Reintegrating ex-rebels into civilian life: Evidence from a quasi-experiment in Burundi*

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Abstract

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1 Introduction

Disarmament, demobilization and reintegration (DDR) programs are central to international interventions to assist countries coming out of civil war. These programs, often provided for in peace agreements[1], are typically carried out alongside economic reconstruction, infrastructure rehabilitation, institutional reform, refugee repatriation and democratization. All of these interventions aim to address the root causes of conflict and reduce the likelihood of another war (Boutros-Ghali

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[1]Not all DDR take place in the framework of peace agreements. In several cases, like Rwanda and Ethiopia, DDR was undertaken after the military defeat of one side. In Colombia, DDR was launched while the civil war was ongoing. Some DDR programs take place as part of army reform processes, such as those that followed World War II and the Cold War (Kingma and Pauwels 2000). This paper focuses on internationally-sponsored DDR in post-conflict settings.

1992; Cousens et al 2001; Paris 2004; Doyle and Sambanis 2006). DDR programs are usually implemented under the direction of international actors, especially the United Nations (UN) and the World Bank. DDR programs encompass a set of activities designed to lay the foundation of stable and self-sustaining peace. These activities entail the discharge of a large number of combatants who must be disarmed and reintegrated into civilian life (Walter 2001; Fortna 2008).

The disarmament and demobilization of former combatants—the first two components of DDR—take place before the reintegration phase in order to create security conditions and trust necessary for implementing peace agreements (Spear 2002). The security resulting from effective disarmament and demobilization is also a prerequisite for the roll-out of recovery and development activities (Kumar 1997; Rubin et al. 2003; Feil 2004). A goal following disarmament and demobilization is for demobilized combatants to find a livelihood and submit to laws and norms that govern civilian society. Based on a presumption that the former combatants cannot or will not achieve this *automatically*, reintegration programs supplement disarmament and demobilization processes. Reintegration programs typically encompass a wide range of activities that include (i) short-term measures such as the provision of cash assistance or in-kind material benefits to address the immediate needs of former combatants and their dependents upon leaving armed groups; and (ii) longer-term measures such as vocational training, skill development and counseling, which aim to reintegrate former combatants into the social and economic structures of society (Colletta et al. 1996; Bryden et al.2005; Muggah 2009). The literature that informs the design of current reintegration programs tends to presume that ex-rebels form a class that, on average, exhibits serious human and social capital deficiencies. If these deficiencies are not attended to, the presumption is that they would significantly limit ex-combatants' ability to establish themselves economically, making them susceptible to recruitment into armed groups or criminality. Sometimes program designers may appreciate that ex-combatants are no more vulnerable than their civilian counterparts, but that they need extra incentives to be steered from the temptations of banditry and extortion—activities that their combatant experience has trained them to do. Finally, in some contexts (e.g. those in which where rebels have won control of the government), reintegration assistance is justified as necessary for honoring the sacrifices of combatants who were not integrated into a new armed forces.

Reintegration is a critical component of DDR both because it links "the more immediate requirements of disarmament and demobilization to the long-term imperatives of social and economic welfare" (Bryden et al.2005) and because it is the set of activities that facilitate effective conversion from combatant to civilian life.[2] Effective conversion to civilian life is achieved not only when former combatants are able to establish peaceful and sustainable livelihoods ("economic reintegration"), but also when they longer see force and violence as a legitimate means to pursue their objectives ("political reintegration"; World Bank, 2002; United Nations, 2000; Pouligny 2004).[3] The ability to establish good working relations with one's community or family ("social reintegration") is often taken to be an important moderator of an ex-combatant's economic and political reintegration prospects. The typical causal model is social reintegration $\rightarrow$ economic reintegration $\rightarrow$ political reintegration. Reintegration programs typically emphasize economic reintegration. They also include reconciliation interventions that attempt to facilitate social reintegration, but these are usually secondary. Political reintegration is not usually directly targeted, but political reintegration is presumed to follow from the boost that a program provides to social and economic reintegration. Spear (2006) argues that among post-conflict interventions, reintegration is the most directly "linked to establishing a *lasting* peace" (emphasis in the original)—a conclusion echoed by Cavallo's (2008) argument that effective reintegration of former combatants is a "*sine qua non*" for the consolidation of peace.

Despite the importance of reintegration programs, there have been few attempts to evaluate their effectiveness (Humphreys and Weinstein 2007; Muggah 2009). Some macro-level studies exist, particularly studies evaluating the reintegration programming of major international insti-

[2] In practice reintegration has been the most neglected component of DDR—at least in terms of funding. Some analysts argue that the fact that reintegration is not strictly a military activity can explain this relative neglect. As Specker (2008) notes "The limited time horizon and the preponderance of security concerns of the international community, however, frequently resulted in resources being used primarily for the DD-phases, leaving inadequate funding for the R-phase."

[3] Rackley (2007) argues that donors have the mistaken idea that "as soon as you get guns out of their hands, they are suddenly innocuous human beings again, but that is not the case at all" (cited in Hanson 2007). Indeed, while disarmament and demobilization processes are key mechanisms to "it reduce the capacity of the former combatants to renew armed conflict" (Russett and O'Neil 2001), they are by themselves not sufficient to *transform* old soldiers into civilians. To complete the conversion process, as Rackley discusses, former combatants must be provided with an economic alternative to living by the gun.

tutions like United Nations agencies and the World Bank, and they typically focus on program performance and outputs.[4] There are also several descriptive studies that focus on the practical challenges that fraught the implementation of reintegration programs,[5] and a growing critical literature that questions the assumptions that underpin these programs.[6] However, focusing as they do on macro factors like reintegration program design and conflict settlement terms, these studies tell us very little about the effectiveness of reintegration programs in helping individual ex-combatants reintegrate into civilian life.

A handful of recent studies has relied on ex-post surveys and other micro-level data to try to identify effects of reintegration programs. Humphreys and Weinstein (2007) investigate the effects of a UN-sponsored DDR program in Sierra Leone. They found reintegration programming to have no measurable effect on improving reintegration prospects, while contextual factors such as wealth and education (ironically) as well as wartime traumas seemed to hinder economic reintegration. Pugel (2006) and Muggah and Bennett (2009) conducted a similar studies in Liberia and Ethiopia, respectively. They both find some evidence (albeit differentiated) of program impacts [SUCH AS???? TO BE INSERTED...].

These quantitative studies have had only a modest effect on thinking about reintegration due mostly to the weak possibilities that they offer for identifying causal effects. The weakness is by no fault of the authors, but rather from the nature of the programs. The material benefits offered by such programs makes the incentive to participate very strong, meaning that those who do not participate are likely to differ in important ways from those who do. In Sierra Leone, for example, rates of participation in the UN-sponsored reintegration program were around 90 percent, in which case the non-participants used to construct the pseudo "control group" (as in Humphreys and Weinstein, 2007) are likely to be a highly self-selected group, even after controlling for observable confounders. Using "completion of the program" only adds additional layers of endogeneity. In the absence of some random shock to program access, identifying impacts of reintegration programs is a task that requires more assumptions than most people would be comfortable to make.

[4]See, for example, Colletta et al (1996) and the studies contributed to the Government of Sweden's *Stockholm Initiative on Disarmament, Demobilization, and Reintegration* (2006).

[5]See, for example, Kingma (1997); UNDP, 2000; Alden (2002); Paes (2005); Baare (2006) and Specker (2008).

[6]See, for example, Pouligny (2004); Muggah (2005) and Jennings (2008).

In this paper we exploit such a shock, which occurred during the reintegration program in Burundi and thus allows us to measure reintegration program effects with minimal taint from selection bias. A peace process in 2003-4 ended a decade of civil war in Burundi. Under the terms of the agreement, a National Commission for Disarmament, Demobilization and Reintegration program—CNDDR by its French acronym—was established to run a reintegration program with the support of the United Nations Operation in Burundi (UNOB) and the World Bank's Multi-country Disarmament and Reintegration Programs (MDRP). A small number of non-governmental organizations (NGOs) were contracted to deliver reintegration training and business start-up packages. An idiosyncratic bureaucratic failure caused reintegration programming to be unexpectedly withheld for a large segment of would-be beneficiaries who were on line to receive a training and economic reintegration package in 2006-2007. The failure was due to planning errors and complications in the personal relationship between CNDRR officials and one of the newly contracted NGOs, Africare. For the segment of beneficiaries registered in the region covered by Africare, the reintegration package was withheld for about a year, although it was available to peers who were assigned to the other agencies. During this time, would-be beneficiaries in the Africare region were forced to live their lives without a reintegration package. In the middle of this period, we fielded a large-scale a survey in Burundi. By making well-structured comparisons between those whose could not receive program benefits with those who could, we have a quasi-experiment on the effects of reintegration programming. Specifically, we can study whether the program had the desired impact on economic reintegration, and whether such impacts themselves have translated into political reintegration.

The paper begins by discussing the outcomes of interest and hypotheses pertaining to reintegration programming. We then discuss the more specific context of rebel reintegration in Burundi in the aftermath of the civil war from 1993-2004 and the nature of the reintegration assistance that the program provided. We then describe the methods that we use to identify effects of the program—or more correctly, effects of the *non-availability* of the program—and then present our estimates of effects on economic and political reintegration.

2 Defining reintegration outcomes

This paper investigates the effectiveness of reintegration programming in Burundi. The United Nations defines reintegration as "the process which allows ex-combatants and their families to adapt, economically and socially, to productive civilian life" (United Nations, 2000). Reintegration has social, economic and political dimensions. Social reintegration refers to an ex-combatant's relations with family and members of his or her community. Economic reintegration refers to an ex-combatant's ability to find a productive and sustained livelihood in the post-war context. Political reintegration refers to an ex-combatant's commitment to the laws and norms of civilian society, and especially, commitment to peaceful and democratic political expression (IDDRS 2.10). In this paper, we study economic and political reintegration. Economic reintegration is the direct target of most reintegration assistance (Spear 2006), including in Burundi. Because economic reintegration is presumed to facilitate political reintegration, we also measure effects on the latter, and we attempt to tease out whether economic effects may have contributed to political reintegration.

The primary objective of economic reintegration programs is to *help ex-combatants gain sustained livelihoods and enhance the economic welfare* (World Bank, 2002; 2004). This objective is based on the premise that a propensity to participate in violence is largely determined by the non-violent livelihood options available to an individual. This objective also draws from the research program on the political economy of conflict, which emphasizes the role of economic opportunities in reducing the risk of conflict (Collier and Hoffler 2004). In its 2003 report entitled *Breaking the Conflict Trap*, the largest sponsor of reintegration programs, the World Bank, claimed that "a structured DDR process, which demobilizes combatants in stages and *emphasizes their ability to reintegrate into society*, may reduce the risk of ex-combatants turning to violent crime or joining rebel groups in order to survive" (emphasis added). There are some reasons to believe that ex-combatants may face fewer economic opportunities and therefore be prone to engage in violence. First, ex-combatants may lack human and social capital endowments necessary to establish themselves economically.[7] These deficiencies may emanate from characteristics that set apart those

[7] For broad-brush reviews on possible sources of vulnerability among ex-combatants, see Annan and Patel (2009) and Tajima (2009).

who decided to participate in fighting , or they may be consequences of soldiering itself. Thus, ex-combatants may also face acute shortages of material capital. Time spent away soldiering may have caused an ex-combatant to have had his or her land taken over by someone else; or, for those with no land endowments to start with, time spent soldiering may have made it difficult for the person to acquire land. These deficiencies may put ex-combatants at an economic advantage and make them more likely to engage in banditry to support themselves. This suggestion is consistent with findings from Collier's (1994) research on demobilization and reintegration of former soldiers in Uganda. He finds that lack access to land significantly increased the likelihood of these former soldiers to commit crime, at least in the short-term. From this perspective, economic reintegration programs can reduce the risk of ex-combatants' involvement in criminality by addressing their human and social deficiencies and helping them become more competitive for employment opportunities.[8]

Economic reintegration may also affect opportunity costs associated with ongoing violent conflict. According to this logic, ex-combatants are rational actors who weigh costs and benefits of war and peace economies and may not return to civilian life unless the latter provide them with greater economic incentives. There are two sides to this argument. The first is that some combatants may have joined armed groups for economic reasons and thus economic reintegration can be seen as addressing these motivations. As Spear (2006) notes "an emphasis on [economic reintegration] recognizes that some of the motives for fighting were economic and that if the economic dimensions of the problem are not addressed, any settlement of the conflict may be short-lived." Second, the absence of law and order in conflict situations may provide combatants with opportunities to enrich themselves through illicit means, including robbery, racketeering and smuggling (Reno 1997). Ending the war is tantamount to giving up these economic opportunities—a prospect that may not be very appealing to many of them unless reintegration packages are attractive. There

[8]Some analysts suggest that the problem of postwar reintegration is not so much that ex-combatants have human and social capital deficiencies, but rather that the local economies they reintegrate *into* have very few economic opportunities available for them. For instance, speaking of the economic reintegration challenges faced by ex-combatants in Mozambique, McMullin (2004) notes "post-conflict states with impoverished economies offer little to reintegrate into. Mozambique, where only a tenth of the population had formal employment, was not exception, giving rise to quip: 'the government told us "now you are all equally poor. You have been reintegrated back into basic poverty" (emphasis in original).

is anecdotal evidence suggesting that at least some combatants make these sorts of calculations. During the DDR process in Liberia, a member of one of the Liberian United for Reconciliation and Democracy (LURD)—the main rebel group during the second civil war—was reported to say that "I still have my 81-mm mortar, but I just come to see whether the UN was giving fighters who disarm something good, If they don't give good money, I will not give the rocket" (Agence France Press, 8 December 2003; cited in Spear 2006). This is an example of spoiler behavior that Stedman (1997) argued can derail the peace process if not dealt with appropriately. More worrisome, however, is that ex-combatants like the one described above can become vulnerable to manipulation by elites who may want to start another war (Russett and O'Neal 2001). From this viewpoint, by providing a range of economic benefits and incentives, reintegration programs make civilian life more attractive and this can reduce the risk of conflict.

These logics constitute channels through which reintegration failure might destabilize the delicate peace in the aftermaths of civil war. As Alden (2002) remarks, "the spectre of former military personnel in criminal networks in the Balkans and Russia, the outbreak of violence inspired by real and self-proclaimed war veterans in Zimbabwe, and the participation of former security force members from Eastern Europe and South Africa in mercenaries in war-torn Angola and the Congo serve to underscore the destabilizing role played by former combatants who remain outside of the economy and society as a whole." Given that reintegration programs are seen as a key mechanism to prevent renewed armed conflict and their centrality in international peacebuilding interventions, there is an urgent need to ascertain their effectiveness.

There are two interrelated hypotheses associated with interventions that intend to boost economic reintegration. The first hypothesis is that reintegration programs can substantially improve the economic welfare of ex-combatants. The second is that improved economic welfare can substantially increase an ex-combatants' respect for the peace agreement and the rule of law. This paper investigates these hypotheses with quasi-experimental evidence in Burundi. We look at whether reintegration programming affected monetary income, occupational choice, and subjective assessments of economic-well-being. We then look at whether reintegration programs have an effect of increasing ex-combatants' satisfaction with the peace agreement and commitment to the rule of law.

3 The Burundi context

We focus on the reintegration of adult (18+) male former rebel combatants in Burundi. Members of the national army were also demobilized and offered reintegration assistance, but we do not study them. The reintegration experience of former army members is likely to be quite different than for former rebels. Being an army soldier is a legal, well defined career. Demobilization and reintegration support is well institutionalized: certain members of the national army had access to a system of pension-type benefits that were separate to those put in place by the internationally assisted reintegration program. The legality and institutionalized nature of an army career implies that there are fewer questions about demobilized army soldiers' "place in society." For these reasons, we do not think it is warranted to lump the two subgroups together. We choose to focus on reintegration of former rebels, which we believe to be of utmost interest to reintegration program designers. We focus on men only because we think women's experiences are likely to be distinct, but our sample of women is too small to study them adequately. Finally, we focus on former rebels who were aged 18 years or older as of fieldwork in 2007. Some of them were recruited before adulthood,[9] but they were all adults at the time of their demobilization, which could have been up to 3 years prior to fieldwork. Ex-combatants under 18 were treated by a different (UNICEF-managed) reintegration program than adults, in which case outcomes associated with them would not necessarily be comparable to those above the age cut-off.[10]

The DDR program in Burundi was initiated following a 2003-4 ceasefire that drew into the peace process the largest rebel group in the country, the National Council for the Defense of Democracy-Forces for the Defense of Democracy, or CNDD-FDD by its French acronym. At the time of our fieldwork in 2007, one rather small rebel faction—the Agathon Rwasa faction of

[9] The youngest ex-rebel in our sample is 19, and the years of recruitment for our respondents was between 1993 and 2003.

[10] Such a strict age cut-off creates an opportunity for a possibly well-identified examination of the benefits of programming for those just under 18 relative to those just over 18. This is something that could be pursued in further work. Those under 18 received considerably more support relative to their counterparts over 18 in mending relations with family and community members. Thus, there is a ripe opportunity for studying the impacts on social reintegration and the downstream benefits of social reintegration on economic and political reintegration.

the National Forces for Liberation, or FNL by their French acronym—remained outside the peace process. The war had begun in 1993 in aftermath of a tumultuous attempt at democratization. Elections in 1993 had resulted in the triumph of a party that represented the aspirations of a long-oppressed Hutu majority. Under still-mysterious circumstances, members of the southern- and Tutsi-dominated army led a failed coup attempt in October 1993; the coup attempt nonetheless involved the assassination of the recently-elected president. The event triggered massive violence throughout the country. The ensuing ferment gave way to outright civil war. The war had a devastating impact on the tiny, landlocked central-African country. Fighting touched most of the country. It resulted in deaths estimated at approximately 300,000 out of a total population of 6-8 million. Burundi's pre-war socio-economic development levels were already among the world's lowest, although for its income level, the country did have relatively well-developed infrastructure and institutions. The war severely stalled development for over a decade, resulting for example in an estimated 20% decline in real GDP over 1993-2002 (World Bank 2004, p. 6).

The outcome of the war and ensuing political developments are important features of the environment into which ex-rebels were reintegrating. The war resulted in a peace accord between the government and the CNDD-FDD that called for elections. Significantly, the 2005 elections resulted in the CNDD-FDD winning an outright majority of national assembly seats (59% of 118 seats) and communal councilor posts (55% of 3,225 posts). This gave the party the strength necessary to elect its political head, Pierre Nkurunziza, to be president. As such, the outcome of the war brought about a near revolution in the institutionalized political context relative to before the war. As of 2005, the former rebels were elected to lead. Contrast this to outcomes in the other countries where reintegration has been studied: in Sierra Leone, the party of former rebels, the Revolutionary United Front Party, managed barely 2% of votes, failing to win a single seat in the 2002 elections that punctuated the end of the war. In Uganda, the Lord's Resistance Army is still an illegal organization, never having had its popular legitimacy tested. Though the number of cases is too small to test the claim rigorously, there is good reason to believe that conditions causing rebel parties to fair well electorally are associated with reintegration prospects of demobilized rebels. These considerations should be taken into account when trying to generalize the findings from this paper.

The 2000 Arusha Accords called for DDR, and these Accords set the parameters of the 2003 Pretoria agreement, which brought the CNDD-FDD into the peace process and set the stage for the 2005 elections. A February 2004 World Bank report set specific DDR targets; a November 2004 Joint Operations Plan (JOP), which was approved by a committee that included all key national and international agencies, made these targets official (World Bank 2004; Republic of Burundi 2004; Boshoff and Vrey 2006, p. 15). The MDRP and national implementing agencies designed a program that was intended to demobilize and reintegrate enough combatants to achieve a new army (Forces de Defense Nationale) of 25,000 and police force of 20,000.[11] The World Bank's estimates for the sizes of the various forces is shown in Table 1. Both members of the national army and the rebel groups would be demobilized. DDR would occur in two phases, with the first phase (2004-5) involving 9,000 former rebels and 5,000 former army. Those not demobilized in the first phase would then join an interim integrated national army and police. Over the ensuing years, some 41,000 additional combatants from the newly integrated security forces were to be demobilized as part of a broader army and police reform program.

The DDR program in Burundi was part of the broader Multi-Country Demobilization and Reintegration Program (MDRP)[12], which was intended to embody a comprehensive strategy to "enhance prospects for stabilization and recovery in the region." The program documentation for the MDRP's programs reflected a strong presumption that demobilized combatants would likely suffer human and social capital deficits that would have to be remedied, otherwise "disaffected ex-combatants can pose a threat to stability." The MDRP characterized economic reintegration as the establishment of "sustainable livelihoods." Assistance was to do no more than would be "necessary to help them attain the general standard of living of the communities into which they

[11]The original project proposal set a demobilization target of 55,000 combatants. However, this target appears to have been an error: the size of the various forces prior to demobilization was estimated to be about 80,000. The target army plus police force size was 45,000. If no new recruitment was to take place, somewhere around 35,000 would need to be demobilized. Recruitment into the new armed forces was to take place, but no where near the 20,000 that would make the 55,000 number realistic. The actual number demobilized as of June 2007 (the time of fieldwork) was about 23,000; to date, the total is about 28,000, about half the 55,000 target proposed in 2004 This error is acknowledged in World Bank (2009).

[12]Participating countries included Angola, Burundi, Central African Republic, Democratic Republic of Congo, Rwanda, Republic of Congo, and Uganda.

reintegrate" (19). Social reintegration, in the form "reconciliation" between ex-combatants and civilians in their communities, was taken to be an important pre-condition for achieving a sustainable livelihood. The program took an "individual-oriented" approach—one that emphasized providing means to individual ex-combatants, as opposed to trying to do much at the level of communities; this orientation was apparently due to an implicit agreement that would have other agencies—namely UNDP—taking responsibility for community reconstruction.

The reintegration program included a few components. The CNDRR directly administered a reinsertion subsistence allowance of XXX Burundian Francs (FBu, with 500 FBu equivalent to 1 2007 dollar in PPP terms) provided in X tranches over XX months. Documentation from the program shows that at the time of our fieldwork in 2007, receipt of these tranches was nearly universal among ex-combatants (XX%).[13] Also, through offices set up in nearly all provinces and through "focal points" appointed in nearly all *communes*, ex-combatants had access to various forms of counseling, including psychological counseling. These too were universally available. Thus, reinsertion allowances and counseling are not sources of variation in our study. The major benefit offered by the program was the so-called "socio-economic reintegration package." The package provided a menu of opportunities from which ex-combatants could choose. They could choose to be admitted into secondary school or university, to take up a one-year or shorter vocational-training program, or to take up a package to help begin "income-generating activities." The latter would involve working with one of the reintegration program's implementing partners to devise a business plan, receiving basic training on running a business, and being provided with in-kind start-up materials (money was not given). Program documentation shows that the "income-generating activities" option was by far the most popular, with XX% taking up this option, in comparison to X% taking up vocational training and X% pursuing their education [I NEED TO GET THE SPECIFICS FROM THE MDRP QUARTERLY REPORTS.]

The CNDRR and MDRP delegated the delivery of the socio-economic reintegration package benefits to NGO partners. Early in the program, when the number of beneficiaries processed

[13] Interestingly, the allowance was administered through transfers to special bank accounts for ex-combatants, giving many ex-combatants their first exposure to bureaucracy and the formal banking sector. Interviews in the field revealed that some ex-combatants applied what they learned through this by offering for-profit services to help other people deal with banks and government offices.

and ready to receive benefits was rather small, a large and rather disorganized collection of local NGOs was contracted on an ad hoc basis to deliver benefits. In 2006, anticipating a surge in the number of ex-combatants who would be coming online to receive benefits, the MDRP decided to tighten up the system and contracted three large NGOs to deal with the coming wave of ex-combatants. These included PADCO and Twitezimbere, two Burundian NGOs, and Africare, an international NGO. The work was divided evenly among the three NGOs. PADCO was assigned to cover ex-combatants registered as residents in the south-west provinces, Twitezimbere was to cover ex-combatants registered as residents in the northern provinces, and Africare was to cover ex-combatants registered as residents in the center provinces. The assignments to the three NGOs were made by the end of the summer in 2006, and programming was to begin as soon as possible. The selection of Africare as one of the three partners was due to pressure by MDRP donors to have at least one of the implementing partners be an international NGO; the pressure was due to certain budget limitations, it seems. This was the case despite the program administrators' and the CNDRR's concerns about the readiness of Africare to implement the program.[14] Indeed, it came to be a major problem: while PADCO and Twitezimbere were able to begin quite quickly, Africare's presence on the ground was barely established by late 2006.[15] This was followed by a contracting dispute between CNDRR and Africare that caused further delays. As a result, designated beneficiaries in the Africare area were denied access to the reintegration package until late 2007. This disruption in program access corresponded to the timing of our fieldwork: PADCO and Twitezimbere had begun reintegration programming by late 2006, whereas the problems with Africare's commencement of delivery would not be resolved until August 2007. Our fieldwork was conducted in June/July 2007. Thus, the respondents in our sample from the PADCO/Twitezimbere areas had access to reintegration programming, but those from the Africare areas did not, or at best, were only just beginning to have access. So long as any other differences between sample respondents from "center" areas and other areas can be controlled for, this bureaucratic breakdown provides us with a source of near-exogenous variation in program access. Such is the cornerstone

[14]Interview with Marcelo Fabre, MDRP, February 2009.

[15]Geenen (2007) notes that Africare had no presence on the ground in the area of her fieldwork, Ruyigi, in November-December 2006, and that they expressed concerns themselves about whether they could implement the program.

of our strategy for identifying the effects of the reintegration program.

4 Identification

Two majors concern in the study of assistance programs such that is one are self-selection by beneficiaries into the program and targeted selection of beneficiaries by the program managers. Such selection is typically based on would-be beneficiaries' and program managers' predictions about how much beneficiaries will gain by participating. Much of the information that forms such judgments is unobservable by the analyst. In the Burundi reintegration program, the disruption in program access was due neither to self-selection among would-be beneficiaries nor targeting by program managers. The arbitrariness of the disruption considerably reduces the scope for confounding due to unobserved factors.

However, this shock to program access is imperfect as a source of quasi-random assignment at the individual level for two reasons. First, there may have been *incidental* imbalances in the attributes of ex-combatants in the Africare versus non-Africare regions prior to the disruption to access. Second, the disruption in program access occurred after some ex-combatants in the Africare region had already received their reintegration package. Unfortunately, our data do not allow us to identify these individuals, although information from the program itself provides the true rate at which this occurred. Third, region-level differences could confound our estimate of the disruption effect. The rest of the section explains our strategy for identifying causal effects in the face of these possible sources of bias.

4.1 Incidental imbalance

Because the shock to program access occurred at the region level (rather than the individual level), incidental imbalances in the attributes of ex-combatants in the Africare and non-Africare regions threaten the validity of causal inferences drawn from comparing the two groups. In the analysis below, we study such possibilities by conducting balance checks on a rather large set of pre-disruption covariates. We find that balance is actually quite good over this large set of covariates, lending more credibility to the idea that the disruption in program access has the character of an experiment. If such balance can be found on such a large set of observable covariates, we have more reason to believe the unobserved factors are also in balance.

As a further step, we used a genetic matching algorithm (Sekhon, n.d.) to match Africare and non-Africare ex-combatants on this set of covariates. Given the rather modest sample size (less then 400 ex-rebel observations), matching achieves remarkably good balance on this large set of covariates. Of course, we cannot control for region-wide differences that distinguish the Africare region from the non-Africare region, as they would coincide perfectly with our shock to program access. An identifying assumption that we make is that once the incidental individual-level differences have been accounted for in our sample, there are no remaining region-wide differences. We examine this assumption below.

In our empirical analysis, we present (1) simple unadjusted differences between Africare and non-Africare regions, (2) differences adjusted via regression on our covariate list, (3) unadjusted matched differences, and (4) regression adjusted matched differences. We take (4) to be the least biased estimate, and thus designate that as the primary estimate of the causal effect. If estimates (1)-(3) tend to be close to (4), then we may infer that the covariate conditioning contributes little to bias reduction, although it may make our estimates considerably less precise. In those cases, estimates (1)-(3) allow us to assess the amount of precision that we are sacrificing, perhaps needlessly.

4.2 Exposure heterogeneity

The disruption in program access in the Africare region occurred after some reintegration programming had already begun. Table 3 provides the relevant figures, obtained from the reintegration program itself. We see that as of the time of our fieldwork in July 2007, the program disruption affected 53% of designated ex-combatant beneficiaries in the Africare region. If we were to simply measure differences between Africare and non-Africare ex-combatants, we would obtain an estimate of the disruption effect that is biased toward zero. Such bias is attributable to the exposure heterogeneity among Africare-region ex-combatants. To correct for this source of bias, we use a strategy that weights our effect estimate by the inverse of the difference in disruption rates across the Africare and non-Africare regions. To see how this works, consider the following model,

$$y_i^* = \beta_0 + \beta_1 t_i + \mathbf{x}_i \boldsymbol{\beta_2} + \varepsilon_i$$
$$t_i = \alpha + \phi z_i + \eta_i,$$

where y_i^* is a latent outcome variable, $t_i \in \{0,1\}$ is an endogenous indicator of exposure to the disruption, $z_i \in \{0,1\}$ and $\mathbf{x}_i \in \mathbf{R}^k$ are exogenous variables in that $\mathrm{E}(\varepsilon_i|(z_i,\mathbf{x}_i)) = 0$, and $\boldsymbol{\beta_2}$ is a $(k-1)\times 1$ vector. For the data considered here, the $z_i = 1$ refers to being in the Africare region, and $z_i = 0$ refers to being in the non-Africare region. The $t_i = 1$ refers to one's access to the program having been disrupted, $t_i = 0$ means access was not disrupted. The coefficient β_1 is our estimate of the program disruption effect. The coefficient ϕ measures the expected difference in the ex post probability of disruption for those with $z_i = 1$ relative to those with $z_i = 0$. The fact that t_i and z_i are binary variables does not affect our interpretation at all (see Angrist and Pischke, 2008, Ch. 4). We use a latent variable, y^*, for generality to generalized linear models that can be expressed in terms of latent variables (e.g. all binary, ordered, or multinomial probit/logit models; see Long, 1997). The transformations discussed in this section would then apply to differences on the latent variable rather than the observed response. When we are working with a linear model, then y^* simply equals the observed response.

We make the following exclusion restriction assumptions:

Assumption A.1: $\mathrm{E}(\eta_i|z_i) = 0$, and *Assumption A.2*: $\mathrm{E}(\varepsilon_i|z_i) = 0$.

If A.1 and A.2 are true, then substituting the right-hand side of the expression for t_i into the right-hand side of the expression for y_i^*, we can write the the expression for y_i^* as follows:

$$y_i^* = \tilde{\beta}_0 + \beta_1(\phi z_i) + \mathbf{x}_i \boldsymbol{\beta_2} + \tilde{\varepsilon}_i,$$

where $\tilde{\beta}_0 = \beta_0 + \beta_1 \alpha_0$ and $\tilde{\varepsilon}_i = \beta_1 \eta_i + \varepsilon_i$ with $\mathrm{E}(\tilde{\varepsilon}_i|(z_i,\mathbf{x}_i)) = 0$. If we know the population value of ϕ exactly, then we can compute the ϕz_i values, and use the results of a regression of y^* on ϕz_i and $\mathbf{x_i}$ to obtain unbiased estimates of the effect, β_1, with the appropriate standard error.[16]

[16]See Murphy and Topel (1985) for properties of estimators such as this that rely on obtaining "missing" regressors from auxiliary information. In our case, since we are assuming that we know ϕ exactly, there is no added approximation error, and thus no additional inflation of the standard errors. Another way to look at this is as a two-sample instrumental variable estimator, along the lines of Angrist and Pischke (2009:147-150), but where the first stage parameters are known exactly.

Documentation from the program (MDRP) itself allow us to obtain a value for the true rates of disruption in the Africare and non-Africare regions. We assume that these rates are equivalent to the ex post expected probability of disruption conditional on whether one was located in the Africare region or not. Our discussion with a key program official suggests that this is a reasonable interpretation, as pre-disruption access seemed to be quite random.[17] If one is wary about this, one may take a conservative position, and assume any estimates based on this interpretation form an upper bound; a lower bound is available by not correcting for ϕ at all, so informative bounds can be constructed.

Table 3 shows the rates of disruption for the population of demobilized combatants registered to receive benefits in the Africare and non-Africare regions. Based on this information, we take $.53 - 0 = .53$ as the value of ϕ. The MDRP documentation also provides information on the rate at which beneficiaries who were due to received benefits after the NGO transition actually *took up* the program by the time of fieldwork in July 2007. These rates reflect the disruption in the Africare region: at the time of fieldwork, about 66% of these post-transition beneficiaries in non-Africare regions had already begun their engagement with the program, while only 16% had begun to do so in the Africare region. We stress that we *do not* want to incorporate this information into the construction of ϕ. The rate of program take-up in the non-Africare region reflects, to a large extent, self-selection; those who had not taken up the program by then should nonetheless be considered as having had uninterrupted access. Individuals in the non-Africare region should have factored the availability of the reintegration program into their decision-making, and so the effect of access to the program should be evident even among those who have chosen not to take it up. The very small number of beneficiaries having begun engagement with the program in Africare regions after the disruption had only recently done so at the time of fieldwork. Considering them to have been subject to the disruption has, at worst, an effect of biasing our effect estimate toward zero. Thus, to be clear, we are measuring the effects of *disrupted access* to a functioning reintegration program. By disregarding these rates of program take-up, we actually take a more conservative approach, in that we do not attempt to make the program look more effective by netting out any dilution due to non-engagement by certain beneficiaries despite their having had access.

[17] Interview with Marcelo Fabre, MDRP executive offices, February 23, 2009.

The above results are based on a homogenous effects model. If we loosen that assumption to allow for variable coefficients, then we could interpret our estimate of β_1 as the effect of the program only on those whose access to the program would depend on z_i (see Angrist and Pischke, 2009, Theorem 4.4.1). It seems to us that the estimate under the homogenous effects assumption is probably higher than the true average effect of the program. This is because people who obtained access to the program in the Africare region are likely to have been more organized generally about their personal affairs; therefore, we suspect for them, the program's benefits would be less. For these reasons, we believe that the scaled estimates are likely to be an upper bound on the true average effect. Reduced form estimates—that, is estimates from regressions using the unadjusted z_i rather than ϕz_i—are a lower bound. We report both below.

4.3 Region level differences

To be inserted: a section that compares contextual variables in the Africare versus the non-Africare region (excluding Buja) to assess any threats to validity that may come from differences in the economic context. We cannot control for these factors because they will be perfectly correlated with our indicator of program disruption. However, we can use this assessment to determine whether any differences will likely tilt the analysis toward or away from the null.

5 Data

The ex-combatant data are drawn from the multi-purpose *Wartime and Post-Conflict Experiences in Burundi* survey.[18] The survey includes data from interviews with civilians, as well as ex-rebels and ex-army, both demobilized and those integrated into the new security forces. This paper works with only the demobilized ex-rebel data. The data were obtained through a multistage random sample from lists of demobilized ex-rebels registered to receive reintegration benefits through Burundi's national DDR program (the CNDRR). The first stage involved randomly selecting half of Burundi's 129 *communes*—Burundi's second-tier administrative unit. We then set a target number of ex-combatant interviews to complete in each commune, with targets proportional to the number of

[18] Details on the survey are available at www.columbia.edu/∼cds81/burundisurvey/

ex-combatants registered with the DDR program in the commune. Targets ranged from 2 to 33. Then, we obtained from the national DDR office the complete lists of ex-combatants registered as residents of each of the selected communes. We then drew a simple random sample (with a random number generator in Stata) of the desired number of interviews from each of these commune lists; we also created a randomly selected reserve list to draw from in the case of non-response. Selected participants were contacted and brought to the respective Provincial Bureau by DDR program staff for interview on a scheduled date by our enumeration team. The rate at which our first choice was interviewed was very high—XX% (CHECK)—and so we assume no need for further adjustment to account for non-response.[19] This was likely due to a few factors: (1) respondents probably took the interview to be a requirement of the DDR program given that they were contacted by the DDR program staff themselves; (2) we accommodated respondents' schedules by setting dates for interviews well in advance; (3) the fieldwork was conducted during the idle interim period between planting and harvesting seasons, and so there was little risk of non-response due to people having to attend to agricultural demands; and (4) while participation was voluntary, a "transport allowance" of about 2 US dollars was provided to each respondent after they completed the interview, thus making it worthwhile for respondents to sit through the entire interview. With the observations limited to former rebel males registered to receive reintegration assistance outside Bujumbura, the dataset includes 110 ex-rebels registered in the Africare region and 261 outside the Africare region.

As is common in large scale surveys such as this, the data exhibit occasional missingness on items. Such missingness is due to "don't know" responses, enumerator error, or data-entry error. In the subset of data that we used, only once did the item missingness rate reach 10% for any given variable—such was the case for the measure of the death rate in an ex-combatant's fighting unit; otherwise the rates of item missingness were usually 0, but occasionally around 1-2%. Listwise deletion would nonetheless have implied discarding 23% of excombatant observations. To avoid the inefficiencies and biases associated with listwise deletion, we used multiple imputation to fill missing values.[20] Multiple imputation was conducted using the "mice" package for the

[19] Keep in mind that the covariate adjustment that we perform below will reduce any biases associated with those factors. With such a low non-response rate, it is reasonable to believe that any residual bias is negligible.

[20] In additional to the loss in statistical power, listwise deletion can bias results by either discarding cases based on the value of the dependent variable or distorting the relationship between the sample and population in ways that bias

R statistical computing environment (van Buuren and Groothuis-Oudshoorn, 2010). "Predictive mean matching" imputation was used for all numeric, binary, and ordered categorical variables; multinomial logit regression imputation was used for non-ordered categorical variables. Predictive mean matching (also known as "nearest neighbor hot deck") is attractive because, when the item-level data exhibit only low levels of missingness, it is robust to a wide range of misspecifications of the imputation model (Little and Rubin, 2002:69). Uncertainty in predictive mean matching arises in the fit of the predictive mean model. With the multinomial logit, uncertainty arises from the fit of the model and the fundamental uncertainty inherent in the outcome being modeled as a random draw from a multinomial distribution. To account for these imputation uncertainties, multiple imputed datasets should be generated (hence "multiple" imputation). The analysis should then be run on each of the datasets, and averaged using the appropriate averaging forumlas. This was the approach that we took, working with five imputed datasets.[21]

6 Inference

For primary estimation on each of the imputation-completed datasets, we use sandwich estimates of coefficient variances that account for clustering at the commune-level. For the quantile regression estimates, we use standard errors from inverted rank-test confidence intervals asymptotically robust to non-iid sampling (Koenker and Hallock, 2000).[22] To combine the estimates from each of the imputation-completed datasets, we used standard formulas based on the properties of mixed normal distributions (known as "Rubin's rules," Rubin, 1987). If a coefficient estimate from dataset j is called $\hat{Q}_j$, then the point estimate is simply, $\bar{Q} = \frac{1}{m}\sum_{j=1}^{m}\hat{Q}_j$, the average over the m imputation-

approximations (e.g. linear or quadratic) of unknown functional forms. This is explained in Samii (2010). If multiple imputation is done in a reasonable manner, it will be considerably less biased and will generate appropriate uncertainty estimates. See King et al (2001) for some general discussion of multiple imputation methods, and Rubin (1987) and Little and Rubin (2002) for deeper treatments. Approximation bias is best understood through the lens of non-random sampling. See, e.g., Korn and Graubard (1999:159-185).

[21]As Rubin (1987:114) explains, rather few imputations are typically needed to achieve reliable estimates.

[22]Bootstrapped confidence intervals are inappropriate on the matched sample (Abadie and Imbens, 2008), and to ensure comparability of results across the estimates from matched and unmatched data, we used only analytically derived standard errors and asymptotic confidence intervals.

completed datasets. The standard error of a point estimate is given by,

$$SE_{mi} = \sqrt{\frac{1}{m}\sum_{j=1}^{m} U_j + \left(1+\frac{1}{m}\right)\left[\frac{1}{m-1}\sum_{j=1}^{m}\left(\hat{Q}_j-\bar{Q}\right)^2\right]},$$

where U_j is the variance of the coefficient estimate from dataset j. Evidently, the standard error is the square root of a weighted average of the mean of the variances within each dataset and the variance of the estimates between datasets. Confidence intervals rely on the asymptotic normality of the underlying parameter estimators. So long as such asymptotic normality holds (which it does for all estimators that we use in this paper), then $\frac{\bar{Q}}{SE_{mi}}$ is distributed asymptotically as $t_{[df]}$ with degrees of freedom, df, given by,

$$df = (m-1)\left(1+\frac{m\bar{U}}{(m+1)B}\right)^2,$$

where $\bar{U} = \frac{1}{m}\sum_{j=1}^{m} U_j$. The 95% confidence intervals and margins of error (equivalent to half the confidence interval) reported below are based on these formulas, using the quantile values from the appropriate t distribution.[23]

7 Covariate Balance and Matching

7.1 Covariates

A covariate should be considered confounding if it is (1) correlated with the treatment, (2) correlated with the outcome of interest, and (3) temporally prior to the treatment. We made the case above that our treatment (in this case delay in receiving program benefits in the areas serviced by Africare) is plausibly randomly assigned. In other words requirement number (1) above should not be met. However we are aware of the fact that our "treatment" (in this assigned to the Africare region) is not truly an experiment and furthermore we make no claims that our subjects were matched prior to assignment (i.e. blocked) so checking balance is required. Furthermore Ho et. al. point out that preprocessing our data with matching in this way addresses the potential problem of model

[23]Estimation was conducted in both R and Stata. In Stata, when possible, we used the imputation combination function provided by the Clarify software (Tomz et al, 2003), which uses precisely the formulas given above. When this was not an option in Stata, and for the estimation in R, we used our own imputation combination programs. The programs are available at http://www.columbia.edu/∼cds81/.

dependence. Our concern was that those ex-combatants in the Africare areas might be substantially different according to important covariates than their fellows in the other areas of the country that received the treatment earlier. Therefore we have to be concerned about making inferences from extreme counterfactuals (King and Zeng). Since we do not know the true functional form of the relationship between our covariates and our outcome of interest (post-war economic well being we cannot assume that we know the relationship between a given independent variable over the whole range of a given the independent variable. By fitting a particular function (say linear) for to the relationship between independent and dependent variables we are implicitly assuming that the relationship between a given independent variable was the same for very hi levels of that variable and very low levels of that variable. Pre-processing the data with matching overcome this problem by making comparisons between treated and control cases that are very close to each other in terms of the values of their covariates. This substantially reduces the implications of our functional form assumptions because the difference in predicted values from various functional forms will be quite similar where covariates are quite similar (Ho et. al, 2007, King and Zeng)

In what follows we list the variables on which we matched and our reasons for including them in our list of covariates. In all cases the data used for the matching were taken from the Burundi Peacebuilding Survey, implemented by the authors.

Age We hypothesize that younger ex-combatants will be less well endowed with pre-existing skills and social networks that are necessary for successful economic reintegration. Previous studies (**?**) have shown that child soldiers in particular are deprived of the crucial period of their lives when important human and social capital skills are developed. Thus or expectation is that younger ex-combatants may experience less economic reintegration than older ex-combatants.

Hutu Since the war had a strong Hutu versus Tutsi ethnic component and since the predominantly Hutu CNDD won the war we would expect Hutu ex-combatants to fare better after the war *ceteris paribus* than Tutsis

Father's education We use an indicator variable for whether the respondent's father completed primary school. We take this to be an indicator of the respondent's family's socio-economic status, which is likely to correlate strongly with the respondent's economic prospects.

Father in Agriculture Prewar An ex-combatant with a father in agriculture before the war was probably relatively poor compared to those ex-combatants whose parents had more lucrative jobs in an urban area. Thus we include this covariate as another control for the ex-combatants' pre-war socioeconomic status.

Prewar Education Ex-combatants with more education should, we expect, have an easier time reintegrating into the economy after the war. For this variable we converted number of years in education into a dichotomous variable equal to one if the ex-combatant

Prewar Wealth Because ex-combatants with greater wealth before the war should, we presume, have higher incomes after the war we include a scale that captures the ex-combatant's stock of assets before the war. We created the scale based on survey respondents' responses to three questions: did their beds have sheets, did they own a radio, and did they own any cattle. The bed-sheets and radio measures were devised after focus groups, in which these assets were deemed markers of a household having moved beyond basic subsistence in their consumption. Ownership of cows a distinctive indicator of wealth. We fit a two-parameter logistic graded response model on these responses, using the "ltm" package in R (Rizopoulous, 2006). We then generated the factor scores from this model using the imputation method described in Rizopoulos and Moustaki (2008).[24] We fit the model on a pooled dataset that included the ex-rebels as well as civilian survey respondents hailing from the same regions as the ex-rebels. We assume that the factor loadings are common across the two groups; including the civilians greatly improves the precision of the fit.

Prewar Relative Economic Deprivation This variable is the respondent's own assessment of whether

[24] We also asked respondents about whether they owned land but did not include it in the scale because there was too little variation in the variable (about 95% of respondents indicated that they possessed land prior to the war). Our choice of these three indicators was based on the results of a Mokken analysis of all the indicators. That analysis showed that these indicators loaded monotonically and contributed a moderately strong, single dimension signal (all *H* scores were at or above about .4; see van der Ark, 2007). We used ANOVA tests to determine whether a one, two, or even higher order logistic model fit the data best, and found that the two parameter model was optimal.

or not he was poorer than his neighbors. It helps to capture features of pre-war economic status that we may not be picking up in our objective measures.

Father Alive and Socializes Most with Family, Prewar Families are the primary support networks for most Burundians and male adults are primary providers within these support networks. If an individual's father was no longer living, then we presume that the individual's family support network was less robust. Individuals who tended to spend time away from their families may not have strong ties to a support network on which they can rely after returning from the war. Both of these factors would put the individual at a disadvantage economically.

Prewar Urban Resident Urban residents are generally wealthier than people who live in the countryside in Burundi. Furthermore more economic opportunities and social networks and sources of human capital exist in urban environments than in the countryside. Therefore we control for prewar urban residence to proxy for these characteristics of urban life.

Postwar Urban Resident Similar to argument made above urban residents have more economic opportunities than those in the countryside so we must control for this factor which is possibly confounding to the ex-combatants economic well being. There is no concern with post-treatment bias by including this covariate because ex-combatnts chose their locale of residence before they entered the reintegration program.

Unit Death Rate We are concerned that ex-combatants who experienced the most mortal combat would be more psychologically traumatized and therefore might find it more difficult to return to normal economic activity.

Year in Faction We hypothesize that ex-combatants who were in combat longer would find it more difficult to reintegrate into society.

Community Violence Like Unit Death Rate our concern with level of community violence is that ex-combatants who were exposed to more violent trauma, in this case because their community was heavily affected by violence, may have a harder time reintegrating and finding gainful employment.

Family Death Rate Our concern was, again, that more exposure to violence would make economic reintegration more difficult. With this covariate we capture exposure to violence in terms of the percentage of family members who were killed in the war. Furthermore postwar economic opportunities are often supplied by social networks like the family. Ex-combatants with a large percentage of family members killed would, we speculate, have fewer contacts with which to pursue employment.

Unit had Written Rules We hypothesize that ex-combatants who were subject to more discipline during the war should find it easier to reintegrate into civilian life. We measure the degree of discipline of the ex-combatants life during the war with a dichotomous variable equal to one if the ex-combatant's unit had formal written rules of conduct and zero otherwise.

Non-CNDD Faction Member While several rebellious factions fought in the war the CNDD came out the clear winner of the conflict. As such CNDD members had more economic opportunities after the war than did members of other factions like the FNL. As such we expect CNDD members to have a more successful economic reintegration experience than non-CNDD members.

Demobilization Date Ex-combatants who demobilized earlier had a longer period to reintegrate than did those who demobilized later so we expect ex-combatants who demobilized later to have less success in economic reintegration.

Propensity Score In order to insure overall balance of our covariates we also matched on the propensity score developed from all of the above mentioned variables.

7.2 Balance

Balance statistics are presented in Tables 4 through 6. Not surprisingly, given the argument we made above about the quasi-experimental nature of the Africare treatment, many of the covariates are well balanced even without matching. This was the case for age, Hutu, father's education, prewar education, prewar relative dependence, Father Alive, unit had written rules and demobilization date. This raises the question of whether these variables are truly confounding. Despite that concern we matched on them to insure that they remained in balance even after matching on the covariates that were not naturally in balance. The fact that so many covariates were in balance even without matching is evidence for our argument about the quasi-experimental nature of the Africare treatment, which in turn should give us confidence that if there are unobserved differences between the Africare and non-Africare regions we have some hope of achieving balance on them.

The fact that several variables were balanced *before* matching also has implication for how we interpret the balance statistics later. Ho and King recommend looking at *improvement* in balance rather than the standard *p*-scores on *t*- and KS tests. Their rationale is that many observations are dropped as a result of matching procedures and so *p*-scores can improve simply by virtue of the standard errors increasing as the sample size gets smaller. Imai et. al (2008) recommend, wisely we think, that the researcher must also make sure that balance is actually improving–means are getting closer together and observation are getting closer to the 45 degree line in the QQ plots. We agree with their argument but hasten to point out that it is not cause for concern that some covariates' balance actually worsens as we attempt to achieve balance on other (previously unbalanced) covariates. These covariates were in balance before matching and continue to meet standard criteria for balance after matching.

In contrast to the covariates described above there were several variables that were not balanced between the Africare and non-Africare areas. These included our scale of prewar socioeconomic status, pre- and post-war urban residence, unit death rates, the ex-combatant's number of years in his faction, level of community violence, non-CNDD faction member and the propensity score. Ex-combatants in the Africare areas scored significantly higher on our prewar socioeconomic scale than did ex-combatants in the other areas suggesting that they had significantly higher levels of wealth *ex ante* than did other ex-combatants in the DDR program. Ex-combatants serviced by Africare were significantly less likely to be urban residents both prior to the war and after. Africare-serviced ex-combatants were exposed to significantly less violence than were ex-combatants serviced by the other NGOs. Unit death rates and especially level of community violence were considerably lower for the former set of ex-combatants than the latter. [25]

As we mentioned above, because of these substantial differences if we simply compared the economic performance of Africare ex-combatants to ex-combatants serviced by other NGOs we would be open to the charge of making inference from extreme counterfactuals. Since we do not know the functional form of the relationship between these variables and economic reintegration we may be guilty of attributing any differences between the treated and control groups to the treatment when in fact it is due to one or more of the covariates behaving differently than our assumed functional relationship. For example suppose we assume that the relationship between exposure to violence is linear and that after controlling for the assumed linear relationship between exposure to violence and economic reintegration we find that Africare areas enjoyed significantly less economic reintegration than non-Africare regions. One explanation of the findings is that the delay in receiving programming in the Africare regions caused ex-combatants to reintegrate less fully than did those ex-combatants in other areas–strong evidence for efficacy of the DDR program. Another explanation however is that the relationship between exposure to violence and the success of economic reintegration is not linear but in fact exposure to violence had a marginally decreasing affect. In that case we would be attributing the better performance on the non-Africare areas to the fact that the program started earlier when in fact it was simply that the higher levels of exposure to violence in the non-Africare areas did not have as deleterious effect on reintegration prospects as our linear assumption predicted. We can avoid this problem by comparing ex-combatants in the Africare areas that have similar levels of exposure to violence (and urban residence and so on) to the ex-combatants in the non-Africare areas. When we do that our functional form assumptions have much less of an impact on our inferences because we are comparing cases in the same "neighborhood" of the data.

The statistics in Table 4 through 6 show that we achieved excellent balance. The t-tests on difference of means indicate that the distribution of our treated and control cases have statistically indistinguishable means. This is true for all variables in all five samples. Indeed the lowest p-value in the tables is 0.307. The Kolmogorov-Smirnov (KS) tests also show that we have achieved

[25] In contrast family death rates were not all that different between the Africare-serviced ex-combatants and the rest of the ex-combatants.

excellent balance for six of the nine variables where that test was appropriate. For these six variables over the five samples the lowest p-score is 0.166 and most were considerable higher. The cases where balance was not so good even after matching include number of years in the faction, community violence and the propensity score. As mentioned above even in these cases we have excellent balance as measured by the t-test on the difference of means tests. Furthermore in most cases there is convergence in the variances of the treated and control groups, so we have good balance at least on the first two moments of the treated and control distributions. Furthermore the percentage improvement in the QQ plots, which is presented in the last column of Table 4 through 6 shows that in most case the balance on these three variables improved considerably as a result of our matching.

8 Effects on economic reintegration

We now present results on the effects of the program disruption on three measures of economic reintegration: (1) income and, as a consequence of income, poverty incidence; (2) livelihood, specifically whether the respondent obtained any livelihood, an agricultural livelihood, a livelihood in the non-agricultural, non-skilled sector, or a livelihood in the non-agricultural skilled sector; and (3) the respondent's own assessment of his economic well-being. All estimates are displayed in tables and figures that are placed in the appendix. A reference guide for the various estimates is given in Table 7. As indicated above, our primary estimates are labeled "iv.a" (lower bound, with respect to the exposure heterogeneity adjustment) and "iv.b" (upper bound).

8.1 Income and poverty incidence

We use a linear regression to examine effects on mean "log-monthly income + 1"; that is, the natural logarithm of monthly personal income (in Burundian Francs, or FBu) reported by the responded, with 1 added. These results are presented in Table 8. We used the natural logarithm as a variance stabilizing transformation and to reduce sensitivity to outliers; we added 1 to handle the few cases of zero reported income. To check for sensitivity associated with adding 1, we also fit a Tobit regression on log of income with zero-income observations treated as censored; the results were identical. Inspection of the distribution of log monthly income variable over the covariates showed that the transformation was effective in removing heteroskedasticity, although the overall variance of the logged variable was still quite high. This is evident in the results. While our primary estimate of the difference in means (coefficients b.iva and b.ivb) is very large, the difference has a p-value (two-sided) of .16.

Given the noisiness of the outcome variable, even after the log transformation, we also fit a quantile regression to study effects at difference income deciles. The quantile regression allows us to study whether the program caused non-constant shifts in the income distribution, perhaps reducing the incidence of very low or very high income, even if the mean was not significantly affected. Our estimates are shown in Table 9. We see very large and statistically significant differences for lower deciles, and smaller (although still statistically significant) differences at upper deciles. The estimates at the end points are very noisy because of sparse data below and above the 10th and 90th percentiles, respectively. The estimates show that the effect of the program was concentrated among those who would otherwise fare very badly. to put it another way, the program introduced a floor on one's potential income, with these floor effects less strongly felt for those who would otherwise earn a lot.

Figure 1 illustrates these results more clearly. In the left panel, we display income distributions (FBu/month, on the log scale) from the five imputed datasets for the Africare respondents and for their matched controls. We see that the Africare distribution is substantially heavier at no income (labeled as "(None)") and at lower income values (e.g., below 10,000 FBu/month), although the Africare respondents also include a handful of high earners. The right panel displays the quantile regression estimates in substantive terms. A model-adjusted fit of the actual cumulative income distribution for Africare-region respondents is plotted with the black line and black dots. Then, the figure plots the lower bound (white dots) and upper bound (gray dots) estimates of what the Africare respondents' cumulative income distribution *would have been* had there been no program disruption. The horizontal bars at each dot show the 95% confidence interval for these counterfactual predictions (refer to the table caption for more details). The figure demonstrates clearly just how large are the differences at the lower end of the income distribution. The gray area of the plot shows the region of the income distribution that is below the \$1.25/day (at purchasing power parity) poverty line. Poverty incidence among the Africare respondents is shown to be about 60% (the point where the black line crosses from the gray into the white region). The counterfactual distributions show that this would be an estiamted 20 percentage points (lower bound estimate) to 40 percentage points lower were there no disruption. We see that this difference is not sensitive to the precise location of the poverty line, as we estimate large and statistically significant differences in the income distributions for a wide range of income values between the 20th and 80 percentiles. A logistic regression of poverty incidence (not shown) estimates precisely the same effect with a p-value (two-sided) of .02.

8.2 Livelihood

We used a multinomial regression to study effects on livelihood, with the outcome categories being (1) no occupation, (2) agricultural occupation, (3) non-agricultural, skilled sector occupation (including returning to school, professional position, or skilled labor, such as automotive repair, electrical technician, etc.) and (4) non-agricultural, unskilled sector occupation (including security guard, manual labor, etc.). Agricultural occupation was set as the baseline category because it is, in many ways, the default occupation. The results are displayed in 10. Unlike other estimates, these effect estimates jump around a bit over the adjustment methods, due most likely to the rarity of values other than “agriculture” for the outcome variable. If we focus on our primary effect estimates (iva and ivb), we find a large effect on the likelihood of being in the skilled sector versus agriculture, with a p-value (two-sided) of .06. The magnitude of the point estimate is very large, but rather imprecise. While the sign of the effect is the same across the models, the magnitude varies substantially, suggesting that the identification may be rather fragile. This is to-be-expected with the multinomial logit regression, given that it is attempting to estimate dozens of parameters on only a moderately sized dataset.

Figure 2 displays the implied effects. The right panel shows the actual distribution of Africare respondents, as well as the model-predicted in-sample distribution (black dots). Then, lower bound (white dots) and upper bound (gray dots) counterfactual distribution are overlaid. We something on the order of a 10 to 20 percentage point increase in the incidence of realizing an occupation in the skilled sector. To unpack what was going on, we examined the source data, which included more details on respondents' occupations. We found that the effect comes almost entirely from differences in proportions of ex-combatants in *skilled labor* occupations rather than differences in proportions of ex-combatants resuming their education or achieving professional positions. In fact, across the imputed unmatched and matched samples, there are *no* ex-combatants from the Africare region engaged in skilled labor, whereas in the non-Africare region, the estimated rate is from 5% to 11%.

As a robustness check, we used rare events logit (King and Zeng, 2001) to re-estimate the coefficient on the Africare indicator on skilled sector occupation versus farming.[26] The validity of this robustness check requires that the IIA property (Train, 1993:18-24) holds and that the model properly specifies the structural relationships. IIA tests did not point conclusively in one way or the other, and the latter assumption is untestable.[27] We also encountered a complete separation problem when trying to estimate the covariate adjusted rare events logit on the matched sample, and so we had to augment the each of the matched datasets with four generated observations that fill in empty cells in the cross-classification table.[28] Our primary estimates (b.iva and b.ivb from Table 7) from the rare events logit were coefficient (standard error) values of -.90 (.79) and -1.69 (1.49), corresponding to a p-value (two-sided) of .26 for each. Thus, the point estimates are in the same direction, but about half the magnitude of the estimates from the multinomial logit regression, and the p-values are quite large. This contributes to our sense that while the direction of the effect from the parameter-heavy multinomial logit is probably correct, the point estimate is rather fragile. Substantially more data would be required for a more robust estimate of the livelihood effects.

[26] Detailed results from these tests are available from the authors.

[27] IIA violations are not a concern for the multinomial logit models estimated above, because the estimation routine computes separate intercepts for each choice, and these can account for many forms of IIA violation (Train, 1993:18-24). We do need IIA for the separate logistic regressions to be a valid way to check robustness, however.

[28] Regularized regression would be most preferable in this situation (Zorn, 2005; Gelman et al, 2008), but no method is currently programmed to combine these features with the rare events logit. Thus, we used data augmentation, which is known to introduce some bias (Heinze and Schemper, 2002), but the bias should be small in this case, and the method is more appealing than the alternatives of excluding these variables from the specification or analyzing subgroups.

8.3 Subjective assessment of economic well-being

The survey asked respondents whether they thought their current economic situation was "dangerously bad", "bad", "good", or "very good." Because of the sparseness of responses in either of the extreme categories, we used these responses to create an indicator variable taking the value 0 for "dangerously bad" or "bad", and 1 for "good" or "very good." We fit a linear probability model to these responses, and found no substantial effect of the program. The coefficient estimates are close to 0 and the p-values (two-sided) are .53 for our primary effect estimates.

9 Effects on political reintegration

9.1 Satisfaction with the peace accords

9.2 Respect for rule of law

10 Discussion

We will discuss the results here.

11 Conclusion

We will put a conclusion here.

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Table 1: **Estimated Sizes of Armed Forces as of January 2004**

Name of Force	Estimated Size
Forces Armées Burundaises (national army)	45,000
CNDD-FDD I (Nkurunziza faction)	25,000
CNDD-FDD II (Ndayikengurukiye faction)	3,000
FNL-PALIPEHUTU I (Rwasa faction)*	3,000
CNDD (Nyangoma faction)	1,000
FNL-PALIPEHUTU II (Mugabarabona faction)	1,000
FROLINA (Kalumba faction)	1,000
PALIPEHUTU (Karatasi faction)	1,000
Total	80,000

Source: World Bank (2004), p. 17.
**Not a party to the peace process until September 2008.*

Table 2: **Military Status of Surviving Civil War Combatants as of June 2007**

Status	Number	Of which from rebel forces
In Forces de Defense Nationale (new national army)	28,390	approx. 9,000
In Police Nationale	approx. 20,000	approx. 8,000
Demobilized	23,185	approx. 14,000*
In FNL-PALIPEHUTU (active factions)	approx. 6,500**	approx. 6,500**
Totals	approx. 78,075	approx. 37,500

Sources: MDRP (2007), Nindorera (2007), World Bank (2009).
** Approximately 9,000 out of an initial 14,000 demobilized in a first phase (prior to 2004) were from the rebel groups. Of the remaining approximately 9,000 that were demobilized (to generate the 23,185 total), personal communications from program staff suggest that about 5,000 of them were from the rebel groups, although this has not been documented to our knowledge.*
***This figure is based on the demobilization targets in World Bank (2009), which were estimated some time after June 2007. There has been some controversy as to whether this overstated the number of "true" FNL-PALIPEHUTU fighters that remained into 2008-9 by a few thousand. Without any other evidence, this is our best estimate, but it may be biased slightly upward.*

Table 3: **Program Access in Africare and Non-Africare Regions, Before and After NGO transition in Fall 2006 and as of the Time of Fieldwork in July 2007**

Region	Pre-transition cases completed by 12/06	Remaining caseload...	...% subject to disruption	Overall disruption rate
Africare provinces	1,982	2,257	100%	.53
Non-Africare provinces	3,213	5,925	0%	0

Sources: World Bank and Multi-country Demobilization and Reintegration Program (MDRP), Quarterly Reports, October-December 2006 and April-June 2007, and Africare Annual Report, 2008. Africare provinces include Cankuzo, Gitega, Karuzi, Muramvya, Mwaro, and Ruyigi. The table refers only to ex-combatants who are (1) male, constituting 97.5% of ex-combatants in the reintegration program, and (2) registered to receive program benefits outside Bujumbura.

Table 4: **Balance Statistics, Sets 1 and 2**

Variable		**Mean treated**	**Mean control**	***t*-test *p*-value**	**K-S test *p*-value**	**Var. ratio (Tr/Co)**	Percent Balance Improvement. **Mean Diff.**	eQQ Mean
Set 1								
Age	Before Matching	34.373	33.310	0.338	0.198	1.196	-34.346	-34.982
	After Matching	34.372	32.945	0.307	0.66	1.416		
Hutu	Before Matching	0.955	0.958	0.888	—	1.081	100.00	NA
	After Matching	0.955	0.955	1.0	—	0.994		
Father's Education	Before Matching	0.282	0.322	0.442	—	0.932	54.569	100.00
	After Matching	0.282	0.264	0.792	—	1.037		
Father in Agriculture, Prewar	Before Matching	0.809	0.678	0.006	—	0.711	93.057	78.431
	After Matching	0.809	0.818	0.880	—	1.032		
Prewar Education	Before Matching	4.746	5.096	0.237	0.414	0.846	71.456	-5.335
	After Matching	4.746	4.646	0.778	0.166	1.398		
Prewar Wealth	Before Matching	-0.059	0.093	0.026	0.004	1.180	78.265	12.754
	After Matching	-0.058	-0.092	0.725	0.226	1.019		
Prewar Relative deprivation	Before Matching	0.364	414	0.365	–	0.959	45.625	-94.118
	After Matching	0.364	0.391	0.718	–	0.966		
Father Alive	Before Matching	0.727	0.747	0.694	–	1.055	8.421	100.00
	After Matching	0.727	0.745	0.790	–	1.039		
Socializes most with Family	Before Matching	0.355	0.280	0.164	–	1.142	87.855	59.559
	After Matching	0.355	0.364	0.903	–	0.983		
Prewar Urban Resident	Before Matching	0.064	0.176	0.001	–	0.413	100.00	73.039
	After Matching	0.064	0.064	1.0	–	0.994		
Postwar Urban Resident	Before Matching	0.073	0.195	0.001	–	0.431	92.589	87.557
	After Matching	0.073	0.064	0.815	–	1.125		
Unit Death Rate	Before Matching	0.086	0.124	0.005	0.086	0.558	84.857	63.827
	After Matching	0.086	0.080	0.692	0.772	1.570		
Years in Faction	Before Matching	9.6	10.697	0.003	0.002	0.621	82.602	19.118
	After Matching	9.6	9.791	0.621	0.120	1.754		
Community Violence	Before Matching	5.499	10.306	0.000	0.000	0.443	95.717	69.892
	After Matching	5.499	5.705	0.881	0.05	1.082		
Family Death Rate	Before Matching	0.145	0.175	0.125	0.136	1.055	88.576	42.273
	After Matching	0.145	0.142	0.891	0.718	1.232		
Unit had Written Rules	Before Matching	0.745	0.801	0.256	–	1.196	83.564	46.078
	After Matching	0.745	0.755	0.892	–	1.019		
Non-CNDD Faction Member	Before Matching	0.091	0.222	0.001	–	0.481	100.00	100.00
	After Matching	0.091	0.091	1.00	–	0.994	0.001	
Demobilization Date	Before Matching	200494	200499	0.346	0.694	0.701	56.899	37.059
	After Matching	200494	200496	0.738	0.882	1.182		
propensity score	Before Matching	0.399	0.253	0.000	0.000	1.206	95.049	70.041
	After Matching	0.399	0.392	0.766	0.03	1.191		
Set 2								
Age	Before Matching	34.373	33.310	0.338	0.186	1.196	33.255	-38.03
	After Matching	34.373	33.664	0.596	0.374	1.492		
Hutu	Before Matching	0.955	0.958	0.888	—	1.081	100.00	NA
	After Matching	0.955	0.955	1.0	—	0.996		
Father's Education	Before Matching	0.309	0.318	0.866	—	0.990	-1.953	100.00
	After Matching	0.309	0.300	0.895	—	1.013		
Father in Agriculture, Prewar	Before Matching	0.818	0.686	0.005	0.694	—	86.263	80.70
	After Matching	0.818	0.836	0.748	—	1.083		
Prewar Education	Before Matching	4.746	5.096	0.237	0.424	0.845	40.316	-24.54
	After Matching	4.746	4.955	0.521	0.336	1.781		
Prewar Wealth	Before Matching	-0.051	0.096	0.031	0.004	1.182	80.286	69.46
	After Matching	-0.051	-0.022	0.751	0.902	0.982		
Prewar Relative deprivation	Before Matching	0.364	0.414	0.365	—	0.959	63.750	-102.63
	After Matching	0.364	0.382	0.976	–	0.802		
Father Alive	Before Matching	0.727	0.747	0.694	—	1.055	100.00	27.63
	After Matching	0.727	0.727	1.0	–	0.996		
Socializes most with Family	Before Matching	0.355	0.276	0.143	—	1.152	76.892	51.75
	After Matching	0.355	0.336	0.799	–	1.021		
Prewar Urban Resident	Before Matching	0.064	0.176	0.001	—	0.413	100.00	87.94
	After Matching	0.064	0.064	1.0	–	0.996		
Postwar Urban Resident	Before Matching	0.073	0.195	0.001	—	0.431	92.589	88.87
	After Matching	0.073	0.064	0.809	–	1.127		
Unit Death Rate	Before Matching	0.093	0.117	0.086	—	0.648	97.823	38.56
	After Matching	0.093	0.093	0.973	0.588	1.401		
Years in Faction	Before Matching	9.6	10.709	0.002	0.002	0.619	87.702	17.13
	After Matching	9.600	9.736	0.720	0.038	1.652		
Community Violence	Before Matching	5.499	10.203	0.000	0.000	0.444	99.663	75.62
	After Matching	5.499	5.515	0.990	0.138	1.123		
Family Death Rate	Before Matching	0.145	0.175	0.125	0.158	1.055	95.601	22.58
	After Matching	0.145	0.146	0.956	0.446	1.287		
Unit had Written Rules	Before Matching	0.736	0.805	0.164	—	1.241	46.708	17.29
	After Matching	0.736	0.773	0.572	–	1.101		
Non-CNDD Faction Member	Before Matching	0.091	0.222	0.001	—	0.481	93.077	68.98
	After Matching	0.091	0.1	0.838	–	0.915		
Demobilization Date	Before Matching	200494	200499	0.384	0.736	0.713	88.606	38.17
	After Matching	200494	200494	0.930	0.708	1.376		
propensity score	Before Matching	0.394	0.255	0.000	0.000	1.175	94.355	70.96
	After Matching	0.394	0.386	0.736	0.088	1.135		

Table 5: **Balance Statistics, Sets 3 and 4**

Variable		**Mean treated**	**Mean control**	***t*-test *p*-value**	**K-S test *p*-value**	**Var. ratio (Tr/Co)**	Percent Balance Improvement **Mean Diff.**	**eQQ Mean**
Set 3								
Age	Before Matching	34.373	33.310	0.338	0.162	1.196	28.120	22.02
	After Matching	34.373	33.609	0.583	0.532	1.365		
Hutu	Before Matching	0.955	0.958	0.888	—	1.081	100.00	NA
	After Matching	0.955	0.955	1.0	—	0.995		
Father's Education	Before Matching	0.291	0.326	0.507	—	0.944	47.695	22.54
	After Matching	0.291	0.273	0.792	—	1.035		
Father in Agriculture Prewar	Before Matching	0.818	0.686	0.005	—	0.694	93.132	89.67
	After Matching	0.818	0.827	0.876	—	1.036		
Prewar Education	Before Matching	4.746	5.096	0.237	0.386	0.846	97.405	20.73
	After Matching	4.746	4.736	0.980	0.428	1.289		
Prewar Wealth	Before Matching	-0.051	0.094	0.034	0.016	1.180	94.212	19.56
	After Matching	-0.051	-0.059	0.927	0.420	1.047		
Prewar Relative deprivation	Before Matching	0.363	0.414	0.365	—	0.959	100.00	69.01
	After Matching	0.363	0.363	1.0	–	0.995		
Father Alive	Before Matching	0.727	0.747	0.694	—	1.055	0.08.421	22.54
	After Matching	0.727	0.745	0.787	–	1.040		
Socializes most with Family	Before Matching	0.355	0.280	0.164	—	1.142	87.855	100.00
	After Matching	0.355	0.345	0.901	–	1.007		
Prewar Urban Resident	Before Matching	0.064	0.176	0.001	—	0.413	91.927	61.27
	After Matching	0.064	0.073	0.815	–	0.879		
Postwar Urban Resident	Before Matching	0.073	0.195	0.001	—	0.431	92.589	88.08
	After Matching	0.073	0.064	0.813	–	1.126		
Unit Death Rate	Before Matching	0.089	0.122	0.013	0.156	0.598	87.921	60.33
	After Matching	0.089	0.085	0.782	0.588	1.525		
Years in Faction	Before Matching	9.6	10.724	0.002	0.002	0.615	97.722	47.94
	After Matching	9.6	9.682	0.833	0.240	1.631		
Community Violence	Before Matching	5.499	10.192	0.000	0.000	0.444	99.398	73.18
	After Matching	5.499	5.470	0.983	0.012	1.113		
Family Death Rate	Before Matching	0.145	0.175	0.125	0.140	1.055	74.674	35.60
	After Matching	0.145	0.138	0.750	0.702	1.414		
Unit had Written Rules	Before Matching	0.736	0.782	0.361	—	1.143	100.00	100.00
	After Matching	0.736	0.736	1.0.	–	0.995		
Non-CNDD Faction Member	Before Matching	0.091	0.222	0.001	—	0.481	100.00	88.93
	After Matching	0.091	0.091	1.00	–	0.995		
Demobilization Date	Before Matching	200494	200499	0.307	0.692	0.710	42.207	54.01
	After Matching	200494	200497	0.619	0.756	1.251		
propensity score	Before Matching	0.395	0.255	0.000	0.000	1.243	98.488	75.11
	After Matching	0.395	0.393	0.927	0.270	1.1333		
Set 4								
Age	Before Matching	34.373	33.310	0.338	0.164	1.196	50.37	-11.24
	After Matching	34.373	33.845	0.705	0.566	1.300		
Hutu	Before Matching	0.955	0.958	0.888	—	1.081	100.00	NA
	After Matching	0.955	0.955	1.0	—	0.996		
Father's Education	Before Matching	0.309	0.318	0.866	—	0.990	100.00	100.00
	After Matching	0.309	0.309	1.0	—	.995		
Father is in Agriculture Prewar	Before Matching	0.800	0.689	0.018	—	0.747	84.08	65.70
	After Matching	0.800	0.818	0.759	—	1.071		
Prewar Education	Before Matching	4.746	5.096	0.237	0.392	0.846	45.51	-20.99
	After Matching	4.746	4.936	0.586	0.410	1.357		
Prewar Wealth	Before Matching	-0.051	0.092	0.037	0.010	1.183	75.78	63.48
	After Matching	-0.051	-0.017	0.701	0.974	1.083		
Prewar Relative deprivation	Before Matching	0.345	0.414	0.214	—	0.937	86.70	15.06
	After Matching	0.345	0.355	0.900	–	0.984		
Father Alive	Before Matching	0.727	0.751	0.639	—	1.066	61.62	25.68
	After Matching	0.727	0.736	0.892	–	1.017		
Socializes most with Family	Before Matching	0.355	0.276	0.143	—	1.152	100.00	17.42
	After Matching	0.355	0.355	1.0	–	0.996		
Prewar Urban Resident	Before Matching	0.064	0.176	0.001	—	0.413	100.00	87.61
	After Matching	0.064	0.064	1.0	–	0.996		
Postwar Urban Resident	Before Matching	0.073	0.195	0.001	—	0.431	92.59	88.56
	After Matching	0.073	0.064	0.811	–	1.127		
Unit Death Rate	Before Matching	0.090	0.123	0.019	0.112	0.665	94.83	49.01
	After Matching	0.090	0.088	0.911	0.576	1.564		
Years in Faction	Before Matching	9.6	10.751	0.001	0.000	0.620	87.36	17.67
	After Matching	9.6	9.746	0.695	0.018	1.898		
Community Violence	Before Matching	5.499	10.185	0.000	0.000	0.444	98.64	70.87
	After Matching	5.499	5.435	0.962	0.088	1.125		
Family Death Rate	Before Matching	0.145	0.175	0.125	0.158	1.055	93.77	10.89
	After Matching	0.145	0.143	0.939	0.326	1.293		
Unit had Written Rules	Before Matching	0.745	0.793	0.329	—	1.163	42.76	10.81
	After Matching	0.745	0.773	0.672	–	1.076		
Non-CNDD Faction Member	Before Matching	0.091	0.222	0.001	—	0.481	86.15	100.00
	After Matching	0.091	0.073	0.658	–	1.220	0.002	
Demobilization Date	Before Matching	200494	200499	0.349	0.848	0.681	22.05	-38.05
	After Matching	200494	200498	0.489	0.552	1.862		
propensity score	Before Matching	0.396	0.255	0.000	0.000	1.211	95.17	64.96
	After Matching	0.396	0.389	0.763	0.030	1.376		

Table 6: **Balance Statistics, Set 5**

Variable		Mean treated	Mean control	t-test p-value	K-S test p-value	Var. ratio (Tr/Co)	Percent Balance Improvement	
							Mean Diff.	eQQ Mean
Age	Before Matching	34.373	33.310	0.338	0.194	1.196	-20.66	-2.649
	After Matching	34.373	33.091	0.363	0.654	1.405		
Hutu	Before Matching	0.955	0.958	0.888	—	1.081	-174.74	NA
	After Matching	0.955	0.964	0.767	—	1.231		
Father's Education	Before Matching	0.309	0.322	0.810	—	0.984	28.69	-233.333
	After Matching	0.309	0.300	0.900	—	1.011		
Father in Agriculture Prewar	Before Matching	0.809	0.682	0.008	—	0.716	92.85	76.190
	After Matching	0.809	0.818	0.882	—	1.032		
Prewar Education	Before Matching	4.746	5.096	0.237	0.382	0.846	94.81	-4.651
	After Matching	4.746	4.764	0.959	0.596	1.442		
Prewar Wealth	Before Matching	-0.051	0.096	0.031	0.004	1.182	51.05	29.996
	After Matching	-0.051	-0.123	0.450	0.424	0.994		
Prewar Relative deprivation	Before Matching	0.354	0.418	0.254	—	0.946	42.35	4.762
	After Matching	0.354	0.391	0.633	–	0.955		
Father Alive	Before Matching	0.727	0.751	0.639	—	1.066	61.62	-66.667
	After Matching	0.727	0.736	0.896	–	1.016		
Socializes most with Family	Before Matching	0.355	0.276	0.143	—	1.512	88.45	62.963
	After Matching	0.355	0.345	0.903	–	1.006		
Prewar Urban Resident	Before Matching	0.064	0.176	0.001	—	0.413	100.00	58.333
	After Matching	0.064	0.064	1.0	–	0.994		
Postwar Urban Resident	Before Matching	0.073	0.195	0.001	—	0.431	92.59	74.359
	After Matching	0.073	0.064	0.817	–	1.125		
Unit Death Rate	Before Matching	0.084	0.123	0.004	0.034	0.606	94.68	60.365
	After Matching	0.084	0.082	0.888	0.606	1.724		
Years in Faction	Before Matching	9.6	10.697	0.002	0.002	0.614	99.17	47.154
	After Matching	9.6	9.609	0.982	0.336	1.652		
Community Violence	Before Matching	5.499	10.180	0.000	0.000	0.0444	97.74	70.338
	After Matching	5.499	5.605	0.939	0.028	1.108		
Family Death Rate	Before Matching	0.145	0.175	0.125	0.132	1.055	71.99	31.729
	After Matching	0.145	0.137	0.740	0.684	1.228		
Unit had Written Rules	Before Matching	0.745	0.801	0.256	—	1.196	50.69	16.667
	After Matching	0.745	0.773	0.683	–	1.074		
Non-CNDD Faction Member	Before Matching	0.091	0.222	0.001	—	0.481	93.08	88.095
	After Matching	0.091	0.082	0.836	–	1.093		
Demobilization Date	Before Matching	200494	200499	0.344	0.756	0.723	58.90	-34.519
	After Matching	200494	200496	0.744	0.630	1.337		
propensity score	Before Matching	0.400	0.253	0.000	0.000	1.222	98.14	70.716
	After Matching	0.400	0.397	0.912	0.162	1.195		

Table 7: **Reference Table for Effect Estimates**

Label Label	Covariate adjustment*	Exposure heterogeneity adjustment	Imputation-Completed Dataset Sample Sizes
b.ia	None	None	371, 371, 371, 371, 371
b.ib	None	Adjust with $\phi = .53$	371, 371, 371, 371, 371
b.iia	Regression adjustment*	None	371, 371, 371, 371, 371
b.iib	Regression adjustment*	Adjust with $\phi = .53$	371, 371, 371, 371, 371
b.iiia	Matching	None	178,186,181,184,176
b.iiib	Matching	Adjust with $\phi = .53$	178,186,181,184,176
b.iva	Matching + weighted regression adjustment*	None	178,186,181,184,176
b.ivb	Matching + weighted regression adjustment*	Adjust with $\phi = .53$	178,186,181,184,176

**When regression adjustment was applied, the specification of the regressions was always, $y_i = g(\beta_0 + \beta_1 \phi \text{Africare indicator}_i + \beta_2 x_2 + \ldots + \beta x_K)$, where $g()$ is the relevant link function, $\phi = 1$ for estimates with no exposure heterogeneity adjustment and $\phi = .53$ for estimates with such adjustment, and $x_1, \ldots, x_K$ are the covariates described in the section on covariates and matching. $K = 18$ for the unmatched data and $K = 19$ for the matched data (the extra covariate is the estimated propensity score). When no regression adjustment was applied, the effect estimates were from regressions of the outcome variable on ϕAfricare indicator$_i$.*

Table 8: **Estimates from OLS Regressions on Log(Income + 1)**

	b.ia	se.ia	p-val	b.iiia	se.iiia	p-val
africare	-0.68	0.32	0.03	-0.66	0.43	0.13
constant	9.35	0.20	0.00	9.32	0.35	0.00
	b.ib	se.ib	p-val	b.iiib	se.iiib	p-val
.53∗africare	-1.28	0.60	0.03	-1.24	0.82	0.13
constant	9.35	0.20	0.00	9.32	0.35	0.00
	b.iia	se.iia	p-val	b.iva	se.iva	p-val
africare	-0.54	0.37	0.14	-0.62	0.45	0.16
age	-0.01	0.02	0.70	-0.03	0.04	0.40
hutu	0.48	1.14	0.67	2.94	2.05	0.15
father's ed.	-0.36	0.31	0.25	-0.35	0.62	0.57
father agr.	-0.33	0.32	0.30	-0.99	1.05	0.35
prewar SES	0.04	0.25	0.86	0.58	0.72	0.42
pre-war ed.	0.14	0.06	0.02	0.20	0.12	0.11
pre-war rel. dep.	-0.06	0.24	0.81	0.63	1.03	0.54
father alive	-0.11	0.35	0.76	0.29	0.66	0.66
pre-war family	-0.45	0.36	0.22	-0.35	0.51	0.49
pre-war urb.	0.49	0.55	0.37	0.79	1.85	0.67
post-war urb.	-0.66	0.55	0.24	0.01	1.54	0.99
unit dth rate	-0.53	1.77	0.76	4.93	3.64	0.18
faction yrs	0.04	0.04	0.39	0.07	0.16	0.69
commune violence	0.00	0.02	0.91	-0.02	0.07	0.74
family dth rate	0.42	0.88	0.63	1.10	2.16	0.62
unit rules	-0.06	0.35	0.88	0.03	0.58	0.96
non-cndd	0.26	0.40	0.52	0.42	1.46	0.78
demob. date	-0.23	0.18	0.20	-0.30	0.22	0.17
pscore				5.49	7.06	0.44
constant	8.65	1.32	0.00	3.90	5.85	0.51
	b.iib	se.iib	p-val	b.ivb	se.ivb	p-val
.53∗africare	-1.01	0.69	0.14	-1.18	0.84	0.16

Standard errors account for clustering at the commune level. Refer to Table 7 for explanations of the different estimates and sample sizes. Coefficients on covariates for b.iib and b.ivb are not reported, as by construction, they are exactly the same as for b.iia and b.iva, respectively. The estimates account for multiple imputation.

Table 9: **Africare and ϕAfricare Coefficient Estimates from Quantile Regressions on Log(Income+1)**

	b.ia	se.ia	p-val	b.iiia	se.iiia	p-val
Decile 1	-1.61	2.87	0.58	-1.24	3.62	0.73
Decile 2	-0.69	0.24	0.00	-0.55	0.61	0.37
Decile 3	-0.69	0.17	0.00	-0.76	0.32	0.02
Decile 4	-0.41	0.18	0.02	-0.41	0.22	0.07
Decile 5	-0.67	0.15	0.00	-0.62	0.31	0.05
Decile 6	-0.69	0.15	0.00	-0.55	0.24	0.02
Decile 7	-0.41	0.16	0.01	-0.41	0.22	0.06
Decile 8	-0.51	0.19	0.01	-0.16	0.36	0.66
Decile 9	-0.27	0.19	0.16	-0.19	0.20	0.36
	b.ib	se.ib	p-val	b.iiib	se.iiib	p-val
Decile 1	-3.04	5.42	0.58	-2.34	6.83	0.73
Decile 2	-1.31	0.46	0.00	-1.05	1.16	0.37
Decile 3	-1.31	0.32	0.00	-1.43	0.60	0.02
Decile 4	-0.76	0.34	0.02	-0.76	0.42	0.07
Decile 5	-1.27	0.29	0.00	-1.16	0.58	0.05
Decile 6	-1.31	0.28	0.00	-1.03	0.45	0.02
Decile 7	-0.76	0.30	0.01	-0.76	0.41	0.06
Decile 8	-0.96	0.35	0.01	-0.30	0.68	0.66
Decile 9	-0.51	0.36	0.16	-0.35	0.38	0.36
	b.iia	se.iia	p-val	b.iva	se.iva	p-val
Decile 1	-1.37	1.78	0.44	-0.52	2.01	0.80
Decile 2	-0.53	0.24	0.03	-0.96	0.51	0.06
Decile 3	-0.37	0.17	0.03	-0.79	0.27	0.00
Decile 4	-0.48	0.17	0.01	-0.59	0.24	0.02
Decile 5	-0.47	0.15	0.00	-0.36	0.22	0.10
Decile 6	-0.41	0.14	0.00	-0.42	0.24	0.09
Decile 7	-0.47	0.13	0.00	-0.51	0.18	0.01
Decile 8	-0.38	0.15	0.01	-0.43	0.16	0.01
Decile 9	-0.36	0.13	0.00	-0.21	0.18	0.24
	b.iib	se.iib	p-val	b.ivb	se.ivb	p-val
Decile 1	-2.58	3.35	0.44	-0.98	3.79	0.80
Decile 2	-1.00	0.46	0.03	-1.82	0.97	0.06
Decile 3	-0.70	0.31	0.03	-1.48	0.52	0.00
Decile 4	-0.91	0.31	0.01	-1.12	0.46	0.02
Decile 5	-0.88	0.28	0.00	-0.68	0.41	0.10
Decile 6	-0.77	0.27	0.00	-0.79	0.45	0.09
Decile 7	-0.88	0.24	0.00	-0.96	0.34	0.01
Decile 8	-0.72	0.29	0.01	-0.80	0.29	0.01
Decile 9	-0.68	0.24	0.00	-0.40	0.33	0.24

The table shows coefficient estimates for the Africare indicator from quantile regressions on deciles 1 through 9 of the log-income distribution. Refer to Table 7 for an explanation of the different estimates and sample sizes. Standard errors were computed from inverted rank-test confidence intervals robust to non-iid errors (Koenker, DATE). Coefficient estimates on the covariates in specifications iia,iib,iva, and ivb are not displayed for reasons of space. The estimates account for multiple imputation.

Table 10: **Africare and ϕAfricare Coefficient Estimates from Multinomial Logistic Regressions on Livelihood (viz., No Occupation, Agriculture, Skilled Sector, or Unskilled Sector)**

Comparison	b.ia	se.ia	p-val	b.iiia	se.iiia	p-val
No occ./Agr.	-0.53	0.43	0.22	0.91	1.10	0.42
Skilled/Agr.	-1.02	0.47	0.03	-0.78	0.65	0.23
Non-skilled/Agr.	-0.34	0.47	0.47	0.32	0.72	0.66
	b.ib	se.ib	p-val	b.iiib	se.iiib	p-val
No occ./Agr.	-1.00	0.82	0.22	1.72	2.08	0.42
Skilled/Agr.	-1.92	0.90	0.03	-1.46	1.22	0.23
Non-skilled/Agr.	-0.64	0.89	0.47	0.60	1.35	0.66
	b.iia	se.iia	p-val	b.iva	se.iva	p-val
No occ./Agr.	0.02	0.46	0.96	1.26	2.07	0.55
Skilled/Agr.	-0.54	0.52	0.30	-2.01	1.04	0.06
Non-skilled/Agr.	0.01	0.37	0.98	0.20	0.81	0.81
	b.iib	se.iib	p-val	b.ivb	se.ivb	p-val
No occ./Agr.	0.05	0.86	0.96	2.37	3.91	0.55
Skilled/Agr.	-1.01	0.97	0.30	-3.79	1.96	0.06
Non-skilled/Agr.	0.01	0.70	0.98	0.38	1.53	0.81

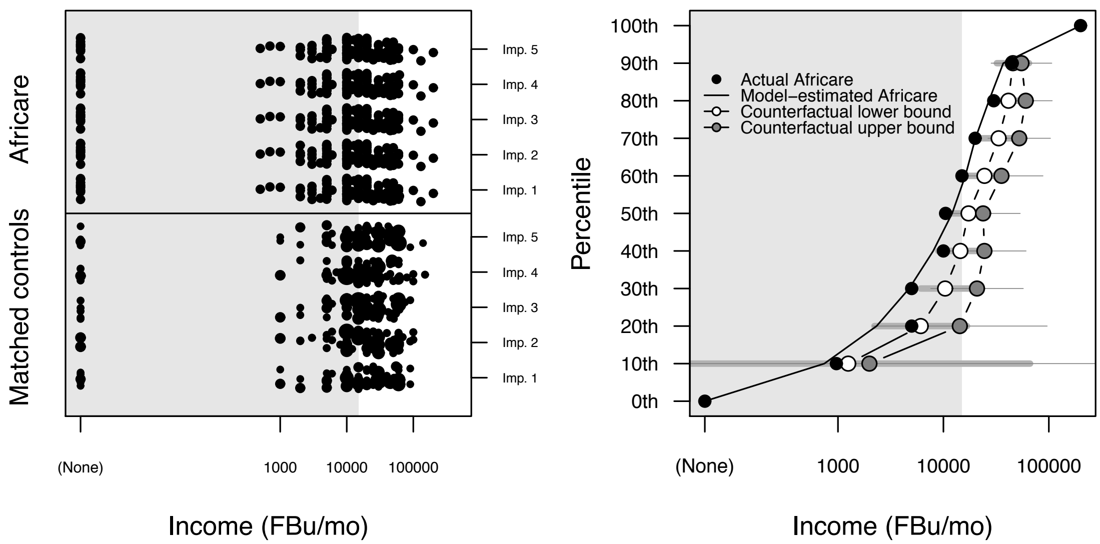
Standard errors for estimates ia, ib, iiia, and iiib account for clustering at the commune level. For iia, iib, iva, and ivb, the multiple imputation estimation routine was not able to incorporate the cluster adjustment, and so the uncorrected standard errors are displayed. To check to see if there were problems with this, we estimated the cluster-corrected models separately. The cluster-adjusted standard errors on the Africare coefficients matched the uncorrected standard errors at the first significant digit in nearly all cases, and the clustered standard errors were often a bit smaller than the uncorrected ones (indicative of some form of negative residual correlation). No inferences would change as a result of the differences. Refer to Table 7 for explanations of the different estimates and sample sizes. Coefficient estimates for covariates in estimates iia, iib, iva, and ivb are not displayed for reasons of space. The estimates account for multiple imputation.

Table 11: **Estimates from Linear Probability Model (OLS) Regressions on Subjective Economic Well-being="Good" or "Very Good"**

	b.ia	se.ia	p-val	b.iiia	se.iiia	p-val
africare	0.02	0.05	0.68	-0.05	0.09	0.64
constant	0.31	0.03	0.00	0.37	0.09	0.00
	b.ib	se.ib	p-val	b.iiib	se.iiib	p-val
.53∗africare	0.04	0.10	0.68	-0.09	0.18	0.64
constant	0.31	0.03	0.00	0.37	0.09	0.00
	b.iia	se.iia	p-val	b.iva	se.iva	p-val
africare	0.00	0.05	0.95	-0.05	0.08	0.53
age	0.00	0.00	0.27	-0.01	0.01	0.42
hutu	-0.12	0.12	0.33	0.36	0.26	0.17
father's ed.	0.05	0.06	0.45	-0.17	0.11	0.12
father agr.	-0.02	0.06	0.72	-0.34	0.21	0.11
prewar SES	0.11	0.04	0.01	0.43	0.14	0.00
pre-war ed.	0.00	0.01	0.85	0.00	0.02	0.94
pre-war rel. dep.	0.01	0.06	0.86	0.30	0.17	0.08
father alive	-0.01	0.05	0.92	0.02	0.13	0.86
pre-war family	0.02	0.05	0.62	-0.02	0.09	0.81
pre-war urb.	0.07	0.06	0.27	-0.02	0.18	0.92
post-war urb.	-0.19	0.07	0.00	0.09	0.30	0.77
unit dth rate	0.16	0.22	0.46	1.28	0.84	0.16
faction yrs	-0.01	0.01	0.35	0.01	0.02	0.75
commune violence	0.00	0.00	0.78	0.01	0.01	0.18
family dth rate	-0.11	0.16	0.48	0.07	0.35	0.85
unit rules	-0.06	0.06	0.30	0.04	0.13	0.74
non-cndd	-0.06	0.05	0.25	0.33	0.27	0.22
demob. date	-0.04	0.02	0.01	-0.02	0.04	0.70
pscore				1.81	1.34	0.18
constant	0.44	0.19	0.02	-0.57	0.91	0.53
	b.iib	se.iib	p-val	b.ivab	se.ivb	p-val
.53∗africare	-0.01	0.10	0.95	-0.10	0.16	0.53

Standard errors account for clustering at the commune level. Refer to Table 7 for explanations of the different estimates and sample sizes. Coefficients on covariates for b.iib and b.ivb are not reported, as by construction, they are exactly the same as for b.iia and b.iva, respectively. The estimates account for multiple imputation.

Figure 1: **Differences in log-income distributions and estimated effects on the cumulative log-income distribution**



The figure on the left shows the distribution of income (on the natural log scale) for Africare and matched control (non-Africare) observations in the matched dataset. Each point is a single observation, and the size of the points for the matched controls is proportional to the weight assigned to that observation after matching. Distributions are shown for each of the 5 imputation-complete datasets. The figure on the right shows the cumulative log-income distribution for Africare respondents, and then predicted counter-factual distributions. The lower and upper bound shown correspond to estimates b.iva and b.ivb from Table 9. The thick and thin horizontal gray bars show the 95% confidence intervals for the lower bound and upper bound counter-factual distributions, respectively. In both figures, the gray zone corresponds to points below the 15000FBu/month ($1/day at PPP) poverty line.

Figure 2: **Livelihood outcome distributions and estimated effects**

The left graph shows the livelihood outcome distribution for the matched controls. The right graph shows estimated counterfactual outcomes imposed over the outcome distribution for the Africare-region ex-combatants. The estimated effect is the difference between the modeled "actual" outcomes (the solid black dots) and the estimated counter-factual outcomes. The white dot shows the lower-bound estimate, from b.iva, and the gray dot shows the upper bound estimate, from b.ivb. The thick and thin horizontal gray bars show the 95% confidence intervals for the lower bound and upper bound counter-factual estimates, respectively.