.0313)	0.00740 (0.0326)
Violence 1999 – 2007 (d)	-0.0489 (0.0353)	0.0450 (0.0575)	0.00394 (0.0407)	-0.0481* (0.0278)	0.0290 (0.0452)	0.0191 (0.0334)
<i>Panel B</i>						
Violence 1993 – 1998 (log)	-0.00587 (0.00650)	0.00129 (0.0103)	0.00458 (0.00946)	-0.0110** (0.00548)	0.00311 (0.00786)	0.00793 (0.00751)
Violence 1999 – 2007 (log)	-0.0159 (0.0136)	-0.00232 (0.0220)	0.0182 (0.0175)	-0.0226** (0.00997)	0.00299 (0.0150)	0.0196 (0.0138)
Observations	784	784	784	1,223	1,223	1,223
Original HH only	×	×	×			
Province (d)	×	×	×	×	×	×

The unit of observation is the household (HH). The dependent variable is equal to 0 for households who are permanently non-poor, 1 for switchers (poor in one or two of the three data waves), and 2 for households who are poor in all three waves. *Violence 1993 – 1998 (d)* (resp., *Violence 1999 – 2007 (d)*) is a dummy equal to one if the household's locality reported at least one war-related casualty between 1993 and 1998 (resp., between 1998 and 2007), and zero otherwise. *Violence 1993 – 1998 (log)* (resp., *Violence 1999 – 2007 (log)*) is equal to the logarithm of one plus the number of war-related casualties reported in the household's locality between 1993 and 1998 (resp., between 1998 and 2007). *Exposed once* is a dummy variable equal to one for households exposed to violence during one subperiod (regardless of which one), and to zero for households never exposed or exposed over the two subperiods. *Exposed twice* is a dummy variable equal to one for households exposed to violence over the two subperiods, and to zero for households never exposed or exposed during one subperiod only.

Multinomial logit. Robust standard errors clustered at the locality level in parentheses. *** p<0.01, ** p<0.05, * p<0.1, [†]p=0.105, [‡]p=0.109.