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Demobilization and Reintegration

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Since 1989, international efforts to end protracted conflicts have included sustained investments in the disarmament, demobilization, and reintegration (DDR) of combatants. Yet while policy analysts have debated the factors that contribute to successful DDR programs and scholars have reasoned about the macro conditions that facilitate successful peace building, little is known about the factors that account for successful reintegration at the micro level. Using a new dataset of ex-combatants in Sierra Leone, this article analyzes the individual-level determinants of demobilization and reintegration. Past participation in an abusive military faction is the strongest predictor of difficulty in achieving social reintegration. On economic and political reintegration, we find that wealthier and more educated combatants face greater difficulties. Ideologues, men, and younger fighters are the most likely to retain strong ties to their factions. Most important, we find little evidence at the micro level that internationally funded programs facilitate demobilization and reintegration.

Keywords: *rebellion; civil war; post-conflict reconstruction; international peacekeeping; reintegration*

Since 1989, multidimensional peacekeeping operations led by the United Nations have been central to efforts to end protracted conflicts in Africa, Latin America, and Asia. Moving beyond traditional monitoring and peacekeeping, multidimensional operations have sought to lay the foundation for stable, self-sustaining transitions by building confidence among warring parties through social and economic cooperation, rebuilding infrastructure, and reforming institutions so as to reduce incentives for future violence. Among the most ambitious of these tasks has been the attempt to dissolve militant organizations and return former fighters to their communities (Berdal 1996).

These efforts to demobilize armed factions and reintegrate individual fighters into civilian life—the latter two of the three elements of disarmament, demobilization, and reintegration (DDR) programs—have emerged as a critical component of these interventions. Formal programs to facilitate DDR date back to the operations

of the UN Observer Group in Central America (ONUCA) in 1989. Since then, DDR has figured prominently as part of UN operations in El Salvador, Cambodia, Mozambique, Angola, Liberia, Sierra Leone, Guatemala, Tajikistan, and Burundi to name a few. By 2000, when the secretary general was asked to report to the Security Council on the role of the DDR efforts, he felt confident enough to conclude that "a process of disarmament, demobilization, and reintegration has *repeatedly proved to be vital* to stability in a post-conflict situation" (United Nations 2000, 1; italics added).

But despite the confidence of policy makers in the impact of DDR programs, there have been few systematic efforts to evaluate the determinants of successful reintegration by ex-combatants after conflict. The literature is chock-full of "lessons-learned" assessments that attempt to parse the factors that account for the success (or failure) of a given DDR program (World Bank 1993).[1] Surprisingly, this debate has typically been carried out without an appropriate source of variation in the key explanatory variables. At the macro level, studies of DDR have typically not engaged in a comparison of outcomes in countries that did and those that did not receive interventions. At the micro level, strikingly few rigorous attempts have been made to identify factors that might explain why some individuals and not others are able to successfully reintegrate after conflict. In particular, no studies have systematically compared the reintegration success of those that have and have not participated in demobilization and reintegration programs.

In practice, identifying the effects of DDR programs on peace building is difficult at the macro level. These programs rarely take place in isolation. They typically are complemented by other military, social, and economic interventions. An academic consensus appears to be emerging that these multidimensional peacekeeping operations improve the prospects for peace, democracy, and improved economic performance in the aftermath of conflict (Walter 1997; Doyle and Sambanis 2000, 2006). Moreover, outside intervention seems to be particularly valuable when hostilities are deep and local capacities have been destroyed (Doyle and Sambanis 2000, 2006). But the multidimensional character of these interventions makes

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it difficult to discern the individual contribution of specific programs to overall success. There are, quite simply, too few cases and too many confounding variables.

In this article, we instead turn to micro-level data, drawing on a survey of 1,043 combatants from the five warring factions in Sierra Leone's civil war. We employ measures of variation in the reintegration experience across former combatants to answer the question, what determines the ability of ex-combatants to reintegrate into society? Our primary goal is to identify the impact of international attempts to facilitate reintegration, but we also explore how the ability of ex-combatants to reintegrate depends on their personal characteristics and on their experience of conflict.

We address these questions using a second-best approach. We emphasize that a first-best approach exists, using a method of randomized intervention, in which (for example) the order in which individuals undergo DDR processes is partly randomized. This method provides enormous power for understanding the impact of external interventions, but we know of no attempt to use the principle of randomization to evaluate DDR efforts in any postconflict country. In the absence of randomization, we use data from ex-post survey work to identify the correlates of successful reintegration and look for evidence of the impact of DDR programs.

To preview our results, we find surprising heterogeneity across outcome measures. The breaking of ties between combatants and their factions, for example, is not associated with more successful reintegration into the economy, the community, or political life. Different processes appear to underlie distinct facets of social, economic, and political integration. Moreover, a number of individual-level determinants of successful reintegration stand out. Past participation in an abusive military faction is the strongest predictor of difficulty in achieving social reintegration. For economic and political integration, we find that individuals from wealthier and more educated backgrounds report greater difficulties. Higher ranking combatants, we find, appear to be less trusting of democratic politics. On our measure of the disestablishment of military factions, the evidence suggests that ideologues are more likely to remain connected to their units, as are male fighters and younger ex-combatants.

Our examination of DDR programs produces little evidence in support of claims that these effectively break down factional structures and facilitate reintegration. Combatants not exposed to the DDR program appear to reintegrate just as successfully as those that participated. In the absence of a randomized trial, however, there are a number of reasons why we might fail to identify effects even if they exist—chief among these are spillover, selection, and sampling biases. We examine each of these sources of bias in turn. Based on the data available to us, our analysis suggests that the nonfinding cannot be easily attributed to selection or sampling effects. There is, however, some evidence for one of our four outcome measures that spillover effects may render our ability to identify program effects particularly difficult. Nonetheless, to discount this prima facie evidence that the DDR program in Sierra Leone had no impact, policy makers will need to employ more robust strategies for demonstrating the efficacy of demobilization and reintegration efforts.

The second section of this article situates demobilization and reintegration in the context of existing theoretical arguments about the determinants of successful peace building. We then explore the question of whether the impact of macro-level programs should be observable at the micro level, drawing out implications from our discussion that then shape our empirical strategy. A fourth section introduces the case of Sierra Leone and describes our research method. The fifth, sixth, and seventh sections describe our empirical strategy for studying the determinants of reintegration success and identifying the effects of international programs. A concluding section discusses the relevance of our results for broader discussions of DDR and postconflict strategy.

Hypotheses on Demobilization and Reintegration

Policy makers have long supported demobilization and reintegration because of the perceived impact such programs have on the risk of a return to conflict. Spear (2002) emphasizes the importance of dissolving armed factions: ''Peace requires breaking the command and control structures operating over rebel fighters . . . thus making it more difficult for them to return to organized rebellion'' (p. 141). Economic dimensions of postconflict reintegration receive priority from the International Peace Academy (2002), which argues that ''ex-combatants must be able to earn a livelihood through legitimate means'' (p. 5). Given that a higher risk of conflict is associated with an absence of income-earning opportunities for young men (Collier and Hoeffler 2004), risk-reducing demobilization and reintegration programs seek to create economic opportunities for combatants.

The United Nations (2000) points to the need to ''convert combatants who pursue their objectives through force to civilians who pursue their objectives through other means'' (p. 11). Generating confidence in a democratic alternative to militarized politics is a ''critical test of the peace process'' (United Nations 2000, 11). Finally, civil society organizations often underscore the need for reconciliation in the aftermath of conflict. To the extent that ex-combatants gain acceptance from family members, friends, and neighbors through formal or informal processes of reconciliation, communities are in a better position to reintegrate former soldiers and facilitate their reinsertion into civilian life.

Policy makers thus recognize a range of distinct channels through which the successful demobilization and reintegration of ex-combatants contributes to successful peace building. The basic hypothesis we examine here is whether DDR programs facilitate this demobilization and reintegration, returning the many benefits attributed to such programs by policy makers. We look for evidence that participation in demobilization and reintegration programs dissolves the factional networks linking ex-combatants to one another, improves income-earning opportunities available to former fighters, generates increased confidence in the democratic process, and facilitates reconciliation with family, friends, and community members.

Our core hypothesis, though motivated by policy debates about appropriate post-conflict strategies, is consistent as well with theoretical work in the literature on civil war termination. Two underlying mechanisms can be identified that link DDR programs to the successful dissolution of warring factions in the aftermath of civil war. The first draws on the logic of the security dilemma (Walter 1997; Roe 1999). Even if all parties favor the dissolution of their military factions, in an institutionally weak environment, mutual mistrust may result in an unwillingness to take the first step toward demobilization. By offering assurances that warring factions will be protected, terms will be fulfilled, and promises will be kept, a credible third-party guarantee provides one solution to this dilemma. By providing an environment in which formerly warring parties can learn of each other's intentions, DDR programs can provide fighters with the security and confidence needed to disengage from their factions and return to civilian life.

The second draws on research that examines the role of "spoilers" (Stedman 1997). Spoilers—individuals that use violence to undermine peace efforts—may seek to maintain the structures of armed factions to bargain for more favorable returns and thereby exacerbate security dilemmas. Yet as argued by Stedman, positive measures may exist to address the grievances of factions that stand in the way of peace. Through the provision of training and transfers of monetary compensation, DDR programs may alter the relative benefits and costs of engagement with a peace process and remove the incentives for spoilers to maintain organizational structures.

These theoretical considerations suggest individual-level features that might render reintegration more difficult in some circumstances:

Hypothesis 1: Individuals that distrust the intentions of other groups should be less likely to reintegrate.

Hypothesis 2: Individuals that are dissatisfied with the terms of the peace should be less likely to reintegrate.

However, the theoretical logic suggests also (consistent with the perspective of policy makers) that DDR programs should be effective in facilitating reintegration. Moreover, the logic suggests that these programs should be particularly salient among individuals for whom distrust and dissatisfaction would otherwise impede reintegration. This reasoning leads to the following three further hypotheses:

Hypothesis 3: Participation in DDR programs will lead to more successful demobilization and reintegration among ex-combatants.

Hypothesis 4: The impact of DDR programs should be most pronounced among individuals who distrust the intentions of other groups.

Hypothesis 5: The impact of DDR programs should be most pronounced among individuals who are dissatisfied with the terms of the peace.

Beyond these hypotheses regarding the impact of DDR programs, we also explore a series of individual- and group-level characteristics that might condition successful demobilization and reintegration. These control variables are of considerable independent interest in that our study provides a first opportunity to identify some of the empirical correlates of reintegration.

Recent research in social psychology and anthropology suggests some plausible determinants of reintegration success. A rich empirical literature has examined the impact on reintegration of exposure to violence on the part of noncombatants (see, for example, Dyregrov, Gjestad, and Raundalen 2002; Husain et al. 1998). Some single-country studies in Sierra Leone have examined the reintegration prospects of particular subgroups of combatants, notably youth (Richards et al. 2003) and women and girls (Mazurana et al. 2002). But beyond these studies, our review of the literature on demobilization and reintegration yields little in the way of systematic theories about the conditions under which some combatants but not others will give up their arms and reintegrate into civilian life. While scholars have devoted much attention to the study of how organizations form, considerably less effort has been directed at their dissolution. Thus, given the rudimentary state of our knowledge, our examination of these factors is exploratory: we seek to document the extent to which a set of demographic and social factors can account for successful reintegration across individual combatants.

In the analysis that follows, we consider, without stating explicit hypotheses, the relationship between reintegration success and eleven individual, group, and community characteristics: five measures of an individual's demographic background (age, gender, ethnicity, education, and wealth), four measures of an individual's experience of the conflict (whether the individual was abducted, whether he or she was a political supporter of the group, whether he or she was an officer, and a measure of the abusiveness of the unit in which an individual fought) and, finally, two measures of community characteristics (an indicator of wealth and an index of the degree to which the community suffered from violence during the war).

Macro Processes and Micro Effects

Before turning to our empirical analysis, we are confronted by an inferential challenge. In shifting from the macro to the micro, we make an implicit assumption that the impact of a DDR program in a given country can be discerned by comparing reintegration outcomes across individuals that did and did not participate in a program. This assumption makes sense if the macro effects of DDR programs work through the positive impact such interventions have on individual combatants, as enumerated in the previous section. To the extent that DDR is designed explicitly to break down ties within factions and facilitate economic, political, and social reintegration, these effects should, in principle, be visible at the individual level. We emphasize, however, that if DDR programs work instead at a more aggregate

level—for example, by enabling factions to commit to a peace process through the reassurance DDR programs offer of each side's intentions—the impact of the intervention cannot be assessed through this micro-level approach.

Empirically, the strategy of comparing outcomes across treated and untreated ex-combatants to identify the efficacy of a national-level process is complicated by three distinct effects: spillover effects, selection effects, and sampling effects. We review these three potential confounds here, as they shape our empirical strategy, and then return to an analysis of the role they play in Sierra Leone in the discussion of our results.

Spillover Effects

The core challenge posed by spillover effects can be illustrated with the following example. Assume that of N individuals, T receive the treatment (with a true average treatment effect on the treated normalized to 1). Whenever some fraction of individuals (T/N) receives the treatment directly, another fraction, δ, of the untreated individuals also receives the treatment indirectly (or alternatively, assume that all the untreated individuals receive a partial treatment of strength δ). In this case, a method that uses differences in reintegration between the treated and untreated but ignores spillovers estimates the total treatment effect as

$$T \times (1 - \delta).$$

However, taking account of spillovers, the true total treatment effect is

$$T + (N - T)\delta.$$

This approach underestimates the overall impact of DDR in two ways. On one hand, the difference between the treated and the untreated is an underestimate of the impact of the program on the treated. In addition, the estimate fails to capture the benefits that spill over to nonparticipants as returns to the program. Plausibly, the spillover parameter, δ, is a function of the share of individuals treated. If spillover effects are increasing in the number treated, then applying the reasoning above, it becomes increasingly difficult to identify treatment effects as the number of individuals that participate in the program increases.

Selection Effects

A second source of bias can arise if there are systematic differences between those that participated in DDR programs and those that did not (other than the fact that they were exposed to the treatment). If selection effects are present, any differences in reintegration success (or the lack of a difference) between those that entered and those that did not could be a result of the selection mechanism rather than the impact (or nonimpact) of the treatment.

Table 1
Program Effects

	Reintegrated	Did Not Reintegrate
Treated	A	B
Untreated	C	D

Sampling Effects

A final source of bias might arise if individuals that did not take part in the DDR program and did not successfully reintegrate were also less likely to appear in our sample of ex-combatants. To see the logic, let the cell entries in Table 1 represent the number of individuals belonging to each combination of reintegrated/nonreintegrated and treated/untreated.

In the absence of selection effects, an estimate of the true average treatment effect is given by

$$\beta = \frac{A}{A+B} - \frac{C}{C+D}.$$

If, however, a share, α, of those that did not reintegrate and did not enter the program were missing from our sample, then our estimate of the average treatment effect would be

$$\beta' = \frac{A}{A+B} - \frac{C}{C+\alpha D},$$

which is less than β. In other words, a sampling frame that systematically missed individuals that did not reintegrate and did not enter the program would result in an underestimation of the program effect. Conversely, if some share, α, of individuals that took part in the program and failed to reintegrate also failed to enter our sample, then the estimate of the average treatment effect is given by

$$\beta'' = \frac{A}{A+\alpha B} - \frac{C}{C+D},$$

which clearly overestimates program effect. We consider these two possibilities in the discussion of our findings.

Surveying DDR in Sierra Leone

In January 2002, when the government of Sierra Leone declared its more than decade-long war officially over, the international community showered it with plaudits for a successful disarmament, demobilization, and reintegration program that paved the way for a stable postwar political order. This turn of events was unexpected for a country that experienced a brutal civil war, which captured international

attention, a stop-and-start peace-building effort lasting more than four years, and the persistent negative spillover effects of violence in neighboring Liberia.

The war in Sierra Leone began in 1991 when a small group of combatants—calling themselves the Revolutionary United Front (RUF)—entered Sierra Leone from neighboring Liberia, backed by Charles Taylor, to fight the Sierra Leone Army (SLA). Over the course of ten years of fighting, three additional factions emerged: the Civil Defense Forces (CDF), a militia group that joined forces with President Kabbah's government when the country returned to civilian rule in 1996; the Armed Forces Revolutionary Council (AFRC), a group of soldiers that overthrew Kabbah in a coup in 1997; and the West Side Boys (WSB), incorporating elements of all the factions in yet another militia group. During the war, Sierra Leone experienced violence of horrific proportions. Tens of thousands of civilians were killed, and hundreds of thousands were displaced from their homes. After attempts at power sharing failed, the war was brought to a close with the capture of the leader of the RUF, an intervention by what was (at the time) the largest UN mission in the world, and robust military action by third parties, notably Guinea and the United Kingdom.

Given the ups and downs of the war itself, it should come as no surprise that the DDR process faced innumerable hiccups in its implementation (Comninos, Stavrou, and Stewart 2002). Boutros Boutros-Ghali called for a demobilization and reintegration effort in Sierra Leone as early as 1995 (Agence France Press 1995) and a DDR program was written into the terms of the 1996 peace agreement. However, the first sustained efforts to demobilize fighters began only in 1998. Kabbah's government led this process after it was returned to power by the Nigerians. But it was wholly unsuccessful, since only 3,000 ex-combatants registered for disarmament and demobilization (Molloy 2004). A second phase began in 1999 after the Lomé Accord was signed, and it continued until 2000 when the war broke out anew. During this period, slightly fewer than 20,000 combatants turned up to be demobilized. Although demobilization continued during negotiations, the bulk of demobilization took place after the United Nations Mission in Sierra Leone was beefed up, following the British intervention in 2001 and 2002. In the third and final phase, close to 50,000 combatants disarmed. This brought the total caseload to approximately 76,000 fighters (Molloy 2004).

The disarmament process was conducted at reception centers distributed around the country. It included five phases: the assembly of combatants, collection of personal information, the verification and collection of weapons, the certification of eligibility for benefits, and transportation to a demobilization center. Once disarmed, combatants were prepared to return to civilian life in demobilization sites where they received basic necessities, reinsertion allowances, counseling, and eventually transportation to a local community where they elected to live permanently. In the community, combatants benefited from training programs (largely vocational skills, including auto repair, furniture-making, etc.) designed

to ease their reentry into the local economy. Moving more than 76,000 soldiers through this process is from an operational standpoint an accomplishment in itself. Our data suggest that program implementation was successful in other ways as well. Rates of participation were nearly equal across the five major factions and we found little evidence that an individual's political affiliation correlated with his or her ultimate satisfaction with the program. Complaints about the program centered mainly on its administrative efficiency and bureaucratic design—common criticisms of UN-sponsored programs—but there is no evidence that the process was manipulated to favor any one group to the exclusion of any other.

To assess the extent to which combatants have been able to reintegrate and identify the relative importance of participation in the DDR program, we gathered systematic data on a sample of ex-combatants, some of whom participated in the formal DDR effort and others who remained outside of it (Humphreys and Weinstein 2004). The survey was conducted between June and August 2003, slightly more than a year after the war came to an end. The study targeted a sample of 1,000 ex-combatants; a total of 1,043 surveys of ex-combatants were completed. The main method for gathering information was through the administration of a closed-ended questionnaire by an enumerator in the respondent's local language. Interviews were conducted at training program sites and in community centers around the country.[2]

To ensure as unbiased a sample as possible, the survey employed a number of levels of randomization. First, surveys were enumerated in forty-five chiefdoms or urban localities that were randomly selected using estimates of the population of ex-combatants residing in the chiefdoms provided to us by the National Commission on Demobilization, Disarmament, and Reintegration (NCDDR 2002), the National Statistics Office, and estimates of experts in Sierra Leone.[3] We note that the fact that the sampling frame depended in part on NCDDR estimates implies that it is possible that areas in which NCDDR was most inactive were underrepresented in our sample.

Within each enumeration unit, sites were also randomly selected, with both urban and rural areas represented. However, because of the relatively small share of combatants to noncombatants and the absence of lists, standard sampling methods could not be used to generate a perfectly random sample of ex-combatants. Instead, enumerators worked through both official (United Nations and government) contacts and local community leaders to identify a pool of ex-combatants at least two or three times as large as the target number from which the actual subjects were randomly selected. In most instances, chiefs and DDR staff asked a number of ex-combatants to meet at a public location, and teams selected candidates randomly from that pool (by choosing every third person or selecting numbers from a hat). While this method worked well, there is no guarantee that the lists generated in this process are statistically representative of the population of fighters in each chiefdom.

The survey elicited a detailed profile of each of the combatants, including their socioeconomic backgrounds, their experience of the war itself, their involvement

in the DDR process, and the realities they have faced in the postwar period. The data are rich and textured, in spite of the survey's closed-ended format. It allows for a careful analysis of the determinants of reintegration success, which we undertake in this article. But it also provides data useful for systematic examination of the strategies of the warring factions and the determinants of levels of violence, which are reported elsewhere (Humphreys and Weinstein 2006).

Measuring Demobilization and Reintegration Success

To reflect the multiplicity of goals associated with a DDR program, we generated four measures intended to capture distinct dimensions of reintegration.

Our first measure, DELINKED, captures the extent to which linkages with factions are maintained by the individual, recording whether the individual has successfully broken his factional ties. The measure combines an individual's responses to three questions.[4] The first asks individuals who they spend their time with. The second asks, were they to start a business, whom they would partner with. For each of these questions, one of the options available to respondents was "Friends that I met in my faction during the war." The final question asks individuals about the avenues they pursue when confronting ongoing, personal problems in the postwar period. The variable DELINKED takes a value of 1 unless (1) individuals spend most of their time with friends from their faction, (2) individuals reported that friends from their faction would be their preferred business partners, or (3) individuals felt that recourse to members of their faction was among the most effective ways to deal with their problems.

The second measure, EMPLOYED, indicates whether an individual has reintegrated into the workforce. The measure takes a value of 0 if the individual declared his or her present occupation to be unemployed or doing "odd jobs." For all other responses, this measure takes a value of 1.[5]

The third measure, DEMOCRATIC, focuses on the confidence ex-combatants express in the democratic system. The measure takes a value of 1 if an individual believes that exercising voice by voting in elections or approaching government officials, either locally or centrally, is the most effective way to deal with community problems. Alternative choices included protests, complaints to nongovernmental organizations, reaching out to traditional leaders or factional authorities, or taking up arms to fight.

Our final measure, ACCEPTED, records whether individuals reported facing ongoing difficulties in gaining acceptance from their families and communities. This measure takes a value of 1 if the individual reported no problems in gaining acceptance from family members and neighbors in the postwar period; if problems are reported, the measure takes a value of 0.

Table 2
Measures of Reintegration

Variable	N	Mean	SD	Mean | CDF	Mean | RUF	Difference
Delinked?	1,007	0.86	0.34	0.88	0.84	.04
						(1.52)
Employed?	1,037	0.84	0.36	0.90	0.79	.11***
						(4.62)
Democratic?	1,009	0.62	0.49	0.61	0.66	−0.05
						(1.57)
Accepted?	1,020	0.93	0.25	0.99	0.86	0.13***
						(8.50)

Note: CDF = Civil Defense Forces; RUF = Revolutionary United Front. T-statistics in parentheses. *significant at 10 percent. **significant at 5 percent, ***significant at 1 percent.

Summary statistics for these four measures are provided in Table 2. We see from the table that there is considerable variation across these measures in the extent to which individuals can be considered successfully reintegrated. In addition, Table 2 shows the average scores on each of these measures for each of the two major factions in Sierra Leone's war—the RUF and the CDF.

We find that 86 percent of combatants in our sample have broken ties with their factions, while 14 percent still consider faction members to be among their closest friends, most likely business partners, or as a primary source of support in the event of problems. The distribution of this measure is similar across the two major factions.

On employment, 84 percent of our sample report some form of permanent occupation. The problem of unemployment is found disproportionately within the RUF subsample; of these ex-combatants, 21 percent report having no present full time occupation.

Turning to democratic politics, we find that 62 percent of combatants express confidence in electoral politics or approaches to state/local officials as among the most effective ways to respond to problems in their communities. The remainder looks either to their old factions or more commonly to outside actors, typically the international community, as a means to effect change. Uniquely, fighters from the CDF are less likely to be reintegrated than RUF members by this measure, although the difference is not statistically significant.

Finally, on the measure of acceptance, we record the highest levels of successful reintegration, with 93 percent reporting no problems. While this measure supports the idea that across individuals in Sierra Leone, reintegration has proceeded with great success, the difficulties faced by 7 percent of respondents should not be underemphasized. If our sample were entirely representative of the ex-combatant population, this figure of 7 percent would correspond to approximately 5,000 former soldiers facing challenges in being accepted into civilian life. In fact, since our sample does not include those combatants that failed to reintegrate and elected instead

to continue fighting in Liberia or Côte d'Ivoire, this number plausibly underestimates the number of nonreintegrated fighters. Note, finally, that these acceptance rates differ markedly across factions. Nonacceptance rates are much higher within the RUF than within the CDF; while CDF members almost universally report no problems of acceptance, 14 percent of ex-RUF members report such problems.

Do these four indicators capture different facets of the same underlying phenomenon? Are individuals that are poorly integrated socially also less likely to find jobs? Are these same individuals more or less likely to maintain ties with their factions? If they were capturing a single underlying process of reintegration, we might expect to find that the measures are positively correlated with one another. Table 3 reports the pairwise correlations between these measures. Uniformly, we find that the correlations among the measures are low. Our measure of factional ties is weakly and typically negatively correlated with the other measures. Faith in democracy as a solution to local problems is also poorly correlated with the other dimensions of reintegration. The highest correlation is between the variables that measure whether combatants were accepted by friends and family and whether they found employment—the two measures most closely associated with membership of the CDF—yet even this correlation is well below 10 percent. These four measures, it seems, capture very different types of experiences.

To determine more formally whether or not these measures capture an underlying phenomenon of reintegration success, we estimate Cronbach's (1951) alpha measure of scale reliability. This measure, ranging from negative infinity to 1, provides an indication of the reliability of a scale generated from our four basic measures. Although there is no clear threshold for when a scale may be considered reliable, Nunnaly (1978) suggests that a cutoff point of 0.7 be used. We generate a reliability measure of .08, indicating that these measures of reintegration success cannot reliably be considered indicators of the same underlying phenomenon.

Reintegration on one dimension is typically not a good predictor of reintegration on another dimension. Although it would be simpler, both from the point of view of analysis and for policy reasons, to generate a single measure of reintegration success, the structure of our data suggests that this is not appropriate. Distinct processes may underlie each of these measures of reintegration success.

Individual, Group, and Community Correlates of Reintegration Success

We begin our analysis by considering the nonprogrammatic determinants of reintegration success. Consistent with the individual-level logic underpinning a security dilemma motivation for DDR programs, *ceteris paribus*, individuals that mistrust the intentions of other fighters should be less likely to leave the security of their units and reintegrate into civilian life. Consistent with the logic of the role of

Table 3
Correlation Matrix of Measures of Reintegration

	Delinked?	Employed?	Democratic?	Accepted?
Delinked?		−0.02	−0.02	0.06
Employed?	−0.02		0.06	0.07
Democratic?	−0.02	0.06		0.02
Accepted?	0.06	0.07	0.02	

spoilers in undermining peace processes, individuals that are dissatisfied with the terms of the peace have a greater incentive to hold out and disrupt a peace process rather than returning quietly to civilian life. While these logics may operate at multiple levels—at the level of armies or of individual units—we examine here whether evidence of these logics can be identified at the individual level.

To measure *distrust*, we asked respondents to describe their beliefs regarding the sincerity of different groups with respect to the implementation of the terms of the Lomé Accord: Did a given combatant believe that other fighters would respect the terms of the agreement, or did they expect them to renege? Our measure of distrust takes a value of 1 if an individual reported a belief that parties to the Lomé agreement were likely to renege on the agreement. In total, slightly more than 20 percent of combatants expressed such concerns (summary statistics for this and all other independent variables are provided in the appendix).

To measure *dissatisfaction*, we asked individual fighters which factions they believed received the best and worst deals from the Lomé negotiations. Approximately 30 percent of combatants claimed that their own faction got the worst deal; these combatants we classify as dissatisfied. Strikingly, this dissatisfaction measure does not correlate with factional affiliation. Thirty percent of RUF fighters felt that the RUF got the worst deal, while 32 percent of CDF fighters claimed that the CDF did worst.

These two measures are used to examine hypotheses 1 and 2 for each of our four measures of reintegration success. Table 4 reports the estimated marginal effect of each measure based on a probit analysis with fixed effects for each faction and clustering of errors by region.

The results suggest that distrust is a significant predictor of reintegration success on one of the four dimensions. Individuals that distrust the intentions of the other side are significantly less likely to place their trust in democratic processes to resolve their concerns. Distrusting individuals also appear less likely to have broken ties with their factions, although the result is on the margins of statistical significance. There is also a negative relationship between distrust and measures of employment and acceptance, although these relationships are statistically weak.

Dissatisfaction accounts for variation on one of four dimensions of reintegration success: it is associated with higher unemployment rates. Individuals that believed

Table 4
Nonprogram Determinants of Reintegration Success

	(1) Delinked?	(2) Employed?	(3) Democratic?	(4) Accepted?
Distrust	−0.030	−0.020	−0.157	−0.014
	[1.59]	[0.96]	[3.18]***	[1.02]
Dissatisfaction	−0.010	−0.044	0.039	−0.009
	[0.51]	[2.44]**	[0.86]	[0.83]
Age	0.002	0.000	0.002	0.000
	[1.78]*	[0.28]	[1.10]	[0.35]
Male	−0.089	−0.012	−0.004	−0.002
	[3.42]***	[0.23]	[0.06]	[0.20]
Mende	0.004	0.014	−0.006	0.014
	[0.22]	[0.64]	[0.13]	[1.93]*
Educated	0.005	−0.026	0.001	−0.001
	[0.40]	[2.00]**	[0.06]	[0.09]
Poor	−0.001	0.036	0.089	0.022
	[0.03]	[1.57]	[2.55]**	[1.92]*
Abducted	−0.000	−0.039	0.193	−0.000
	[0.01]	[1.29]	[4.39]***	[0.02]
Political support	−0.055	0.008	0.180	−0.028
	[2.22]**	[0.35]	[3.53]***	[2.59]***
Officer	−0.045	−0.021	−0.096	0.003
	[1.40]	[0.54]	[2.10]**	[0.20]
Abusiveness	0.009	−0.064	−0.217	−0.087
	[0.08]	[1.34]	[1.51]	[2.66]***
District wealth	0.049	−0.102	−0.109	0.005
	[1.40]	[1.99]**	[2.09]**	[0.33]
Community suffering	−0.056	−0.065	0.556	−0.082
	[0.43]	[0.30]	[1.94]*	[2.87]***
SLA	−0.017	−0.004	−0.073	0.032
	[0.21]	[0.09]	[0.56]	[1.93]*
AFRC	0.062	0.083	0.271	0.011
	[1.20]	[1.86]*	[2.47]**	[0.71]
CDF	0.068	0.057	0.027	0.082
	[1.25]	[1.65]*	[0.35]	[4.20]***
WSB	0.067	−0.024	0.085	0.018
	[0.70]	[0.29]	[0.68]	[0.97]
Observations	900	924	903	913
Pseudo R^2	0.03	0.07	0.07	0.23

Note: Robust z statistics in brackets. Marginal coefficient estimates (at mean values for the explanatory variables) from probit analyses reported. Faction fixed effects are included in all specifications. Revolutionary United Front (RUF) is the omitted category. All models allow errors to be clustered geographically at the chiefdom level. SLA = Sierra Leone Army; AFRC = Armed Forces Revolutionary Council; CDF = Civil Defense Forces; WSB = West Side Boys.
*significant at 10 percent. **significant at 5 percent, ***significant at 1 percent.

their group did badly from the political allocation of resources at Lomé have also fared badly in the postconflict economic environment. Consistent with the logic described in the analysis of spoilers, it may be that these individuals are slower to reintegrate economically because they are holding out for economic benefits from the political processes. It could also be, however, that this correlation reflects a mundane reporting bias: individuals that have failed to find employment may simply assess the political benefits received in the peace agreement more harshly.

Table 4 also reports the results of our empirical investigation of additional correlates of reintegration success. Two characteristics thought to be major factors in the reintegration process, age and gender, exhibit weak effects across dimensions. Consistent with the prevailing view that reintegration is harder for younger fighters, we find that older ex-combatants are more likely to have broken ties to their factions. But it turns out that younger combatants are no less likely to be accepted by their communities, to place their faith in democratic processes, or to have found gainful employment. We emphasize again that these results are conditional on faction fixed effects: there is, for example, a strong bivariate relationship between age and acceptance, with younger participants likely to have greater problems in reintegrating; but this relationship is not significant once we take account of fixed effects.[6]

Perhaps, surprisingly, given the extensive focus on the difficulties faced by female ex-combatants, we find significant differences between male and female ex-fighters on only one dimension. Female ex-combatants are more likely to have broken ties to their factions. The difference is substantively large and significant at the 99 percent level. While the qualitative literature has focused on the difficulties women face in the reintegration, our evidence suggests that gender has no measurable impact on most outcomes, except for the fact that men appear less willing to sever their ties to other combatants. Again, we emphasize that the results reported here condition on fixed effects: women are (on average) considerably more likely to report problems gaining acceptance, but since women are more likely to be members of the RUF, this relationship, though strong in a bivariate analysis, disappears when we account for faction fixed effects.[7]

Members of the Mende ethnic group—more strongly associated with the leadership of the CDF faction and the current ruling government—exhibit somewhat higher levels of acceptance (and this, conditional on CDF membership), although this fails to reach significance at conventional levels. Overall postconflict reintegration success does not appear to be strongly structured along ethnic lines.

The effects of (prewar) poverty and to a lesser extent, education, appear to be consistent across the indicators with less well educated and poorer individuals typically having more success in reintegrating. Poverty, measured at the individual level (using a dummy variable capturing whether the walls of the prewar home were constructed of mud and sticks), is associated with a higher likelihood of adopting democratic norms, gaining acceptance by community members, and (although not significant) finding employment. Strikingly, more educated ex-combatants

(our education measure takes a value of 0 for no education, 1 for at least some primary, and 2 for at least some secondary education) were less likely to find employment in postconflict Sierra Leone. We find no relationship, however, between an individual's socioeconomic status and the likelihood that they break ties with their factions.

In addition, we include a series of measures reflective of an individual's personal experience of the war. These measures include whether fighters were abducted into a faction, whether they joined because they supported the political causes of the faction, and whether they served as officers. Each of these variables is measured using a single question administered during the survey.

We find that although there is a strong, negative bivariate relationship (not reported) between whether an individual was abducted and his or her progress in gaining acceptance, the relationships are weaker once we condition on faction effects. We find a relationship between abductee status and reintegration rates on only one indicator: abductees were considerably more likely to turn to government for support rather than to rely on traditional, factional, or international sources of support. The relationship between political motivations for participation and our indicators of reintegration appears particularly complex. If individuals joined because they supported the cause of the group, they face more difficulty gaining acceptance in the postwar period and are more likely to remain attached to their factions. Strong believers, across factions, have a harder time readjusting to civilian life. Surprisingly, however, these individuals also appear to place the greatest faith in the electoral process.

Disturbingly, across most measures, higher ranking officers in the various military factions encounter more severe problems in reintegration. While these relationships are generally not significant, we do find a strong rejection of democratic processes among higher ranking officers.

The final measure of the individual's experience of the war captures a characteristic of the units in which they fought. Substantial differences exist in Sierra Leone across the fighting factions, but for the purposes of this analysis, we focus on one key group characteristic that is likely to affect an individual's prospects in the postwar period: the extent to which a unit was highly abusive toward civilian populations. To the extent that individuals committed heinous crimes against noncombatants, one might expect that they would face a more difficult process of gaining acceptance by community members and resettling into a nonmilitary way of life. Our measure used answers to eight related questions given by respondents who fought in the same area, for the same faction, during the same period of the war. The weights derived from a factor analysis were then used to create a single measure, *abusiveness*, which ranges from 0 to 1.[8] Controlling for faction-level fixed effects, this measure is strongly and negatively associated with an individual's reported ease in gaining acceptance. Individuals from nonabusive units exhibit acceptance levels nearly one standard deviation

higher than those from highly abusive units. The size of the coefficient is large and in a bivariate setting, accounts for about 9 percent of total variation in acceptance. This may be the result of the psychosocial impact of the conflict on individual fighters (Blattman 2006) or reflect the unwillingness of host community members to accept a returning fighter. We cannot distinguish between these explanations, although we emphasize that the result is independent of our estimate of the degree of abuse to which host communities were exposed. Although the effects are not significant, individuals from abusive units also have a more difficult time gaining employment and are less likely to place their faith in democratic processes.

Table 4 also reports results on two community characteristics that may shape the reintegration prospects of individual fighters. The first is an indicator of district wealth using data from the Sierra Leone Central Statistics Office. The index—which ranges from 0 to 1—uses factor analysis to combine measures of typical (imputed) rent payments in each district and an index of food poverty. Both use information gathered just as the war came to an end but before the survey was completed. The results suggest that on two of four measures, individuals who settle in wealthier locations face more difficulty reintegrating. They find it more difficult to find employment, and they are less likely to have faith in democratic processes. Plausibly, fighters relocated to wealthier districts, such as Freetown and the diamond mining areas, to improve their employment opportunities but with little success.

Finally, we develop a measure of how host communities experienced the war. A number of our respondents described to us how membership in a faction affected their experience in the postwar period not because of their personal characteristics but because of the reputation of the faction in the area where they lived. To estimate these effects, we calculate a measure of community suffering. This variable captures the average level of abusiveness of combatants who were operational during the course of the war in each of the chiefdoms. In computing these averages, we utilize the index of abusiveness for all fighters who declared themselves active in a chiefdom at any time during the war (even if these fighters did not subsequently attempt to reintegrate in those areas). We find that the degree of abuse of local communities during the war is powerfully related to the level of acceptance of ex-combatants. There is a weak positive relationship, however, with the degree of acceptance of democratic principles.

Overall, the results from Table 4 suggest that very different processes appear to underlie the different dimensions of reintegration. For each dimension of reintegration, we can identify a number of explanatory variables that emerge as relevant, but these are rarely the same variables across indicators and in some cases, the effects appear to work in different directions. Some differences remain across factions in the reintegration success of ex-combatants even after accounting for individual, group, and community correlates; specifically, fighters in the CDF are more likely

to be economically and socially reintegrated than those in the RUF. While theory provides us with few strong priors about the direction and magnitude of the impact of these various individual, group, and community characteristics, accounting for these effects is important as we turn now to examine our core hypothesis on the effectiveness of DDR programs.

The Impact of DDR Programs on Reintegration

To assess the effectiveness of the DDR program in facilitating reintegration, we use two measures of the treatment. Our first measure emphasizes participation in the program—87 percent of our sample joined the DDR program, while 13 percent elected to reintegrate on their own. This estimate of the rate of nonparticipation is somewhat higher than the official estimate, which suggests a total ex-combatant population (within Sierra Leone) of 84,200 (FAO 2004) and thus a participation rate of $76{,}000/84{,}200 = 90$ percent. In addition, we collected a second measure that captures whether an individual completed DDR. By this measure, 46 percent of our sample completed the DDR program at the time of our survey, and 54 percent had not. Combining both measures allows us to distinguish between the effects of initial and complete exposure to the DDR program.[9]

The results of our analyses of program effects are presented in Table 5. The table shows the marginal effect of exposure to each type of intervention on the likelihood of reintegration for each of the four indicators. In all cases, the models control for the collection of independent variables used in Table 4 along with faction fixed effects.

The most striking result is the lack of evidence that international programs are returning the benefits attributed to them, as observed at the individual level. Evidence from Sierra Leone does not support the hypothesis that participation in a DDR program increases the degree to which combatants are accepted by their families and communities. Nor is there a relationship between participation in these programs and democratic attitudes, the likelihood that an individual breaks ties with his or her faction, or the likelihood that he or she returns home. The only significant results work in the ''wrong'' direction: individuals that have entered DDR programs are less likely to place their faith in governmental structures, and those leaving DDR programs are significantly less likely to be employed. Further analysis of the data indicates that the negative employment result does not arise from the difference between those that completed the program and those that never entered but between those that completed and those that entered but did not complete. The most likely interpretation is that individuals consider themselves to have an occupation over the period in which they are active participants in the program. Other than the short-term occupation provided during training, however, we find no effect of exposure to DDR programming on the employment prospects of ex-combatants.

Table 5
Impact of DDR Program on Reintegration

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Program Variable	Enter DDR				Completed DDR			
Dependent Variable	Delinked?	Employed?	Democratic?	Accepted?	Delinked?	Employed?	Democratic?	Accepted?
DDR	−0.038	0.024	−0.050	−0.008	−0.011	−0.095	−0.017	−0.003
	[1.09]	[0.65]	[1.65]*	[0.69]	[0.42]	[4.05]***	[0.61]	[0.34]
Distrust	−0.029	−0.020	−0.157	−0.013	−0.030	−0.017	−0.157	−0.013
	[1.57]	[0.96]	[3.20]***	[0.99]	[1.57]	[0.80]	[3.17]***	[1.02]
Dissatisfaction	−0.009	−0.045	0.039	−0.009	−0.009	−0.037	0.040	−0.009
	[0.43]	[2.51]**	[0.85]	[0.79]	[0.48]	[2.09]**	[0.88]	[0.82]
Age	0.002	0.000	0.002	0.000	0.002	0.001	0.002	0.000
	[1.82]*	[0.30]	[1.08]	[0.40]	[1.80]*	[0.64]	[1.12]	[0.37]
Male	−0.089	−0.010	−0.015	−0.001	−0.089	−0.016	−0.005	−0.002
	[3.44]***	[0.19]	[0.21]	[0.14]	[3.43]***	[0.30]	[0.07]	[0.20]
Mende	0.002	0.016	−0.011	0.014	0.004	0.012	−0.006	0.014
	[0.08]	[0.71]	[0.26]	[1.88]*	[0.19]	[0.57]	[0.15]	[1.94]*
Educated	0.005	−0.026	0.002	−0.000	0.005	−0.025	0.001	−0.001
	[0.42]	[1.98]**	[0.08]	[0.06]	[0.41]	[1.96]*	[0.06]	[0.09]
Poor	0.001	0.036	0.089	0.023	−0.000	0.040	0.091	0.022
	[0.02]	[1.56]	[2.59]***	[2.04]**	[0.00]	[1.74]*	[2.63]***	[1.96]**
Abducted	0.002	−0.040	0.196	0.000	−0.000	−0.033	0.193	−0.000
	[0.05]	[1.29]	[4.47]***	[0.03]	[0.01]	[1.02]	[4.38]***	[0.02]
Political support	−0.052	0.007	0.183	−0.026	−0.055	0.014	0.180	−0.028
	[2.08]**	[0.27]	[3.66]***	[2.51]**	[2.18]**	[0.60]	[3.57]***	[2.57]**

Officer	−0.043	−0.022	−0.093	0.003	−0.044	−0.011	−0.094	0.003
	[1.35]	[0.56]	[2.01]**	[0.22]	[1.31]	[0.30]	[1.98]**	[0.20]
Abusiveness	0.001	−0.059	−0.229	−0.087	0.008	−0.066	−0.217	−0.087
	[0.01]	[1.17]	[1.61]	[2.64]***	[0.08]	[1.39]	[1.50]	[2.66]***
District wealth	0.038	−0.095	−0.124	0.002	0.047	−0.119	−0.111	0.004
	[1.04]	[1.69]*	[2.43]**	[0.15]	[1.35]	[2.62]***	[2.12]**	[0.27]
Community suffering	−0.050	−0.079	0.593	−0.084	−0.060	−0.084	0.551	−0.083
	[0.38]	[0.36]	[1.95]*	[2.92]***	[0.45]	[0.42]	[1.94]*	[2.86]***
Observations	899	922	902	911	900	923	903	912
Pseudo R^2	0.04	0.07	0.07	0.23	0.03	0.09	0.07	0.23

Note: Robust z statistics in brackets. Faction fixed effects are included, and the model allows errors to be clustered geographically at the chiefdom level. DDR = disarmament, demobilization, and reintegration.
*significant at 10 percent. **significant at 5 percent, ***significant at 1 percent.

Table 6
Impact of DDR Program on Reintegration by Type

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Program Variable	Enter DDR				Completed DDR			
Dependent Variable	Delinked?	Employed?	Democratic?	Accepted?	Delinked?	Employed?	Democratic?	Accepted?
DDR	−0.003	0.066	−0.068	−0.009	0.023	−0.088	−0.043	0.001
	[0.08]	[1.62]	[1.47]	[0.70]	[0.71]	[2.90]***	[1.23]	[0.13]
Distrust	0.051	−0.035	−0.099	−0.017	0.004	−0.028	−0.196	0.013
	[0.51]	[0.41]	[0.87]	[0.66]	[0.11]	[0.88]	[3.17]***	[0.72]
Distrust × DDR	−0.110	0.012	−0.062	0.004	−0.075	0.018	0.077	−0.073
	[0.80]	[0.14]	[0.51]	[0.15]	[1.29]	[0.44]	[1.24]	[1.79]*
Dissatisfaction	0.089	0.084	−0.059	−0.009	0.021	−0.019	0.025	−0.016
	[1.00]	[1.72]*	[0.56]	[0.32]	[0.85]	[0.63]	[0.41]	[0.86]
Dissatisfaction × DDR	−0.136	−0.180	0.108	0.000	−0.069	−0.032	0.032	0.011
	[1.17]	[2.59]***	[1.00]	[0.00]	[1.50]	[0.79]	[0.50]	[0.55]
Age	0.002	0.000	0.002	0.000	0.002	0.001	0.002	0.000
	[1.76]*	[0.22]	[1.09]	[0.40]	[1.77]*	[0.62]	[1.15]	[0.30]
Male	−0.087	−0.009	−0.014	−0.001	−0.088	−0.016	−0.006	−0.002
	[3.48]***	[0.18]	[0.21]	[0.14]	[3.46]***	[0.30]	[0.10]	[0.25]
Mende	−0.000	0.016	−0.011	0.014	0.006	0.012	−0.008	0.015
	[0.01]	[0.72]	[0.25]	[1.89]*	[0.31]	[0.57]	[0.18]	[2.02]**
Educated	0.005	−0.026	0.002	−0.000	0.005	−0.025	0.002	−0.001
	[0.41]	[2.01]**	[0.08]	[0.05]	[0.41]	[1.98]**	[0.09]	[0.16]
Poor	0.002	0.037	0.088	0.023	−0.001	0.039	0.091	0.023
	[0.05]	[1.58]	[2.57]**	[2.00]**	[0.04]	[1.73]*	[2.64]***	[2.15]**
Abducted	−0.002	−0.041	0.196	0.000	−0.001	−0.032	0.193	−0.001
	[0.04]	[1.34]	[4.47]***	[0.03]	[0.02]	[1.01]	[4.39]***	[0.06]
Political support	−0.052	0.008	0.182	−0.026	−0.056	0.013	0.182	−0.025
	[2.02]**	[0.32]	[3.68]***	[2.67]***	[2.21]**	[0.56]	[3.62]***	[2.29]**

Officer	−0.038	−0.015	−0.098	0.003	−0.041	−0.011	−0.096	0.003
	[1.19]	[0.38]	[2.08]**	[0.21]	[1.25]	[0.29]	[2.03]**	[0.28]
Abusiveness	−0.001	−0.068	−0.220	−0.087	0.016	−0.067	−0.223	−0.086
	[0.01]	[1.38]	[1.57]	[2.70]***	[0.16]	[1.41]	[1.54]	[2.59]***
District wealth	0.034	−0.098	−0.123	0.002	0.046	−0.120	−0.111	0.006
	[0.92]	[1.84]*	[2.37]**	[0.15]	[1.31]	[2.66]***	[2.10]**	[0.39]
Community suffering	−0.048	−0.072	0.587	−0.084	−0.055	−0.079	0.550	−0.089
	[0.37]	[0.33]	[1.92]*	[2.88]***	[0.42]	[0.40]	[1.95]*	[3.18]***
Observations	899	922	902	911	900	923	903	912
Pseudo R^2	0.04	0.07	0.07	0.23	0.04	0.09	0.07	0.24

Note: Robust z statistics in brackets. Faction fixed effects are included and the model allows errors to be clustered geographically at the chiefdom level. DDR = disarmament, demobilization, and reintegration.
*significant at 10 percent. **significant at 5 percent, ***significant at 1 percent.

Our discussion of the theoretical motivations for DDR suggested that the program might be particularly effective for some types of combatants. By providing a secure environment in which to engage with members of rival groups, we hypothesized that the DDR process would be especially beneficial for individuals that distrust the motivations of other groups. By providing material benefits, we anticipated that the program would have greater impact on combatants that felt the political process yielded a poor deal for their group. To examine these claims, we enter interaction terms into our specification in Table 6. These interaction effects allow us to observe whether the marginal effect of the program is greater for these populations.

Universally, we find that it is not. With one exception, the interaction terms are not significant, and in half of the cases, the estimated coefficients are negative (including the one significant finding). In our basic specification, we not only find no impact of the program in general, but we also fail to find effects among those populations for whom theory would predict the strongest effect.[10]

So far, there is little evidence of a relationship between participation in the DDR program and the degree to which ex-combatants have reintegrated in Sierra Leone. While the multidimensional peacekeeping operations in Sierra Leone may have been effective at the macro level, we cannot identify an impact for the DDR component of these programs at the micro level. As discussed previously, we must be cautious in interpreting these findings as evidence that the DDR process had no impact. It is possible that spillover effects, selection effects, and sample bias may undermine our ability to properly identify the causal impact of the program. We discuss each of these possibilities in turn.

Spillover Effects

Consider first the challenge posed by spillover effects. Arguably, the fact that nearly 90 percent of combatants in Sierra Leone participated in the DDR program may generate positive spillovers in communities that ease the reintegration of others, even if they did not participate in the program. A number of distinct mechanisms could underpin such spillover effects. For example, when a program seeks to separate ex-combatants from their factions, success in breaking any individual's ties to the network may undermine the network as a whole. A similar dynamic might take place with respect to acceptance by families and communities. In principle, combatants that did not take part in DDR programs may find their relationships with community members improved precisely because those combatants that did take part are successfully reintegrating with family and community members.

We test explicitly for such positive spillover effects by generating a measure of the percentage of soldiers in a given chiefdom that participated in the demobilization program.[11] While such geographically structured spillovers are not the only possible type of spillover (in particular, spillovers may occur between individuals that are structurally linked but geographically separate within an organization),

Table 7
Spillover Effects

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable	Delinked?	Employed?	Democratic?	Accepted?	Delinked?	Employed?	Democratic?	Accepted?
DDR	−0.029	0.005	−0.045	−0.015	−0.016	−0.098	0.081	−0.020
	[0.97]	[0.14]	[1.14]	[1.03]	[0.14]	[0.98]	[0.66]	[0.59]
Spillover	−0.068	0.131	−0.034	0.062	−0.054	0.012	0.071	0.054
	[0.55]	[0.92]	[0.31]	[1.79]*	[0.28]	[0.07]	[0.62]	[0.86]
DDR × spillover					−0.020	0.180	−0.171	0.012
					[0.13]	[1.11]	[1.11]	[0.18]
Distrust	−0.029	−0.020	−0.157	−0.014	−0.029	−0.018	−0.159	−0.014
	[1.59]	[0.96]	[3.21]***	[1.09]	[1.61]	[0.88]	[3.27]***	[1.09]
Dissatisfaction	−0.008	−0.046	0.039	−0.009	−0.008	−0.045	0.039	−0.009
	[0.41]	[2.54]**	[0.85]	[0.81]	[0.41]	[2.51]**	[0.84]	[0.83]
Observations	899	922	902	911	899	922	902	911
Pseudo R^2	0.04	0.07	0.07	0.24	0.04	0.07	0.07	0.24

Note: Robust z statistics in brackets. Faction fixed effects are included and the model allows errors to be clustered geographically at the chiefdom level. Additional controls include all variables in previous multivariate models (Tables 4 and 5). DDR = disarmament, demobilization, and reintegration
*significant at 10 percent. **significant at 5 percent. ***significant at 1 percent.

many plausible spillover mechanisms have a geographic component—in particular, spillover mechanisms that work through hypothesized impacts on host communities. Our measure of chiefdom exposure takes an average value of 0.87 and has a standard deviation of 0.16. In Table 7, we show the marginal effect of our estimate of the share of fighters in a combatant's community that did enter these programs on the likelihood of reintegration. In columns 1 through 4, the spillover term is entered on its own; in columns 5 through 8, the term is entered interactively with the program effect. This second specification allows us to examine directly the estimated impact of DDR spillovers on the treated and the untreated. In addition, it allows us to estimate the effect of treatment in the presence (and in the absence) of spillovers. We condition again on fixed effects for each faction and all of the correlates used in the analyses in Tables 4, 5, and 6.

On three of the four measures of reintegration success, we continue to find no evidence that DDR programs, either directly or through spillover effects, increase the likelihood of reintegration. Notably, we find a negative relationship on our measure of delinking, which is consistent with the negative relationship we find on the direct effect, although it is not significant at conventional levels.

We do, however, find some evidence of spillover effects of participation in DDR for acceptance. The relationship we identify, in model 4, is significant only at the 90 percent level but is substantively large. A hypothetical one-unit shift in the independent variable is equivalent to a comparison of a situation with no DDR program in a given chiefdom to one with universal coverage. Setting all other values at their means and assuming the relationship we identify here obtains also in hypothetical states of the world outside of our sample space,[12] the model suggests that individuals that do not join the program have a 92 percent (95 percent CI: 67 to 99.7 percent) chance of gaining acceptance if no individuals in their chiefdom join programs and a 99 percent probability if all individuals enter the program (95 percent CI: 95 to 99.9 percent). With 87 percent joining in a given chiefdom (our estimated take-up rate in the population), an individual not joining has an estimated 98 percent probability of gaining acceptance (95 percent CI: 94 to 99.9 percent). In other words, even though we find no evidence of direct effects, the estimated effect of complete coverage is, in principle, large enough to overcome the problems faced by ex-combatants in gaining acceptance. Note that in model 8, we do not find that the spillover effect from the treated to the untreated is greater than that from the treated to the treated (indeed it is slightly weaker). Moreover, the model fails to produce evidence for a positive direct effect of the program for any level of spillover.

Selection Effects

A second possibility is that we find no evidence of direct impacts at the individual level because there is a selection effect in operation. That is, the population of combatants who participated in DDR may be systematically different from those

that elected to reintegrate without external assistance. It may be that DDR took on the very difficult cases—such as hardcore members of the RUF—while the rank and file of the CDF (which was widely seen as victorious in the conflict) decided to return home on their own. Such differences, if unobserved and not controlled for in our models, might explain the nonresult. We emphasize, however, that precisely the opposite argument may be made for the bias introduced by selection effects. Plausibly, it is the difficult cases—those afraid of being identified by authorities or those unwilling to cooperate with the government—that refused to enter DDR, whereas those simply needing a means to reenter civilian life elected to participate. If such a selection effect were in operation, we would find that participants fared better than nonparticipants even if the program had no impact.

Whether our finding of no program effect can be attributed to selection then depends on what form of selection was in operation. One of the advantages of a survey approach is that we could ask individuals directly why they did or did not enter, allowing for an open-ended response.

The answers are revealing. In many cases, the answers do not suggest an obvious selection effect. In a number of cases, respondents reported that they had wanted to enter, but happened to be traveling or sick at the time. Other answers suggest that the selection effect is likely to work against finding evidence of an impact of the program. Some that didn't participate simply had other options; they reported having communities and jobs waiting for them. One claimed that he was "not interested because of the delay and the waste of time." Another explained that he did not register "because my parents were willing to assist me." Finally, some responses suggest a selection effect that would bias the results toward finding an effect of the program. Some refused to enter because of distrust or suspicion. "It was a waste of time because they were lying," one said. Another explained that "my husband threatened that the disarmament records were going to be used after four to five years to punish all those who took part in the war so I gave my weapon to another member of my unit to disarm."

Based on these open-ended responses, then, it is not clear that selection effects work either to hide or magnify a program effect. Beyond our qualitative examination of the determinants of selection, however, there are statistical approaches we can employ to explore the issue of selection more systematically. An optimal approach is to employ an instrument, but finding a variable that explains participation but is otherwise unrelated to reintegration success is difficult.[13] We concentrate here on another approach: using propensity matching estimators to compare outcomes across individuals with a similar propensity to take part in the program.

Propensity matching indicators estimate, for each individual, a probability of entering DDR based on all relevant available data. Based on these probabilities, the method matches pairs of individuals that have the same estimated propensity of joining, but one of whom did and the other of whom did not join. If our estimates for the propensity of joining are accurate, then for any pair matched in this way, we

can treat the difference in reintegration success for those that do join DDR and those that do not as a result of the fact of joining. The method, however, is only as good as our ability to predict joining probabilities, and selection effects may still obtain if unobservable characteristics of individuals simultaneously determine their decision (or ability) to enter the program and the likelihood of successfully reintegrating.

We employ propensity matching on our sample of respondents, using as predictors of joining DDR all of the explanatory variables in Tables 3 and 4 as well an indicator of their location at the end of the war. Beyond capturing key demographic features, this set includes conflict-relevant variables that we know in some cases to be related to reintegration success but that could in principle explain participation in the DDR program as well. As treatment variables, we examine both the decision to join and the completion of the DDR program. In addition, we compute estimates of the treatment effect for each of the outcome variables of interest.

In conducting each test, we generate information not just about the treatment effect, but also about the determinants of selection into the treatment. For the case of joining DDR, our explanatory variables account for approximately 24 percent of the variation across ex-combatants. We find that officers were more likely to enter the DDR program and more educated fighters were also more likely to participate. Strikingly, there is no clear evidence of factional differences in participation; nor is it the case that fighters from abusive units were any more or less likely to participate. Gender and age also have no impact. Much of the predictive power comes from location fixed effects rather than the measures of individual level attributes. We can account for only 11 percent of the variation in our completion measure. Officers and members of the Mende tribe were more likely to have completed the program. Older fighters were also more likely to have completed the programs, and there was some variation across factions, with CDF fighters more likely to have completed DDR relative to RUF fighters.

The results of our propensity matching analysis are given in Table 8. The table shows that even when we condition on all selection effects identified by observable characteristics, we continue to find no evidence that participation in DDR contributed to reintegration success on any dimension. To check that this nonresult is not driven by the particular method of propensity matching we employ (nearest neighbor with replacement), we also examined methods in which we match nearest neighbors without replacement and in which we condition on cases with overlapping support (not reported). In addition, we report results from matching based on Mahalanobis distance (Cochran and Rubin 1973). In this approach, we match each treated unit with the closest control unit where the distance is defined over our set of determinants of participation in the DDR program. Again, we find that even when accounting for selection effects in this way, we cannot recover evidence of program effectiveness.

Table 8
Selection into the DDR Program

Outcome Variable	Delinked?		Employed?		Democratic?		Accepted?	
Method	*A*	*B*	*A*	*B*	*A*	*B*	*A*	*B*
Treatment								
Entered DDR	−0.09	−0.04	−0.05	−0.01	−0.13	0.03	−0.01	−0.05
	[1.51]	[.83]	[0.43]	[0.13]	[1.09]	[.37]	[0.21]	[1.51]
Completed DDR	−0.06	−0.05	−0.10	−0.10	−0.03	0.00	−0.04	0.02
	[1.83]	[1.45]	[2.85]	[2.96]	[0.52]	[0.10]	[1.53]	[0.61]

Note: Each cell reports the estimated treatment effect on the treated (ATT). Estimated *t* statistics reported in brackets. Method *A* employs nearest neighbor propensity score matching with replacement; Method *B* uses full Mahalanobis matching. All estimates are derived using the PSMATCH2 module for STATA (Leuven and Sianesi. 2003). DDR = disarmament, demobilization, and reintegration.

Sampling Bias

Finally, we explore the possibility that the nonresult on program effects is driven by an imperfection in our sample of respondents. In particular, it may be the case that those ex-combatants that faced the greatest difficulty reintegrating were also the least likely to be enumerated when our survey teams came to the selected chiefdoms. Indeed, if the hard-core fighters from Sierra Leone that migrated to take part in the civil wars of neighboring countries did not demobilize and reintegrate and, in addition, were absent from our sample, it is possible that a sampling bias accounts for our nonfinding.

There are three important responses to this charge of sampling bias. We already signaled the first: if fighters took part in the DDR programs and subsequently left the country to fight in other wars, then the bias works in the opposite direction. If this effect predominates, then our estimate in fact overestimates program effectiveness. Relatively little is known about the fighters that left Sierra Leone to fight elsewhere in the subregion. Perhaps the most careful study of these fighters has been undertaken by Human Rights Watch in a report on West Africa's regional warriors. The report cites multiple instances of individuals that took part in the Sierra Leone DDR process and later moved to fight in Liberia with, in some cases, recruitment to the Liberia war linked to their participation in the Sierra Leone DDR process (Human Rights Watch 2005, 22-24).

Second, it may be that for individuals that wished to take part in the DDR process but were unable to participate, their lack of access to the program could have actively contributed both to their failure to reintegrate and their absence from the sample. Indeed, Human Rights Watch (2005) reports that, "the majority of those

[regional warriors] interviewed had negative experiences with the DDR program in Sierra Leone . . . the program's failure to engage them contributed to their decision to take up arms with another armed group'' (p. 49). In this case, including these individuals in the sample would lead to a more favorable measurement of the impact of DDR but only because of the adverse effect of the program on the untreated rather than its positive impact on the treated.

The third response relates to the fact that those populations not available to our enumerators likely reflect in part those same samples that were not available for the DDR program. If fighters left the country to pursue more lucrative soldiering options elsewhere and if this fact explains why they did not take part in DDR programs, then what appears to be a sampling problem in fact masks a selection problem. It is the lack of reintegration that explains their failure to participate in the program, not vice versa. Attributing their failure to reintegrate as evidence of the program's success is in this case a fallacy. Instead, our goal should be to estimate the impact of the program on the relevant population of potential program participants. If the same individuals that select out of the population of potential beneficiaries also select out of our sample, then in the absence of other selection effects, our estimate is not a biased estimate of program impact on the relevant population.

In short, sampling biases could have effects in either direction; the qualitative evidence suggests, however, that the bias is likely either to result in an overestimation of the positive effects or an underestimation of the adverse effects of the program on the untreated. Together, these three considerations—spillover, selection, and sampling—point to the complexity of interpreting simple two-sample comparisons in the absence of a randomized intervention. We now turn to alternative designs that can surmount these challenges.

Improving the Measurement of the Impact of Humanitarian Intervention

In our study of program effects, we find little evidence that participation in the DDR program increased the likelihood that combatants successfully reintegrated. Our examination of the three major threats to the validity of our findings, however, underscores just how difficult it is to identify a program's causal effects in the absence of an experimental design. We believe that it is not appropriate, based on the results presented here, to conclude that the DDR program had no positive micro-level impacts in Sierra Leone. Nonetheless, the nonfindings should be seen as a wakeup call to advocates of these programs. Needed now is a method that is better suited to identify causal impacts in the presence of the confounding effects we have discussed. The best approach involves the development of monitoring and evaluation systems that employ some form of randomized intervention.

If scholars and policy analysts are to disentangle the effects of demobilization programs from the range of other initiatives launched as part of multidimensional peacekeeping operations, DDR programs must be designed in such a way that the reintegration trajectories of participants can be usefully compared with those of nonparticipants, when both groups are identical except for the treatment itself. With an appropriate treatment and control group, selected by lottery, data collected after the DDR program is completed (for the treatment group) can be used to answer the relevant counterfactual, how successfully do ex-combatants reintegrate if they do not participate in the program? Selection effects then no longer represent the threat to valid measurement of program effects described above.

How can practitioners identify an appropriate control group for an experimental study of DDR programs? It does not make sense in a postconflict environment to simply deny treatment in the demobilization process to a group of combatants for the purpose of evaluating the program's impact. To overcome this problem, we advocate an experimental approach that randomizes the timing of participation in DDR programs. The core idea is that although all ex-combatants will eventually participate in the program, the timing of their entry into the program will be determined by lottery, allowing for comparisons of program participants and nonparticipants (that is, combatants that have not yet entered the program).

This approach to evaluating the effects of DDR is increasingly feasible as the United Nations and its partners have moved beyond an individual-level program model in which combatants register for cash payments and training to include more community-focused forms of demobilization and reintegration in which assistance is provided to communities with significant populations of ex-combatants. With this new model, the rollout of UN program efforts is constrained by logistical considerations and staffing capacity, necessitating some delays in the extension of programs to different communities. Any limit on the feasibility of providing a treatment to the full sample of recipients at a single point in time provides the opportunity for an experimental design. It is reasonable to consider making decisions about which communities should receive the program first by lottery to ensure fairness, although some localities might be excluded from the process of random selection if their situation is particularly dire. With a sufficient number of communities included in the lottery, identifying program effects by comparing participants to nonparticipants is feasible. Moreover, by conducting such an evaluation using a community-focused approach, both individual effects (such as those measured in this article) and community-level outcomes, such as levels of conflict, can be assessed in the same design.

Beyond selection issues, a randomized design can also help to mitigate problems posed by spillover effects and sampling bias. With DDR programs implemented community by community and stratified by region within a country, one can use the (exogenous) variation in the density of treatment communities in a given geographic region as the program is phased in, to empirically estimate the positive (or

negative) externalities experienced by communities not yet included in the program (following Miguel and Kremer 2004). Other nongeographic spillover effects may also be studied with random sources of variation in treatment at the individual or unit level. Of particular interest because of the potential they hold to shed light on organizational cohesion and resilience are spillovers that occur through vertical and horizontal organizational channels.[14]

An additional advantage of a randomized intervention is that the population for which inferences can be made is well defined and identified. It is precisely that population over which the randomization in the allocation of the treatment is undertaken. Although individuals that are not available for program assignment may fare worse systematically than program participants, the marginal impact of the program on the relevant population can be estimated without bias. In addition, although individuals that take part in the program may subsequently exit a sampling frame, a randomized design with follow-up allows for this fact to be observed and accounted for.

While the discussion above underscores how randomized intervention can be useful for assessing the overall *impact* of DDR programs on individual and community-level outcomes, the logic can also be extended to questions of program design. Indeed, UN officials grapple consistently with questions that can be addressed in this manner: how much power to provide in community-level initiatives to ex-combatants versus community leaders; what role to reserve for women in the leadership and management of community efforts; and what forms of assistance are likely to be most beneficial for ex-combatant reintegration.

Conclusion

With the growing involvement of external actors in postconflict situations, increasing attention is being dedicated to the challenges of peace building. The disarmament, demobilization, and reintegration of combatants is a central component of efforts to reestablish legitimate governance and prevent the recurrence of conflict. Yet in spite of nearly a decade of involvement in demobilizing warring factions, there is little evidence about the factors that explain whether individuals can successfully reintegrate after conflict and the precise causal impact of externally funded programs to reintegrate combatants. Instead, the scant literature on demobilization has focused attention on details of program design and implementation.

In a first attempt to correct for these shortcomings, we present the results of a large *N* survey of combatants in Sierra Leone, which allowed us to track the progress of DDR participants and nonparticipants in the postwar period. Our findings provide insights useful to practitioners of postconflict reconstruction. At the individual level, we find that contrary to conventional wisdom, there is little evidence that women or young people faced a significantly harder time gaining acceptance

into civilian life, finding employment, breaking ties to their factions, or adopting the new democratic political system. Instead, aspects of an individual's experience of the conflict seem to exert more powerful effects. Higher ranking officers are considerably less likely to exhibit faith in democratic processes, and the abusiveness of the unit in which an individual fought is strongly associated with problems in gaining acceptance, even controlling for unobserved attributes correlated with membership in the different factions. The implication is that aspects of a combatant's wartime history should be taken into account more prominently in the design of DDR programs.

Perhaps the most surprising result, however, is that we find little evidence that UN operations were instrumental in facilitating DDR at the individual level. Nonparticipants in DDR do just as well as those who entered the formal demobilization program. Without a complete handle on spillover effects, selection effects, and sample bias, however, we argue that these negative results should be treated with caution. The results may suggest that other factors—measurable only at the country level—may have been far more important for determining the path of reintegration than the DDR programs that were implemented in Sierra Leone. In particular, the fact that the war ended decisively with a major military intervention by the British may be consequential for the high rates of reintegration success both among soldiers formally demobilized and those who returned home on their own. Alternatively, it may be that the effects of DDR programs only become apparent after longer periods of observation or that the impacts are more apparent at the level of communities rather than individuals. Finally, it may be that genuine medium-term program benefits existed but were not sufficiently large to overcome spillover, selection, or sampling biases. In any event, our study suggests that the impact of the program is not identifiable using the methods we have at our disposal. Policy makers concerned with demonstrating the efficacy of DDR programs will need to employ more robust strategies for identifying program effects, specifically randomized intervention.

More generally, in the absence of sufficient cross-national variation to allow for an assessment of the impact of individual components of multidimensional peacekeeping operations, we advocate an approach that exploits within-country variation. The advantage of a micro-level approach is that it can increase our confidence that the mechanisms attributed to work in a given case indeed function as believed. By exploiting subnational variation, we can work out with greater confidence whether a program is effective but also for whom a program is failing. A disadvantage, however, is that the external validity of the results may reasonably be called into question. If there is little evidence that DDR programs were effective in Sierra Leone, this does not mean that DDR programs are never successful. While the Sierra Leone case is an important case—regarded as a success story, elements of the Sierra Leone model are being replicated in neighboring Liberia, in Burundi, and now as far away as Haiti—it should still be seen as a single data point in a

larger model that attempts to explain cross-national variation in program effectiveness. Single-country experimental designs need to be complemented by attention to country-specific factors that impact the trajectory of DDR programs. To understand how DDR contributes to successful peace building, subnational studies represent only the building blocks for a richer analysis of how external interventions affect postwar trajectories.

Appendix
Summary Statistics (Independent Variables)

Variable	N	Mean	SD	Min	Max
Distrust	976	0.19	0.39	0	1
Dissatisfied	978	0.31	0.46	0	1
Age	1,039	31	10	14	77
Male	1,035	0.89	0.31	0	1
Mende	1,037	0.53	0.50	0	1
Abducted	1,043	0.36	0.48	0	1
Political support	1,043	0.39	0.49	0	1
Educated	1,043	1.08	0.86	0	2
Poor	1,040	0.68	0.47	0	1
Officer	1,040	0.12	0.32	0	1
Abusiveness	1,024	0.20	0.21	0	1
District wealth	1,043	0.28	0.32	0	1
Community suffering	1,043	0.20	0.12	0.03	0.53
Entered DDR program	1,038	0.87	0.34	0	1
Completed DDR program	1,039	0.46	0.50	0	1
Share in chiefdom demobilized	1,043	0.87	0.17	0.18	1

Note: DDR = disarmament, demobilization, and reintegration.

Notes

1. A number of studies exist for the case of Sierra Leone, including Comninos, Stavrou, and Stewart (2002), Ginifer (2003), Richards et al. (2003), and Stavrou et al. (2003).

2. An obvious concern with survey work is truth telling. Respondents may have strong incentives to misrepresent the facts. In the training, a script was developed for enumerators to help allay these concerns. It was also important that survey teams administer the survey in private in an effort to protect people's privacy, that anonymity was preserved throughout, and that questions of an incriminating nature were not asked.

3. The data provided by the National Commission on Demobilization, Disarmament, and Reintegration (NCDDR) for the distribution of ex-combatants were incomplete. We have since received data made available by the Food and Agricultural Organization that provide a more complete sample frame for ex-combatants in Sierra Leone and which allow for the possibility to reweight our data ex-post. The core results on programmatic effects and spillovers presented in this article, however, are invariant to ex-post adjustments in sampling weights.

4. To examine the precise wording of this question and all other questions used in the analysis, see the survey instrument, available online at http://www.columbia.edu/∼mh2245/SL.htm.

5. The employment variable is coded based on a question about the respondent's occupation rather than whether individuals have a job. When asked about their occupation, only 12.5 percent indicate that they have no employment whatsoever. Twenty-three percent report farming as their primary occupation; 16 percent are artisans; approximately 5 percent are traders. Plausibly, if one asked most of these individuals whether they have a job, they would say no. Insofar as jobs are thought of as formal sector occupations, a broader definition of unemployment than the one we use—to include those in the informal sector and the underemployed—might yield substantially different results.

6. This finding should be interpreted with caution. Human subjects' concerns prevented us from interviewing soldiers who were children at the end of the fighting. Nonetheless, our sample includes a substantial proportion of individuals who joined the factions as children and were over eighteen when the war came to an end.

7. Note, however, that if nonintegrated women are more likely to conceal their involvement in the conflict and thus more likely to be absent from our sample, then we would overestimate reintegration success among women ex-combatants.

8. The measures used to construct the index include three distinct types of questions. First, we include questions that assess whether the environment was permissive of abuse using a measure of the likelihood that an individual would be punished for stealing, amputating, and raping a civilian if these were done without the express order of a commander. Second, we add questions about the ways in which food was collected, including whether food was taken forcibly or through more contractual arrangements. Finally, the index includes the respondents' evaluation of actions undertaken by the group for the benefit of civilian populations, including educational and ideological training. Two versions of the index are constructed, one using individuals in the same chiefdom and same faction and one using individuals in the same district and subfaction. The variable used here averages across these two indices. Note that the indices combine negative sanctions (violence, forcible food collection) and positive benefits (security, education). Although in some cases, the logics behind these positive and negative strategies may differ, results in previous work with this variable are robust to disaggregation. For more information on this measure, see Humphreys and Weinstein (2006).

9. In a recent analysis of ex-combatant reintegration in Liberia, Pugel (2007) emphasizes a further distinction among participants in the disarmament, demobilization, and reintegration (DDR) program. He finds that on some outcome measures for Liberia, differences arise between those that have accepted a reinsertion benefit and initiated a skills training program and those that have a reinsertion benefit but have not started any training. In the Sierra Leone data, however, we continue to find no evidence of a program impact of DDR when making these distinctions among program participants (results not shown).

10. We look also for heterogeneous treatment effects across the five major factions. There is weak evidence in a bivariate specification that Revolutionary United Front (RUF) combatants who participate in DDR are more likely to express faith in the democratic process, but we cannot reject the null hypothesis that the aggregate impact of entering DDR on democratic beliefs for the RUF is zero. We also find some evidence that DDR participants from the RUF are more likely to be employed; this effect, however, is reversed when the completion of DDR is used as the treatment variable, reinforcing the fact that employment reflects participation in skills training and nothing more permanent. In short, we find no evidence that DDR was particularly effective for some armed groups and not for others.

11. In testing for spillovers, we focus on entrance into the DDR program as the treatment, although the results are substantively similar if we use completion of the program instead. We note that better tests for externalities can be used in settings with random assignment of the treatment and more geographically precise data. Under such conditions, exogenous variation in the density of treatment units across space can be used to empirically identify spillover effects (see Miguel and Kremer [2004], for example).

12. We emphasize that this is a strong assumption. In fact, this hypothetical comparison requires making statements about a part of the space that we never observe: within our sample there are no chiefdoms with 0 percent demobilized. As noted by King and Zheng (2006), the comparison then depends strongly on our assumption regarding the functional form of our model.

13. To employ instrumental variables estimation, we constructed an instrument based on the distance between where an individual fought in the closing stages of the war and the closest DDR site. This instrument is plausibly related to whether an individual joined DDR in terms of the costs of moving oneself to a DDR site. We constructed a second instrument that records the distance between the nearest DDR site and an ex-combatant's preconflict home. While it is plausible that remoteness is not otherwise related to acceptance, one could imagine arguments that suggest a violation of the exclusion restriction for this instrument. Our results using both of these instruments, not reported here, do not provide new evidence supporting a link between DDR and successful reintegration.

14. We caution that as noted in section 2 above, some forms of spillover that work through organizational structures may not be identified using our proposed randomization procedure. As an example, consider a situation with strong structures of command and control in which commanders permit effective demobilization only when all combatants have been admitted to a demobilization program. In such a case, the effect of treatment on the treated may only be observable once the control group is also treated.

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