The World Bank Economic Review, 32(2), 2018, 357–382
doi: 10.1093/wber/lhw033
Article

Who Benefited from Burundi's Demobilization Program?

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Abstract

This paper assesses the impact of the demobilization, reinsertion and reintegration program in post-war Burundi. Two major rebel groups benefited from cash and in-kind transfers, the CNDD-FDD from 2004, and the FNL from 2010. We combine panel data of households collected in 2006 and 2010 with official records from the National Commission for Demobilization, Reinsertion and Reintegration. We find that the cash payments received by FNL demobilized households had a positive impact on consumption, nonfood spending and investments. The program also generated positive spillovers in the villages where FNL combatants returned. Ex-combatants indeed spent a large part of their allowance on consumption goods and clothing, thereby generating a short-run economic boom in villages. However, the long-run evolution of consumption indicators is negative for CNDD-FDD households, as well as for villages where CNDD-FDD combatants returned, suggesting that the direct impact and the spillovers of the program vanished in the long run.

JEL classification: D74, I32, I38, O12, N47

Disarmament, demobilization and reintegration (DDR) programs are an essential component of most peace-building programs. The primary goal of such programs is to secure short-run stability in postconflict countries, clearing the way for structural reforms (Pugel 2009). Over the last two decades, such programs have been implemented in more than 30 countries. More than 2.5 million people have benefited

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from some form of demobilization worldwide since 1989, with an average annual cost of more than $US 600 million (Muggah 2009).

Despite their high costs and a seemingly widespread belief in their extreme importance for stability in postconflict countries, empirical evidence on the effectiveness of DDR programs remains scarce. Comparing soldiers who voluntarily participated in one of these programs to those who did not, Humphreys and Weinstein (2007) find little evidence that the DDR program in Sierra Leone facilitated demobilization and social reintegration. By exploiting a disruption in the DDR transfers in Burundi, Gilligan et al. (2013) found that in-kind allocations led to a decrease in poverty and improved livelihoods among beneficiary ex-combatants. While they concluded that the program was successful in terms of economic reintegration, they found no effect on ex-combatants' overall satisfaction and attitudes towards norms.

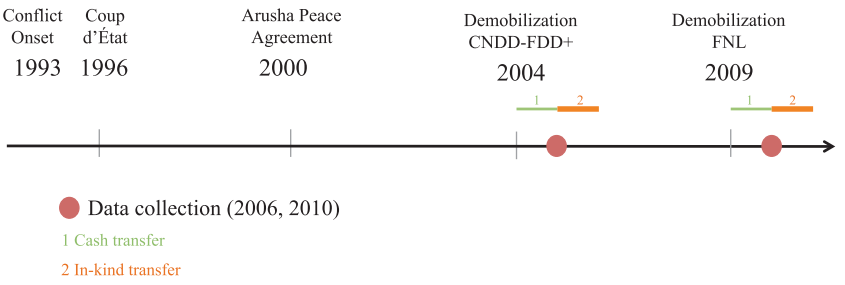
Building on these insightful studies, we study the impact of the Burundian DDR program and extend the literature on DDR assessments in three ways: (i) by capturing the effect of potential spillovers to civilian households; (ii) by disentangling the short-run impacts from long-run dynamics; and (iii) by distinguishing between DDR phases. The DDR program in Burundi targeted 33,500 ex-combatants at the end of the 1993–2009 civil war. The program was conducted in two waves. The first wave started in December 2004 with the demobilization of the major Hutu rebel group, the CNDD-FDD[1] and the demobilization of a few other minor factions. The second wave started in 2009 with the demobilization of the second larger Hutu rebel group, the Palipehutu-FNL.[2] We denote by CNDD+ the beneficiaries of the first wave of demobilization and by FNL the beneficiaries of the second wave. The program featured two subsequent components. Through the reinsertion component, demobilized combatants received a cash allowance of at least US$ 515 distributed over 18 months. Demobilized combatants then benefited from a reintegration grant, worth US$ 545 and distributed in-kind.

According to economic theory, the impact of cash and in-kind transfers to demobilized combatants depends on whether they face financial, social or behavioral constraints which are relaxed following the transfers (Fafchamps et al. 2011; Blattman et al. 2014). If agents do not face such constraints, they should already produce at their efficient scale, and it follows that DDR allowances should be expected to be consumed or saved for future consumption. Greater demand should then generate a temporary economic boom at the local level. The resulting increases in price[3] and turnover should benefit local vendors, but disadvantage buyers (Cunha et al. 2011; D'Aoust et al. 2013). If, however, there exist financial, social, or behavioral constraints that are relieved by demobilization transfers, demobilization allowances would be expected to be invested in productive assets. In this case, the impact on consumption and assets of beneficiary households should be positive and long-lasting, but the magnitude of the consumption increase should be rather small (Gertler et al. 2012). Spillovers on nonbeneficiaries at the community level are expected to be marginal. Examining which of the consumption or investment scenarios prevails is an empirical question, which we tackle in the empirical analysis.

We use a household panel dataset to capture the direct and indirect effect of the DDR program on beneficiary and nonbeneficiary households. The first wave of the panel was collected in 2006, while the reintegration phase of the demobilization of CNDD+ combatants was taking place. This wave is representative at the provincial level. The second wave was collected in 2010, in the midst of the reinsertion phase for FNL combatants, and about five years after the demobilization of CNDD+ combatants. The timeline below summarizes graphically the various components of our assessment (fig. 1).

1 Conseil National pour la Défense de la Démocratie—Forces de Défense de la Démocratie.
2 Palipehutu—Forces Nationales de Libération.
3 This holds only if the degree of openness and competition in the local economy is low, as is the case in rural Burundi.

Figure 1. Timeline



Our identification strategy exploits several key facts: (i) not all households hosted ex-combatants; (ii) not all households with ex-combatants benefited from allowances; (iii) the waves of the demobilization program took place before and in between the two survey rounds of the panel; and (iv) some communities in the panel had ex-combatants among their populations whereas other communities did not. Our outcomes of interest include indicators of consumption, nonfood spending, livestock, and prices. We call all households hosting an ex-combatant *returnees*, and we call the subset of returnees which received demobilization allowances *demobilized*. In order to measure the short-run impact of reinsertion allowances on beneficiaries, we compare the outcomes of FNL demobilized households and FNL returnee households that did not benefit from reinsertion allowances. In the same vein, we measure the long-run evolution of the impact of program on CNDD+ by comparing outcomes of the CNDD+ demobilized households and CNDD+ returnee households that did not benefit from the DDR program. We measure the short-run (resp. long-run) spillovers of the program on non-beneficiaries by studying how households' outcomes are affected by the density of demobilized FNL (resp. CNDD+) in their community.

We find that demobilization grants had a positive and significant impact on economic outcomes of FNL beneficiaries in the short run. Importantly, we also identify the presence of positive spillovers on nonbeneficiary households in the short run. This result is encouraging as it shows that reinsertion grants benefited the local economy, which may have increased local acceptance of ex-rebels. However, our analysis identifies a negative evolution of consumption and nonfood spending for CNDD+ ex-combatants. Similarly, we find that the density of demobilized combatants who benefited from the first wave of the DDR program is negatively correlated with outcomes of interest.

These results show that the consumption scenario prevailed in Burundi. When they returned home, demobilized FNLs spent a large part of their allowances on their immediate needs such as food and other basic spending. The demand for locally produced goods rose, which generated a short-run economic boom in host villages. Consistent with this narrative, we find that prices increased in the local market. The results on CNDD+ demobilized households are also consistent with the consumption scenario. Like demobilized FNL households in 2010, we argue that CNDD+ households are likely to have experienced a consumption boom when they benefited from reinsertion allowances as of 2004. Verwimp and Bundervoet (2009) found that the consumption of CNDD+ ex-combatants' households increased by 34% between 1998 and 2007 compared to civilian households. Qualitative evidence also confirms that CNDD+ ex-combatants spent a large share of their grants in nonproductive ways (Uvin 2007). Under the assumption that CNDD+ households experienced a consumption boom similar to FNL households when they benefited from reinsertion allowances, our findings show that CNDD+ households were not able to maintain a high level of consumption after the exhaustion of their demobilization money.

Importantly, Gilligan et al. (2013) also assessed the impacts of the Burundian DDR and focused on the short-run impact of the reintegration phase for CNDD+ combatants. Their analysis exploited an

exogenous bureaucratic failure in the implementation of the program, which resulted in delays of the distribution of in-kind reintegration allowances for demobilized combatants living in certain provinces. Our analysis complements the study of Gilligan et al. (2013) by using panel data methods to assess the impact of the reinsertion phase on FNL combatants, by studying long-run dynamics for CNDD+ combatants, and by capturing the impact of spillovers on civilian households.

The paper is organized as follows: Section I provides details on Burundian DDR. In section II, we present a theoretical framework describing the expected effects of demobilization allowances. In section III, we describe the data and introduce the identification strategy. Results are presented in section IV and interpreted in section V. Section VI concludes.

I. Background of the DDR Program in Burundi

The 1993–2009 conflict in Burundi was driven by years of ethnic discrimination (Vandeginste 2009). A few years after independence in July 1962, the Tutsi elite established a military dictatorship. Tensions between the Hutu majority and the Tutsi authoritarian government regularly triggered violent clashes. In 1988, Tutsi President Pierre Buyoya launched a process of political liberalization, establishing a government of national unity and organizing elections in 1993. In October 1993, four months after his election, the newly elected Hutu President Ndadaye was assassinated. This event set off a decade of civil war between Hutu rebel groups and the Tutsi-led Burundian army.

In 2000, the Arusha Peace Agreement laid the foundations for a peace process and a new constitution based on power-sharing and de-ethnicized political competition. Two Hutu rebel groups, the CNDD-FDD and the Palipehutu-FNL, refused to sign the peace agreement. The ethnic conflict turned into a civil war opposing the transitional government and the two Hutu rebel groups. In 2003, the CNDD-FDD agreed to lay down its weapons and its leaders were given positions in the government, in the national assembly and in the administration. Combatants from the national army (FAB) and from the CNDD-FDD were selected to join the national police and the new national army (The Forces de Défense de la Nation—FDN). Selection criteria were based on stated preferences, age, health status, and experience. Those who were not selected, about 23,000 combatants from CNDD-FDD and FAB, were assigned to the disarmament, demobilization, and reinsertion (DDR) program. The DDR program was officially launched in December 2004.

In 2009, the Palipehutu-FNL rebels in turn gave up their arms, and the FNL became a political party, with 33 minor posts given to the FNL leadership. Of the 20,000 Palipehutu-FNL members,[4] 3,500 were reintegrated in the Burundian army and 6,500 benefited from the DDR program.

The program was coordinated by the National Commission for Demobilization, Reinsertion and Reintegration,[5] under the supervision of the World Bank. The program was organized in three phases: the demobilization, the reinsertion and the reintegration. The demobilization phase started with disarmament, followed by the transfer of ex-combatants to a demobilization center. Ex-combatants spent eight days in the center, attending training on economic strategies and opportunities, HIV/AIDS, civic responsibility, as well as peace and reconciliation.

As part of the reinsertion phase, demobilized combatants received a cash allowance worth 18-month salary, paid in four installments over a period of eighteen months. Demobilized combatants received the first reinsertion payment when leaving the demobilization center. Called the Transitional Subsistence Allowance (TSA) by the World Bank, the reinsertion money was dedicated to "enable the ex-combatants to return to their community and to sustain themselves and their families for a limited period following

4 This number was inflated by the FNL leadership. Knowing that the CNDD-FDD had received the DDR and expecting to benefit from such a program, many people did join the rebel group right before the agreement was signed.

5 *Commission Nationale de Démobilisation, Réinsertion and Réintégration* (CNDRR).

demobilization" (The World Bank Group 2004). The total amount of the cash allowance increased with military rank, starting from a minimum of FBU 566,000 (US$ 515).[6] The average benefit amounted to FBU 644,000 per demobilized combatant (US$ 586). Data from our survey in 2010 shows that civilian households consumed on average about FBU 190,000 (US$ 173) per adult equivalent per year, which is about one third of the minimum cash allocation to FNL rebels.

Finally, the reintegration phase consisted of a one-off in-kind transfer worth FBU 600,000 (US$ 545). The ex-combatants could choose from a range of options, including vocational training, support for agro-pastoral activity, and start-up material for a small business or a construction project. This phase was launched in September 2006 for the CNDD+.[7] Of the 23,000 CNDD+ beneficiaries, 85% had received the reintegration support by December 2008. This phase was just starting for the FNL ex-combatants at the time of our 2010 survey.

Some rebels had ties with the factions, but did not benefit from the DDR program. A list of criteria was established in order to assess the military aptitudes of candidates, defining whether rebels were eligible or not for the DDR program. The DDR program also included the disarmament and the dismantling of militias. These were formed by people helping the factions, notably in terms of logistics. These people were called "Gardiens de la Paix" (GdP) if they belonged to the FAB, "Militants Combatants" (MC) if they were part of the CNDD-FDD and "Adultes Associés" (AA) if they supported the FNL. 20,000 GdP, 10,000 MC and 11,000 AA benefited from the program. They received FBU 100,000 (US$ 91) as compensation.

II. Theoretical Framework

The DDR program in Burundi was a social transfer program combining cash and in-kind benefits. We aim to assess its short- and long-run impacts, and to disentangle its direct impact on beneficiaries from its indirect impact on civilians. In this section, we rely on the theoretical and empirical literature on cash and in-kind transfers to predict the direction and the size of these effects.

Standard models of investment predict that agents produce at their efficient scale if financial markets function well (Blattman et al. 2014; Fafchamps et al. 2011). With well-functioning financial markets, cash and in-kind allowances are therefore not used to expand production, but rather to increase current consumption and future consumption through savings. Consumption smoothing depends on individuals' discount rate: individuals with strong preference for the present, that is, a high discount rate, will save less. Cash and in-kind transfers have therefore no impact on capital and profit in the long run in this case.

These predictions change if agents face financial, social or behavioral constraints which are relaxed as a result of the cash and in-kind transfers. For example, businesses facing liquidity or credit constraints should grow rapidly when given additional capital (Besley 1995; Sadoulet et al. 2001). Whether this capital is provided in cash or in-kind should not affect the level of investment as long as people make decisions regarding asset accumulation in an integrated manner, that is, if consumption, profit, capital and saving are regarded as fungible (Fafchamps et al. 2011). However, cash and in-kind allowances can have systematically different effects if asset integration fails, for example because of self-control issues (Banerjee and Mullainathan 2010), intra-household allocation inefficiencies (Udry 1996) or redistributive pressures (Platteau 2000).

A large empirical literature documents the positive impacts of cash transfers on consumption (see, e.g., Duflo 2003; Hoddinott and Skoufias 2004; Fafchamps et al. 2011; Cunha 2014; Blattman et al. 2014; Haushofer and Shapiro 2014; Hidrobo et al. 2014). Studies on the dynamics of social transfers

6 All US$ equivalents are expressed in 2010 US$. US$ 1 was worth 1,100 Burundian francs in 2010.

7 This phase did not start before March 2008 in the central provinces (Gilligan et al. 2013); the provinces included in the present analysis were not affected by this disruption.

and consumption show that consumption tends to peak just after the payment (Stephens Jr. 2003; Shapiro 2005; Stephens 2006; Mastrobuoni and Weinberg 2009). The empirical literature also documents that cash transfers can have a positive impact on assets (Haushofer and Shapiro 2014; Blattman et al. 2014; de Mel et al. 2008). Investment in assets can in turn generate additional income and consumption, thereby generating a multiplier effect (Sadoulet et al. 2001; Gertler et al. 2012). The impact of transfers depends on the presence of conditionalities (Baird et al. 2011), on the timing and frequency of transfers (Stephens Jr. 2003; Haushofer and Shapiro 2014), on self-control issues, and on the extent to which preferences are present-biased (Shapiro 2005; Fafchamps et al. 2011; Ozdenoren et al. 2012; Evans and Popova 2014).

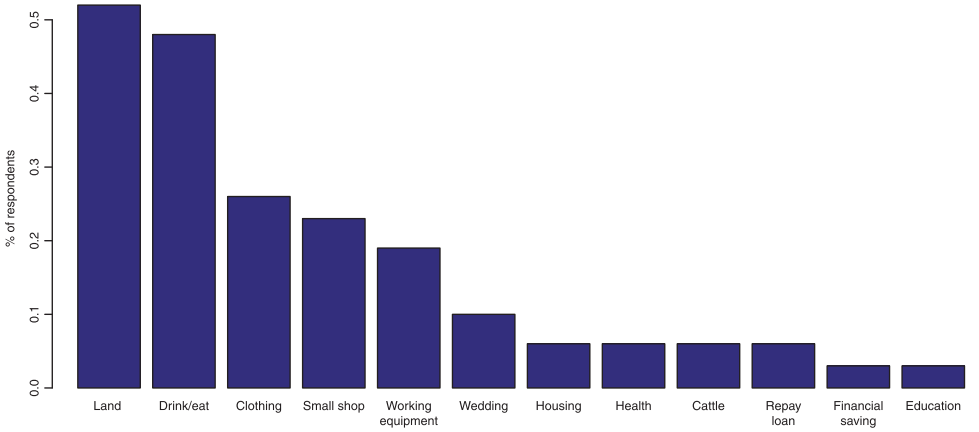
Recently, several studies in development economics have identified large externalities of development programs on nonbeneficiaries. These studies conclude that the impact of interventions may be substantially underestimated if such spillovers are not considered. Nonbeneficiaries may be affected by the interventions through three main channels (Glennerster and Takavarasha 2013). The first channel is physical. For example, untreated individuals can be positively affected by a treatment slowing the spread of a contagious epidemic (Miguel and Kremer 2004). The second channel is informational or behavioral: programs can have an impact on the untreated through imitation and communication with treated peers (Kim et al. 1999; Bobonis and Finan 2009). The third channel relates to market-wide effects. In our case, social transfer programs can generate externalities in village economies because the program's money immediately passes from recipient to nonbeneficiary households as the cash is used to purchase goods and services or to transfer money to peers. Resulting general equilibrium effects have been highlighted in research on agricultural growth multipliers (Haggblade et al. 1991; Block 1999; Filipski et al. 2015) as well as in work on structural models of village economies (Kaboski and Townsend 2011; Attanasio et al. 2012).

As local demand increases, local traders' revenue increases. Under general assumptions, prices of normal goods in the local market should then increase in the short run if the degree of openness and competition in the local economy is low (Cunha et al. 2011; D'Aoust et al. 2013). In the long run, prices should return to their equilibrium as local traders adapt to the increased demand and as excess money escapes the local economy for purchasing goods that are not produced locally (Cunha et al. 2011; D'Aoust et al. 2013). The short-run increase in price should further increase the profit of local traders, while being detrimental for buyers (Filipski et al. 2015). The price increase may be attenuated if cash allowances are partly invested in assets that increase productive capacity in villages. For goods that are imported locally and characterized by oligopolistic competition, prices could even decrease following the positive demand shock if local traders face fixed costs in a Bertrand competition model or if the demand shock causes more stores to sell the good, thereby increasing competition in a Cournot-Nash framework (Cunha et al. 2011; D'Aoust et al. 2013).

Few empirical studies have examined the price effects and the externalities of cash transfers on nonbeneficiaries. For example, using the data from Progresa in Mexico, Angelucci and De Giorgi (2009) and Barrientos and Sabatés-wheeler (2009) find that noneligible households in treatment areas show significantly higher levels of food consumption and asset holdings following the introduction of the conditional cash transfer program, compared to noneligible households in control areas. Studying the impact of the PAL program in Mexico, Cunha et al. (2011) find that cash transfers in remote villages led to higher food prices, which in turn increased agricultural profits and household wealth. They also show theoretically and empirically that prices are lower following in-kind transfers, which affect supply whereas cash transfers affect demand.

This review of the literature shows that the impacts and externalities of the Burundian DDR are expected to depend on how ex-combatants used their allowances. The use of allowances ultimately depends on socio-economic constraints faced by beneficiaries and socio-economic conditions in the village economies. Figure 2 shows how ex-combatants from our sample reported spending their grant. It

Figure 2. DDR Grants Spending by Ex-combatants



Source: Authors' analysis based on data described in the text.

summarizes the self-reported information provided by 22 demobilized ex-combatants and 9 GdP/MC/AA who were interviewed in 2010. Respondents could give a maximum of three answers.[8]

In line with the literature, we observe that a large proportion of ex-combatants used a share of the money to buy consumption goods: 48% reported purchasing food and drinks, and 26% reported buying clothes. This consumption boom is expected to be short-lived, especially if the discount rate of ex-combatants is low and if saving opportunities are scarce. In the short run, the demand increase should induce a jump in prices. This change in prices should increase the sales of shopkeepers and farmers, who are therefore expected to benefit from the return of demobilized ex-rebels. Other households may suffer from inflation. However, the global effect on nonbeneficiaries is expected to be positive, as most households are involved in some kind of trading activity.

Ex-combatants also invested part of their allowances in productive assets. More than 50% of ex-combatants reported investing part of the grant in a plot of land, 23% of them invested in a small shop, 19% in working equipment, and one ex-combatant reported buying a cow. The economic situation of ex-combatants who invested in productive assets is expected to improve in the short run and in the long run following their enhanced production capacity. The resulting long-run increase in consumption is expected to be marginal compared to the size of the immediate consumption boom following the distribution of reinsertion allowances.[9] As a corollary, the price effect and the externalities generated by investments of DDR money should be marginal except if investments were concentrated in specific domains. The in-kind allowances, which were distributed as part of the reintegration package, are likewise expected to have a positive but small impact on consumption in the long run.

In accordance with this line of reasoning, we propose to test the following hypotheses:

- *Hypothesis 1.* Reinsertion allowances received by demobilized combatants had a positive impact on the levels of consumption and investment of their household.
- *Hypothesis 2.* The short-run impacts on consumption of beneficiary households were short-lived. The impacts on investment lasted in the long run

8 In most households, they did not remember exactly how much they had spent for each category of expenses so fig. 2 only shows the proportion of respondents that declared having spent part of their allowance in each category.

9 For example, in their evaluation of Progresa in Mexico, Gertler et al. (2012) estimated that for each peso transferred, households consumed 74 cents and invested the rest, increasing long-term consumption by only 1.6 cents.

- *Hypothesis 3*. Nonbeneficiaries were also indirectly affected by the program. This indirect impact depends on the main occupation of households.
- *Hypothesis 4*. Higher demand for normal goods induced a price increase in the short run.

By testing these hypotheses, we contribute not only to the scarce literature on the impact of demobilization programs, but also to the literature on the impact of cash and in-kind transfer programs on beneficiaries, nonbeneficiaries and prices.

III. Empirical Analysis

The Data

The empirical analysis draws on different types of data. The first dataset consists of a panel of households. The second dataset is a community questionnaire. The third dataset consists of administrative data from the National Commission for Demobilization, Reinsertion and Reintegration.

The Panel Data Set

The first data set constitutes a two-round household survey undertaken in Burundi. The first round is a QUIBB/CWIQ survey that is representative at the provincial level[10] collected in February 2006. The survey was requested by the Ministry of Planning and designed by the World Bank. Data collection was coordinated by the University of Burundi. The CWIQ Survey Technology has been applied in different African countries for the purpose of generating standardized indicators of poverty and welfare. Developed by the World Bank in collaboration with other international agencies, it seeks to reduce the untimeliness of data and poor data quality with a less expensive alternative optimizing the sampling procedure and the structure of the questionnaire (Ajayi 2006; Zoyem et al. 2008). Besides the core modules on household characteristics and consumption behavior, each country can implement additional modules according to its specificities. In the QUIBB for Burundi, questions about displacement and return were included. The sample consists of 6,700 households. The QUIBB sample is characterized by two-stage cluster sampling. In the first stage, 548 sous-collines[11] were sampled and in the second stage, 15 households were sampled in each primary unit.[12] From the original data set, we applied a standard data cleaning process, where missing observations, data entry errors, potential measurement errors, distillation errors and outlier from unusual consumption were deleted.

The second round, undertaken by the authors in April 2010, only retained three provinces of the QUIBB sample: Bubanza, Bujumbura Rural, and Cibitoke, located in the North-West of the country. The choice of these provinces is justified by the concentration of ex-combatants in these three provinces in combination with high level of violence in the region over the last years, as well as by budgetary constraints. We traced 1284 households which were interviewed for the QUIBB survey in the three provinces of interest. A total of 1064 (or 83%) households were found and interviewed in 85 collines.[13] We examine issues related to attrition more closely later in this section.

10 "Questionnaire des Indicateurs de Base du Bien-être" or "Core Welfare Indicator Questionnaire."

11 There are four administrative levels in Burundi: the province, the commune (translated "municipality"), the colline (translated "hill"), and the sous-colline (translated "sub-hills").

12 The QUIBB survey used the same sampling strategy as the Multiple Indicator Cluster Survey (MICS), collected in September 2005 by UNICEF. The MICS survey mostly focuses on health and gender issues, and contains little useful information about economic outcomes. The sampling weights of the MICS survey will be accounted for in the empirical analysis (results do not change without taking into account these sampling weights).

13 There were three collines in which we could not trace households, all located in Bujumbura Rural. In two collines, the villagers reported not to know the households, either because they had migrated or were invented by 2005 interviewers. The remaining colline was not secure enough to conduct the survey.

In order to maximize the reliability of the data, we trained interviewers for a week. Interviews were conducted in Kirundi, the local language. After the training, interviewers were selected on the basis of an exam and simulated interviews. The questionnaire was then tested during a pilot study in a colline that was not included in our sample. We assigned teams of five interviewers, each including a team leader and at least two women. Each interviewer did two interviews per day on average. The questionnaires were then checked for accuracy and entered in a CS-PRO program by data entry agents. We checked the accuracy of entry ex-post, too.

Community Data

During the 2010 survey, enumerators also undertook a community survey in each colline. This survey included data on past violence, public services, and community initiatives. The data on violent events is used as a control variable in our econometric model. We also use population data, based on the 2008 census, in order to scale the number of ex-combatants in each colline.

Official Demobilization Registers

The National Commission for Demobilization, Reinsertion, and Reintegration in Burundi provided us with registers of ex-combatants by colline and faction, along with their sex, age, military rank, colline of origin and of return, as well as the date of their demobilization. The registers contain precise information about each demobilized ex-combatant as well as the exact number of demobilized soldiers in each colline. The large variation in the number of demobilized ex-combatants per colline allow us to identify the spillovers of the DDR program in Burundi.

Identification Strategy

Our identification strategy is based on a lagged dependent variable model with province fixed effects. The lagged dependent variable model should be preferred to the difference-in-differences model when the assumption "that the most important omitted variables are time-invariant doesn't seem plausible" (Angrist and Pischke 2008). The particular histories of civilian and ex-combatants households motivate an estimation strategy that controls for lagged dependent variables directly and dispenses with households fixed effects. Furthermore, when autocorrelation of outcomes is low or when data is imprecisely measured, as is the case for many economic variables such as household incomes and expenditures (McKenzie 2012), controlling for the lagged dependent variable is more powerful than either employing the difference-in-differences estimator or the single difference estimator using only the follow-up data. Intuitively, "when the baseline data have little predictive power for future outcomes, it is inefficient to fully correct for baseline imbalances between treatment and control groups" (McKenzie 2012). Because the coefficients of correlation correlation between our outcomes of interest in 2006 and in 2010 are low,[14] we include the lagged dependent variable as a control variable in the regressions.

According to this in accordance with this approach, we propose to estimate the following equation:

$$\begin{aligned}\Delta \log Y_{i,2010} = {} & \beta_0 + \eta_1 R_i^{CNDD+} + \beta_1 R_i^{FNL} + \eta_2 D_i^{CNDD+} + \beta_2 D_i^{FNL} \\ & + \eta_3 S_i^{CNDD+} + \beta_3 S_i^{FNL} + \delta \log Y_{i,2006} + \mathbf{X}_i' \boldsymbol{\gamma}_i + Z_k + \epsilon_i,\end{aligned} \tag{1}$$

We consider six dependent variables. The first three economic outcomes are consumption aggregates that are constructed following the guidelines of Deaton and Zaidi (2002) (see supplemental appendix B.1 for details, available at https://academic.oup.com/wber). The first indicator measures the total

14 The coefficient of correlation between the logarithm of total consumption per adult equivalent in 2006 and 2010 is 0.12. For other dependent variables—the logarithm of consumption expenditures per AE, the logarithm of consumption from stocks per AE, the logarithm of nonfood expenditures and the logarithm of tropical livestock units—coefficients of correlation between 2006 and 2010 are equal to 0.19, 0.10, 0.18, and 0.28 respectively.

consumption per adult equivalent (AE) of 47 consumption goods. About two thirds of this aggregate is related to the consumption of cereals, fruits and vegetables. The other important food categories are beers and sodas (7.7%), fish, meat, and eggs (7%), condiments (3.3%) and milk (3.3%). The second dependent variable is the part of total consumption that was purchased over the 15 days preceding the survey. This variable is labeled consumption expenditures per adult equivalent. Similarly, the third dependent variable is the share of the total consumption aggregate that was taken from stocks. It is also expressed per adult equivalent. The fourth dependent variable is an indicator of nonfood spending per adult equivalent, which includes spending in terms of clothing, housing, leisure, transport and transfers during the last year. The fifth dependent variable is the tropical livestock units (TLU), which summarizes in one indicator the possession of a wide range of livestock, weighted according to their type and size.[15] In order to improve statistical power, the last variable is a summary index that aggregates information over the five main dependent variables (Kling et al. 2007). The summary index is defined to be the equally weighted average of z-scores of the five components. The z-scores are calculated by subtracting the mean for civilian households and dividing by the standard deviation for civilian households for each component. Descriptive statistics are presented in table 1.

All the household welfare indicators have decreased on average between 2006 and 2010. This trend shows that living conditions have deteriorated in these three provinces of Burundi during that period. Meanwhile, household size increased during the same period. Table 1 suggests that attriting households purchased more, had fewer stocks, were smaller, and were more affected by the conflict. Attrition is discussed in more detail at the end of this section.

The dummies R_i^{FNL} and R_i^{CNDD+} are equal to one for *returnee* households. This category includes all households that declared having one member with ties to the factions, whether or not they benefited from DDR allowances.[16]

The dummies D_i^{FNL} and D_i^{CNDD+} refer to demobilized households, that is, *returnee* households with a member who benefited from the reinsertion grants. We constructed these dummies according to three definitions, provided in table 2 along with the corresponding number of ex-combatants falling into that category. In the first definition, demobilized ex-combatants are those who declared ties to a faction and were registered in the official demobilization registers (D_i^{FNL1} and D_i^{CNDD+1}). In order to minimize misreporting, the second definition adds households from which individuals were matched[17] with the official demobilization registers (D_i^{FNL2} and D_i^{CNDD+2}). The third definition is based on self-reported data, and includes anyone who declared to be demobilized, that is, to have received the cash (D_i^{FNL3} and D_i^{CNDD+3}). The last definition is more likely to contain measurement errors as it is based on self-reports;

15 Conversion factors used are the following: cattle (0.50), sheep and goats (0.10), pigs (0.20), poultry and rabbits (0.01) (Harvest Choice 2011).

16 *Returnees* include *demobilized* households but also "gardien de la paix," "adultes associés," "militant combatant," and people without any status but that declared themselves as members of a rebel faction.

17 We undertook this matching exercise using generalized Levenshtein edit distance, which is the total number of insertions, deletions, and substitutions required to transform one string into another. We matched the names, age, sex, and the code of the colline of return of the ex-combatants listed in the official demobilization registers with the household information available in our panel dataset. We use a maximum number of transformation of 2 for string variables (first name, last name, both), a range of [-10,+10] from the age reported, and the match had to be perfect for the colline's code and gender. We did this exercise twice, once matching each entry in the registers to the panel data; and once the other way around. As names are very similar in Burundi and many people can have the same last name (all twins have the same last names; siblings' last names are different and are chosen according to various contextual factors), we made sure to only consider as matches cases where names contain typos. We found eight matches, which supports the fact that we were very restrictive.

Table 1. Descriptive Statistics

	Sample mean (sd)			T-test *p*-value	
	2010	2006	Attrition	2010/2006	2006/attrition
Economic outcomes					
Consumption per AE	14387	16101	17706	0.00	0.15
	(11349)	(10996)	(14716)		
Cons. expenditure per AE	7971	8828	11314	0.01	0.00
	(7045)	(7765)	(11155)		
Cons. from stock per AE	6566	7246	6114	0.08	0.09
	(9281)	(8181)	(8679)		
Nonfood spending per AE	47191	26478	31776	0.00	0.32
	(148397)	(78022)	(70344)		
Tropical livestock units	0.09	0.16	0.09	0.00	0.03
	(0.16)	(0.19)	(0.19)		
Demographic characteristics					
Adult equivalent (AE)	2.90	2.66	2.48	0.00	0.00
	(0.92)	(0.88)	(0.80)		
HH size	5.89	5.37	4.91	0.00	0.00
	(2.34)	(2.34)	(2.21)		
Sex Head	0.78	0.80	0.81	0.13	0.94
	(0.42)	(0.40)	(0.40)		
Age Head	46	42	41	0.00	0.37
	(14)	(14)	(16)		
Head education					
No school	0.37	0.36	0.34	0.78	0.49
	(0.48)	(0.48)	(0.47)		
Primary school	0.38	0.36	0.41	0.47	0.22
	(0.49)	(0.48)	(0.49)		
Secondary school	0.04	0.03	0.04	0.20	0.43
	(0.20)	(0.18)	(0.20)		
Coranic school	0.21	0.24	0.21	0.08	0.29
	(0.41)	(0.43)	(0.41)		
Head marital status					
Single	0.02	0.03	0.04	0.70	0.20
	(0.15)	(0.16)	(0.20)		
Married	0.76	0.79	0.76	0.07	0.28
	(0.43)	(0.40)	(0.43)		
Divorced	0.02	0.02	0.03	0.25	0.25
	(0.15)	(0.13)	(0.17)		
Widow	0.19	0.16	0.16	0.09	0.97
	(0.39)	(0.37)	(0.37)		
Occupation					
Agriculture	0.79	0.79	0.78	0.97	0.68
	(0.41)	(0.41)	(0.42)		
Small business	0.12	0.08	0.07	0.00	0.51
	(0.32)	(0.27)	(0.25)		
Construction	0.04	0.06	0.09	0.09	0.09
	(0.20)	(0.23)	(0.29)		
Public sector	0.02	0.04	0.02	0.09	0.05
	(0.16)	(0.19)	(0.13)		
Extractive activities	0.03	0.01	0.00	0.00	0.41
	(0.16)	(0.09)	(0.07)		

Table 1 (continued)

	Sample mean (sd)			T-test *p*-value	
	2010	2006	Attrition	2010/2006	2006/attrition
Colline characteristics (village level)					
Violent events (last 4 years)	0.44	1.14	1.39[a]	0.00	0.23
	(0.86)	(1.63)	(1.71)		
Ex-combatant Return, per 1000	3.76	2.99	3.00[a]	0.31	0.98
	(5.43)	(4.48)	(5.15)		

Note: [a] These statistics were computed at the household level.
Source: Authors' analysis based on data described in the text.

Table 2. Construction of Ex-Rebel Household Variables

	CNDD+	FNL	Total	Variable of interest	
				CNDD+	FNL
Member declared having ties with the factions					
but did not receive anything	8	10	18		
and to be GdP/MC/AA	6	4	10		
Demobilized ex-combatant member					
Member declared to be demobilized	14	9	23	D_i^{CNDD+3}	D_i^{FNL3}
And not recorded in official registers	8	5	13		
And recorded in official registers	6	4	10	D_i^{CNDD+1}	D_i^{FNL1}
Not declared but matched with registers	1	7	8		
Total matched with registers (declared or not)	7	11	18	D_i^{CNDD+2}	D_i^{FNL2}
Totals					
Households belonging to a faction (without matches)	28	21*	49	R_i^{CNDD+1}	R_i^{FNL1}
Households belonging to a faction (with matches)	29	28	57	R_i^{CNDD+2}	R_i^{FNL2}

Notes: * For two households, there were two persons reporting to have ties with the FNL. In one household, both declared to be demobilized in 2009 but none was officially recorded. In another household, there was one recorded ex-combatant, while his brother had not received anything. The related dummies take the value one for each case but the return variable is restricted to one for both cases.
Source: Authors' analysis based on data described in the text.

its associated coefficients will presumably be biased towards zero (Hausman 2001). The three different indicators will be compared in the empirical analysis.[18]

We argue that the second definition is the most appropriate for two reasons. First, the matching exercise with official demobilization registers should capture respondents who feared reporting that there is an ex-combatant in their family. Second, cross-checking self-reported demobilization status with official demobilization registers should exclude households who wrongly categorized themselves as having benefited from demobilization allowances. Indeed, many people helped the rebels during the war and they may have wrongly categorized themselves as demobilized ex-combatants. Similarly, some respondents may have claimed to be demobilized in expectation of rewards in exchange for their participation to the survey.

The short-run direct impact of the DDR program is measured by the dummies R_i^{FNL} and D_i^{FNL}. In particular, the coefficient β_1 measures the impact of having a FNL *returnee* in the household who did not benefit from reinsertion allowances. The coefficient β_2 measures the marginal impact of having benefited

18 One should note that the number of registered ex-combatants in our sample is representative of the true ex-combatants' density and that an over-sampling of ex-combatants was not compatible with having a panel.

from the FNL reinsertion allowance.[19] We therefore compare the *returnee* households that have received the grant (demobilized) to the *returnee* households that reported having a member who was part of the FNL but did not receive reinsertion grants (hypothesis 1).

The variables R_i^{CNDD+} and D_i^{CNDD+} capture the long-run evolution of outcomes for CNDD+ households (hypothesis 2). To interpret the coefficients associated with these variables, we need to carefully consider the timing of the demobilization of CNDD+ combatants, which started before the first survey at the end of 2004. Thus, by the time of the QUIBB survey, which is used as our baseline, these ex-combatants had already benefited from reinsertion allowances. Then, they benefited from the in-kind reintegration grants between both surveys. Hence, we are not able to measure the long-term impact of the program on their economic outcomes. What we can assess, however, is the evolution of their economic outcomes between the two surveys.

We explain this argument formally. In an ideal scenario in which we would have had data before the peace agreement, let σ denote the short-run impact of the demobilization program, that is, the one-year impact between the reception of the allowances and the QUIBB survey. Similarly, λ denotes the measure of the long-run impact of the demobilization program, that is, the five-year impact between the reception of the allowances and the 2010 panel survey. Unfortunately, we are not able to measure σ and λ separately. However, we can capture the difference $\eta = \lambda - \sigma$, which can be thought of as the long-run evolution of the economic outcomes of CNDD+ ex-combatants. Therefore, in equation (1), the coefficient η_1 measures the evolution of outcomes for *returnee* CNDD+ households that did not benefit from the DDR, and the coefficient η_2 measures the differential evolution for CNDD+ *demobilized* households.[20] We recommend caution when comparing the impacts of the DDR on FNL and CNDD+ demobilized combatants given the differences in the timing, the type, and the size of the benefits received by these two groups.

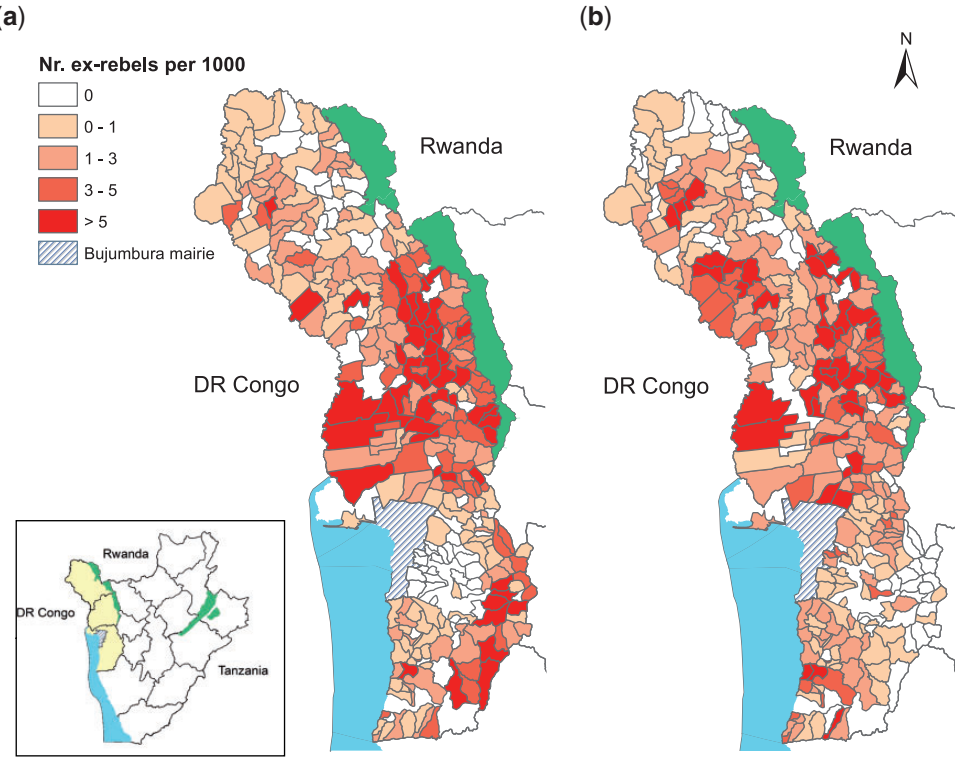
We measure the indirect impact of the demobilization program on civilians by looking at the proportion per 1,000 people of ex-combatants living in each colline[21] (hypothesis 3). The variables of interest are denoted S_i^{CNDD+} for the proportion of ex-combatants demobilized between 2004 and 2006 and S_i^{FNL} for the proportion of ex-combatants demobilized after 2009. The coefficient β_3 gives "a community average effect" of having more or less demobilized FNL combatants when the direct effect on those who received the money is controlled for. In this sense, it captures the indirect effect of the FNL demobilization program on civilians. Similarly, the coefficient η_3 captures the long-run evolution of spillovers generated by the demobilization of CNDD+ combatants. On average, there were 3.8 ex-combatants per 1000 inhabitants that came back following the first wave of the program from 2004 onwards. The FNL demobilization process of 2009 led to an average of 3 ex-combatant returns per 1000 inhabitants. There are substantial differences between collines, which are highlighted in fig. 3 for our provinces of interest. These maps present the distribution of ex-combatants in each colline, scaled by population.

19 Some of the returnees have benefited from the FBU 100,000 (US$ 91) allocated to "Gardiens de la Paix," "Militants Combatants," and "Adultes Associés." We do not have access to registers to verify which households benefited from these reduced grants. Our empirical analysis will therefore slightly underestimate the effect of the demobilization program.

20 In table S.5 in appendix, we show descriptive statistics disaggregated by demobilization status and faction along with seven joint tests of equality of means between the nondemobilized households and the civilians, and the nondemobilized and demobilized within and between faction. The statistics related to the demobilized are computed considering the second definition in table 2.

21 Note that this indicator is computed at the colline level, which is one administrative level above the villages sampled ("sous-colline"). We therefore consider ex-combatant returns in the village of the household, as well as in neighboring villages. While the villages may be connected to each other, the collines are not. Given the size of collines and the difficulties to move in the country, it is very unlikely that the returns in one colline have affected neighboring collines.

Figure 3. Demobilized Ex-Combatants per 1000 Inhabitants in Bujumbura Rural, Bubanza and Cibitoke Provinces



Source: Authors' analysis based on data described in the text.

The vector X_i regroups several control variables that may explain changes in economic outcomes between the two rounds of the panel. It includes the sex, age, education, and marital status of the household head, a dummy that accounts for a change of household head between 2006 and 2010, and the main occupation of households. These variables are not first-differenced to avoid the multicollinearity problem. We also control for past violence, using two variables measuring the number of violent events that occurred in the colline before and after the baseline survey. Summary statistics are presented in table 1. Variables that we expect to have been affected by the demobilization program, such as production, land or other assets, are excluded from the regression (they can be considered as "bad controls" [Angrist and Pischke 2008]). We include province fixed effects Z_k. Standard errors are clustered at the colline level to account for intra-cluster correlation. Sampling weights are accounted for.[22] Outliers are excluded from the regressions (see appendix B.1 for more details).

Before presenting the results, we discuss the problem of attrition by following the methods proposed by Becketti et al. (1988) and Fitzgerald et al. (1998) and applied by Alderman et al. (2001, 2006). The analysis of table 1 suggests that attriting households purchased more, have fewer stocks, are smaller and were more affected by the conflict. Attrition could bias the estimations if it is selective, that is, if the relationship of interest is different for households that were observed and for households that attrited. To test if selective attrition is likely to bias our results, we first estimated the determinants of our dependent variables in 2006 separately for traced and attriting households (Becketti et al. 1988). Fortunately, we

22 The results are similar if we do not use the sampling weights.

do not reject the null hypothesis that the coefficients of these regressions are the same (table S.1 in appendix). Second, we estimated a Probit model to test whether attrition is correlated with dependent variables (Fitzgerald et al. 1998). As shown in appendix (table S.2), the dependent variables are not significantly correlated with the probability of being sampled both with and without controls. We conclude that differential attrition is unlikely to be a concern in our analysis.

IV. Results

Table 3 presents the estimation of the lagged dependent variable model in which the dependent variable is the change in the log of the total consumption per adult equivalent. It shows that there is no significant difference between the consumption per adult equivalent of FNL *returnee* households that did not benefit from reinsertion allowances and the consumption per adult equivalent of civilian households. However, having benefited from the demobilization program had a large impact on the consumption of FNL *demobilized* households (hypothesis 1). The coefficient of the variable D_i^{FNL}, which measures the short-run direct effect of the cash transfer, is positive, significant, and strikingly high when self-reported information about demobilization status is cross-checked against official demobilization registers (columns [1] to [4]). It suggests that the consumption of demobilized FNL households is between 77% and 144% higher[23] than the consumption levels of FNL *returnee* households that did not benefit from reinsertion allowances but had the same consumption level in 2006 as the demobilized households. This effect is also positive when using the definition of demobilization status based on self-reported information (columns [5] and [6]), but it loses its significance. This reduced precision can be explained by the presence of noise due to misreporting (Hausman 2001).

The long-run evolution of total consumption per adult equivalent does not seem to differ between civilian households and CNDD+ *returnee* households that did not benefit from the DDR. In contrast, the long-run evolution of total consumption turns out to be negative for households that benefited from the first wave of the DDR program from 2004 (hypothesis 2). The coefficient of the variable D_i^{CNDD+} is significant and large but negative. These estimates suggest that the consumption of CNDD+ households that benefited from the first wave of the program is between 40% and 50% lower in 2010 than the total consumption of nonbeneficiary CNDD+ households that had the same consumption level in 2006 as beneficiary households. This observation holds for all three definitions. The coefficients associated with the variables D_i^{FNL} and D_i^{CNDD+} are of similar size, but of opposite sign (the F-test p-values reported at the bottom of table 3 are above 0.48 for all specifications).

Table 3 also highlights the presence of spillovers (hypothesis 3). The coefficient associated with the proportion of ex-FNLs in collines S_i^{FNL} is positive and significant. This result suggests that households living in collines with a large number of demobilized ex-combatants benefited from positive spillovers. By contrast, the coefficient of the variable S_i^{CNDD+} is negative and significant, showing that in the long run, households living in areas with numerous CNDD+ ex-combatants consumed on average less in 2010 than households who had a similar standard of living in 2006 and who lived in areas with fewer CNDD+ ex-combatants. The coefficients associated with the variables S_i^{FNL} and S_i^{CNDD+} are of similar size, but of opposite sign across definitions (F-test p-values are all above 0.78).

Finally, the coefficient associated with the lagged dependent variable is negative and significant. This further illustrates the importance of including the lagged dependent variables in the regressions to account for low auto-correlation, noise, or catch-up effects in the outcomes of interest.[24]

23 As the dependent variables of the regressions are expressed in log , the interpretation of the coefficients in terms of percentage should be corrected according to the formula: $e^{\text{coef.}} - 1$.

24 As shown in table S.4 in appendix, the coefficients associated with the long-run indirect effect lose significance when we do not control for the lagged dependent variables.

Table 3. Lagged Dependent Variable Model—Aggregate Consumption

Dependent variable: Δ Log total consumption per AE in 2010

	D_i cross-check		D_i incl. matches		All D_i	
	(1)	(2)	(3)	(4)	(5)	(6)
Ex-combatant return in HH						
CNDD+ factions (R_i^{CNDD+})	0.128	0.066	0.131	0.068	0.280	0.193
	(0.144)	(0.144)	(0.144)	(0.143)	(0.173)	(0.165)
FNL factions (R_i^{FNL})	0.016	−0.016	0.019	−0.016	0.056	0.043
	(0.215)	(0.200)	(0.215)	(0.201)	(0.256)	(0.241)
Demobilized in HH						
CNDD+ factions (D_i^{CNDD+})	−0.610***	−0.696***	−0.510**	−0.588***	−0.546**	−0.537**
	(0.226)	(0.203)	(0.223)	(0.194)	(0.226)	(0.220)
FNL factions (D_i^{FNL})	0.890**	0.862**	0.586**	0.572**	0.322	0.250
	(0.367)	(0.399)	(0.240)	(0.250)	(0.367)	(0.366)
# of demobilized in the hill (per 1000 inhab.)						
CNDD+ factions (S_i^{CNDD+})	−0.013*	−0.015**	−0.013*	−0.015**	−0.012*	−0.014**
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
FNL factions (S_i^{FNL})	0.012**	0.013***	0.012**	0.013***	0.012**	0.013***
	(0.005)	(0.003)	(0.005)	(0.004)	(0.005)	(0.003)
Lagged dependent variable	−0.819***	−0.849***	−0.813***	−0.842***	−0.821***	−0.850***
	(0.037)	(0.047)	(0.038)	(0.047)	(0.037)	(0.046)
Constant	7.574***	7.787***	7.511***	7.705***	7.585***	7.792***
	(0.360)	(0.507)	(0.366)	(0.500)	(0.352)	(0.502)
Controls	No	Yes	No	Yes	No	Yes
Province FE	No	Yes	No	Yes	No	Yes
Observations	969	958	969	958	969	958
R^2	0.368	0.413	0.369	0.413	0.366	0.411
F-test ($\eta_1 = -\beta_1$) p-value	.6	.84	.57	.84	.27	.42
F-test ($\eta_2 = -\beta_2$) p-value	.48	.66	.81	.96	.6	.48
F-test ($\eta_3 = -\beta_3$) p-value	.93	.78	.90	.81	.96	.84

Notes: In this table, we compare the three definitions of demobilization, with and without controls and province fixed effects. The table reports OLS estimates. The unit of observation is a household. Total consumption is the sum of consumption expenditures and consumption taken from households' stocks per adult equivalent. In column (1) and (2), D_i only included ex-combatants cross-checked with official registers. In column (3) and (4), D_i adds matches. In column (5) and (6), all self-reported ex-combatants are included. Odd-numbered columns do not include control variables or fixed effects; the remaining columns include both. We report the p-values of F-tests assessing whether the coefficients associated with CNDD+ and FNL variables are of similar size, but of opposite sign. Controls include past violence, whether the household head changed, his sex, age, matrimonial status, education, and occupation. Outliers are excluded (as explained in appendix B.1). Sampling weights are accounted for. Clustered-robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Authors' analysis based on data described in the text.

Table 4 displays the estimates of the lagged dependent variable model for the five other economic outcomes. This table relies on the second definition of demobilized ex-combatants, namely the one using self-reported information cross-checked with the official demobilization registers and supplemented with matches.[25]

Consumption expenditures, consumption from stocks, nonfood spending, livestock owning, and the summary index are significantly higher in households that benefited from the FNL demobilization program (hypothesis 1). Consumption expenditures of demobilized FNL households are on average 67% higher than consumption expenditures of FNL *returnee* households that did not benefit from reinsertion

25 D_i^{CNDD+2} and D_i^{FNL2} in table 2. The use of other definitions leads to similar results. When self-reported information is used, the results are weakened by the presence of noise.

Table 4. Lagged Dependent Variable Model—Disaggregated Consumption, Nonfood Spending, Tropical Livestock Units (TLU), and a Summary Index *Z* That Aggregates Information over the Five Main Dependent Variables (Kling et al. 2007).

	Δ Log cons. expenditures per AE		Δ Log cons. from stocks per AE		Δ Log non-food spending per AE		Δ Log Tropical Livestock		Δ *Z* (Kling et al. 2007)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Ex-combatant return in HH										
CNDD+ factions (R_i^{CNDD+})	0.306	0.069	−0.056	−0.007	0.431	0.181	0.016	0.014	0.131	0.068
	(0.204)	(0.164)	(0.276)	(0.270)	(0.382)	(0.404)	(0.042)	(0.047)	(0.190)	(0.198)
FNL factions (R_i^{FNL})	0.141	0.158	−0.335	−0.394	−0.243	−0.261	−0.062***	−0.068***	−0.086	−0.110
	(0.215)	(0.217)	(0.461)	(0.417)	(0.311)	(0.317)	(0.015)	(0.019)	(0.203)	(0.186)
Demobilized in HH										
CNDD+ factions (D_i^{CNDD+})	−0.869***	−0.775***	0.015	−0.059	−1.279**	−1.158*	0.016	−0.013	−0.341	−0.321
	(0.312)	(0.256)	(0.480)	(0.503)	(0.583)	(0.684)	(0.047)	(0.041)	(0.225)	(0.232)
FNL factions (D_i^{FNL})	0.611***	0.510**	0.766	0.894*	1.657***	1.593***	0.077*	0.098**	0.686***	0.719***
	(0.227)	(0.246)	(0.522)	(0.486)	(0.388)	(0.424)	(0.046)	(0.046)	(0.221)	(0.218)
# of demobilized in the hill (per 1000 inhab.)										
CNDD+ factions (S_i^{CNDD+})	−0.022***	−0.020**	−0.002	−0.016	−0.049***	−0.042***	−0.003**	−0.001*	−0.020***	−0.020***
	(0.008)	(0.008)	(0.017)	(0.013)	(0.010)	(0.012)	(0.001)	(0.001)	(0.006)	(0.006)
FNL factions (S_i^{FNL})	0.020***	0.020***	0.015	0.013	0.031***	0.031***	0.002***	0.002***	0.015**	0.016***
	(0.004)	(0.004)	(0.015)	(0.010)	(0.007)	(0.006)	(0.001)	(0.001)	(0.007)	(0.006)
Lag										
Lagged dependent variable	−0.789***	−0.833***	−0.858***	−0.889***	−0.794***	−0.837***	−0.858***	−0.878***	−0.759***	−0.773***
	(0.038)	(0.040)	(0.042)	(0.041)	(0.040)	(0.040)	(0.056)	(0.050)	(0.040)	(0.042)
Constant	6.785***	7.366***	7.119***	7.336***	7.911***	8.393***	0.069***	0.003	0.036	0.001
	(0.333)	(0.352)	(0.379)	(0.505)	(0.365)	(0.414)	(0.007)	(0.028)	(0.083)	(0.169)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Province FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Observations	985	974	796	786	857	850	968	952	690	683
R^2	0.392	0.446	0.460	0.496	0.417	0.458	0.483	0.516	0.387	0.432
F-test ($\eta_1 = -\beta_1$) *p*-value	.15	.42	.45	.42	.72	.87	.3	.3	.87	.87
F-test ($\eta_2 = -\beta_2$) *p*-value	.45	.42	.3	.24	.54	.51	.15	.18	.24	.21
F-test ($\eta_3 = -\beta_3$) *p*-value	.78	.96	.48	.84	.18	.42	.54	.27	.57	.51

Notes: The table reports OLS estimates. The unit of observation is a household. This table relies on the second definition of demobilized ex-combatants (D_i^{CNDD+2} and D_i^{FNL2}): cross-checked self-reports and matches. Odd-numbered columns do not include control variables or fixed effects; the remaining columns include both. We report the *p*-values of F-tests assessing whether the coefficients associated with CNDD+ and FNL variables are of similar size but of opposite sign. Controls include past violence, whether the household head changed, and the head's sex, age, matrimonial status, education, and occupation. Outliers are excluded (as explained in appendix B.1). Sampling weights are accounted for. Clustered-robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Authors' analysis based on data described in the text.

allowances (column [2]). Similarly, consumption from stocks is 144% higher (column [4]), nonfood purchases are 392% higher (column [6]), and the total livestock units are 10% higher (column [8]).

The coefficient associated with the long-run evolution of demobilization variable for CNDD+ demobilized combatants exhibits interesting patterns (hypothesis 2). When the dependent variables involve spending money, as for consumption or nonfood purchases, the coefficients of the variable D_i^{CNDD+2} are negative and significant (columns [2] and [6]). For these economic outcomes, the coefficient associated with the CNDD+ demobilization have the opposite signs of those associated with FNL demobilization but are of similar size (F-test p-values reported at the bottom of Table 4 are not significant). In contrast, for variables associated with asset holdings such as consumption from stocks (which includes consumption from one's own agricultural production) and livestock, coefficients are close to zero and not significant. The coefficient associated with the variable D_i^{CNDD+2} is negative but not significant at conventional thresholds when the dependent variable is the summary index (p-values = .13 and .17 in Columns [9] and [10], respectively).

The results we find for the spillovers of the DDR program are consistent with this picture (hypothesis 3). The effects of spillovers of the FNL demobilization on nonbeneficiaries are positive—around 2%—and significant for consumption expenditures, nonfood purchases, and the summary index. For these three dependent variables, the impact of spillovers from the CNDD+ demobilization are of similar size but of opposite sign. The spillovers to livestock follow a similar pattern, positive and significant for FNL demobilization, and negative and significant for CNDD+ demobilization. However, estimated coefficients are close to zero. There is no significant effect of spillovers on consumption from stocks.

V. Discussion

In this section, we discuss and interpret the empirical results in light of economic theory and we put them in perspective with regard to existing literature. We first focus on the direct impact on ex-combatants and then turn to the analysis of spillovers.

Direct Effect of the DDR on Ex-Combatants' Households

Our first finding is the large positive impact of demobilization on the economic situation of households hosting a demobilized FNL (hypothesis 1). In the short run, the FNL demobilized households consumed more, spent more in nonfood items and had a larger stock of livestock. This finding is consistent with the theoretical framework, since at the time of the interviews, the demobilized FNLs were in the process of receiving the equivalent of 18-months' salary in four cash installments. In a context of poorly functioning financial savings institutions,[26] none of the FNL households reported saving part of their allowance in financial assets. Instead, a large proportion of ex-FNLs reported buying consumption goods (46%), buying clothes (15%), and paying medical bills (8%). This observation is consistent with previous qualitative evidence from Burundi as well. Uvin (2007) showed that some ex-combatants, depending on the region they returned to, faced immediate needs such as building houses, paying medical bills, and buying food for their family. These arguments elucidate the substantial increase in consumption of ex-FNLs as they "fell into a lot of money" (Uvin 2007). However, FNLs also used demobilization money to invest in land (69%) and construction (8%), to invest in an income generating activity (31%), and to pay off debts (15%). These investments can be regarded as rational forms of nonfinancial saving in the absence of financial savings institutions.

Our second finding is the negative evolution of spending indicators for demobilized CNDD+ between 2006 and 2010 (hypothesis 2). We have reason to believe that CNDD+ households also experienced a

26 Our community data reveals the absence of formal financial institutions in 84% of the collines surveyed. Informal financial institutions are also scarce, with 87.5% of the collines lacking such associations.

consumption boom after "falling into money" in 2004. For the same reasons than their FNL counterparts, they are likely to have spent most of their allowances in consumption goods or in investments that were not productive enough to sustain their consumption in the long run. This tendency would explain why we observe a decrease in their consumption between 2006 and 2010. An alternative explanation for our results is that we are actually measuring the continuation of a decreasing trend. In this scenario, this trend would not be due to the grants, but would rather be explained by unobservable characteristics correlated with having received the grants.

There is evidence supporting the first interpretation and discrediting the second. First, Verwimp and Bundervoet (2009) have shown that consumption by CNDD+ ex-combatants' households increased by 34% between 1998 and 2007. Their second wave of data was collected after the start of the program and after ex-CNDD+ received their cash transfers. These data suggest that CNDD+ ex-rebels were not on a decreasing trend before 2007, which dismisses the second interpretation. Second, qualitative evidence suggests that ex-combatants spent a large share of their allowance in nonproductive ways: food, drinks, or clothes. They are therefore likely to have experienced a consumption boom similar to ex-FNLs as of 2005. From our survey, we find that 50% of CNDD+ ex-combatants spent part of their allowance in consumption goods and 33.3% in clothing; only 5.6% reported having saved part of the allowances. Ndayiziga et al. (2008) underlines the tendency of ex-combatants to "overspend the demobilization indemnities in an unorganized way, quickly putting them back into hardship," while Willems et al. (2010) conclude that "although the total sum of the benefits given to ex-combatants was rather large, many of them found it difficult to effectively use it for their reintegration because they did not have the capacity to handle money." Third, our results are also consistent with the fact that the indicators related to the stocks of productive assets exhibit a constant trend, while consumption indicators exhibit a downward trend. According to our data, a substantial fraction of ex-combatants invested in a plot of land (39%), in a small shop (28%), in working material (22%), or in livestock (11%). These investments are more durable and hence still observable in 2010 (table 4, columns [4] and [8]). On the contrary, the spike in food and nonfood spending was short-lived and vanished when ex-combatants ran out of demobilization money (table 4, columns [2] and [6]). Fourth, our interpretation is consistent with the evolution of total consumption percentile ranks of demobilized households. Between 2006 and 2010, the average consumption of FNL households increased from the 42nd percentile to the 76th percentile. The evolution is almost the opposite for CNDD+ households: average consumption decreased from the 69th to the 48th.

Finally, we relied on two more formal approaches. We applied the method proposed by Altonji et al. (2005) and Bellows and Miguel (2009) to test whether our results are likely to be driven by unobservables. Results presented in appendix (table S.6) show that our findings cannot be plausibly explained entirely away by unobservables. To see whether the direct effect could be driven by factors or policies at the community level, we added colline fixed effects to our benchmark OLS regressions. Similar results are obtained, discrediting this explanation (table S.7).

These arguments support the interpretation that ex-combatants were not able to sustain the consumption levels reached during the boom. However, we cannot conclude whether the total effect of the program in the long run was positive or not. In particular, we are unable to test whether their consumption level before the onset of the DDR program in 2004 was higher or lower than their consumption level in 2010. In 2010, the average consumption of CNDD+ households was close to the median level in the population. The long-run effect of the reintegration phase of the program, that is, the in-kind transfers, seems to have been marginal for the CNDD+ ex-combatants, at least when compared to the large effect of reinsertion allowances on consumption. This small effect contrasts with Gilligan et al. (2013), who find that reintegration grants have had a positive impact on self-reported income and decreased poverty incidence. However, they reckon that they are "evaluating program effects within a very short time

frame" (Gilligan et al. 2013). The difference in indicators[27] and in the time frame, as well as the lower statistical power associated with our demobilization variables, could explain why we reach different conclusions.

Spillovers on Civilians

In section 4, we showed that one additional FNL demobilized combatant per 1000 inhabitants generated on average a 2% increase in consumption purchases and nonfood spending in 2010 (hypothesis 3). We also found that one additional CNDD+ ex-combatant per 1000 inhabitants generated a negative evolution of consumption indicators between 2006 and 2010. Our preferred interpretation for these results is that the cash transfer distribution generated a local economic boom in villages hosting numerous ex-combatants, both for the CNDD+ and the FNL demobilization waves. However, the positive economic environment generated by the demobilization of CNDD+ combatants has become sluggish over time, as the surplus of money brought by demobilized combatants gradually moved outside the local economy because households also purchased goods produced outside their villages.

Our interpretation is consistent with prices' variations in the collines (hypothesis 4). Economic theory predicts that a sudden increase in cash will push demand upwards, leading to a temporary increase in the prices of normal goods under general conditions. Table S.8 in appendix documents how median prices in the local markets have evolved given the intensity of returns in the villages.[28] In accordance with theory, we find that prices in 2010 were significantly higher in collines with a higher density of demobilized FNL ex-combatants. A one-standard-deviation increase in the density of demobilized FNL led to a 1.8% increase in median prices (p-value = .048). In contrast, the evolution of prices was negatively affected by the presence of CNDD+ ex-combatants. A one-standard-deviation increase in the density of CNDD+ demobilized combatants generated a 2.1% reduction in prices (p-value = .021). While variations in prices are consistent with our story, we cannot formally test whether these variations are due to demand or supply changes since only final (equilibrium) prices are observed.

Our story is also consistent with the estimated differential impact according to professions (hypothesis 3). In order to study the differential impact of the DDR according to households' main occupation, table 5 adds interaction terms between the main occupation of households and the proportion of ex-combatants living in collines to equation (1). It is important to note that our data does not allow us to clearly distinguish supply-side from demand-side effects. In particular, it is difficult to measure how the supply of goods and services changed in communities with the return of ex-combatants, given the fact that we only have information about 15 households at most in each sous-colline.

Ex-combatants spent a large part of their money on food and traditional drinks, which are produced by farmers. Given the increase in demand and prices, farmers selling part of their production in the local market should have benefited from increased revenue.[29] In line with theoretical predictions, we find that farmers consumed on average more in villages where FNL returns were higher and less in collines where more CNDD+ returned. We find that construction workers also indirectly benefited from the program. The construction sector indeed gained from ex-combatants' investments in rebuilding houses. When returning home, around 10% of ex-rebels invested part of their allowance in the construction of a dwelling. Some civilian households probably also invested in their housing following the end of the conflict. Again, construction workers living in collines with numerous CNDD+ demobilized ex-combatants seemed to experience a slight slowdown comparing 2010 to 2006 (although this effect is not significant

27 We only managed to collect precise income data for a minority of households, so we are not able to assess the impact of the program on income.

28 We estimated a lagged dependent variable model with median prices at the colline level with product fixed effects.

29 For the farming sector, the effect of the return of ex-combatants on the supply of goods is expected to be marginal, as a majority of households were already involved in farming before demobilization.

Table 5. Channels—Spillovers and Sector of Activity

Dependent variable: Δ Log total consumption per AE in 2010	(1)	(2)	(3)	(4)	(5)	(6)
Ex-combatant return in HH						
CNDD+ factions (R_i^{CNDD+})	0.233	0.137	0.213	0.114	0.143	0.047
	(0.160)	(0.161)	(0.157)	(0.158)	(0.149)	(0.146)
FNL factions (R_i^{FNL})	−0.045	−0.075	−0.032	−0.072	−0.001	−0.040
	(0.257)	(0.237)	(0.251)	(0.234)	(0.219)	(0.204)
Demobilized in HH						
CNDD+ factions (D_i^{CNDD+})	−0.611*	−0.569	−0.524*	−0.512*	−0.687***	−0.678***
	(0.349)	(0.345)	(0.304)	(0.295)	(0.238)	(0.230)
FNL factions (D_i^{FNL})	0.488*	0.494**	0.496**	0.511**	0.587**	0.597**
	(0.252)	(0.243)	(0.245)	(0.242)	(0.243)	(0.242)
# of demobilized in the hill (per 1000 inhab.)						
CNDD+ factions (S_i^{CNDD+})	−0.020***	−0.023***	−0.017**	−0.019***	−0.017**	−0.020***
	(0.006)	(0.006)	(0.007)	(0.007)	(0.006)	(0.006)
FNL factions (S_i^{FNL})	0.018***	0.015***	0.018***	0.016***	0.018***	0.016***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Channel						
S_i^{CNDD+} × Small business	0.066**	0.064**	0.048*	0.051**	0.014	0.016
	(0.029)	(0.028)	(0.025)	(0.023)	(0.026)	(0.025)
S_i^{CNDD+} × Construction	−0.104	−0.096*	−0.108	−0.094	−0.092	−0.083
	(0.068)	(0.058)	(0.067)	(0.058)	(0.083)	(0.080)
S_i^{CNDD+} × Public sector	0.066***	0.061***	0.078***	0.069***	0.071***	0.060***
	(0.016)	(0.014)	(0.016)	(0.016)	(0.015)	(0.016)
S_i^{CNDD+} × Extractive activities	0.046	0.047	0.042	0.042	0.014	0.016
	(0.042)	(0.041)	(0.042)	(0.041)	(0.026)	(0.025)
S_i^{FNL} × Small business	−0.022**	−0.015	−0.018**	−0.013	−0.009	−0.004
	(0.011)	(0.011)	(0.008)	(0.009)	(0.008)	(0.008)
S_i^{FNL} × Construction	0.283***	0.281***	0.292***	0.281***	0.134	0.125
	(0.079)	(0.073)	(0.082)	(0.078)	(0.110)	(0.109)
S_i^{FNL} × Public sector	−0.041***	−0.032***	−0.042***	−0.034***	−0.026***	−0.015***
	(0.004)	(0.005)	(0.005)	(0.005)	(0.004)	(0.005)
S_i^{FNL} × Extractive activities	−0.021*	−0.008	−0.021*	−0.009	−0.021***	−0.013*
	(0.011)	(0.011)	(0.011)	(0.011)	(0.008)	(0.008)
Lag						
Log consumption per AE in 2006	−0.796***	−0.822***	−0.816***	−0.840***	−0.821***	−0.847***
	(0.054)	(0.055)	(0.052)	(0.053)	(0.044)	(0.045)
Constant	7.339***	7.963***	7.506***	8.034***	7.552***	8.119***
	(0.519)	(0.578)	(0.504)	(0.566)	(0.430)	(0.516)
Controls	No	Yes	No	Yes	No	Yes
Province FE	No	Yes	No	Yes	No	Yes
Observations	858	853	909	904	963	958
R^2	0.097	0.133	0.088	0.121	0.070	0.102

Notes: This table reports OLS estimates for the second definition of demobilization (D_i^{CNDD+2} and D_i^{FNL2}). We grouped households into five employment categories: farming, small business, construction, public sector, and extractive activity. We define as the main occupation the one which generated the highest income the year before the survey. Constructing this variable was problematic for households that did not report the revenue associated with their activities. These households are therefore excluded in columns (1) and (2). In columns (3) and (4), households that did not report their revenue but only engaged in one type of activity are included. In columns (5) and (6), we also add the households that did not report their revenue and engaged in multiple activities by assuming that their main income generating activity was the nonagricultural one. In all columns, the dummy for farming activity—the main activity for 79% of households—is omitted. There are no controls in columns (1), (3), and (5). In the remaining columns, controls include whether the household head has changed, as well as the head's sex, age, matrimonial status, and education. Outliers are excluded (as explained in appendix). Sampling weights are accounted for. Clustered-robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source: Authors' analysis based on data described in the text.

in some specifications). Despite the consumption boom induced by demobilization cash transfers, we find that the DDR program had no significant indirect effect on shopkeepers. This surprising result may be due to an increasingly competitive environment following the return of demobilized combatants. Table S.9 in the appendix indeed shows that shop ownership increased for FNL households between 2006 and 2010, from 12% to 45% (p-value = .12). In contrast, we find that shop ownership of CNDD+ households declined between 2006 and 2010, from 56% to 30% (p-value = .29). These changes go in the direction consistent with our interpretation but are not significant at conventional levels. Increased competition could also explain why we observe a reduction in the price of bottled beers in collines characterized by a high density of demobilized FNL returns and a positive evolution in the price of bottled beers in collines with a high density of CNDD+ returns (table S.8 in the appendix). Finally, the impact on public sector employees is striking and should be emphasized. The relative situation of public employees has sharply improved in collines with a high proportion of CNDD+ returns. On the contrary, public employees living in collines with numerous FNL ex-combatants are relatively worse off. This is not surprising if we remember that the actual president of Burundi, Pierre Nkurunziza, is the former leader of the CNDD-FDD rebel group; public employees working in collines with numerous CNDD+ ex-combatants may have been favored following his election in 2005.

An alternative explanation for our findings could be that collines hosting FNLs and CNDD+ ex-combatants were following different trends: a positive trend for FNL collines and a negative trend for CNDD+ collines. This explanation is unlikely for a few reasons. First, there is no evidence of strategic relocation of ex-combatants. The registers include data on village of origin and return. Comparing these for each of the ex-combatants, we find that around 88% have come back to their colline of origin, and 92% to their home municipality. Formally, our analysis of the relative influence of unobservables compared to observables suggests that our results are not driven by unobservable characteristics, such as the drivers of relocation (table S.6 in the appendix).

Second, when the proportion of CNDD+ demobilized soldiers is included in the analysis of Verwimp and Bundervoet (2009), the coefficient associated with these spillovers is positive and equal to 0.01 (p-value = .04) without province fixed effects and 0.006 (p-value = .13) with fixed effects (available upon request). Its size is hard to compare with that of our estimates as their sample is different. In 2007, they did not go to two of our three provinces (Bubanza and Bujumbura Mairie) because the situation was deemed too volatile. Moreover, we do not use the exact same specification and indicators. Nevertheless, this suggests that collines hosting numerous CNDD+ ex-combatants were following a positive trend between 1998 and 2007.

VI. Concluding Remarks

Following the end of the Cold War, multidimensional peacekeeping operations have been implemented to facilitate the transition from war to peace in Africa. Disarmament, demobilization, and reintegration programs have been an essential component of this process. These programs aim at "disarming combatants, removing them from military structures, and socially and economically integrating them into society" (World Bank 2009).

Burundi is recovering from a civil war that lasted more than a decade. The armed conflict ended in 2009 with the voluntary demobilization of the last Hutu rebel group, the Palipehutu-FNL. In exchange for laying down their arms, ex-combatants received reinsertion allowances equivalent to an 18-month salary in the army. Four years before, another Hutu rebel group, the CNDD-FDD, benefited from the same allocations, and additionally received in-kind payments.

In this paper, we assessed the impact of this disarmament, demobilization, and reintegration program by analyzing a panel dataset collected in 2006 and 2010 in three rural provinces heavily affected by the

conflict. Our objectives were to assess the short- and long-run impacts of the demobilization program on ex-combatants economic outcomes but also to capture the spillovers to civilian households.

Our empirical analysis highlighted the positive and significant impact of the demobilization program on ex-combatant households in the short run. The large amounts of money that were introduced in the local economy through the demobilization program generated an economic boom in the short run. As a result, households that did not participate actively in the conflict also indirectly gained from the demobilization program. In the long run, however, the positive impact of the DDR on beneficiaries and the positive externalities on nonbeneficiaries seem to have vanished as the money received by demobilized ex-combatants ran low.

The conclusions of our empirical analysis should be considered by policymakers while implementing DDR processes in other regions. In the short run, the positive direct effect of the DDR program shows that the program indeed provides a safety net to ex-combatants. The positive spillovers further show that the program does not only benefit beneficiaries but also communities where ex-combatants returned, increasing the range of impact. This second effect is likely to ease the return of ex-combatants to their villages. Our study therefore supports the reinsertion program implemented in post-conflict countries as a short-run strategy to reduce the risk of relapse into conflict by alleviating poverty of both recipient and nonrecipient households. However, our results suggest that the reinsertion phase is not sufficient for creating a virtuous circle towards economic development, as the positive impact of cash transfers is short-lived. The return to a precarious economic situation may encourage ex-rebels to take up arms again and re-engage in violence (Colombo et al. 2014).

We emphasize three limitations of our study, which imply the need for further research. First, the absence of long-run positive effects associated with the subtler reintegration benefits could be due to the low predictive power given the small number of officially demobilized soldiers. Our analysis of spillover effects does not suffer from this caveat, as the combination of household panel data and the variation in the number of demobilized combatants across collines allowed us to quantify these effects both in the short and the longer run. Second, our analysis focused on an area in north-western Burundi in which ex-combatant density was above the country average, and it should not be seen as representative of the country. The theoretical framework highlighted that the impact of cash and in-kind transfers may vary according to the context and the way the program is implemented. More research is needed to assess the external validity of our findings. Finally, our interpretation of the evolution of the impact of the DDR program on CNDD+ demobilized combatants relies on the indirect observations of a short-run positive impact identified in the literature (Ndayiziga et al. 2008; Verwimp and Bundervoet 2009; Willems et al. 2010). More research is needed to test the relevance of our interpretation and, more generally, to assess how impacts of demobilization programs evolve over time.

In spite of its limitations, we believe that our study sheds light on some important effects of the DDR program on both ex-combatants and civilians. We hope that our study, together with other analyses of the effects of the DDR program, can pave the way towards innovative designs for evaluating the impact of such important programs.

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