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Journal of Conflict Resolution published online 28 June 2012
DOI: 10.1177/0022002712448908

Journal of Conflict Resolution
00(0) 1-29

DOI: 10.1177/0022002712448908
http://jcr.sagepub.com

Reintegrating Rebels into Civilian Life: Quasi-Experimental Evidence from Burundi

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Abstract

Considerable resources are devoted to ex-combatant reintegration programs in current peace processes, but evidence on their effectiveness remains thin. We use original survey data to study an ex-combatant reintegration program implemented after Burundi's 1993-2004 civil war. Previous quantitative studies have found reintegration programs to be ineffective, but only ex-combatants who self-selected into programs were studied. We avoid such selection problems with a quasi-experimental design exploiting an exogenous bureaucratic failure. We find the program resulted in a 20 to 35 percentage point reduction in poverty incidence among ex-combatants and moderate improvement in livelihoods. But this economic boost does not seem to have caused political reintegration: while we find a modest increase in propensities to report civilian life as preferable to combatant life, we find no evidence that the program contributed to either more satisfaction with the peace process or a more positive disposition toward current government institutions.

Keywords

civil war, rebellion, post-conflict reconstruction, reintegration

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Are postconflict disarmament, demobilization, and reintegration (DDR) programs effective in achieving their programmatic aims of economic rehabilitation? Does such economic assistance make ex-combatant beneficiaries more likely to reintegrate politically and socially? We address these questions, exploiting quasi-experimental evidence from an ex-combatant reintegration program implemented in Burundi after the 1993–2004 civil war. We find that the DDR program in Burundi produced significant increases in incomes and a reduction in poverty levels among the program's recipients but that these economic gains did not translate into greater political and social integration. As such this is the first study of which we are aware that finds evidence for programmatic effects (albeit only economic effects) of a DDR program while explicitly addressing the nonrandom assignment of receipt of program benefits.

Postconflict DDR programs have become key features of most peace agreements that end civil wars. According to the 2006 Uppsala Peace Agreement data set, 36 percent of peace agreements struck in 1989–1999 period contained DDR provisions, while in 2000 through 2005, the percentage rose to 59 percent.[1] DDR programs are typically carried out alongside economic reconstruction, refugee repatriation, and democratization. All of these "peacebuilding" interventions aim to address the root causes of conflict and reduce the likelihood of another war (Boutros-Ghali 1992; Cousens, Kumar, and Wermester 2001; Paris 2004; Doyle and Sambanis 2006).

Disarmament and demobilization, the first two components of DDR, take place before the reintegration phase in order to create the security and trust necessary for implementing peace agreements (Walter 2001; Spear 2002; Fortna 2008) and starting reconstruction (Kumar 1997; Rubin 2003; Feil 2004). Following disarmament and demobilization, the goal is for ex-combatants to find a livelihood and submit to laws and norms that govern civilian society. The fear is that demobilized combatants may have difficulty finding a productive position in the legal civilian economy and may maintain an oppositional stance toward society and government. Such marginalization may increase the propensity to engage in crime or renewed violence. 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." Spear (2006) argues that among postconflict interventions, reintegration is the most directly "linked to establishing a lasting peace"—a conclusion echoed by Castillo's (2008) argument that ex-combatant reintegration is a " sine qua non" for consolidating peace.

By providing economic benefits, reintegration programs try to make civilian life more attractive to ex-combatants and thus reduce the risk of political disorder. The presumption is that the former combatants cannot or will not achieve productive civilian livelihoods automatically. This might be due to economic barriers, such as lack of access to productive resources or a soft labor market, as well as social factors, such as an oppositional disposition from the ex-combatants themselves or discrimination by others. Reintegration programs usually include (1) short-term measures such as cash assistance or in-kind material benefits to address immediate needs upon leaving armed groups and (2) longer-term measures such as vocational training, seed capital, and counseling, which aim to reintegrate former combatants into the social and economic structures of society (Colletta, Kostner, and Ingo 1996; Bryden and Hanggi 2005; Muggah 2009).

Despite the importance of reintegration programs, there are few attempts to measure their effectiveness. Some macro-level studies exist that evaluate reintegration programming by major international institutions like the United Nations and World Bank, but they usually focus on program management and outputs.[2] Several descriptive studies focus on practical challenges plaguing the implementation of reintegration programs,[3] problems with assumptions that underpin reintegration programs,[4] and detailed accounts of beneficiaries' experiences.[5] While useful, these studies do not measure the benefits of reintegration programs relative to their costs, including the costs of diverting resources from others in need and of apparently "rewarding" former combatants for engaging in violence (Muggah 2009).

Two recent studies have tried to measure the benefits of reintegration programs, using large scale surveys of ex-combatants. Humphreys and Weinstein (2007) investigate the effects of a UN-sponsored DDR program in Sierra Leone. They used propensity score matching on ex-combatants who did and did not enroll or complete the program. For both types of participation, these comparisons suggested no discernable effect on economic or political reintegration. Pugel (2009) conducted a similar study among ex-combatants in Liberia. Using regression analysis to control for background characteristics, Pugel found that those who had completed the UN reintegration program were significantly more likely to have a "livelihood-producing activity," although no significant effects were evident on poverty status or "spending patterns indicative of excess earning capacity." Pugel found no significant effects associated with other forms of participation in the program. In a reanalysis of Pugel's data using adjustment techniques similar to those of Humphreys and Weinstein (2007), Levely (2010) found no significant effects of registration into the reintegration program on income, but he found some indication that enrollment and completion of the program increased income (on the order of 10 percent). These findings raise questions about whether reintegration programs, as currently designed, are effective *at all* in achieving the programmatic goal of economic reintegration, much less the downstream goal of political reintegration.

As the authors themselves admit, these studies cannot be definitive on the effectiveness of reintegration programs in Liberia and Sierra Leone, much less in general, because of weak possibilities that they offer for identifying causal effects. This is by no fault of the authors, but rather by the nature of the programs. Those who do not participate in the program are likely to differ in important ways from those who do. In Sierra Leone, rates of enrollment in the reintegration program at the time of Humphreys and Weinstein's fieldwork were around 90 percent (Humphreys and Weinstein 2007), in which case the nonparticipants used to construct a pseudo "control group" are a highly self-selected group. In Liberia, the rates of enrollment at the time of Pugel's fieldwork were lower—about 25 percent—but Pugel indicates that enrollment was voluntary. Even among subjects matched on observed covariates, there is strong reason to remain concerned about bias due to unobserved factors associated with program take-up.[6]

Sekhon (2009) argues persuasively that adjustment methods, such as matching or regression, should be tied to a discontinuity or exogenous shock to make the causal story persuasive. A convincing causal interpretation requires that we can explain how two individuals who share similar observable traits may differ in their program status for reasons that will not bias the analysis. 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. We exploit precisely such a shock, which occurred during the reintegration program in Burundi, allowing us to measure program effects with minimal threat of self-selection bias.

General Hypotheses about Reintegration Programs

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 is thought to be critical in peace processes because it links "the more immediate requirements of disarmament and demobilization to the long-term imperatives of social and economic welfare" (Bryden and Hanggi 2005) and because it is the set of activities that facilitate effective conversion from combatant to civilian life. 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 no longer see violence as a legitimate means to seek political or personal gain and they submit to the laws and norms of civilian society (political reintegration; World Bank 2002; United Nations 2000; Pouligny 2004; Hanson 2007; United Nations 2010). 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 posited in reintegration program documentation is that economic reintegration fosters political reintegration, with family- and community-level social reintegration moderating the process. Reintegration programs typically emphasize economic reintegration as their main programmatic objective, but political reintegration is usually the ultimate goal. Reintegration programs may also include reconciliation interventions that attempt to facilitate social reintegration, but these are usually secondary. Political reintegration is not directly targeted but presumed to follow from the boost that a program provides to social and economic reintegration. In this way, the programmatic economic reintegration effects are presumed to have downstream political reintegration effects.[7]

The emphasis on enhancing ex-combatants' economic welfare draws in part on the research program on the political economy of conflict (Collier and Hoffler 2004; World Bank 2002, 2004a; Spear 2006). In its 2003 report titled *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). Ex-combatants may lack human and social capital endowments necessary to establish themselves in the peaceful economic sector.[8] These deficiencies may result from characteristics that set apart those who decided to participate in fighting or they may be consequences of soldiering itself. Ex-combatants may face acute shortages of human and material capital. Time spent away soldiering may have caused ex-combatant to lose their land or other capital, and time spent soldiering may have disrupted their accumulation of material or human capital.

These deficiencies may put ex-combatants at an economic disadvantage and make them more likely to shirk the law in finding ways to support themselves. This suggestion is consistent with findings from Collier's (1994) research on demobilization and reintegration of former soldiers in Uganda. McMullin (2004) suggests 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. Some combatants may have joined armed groups because the economic opportunity costs associated with doing so were low. Thus, 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." 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 1999). Ending the war is tantamount to giving up these opportunities. Anecdotal evidence suggests that some combatants make these calculations. During the DDR process in Liberia, a member of the armed group, Liberians United for Reconciliation and Democracy told a reporter, "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" (cited in Spear 2006). Finally, grievances over lack of access to opportunities may have motivated a willingness to challenge the state. Providing opportunities in the legal economy may remove this motivation and, as a result, an antistate orientation.

In our empirical analysis, we test two interrelated hypotheses:

Programmatic hypothesis: Reintegration programs improve the economic welfare of ex-combatants, as they are programmatically designed to do.

Downstream hypothesis: By improving economic welfare, reintegration programs increase ex-combatants' willingness to respect the rule of law and adopt an orientation that favors societal stability.

We investigate the extent to which the reintegration program achieved its proximate aims of improving income and livelihood outcomes. We then look at whether the programs achieved its downstream aims of increasing ex-combatants' satisfaction with civilian life relative to combatant life, inducing a more positive orientation toward the peace agreement, and inducing a more positive orientation toward stable government institutions.

The Burundi Context

We focus on the reintegration of adult (eighteen years old or more) male former rebel combatants. 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 eighteen 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 three years prior to fieldwork. Ex-combatants under eighteen were treated by a different (United Nations Children's Fund-managed) reintegration program than adults, so their outcomes are not comparable to those above the age cutoff.

Background to the Reintegration Program

The DDR program in Burundi was initiated following a 2003–2004 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 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 approximately 300,000 deaths out of a total population of 6 to 8 million. Burundi's prewar socioeconomic development levels were already among the world's lowest, although for its income level, the country did have relatively welldeveloped infrastructure and institutions. The war severely stalled development for over a decade, resulting in an estimated 20 percent decline in real gross domestic product (GDP) during 1993 to 2002 (World Bank 2004b, 6).

The outcome of the war and ensuing political developments were important features of the environment into which ex-rebels were reintegrating. The war ended in a peace accord between the government and the CNDD-FDD that called for elections. The 2005 elections resulted in the CNDD-FDD winning an comfortable majority of national assembly seats (59 percent of the 118 seats) and communal councilor posts (55 percent of the 3,225 posts) giving the party the strength it needed to elect its political head, Pierre Nkurunziza, 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.[10]

The "Treatment": Reintegration Program Benefits

The DDR program in Burundi was part of the broader Multi-Country Demobilization and Reintegration Program (MDRP),[11] which was intended to embody a comprehensive strategy to "enhance prospects for stabilization and recovery in the region." The World Bank's estimates for the sizes of the various forces is shown in the supplementary appendix.[12] Both members of the national army and the rebel groups would be demobilized. The reintegration program included several components. The National Commission for Disarmament, Demobilization and Reintegration (CNDRR), a Burundian government agency, directly administered a reinsertion subsistence allowance of between 30,000 and 185,000 Burundian francs per month or 60 to 370 US dollars per month at purchasing power parity, depending on rank. This assistance was provided in four tranches over eighteen months. Documentation from the program shows that at the time of our fieldwork in 2007, receipt of at least the first tranche was nearly universal among ex-combatants (98.5 percent). Also, through offices set up in nearly all provinces and through "focal points" appointed in nearly all *communes*,[13] 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 source of variation that we study was the major benefit offered by the program: the "socioeconomic reintegration package." The package provided a menu of opportunities from which ex-combatants could choose: (1) secondary school or university, (2) a one-year or shorter vocational-training program, or (3) in-kind (nonfinancial) start up materials to 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 receiving in-kind start-up materials for the new business (cash was not given). Regardless of the program chosen, the total value of benefits was to be 600,000 Burundian francs (about 1,200 US dollars at purchasing power parity) or less. Program documentation shows that the "income-generating activities" option was by far the most popular among the ex-combatants that were to receive benefits at the time of fieldwork in 2007: out of the just over 13,000 ex-combatants to have received benefits by 2007, 96 percent chose income-generating activities option, 3.6 percent chose vocational training, and less than 1 percent chose to resume formal education (MDRP 2007). The income-generating activities assistance would often take the form of providing goods (e.g., beer and soda) to sell in a small shop, partial payment for a motorcycle to start a "moto-taxi" business, or agricultural assets (livestock and farm tools) to produce marketable farm goods.

The Source of Exogenous Variation: A Disruption in Access

The CNDRR and MDRP delegated the delivery of the socioeconomic reintegration package benefits to nongovernmental organization (NGO) partners. Early in the program, when the number of beneficiaries 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 receiving benefits, the MDRP decided to formalize the system and contracted three large NGOs to deal with the coming wave of ex-combatants. These included Twitezimbere, a Burundian NGO, as well as Planning and Development Collaborative (PADCO) and Africare, two international NGOs. The work was divided evenly among the three NGOs. PADCO was assigned to cover ex-combatants registered as residents in the southwest 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 use international NGOs as implementing partners; the pressure was due to certain procurement restrictions, 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 Africare did not commence delivery 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 Africare areas and other areas can be controlled for, this service disruption provides a source of exogenous variation in program access. This is the cornerstone of our strategy for identifying the effects of the reintegration program.

Table 1. Summary Statistics

Variable	*Mean*	(*SD*)	Min.	Max.	*N*
Africare	0.3	(0.46)	0	1	371
Age	33.63	(9.38)	19	60	371
Hutu	0.96	(0.20)	0	1	371
Father in agriculture, prewar	0.72	(0.45)	0	1	362
Prewar education (years)	4.99	(2.69)	0	14	371
Prewar wealth index	0.04	(0.58)	−1.13	0.73	371
Noncomm. officer	0.68	(0.47)	0	1	371
Comm. officer	0.16	(0.37)	0	1	371
Unit death rate	0.11	(0.13)	0	0.83	333
Years in faction	10.38	(3.53)	2	36	366
Family death rate	0.17	(0.17)	0	0.8	371
Non-CNDD-FDD	0.18	(0.39)	0	1	371
Demob. Date (standardized)	0.02	(1.06)	−8.55	8.44	362
War violence index	8.08	(8.75)	0.1	60.43	371
Log(pop. density)	5.72	(0.41)	4.91	7.36	368
Ruling party province	0.61	(0.49)	0	1	371
Log(income+1)	9.16	(2.70)	0	12.9	359
Skilled occupation	0.12	(0.33)	0	1	371
Unskilled, nonagr. occupation	0.16	(0.36)	0	1	371
Agricultural occupation	0.60	(0.49)	0	1	371
No occupation	0.10	(0.30)	0	1	371
"Life better as civilian" indicator	0.70	(0.46)	0	1	371
"Very satisfied with accords" indicator	0.65	(0.48)	0	1	371
"Govt. should have more time" indicator	0.88	(0.32)	0	1	362

Note. CNDD-FDD = National Council for the Defense of Democracy-Forces for the Defense of Democracy.

Methodological Challenges

We have explicitly addressed three threats to the validity of our inferences. Our first methodological challenge is missing data. The ex-combatant data are drawn from an original survey of civilians and ex-combatants in Burundi collected by the authors in the summer of 2007.[16] Our response rate was very high—about 90 percent—and so we assume no need for further adjustment to account for nonresponse.[17] With the observations limited to former rebel males registered to receive reintegration assistance outside Bujumbura, the data set includes 110 ex-rebels registered in the Africare region and 261 outside the Africare region. Summary statistics of the raw survey data are presensented in Table 1.

As in any large-scale survey, our data do exhibit occasional missingness on items due to "don't know" responses, enumerator error, or data-entry error. Only once did the item missingness rate reach 10 percent for any given variable (the death rate of the ex-combatant's fighting unit). Otherwise, the rates of item missingness were usually 0, but occasionally around 1 to 2 percent. Listwise deletion would nonetheless have implied discarding 23 percent of excombatant observations. To avoid the inefficiencies and biases associated with listwise deletion, we conducted multiple imputation to fill missing values.[18] We used predictive mean matching imputation for all numeric, binary, and ordered categorical variables and multinomial logit regression imputation for nonordered categorical variables.[19]

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, we generated five multiple imputed data sets, ran the analysis on each of them, and averaged using the appropriate averaging forumlas.[20] For inference, we take covariate and treatment values as fixed and base our inference on presumed sampling variability. On each of the imputation-completed data sets, we use sandwich estimates of coefficient covariances that account for clustering at the commune level. We consider communes to be the relevant area within which an individual's routine economic interactions is confined. For the quantile regression estimates reported below, we use standard errors from inverted rank-test confidence intervals asymptotically robust to nonindependent and identically distributed sampling (Koenker and Hallock 2000).[21] To combine the estimates from each of the imputation-completed data sets, we used standard formulas based on the properties of mixed normal distributions (known as Rubin's rules; Rubin 1987; King et al. 2001, 53).[22]

A second methodological challenge is selection bias. We avoid self-selection bias because we have a disruption in program access that had nothing to do with either beneficiaries or program managers' choices. Nonetheless, we cannot take this shock to program access as an unproblematic source of quasi-random assignment at the individual or community levels. First, there may have been incidental imbalances in the characteristics of ex-combatants in the Africare versus non-Africare regions prior to the disruption to access. Also, there may have been differences in the types of communities in which would-be beneficiaries resided in the Africare and non-Africare regions. To reduce biases from these possible imbalances, we use covariate information to identify individuals in the non-Africare regions that resemble those in the Africare region in terms of their individual characteristics and the communities into which they are reintegrating. As such, we identify an "average effect of the treatment on the controls" (ATC; Angrist and Pischke 2008, 70). We conduct balance checks on a rather large set of predisruption covariates.[23] Using these matched data, we estimate program effects with a regression on an indicator for residence in the Africare region as well as all fourteen of the covariates used for matching. Including covariates helps to reduce bias due to imperfect matching, while the matching helps ensure that our estimated causal effects are robust to specification choice (Ho et al. 2007). As shown in the supplementary appendix, matching achieves remarkably good balance on this large set of covariates.

A third methodological challenge is *exposure heterogeneity*. The disruption in program access that is the basis of our identification strategy occurred after some ex-combatants in the Africare region had already received a reintegration package during a preliminary phase of the program. Our data do not allow us to identify these individuals, but documentation from the program provides the true rate at which this occurred.[24] As of the time of our fieldwork in July 2007, the program disruption affected 53 percent 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 attenuated estimate of the disruption effect due to the exposure heterogeneity among Africare region ex-combatants. To correct for this source of bias, we weight our effect estimate by the inverse of the difference in disruption rates across the Africare and non-Africare regions as described in the supplementary appendix. The weighted treatment effect estimates are presented as an upper bound estimate, and nonweighted estimates are presented as a lower bound.

Programmatic Impacts: Economic Reintegration

We now present estimates of program effects. We begin with effects on economic reintegration. As discussed above, these are estimates of *programmatic effects* in that the reintegration program provided direct assistance to ex-combatants for realizing a productive civilian livelihood. By hypothesis, effects on economic reintegration are a necessary part of the causal chain outlined above, which proposes that a boost to an individual's economic conditions can induce that individual to subscribe more faithfully to the laws and norms that govern civilian society. Our estimates of economic effects provide a basis on which we can judge whether these data provide a good test of this proposition. If we do find strong economic effects, then we are in a good position to assess the claim that political integration follows from a boost to economic well-being. Of course, for well-understood reasons associated with the study of mediated effects, this kind of analysis can only test the plausibility of such a two-step causal process (Green, Ha, and Bullock 2010).

From a policy perspective, it might seem trivial that there should be *some* effect on livelihoods, but remarkably, as discussed above, past studies in Sierra Leone and Liberia (e.g., Humphreys and Weinstein 2007; Levely 2010) raise questions about whether such benefits of reintegration programs exist. If we also find that they do not, then there is a strong case for a serious overhaul of reintegration assistance programs. For example, such findings might strengthen the case that individual-level targeting is ineffective without strong community-level assistance (Muggah 2009). If we do find substantial effects, however, then it raises question as to whether conditions in Burundi were different than in Sierra Leone or Liberia, or whether the latter studies generated biased estimates. As we discussed above, there are good reasons to suspect the latter.

We present results for two measures of economic reintegration: (1) monthly income and, as a consequence, poverty incidence and (2) livelihood, specifically the

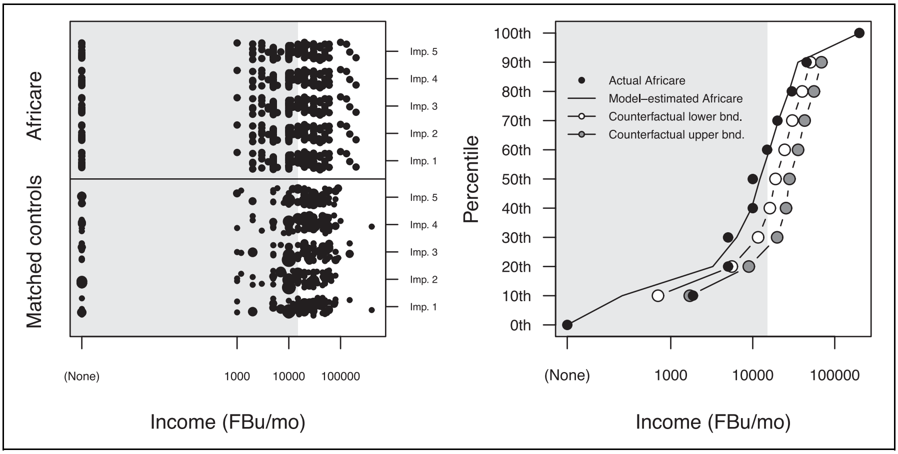

Figure 1. Differences in log-income distributions and estimated effects on the cumulative log-income distribution. The figure on the left shows the distribution of income (on the natural log scale) for Africare and matched control (non-Africare) observations. Distributions are shown for each of the five imputation-complete data sets. The size of the points for the matched controls is proportional to the weight assigned to that observation after matching. The figure on the right shows the cumulative log-income distribution for Africare respondents, and then predicted counter factual distributions. The gray zone corresponds to points below the 15,000 FBU/30 USD month poverty line.

nature of the occupation that the respondent obtained. We present unadjusted estimates on the raw data and then regression-adjusted estimates on the matched data. Having the two side by side helps us to assess the bias reduction as well as potential efficiency gains and losses from the adjustment methods. Our primary estimates of average individual-level effects are the regression-adjusted estimates on the matched data. A lower bound treatment effect estimate comes from regressions that do not correct for exposure heterogeneity, and an upper bound coming from regressions that do. We also present graphs that display estimated causal effects for the population of ex-combatants serviced by Africare. These show what would have been the gross impact of the program on the Africare beneficiaries had there been no interruption (i.e., the effect of the treatment on the controls).

Income and Poverty Incidence

Our economic outcome measure is the natural logarithm of monthly personal income (in Burundian Francs) reported by the respondent.[25] We used the natural logarithm as a variance stabilizing transformation to reduce estimation sensitivity to outliers. The left panel in Figure 1 shows the income distributions for Africare respondents and the matched controls from the five imputation-completed data sets. The gray blocks in the plots show regions of the income distribution below the poverty line,

Table 2. Estimates from Ordinary Least Squares (OLS) Regressions on log(income/month + 1)

	No adjustment			Matching and regression			Matching, regression, and het. exposure adjustment		
	Coef.	*SE*	*p* value	Coef.	*SE*	*p* value	Coef.	*SE*	*p* value
Africare	−0.67	0.33	.04	−0.59	0.53	.26	−1.12	1.00	.26
Age				−0.06	0.04	.14	−0.06	0.04	.14
Hutu				3.31	2.10	.12	3.31	2.10	.12
Father in agriculture, prewar				−1.18	0.74	.11	−1.18	0.74	.11
Prewar education				0.17	0.13	.22	0.17	0.13	.22
Prewar wealth index				0.52	0.50	.30	0.52	0.50	.30
Noncomm. Officer				−0.19	0.51	.71	−0.19	0.51	.71
Comm. officer				0.07	0.88	.93	0.07	0.88	.93
Unit death rate				6.30	3.40	.07	6.30	3.40	.07
Years in faction				0.09	0.17	.62	0.09	0.17	.62
Family death rate				1.84	1.25	.14	1.84	1.25	.14
Non-CNDD-FDD				0.60	1.07	.58	0.60	1.07	.58
Demob. Date (std.)				−0.83	0.63	.19	−0.83	0.63	.19
War violence index				0.16	0.09	.09	0.16	0.09	.09
Log(pop. density)				−0.14	0.68	.84	−0.14	0.68	.84
Ruling party province				−1.19	0.59	.04	−1.19	0.59	.04
Propensity score				8.67	5.34	.11	8.67	5.34	.11
Constant	9.37	0.20	.00	4.15	6.61	.53	4.15	6.61	.53
N from imputed data sets	371, 371, 371, 371, 371			177, 177, 181, 178, 177			177, 177, 181, 178, 177		

Note. CNDD-FDD = National Council for the Defense of Democracy-Forces for the Defense of Democracy. Robust standard errors computed with clustering at commune level. Two-sided test *p*-values are shown.

equivalent to about thirty US dollars per month at purchasing power parity. The dots for the matched controls are scaled proportional to the weight they receive in the analysis. We see that the Africare distribution is substantially heavier at lower-income values (e.g., below 10,000 Burundian francs or 20 US dollars per month). Indeed, the most striking visual impression is that the matched control income distribution exhibits a "floor" just below 10,000 Burundian Francs (FBU)/20 USD per month, whereas this floor is not evident for the Africare respondents. The Africare respondents also include a cluster of high earners (centered on 100,000 FBU or 200 USD per month) in an area where the matched control distribution is very thin.

We quantify these differences by examining differences in means, with linear regression, and then differences in quantiles, with quantile regression. The linear regression estimates appear in Table 2. The results show that in the raw data, Africare respondents' monthly income was about 50 percent lower on average. On the matched data, before adjusting for exposure heterogeneity, the point estimate for the Africare coefficient very similar as in the raw data (−0.59 vs. −0.67), although the p value (.26) is quite large for the matched data. The stability of the coefficient

Table 3. Estimates from Quantile Regressions on Log(income/month+1)

	No adjustment			Matching and regression			Matching, regression, and het. exposure adjustment		
	Coef.	*SE*	*p* value	Coef.	*SE*	*p* value	Coef.	*SE*	*p* value
Decile 1	−0.92	2.89	.75	−1.01	2.24	.65	−1.90	4.22	.65
Decile 2	−0.69	0.24	.00	−0.54	0.98	.59	−1.01	1.85	.59
Decile 3	−0.69	0.16	.00	−0.60	0.31	.06	−1.14	0.58	.06
Decile 4	−0.41	0.17	.02	−0.51	0.28	.07	−0.97	0.52	.07
Decile 5	−0.64	0.17	.00	−0.44	0.24	.07	−0.83	0.45	.07
Decile 6	−0.60	0.21	.01	−0.43	0.18	.02	−0.80	0.34	.02
Decile 7	−0.41	0.14	.00	−0.39	0.18	.03	−0.74	0.33	.03
Decile 8	−0.51	0.18	.00	−0.37	0.22	.11	−0.70	0.42	.11
Decile 9	−0.25	0.17	.14	−0.35	0.21	.10	−0.66	0.40	.10
N from imputed data sets	371,371,371,371,371			177,177,181,178,177			177,177,181,178,177		

Note. Standard errors computed using robust inverted rank test intervals. Regressions on matched data include the covariates in Table 2. The coefficients on these covariates are not displayed to save space. Two-sided test *p*-values are shown.

estimate suggests that there was little confounding to be removed in this case; the inflation of the p value reflects the rather small sample size in the matched data. When we run a leaner specification that includes only the unit death rate, war violence index, ruling party province, and the propensity score, the estimate does not change much (coefficient of −0.53 with p value of .29). The next estimate in the table adjusts further for exposure heterogeneity, providing our upper bound estimate. The adjustment essentially scales the Africare coefficient by about 1.9. This estimate suggests that Africare respondent incomes are 70 percent lower income on average, but with the same large p value.

However, differences in averages often hide important differences between groups' incomes, because income distributions can be very poorly behaved in terms of skew or massing at extreme values even after stabilizing transformations (Koenker and Hallock 2000; Angrist and Pischke 2008, 269–70). Figure 2 already showed that there appear to be important differences in the income distributions of the Africare respondents and their matched counterparts. We used quantile regression to estimate more general income effects, including effects on incidence of very low or very high incomes. We estimated differences in income deciles 1 through 9. The estimates are shown in Table 3.

The coefficients reveal two patterns. First, since they are all negative, they show that Africare recipients obtained lower incomes at every decile than did the other recipients. Second, this effect is mitigated as incomes increased as shown by the generally decreasing coefficients as the deciles increase. Setting aside the statistically insignificant coefficients for the lowest two deciles, the coefficients monotonically decrease in magnitude from −0.60 for the third decile to −0.35 for the ninth decile.

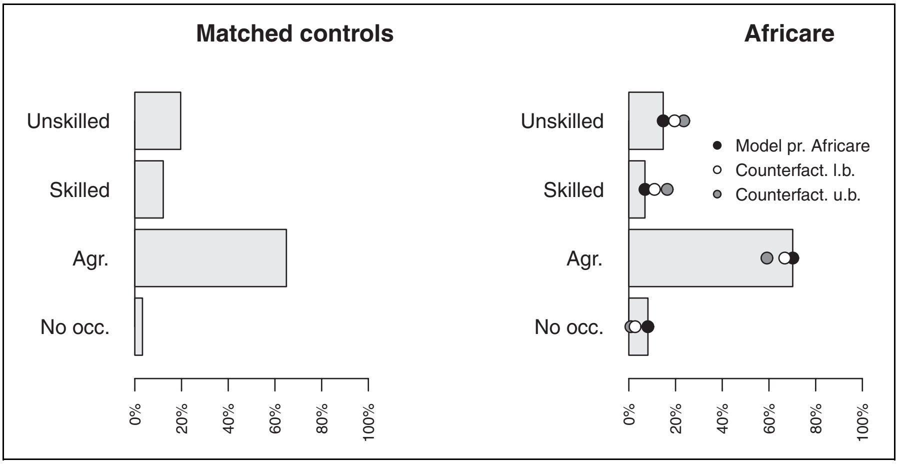

Figure 2. Livelihood outcome distributions and estimated effects. The left graph shows the livelihood outcome distribution for the matched controls. The right graph shows counterfactual outcomes, estimated from a multinomial logit specification (not shown), superimposed over the outcome distribution for the Africare-region ex-combatants. The estimated effect is the difference between the modeled "actual" outcomes (the solid black dots, which show in-sample fitted values) and the estimated counter-factual outcomes. The white dots show the lower-bound counter factual estimates, and the gray dots shows the upper-bound counterfactual estimates.

There are large differences in the decile values, and *p* values are quite small. Using the coefficients from the matched data, we estimate that the third decile among program nonrecipients in the Africare region is 45 percent (lower bound) to 68 percent (upper bound) of what it would have been had there been no interruption. By decile 7, the gap narrows to a difference of 32 percent (lower bound) to 52 percent (upper bound). Thus, we find that the income effect is declining in potential income, at least on the log-scale.[26]

The quantile effects are easier to observe from the plotted cumulative log-income distributions as shown in the right panel of Figure 1. The results indicate dramatic effects on poverty incidence. The actual cumulative log-income distribution for Africare region respondents is plotted with the black dots and fitted values from the quantile regressions are given by the black line. We plot 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 gray area shows the range of incomes below the poverty line. Poverty incidence among the Africare respondents is shown to be about 55 percent (the point where the black line crosses from the gray into the white region). The counterfactual distributions show that this would be an estimated 20 percentage points (lower-bound estimate) to 35 percentage points lower had there been no disruption. Furthermore, by the observation of Figure 1, it is obvious that this difference is not sensitive to the precise location of the poverty line. Finally, a logistic regression on a poverty incidence indicator (not shown) estimates precisely the same effect with a p value of .02.

What is the significance of this income effect relative to the value of the program benefits? An answer helps to assess the material benefits of the program against the costs. Above, we described that the the value of the benefit was up to 600,000 FBU (1,200 USD). From the cumulative log-income distribution plot in Figure 1, we see that the median earner in the Africare region was earning about 10,000 FBU (20 USD) per month. We estimate that this is about 64 percent (lower bound) to 44 percent (upper bound) of what that person would have earned had there been no disruption, implying a counterfactual incomes of about 31 USD (lower bound) and 46 USD (upper bound per month. Thus, the full value of the program benefit could not be immediately converted into income. Rather, the income effects of about 11 to 27 USD per month imply that it would take four to nine years to recuperate the value of the 1,200 USD investment. This recuperation depends on the short-run income differences that we measure being sustained over such a period—something that we cannot assess. Future research should look more closely at the factors that constrain ex-combatants' ability to convert program benefits into income.

Livelihood

To study effects on livelihood, we examine respondents answers to a question about what kind of work they did to sustain themselves. We coded their responses as (1) no occupation; (2) an agricultural occupation; (3) a nonagricultural, skilled sector occupation (including returning to school, professional position, or skilled labor, such as automotive repair, electrical technician, etc.); and (4) a nonagricultural, unskilled sector occupation (including security guard, manual labor, etc.). The distribution for the Africare respondents and their matched controls are displayed in Figure 1 (the displays average over the five imputation-completed data sets). We see that agricultural occupations dominate, accounting for about 60 to 70 percent of responses overall. We see some indication that Africare respondents are more likely to report that they have "no occupation" and less likely to report a "skilled" occupation. Looking more closely at these differences, we found that for "skilled" occupations, the pattern is driven by differences in attainment of skilled labor occupations rather than returning to school or obtaining a professional position. Indeed, there were *no* Africare respondents reporting having obtained a skilled labor occupation, whereas the rate among the matched non-Africare respondents ranged from 7 percent to 9 percent.

Table 4 answers whether livelihood outcomes of the non-Africare beneficiaries generally improved. In order to measure this, we need to impose some ordering on the livelihood outcomes. Based on our observations in the field and discussions with Burundians, we felt the following ordering made the most sense: "no occupation" would be the least desirable category in terms of social status and ability to sustain oneself, "agriculture" and "nonagriculture, unskilled" occupations would

Table 4. Estimates from Ordered Logistic Regressions on Occupation Categories

	No adjustment			Matching and regression			Matching, regression, and het. exposure adjustment		
	Coef.	*SE*	*p*value	Coef.	*SE*	*p* value	Coef.	SE	*p* value
Ordered logit results	−0.23	0.20	.25	−0.77	0.50	.13	−1.46	0.95	.13
N from imputed data sets	371,371,371,371,371			177,177,181,178,177			177,177,181,178,177		

Note. 1 = *no job*, 2 = *agriculture or unskilled*, 3 = *skilled*. Regressions on matched data include the covariates in Table 2. The coefficients on these covariates are not displayed to save space. Robust standard errors computed with clustering at commune level. Two-sided test p-values are shown.

be at the same middle level of desirability, and "skilled" occupations would be to be the highest. We fit an ordered logistic regression to these outcomes on the raw data and then the matched data, using the same covariate and exposure heterogeneity adjustments as in the above regressions. Using the raw data, that overall Africare respondents have 20 percent lower odds of being in a higher-ranked category than non-Africare respondents. After removing confounding bias through matching and regression adjustment, the difference becomes much larger. Indeed, we estimate that the effect of the reintegration program was such that Africare respondents had between 54 percent and 77 percent lower odds of being in a higher-ranked income category. The *p* value for this difference is .065 for a one-tailed test, which is appropriate here since we hypothesize a direction for the effect. Given the sample size, these results provide good evidence of a genuine improvement in livelihoods of the non-Africare group compared to the Africare group.

Downstream Impacts: Political Reintegration

To what extent do programmatic economic impacts contribute to political reintegration? Can a boost to economic well-being affect orientation toward political order and deepen appreciation of norms that govern civilian society? The empirical analysis thus far shows that the reintegration program substantially boosted the income of ex-combatants whose earnings would otherwise have been low. The conditional income effects resulted in large differences in poverty. These kinds of concentrated effects may be desirable. The logic of opportunity costs suggests that it is precisely these low potential-income ex-combatants who may be most at risk to be drawn back into action against the state or crime. Concentrating benefits among this segment may be the most efficient way to contribute to postconflict stability.

As described earlier, we define political reintegration as an ex-combatant accepting that violence is not a legitimate means to seek political or personal gain. Such reintegration should be reflected in attitudinal changes. These include a favorable view of civilian life relative to combatant life and attitudes consistent with norms of peaceful and democratic conflict management.[27] Attitudinal survey questions such as these tend to be quite noisy. Thus, we expect this to be a rather low-powered test of political reintegration effects. With those caveats, we consider three attitudinal measures: (1) responses to a question about whether life was better as a combatant; (2) responses to a question of whether one is very satisfied, somewhat satisfied, or unsatisfied with the peace accords, which were ostensibly based on norms of peaceful and democratic political competition; and (3) responses to a question about whether Burundians should allow the government as currently constituted to have a lot of time to sort out the many problems facing Burundian society or whether Burundians should start thinking about changing the government.

In studying downstream political reintegration impacts, we actually focus on reduced form relationships between Africare status and these outcomes. This may seem like a strange way to test what is essentially a mediated relationship between a reintegration program and political reintegration, with economic well-being being the mediator. The reasons have to do with what our quasi-experiment allow us to identify. We cannot use Africare status as an instrumental variable for any single economic outcome, because we have already seen that Africare status is associated with substantial differences on multiple dimensions. Recently expounded problems with linear mediation analysis are so profound (Green, Ha, and Bullock 2010) that we decided to forgo such an analysis. Nonetheless, we think that our reduced form estimates provide a useful probe into the plausibility of the claim that economic reintegration contributes to political reintegration. The reintegration program in Burundi was tightly focused on individual-level economic assistance. Thus, we see little reason to doubt that any apparent effects on political reintegration arise through the economic assistance role of the program.

We asked whether respondents thought life is better as a civilian (=1), with the alternative (=0) being that life was either better as a combatant or there was no difference. About 81 percent of matched controls indicated that life was better as a civilian life, whereas only 71 percent of Africare respondents indicated as such, implying 43 percent lower odds for Africare respondents. Thus, the strong tendency overall is to indicate a preference for civilian life, but that tendency is not as strong among respondents in the Africare region. In depth interviews that we conducted with ex-combatants alongside the survey, told of a general sense of "war fatigue."

More refined estimates of the program effects are displayed in Table 5, which presents results from logistic regressions. In the raw data, we see that the Africare coefficient is essentially zero. In the matched data however, we estimate that the odds of stating that civilian life is better are 51 to 74 percent less among Africare recipients. The p value of .085 for a one-tailed test, which is somewhat large, indicative of how imprecisely we have measured the effect. None of the other covariates included in the matched data regressions exhibited a strong relationship to this outcome measure. We estimated a leaner specification, including only the Hutu indicator, and it yielded even more attenuated results (coefficient of -0.58 and p value of

Table 5. Estimates from Logistic Regressions on Political Attitudes Outcomes

	No adjustment				Matching and regression				Matching, regression, and het. exposure adjustment			
Outcome	Coef.	OR	*SE*	*p* value	Coef.	OR	*SE*	*p* value	Coef.	OR	*SE*	*p* value
i. "Life better as civilian than combatant"	−0.06	0.94	0.29	.83	−0.72	0.49	0.51	.17	−1.36	0.26	0.96	.17
ii. "Very satisfied with peace accords"	0.05	1.05	0.23	.83	−0.02	0.99	0.39	.97	−0.03	0.97	0.74	.97
iii. "Government should have more time to solve problems"	0.19	1.21	0.35	.60	0.74	2.11	0.68	.29	1.41	4.08	1.29	.29
N from imputed data sets	371, 371, 371, 371, 371				177, 177, 181, 178, 177				177, 177, 181, 178, 177			

Note: Robust standard errors computed with clustering at commune level. Two-sided test p-values are shown. Regressions on matched data include the covariates in Table 2. The coefficients on these covariates are not displayed to save space. "OR" refers to odds ratio.

.26). Thus, the evidence of effects on attitudes toward civilian versus combatant life is not particularly strong statistically, despite the rather large estimated odds ratios.

Our next outcome variable was based on whether respondents said that they were "very satisfied" with the peace accords (=1) versus "satisfied" or "dissatisfied" (=0). The proportion responding "very satisfied" was 68 percent for both the matched controls and the Africare respondents. Thus, while there is a reasonable amount of variation on this question, there is no difference in rates across the two groups of respondents. The more refined estimates in Table 5 show as much, where the estimated coefficients are essentially 0. Thus, effects on attitudes toward the peace accords appear to be trivial or nonexistent.

Our final attitudinal measure of political reintegration was a question about whether the government should be given more time (=1) to sort out the major problems facing Burundian society or whether Burundians should consider ways to change the current government (=0). The question interacts with the political context in a number of ways that we should elaborate. First, the outcome of the war was one that brought about a fundamental transformation in opportunities for Hutu mobility, including the ethnic integration of major institutions such as the army, the civil service, and educational systems. At the same time, more radical Hutu parties, including FNL factions, accused the CNDD-FDD of "selling out" far short of achieving an ethnic balance in public institutions reflective of the ethnic balance in society. The ethnic quota in the military, for example, posited a 50-50 ethnic balance, far from the 85-14-1 Hutu-Tutsi-Twa balance typically presumed among Burundians. Among the ex-rebels included in our ex-combatant sample, we presume that expressions of the need to "consider ways to change the current government" would tend to reflect respondents' sympathizing with the FNL's accusations about the CNDD-FDD "selling out." In our data, we find that about 82 percent of the matched controls indicated that the government "should be given more time," whereas about 90 percent of the Africare respondents indicated as such, indicating the odds of a progovernment response are about 2 times higher for Africare respondents. This difference goes in the *opposite* of the hypothesized direction. The logistic regression estimates in Table 5 show that once we take the covariates into account, we estimate that the odds of indicating the the government should be given more time are 2 to 4 times higher, although the p value is rather large at .24. Thus, there is no evidence that the effect of the programming made ex-combatants less likely express impatience with the government.

Discussion

Our findings suggest that over the rather short duration that we could assess, the reintegration program produced a significant boost to income among ex-combatants who would otherwise have been among the worst off, resulting in a substantial lowering of poverty incidence. The program also improved the livelihood prospects for ex-combatants. We sought to assess whether economic assistance of this form can also translate into political reintegration effects, which we measured as respondents attitudes toward civilian life, the peace accords, and the government. The aim was to test a key proposition underpinning much of current reintegration programming: improvements to economic well-being may induce a more positive disposition toward political order and the laws and norms governing civilian society. We do not find compelling evidence of such downstream effects.

We recognize that there are a number of limitations to our analysis. First, we are evaluating program effects within a very short time frame. Non-Africare respondents had received their socioeconomic reintegration packages not more than nine months prior to our fieldwork. In fact depending on one's priors, it may be remarkable that we were able to find any effects, political or economic, given the short period. In that sense, the economic effects of the program are quite strong and the absence of political effects is not unexpected especially since presumably the ex-combatants expected that the problems with Africare would be solved in the short run.[28] A longer time frame would have been better, but good causal identification for such a study would be extremely unlikely. Indeed, in our case, Africare beneficiaries began to receive reintegration packages shortly after we completed fieldwork, thus erasing the discontinuity that we have exploited for our study.

Second, we relied on a relatively small sample due to the need to remove incidental imbalances for good balance and causal identification. Our matching algorithm discarded over 70 percent of the non-Africare respondents, and this after we had already discarded Bujumbura-based respondents, who constituted 12 percent of the original sample. Future research ought to anticipate the need for such balance and design more efficient sampling strategies (e.g., by sampling on already-measured community-level attributes). Third, our analysis is confined to those registered to receive benefits outside the capital city. In many postconflict contexts, rather large numbers of ex-combatants congregate in capital cities, raising questions about the external validity of our findings. Nonetheless, in Burundi, the vast majority of ex-combatants settled outside Bujumbura, and so we think our study is well motivated on those grounds, not to mention the fact that excluding Bujumbura was necessary for good causal identification. Finally, we would have greatly preferred to have had unobtrusive behavioral measures of political reintegration. Our assessment of political reintegration only gets at the self-reported attitudes. Data on criminality and violence behavior would be ideal, if it were available. We were unable to locate sufficiently fine-grained data for these purposes. Fourth, the discontinuity that we exploited was regional. This forced us to make an assumption that, once we had controlled for individual- and community-level attributes, we had achieved exchangeability all the way down to the individual level. This assumption cannot be tested.

These limitations aside, we have made progress in important ways. First, the type of discontinuity that we exploit offers the best feasible design for measuring the impact of reintegration programs in their totality. Because these are "emergency" programs in fragile political environments, there is little hope of a reintegration program being randomized fully or even being subject to a randomized rollout design.[29] In future research, we advise seeking these kinds of discontinuities, often due to program failures as well as continuing to seek opportunities to randomize features of reintegration programs—for example, different combinations of community-level and individual-level assistance and different ways of either promoting or discouraging the creation of associations among ex-combatants. These kinds of within-program experiments could allow us to assess causal mechanisms and policy options. Second, while the regional nature of our discontinuity forced us to make assumptions about exchangeability across regions, it also allowed us to ensure that broader equilibrium effects were incorporated into our estimates of program effects. Income, occupation, and political attitudes are all likely to exhibit substantial local spillover. If we had attempted to identify causal effects by comparing individuals within the same locality, these spillovers would have been hidden. Finally, our adjustment strategy resembles the long line of labor economics research on the United States National Supported Work program (Dehejia and Wahba 1999; Smith and Todd 2005). Smith and Todd (2005) have shown that program effect estimates are quite sensitive to the covariate set that one chooses. This does not seem to be such an important problem in our case since the differences in the estimates on the raw and matched data are small in almost all cases. We have used a very rich covariate set that, unlike other studies, accounts for both individual characteristics and community characteristics. This is crucial, as economic and political reintegration outcomes are most certainly the result of an interaction between these two levels.

These findings also produce several new questions for future research. What is perhaps most surprising about the results is how high the level of political reintegration is, even among those that had not yet received their economic reintegration package. The only significant difference between the Africare and the other recipients was in the question asking about preference for civilian over combatant life, but even there, 71 percent of Africare respondents preferred civilian life, versus 81 percent of the other respondents. In the other categories, 68 percent were very satisfied with the peace accords, while 82 percent and 90 percent of the respondents were willing to give the government more time to improve things. In a post-conflict situation, these are perhaps strikingly high numbers.[30]

As we already mentioned, the weakness of these findings is that it is very easy for respondents to profess satisfaction and patience in response to a questionnaire, while observation of their actions or other behavioral measures might show such sentiments to be inaccurate. Still if we are to take these results at face value, then these results raise policy-relevant question: Given this high level of satisfaction is political reintegration even necessary? And if a major driver of economic reintegration programming is to produce political reintegration benefits are DDR programs like the one we study really necessary? If changing the political attitudes of ex-combatants is a major justification of DDR, then these results indicate that that argument can be overstated. If ex-combatants are sufficently reintegrated politically even without the program, then an argument could be made that economic benefits should instead be offered to displaced people and other victims of the war. To address that question, one could explicitly compare the political attitudes of civilians and ex-combatnats. If the civilians have higher reintegration levels, then it shows that DDR really is necessary (as long as one accepts the downstream benefit argument, an argument we have not provided evidence for), whereas if the civilians have more or less equivalent levels of satisfaction, then one could question if DDR is really necessary. If on the other hand civilians have lower levels of political integration and satisfaction with the postwar settlement, one could argue that DDR does more harm than good. Such a finding would provide support for the argument that Muggah and others have made, namely, that standard DDR programs cause resentment among the general population, resentment that can be ameliorated by community-based DDR programs.[31]

Conclusion

Ex-combatant reintegration programming is central in most transitions from civil war to peace. Considerable sums are expended on such programs worldwide. Peace researchers and program managers attribute great importance to such programs in helping societies move past critical barriers to sustainable peace. These programs provide incentives that ostensibly lure ex-combatants away from the use of violence to meet their material and psychological needs, and reorient them toward subscribing to norms that govern civilian society. But the evidence on the impact of these programs is very thin. There is a worrying gap between effort, expectations, and evidence.

We make a major contribution in the way of evidence. We use a disruption in the implementation of the ex-combatant reintegration program in Burundi as a source of exogenous variation to measure the impact of the program. The disruption occurred when one of the implementing partners fell into dispute with the government for reasons quite independent of the characteristics of the ex-combatants that were due to be served. This groups of intended beneficiaries included approximately one-third of the overall ex-combatant beneficiary population. Because of the dispute, this large group of would-be beneficiaries had their benefits withheld for nearly a year. During that time, we were able to launch a field study to measure effects of reintegration programming. We studied effects on both the proximate economic outcomes that were directly targeted (programmatic effects) and the second-order effects on political attitudes that were expected to follow from a boost to economic well-being (downstream effects). To achieve good causal identification, we matched ex-combatants whose benefits were withheld to those that suffered no such disruption on a rich set of individual-level and community-level characteristics. We used regression adjustment to correct for the fact that the matches were not exact. We also used an inverse propensity adjustment to make up for the fact that a minority segment of ex-combatants in the region suffering the disruption had been able to access program benefits prior to the disruption.

Our assessment of "programmatic" effects is important, because past research has raised questions about whether even these proximate impacts exist. We find that they do. The program affected a substantial boost to the income of those who would otherwise have been among the worst off, leading to a large reduction in poverty incidence. In addition, livelihood outcomes improved. We do not find compelling evidence for downstream effects on political reintegration, at least as measured through attitudes. While we found a moderate effect on ex-combatants' sense that civilian life was preferable to combatant life, there was no effect of either increasing levels of satisfaction with the peace accords or increasing levels of support for current governing institutions, raising questions about a possible positive link between a boost to economic well-being and a more positive orientation toward stability and norms governing civilian society.

We hope that our analysis provides an example of a research design that successfully exploits unusual programmatic discontinuities to estimate important causal effects. Sometimes these discontinuities happen by design, as when programs are rolled out in a random fashion. Sometimes the discontinuities happen by accident, as in the program we study in this article. We suspect that bureaucratic failures, halts to service delivery, and other such unanticipated sources of variation in program performance are quite common and that they would provide excellent opportunities to measure program effects, particularly if the disruptions are long in duration and we encourage fellow researchers to seek such sources of exogenous variation in future research.

Acknowledgements

We thank Iteka - Ligue Burundaise des Droits de l'Homme for being our partners, and Dingamadji Solness and Marcelo Fabre from the World Bank/Multi-Country Demobilization and

Reintegration Program and members of the executive secretariat of Burundi's *Commission Nationale de Demobilisation, Reinsertion* et Reintegration for allowing us to conduct this research. We received valuable feedback from Chris Blattman, Macartan Humphreys, Birger Heldt, Kristin Michelitch, Philip Verwimp, and participants at the *Evolution de la pauvrete et du bien-etre au Burundi* conference in Bujumbura, the Columbia-NYU African Political Economy Research Seminar, and World Bank Development Impact Evaluation Initiative. This study is under IRB-approval at Columbia University (no. AAAB8249) and New York University (no. HS-5279). This work is solely the authors' responsibility, and in no way represents positions of any of the above-named organizations.

Declaration of Conflicting Interests

The author(s) declared no potential conflicts of interest with respect to the research, authorship, and/or publication of this article.

Funding

The author(s) disclosed receipt of the following financial support for the research, authorship, and/or publication of this article: This research is part of the *Wartime and Post-Conflict Experiences in Burundi* survey, sponsored by the Folke Bernadotte Academy, Sweden, and the United States Institute of Peace.

Notes

1. The Uppsala Peace Agreement data set is available online at http://www.pcr.uu.se/research/UCDP/.
2. See, for example, Colletta, Kostner, and Ingo (1996) and the studies contributed to the Government of Sweden's *Stockholm Initiative on Disarmament, Demobilization, and Reintegration* (2006).
3. See Kingma (1997), UNDP (2000), Alden (2002), Paes (2005), Baare (2006), and Specker (2008).
4. See Pouligny (2004), Muggah (2005), and Jennings (2008).
5. See Jennings (2007), Soderstrom (2010), and Uvin (2007).
6. DiNardo and Lee (2010) discuss conditions under which matching can even exacerbate self-selection bias.
7. This implicit "theory" of reintegration is well articulated in World Bank (2004a), which layed the foundation for recent reintegration programming in Angola, Burundi, Central African Republic, Democratic Republic of Congo, Republic of Congo, Rwanda, and Uganda.
8. For reviews of possible sources of vulnerability among ex-combatants, see Annan and Patel (2009) and Tajima (2009).
9. The youngest ex-rebel in our sample is 19, and the years of recruitment for our respondents were between 1993 and 2003.
10. Contrast to this 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 percent 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 article.

11. Participating countries included Angola, Burundi, Central African Republic, Democratic Republic of Congo, Rwanda, Republic of Congo, and Uganda.
12. Available at https://files.nyu.edu/cds2083/public/burundisurvey/
13. Communes are the second-tier administrative units in Burundi, after provinces. A commune is a collection of villages and communities similar to a "county" in the western world. If we exclude the dense urban capital of Bujumbura, communes range in size from 15,000 to 100,000 individuals (or about 3,000 to 20,000 households).
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.
16. Details on the survey are available at www....
17. The covariate adjustment that we perform in the following will reduce any biases associated with those factors. With such a low nonresponse rate, it is reasonable to believe that any residual bias is negligible.
18. 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 approximations (e.g., linear or quadratic) of unknown functional forms. This is explained in Samii (2011). 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 nonrandom sampling. See, for example, Korn and Graubard (1999, 159–85).
19. Predictive mean matching 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).
20. As Rubin (1987, 114) explains, rather few imputations are typically needed to achieve reliable estimates. Summary statistics for the five imputation-completed samples are presented in the supplementary appendix.
21. Bootstrapped confidence intervals are inappropriate on the matched sample (Abadie and Imbens 2008).
22. The programs used to generate the results reported here are available at http://www...[
23. Balance statistics and the geographic distribution of the prematched and matched data are presented in the supplemenatary appendix.
24. The relevant figures are provided in the supplementary appendix.
25. We added one FBU to all values to accommodate reported incomes of zero. To check for sensitivity associated with adding one, we also fit a Tobit regression on log of income

with zero-income observations treated as censored; results were identical, and so we do not display them.

26. One reason for this pattern could be a statistical artifact: if the income effect was, for the most part, a simple linear shift, then it would appear as a declining percentage shift after the log transformation. Running the same linear regressions on the raw (not log-transformed) income measure, we estimate that Africare respondent incomes were between 14 USD (lower bound) to about 27 USD (upper bound) per month lower on average, although the *p* value again is high at .17. Another explanation may be that the pattern is a genuine reflection of diminishing returns of the program over potential incomes. The data do not allow us to distinguish between these two stories.
27. Reintegration should also be reflected in conduct—for example, in refraining from participation in political violence or using threats for personal gain. Unfortunately, our study did not acquire such behavioral measures, and so we can only examine effects on the expressed political attitudes.
28. We thank an anonymous reviewer for making this point.
29. This is based on our own field experiences as well as discussions with many World Bank and United Nations program administrators.
30. We thank an anonymous review for making this point.
31. We thank an anonymous reviewer for pointing out these policy implications of our results.

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