Can employment reduce lawlessness and rebellion? A field experiment with high-risk men in a fragile state*

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Abstract

In fragile states, job programs are designed to deter high-risk men from crime and violence. These programs assume that supplying skills and capital stimulates lawful employment, that employment deters illegal or violent work, and that employment will increase socio-political integration. Rigorous, individual-level evidence for these assumptions is rare. We evaluate a program of agricultural training and capital for Liberian ex-fighters. The agricultural skills and capital increased returns to lawful employment. Consequently, the men were 24% less engaged with mercenary recruiters during a neighboring war. They also shifted hours from illicit work (e.g. illegal mining) to agriculture by 20%. Some men did not receive the capital inputs and expected a cash transfer instead. Expecting future transfers was especially influential in deterring illicit and mercenary work. We see no evidence, however, that employment affects non-material violence or socio-political integration. The findings challenge strategies for employing and rehabilitating high-risk men.

Keywords: post-conflict, crime, violence, reintegration, mercenaries, employment, training, agriculture, field experiment

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1 Introduction

After war, one of the key problems faced by governments and peacebuilding forces is what to do with poor and unemployed young men, especially high-risk men such as ex-fighters. Poor economic opportunities could mean they are easier to re-recruit into rebellious groups, increasing the risk civil war recurs (Walter, 2004; Blattman and Miguel, 2010). They pose other security risks as well. One is election violence. In Sierra Leone and Burundi, for instance, political parties paid ex-fighters to intimidate opposing parties and voters (Christensen and Utas, 2008; Amnesty International, 2014). Another is crime. Former paramilitaries in Colombia, for example, have been recruited into criminal bands (Nussio and Oppenheim, 2014). And, as this paper describes, ex-fighters in Liberia were drawn into illegal resource extraction and mercenary work.

As a result, nearly every fragile state funds some form of public works scheme, training, or other employment intervention for young men (del Castillo, 2008; World Bank, 2012). It is also the reason most demobilization, disarmament, and reintegration (DDR) programs have a heavy employment component. But can post-war employment programs turn swords into ploughshares?

Post-conflict reintegration and employment programs are usually rooted in three assumptions: first, that states can stimulate lawful employment by supplying inputs such as training or capital; second, that more lawful employment will decrease incentives for re-recruitment or other violent work; and third, that increasing employment will lead men to become more socially and politically integrated into society. This paper investigates all three claims via a field experiment in Liberia.

The first assumption, that supply-side interventions can stimulate self-employment, is plausible but uncertain. Using a standard model, we outline the conditions under which men have high returns to transfers of human capital (skills) or physical capital (e.g. capital inputs). A growing body of evidence suggests that the average poor person has high returns to capital inputs and sometimes to skills, in large part because they are able but credit

constrained (Banerjee and Duflo, 2011). High-risk men in fragile states are not average, however. They have a comparative advantage in violence, and they often lack the human, social, and physical capital to succeed in peacetime labor markets (Tajima, 2009). If so, their returns to skills or capital transfers could be low.

The evidence on high-risk men is limited and somewhat inconclusive. One source is observational studies of DDR programs, most of which report low or inconclusive effects on employment and political reintegration (e.g. Humphreys and Weinstein, 2007; Levely, 2011; Gilligan et al., 2012).[1] By their own admission, however, most DDR programs are poorly executed (Kingma and Muggah, 2009; Tajima, 2009). Also, often the primary goal of DDR is to get a peace agreement signed, and it might not be reasonable to expect sustained economic reintegration.

The second assumption, from employment to more lawful work, is rooted the idea that fighters are rational and that crime and rebellion rise and fall with the opportunity cost of participation (e.g. Becker, 1968; Popkin, 1979). While persuasive, there is little rigorous, individual-level evidence outside the United States (US). Most evidence comes from observational, meso-level data. In developing countries, it comes mainly from country- and district-level analysis of income shocks on crime and conflict.[2] Similarly, in developed countries, studies also suggest city-level crime rates fall as wages rise.[3] Nonetheless, there are limits to testing theories of individual behavior with meso-level data, especially because income shocks affect the incentives of rebel groups, states, and civilian populations in addition to potential recruits.[4]

[1] In Burundi, Gilligan et al. (2012) compare men in an unserved DDR region to men in two served regions, and see that men in the program region have greater incomes.

[2] There is evidence that weather and trade shocks intensify ongoing wars (e.g. Bazzi and Blattman, 2014; Miguel et al., 2004; Dube and Vargas, 2013) and municipal-level drug production in Mexico (Dube et al., 2014).

[3] See Freeman (1999) for a review. More recently, a small body of evidence shows that residential job training programs have at least short term success in reducing crime and increasing incomes, but that these effects may be short-lived (Heckman and Kautz, 2013). There is concern that the problem is with the residential approach rather than the job training itself.

[4] For instance, income shocks could affect conflict and crime because they lower policing or counterinsurgency capacity. Aggregate shocks may also affect the recruitment strategies of armed groups or incentives to pillage. Finally, weather and other income shocks could incite conflict by inducing migration (such as

Also, some scholars doubt that job programs meaningfully reduce crime and violence. Not all criminal activities have a high opportunity cost of time, and insurgent groups might not be labor constrained (Berman et al., 2011). Moreover, studies of criminal and revolutionary organizations suggest that the key motivator might not be wages but rather status, ideology, outrage, or a desire for justice. For example, Levitt and Venkatesh (2000) argue for the symbolic value attached to upward mobility in US drug gangs. Scholars of revolution argue that injustices, whether violence or economic marginalization, generates outrage and with it an intrinsic satisfaction from crime, protest, or rebellion (e.g. Merton, 1938; Gurr, 1971; Wood, 2003).

Finally, the third assumption, from employment and incomes to socio-political integration, is intuitively plausible but has no firm basis in theory or evidence.[5] The little evidence is pessimistic. One of the few employment interventions to measure these outcomes, a post-war cash transfer program in Uganda, finds large employment and income gains but little change in socio-political behavior (Blattman et al., 2014). Gilligan et al. (2012) reach similar conclusions with a DDR program in Burundi. To date, however, there is no experimental evidence with high-risk men.

To build this evidence, this paper experimentally evaluates a program that provided agricultural training and capital inputs to high-risk men in post-war Liberia. Liberia's war ended in 2003, but in 2009 thousands of ex-fighters still illegally occupied rubber plantations, illicitly mined gold and diamonds, or illegally logged. They and other high-risk men clustered in "hotspots" where the state had little control, such as plantations and remote mining camps. These represented some of the state's most valuable natural resources, and their unlawful occupation and extraction hindered the country's post-war recovery. In addition, these men regularly threatened the peace. A 2008 coup in neighboring Guinea fueled rumors of recruitment of Liberians as mercenaries, and there were regular violent clashes between the

pastoral people moving to settled lands) or increasing struggles for resources like water.

[5]In principle, poverty and unemployment could drive grievances or anomie that dissociate young men from mainstream society.

state and plantation squatters.

To shift men away from illegal activities and mitigate future mercenary recruitment, the non-profit Action on Armed Violence (AoAV) designed a program with four components: several months of residential agricultural training; counseling and "life skills" classes; relocation assistance after graduation; and startup materials worth $125. The residential, multifaceted nature of the program resembles many US programs for high-risk adolescents (Heckman and Kautz, 2013). AoAV recruited over 1100 high-risk men in 138 hotspot communities. Roughly half were randomly offered the program, and nearly three quarters of these attended.

14 months after training, we observed several impacts. First, even the highest risk men were overwhelmingly interested in farming. This itself was surprising, and runs against the conventional wisdom in many DDR and employment programs.

Second, treated men shifted their hours of work away from illicit resource extraction towards farming by roughly 20%. Almost none exited illicit work completely, however. Rather they shifted their portfolio of occupations. Their incomes increased modestly as a result, by about $12 a month.

Third, when an election crisis in Côte d'Ivoire led to a short war next door, between 3 and 10% of men in the control group reported actions such as interacting with recruiters or moving to the border towns where recruits were massing. None of our sample went to fight since the war ended abruptly. But treated men were about a quarter less less likely to report engaging in recruitment activities.

Third, despite these economic successes, the program had little effect on peer networks, hierarchical military relationships, aggression, participation in community life and politics, or attitudes to violence or democracy.

Fourth, future economic incentives seem to be at least as important in deterring illicit and mercenary work as past provision of inputs. Roughly a third of treated men did not receive their package of farm inputs because of unexpected supply issues. At the time of the

survey, AoAV had told these men to expect to receive a cash equivalent in the near future. Men would miss the transfer if they left their farming villages to fight abroad or mine in remote areas, meaning the cash transfer was de facto conditional on not doing these illegal activities. We use arguably exogenous variation in the receipt of inputs (and the expectation of a future transfer) to identify the effect of this cash incentive, and find that it explains a large portion of the reduction in illicit mining and much of the reduction in mercenary activity.

One caveat is that all outcomes are self-reported. The treated could be more likely to report desirable outcomes, overstating treatment effects. We argue this is improbable given the pattern of outcomes we see—no treatment effect on the anti-social behaviors that were targeted by the program (e.g. interpersonal violence) and large effects on behaviors ignored in the curriculum (illicit mining). Also, as we discuss, illegal behaviors were not underreported in a similar, urban ex-combatant program in Liberia. But misreporting is a risk.

These results have implications for understanding the social and economic rehabilitation of high-risk men. First, we see little link between employment and incomes and socio-political behavior. Even direct attempts to change social behavior and community integration through counseling and life skills training had little impact. We discuss why this might be the case and what other programs have better track records of success.

Second, we see that the men did have positive returns to a supply-side intervention of capital and skills. Though, given the high cost of the intervention, a gain in $12 per month is not a high return on investment. Cost-effectiveness thus hinges on the hard-to-quantify social returns to lower crime and violence.

We also provide the first experimental evidence in a developing country that incentives for crime and mercenary work respond to changing returns to lawful work. While this is not necessarily a shocking result, so we learn important lessons about illicit labor markets. Agriculture is an attractive alternative, at least in Liberia. We also see a relatively large

shift in illicit employment for a relatively small increase in daily incomes (about 40 cents a day), suggesting that the labor supply between the illegal and legal sectors is fairly elastic. Furthermore, we illustrate how simultaneous employment in legal and criminal sectors is commonplace and a rational response to risk and imperfect insurance. Thus it is easier to deter criminal activity on the intensive margin rather than get men to exit altogether.

The promise of future payouts, moreover, appears to have deterred men from an imminent mercenary opportunity and from illicit resource extraction. The potential policy implication is that one-time transfers will not fully deter future criminal or mercenary opportunities. Ongoing incentives, such as cash-for-work programs or other conditional transfers, could be important complements.

These findings also speak to one of the great debates in the study of rebellion, the rationality of peasant political mobilization. Our evidence can't speak directly to the role of non-material incentives in recruitment, such as grievances, moral economy, or group-based identity, but our results suggest that agrarian ex-fighters act at least in part like rational agents, and that relatively small incentives can significantly reduce interest in mercenary opportunities. The advantage of this Liberia case is that offers a relatively clean case to isolate the opportunity cost mechanism, since non-material incentives were relatively unimportant in the illegal work and mercenary opportunities facing our sample.[6] Ideally, future research would be able to compare the role of material incentives in deterring armed recruitment with and without non-material incentives.

2 Intervention and experiment

From 1989-96 and 1999-2003 two civil wars wracked Liberia. They killed nearly 10% of Liberia's 3.5 million people, displaced a majority, and recruited tens of thousands of young men into combat (Republic of Liberia, 2008). Since 2003, however, Liberia has been at peace

[6]Also, the socialization component of the intervention had almost no impact on social networks or values, and so we can be more confident the reduction in crime and mercenary activity come from a change in opportunity cost rather than a change in peers, norms, or values.

under a United Nations (UN) peacekeeping force. While still one of the poorest countries in the world, before and during the study period it experienced robust growth.

By 2008, the government and UN estimated roughly 9,000 ex-fighters living in remote "hotspots" were engaged in illegal resource extraction, including alluvial gold and diamond mining, logging, and rubber tapping (Republic of Liberia, 2008). The government was eager to curb resource theft so that the concessions could be licensed and taxed, typically to foreign firms. There were the important sectors in the Liberian economy and ceasing the illegal use of concessions was one of the most important aspects of reconstruction. Peacekeepers also viewed these men and the hotspots as threats to regional peace. For decades, regional conflicts have been fueled by cross-border mercenary recruitment of men like these.

2.1 The program

As a result, one of the highest priorities was to create more stable jobs for ex-combatants and other high-risk men. To do so, AoAV designed an agricultural employment intervention. They rebuilt and operated two training centers, one in central Bong County, and one in the eastern Sinoe County. Their program had four main components:

1. A residential training program (three months in Sinoe and four months in Bong) with coursework and practical training in rice and vegetable farming, animal husbandry, rubber and palm cultivation, and basic literacy. In residence, AoAV also provided meals, lodging, clothing, and basic medical care and personal items.

2. Integrated into the residential program was an explicit attempt to socialize men to peacetime life, principally via a "life skills" class. It met three times a week in groups of 20 and followed a locally-developed manual and semi-scripted lectures and group discussion. It was led by facilitators who were ex-combatants themselves, and focused on: reframing and understanding wartime actions; dealing with symptoms of traumatic stress; managing anger; being an effective community member; and resolving disputes

peacefully. There was also informal out-of-classroom mentoring by the facilitators.

3. After graduation, transport to a community of their choice, coordinating with the community for access to farmland.

4. A two-stage package of tools and supplies tailored to the interests of the trainee, such as vegetable farming or pig/poultry raising. This package cost roughly $125, but the expense of buying similar items in remote areas increased its real value. Men received the first half upon graduation and the second half several weeks later, if AoAV could locate them and confirm they had initiated farming or animal raising. In addition, Sinoe graduates were given $50 cash. This was not part of the program plan but was negotiated after a miscommunication during recruitment.

AoAV estimated the cost (excluding fixed costs such as training site construction and head office expenses) to be roughly $1275 per person in 2009.[7]

The government and UN peacekeeping force used the exit of ex-combatants from the enclaves to increase state control of the area, which typically meant a civilian administrator, periodic UN peacekeeper patrols, and the preparations to sell mining or rubber tapping licenses to small and medium firms.

2.2 Target sites and population

For the Sinoe site, AoAV recruited in 35 communities on and around the Sinoe Rubber Plantation. A few months before, it had reverted to state control after the expulsion of a former rebel general from control. Hundreds of squatters, mainly non-ranking ex-fighters and their families, still remained.

For the Bong program, AoAV recruited in 103 communities in three regions. First, several dozen remote villages and mining camps in Gbarpolu County—one of the most isolated counties, known for illicit logging and mining. The camps were magnets for opportunistic

[7]Appendix A (enclosed) provides a full curriculum and budget.

youth and had many ex-fighters, some led by ex-commanders. Second, they recruited in 12 villages and towns in and around Ganta, a small city bordering Guinea, where at the time of registration there were reports of mercenary recruitment in response to a Guinean coup. Third, they recruited ex-combatants from villages near the training site.

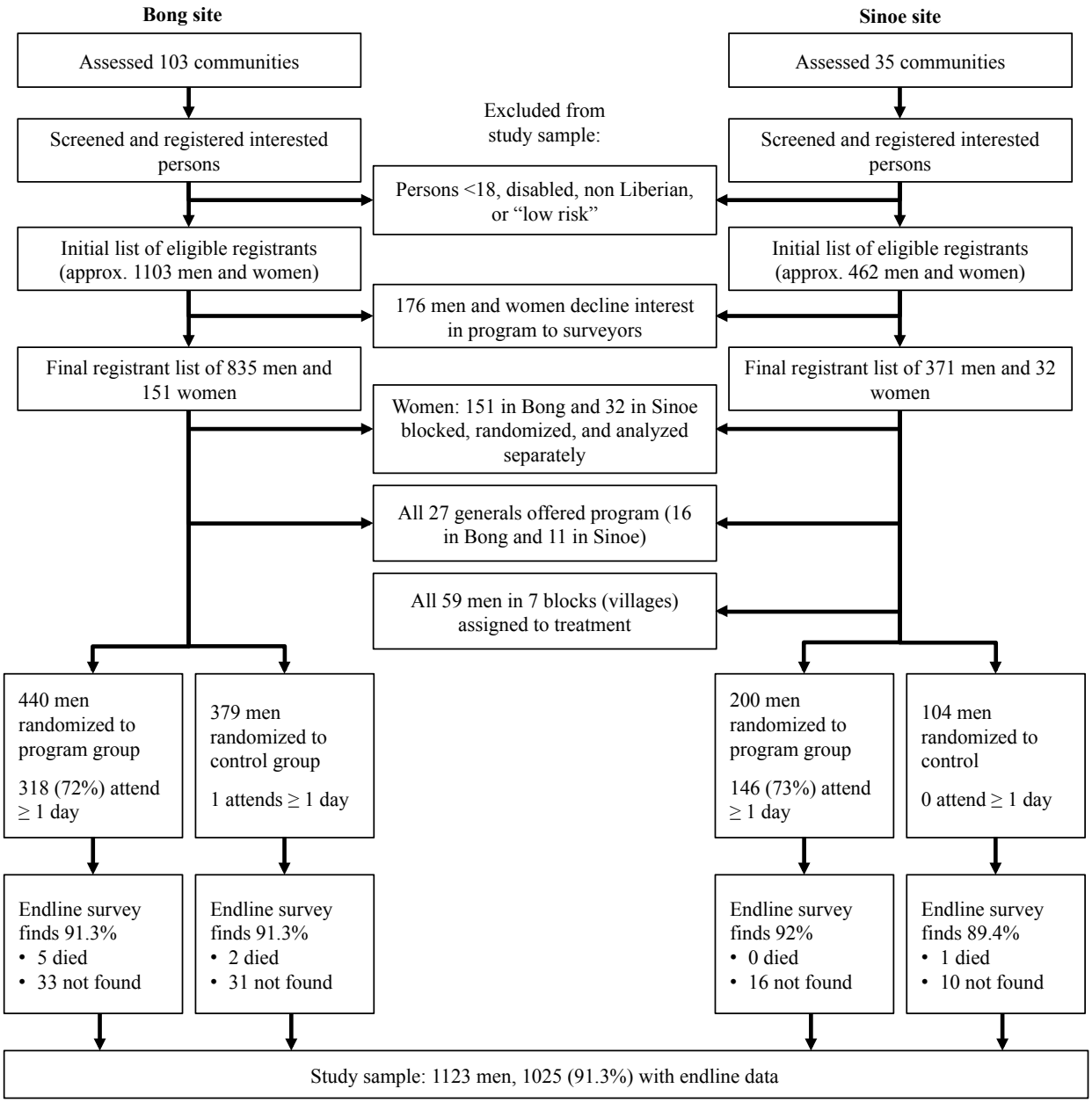
AoAV's main targets were adult ex-combatants and other high-risk men who were poor, engaged in illicit activities. There was overwhelming interest in the program among the hotspot population, many of whom were ex-combatants or at-risk young men. AoAV decided to focus on men who were the least served by previous postwar programs.[8] AoAV also excluded people deemed physically incapable of agriculture and non-Liberians.

2.3 Experimental procedures

The Bong training site accommodated 350 men and 50 women, while the Sinoe site accommodated 175 men and 25 women. Interested high-risk men in the target sites far exceeded the number of program slots, and so we worked with AoAV to assign offers to the program randomly. Figure 1 illustrates sample recruitment, selection, randomization, and data collection.

[8]Immediately following the war, a national demobilization program provided cash and training vouchers to tens of thousands of combatants, but the training component was widely regarded as a failure.

Figure 1: Sample recruitment, selection, randomization, and data collection



From May to October 2009 AoAV advertised the program in community meetings, and screened and registered interested and eligible people. We do not have data on those turned away, but qualitatively the vast majority of high-risk men expressed interest and were turned away. AoAV registered 1,565 men and women and passed them to a survey team who restated study procedures, obtained consent, and conducted the baseline survey. 176 withdrew their

interest or could not be found, resulting in 1,206 registered men and 183 women.

Our experimental analysis excludes 27 high-ranking "generals" who were automatically offered the program, as the UN considered it too risky to exclude them. We also exclude women, who have very different characteristics and risks.[9]

To randomize men, we blocked by training site, rank, and community and, within blocks, assigned each person a uniform random variable and sorted in ascending order. Men were assigned to an offer to enter the program in this random order within blocks until a target number per block was reached. If a person refused or could not be located, we assigned them to treatment and the offer went to the next person on the list. We adopted this method because AoAV had a fixed number of program spots to fill and a short time in which to inform and pick up the dispersed men. In Sinoe, 59 men from 7 blocks were dropped from the study because their block was fully assigned to treatment.[10]

Training ran November 2009 to February 2010 in Bong and September to December 2009 in Sinoe. In the end 72% of those assigned to treatment accepted, in that they attended at least a day.[11] Of 1,123 men, 57% were assigned to treatment.

3 Data

Table 1 describes men at baseline.[12] On average, the men were aged 30, had 5.9 years of schooling, with 27% literate. They reported cash earnings of $47 in the past month, savings of $46, and debts of $7.

Most men already engaged in subsistence farming, as markets were distant and food expensive. On average the men had 4.8 years of farming experience. 32% report that

[9]Few were involved in illicit resource work, almost none were fighters, and many engaged in transactional sex rather than violence. Thus we blocked randomization by generals and gender and drop both from our analysis.

[10]Bridge collapses and construction delays meant that they received only one or two days notice before pickup, thereby increasing refusal rates such that all men received the offer.

[11]One control member was mistakenly allowed to attend.

[12]Full baseline covariates are reported in Appendix B.1. Surveyors failed to collect data on 13 (1.2%) of respondents. 5% also opted to skip some sensitive questions on war experiences. For treatment effects analysis, we impute the median for missing data.

farming was their main source of income, and 29% report it was non-farm labor or business. 47% reported any days of work in illicit activities in the past week, but only 23% report that this was their main source of income. 87% said they were "very interested" in being a farmer in future.

74% were in a wartime faction, though only 17% reported being on the front lines. 13% reported close relations with a former commander, 5% said they were receiving support from one, and 2% said they reported to one.

Randomization balance. The sample was broadly balanced along baseline covariates. Columns 2 and 3 of Table 1 report the treatment and control group difference in select baseline covariates. Balance tests for all 83 covariates are listed in Appendix B.1. Six of the 83 covariates (7%) have a p-value ≤ 0.10.

Table 1: Baseline summary statistics and test of randomization balance

		Test of balance (n=1025)	
Baseline covariate	Control mean (1)	Treatment difference (2)	p-value (3)
Age	30.5	-0.617	0.256
Lives with spouse/partner	.722	-0.035	0.215
Number of children	2.5	-0.207	0.148
Disabled, injured, or ill	.349	-0.011	0.664
Years of schooling	5.68	0.210	0.364
Said would attend if selected	.984	0.002	0.616
Durable assets index (z-score)	-0.04	-0.006	0.909
Stock of savings (USD)	44.2	-13.066	0.025
Debt stock (USD)	7.2	-0.573	0.587
Agricultural experience index (z-score)	0.09	-0.062	0.390
Aggressive behaviors index (0-12)	1.18	0.125	0.248
Main income: Illicit resources	.228	0.001	0.955
Main income: Legal nonfarm work	.292	-0.029	0.362
Very interested in farming	.863	-0.012	0.561
Ex-combatant	.727	-0.006	0.826
Months in a faction	23.8	5.731	0.002
Ex-commander relations index (z-score)	0.04	-0.085	0.171
Patience index (0-4)	2.97	0.010	0.878
Risk affinity index (0-3)	0.33	0.037	0.370

Notes: Columns (2) and (3) report the coefficient and p-value on assignment to treatment from an OLS regression of each covariate on the treatment indicator and block fixed effects. Standard errors are clustered by village. Missing baseline data are imputed at the median. USD variables are censored at the 99th percentile.

The treatment group, however, had significantly lower savings and spent more time in an armed faction. A joint test of significance of all 83 baseline covariates has a p-value of .41 excluding these two covariates, but is $<.01$ including them. Note, however, that other variables related to wealth, debts, armed group activity, and violence have little association with treatment. Moreover, there is little treatment-control difference in predicted outcomes using baseline covariates (see Appendix B.1). As a result, imbalance is unlikely to be an identification concern. Nonetheless, all treatment estimates will control for all covariates.

Endline surveys and attrition. We held an endline survey from February to June 2011, 18 months after baseline and 14 months after training. The sample was mobile and difficult to track, but we nonetheless surveyed 1025 (91.3%) of 1,123 men.[13] Many had multiple aliases. Roughly 45% moved between surveys, many changing locations every few months. Few had mobile phones. We invested heavily in tracking out of concern that the hardest to find would be the most prone to violence. We made at least four attempts to locate each person. To mitigate excess attrition among the untreated, they received a phone worth $15 for completing the endline.

Attrition is not significantly correlated with treatment, and all baseline covariates explain just 11 percent of the variation. Some covariates are significantly related, and imply unfound could be those with a higher propensity for illicit activities and violence—they are slightly more likely to be ex-combatants, have slightly higher baseline aggression, and have been illicit rubber tappers.[14] Even so, similar risk factors, such as being illicit miners or long term fighters, are not associated with attrition.

Qualitative data. We also conducted eight weeks of unstructured interviews before, during, and after the program with participants, community leaders, UN and government personnel, and non-study residents. Furthermore, under our supervision, one American and two Liberian research assistants followed 26 treated men over two years, typically interviewing them four times (before, during and twice after training).[15]

[13]Nine had died and the remainder could not be found. Roughly two-thirds of the sample was found in the first 10 weeks. The remaining third took three months to track. To reduce bias from the timing of their survey, we first tracked a random half of the unfound, adding the second half after two months. Appendix B.2 discusses correlations between timing of surveys and responses and finds little relation.

[14]See Appendix B.3 for regression results. A test of joint significance of all covariates, however, has a p-value of $<.01$.

[15]They followed semi-structured questionnaires at each stage, with topics including: program experiences, economic activities, social relationships, war experiences, aggression, and aspirations. In addition to interviews, research assistants accompanied these individuals to class, to their fieldwork, mealtimes, and free time. They took detailed notes and recorded and transcribed interviews.

4 Conceptual framework

DDR and employment programs seldom spell out their assumptions explicitly, yet they are central for program design and targeting. Here we review the theoretical basis for two the the crucial assumptions underlying many programs: the link from providing capital and skills to generating employment, and the link from greater lawful employment opportunities to reduced unlawful ones.

AoAV designed the intervention to affect employment and production decisions in three ways. First, they used training and knowledge transfer to raise the returns to labor and capital in agriculture. Second, the input package aimed to relieve a constraint on available capital, with inputs that were difficult to sell or use in other sectors.

A third aspect of the intervention, the counseling and life-skills, aimed at something less conventional: socialization. The idea was that some actions or professions have direct utility benefits or penalties—that people have preferences over how their income is earned (e.g. Akerlof and Kranton, 2000). These preferences are thought to be partly rooted in one's self-image and social category. By providing education, a new profession, and relocation, AoAV's intervention tried to affect occupational choice by changing self-image and peers, and thus affecting penalties from oneself or peers for deviant behavior.

Each of these explanations can be captured in a simple model of occupational choice between legal and illegal occupations. We develop the model formally in Appendix C and outline the intuition and predictions here. In brief, we consider the choice of allocating labor between agriculture and illicit activities, such as mining and mercenary work, where this illicit work carries a small risk of capture and punishment. We introduce preferences over the type of occupation by allowing for positive or negative utility from illicit or rebellious activity (because of peer influences or own preferences).[16]

First, the success of the intervention (and deterrence of criminal or mercenary activities)

[16]In principle, rebellious or illicit activity could confer positive utility if were sanctioned or encouraged by peers, or if it satisfied some preference for justice or revenge.

hinges on the existence of specific market failures. In particular, the provision of training and capital will only shift employment patterns if credit markets function poorly, agricultural knowledge is imperfect, and hence the men are below their ideal and efficient scale of peaceful enterprise. Otherwise the men would be able to access the needed technology and borrow to finance any training and inputs until they reach efficient scale, and new in-kind inputs would be liquidated or divested.

These assumptions seem reasonable in rural Liberia where credit and insurance are almost non-existent, the supply of inputs is limited by high transport costs, and agricultural technology is rudimentary. But success also requires that men have high returns to these skills and capital once the program relieves these constraints. In particular, men cannot be bound by some other constraint, such as inadequate insurance.

The model also illustrates why it might be difficult to persuade men to exit illicit activities entirely. If there are high but diminishing returns to agriculture, then people will optimally engage in both farming and illicit activities, allocating their time so that the net marginal product of labor in agriculture equals that in illicit work. This is even more the case when there is risk aversion and uncertainty, since men have additional an incentive to perform both activities to reduce risk.

Punitive incentives can encourage exit. Increasing the risk of being caught, or the penalties once caught, are one way to do so (and may not be subject to the same diminishing returns). This is the common rationale for policing and punishment. In principle there are other ways to penalize crime, such as withdrawing a benefit. For instance, a cash transfer program conditional on no arrests, or living away from lootable resources, could have similar effects.

Finally, the model suggests who ought to be targeted by agriculture-oriented reintegration program, in terms of who is more likely to engage in illicit activities but potential to be influenced by policy: people with low initial productivity but interest in learning (in agriculture, in this case, though the same argument could be made for other peaceful activities);

and who have little capital and are credit constrained. Also, note it will be more difficult to persuade men to pick up agriculture when their disutility of illicit work is low, when the local returns to illicit work are high (such as rising gold prices), and when agricultural input prices are high relative to the output price.

5 Empirical strategy

We estimate the simple intent to treat (ITT) effect via an ordinary least squares (OLS) regression of the outcome on an indicator for receiving an offer to enter the program. Only three-quarters accepted the offer, however, and so we also estimate the effect of treatment on the treated (TOT) using assignment to treatment as an instrument for attending at least a day. The TOT is the effect of the program on people who comply with the program (accept the offer)

Both the qualitative and quantitative data suggest that non-compliance was fairly unsystematic, and hence our discussion focuses on TOT estimates. Qualitatively, people did not attend largely because a few days was inadequate notice. Others mentioned family obligations, debts outstanding, an illness, or an employer who would not permit them to return if absent. In general, baseline covariates have low explanatory power over compliance.[17]

To account for observable attrition and imbalance, we control for all baseline covariates (see Appendix B.1). To reduce sensitivity to outliers, we top-code all continuous variables at the 99th percentile. Because we are testing multiple outcomes, we test whether an additive standardized treatment effect of measures in "families" of outcomes is different from zero.[18] Finally, we cluster standard errors by baseline village.

Appendix B.4 discusses the potential for within-village spillover effects. In brief, spillovers are unlikely because the sample was a small proportion of the village population and agri-

[17]The R-squared statistic is low (0.056) and most of covariates are unrelated to compliance. Compliance is slightly but significantly increasing in savings stocks and length of time in a faction, and falling in debts. An F-test of all covariates, however, has $p<.01$. See Appendix B.3 for details.

[18]In general, these families were pre-defined by virtue of belonging to the same survey sub-section.

cultural production (typically well less than 5% of the population).

Identifying the marginal impact of capital inputs. Graduates mainly selected packages for vegetable farming (60%) and pig and poultry husbandry (29%). Roughly half of graduates, however, report that they did not receive the second installment. This includes all who chose pigs and poultry—they received materials for constructing pens but due to supply problems no animals.

Some months before the endline survey, AoAV announced that they would give a $100 cash grant to the men who selected the animals package in lieu of the animals themselves, though they gave no specific date. We ran the endline survey before disbursal.

We can estimate the causal effect of receiving inputs under the assumption that the choice of animal-raising versus farming is conditionally unconfounded. While this choice was hardly random, the data suggest that conditional unconfoundedness is plausible.

Animals have lower cash flow than vegetables, are more capital intensive, involve less labor, and are perceived to be more profitable. Thus we might expect more patient, wealthier men with other labor opportunities to choose animals. We see no such pattern; the choice of specializations seems to be unsystematic. Table 2 report an OLS regression of poultry/pig package choice on select baseline covariates among graduates. Only one covariate is significant: a 1 SD increase in agricultural experience is associated with a 7 percentage point increase in poultry/pig choice.

Table 2: Correlates of package selection

	Chose poultry or pigs (if graduated)	
Baseline covariate	Coeff.	Std. Err.
Age	-0.007	[.005]
Lives with spouse/partner	0.070	[.062]
Number of children	-0.022	[.015]
Disabled, injured, or ill	-0.051	[.053]
Years of schooling	0.008	[.007]
Said would attend if selected	0.165	[.380]
Durable assets (z-score)	0.012	[.028]
Stock of savings (USD)	0.000	[0000]
Debt stock (USD)	0.001	[.002]
Agricultural experience (z-score)	0.070	[.034]**
Aggressive behaviors (0-12)	-0.004	[.013]
Main income: Illicit resources	0.081	[.059]
Main income: Nonfarm work	0.036	[.056]
Very interested in farming	0.007	[.082]
Ex-combatant	-0.068	[.066]
Months in a faction	0.000	[.001]
Ex-commander relations (z-score)	0.023	[.027]
Patience index (0-4)	0.003	[.024]
Risk affinity index (0-3)	-0.028	[.046]
Observations	407	
Dependent variable mean	0.295	
R-squared	0.10	

Notes: Calculated via OLS regression with block fixed effects. The F-test is on all covariates excluding block and region dummies. Robust standard errors are in brackets, clustered by village. *** p<0.01, ** p<0.05, * p<0.1

6 Results

Based on our qualitative observation during the course, classroom instruction was practical and pitched at an appropriate level. Instruction involved substantial field training with animals and crops. Students learned farm practices appropriate for small-scale cash cropping and animal husbandry unavailable before, such as seed germination or fertilizer, pesticide and vaccine use.

Fights, angry protests, strikes, and the threat of violence were weekly occurrences on the

training sites. While the events were disruptive, they were also opportunities for the students to learn to work out grievances peacefully and apply lessons from the life skills class.

Overall, students were enthusiastic about the life skills and counseling. In interviews a year later, they brought up slogans and examples from the class in order to express themselves or explain a point, and spoke frequently of its impact on their lives. In the endline survey, when asked what part of the program most changed their life, 23% of graduates said the life skills curriculum and 19% said counseling, compared to 44% who said skills training and 3% who said inputs.

Based on administrative data, 94% graduated. More than half chose to return to their baseline community after graduation, and most others chose a community in the same county. Across Liberia farmland is plentiful and arranging for a few acres of customary land was straightforward. Community members often said they were proud of their new or returned residents.

Graduates faced steep challenges on return, however. Qualitatively, we observed that graduates returned to remote communities with sizable local markets but difficult road access and limited access to external markets and inputs. Furthermore, in interviews graduates reported serious liquidity constraints, and hence difficulty accessing tools and inputs beyond what AoAV provided. Farmland was plentiful and rich but typically rugged, uncleared rainforest. There was no access to plows, draft animals or tractors and most work is performed by hand. Pests and too much or too little water are also persistent challenges. Program impacts need to be considered in light of these challenges.

6.1 Impacts on occupational choice and incomes

Table 3 reports control group means and ITT and TOT estimates for key economic outcomes. Men typically have a portfolio of occupations. Illicit opportunities are often distant from village settlements, and so it is common for men to farm some weeks of the year in their base village, leave temporarily for petty trading, then move elsewhere to mine, log or tap

rubber for a period. Marginal changes in occupation might mean spending fewer weeks in "the bush" illicitly mining, and more weeks in town farming or trading.

The program led to a large increase in agricultural work on the extensive margin. 61% of control men said they were engaged in farming or animal raising, and this increases 15.5 percentage points among the treated—a 26% increase relative to control men. Treated men also express more interest in agriculture as a career. Interest in farming is high even without treatment: 95% of control men agree that they could make a good living farming, 78% are interested in farming in future, and 90% are interested in raising animals. Treated men are no more likely to think that farming is a good career (since opinion is nearly unanimous) but they are 12 percentage points more likely to be interested in farming.

Hours worked in agriculture increase by 4 hours per week, or 33% relative to control men. Communal farmland is easily available: roughly three-quarters of men report access to ten acres of land, and this is no higher with treatment. Treated men are farming 2 more acres (48%) than control men, though this effect is not statistically significant.[19]

We also see a shift on the intensive margin away from illicit resource extraction. We collected days and hours worked at 15 activities in the previous month (a time of dry season farming and crop sales). Control men report 49 hours of work per week, and total employment was not affected by treatment. But treated men decreased their hours of resource extraction by 3.7 hours (23%) and increased their hours in other work by 5 hours, mainly agriculture. Importantly, the treated do not exit illicit activities—40% of control men engage in any illicit extraction and this is only 3.2 percentage points lower among the treated, not statistically significant.

[19]Appendix D.1 examines individual farm activities. Treated men were more than twice as likely to be using improved techniques, such as growing seedlings, and were 43% more likely to have sold crops.

Table 3: Program impacts on occupational choice and income

		Treatment effect estimates (n=1025)			
	Control	ITT		TOT	
Outcome	Mean	Coeff.	Std. Err.	Coeff.	Std. Err.
	(1)	(2)	(3)	(4)	(5)
Agricultural engagement:					
Raising crops/animals†	0.61	0.118	[0.030]***	0.155	[0.036]***
Acres under cultivation	4.43	1.556	[2.146]	2.037	[2.573]
Thinks farming is a good living	0.95	0.008	[0.016]	0.010	[0.019]
Interested in farming	0.78	0.090	[0.029]***	0.118	[0.035]***
Interested in raising animals	0.90	0.049	[0.019]**	0.064	[0.023]***
Hours worked/week, past month	49.33	0.978	[2.357]	1.278	[2.824]
Illicit resource extraction	15.57	-2.829	[1.350]**	-3.697	[1.593]**
Logging	2.79	-0.926	[0.649]	-1.210	[0.773]
Mining	10.53	-1.356	[1.140]	-1.772	[1.362]
Rubber tapping	2.25	-0.547	[0.573]	-0.715	[0.682]
Farming and animal-raising	11.91	3.131	[1.180]***	4.090	[1.415]***
Farming	10.45	2.620	[1.037]**	3.423	[1.242]***
Animal-raising	1.46	0.511	[0.508]	0.667	[0.609]
Contract agricultural labor	1.82	-0.116	[0.320]	-0.152	[0.383]
Palm, coconut, sugar cutting	0.35	0.264	[0.343]	0.345	[0.413]
Hunting	1.18	0.215	[0.334]	0.281	[0.401]
Non-farm labor and business	18.16	-0.170	[2.055]	-0.222	[2.464]
Other activities	0.35	0.483	[0.571]	0.632	[0.682]
Other illicit activities:					
Any illicit resource extraction	0.40	-0.025	[0.032]	-0.032	[0.038]
Sells any soft or hard drugs	0.02	-0.008	[0.011]	-0.010	[0.013]
Stealing activities (z-score)†	-0.05	0.046	[0.064]	0.060	[0.077]
Income index (z-score)	-0.08	0.120	[0.059]**	0.157	[0.071]**
Cash earnings, past month (USD)	94.29	9.076	[9.555]	11.820	[11.398]
Durable assets (z-score)	-0.11	0.122	[0.059]**	0.160	[0.071]**

Notes: Columns (2) and (3) report the intent-to-treat (ITT) estimate, and columns (4) and (5) estimate the effect of treatment on the treated (TOT) via two-stage least squares. All regressions include block dummies and baseline covariates. Standard errors are robust and clustered at the village level.

† See Appendix D.1 for all index components.

*** p<0.01, ** p<0.05, * p<0.1

Reports of felony crime are rare and perhaps for this reason we see little effect of treatment. We ask about drug selling and several forms of theft (stealing, pickpocketing, and

armed robbery) which we assemble into a z-score. Only 2% of control men report drug selling (usually marijuana) and only about 2% of men report thievery.[20] Treated men are half as likely to report they sell drugs, but the result is not statistically significant. There is little effect of treatment on self-reported stealing.

Incomes rose as a result. We measured income by combining two measures, which together increase .16 standard deviations. First, we asked each respondent their net cash earnings in the past four weeks activity by activity, after expenses, including earnings received, cash earned but as yet unpaid, and the estimated value of any in-kind payment. This earnings measure are not only subject to recall and other biases, but they do not capture home production. Also, agricultural incomes may not have been earned in the past month. Thus it is a poor measure of permanent income. We approximate a measure of permanent income using durable assets—a z-score constructed by taking the first principal component of 42 measures of land, housing quality, and small and large household assets.

Control men reported $95 in earnings in the month prior to the survey, and this was $11.82 higher among treated men, a 12% increase (albeit not individually significant, in part because of high variance). Treated men also reported a 0.16 standard deviation increase in the durable assets index, significant at the 5% level. The family index (permanent income measure) is statistically significant. This durable asset increase is likely a result of previous harvest (of which there had been 2-3 since the end of the training program), and is probably a more reliable guide to income than recent cash earnings.[21]

In general, these results are robust to alternate treatment effects specifications and attrition bounds (see Appendix D.2).

Returns. A simple cost-benefit test considers the private returns alone. In the simplest case, we imagine the $11.82 earnings treatment effect represents a permanent increase in

[20] See Appendix D.1 for a breakdown of the index.

[21] The durable assets effect is unlikely to be a direct effect of receiving the inputs package. Package assets are not included in the index, and only 5% of the sample reported liquidating their packages. This is consistent with our qualitative observations, which suggested that selling of packages was exceptionally rare.

monthly income—-$141 annually. This is 11% of the per person program cost of $1275, implying "payback" (ignoring interest) in nine years. This 11% is also the cost of capital at which the $141 perpetuity breaks even. This is not an especially high or rapid private return. Moderate social externalities, however, could make it a more promising investment. In this case, the intervention allowed the Liberian government to peacefully reclaim mining and rubber concessions and lease these to private firms for development—one of the chief sources of national income. Reduced risk of future rebellion was also on the minds of policymakers. We turn to this next.

6.2 Impact on mercenary recruitment activities

We ran our endline survey at a time of escalating violence in neighboring Côte d'Ivoire (CI). The incumbent president, Laurent Gbagbo, lost but disputed a November 2010 election to his rival Alassane Ouattara. Both sides began mobilizing armed forces in December, and there were sporadic but increasing outbreaks of violence through February 2011. Serious fighting began in February near the Liberian border, and full-scale war broke out by March. By early April, however, French and UN forces helped to capture and defeat the former President, and hostilities quickly and unexpectedly ceased.

Both sides were accused of recruiting Liberian ex-fighters. Undetermined numbers crossed from Liberia to Côte d'Ivoire starting in December 2010. Observers estimated that 10,000 Liberian mercenaries fought in Côte d'Ivoire during a previous crisis, 2002-07 (ICG, 2011). Our qualitative work and news reports suggest that, by March 2011, no more than 500 Liberian mercenaries had crossed to Côte d'Ivoire. These men were primarily from the capital and border towns, were some of the most experienced ex-fighters, and were offered $500 to $1500 to join one of the forces (ICG, 2011). According to one Liberian recruit interviewed by Reuters, "Some of us are not working. Our government [in Liberia] disarmed us, but they have refused to take us into the new army" (Garnaglay, 2011). "We have been in this business for many years," another said. "We know how to fight well and if Gbagbo

or Ouattara's men can employ us to fight, that will be good."

To the best of our knowledge, none of our sample (treatment or control men) went to fight in Côte d'Ivoire. This is not surprising given the small numbers that went before the war came to a sudden end. Ex-commanders did activate recruitment networks, and had begun to approach and make offers to ex-fighters to prepare for a longer war. In small communities across Liberia, recruitment activities began to proliferate. Former mid-level commanders and generals would call secretive meetings of former fighters in the village. Rumors circulated about the sums promised to men to go, and appropriate terms might be discussed in the meetings. Ex-fighters, if interested, could seek out these meetings, mobilizers, or in the extreme move to one of the border towns where forces were expected to amass. Others were more likely to be recruited by dint of their profession or location (e.g. in a mining town).

Table 4 lists control means and treatment effects for 16 self-reported measures. Some are indirect. For instance, 66% of control men had a partisan preference and 68% talked about the war with friends. Other measures reflect more active demand or supply of offers. For instance, 45% had talked with a commander in the past 3 months, 8% said they had been approached to go, 10% said they were making plans to move to the CI border, 4% said they were invited to go to a secret recruitment meeting and 3% reported attending. 3% also reported being offered money to go. 5% reported they would go for $1000, and 1% said they had concrete plans to go in the next month. We group the measures into 12 more direct actions and plans as well as 4 more indirect questions.

Table 4: Program impacts on mercenary recruitment activities

Outcome	Control Mean (1)	TOT estimate Coeff. (2)	TOT estimate SE (3)
Mobilization activities/attitudes (z-score)	0.09	-0.204	[0.079]***
Direct recruitment activities (0-12)	0.94	-0.239	[0.118]**
Talked to a commander in last 3 months	0.45	-0.108	[0.044]**
Would go if called to fight for tribe	0.05	-0.015	[0.013]
Has been approached about going to CI	0.07	0.001	[0.021]
Would go to CI for $250	0.01	-0.006	[0.010]
Would go to CI for $500	0.03	-0.009	[0.012]
Would go to CI for $1000	0.08	-0.041	[0.019]**
Will move towards CI border area	0.10	-0.022	[0.024]
Invited to secret meeting on going to CI	0.04	0.004	[0.016]
Attended secret meeting on going to CI	0.03	-0.013	[0.011]
Was promised money to go to CI	0.03	0.001	[0.014]
Willing to fight if war breaks out in CI	0.04	-0.018	[0.015]
Has plans to go to CI in the next month	0.01	-0.012	[0.009]
Indirect recruitment measures (0-4)	1.48	-0.158	[0.076]**
Talks about the CI violence with friends	0.68	-0.046	[0.041]
Has a partisan preference in CI	0.66	-0.117	[0.041]***
Knows people who went to CI to fight	0.10	-0.021	[0.019]
Knows people given money to go to CI	0.04	0.026	[0.016]

Notes: Columns (2)-(3) report the the effect of treatment on the treated (TOT) via two-stage least squares. Regressions include block dummies and baseline covariates. Standard errors are clustered at the village level. See Appendix D.1 for ITT results.

*** p<0.01, ** p<0.05, * p<0.1

12 of the 16 measures are lower for treated men than controls, often by large proportions (even though absolute magnitudes are small). For example, those who would go for $1000 falls 51%, attending a secret meeting falls 43%, planning to move to the border area falls 21%, talking with a commander in the last 3 months falls 24%, and having concrete plans to go drops 86%. Of these, only the treatment effects talking to commanders and willingness to go for $1000 are individually significant. Jointly, however, these falls are significant—engagement in all 12 more direct recruitment activities falls 24%. The other indirect recruitment indicators fall 11%, and an index of all 16 CI recruitment measures falls

by 0.204 standard deviations lower among treated men. If we discard two of the most significant effects—partisan preference and talking to commanders—the TOT estimate actually increases in magnitude.

6.3 The impact of in-kind inputs

These average treatment effects conceal important heterogeneity driven by variation in who received the in-kind capital inputs. Some graduates did not receive their full packages because AoAV could not find them. As discussed above, however, a third did not receive their package for relatively exogenous animal supply problems.

To the extent that agricultural skills and capital are complements, our model predicts that the occupational shift to agriculture will be lower when capital is not received. The predicted effect on illicit activities is ambiguous, however. Since agricultural returns are lower, illicit activities shouldn't decrease. In practice, however, mining and mercenary work requires that men leave the village and risk missing the disbursement of animals or cash. This could dissuade men from illicit work. In this sense, missing the disbursement is akin to the risk of punishment in our model of occupational choice.[22]

We estimate the effect of choosing animals in Table 5, which estimates the ITT impact of the program (in columns 1 and 2) including an indicator for whether the man chose to specialize in animals (in columns 3 and 4).[23] The sum of these two coefficients (in columns 5 and 6) is the net effect of the program on those who chose pig and poultry.

We see that those who chose pig and poultry are less likely to increase their agricultural engagement or hours of work. The decrease on the extensive margin is not statistically significant, but the decrease in hours worked is. Illicit activities fall in both groups, though they appear to fall more in the animals group (though the difference is not statistically significant).

[22]With some probability, men who graduated and selected poultry or pigs expect to lose their grant if they leave. Appendix C illustrates the trade off and comparative statics.

[23]Treatment effects by specialization for all outcomes are detailed in Appendix D.1.

Table 5: The marginal effect of capital inputs: Heterogeneity of program impacts by package choice

		ITT estimates					
	Control	Impact of assignment to program		Marginal effect of choosing animals package		Program impact on animal choosers (2+4)	
Outcome	Mean	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Engaged in agriculture now	0.61	0.127	[0.033]***	-0.046	[0.045]	0.081	[.041]**
Hours worked/week, past month	49.33	2.502	[2.616]	-8.065	[5.190]	-5.563	[4.757]
Illicit resource extraction	15.57	-2.318	[1.406]	-2.706	[2.490]	-5.024	[2.484]**
All other activities	33.77	4.820	[2.599]*	-5.359	[4.337]	-0.539	[4.157]
Farming	10.45	3.503	[1.198]***	-4.675	[1.884]**	-1.172	[1.571]
Animal-raising	1.46	0.501	[0.579]	0.054	[1.321]	0.555	[1.157]
Any illicit resource extraction	0.40	-0.014	[0.032]	-0.057	[0.051]	-0.071	[.056]
Index of mobilization risk (z-score)	0.09	-0.122	[0.069]*	-0.182	[0.094]*	-0.304	[.094]***
Direct recruitment activities (0-12)	0.94	-0.157	[0.107]	-0.138	[0.142]	-0.295	[.13]**
Indirect recruitment measures (0-4)	1.48	-0.080	[0.066]	-0.217	[0.091]**	-0.296	[.097]***

Notes: Column (2) reports the ITT coefficient of program assignment and Column (4) reports the coefficient on an interaction between program assignment and choosing poultry/pigs. Column (6) lists the sum of the coefficients in Columns (2) and (4). The regression includes baseline covariates and regional dummies are used instead of block dummies. Robust standard errors in brackets, clustered by community.

*** p<0.01, ** p<0.05, * p<0.1

The fall in both illicit resource extraction and mercenary recruitment activities is largest, however, among those who chose to specialize in animals and were told to expect a transfer of animals or a cash equivalent in the next few months. This is consistent with men staying in villages to await the transfer. Resource extraction and especially mercenary work would take men out of the return villages. If AoAV arrived to distribute cash or animals, they would miss them. Travel in Liberia is slow and expensive, and communication is limited in the hotspot areas, making it difficult to return on short notice.

This presumes AoAV's promises were credible after failing to deliver for a year. Our qualitative interviews suggest that while men were worried AoAV might not fulfill their promises, most believed AoAV would deliver. Not only had the men received intensive training, but they had received the materials to build the chicken coop or pig sty, and had witnessed AoAV return twice to deliver goods to the men specializing in vegetables.

6.4 Could results be biased by self-reported data?

One concern with these effects is they use self-reported data. If the treated feel pressure to report good behaviors then we overestimate impacts on them. (On the other hand, if the control group wishes to present itself well, in the hopes of being eligible for future aid, this would lead us to underestimate impacts.) This is a common challenge for studies of illicit behavior, especially in developing countries where administrative data are nonexistent.

Measurement error is a risk, but there are several reasons to think it is a modest one. First, the patterns we observe are inconsistent with the obvious incentives to misreport. The counseling and life skills components of the program stressed certain forms of behavior change: ending use of war names, lowering interpersonal aggression, solving disputes peacefully, and increased community participation, among other behaviors. Occupational choice, including resource extraction, was not discussed or discouraged. Thus if treated men have a tendency to report "good" behavior to surveyors, we should expect significant treatment effects to be largest for the behaviors emphasized by their counselors. Below we will see the

opposite is true—treated men do not report better behavior in any of these domains. In Table 3 we also saw no impact on exit from illicit activities or felony crimes. It is hard to reconcile social desirability bias with the pattern of treatment effects we observe.

Second, since resource extraction and mercenary actions mainly decrease among animal choosers, the incentives to misreport would have to be concentrated there. Those who selected into animal husbandry could have worried their cash transfer was conditional on reporting "good behavior" in the survey, while those who chose vegetables (and received their package) do not. But then it is puzzling that we do not see this appear in the good behaviors explicitly emphasized by the program. Furthermore, the control group, who were eligible for future training, did not respond to a similar incentive.

Third, we attempted to measure social desirability bias. Logistically it was not possible to validate data for our dispersed, mobile sample. Instead, we conducted a survey data validation exercise in the capital among a similar population of high-risk men in the slums of Monrovia, principally men engaged in petty crime. These men were part of a field experiment that tested a similar program of socialization and behavior change targeting crime, drug use, and aggression. The validation exercise is detailed in Blattman et al. (2014). Briefly, Liberian research assistants shadowed and interviewed 240 men for three days within ten days of a written survey. They used personal interviews, close observation, and discussions with family and friends to gather information on the respondent's participation in theft, drug use, and gambling. We find no evidence of measurement error in these self-reported behaviors; The correspondence between the validated and survey data is generally 70 to 85%, there is little mean difference between the survey and qualitative responses to sensitive questions, and there is no correlation between the difference and assignment to treatment.

Fourth, in terms of mercenary recruitment, we see significant decreases in both the less sensitive, indirect questions as well as the direct questions. Also, "yes" responses to the various mercenary recruitment activity questions are not clustered among a few people, but rather are spread among a large proportion. Thus it is not the case that a small number of

people who are socially sensitive could drive the result.

As with many studies of illicit activity, self-reporting bias remains a risk. But we see little reason to think that it explains such large shifts—a 24% fall in recruitment activities, or a 20% fall in illicit extraction hours.

6.5 Socio-political impacts

The program seems to have had little effect on peer groups, risky social networks, anti-social behaviors, or community engagement and leadership—all of which were the main aims of the "socializing" life skills training and counseling. We also see little effect on attitudes to violence and democracy.

Table 6 reports control group means and TOT estimates for several family indices plus a subset of the index components as examples (Appendix D.1 lists all components). We do not find evidence that the program broke down military chains of command or reduced interaction among ex-combatants, perhaps because the training maintained or increased most people's exposure to ex-combatants and commanders. An index of four measures of ex-combatant relationships increases 0.073 standard deviations (not significant). An index of relationships with former commanders declines 0.154 standard deviations among treated men (also not significant). Treated men do report small decreases in close relations with a commander, or receiving support or jobs from a commander.

We also asked respondents about their closest friends and whether or not they have 19 different qualities ranging from "positive" (e.g. have a business or job, participate in community meetings) or "negative" ones (e.g. get drunk regularly, use drugs, steal, or have conflicts with the authorities). An index increasing in positive qualities and decreasing in negative ones is 0.027 standard deviations higher among the treated (not significant). Table 4 displays 6 of qualities. None of the negative qualities and only one of the positive qualities (having a business or job) are higher among the treated.

Table 6: Program impacts on social relations, networks and peers

Outcome	Control Mean (1)	TOT estimate Coeff. (2)	TOT estimate SE (3)
Ex-combatant relations, 4 measures (z-score)†	0.07	0.073	[0.080]
Has friends who are ex-combatants	0.58	0.111	[0.046]**
Half or more of friends are ex-combatants	0.50	-0.018	[0.044]
Fought in the same unit with these friends	0.38	0.03	[0.036]
Talks to them about good times during war	0.13	-0.065	[0.083]
Ex-commander relations, 4 measures (z-score)	0.02	-0.154	[0.100]
Has friends who are former commanders	0.20	0.006	[0.041]
Has close relations with a former commander	0.30	-0.055	[0.036]
Former commanders give support or jobs	0.08	-0.017	[0.026]
Currently reports to a commander	0.04	-0.012	[0.015]
Peer group qualities, 19 measures (z-score, with bad qualities reducing index)†	0.05	0.027	[0.063]
Have a business or a job	0.58	0.072	[0.035]**
Comfort you when you are feeling bad	0.90	0.042	[0.027]
Can be trusted to guard your valuables	0.87	0.003	[0.024]
Use drugs	0.06	-0.014	[0.020]
Steal other people's property	0.03	0.004	[0.013]
Often have conflicts with authorities	0.05	0.024	[0.017]
Quality of social relations			
Index of social support received (z-score)†	-0.06	0.188	[0.085]**
Index of family relations (z-score)†	-0.00	0.133	[0.075]*

Notes: We estimate the effect of treatment on the treated (TOT) via two-stage least squares. All regressions include block dummies and baseline covariates. Standard errors are robust and clustered at the village level. See Appendix D.1 for ITT estimates.

† See Appendix D.1 for all index components

*** p<0.01, ** p<0.05, * p<0.1

Treated men do, however, report better support from their existing networks. Table 6 also reports an index of 8 questions about concrete forms of social support received in the past month (such as people in your life who can give you advice or financial support) and 7 questions about the quality of family relationships (such as frequency of interaction or whether there are serious disputes). Social support is 0.19 standard deviations higher among the treated, and the family index is 0.13 standard deviations greater (though it is

only significant at the 10% level).

Table 7: Program impacts on anti-social behavior and attitudes to violence and democracy

Outcome	Control Mean (1)	TOT estimate Coeff. (2)	TOT estimate SE (3)
Antisocial behaviors, 13 measures (z-score)†	-0.06	0.036	[0.078]
Was unable to control your anger (past month)	0.48	0.058	[0.056]
Threatened people (past month)	0.10	0.002	[0.035]
Took other people's things (past month)	0.03	0.060	[0.023]***
Had a fight or angry dispute (past 6 months)	0.70	0.000	[0.138]
Uses a war name (nom de guerre)	0.32	-0.009	[0.045]
Approval for use of violence, 12 measures (z-score)†	-0.05	-0.064	[0.072]
Neighbor beats the man who robbed his home	0.08	-0.032	[0.018]*
Take things from home of man refusing to repay you	0.04	-0.001	[0.015]
Community beats a corrupt leader	0.07	-0.005	[0.019]
Community beats policeman bribed to release rapist	0.22	-0.042	[0.033]
Community participation/leadership, 13 measures (z-score)†	-0.01	0.112	[0.074]
Is a community leader	0.29	-0.024	[0.034]
Contributed to care of community water sources	0.67	0.027	[0.043]
People often come to you for advice	0.38	0.018	[0.039]
Community members come to you to solve disputes	0.28	0.015	[0.039]
Attitudes to democracy, 10 measures (z-score)	7.50	-0.164	[0.131]

Notes: We estimate the effect of treatment on the treated (TOT) via two-stage least squares. All regressions include block dummies and baseline covariates. Standard errors are robust and clustered at the village level. See Appendix D.1 for ITT estimates.

† See Online appendix D.1 for all index components

*** p<0.01, ** p<0.05, * p<0.1

Table 7 looks at the pro- and anti-social behaviors targeted by the intervention. Again we report family indexes with examples of the components (with all in Appendix D.3). We see no significant change in an index of 13 questions about aggressive and other anti-social behaviors in the past four weeks (such as being quick to anger, threatening people, destroying their property, or having physical fights). Also, about a third of the control group use a *nom de guerre*, a practice actively discouraged by the facilitators as a symbol of personal change, and treatment has no effect on its use.

We also asked about 12 attitudes towards violence as a legitimate means of maintaining order or justice (such as mob justice and other forms of extrajudicial punishment). The index is 0.064 standard deviations lower among the treated (not significant). We asked 13 questions about community participation and leadership (such as attending community meetings, or contributing to public goods). An index of these is 0.112 standard deviations greater among the treated (not significant). Finally, we see no significant difference in 10 measures of pro-democratic attitudes, such as whether they disapprove of military coups when the President's performance is bad.

7 Discussion and conclusions

This experiment not only provides the first opportunity to experimentally study a rehabilitation ex-fighters, but is also one of the first rigorous studies outside the US of a labor market intervention on illegal and rebellious activity. We show that peaceful economic opportunities help to employ and socialize ex-fighters.

Beyond providing strong individual-level evidence for the opportunity cost mechanism, the size and the specifics of the illicit labor supply response are important. Increasing agricultural productivity and capital led men to shift away from illicit resource extraction on the intensive but not the extensive margin—a pattern shared with low-level drug dealers in US gangs. The extensive margin responded more to future and ongoing incentives—a conditional cash transfer—rather than a one-time increase in capital and productivity.

These findings have implications for program design in fragile states. First, to persuade men to exit rather than simply decrease illicit activities, a single-sector focus (e.g. agriculture) is insufficient. In the absence of an insurance system, only multiple streams of income from multiple occupations reduce income risk. Hence DDR or employment programs that promote both agricultural and non-agricultural employment, such as petty business, could further reduce incentives for illicit or mercenary work. Alternatively programs could provide

more liquid capital that can be used for multiple sectors.

Second, this experiment joins a growing amount of work in developing and fragile states that suggests that employment programs should emphasize capital over skills. Our results suggest that agricultural training alone may have low returns for the poor and credit-constrained, and that returns to capital are high. This is consistent with studies of vocational training that show limited impacts (e.g. Attanasio et al., 2011; Cho et al., 2013). Programs providing capital to the poorest, on the other hand, have been shown to yield high returns (Blattman et al., 2014; Bandiera et al., 2013). In the program we study, a reasonable hypothesis is that increasing the ratio of inputs to training would have preserved or increased the economic impact while reducing the most expensive part of the intervention: the training.

Third, the response to the promise of a future cash transfer was insightful. It suggests that one-time transfers of storable or transferable skills and capital may have little deterrent effect in future. Ongoing and conditional incentives are needed to deter mercenary and criminal opportunities. Cash-for-work programs are a potential example. They consciously do what the animal supply problem (and promised cash transfer) did by accident: condition a payment on men's location—out of hotspots and not in mercenary work. Cash-for-work also provides a direct incapacitation effect.

Finally, the program we studied was ineffective at breaking armed social networks, and we see no evidence the socialization was effective. This is not to say that socialization or changing networks is not possible. Other programs targeting high-risk urban youth, in both the US and Africa, have more success at socialization (Heller et al., 2013; Heckman and Kautz, 2013; Blattman et al., 2014). Experience from a large literature on programs for at-risk youth and offenders provides guidance. In general, successful programs target behavior change directly, through behavioral therapy, rather than indirectly through employment. This literature also discourages residential programs as encouraging change in artificial environment (Blattman et al., 2014; Gendreau and Andrews, 1994). The intervention we study sought to socialize the men in an artificial and temporary setting and social group, without

access to new, noncombatant peers until after the intervention was complete. Finding low-cost ways to target and deliver work opportunities in fragile states, especially to high-risk populations, remains a crucial area for policy experimentation.

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Appendix for online publication

A Program, curriculum, and budget details

This section details the nature, content, and cost of the program. Table A.1 reports statistics on program attendance and choices.

Table A.1: Program attendance, performance and choices

	Sample		
Program choice or outcome	Men assigned to treatment	Men assigned to treatment who attended	Men assigned to treatment who graduated
Attended >1 day of training	74%		
# training days completed	83.74	113.03	117.63
Graduated	69%	94%	
Migrated since baseline	48%	46%	46%
Received part 1 of package	65%	88%	93%
Received part 2 of package	34%	46%	49%
Chose vegetable package			60%
Chose poultry or pigs package			28%
Chose rubber package			7%
Chose other package			5%
% that received both packages:			
Chose vegetable			67%
Chose poultry or pigs			7%
Chose rubber			70%
Chose other			48%
Sold part of packages			5%
Observations	586	433	407

A.1 Agricultural training curriculum

The syllabus for the Bong site (called the Tumutu Agricultural Training Program) has 5 major modules. The learning objectives and outcomes for each module is as follows:

1. Rice production (72 hours). After participating in all the module-training activities (both theory and practical sessions) the trainee is expected to gain broader knowledge

on the growth and development of the rice plant and the use of sustainable agricultural methods in rice production, and link rice production to household/national food security and markets for sustainable livelihoods. At the end of the six month training in rice production, the trainee should be able to:

(a) Understand and be able to produce upland and lowland rice

(b) Gain a broader knowledge of the importance of proper management of natural resources (soil and water) and the environment for sustainable rice farming

(c) Develop swamp beds for rice production

(d) Identify sources/types/quality of rice seed and the advantages of using high quality seed adapted to the local conditions

(e) Lay out, establish, and maintain a nursery

(f) Understand and implement techniques for transplanting/planting of rice seed

(g) Understand the different growth stages in rice development

(h) Maintain a rice field ensuring sustainable management of soil nutrients and optimum conditions for rice growth

(i) Develop capacity to identify diseases and pests and to implement the necessary control and or treatment measures

(j) Understand techniques for sustainable and profitable harvesting, processing, and storage of rice

(k) Understand the importance of markets in rice production systems

2. Rubber culture (72 hours). After participating in all the module-training activities (both theory and practical sessions) the trainee is expected to develop broader knowledge on sustainable rubber production through the use of sustainable agricultural methods and develop capacity to practice rubber farming and/or gain skills for

employment in the rubber industry for sustainable livelihoods. At the end of the six month training in rubber production, the trainee should be able to:

(a) Understand the importance of rubber in Liberia

(b) Techniques for bark maintenance, tapping, and latex collection

(c) Develop capacity to establish and maintain a nursery

(d) Prepare pre-germination beds

(e) Establish bud wood garden

(f) Develop techniques for budding

(g) Conduct tasking, paneling, furnishing and opening

(h) Develop sustainable tapping systems and methods

(i) Prepare and apply solution (Latex, Coagulum and Cup – Lump)

(j) Develop capacity for quality control in rubber production

(k) Understand disease and pest management in rubber production

(l) Maintain production equipment

(m) Develop skills for plantation management, product storage, marketing and post harvest management

3. Vegetable Production (72 hours). After participating in all the vegetable production module-training activities (both theory and practical sessions) the trainee is expected to develop broader knowledge on small-scale vegetable production through the use of sustainable agricultural methods and locally available resources and develop capacity to practice vegetable farming for household consumption and produce a surplus for market-ing thereby securing livelihoods income. At the end of the six month training in rubber production, the trainee should be able to:

(a) Understand the importance of vegetable production for household and national food security

(b) Understand and classify types of vegetables

(c) Develop knowledge on the proper usage and maintenance of tools in vegetable production

(d) Understand the importance of and operate records in vegetable production

(e) Understand the factors considered for vegetable production site selection and prepare selected land sites for production

(f) Establish and maintain a vegetable nursery

(g) Implement different vegetable production systems

(h) Develop capacity for sustainable disease and pest management

(i) Understand the benefits of organic manuring for sustainable natural resource management and food quality

(j) Understand compost making and manure selection and application

(k) Understand and develop capacity to harvest different types of vegetables including quality control and storage

(l) Understand the importance of markets in vegetable production.

4. Tree crops/oil palm (106 hours). After participating in all the tree crops production module-training activities (both theory and practical sessions) the trainee is expected to gain broader knowledge on how to grow oil palms through the use of sustainable agricultural methods and develop capacity to practice oil palm farming as a cash crop and be in a position to generate an income for long-term sustainable livelihoods. At the end of the six month training in oil palm farming, the trainee will be expected to:

(a) Understand the importance of oil palm as a cash crop

(b) Understand the required soil and climatic conditions

(c) Be able to select varieties best suited for their communities

(d) Lay out paths and nursery beds and put up shelters in the nursery

(e) Understand factors determining plantation site selection

(f) Carry out pegging of the plantation pattern, planting out the oil palm seedlings and fencing around the seedlings

(g) Learn how to maintain an oil palm plantation (e.g. cultivation, trimming the plants, soil nutrient and water management)

(h) Implement sustainable pest and disease management practices

(i) Understand methods of and be able to implement oil palm harvesting, processing and storage

(j) Understand the importance of markets in oil palm production and develop capacity to access and derive benefits from the market.

5. Animal husbandry (78 hours for poultry, 89 for pigs, and 55 for rabbits). The training module is designed for beginners to provide fundamental theory of tools, materials and work practices in animal production (poultry, rabbitory and piggery). After participating in all the animal husbandry production module-training activities (both theory and practical sessions) the trainee is expected to gain broader knowledge on how to keep poultry, rabbits and pigs for household food security and to be in a position to generate an income through this activity for sustainable livelihoods. At the end of the training in animal husbandry, the trainee will be expected to:

   (a) Develop basic skills for poultry, rabbitory and piggery production

   (b) Be able to construct required housing for poultry, rabbits and pigs through the sustainable use of locally available resources

(c) Understand the breeding and reproductive systems and requirements of poultry, rabbits and pigs and the methods used to enhance quality of breeds

(d) Develop capacity to implement disease and parasite prevention and treatment and control in poultry, pig and rabbit production

(e) Develop capacity for animal slaughter (poultry, rabbits and pigs) with an emphasis on animal welfare, quality control and hygienic standards including preservation methods

(f) Understand the importance of markets in poultry, pig and rabbit production and strengthen skills to compete profitably

A.2 Life skills curriculum

For the life skills class and on-site counseling, AoAV contracted members of the Network for Empowerment and Progressive Initiative (NEPI), a local non-profit organization. NEPI had developed and implemented previous programs in Liberia with war-affected youth and AoAV used and adapted their curriculum for this program. The stated goal was to transform the lives of participants so they may become more productive members of society. More broadly, the class and counseling is designed to help the war affected improve their coping mechanisms to trauma; and foster relationships between and amongst former fighters, conflicting parties, varying socioeconomic groups, and tribal factions; and promote peace and reconciliation principles as tools to effectively build, strengthen, and promote positive social change within communities. NEPI trainers and facilitators are largely former combatants themselves who perform social work and other social services for war-affected youth. These counsellors serve as the participants' strongest positive role models. The curriculum itself has 17 major modules:

1. Effective communication (Definition; Types; Barriers; Ways of Improving; Listening Skills)

2. Perception and role reversal (Definition; Case Study; The Way Forward)

3. Understanding conflict (Definition; Types; Causes; Effects; Tools for Conflict Management; Tools for Conflict Resolution)

4. Conflict analysis and transformation (Definition; Strategies; History; People; Tools for Conflict Analysis & Transformation)

5. Violence and its cycle (Definition; Types; Causes; Effects; Breaking the Cycle of Violence)

6. Understanding trauma and substance abuse (Effects of Trauma; Definition; Types, Causes and Effects; The Way Forward)

7. Post-traumatic stress disorder (Definition; Causes; Symptoms and Signs; Developing Coping Mechanism)

8. Career counseling (Definition; Types; Importance of Career; Career Selection; Two aspects of Career; Principles for Effective Career)

9. Self image and recovery (Definition; Types; The Building Process)

10. Early warning and early response (Definition, Group Discussion. Mechanism for Prevention & Transformation, The Way Forward

11. Community outlook (Definition; Structure; Norms; Realization & Transformation; Reintegration)

12. Community initiatives and development (Definition; Types; Ownership and Sustainability)

13. Peace building - Levels and approaches (Definition; Role of a Peace Builder; Peace Keeping; Peace Making; Peace Building)

14. Challenges of reconciliation (What is Reconciliation?; Steps to Reconciliation; Religious & Traditional Perspectives of Reconciliation; Dilemmas of Reconciliation; Sustaining Reconciliation Work)

15. Leadership styles and skills (What is leadership?; Why is it important?; Can Leadership be Learned?; Best Leader/Role model; Characteristic of Admired Leadership; Your Leadership Strengths; Seven Critical Leadership Skills; Approaches to Decision Making)

16. General review

17. Re-entry strategies (Identifying communities for the purpose of possible re-insertion of trained participants; Confidence building meetings with community leadership; Linking participants to host communities)

A.3 Example of program budget

Table A.2 reports the budget AoAV submitted (via the UNDP) to the UN Peacebuilding Fund in 2008 to funding two training courses of 400 students each at the Bong training site. The first cycle was the focus of the evaluation. Figures reflect the high cost of any operations and supplies in Liberia, as with many post-conflict regions.

Table A.2: Sample budget from Bong course site

	Cost in USD (2 courses, 800 beneficiaries)	
Expense category	Total	Per beneficiary
Personnel		
National and international staff	300,000	375
Subcontractors (counselors/life skills trainers)	10,000	13
Course costs		
Course equipment	100,000	125
Food and medical supplies	140,000	175
Other course costs	50,000	63
Reintegration packages	100,000	125
Operations expenses		
Transport, fuel and maintenance	140,000	175
Travel	50,000	63
Office, utilities and communications	110,000	138
Headquarters support	21,000	26
Sub-total	1,021,000	1,276
UNDP fee	73,500	92
Contribution to randomized evaluation	50,000	63
Total	1,144,500	1,431

B Further details on empirical strategy

B.1 Baseline covariates and balance

Table B.1 displays summary statistics for the 83 baseline covariates used in the treatment effects regressions along with baseline tests of balance. The first column reports the mean among all men, and the second column for women. Women are different in several respects: they are younger and less likely to be married or partnered—an uncommon situation for adult women in Liberia. A quarter admits to sex work, but qualitatively our sense is that this is much higher. Very few are ex-combatants and aggression is low.

The third and fourth columns display the mean difference between control and treatment men, based on an OLS regression of each baseline covariate on an indicator for treatment assignment, controlling for block fixed effects only (with standard errors clustered at the

village level). 6 of the 83 covariates (7%) have a p-value less than 0.10, which is no more than would be expected at random.

Two of the 83 baseline covariates do show significant imbalance: savings and months spent in a faction. A joint test of significance of all 83 baseline covariates has a p-value of .41 excluding these two covariates, but is <.01 including them. Other variables related to wealth, debts, armed group activity, and violence have little association with treatment.

Table B.1: Baseline descriptive statistics and test of randomization balance

	Means		Balance test (men only)	
Baseline covariate	All men (n=1123)	All women (n=151)	Difference (Control - Treatment)	p-value
Missing baseline data	0.01	0.07	-0.00	0.54
Age	30.28	26.17	-0.62	0.26
Muslim	0.15	0.13	-0.00	0.97
Gola tribe	0.05	0.07	-0.01	0.38
Kpelle tribe	0.34	0.38	0.01	0.80
Kru tribe	0.09	0.02	0.00	0.75
Mano tribe	0.11	0.18	0.01	0.60
Sapo tribe	0.11	0.01	-0.01	0.54
Lives with spouse/partner	0.71	0.51	-0.03	0.22
Number of children	2.34	1.68	-0.21	0.15
Currently pregnant		0.04		
Disabled	0.06	0.05	0.00	0.89
Injured	0.23	0.20	-0.02	0.39
Seriously ill	0.14	0.21	0.01	0.52
# days drank alcohol in the past week	1.00	0.37	-0.09	0.38
Index of risk seeking (0-3)	0.33	0.33	0.04	0.37
Index of patience (0-4)	2.96	3.17	0.01	0.83
Said would attend program if selected	0.99	0.99	0.00	0.62
Index of wealth (z-score)	0.01	-0.38	-0.01	0.91
Monthly cash earnings (USD)	47.42	29.33	-4.19	0.33
Stock of savings (USD)	45.79	17.70	-13.07	0.02
Saves monthly	0.25	0.16	-0.01	0.75
Debt stock (USD)	7.45	2.83	-0.57	0.59
Main income source:				
Farming and animal-raising	0.32	0.24	0.04	0.21
Non-agricultural labor or business	0.29	0.58	-0.03	0.36
Sale of hunted meat	0.04	0.02	0.01	0.46
Potentially illicit activities	0.33	0.03	0.00	0.94
Firewood/charcoal sales	0.02	0.01	0.01	0.24
Mining	0.11	0.01	-0.01	0.53

	Means		Balance test (men only)	
Baseline covariate	All men (n=1123)	All women (n=151)	Difference (Control - Treatment)	p-value
Rubber	0.12	0.00	-0.00	0.89
Other	0.08	0.14	-0.01	0.76
Days of employment in past week	5.97	4.64	-0.11	0.30
Farming and animal-raising	2.75	1.63	-0.11	0.50
Skilled work	1.12	0.40	0.00	1.00
Petty business	1.03	1.87	-0.07	0.51
Casual work	2.16	0.83	-0.16	0.28
Rubber tapping	0.48	0.00	-0.03	0.72
Mining	0.63	0.03	-0.06	0.56
Logging	0.32	0.03	-0.01	0.90
Hunting	0.59	0.19	-0.09	0.25
Other	1.26	0.49	0.01	0.95
Any illicit activities in past week	0.32	0.03	0.01	0.70
Engaged in paid sex in past year		0.25		
Very interested in farming in the future	0.87	0.81	-0.01	0.56
Prefers ag. training to other skills	0.28	0.25	-0.01	0.68
Can access 10 acres farmland	0.90	0.91	0.03	0.21
Months of agricultural training	0.56	0.11	-0.00	0.99
# Years has raised animals	2.91	1.81	-0.26	0.36
# Years has farmed	4.76	1.79	-0.13	0.75
Times has sold crops	6.50	3.27	-0.32	0.71
Largest land ever farmed (acres)	20.50	16.85	-0.75	0.44
Educational attainment	5.88	3.77	0.21	0.36
Has very basic literacy	0.44	0.34	-0.02	0.49
Literate	0.27	0.12	0.03	0.31
Math questions correct (0-5)	2.33	1.50	0.19	0.04
Total months of training (including agricultural training)	3.02	1.74	0.01	0.99
Distress symptoms (0-3)	1.16	1.36	0.02	0.54
Post-traumatic stress symptoms (0-3)	0.93	1.13	0.05	0.11
Index of family relations (0-18)	13.59	11.93	0.27	0.17
Index of aggressive behaviors (0-12)	1.29	1.71	0.12	0.25
Dispute with authorities in past year	0.05	0.03	-0.01	0.34
Dispute with neighbor in past year	0.14	0.26	-0.01	0.67
Reports a physical fight in the past year	0.09	0.09	0.00	0.90
Fought with weapons in the past year	0.02	0.01	0.00	0.64
Ex-combatant	0.74	0.13	-0.01	0.83
Was on front lines of battle	0.17	0.05	-0.00	0.90
Months in a faction	27.4	6.95	5.73	0.00
Violent acts committed (0-3)	0.54	0.26	0.06	0.34
Violent acts experienced (0-9)	4.82	3.64	-0.01	0.94
Violent acts experienced by family (0-9)	4.64	4.68	0.02	0.82
Feels life better now than during war	0.98	0.99	0.02	0.05
Regrets wartime actions	0.56	0.36	0.08	0.01
Problems reintegrating with family	0.44	0.22	-0.13	0.06

	Means		Balance test (men only)	
Baseline covariate	All men (n=1123)	All women (n=151)	Difference (Control - Treatment)	p-value
Problems reintegrating with neighbors	0.36	0.15	-0.05	0.38
Faction caused trouble for own family	0.76	0.80	0.00	0.98
Faction caused trouble for hometown	0.95	1.06	-0.02	0.79
Faction caused trouble for current town	0.82	0.74	-0.08	0.26
Commander(s) gives support/jobs	0.05	0.02	-0.02	0.17
Has close relations with a commander	0.13	0.03	-0.01	0.62
Reports to a commander	0.02	0.01	-0.02	0.14
Index of ex-combatant relations (0-10)	5.48	3.11	0.03	0.84
Believes war will come again in Liberia	0.01	0.01	0.00	0.87
Would become fighter again	0.02	0.01	0.00	0.85
Would consider fighting in war elsewhere	0.01	0.01	-0.00	0.71

Notes: The third and fourth columns report the mean difference between the treatment and control groups, calculated using an OLS regression of baseline characteristics on an indicator for random program assignment plus block (village) fixed effects. USD variables are censored at the 99th percentile. Missing baseline data imputed at the median.

Estimating potential bias from imbalance. One way to assess potential bias from this imbalance is to use all available baseline covariates to predict the main outcomes. "Treatment effects" on these predicted outcomes can approximate the amount of bias we might expect from randomization imbalance. We report these results in Table B.3 for six major outcome families.

Table B.3: "Treatment effects" on predicted outcomes using baseline covariates

Outcome	Full sample	Non-attritors
Interest in agriculture index (z-score)	-0.013	-0.016
	[.014]	[.014]
Income index (z-score)	-0.018	-0.020
	[.020]	[.021]
Hours in potentially illicit activities	0.132	0.136
	[.310]	[.335]
Average weekly hours in legal activities	-0.277	-0.221
	[.464]	[.480]
Relationships with commanders (z-score)	-0.027	-0.023
	[.013]**	[.014]
Mobilization risk (z-score)	-0.005	-.008
	[.015]	[.015]

Notes: Each outcome is regressed on the baseline variables in Table 1 as well as randomization block dummies. Then the fitted value is regressed on assignment to treatment and strata dummies.

In general the treatment-control difference is small (e.g. $<.02$ standard deviations in agricultural interest or income). The sign is also such that the predicted bias leads us to underreport treatment effects. The exception is relationships with commanders, where the predicted bias is negative and statistically significant (probably because of the length of time in a faction variable). As we will see, we see only a weak negative treatment effect on relations with commanders, and already treated this decline with caution, concluding there is little evidence of an effect of treatment on this variable. These predicted bias results only bolster this conclusion.

B.2 Assessing potential bias from endline survey timing

Roughly two-thirds of the sample was found in the first 10 weeks. The remaining third took three months to track. To reduce bias from the timing of their survey, we first tracked a random half of the unfound, adding the second half after two months.

In principle, this long survey period could introduce bias if late respondents are systematically different and have seasonal or other time-varying outcome responses. For two reasons

this does not appear to be the case in our sample.

First, there is little difference between those found in the first 10 weeks and those who had migrated and hence took longer to survey and track, implying that timing of the survey is not too systematically selective. A test of balance between those found before and after 10 weeks (not shown, but available on request) shows that those found after 10 weeks were a year younger, were 15 percentage points less likely to be married, and were 2 percentage points less like to be an ex-combatant, but there is no statistically significant difference in baseline wealth, education, health, occupation, income, aggression, or war experiences.

Second, if we compare treatment effects between men found before and after the 10 weeks, or between the two random groups of unfound men, we see generally the same sign and magnitude of treatment effects. Results are not displayed but are available on request.

B.3 Attrition and compliance analysis

Table B.4 presents results from an ordinary least squares (OLS) regression of attrition on treatment assignment, covariates, and block fixed effects.

Table B.4: Correlates of attrition, compliance, and package selection

	Dependent variable (and sample)			
	Unfound at endline (full sample)		Attended ≥1 day (if assigned to treatment)	
Baseline covariate	Coeff.	Std. Err.	Coeff.	Std. Err.
Assigned to treatment	-0.020	[.016]		
Age	0.000	[.002]	-0.006	[.004]
Lives with spouse/partner	0.000	[.023]	-0.090	[.036]**
Number of children	-0.006	[.004]	0.008	[.012]
Disabled, injured, or ill	-0.003	[.016]	-0.062	[.042]
Years of schooling	0.001	[.002]	-0.007	[.004]*
Said would attend if selected	0.114	[.057]**	0.007	[.190]
Durable assets (z-score)	0.000	[.010]	-0.002	[.017]
Stock of savings (USD)	0.000	[0000]	0.000	[0000]
Debt stock (USD)	-0.001	[0000]**	-0.003	[.001]***
Agricultural experience (z-score)	0.001	[.008]	0.068	[.020]***
Aggressive behaviors (0-12)	0.016	[.006]***	0.004	[.009]
Main income: Illicit resources	-0.006	[.020]	0.071	[.046]
Main income: Nonfarm work	0.026	[.021]	0.014	[.038]
Very interested in farming	0.012	[.022]	0.097	[.056]*
Ex-combatant	0.071	[.019]***	0.077	[.050]
Months in a faction	0.000	[0000]	0.000	[0000]
Ex-commander relations (z-score)	-0.017	[.010]*	-0.025	[.024]
Patience index (0-4)	-0.002	[.010]	-0.009	[.019]
Risk affinity index (0-3)	0.003	[.018]	-0.007	[.032]
Observations	1,123		586	
Dependent variable mean	0.077		0.422	
R-squared	0.11		0.15	

Notes: All columns are calculated via OLS regression with block fixed effects. Missing baseline data are imputed at the median. The F-test is on all covariates excluding block and region dummies. Robust standard errors are in brackets, clustered at the village level. *** p<0.01, ** p<0.05, * p<0.1

Note that the program appears to have had only a small effect on migration at the time of the endline. 45% of both the treatment and control group changed villages since baseline, and 37% moved within the six months before the survey. The control group was slightly less likely to change their county than treated men: 14% of controls changed their county and this is 5.7 percentage points higher among treated men, perhaps because they chose to relocate to more central agricultural markets. 74% of control men also express an interest

in staying in their current community and this settledness is 7.8 percentage points (11%) higher among treated men.

B.4 Accounting for potential spillover effects

Hotspot communities were limited, and we could not randomize at the community level. It is possible there are unobserved spillover effects from the treatment group to controls.

For instance, the departure of a small number of high risk men from each community could reduce the local labor supply and increase wages for illicit work. It could also break down armed social networks and the power basis of local strongmen, or increase state and UN presence in the community, thus reducing the returns to illicit work. When the trainees return, agricultural knowledge could be passed to the control group, increasing their productivity. Or successful socialization could influence disutility from illicit labor through peer effects.

Accurate population figures do not exist, but we estimate treated men typically represent only 1 to 5% of the adult workforce in these villages. Also, roughly half of both the treatment and control group changed communities between baseline and endline. Moreover, while transport costs are high in Liberia, there is considerable migration by this population for work, especially in mining and local labor supply is highly elastic (implying that we should not be affected by the departure of a few men). As a result, we expect within-community spillovers to the control group to be minor. Finally, agricultural hours increase just 20% for a handful of men per community, and so general equilibrium effects of output on prices seem extremely unlikely.

Moreover, even if present, the spillovers discussed above should not lead us to overstate treatment effects. To the extent that the control group become more productive in agriculture, or are influenced by positive peer effects, our estimated treatment effects towards zero will understate the true program impacts on occupational choice. Any impact on illicit wages will affect the incentives for both treatment and control group members (although control

group members would be more affected by an increase in w than someone who received inputs and training).

In principle, individuals who are not assigned to treatment may feel discouraged and thus reduce the hours they devote to agriculture, but we see no such trend. Alternatively, returning farmers could have a negative impact on the control group by crowding them out of agriculture or if increased output lowered local prices, but qualitatively our assessment is that production was too small to affect local prices. We do not, however, have the data or identification to test this formally.

C A model of illegal occupational choice

C.1 Setup

We assume people allocate a fixed total labor allocation $\bar{L}$ between leisure l, legal activities L^a (such as agriculture), and illicit activities L^m (such as unlicensed mining or mercenary work). We assume people have the utility function $U(c, l, \sigma L^m)$, where σ represents a preference for peaceful work. For instance, some people may receive disutility from simply engaging in illicit activities. We include this preference term in the utility function to distinguish it from punitive or pecuniary disincentives to engage in crime (such as the risk of punishment).[24]

People have farming technology $F(\theta, L_t^a, X_{t-1})$ where θ is productivity (driven by locally-available technologies and techniques) and X_{t-1} is a vector of non-labor inputs such as seeds that require capital to buy. We assume investment decisions on non-labor inputs are made in the previous period, while hours in agricultural labor are determined in the current period.[25]

[24]Other than this nonstandard preference term, we make the standard assumptions that $U_c^{'} \geq 0$, $U_l^{'} \geq 0$, $U_{\sigma L^m}^{'} \leq 0$ and $U_{cc}^{''} < 0$, $U_{ll}^{''} < 0$, $\partial^2 U / \partial L_m^2 \leq 0$. Examples of utility functions that satisfy these assumptions are $U(c, l, \sigma L^m) = u(c, l) - \sigma L^m$ and $U(c, l, \sigma L^m) = u(c, l) - \sigma (L^m)^2$.

[25]For the production function, we make the standard assumptions that $F_\theta^{'} \geq 0$, $F_L^{'} \geq 0$, $F_X^{'} \geq 0$, $F_{\theta\theta}^{''} < 0$, $F_{LL}^{''} < 0$, $F_{XX}^{''} < 0$, and $F_{\theta L}^{''} \geq 0$, $F_{\theta X}^{''} \geq 0$, $F_{LX}^{''} \geq 0$. For ease of analysis, we also assume that marginal product of labor in agriculture is zero when there is no input; but as long as there is some level of input, marginal product of labor for the first unit of labor will be infinity: $F_L^{'}(\theta, L^a, 0) \equiv 0$, but $\lim_{L^a \downarrow 0} F_L^{'}(\theta, L^a, X) = +\infty$ as long as $X > 0$. This assumption guarantees that as long as there is positive investment in inputs,

We assume the illicit activity pays an hourly wage that may vary over time, w_t.[26] Illicit work also comes with a risk of future punishment. We assume this cost is a linear function of last periods' hours in illicit activities:$\rho f L^m_{t-1}$, where ρ is the probability of apprehension and f is the punishment. Punishment includes imprisonment and foregone wages, but it could also include the withholding of a "peace dividend" such as a cash transfer.[27]

Total earnings from agricultural production and illicit activities is thus $y_t \equiv p_t F(\theta, L^a_t, X_{t-1}) + w_t L^m_t - \rho f L^m_{t-1}$, where p is the price for output and q prices for inputs. In addition to investing in agricultural inputs, the person can also invest or borrow through a riskless asset with constant returns $1 + r$. In each period t, the person decides how much to invest for next period a_{t+1} and reaps interests ra_t from last period's investments.

We begin by ignoring uncertainty and risk aversion. Then we introduce uncertainty in the form of prices, wages, and productivities following independent stochastic processes. Uncertainty in prices can reflect variation in general supply and market conditions, uncertainty in wages can reflect the fact that labor returns are typically conditional on output (e.g. a minimum wage plus a payment proportional to gold or diamonds discovered or battles won), and uncertainty in productivity is a simple way of capturing uncertainty in output due to weather and other unexpected shocks. We keep the assumption that input investment decisions are made one period ahead of production, however we add a new assumption that decision on hours in both sectors L^a_t and L^m_t are made at time $t-1$, one period before all prices and productivity levels are realized.

hours in agricultural labor will be positive. We also assume that returns to inputs is non-negative but bounded above $0 \leq F'_X(\theta, L^a, X) \leq \theta M$ (which is likely the case in agriculture production).

[26] In other words, crime principally uses labor as an input. For example, mining requires capital and land rights, and the "bosses" who hold these hire men as "mining boys" on short-term renewable contracts that pay a daily wage plus a payment tied to output. While there is uncertainty in output, and hence the wage, output is principally a function of labor inputs by the laborer (given a boss' capital).

[27] As described in the paper, treated men who chose to specialize in animal-raising expected to receive an in-kind capital or cash transfer. In this case, ρ is the possibility of missing the transfer if he leaves town to mine or fight, and f the value of the transfer.

C.2 Benchmark case: Perfect financial markets and no uncertainty

We begin with the case where there are no financial market imperfections. The person's problem is

$$\underset{c_t>0,0\leq l_t\leq \bar{L},X_t,L_t^m,L_t^a}{max} \sum_{t=0}^{\infty} \delta^t \left[U(c_t, l_t, \sigma L_t^m)\right]$$

$$\begin{aligned} s.t.\ \ c_t + a_{t+1} + q_t X_t &= y_t + (1+r)a_t \quad for\ each\ t \\ a_0 &\qquad given \end{aligned}$$

where $y_t \equiv p_t F(\theta, L_t^a, X_{t-1}) + w_t L_t^m - \rho f L_{t-1}^m$ and $L_t^a + L_t^m + l_t \equiv \bar{L}$.

The first order conditions are as follows:

$$\frac{U_l'(t)}{U_c'(t)} = p_t F_{L^a}'(t) \quad if\ L_t^a > 0 \tag{1}$$

$$\frac{U_l'(t)}{U_c'(t)} - \sigma \frac{U_{\sigma L^m}'(t)}{U_c'(t)} = w_t - \frac{\rho f}{1+r} \qquad if\ L_t^m > 0 \tag{2}$$

$$\frac{U_c'(t)}{U_c'(t+1)} = \delta \frac{p_{t+1}}{q_t} F_X'(t+1) \quad if\ X_t > 0 \tag{3}$$

$$\frac{U_c'(t)}{U_c'(t+1)} = \delta(1+r) \tag{4}$$

$$c_t + a_{t+1} + q_t X_t = p_t F(\theta, L_t^a, X_{t-1}) + w_t L_t^m - \rho f L_{t-1}^m + (1+r)a_t \tag{5}$$

where for ease of notation we use $U(t)$ to denote $U(c_t, l_t, \sigma L_t^m)$ and $F(t)$ to denote $F(\theta, L_t^a, X_{t-1})$.

C.2.1 Occupational choice

To find the conditions for engaging in each sector, first consider the case where illicit activity is not feasible. In this case the decision to engage in agricultural production depends on his productivity θ, the output-input price ratio p_{t+1}/q_t, his wealth level and the returns on other financial assets r. We use c^{aa}, L^{aa} and X^{aa} to denote consumption, labor and input choices in this scenario. In each period t the person chooses L_t^{aa} to satisfy $\frac{U_l'(c_t^{aa}, \bar{L}-L_t^{aa}, 0)}{U_c'(c_t^{aa}, \bar{L}-L_t^{aa}, 0)} = p_t F_L'(\theta, L_t^{aa}, X_{t-1}^{aa})$ taking X_{t-1}^{aa} as given, and he chooses agricultural investment X_t^{aa} to satisfy

$\frac{p_{t+1}}{q_t}F_X^{'}(\theta, L_{t+1}^{aa}, X_t^{aa}) = 1 + r$, taking expected p_{t+1} and L_{t+1}^{aa} as given.

Now, taking levels of c^{aa}, L^{aa} and X^{aa} as given, we can look at people' decision to engage in illicit activities. People will engage in illicit activities if and only if

$$w_t - \frac{\rho f}{1+r} \geq \frac{U_l^{'}(c_t^{aa}, \bar{L} - L_t^{aa}, 0)}{U_c^{'}(c_t^{aa}, \bar{L} - L_t^{aa}, 0)} + \sigma \frac{-U_{\sigma L^m}^{'}(c_t^{aa}, \bar{L} - L_t^{aa}, 0)}{U_c^{'}(c_t^{aa}, \bar{L} - L_t^{aa}, 0)}. \tag{6}$$

which says the expected returns from illicit activities (wage minus the present value of expected punishment) must be higher than the highest possible marginal rate of substitution between leisure and consumption the person can achieve without engaging in illicit activities. Since $-U_{\sigma L^m}^{'}/U_c^{'} > 0$, a rise in σ means more people will drop out of illicit activities.

If condition 6 is satisfied and if $X_{t-1} > 0$, the person then chooses L_t^m and L_t^a such that the marginal product of labor in agriculture equals his net marginal gains from illicit activities, which also equals his marginal rate of substitution between leisure and consumption: i.e. conditions 1 and 2 will be satisfied. Notice that L_t^m may not always be positive. People will not engage in illicit activities if any or all three of the following happens: (1) w_t is very low relative to price level p_t and potential punishment ρf; (2) productivity in agriculture θ is very high; and (3) the degree of aversion to illicit activities σ is very high.

Now we come back to the level of X_t. People choose inputs for period $t+1$ at time t with the correct expectation of next period's prices and wages. Each person chooses X_t such that investment returns in agriculture and alternative assets are equalized: i.e. condition 3 $\frac{p_{t+1}}{q_t}F_X^{'}(\theta, L_{t+1}^{a}, X_t) = 1 + r$ will be satisfied.

Comparative Statics

First, we define the elasticities of illicit labor to the parameters or variables most likely to be affected by the intervention, θ, X, ρf and σ: $\varepsilon_\theta = \frac{dL_m}{d\theta}/\frac{L_m}{\theta}$, $\varepsilon_X = \frac{dL_m}{dX}/\frac{L_m}{X}$, $\varepsilon_{\rho f} = \frac{dL_m}{d\rho f}/\frac{L_m}{\rho f}$, , $\varepsilon_\sigma = \frac{dL_m}{d\sigma}/\frac{L_m}{\sigma}$. We are also interested in the responsiveness of labor supply to the illicit wage.

w_t

We focus on the case where, as wages w_t or the marginal product of labor $p_t F_L^{'}(t)$ increases, the substitution effect is greater than the income effect on hours (a reasonable assumption when both wealth and income are relatively low). If the illicit wage rises, people will engage in illicit activities as w_t surpasses their threshold wage level defined in inequality (6). For those who already engage in both activities, L_t^m rises and L_t^a falls so that equations (1) and 2 are satisfied. The ratio $\frac{L^m}{L^m+L^a}$ rises–people are more inclined to engage in illicit activities as w rises. Then the right hand side of (3) falls (since labor and other inputs are complements), which means the optimal level of X_{t-1} must fall in order to satisfy equations (3) and (4). Since we assumed substitution effects always dominate income effects, total hours $L_t^a + L_t^m$ should rise. Total earnings $y_t \equiv p_t F(\theta, L_t^a, X_{t-1}) + w_t L_t^m$ would rise as w_t rises because equilibrium returns to both sectors are now higher. Therefore, $\frac{\partial L_t^a}{\partial w_t} < 0$, $\frac{\partial L_t^m}{\partial w_t} > 0$, $\frac{\partial X_{t-1}}{\partial w_t} < 0$, $\frac{\partial l_t}{\partial w_t} < 0$ and $\frac{\partial y_t}{\partial w_t} > 0$. It's worth noting that because of equations (3) and (4) the returns to investment in inputs will not change as w_t changes, despite the changes in L^a and X.

ρf

In the absence or risk and uncertainty the effect of an increase in ρf will be the same as the effect of a fall in w_t. Future punishment essentially acts as a monetary penalty to wages in the illicit sector. Therefore, an increase in ρf will increase agricultural hours and earnings from agriculture, but reduce hours in illicit activities, total hours, and total earnings: , $\frac{\partial L_t^a}{\partial \rho f} > 0$, $\frac{\partial L_t^m}{\partial \rho f} < 0$ (i.e. $\varepsilon_{\rho f} < 0$), $\frac{\partial X_{t-1}}{\partial \rho f} > 0$, $\frac{\partial l_t}{\partial \rho f} > 0$ and $\frac{\partial y_t}{\partial \rho f} < 0$.

σ

As σ rises, we will see a change in illicit work on the extensive margin: fewer people will engage in these activities since a higher σ implies a higher right hand side in inequality (6). For those who already engage in both activities, to keep equation (2) an identity, the optimal level of L_t^m must fall and L_t^a must rise. The ratio $\frac{L^m}{L^m+L^a}$ falls–people take time

away from illicit activities and put more time into agriculture production. X_{t-1} then rises because of the rise in L_t^a . The effect on hours and total earnings is less obvious. But holding everything else constant (i.e. marginal utility of consumption and leisure, wages, prices and productivity), an increase in σ will lead to a fall in $w_t + \sigma \frac{U'_{\sigma L^m}(t)}{U'_c(t)}$, which means a lower level of both $\frac{U'_l(t)}{U'_c(t)}$ and marginal productivity of labor $p_t F'_L(t)$ in equilibrium. Even though earnings from agriculture will be higher, the first order effect of σ on L_t^m dominates its effects on L_t^a , which means c_t and y_t will be lower in equilibrium, and l_t higher. Therefore, $\frac{\partial L_t^a}{\partial \sigma} > 0$, $\frac{\partial L_t^m}{\partial \sigma} < 0$, $\varepsilon_\sigma < 0$, $\frac{\partial X_{t-1}}{\partial \sigma} > 0$, $\frac{\partial l_t}{\partial \sigma} > 0$ *and* $\frac{\partial y_t}{\partial \sigma} < 0$.

θ

Now we consider the effect of an increase in the available agricultural technology θ. On the extensive margin, fewer people will engage in illicit activities and more will engage in agriculture activities.[28]

On the intensive margin, for those who already engage in both activities, the effect of a rise in productivity level θ is ambiguous. Because of equations (3) and (4), the returns to investment in inputs will not change (at least so long as we don't have any financial constraints or risk). Input level X would rise and labor-input ratio $\frac{L^a}{X}$ will fall. However, the direction of change in L^a depends on the shape of production and utility functions as well as the person's wealth level. So do the effects on leisure and hours in illicit activities. Both earnings in agriculture and total earnings y will rise. Consumption will rise. If agricultural production is very labor intensive (which is likely the case), then a rise in θ will lead to a rise in both L^a and $p_t F'_L(t)$, which means a higher level of $\frac{U'_l(t)}{U'_c(t)}$ in equilibrium, and therefore a lower level of leisure. The effect on L^m depends on the sign of $U'_{\sigma L^m}$. To sum up, $\frac{\partial X_{t-1}}{\partial \theta} > 0$, $\frac{\partial y_t}{\partial \theta} < 0$ for certain, and $\frac{\partial L_t^a}{\partial \theta} > 0$ and $\frac{\partial l_t}{\partial \theta} < 0$ if agriculture production is labor intensive and ambiguous otherwise.

[28] A higher θ leads to a relatively large increase in c^{aa} and an ambiguous but relatively small change in l^{aa}, which together implies a higher right hand side in inequality (6).This is intuitive: as productivity rises, agriculture alone can provide high levels of consumption and leisure for the person such that illicit activities do not seem attractive any more.

Averaging the effect on both margins, a rise in θ leads to a fall in average L^m and arise in L^a. Therefore, $\frac{L^m}{L^m+L^a}$ falls and $\varepsilon_\theta < 0$. Note that an increase in p_t (or decrease in q_t) would have similar effects as an increase in θ.

X_t

Since input X_t is a choice variable and there are no financial market imperfections, any intervention that provides X_t will only have effects in the short-term: reducing returns in agriculture production to a level below $1 + r$, increasing consumption and total earnings, and reducing hours in illicit activities. The short-term effects on L^a and l will depend on the labor intensity of the production function and how important leisure is in the utility function. However, in the long run, this capital will be divested and everything will go back to equilibrium levels. In the short run, $\frac{L^m}{L^m+L^a}$ falls and $\varepsilon_X < 0$; in the long run, $\frac{L^m}{L^m+L^a}$ returns to its normal level and $\varepsilon_X = 0$.

C.3 The case of credit constraints without uncertainty

We consider the simplest case of credit constraints where there is no borrowing whatsoever: $a_t \geq 0$. Equation (4) becomes $\frac{U_c'(t)}{U_c'(t+1)} = \delta(1 + r)$ if $a_t > 0$ and $\frac{U_c'(t)}{U_c'(t+1)} > \delta(1 + r)$ if $a_t = 0$. Combining this with(3), we have

$$\frac{p_{t+1}}{q_t} F_X'(\theta, L_{t+1}^a, X_t) = max\{1 + r, \frac{1}{\delta}\} \tag{7}$$

This implies that fewer people with low wealth will engage in agriculture, and those that do will invest less in inputs if the credit constraint binds. Unlike the benchmark model, patience now matters for input decisions: the impatient will now under-invest in agriculture.

In a low wealth sample like ours, compared to the benchmark case, the credit constraint leads to a lower level of investment X for the impatient types whose $\delta < \frac{1}{1+r}$, fewer hours in agriculture (lower L^a), but more hours in illicit activities (higher L^m). Some low patience

people will have to give up agriculture altogether because of the credit constraints. On average, $\frac{L^m}{L^a+L^m}$ will be higher than in the benchmark case.

Interventions in θ and σ will have similar effects as in the benchmark case; ε_θ and ε_σ will have the same signs as in the benchmark case. However, the magnitude of the effects change. A rise in θ will now have a smaller effect than in the benchmark case, because the credit constraint makes it harder for everyone to increase their investment in agriculture. On the contrary, a rise in σ will now have a bigger effect than in the benchmark case, because the credit constraint makes illicit activities more attractive than in the benchmark case.

Perhaps most importantly, interventions in X_t will now have long-term effects: inducing people to engage in agriculture activities, and increasing input investments for those who were credit constrained. $\varepsilon_X < 0$ both in the long and in the short run. It is worth noting that in this case giving people ΔX_t amount of inputs should have the exact same effect as giving them a cash transfer of $q_t \Delta X_t$.

In other words, $\epsilon_\sigma < 0$, $\varepsilon_\theta < 0$ and $\varepsilon_X < 0$; the magnitude of ε_θ and ε_σ are lower than in the benchmark case; while ε_X is higher.

C.4 The case of credit constraints with uncertainty and incomplete insurance

We now turn to credit constraints in the presence of uncertainty and risk aversion. For simplicity we assume there is no insurance market, and that the riskless asset remains riskless.

The first order conditions now become

$$
\begin{aligned}
\mathbb{E}_{t-1}\left[U_l^{'}(t)\right] &= \mathbb{E}_{t-1}\left[U_c^{'}(t)p_t F_L^{'}(t)\right] \qquad if\ L_t^a > 0 \\
\mathbb{E}_{t-1}\left[U_l^{'}(t)\right] - \mathbb{E}_{t-1}\left[\sigma U_{\sigma L^m}^{'}(t)\right] &= \mathbb{E}_{t-1}\left[U_c^{'}(t)w_t\right] - \delta\mathbb{E}_t\left[U_c^{'}(t+1)\right]\rho f \quad if\ L_t^m > 0 \\
U_c^{'}(t) &= \delta\mathbb{E}_t\left[U_c^{'}(t+1)\frac{p_{t+1}}{q_t}F_X^{'}(t+1)\right] \quad if\ X_t > 0 \\
U_c^{'}(t) &= \delta(1+r)\mathbb{E}_t\left[U_c^{'}(t+1)\right] \qquad if\ a_{t+1} > 0 \\
c_t + a_{t+1} + q_t X_t &= p_t F(\theta, L_t^a, X_{t-1}) + w_t L_t^m + (1+r)a_t
\end{aligned}
$$

Inequality (6), the threshold level of w_t that people will engage in illicit activities, now becomes

$$
\begin{aligned}
&\mathbb{E}_{t-1}\left[U_c^{'}(c_t^{aa}, \bar{L} - L_t^{aa}, 0)w_t\right] - \delta\mathbb{E}_t\left[U_c^{'}(c_{t+1}^{aa}, \bar{L} - L_{t+1}^{aa}, 0)\right]\rho f \quad \geq \\
&\mathbb{E}_{t-1}\left[U_l^{'}(c_t^{aa}, \bar{L} - L_t^{aa}, 0)\right] - \mathbb{E}_{t-1}\left[\sigma U_{\sigma L^m}^{'}(c_t^{aa}, \bar{L} - L_t^{aa}, 0)\right]
\end{aligned}
$$

and we have another equation for the returns of input investments under uncertainty

$$
\mathbb{E}_t\left[\frac{p_{t+1}}{q_t}F_X^{'}(\theta_{t+1}, L_{t+1}^a, X_t)\right] - (1+r) = -(1+r)Cov_t\left(\frac{p_{t+1}}{q_t}F_X^{'}(\theta_{t+1}, L_{t+1}^a, X_t), SD_t\right) \quad (8)
$$

and

$$
(1+r)\left[1 + Cov_t\left(\frac{p_{t+1}}{q_t}\theta_{t+1}M, SD_t\right)\right] \leq \mathbb{E}_t\left[\frac{p_{t+1}}{q_t}\theta_{t+1}M\right] \quad (9)
$$

where $SD_t = \frac{\delta U_1^{'}(t+1)}{U_1^{'}(t)}$ is the stochastic discount factor.

Now input X and hours L^m and L^a all depend both on the variance of returns in the two sectors and the level of initial wealth a_0. Those with high levels of wealth a_0 will turn away from both activities by reducing X, L^m and L^a and investing instead in other riskless assets X. L^m and L^a will all be lower than both the benchmark case and the credit constraint only case.

People with low levels of wealth (i.e. our sample) will not be able to live off savings alone,

so they will have to invest more in both sectors by increasing X, L^m and L^a if both sectors are equally risky. Otherwise, if one of the sectors are less risky than the other, people will invest more time in that sector. $\frac{L^m}{L^a+L^m}$ will be higher than in the case without uncertainty only if illicit activities are less risky than agriculture.

Importantly, interventions in θ will have greater effects than in the benchmark and credit constraint case, because an increase in θ now also makes agriculture relatively less risky. A rise in σ will also have a bigger effect than without uncertainty, because risk aversion will reinforce the rise in aversion and further reduce hours in illicit activities. Inventions in X_t will have a similar long-term effect as in the credit constraint only scenario. However, for the risk averse people the effect of ΔX_t will be greater than a cash transfer $q_t \Delta X_t$. The effects increase as the level of risk aversion increases, and also as the level of risk increase. Similarly, a change in either the probability or extent of punishment will have a bigger effect on hours and earnings: an increase in ρf will make illicit activity even more unattractive, as it reduces the expected returns to illicit activity while not reducing the risk of such activities.

In other words, $\epsilon_\sigma < 0$, $\varepsilon_\theta < 0$, $\varepsilon_{\rho f} < 0$ and $\varepsilon_X < 0$; the magnitude of all four elasticities will be higher than in the credit constraint without uncertainty case.

D Additional treatment effects analysis

D.1 ITT and TOT estimates with all index components

Table D.1 reports intent-to-treat (ITT) impacts of the program on our measures of mercenary recruitment. Tables D.2 to D.7 expand the family indices that appear in the main paper. Finally, Tables D.9 and D.10 expand the number of outcomes analyzed for the marginal impact of package choice.

Table D.1: Intent to treat impacts on mercenary recruitment activities

Outcome	Control Mean (1)	ITT estimate Coeff. (2)	ITT estimate SE (3)
Mobilization activities/attitudes (z-score)	0.09	-0.156	[0.065]**
Direct recruitment activities (0-12)	0.94	-0.183	[0.097]*
Talked to a commander in last 3 months	0.45	-0.083	[0.037]**
Would go if called to fight for tribe	0.05	-0.012	[0.011]
Has been approached about going to CI	0.07	0.001	[0.017]
Would go to CI for $250	0.01	-0.005	[0.008]
Would go to CI for $500	0.03	-0.007	[0.010]
Would go to CI for $1000	0.08	-0.032	[0.016]*
Will move towards CI border area	0.10	-0.017	[0.020]
Invited to secret meeting on going to CI	0.04	0.003	[0.013]
Attended secret meeting on going to CI	0.03	-0.010	[0.009]
Was promised money to go to CI	0.03	0.001	[0.011]
Willing to fight if war breaks out in CI	0.04	-0.014	[0.012]
Has plans to go to CI in the next month	0.01	-0.009	[0.007]
Indirect recruitment measures (0-4)	1.48	-0.121	[0.062]*
Talks about the CI violence with friends	0.68	-0.035	[0.034]
Has a partisan preference in CI	0.66	-0.089	[0.034]***
Knows people who went to CI to fight	0.10	-0.016	[0.016]
Knows people given money to go to CI	0.04	0.020	[0.013]

Notes: This table includes all questions used in a "mobilization risk" survey module.Standard errors are robust and clustered at the village level.

*** p<0.01, ** p<0.05, * p<0.1

Table D.2: Economic family indices expanded

Outcome	Control Mean	Treatment effect estimates (n=1025) ITT Coeff.	ITT Std. Err.	TOT Coeff.	TOT Std. Err.
	(1)	(2)	(3)	(4)	(5)
Raised crops/animals in past year:					
Cleared land for themselves	0.53	0.0920	[0.033]***	0.1200	[0.039]***
Grew seedlings for themselves	0.23	0.2700	[0.034]***	0.3530	[0.038]***
Planted crops for themselves	0.44	0.1350	[0.035]***	0.1770	[0.042]***
Sold crops for themselves	0.27	0.0910	[0.031]***	0.1190	[0.037]***
Raised animals for themselves	0.26	0.0610	[0.032]*	0.0800	[0.039]**
Sold animals for themselves	0.11	0.0020	[0.023]	0.0030	[0.027]
Index of stealing/theft activity (z-score)	-0.05	0.0460	[0.064]	0.0600	[0.077]
When things are rough, do you sometimes "hustle from people" (steal)?	0.01	0.0070	[0.011]	0.0090	[0.013]
You sometimes "correct someone's mistake" (pickpocket)?	0.02	0.0130	[0.011]	0.0170	[0.013]
You sometimes "take things from behind someone" (steal, shoplift)?	0.03	0.0020	[0.011]	0.0030	[0.013]
Can you sometimes "scrape from people" (cheat or con)?	0.03	0.0050	[0.012]	0.0060	[0.014]
You sometimes "jump on people" to take their things (mug, rob)?	0.01	-0.0030	[0.005]	-0.0040	[0.006]
You "use anything when you hustling" (are you armed)?	0.01	-0.0010	[0.007]	-0.0020	[0.008]
How many times every week you can hustle (steal) from people?	0.06	0.0300	[0.035]	0.0390	[0.042]
Do you plan to continue this hustle in future?	0.01	0.0010	[0.005]	0.0010	[0.006]
Are you surviving mainly on hustling from people?	0.02	-0.0040	[0.009]	-0.0050	[0.011]
Settlement and Migration					
Changed county since baseline survey	0.14	0.044	[0.023]*	0.057	[0.027]**
Changed communities since baseline survey	0.45	0.005	[0.038]	0.007	[0.045]
Changed communities in the past 6 months	0.37	-0.006	[0.033]	-0.008	[0.040]
Interested in staying in current community	0.74	0.058	[0.030]*	0.076	[0.036]**

Notes: Columns (2) and (3) report the intent-to-treat (ITT) estimate, and columns (4) and (5) estimate the effect of treatment on the treated (TOT) via two-stage least squares, where assignment to treatment is used as an instrument for attending at least a day. All regressions include block dummies and baseline covariates. Standard errors are robust and clustered at the village level.

*** p<0.01, ** p<0.05, * p<0.1

Table D.3: Peer group index expanded

		Treatment effect estimates (N=1025)			
	Control	ITT		TOT	
Outcome	Mean	Coeff.	SE	Coeff.	SE
	(1)	(2)	(3)	(4)	(5)
Are your closest friends...?					
Interested in school	0.78	-0.002	[0.027]	-0.003	[0.032]
Participate in community meetings	0.96	-0.014	[0.015]	-0.018	[0.018]
Go to church or mosque	0.91	-0.015	[0.021]	-0.019	[0.025]
Have a business or a job	0.58	0.055	[0.030]*	0.072	[0.035]**
Save money regularly	0.77	0.043	[0.030]	0.056	[0.036]
Give you advice	0.97	-0.015	[0.012]	-0.019	[0.014]
Work hard	0.97	0.000	[0.010]	0.000	[0.013]
Share with you if they have money and you don't	0.95	-0.024	[0.016]	-0.032	[0.019]*
Make you feel better when you are feeling badly	0.90	0.032	[0.023]	0.042	[0.027]
Can be trusted to guard your valuables	0.87	0.003	[0.020]	0.003	[0.024]
Get drunk regularly	0.26	-0.004	[0.030]	-0.005	[0.036]
Beg for money from strangers	0.06	0.013	[0.017]	0.017	[0.020]
Use drugs	0.06	-0.010	[0.016]	-0.014	[0.020]
Gamble	0.06	0.001	[0.019]	0.002	[0.023]
Steal other people's property	0.03	0.003	[0.011]	0.004	[0.013]
Break and enter houses and businesses	0.01	0.004	[0.010]	0.006	[0.012]
Do armed robbery/mugging	0.00	-0.003	[0.007]	-0.003	[0.008]
Often have small conflicts with authorities	0.22	-0.011	[0.030]	-0.015	[0.035]
Often have major conflicts with the authorities	0.05	0.018	[0.014]	0.024	[0.017]

Notes: See Table D.2.

Table D.4: Social and family support family indices expanded

		Treatment effect estimates (N=1025)			
	Control	ITT		TOT	
Outcome	Mean	Coeff.	SE	Coeff.	SE
	(1)	(2)	(3)	(4)	(5)
Index of social support in last month (z-score)	-0.06	0.144	[0.072]**	0.188	[0.085]**
Anyone joked with you to make you happy?	1.41	0.142	[0.076]*	0.186	[0.092]**
Anybody help take care of your things/family?	1.71	0.062	[0.082]	0.082	[0.098]
Anybody help you with your work?	1.10	0.099	[0.063]	0.130	[0.074]*
You shared your feelings and they listened?	1.59	0.028	[0.061]	0.037	[0.073]
Anybody sat with you when you feeling lonely?	1.38	0.022	[0.068]	0.029	[0.081]
Anybody helped you to make your way through life?	0.99	0.030	[0.061]	0.039	[0.073]
Anybody lent you things beside money?	0.67	0.163	[0.062]***	0.213	[0.074]***
Anyone lent or gave you money?	0.55	0.011	[0.066]	0.014	[0.080]
Index of family relations (z-score)	-0.00	0.101	[0.062]	0.133	[0.075]*
See members of your family often?	2.06	0.114	[0.062]*	0.149	[0.076]**
Do you attend family meetings?	1.50	0.127	[0.080]	0.166	[0.097]*
Your family concerned about you?	2.40	0.029	[0.053]	0.037	[0.063]
Do they advise or encourage you?	2.22	-0.007	[0.063]	-0.010	[0.075]
Family members help you when you are stuck?	1.36	0.152	[0.071]**	0.198	[0.087]**
You have disputes in your family?	2.68	-0.027	[0.044]	-0.036	[0.053]
You caused trouble for them?	2.87	-0.017	[0.032]	-0.022	[0.038]

Notes: See Table D.2.

Table D.5: Antisocial behaviors family index expanded

		Treatment effect estimates (N=1025)			
	Control	ITT		TOT	
Outcome	Mean	Coeff.	SE	Coeff.	SE
	(1)	(2)	(3)	(4)	(5)
In the last month...					
Was unable to control your anger	0.48	0.044	[0.046]	0.058	[0.056]
Was quick to react against others	0.19	0.059	[0.042]	0.077	[0.050]
Said cruel things to other people	1.36	-0.033	[0.076]	-0.043	[0.091]
Let other people see your frustration	0.49	0.111	[0.057]*	0.146	[0.068]**
Intentionally destroyed property	0.05	-0.003	[0.020]	-0.004	[0.024]
Refused to take advice	0.14	0.022	[0.036]	0.029	[0.044]
Cheated other people	0.13	0.034	[0.037]	0.044	[0.044]
Had major arguments with others	0.07	0.000	[0.018]	0.001	[0.022]
"Held your heart" when angry	0.94	0.036	[0.068]	0.047	[0.081]
Threatened people	0.10	0.002	[0.030]	0.002	[0.035]
Took other people's things without asking	0.03	0.046	[0.019]**	0.060	[0.023]***
In the past 6 months....					
Had a fight or angry dispute	0.70	0.000	[0.115]	0.000	[0.138]
Had a confrontations with leaders or police	0.64	-0.179	[0.148]	-0.234	[0.178]

Notes: See Table D.2.

Table D.6: Approval for use of violence family index expanded

		Treatment effect estimates (N=1025)			
	Control	ITT		TOT	
Outcome	Mean	Coeff.	SE	Coeff.	SE
	(1)	(2)	(3)	(4)	(5)
Your neighbor beats the man who robbed his home	0.08	-0.025	[0.015]	-0.032	[0.018]*
Take things from home of man refusing to repay money	0.04	-0.001	[0.013]	-0.001	[0.015]
Police don't investigate the killer of a known robber	0.10	-0.012	[0.022]	-0.016	[0.027]
Chief uses trial by ordeal on a suspected thief	0.27	0.026	[0.029]	0.034	[0.035]
No one punishes shop owners who beat a market thief	0.51	0.048	[0.031]	0.063	[0.038]
No one punishes shop owners who kill a market thief	0.20	-0.030	[0.031]	-0.039	[0.036]
Chase and beat a wife who runs off with your things	0.19	-0.024	[0.025]	-0.031	[0.030]
Community destroys the property of a captured bandit	0.39	-0.001	[0.035]	-0.001	[0.042]
Your friend threatens the man trying to steal girlfriend	0.10	-0.025	[0.020]	-0.033	[0.024]
Community beats a corrupt leader	0.07	-0.004	[0.015]	-0.005	[0.019]
Husband beats a wife who challenges him in public	0.14	-0.026	[0.023]	-0.034	[0.028]
Community beats policeman bribed to release rapist	0.22	-0.032	[0.028]	-0.042	[0.033]

Notes: See Table D.2.

Table D.7: Community participation family index expanded

		Treatment effect estimates (N=1025)			
	Control	ITT		TOT	
Outcome	Mean	Coeff.	SE	Coeff.	SE
	(1)	(2)	(3)	(4)	(5)
Number of groups involved in	5.36	0.288	[0.142]**	0.376	[0.168]**
Is a group leader	0.48	0.053	[0.033]	0.069	[0.039]*
Is a community leader	0.29	-0.019	[0.028]	-0.024	[0.034]
Attended community meetings	0.94	-0.017	[0.020]	-0.022	[0.025]
Believes can do things to improve community	0.89	0.018	[0.018]	0.024	[0.021]
Volunteered for road clearing	0.78	0.007	[0.030]	0.009	[0.036]
Contributed to care of community water sources	0.67	0.021	[0.036]	0.027	[0.043]
Contributed to other public facilities	0.63	0.010	[0.035]	0.013	[0.042]
Is a "big man" in community	0.35	0.003	[0.034]	0.004	[0.041]
Organizes new groups	0.50	0.009	[0.038]	0.011	[0.045]
People often come to you for advice	0.38	0.014	[0.033]	0.018	[0.039]
Community members come to you to solve disputes	0.28	0.011	[0.033]	0.015	[0.039]
Your friends come to you to solve disputes	0.83	0.011	[0.031]	0.014	[0.038]

Notes: See Table D.2.

D.2 Robustness and sensitivity analysis

Results in the main paper are generally robust to different models and missing data assumptions. Table D.8 reports sensitivity analysis for key outcomes. Column 1 replicates TOT estimates from Tables 3 to 7 in the main paper. Column 2 reports TOT without baseline covariates. Column 3 reports TOT estimates with covariates but using inverse propensity weighting instead of block fixed effects to account for varying probabilities of assignment. Column 4 uses an alternate instrument for assignment to treatment—instead of counting the first m men in each block as assigned to treatment up until the that block's quota b is filled (so that $m \geq b$), the results in Column 4 do not count the $m > b$ men as assigned to

treatment (rather, they are non-compliant).[29]

In general the qualitative conclusions are the same. Inverse propensity weighting tends to increase the size of most effects. Omitting covariates or using the alternate instrument slightly reduces the statistical significance of some effects, as one would expect.

Columns 5 to 7 consider whether our treatment effects could be the result of selective attrition. We estimate bounds by imputing outcome values for unfound individuals at different points of the observed outcome distribution, focusing on the cases that reduce program impacts. For positive outcomes we impute the observed mean plus x standard deviations of the distribution for the control group, and for the treatment group we impute the observed treatment mean *minus* x standard deviations of the distribution. We calculate estimates for $x = 0.1$, 0.25, and 0.5. Note these imply large systematic differences between the missing treatment and control members. All treatment effects in are robust to $x = 0.1$ and the majority are still robust to $x = 0.25$. The sign on treatment is preserved when $x = 0.5$.

[29]This increases noise and estimates a different local average treatment effect than in Column (1), but avoids the small risk that the marginal person selected into treatment assignment in each is endogenously a more compliant type. The quota was usually 50% of the men registered in the block, whereas on average 57% were assigned to treatment using our ordered assignment method. Ideally we would use this 57% average as the instrument, counting people as assigned if they were in the first 57% of the block order, but we do not have the historical ranking in every block to do so.

Table D.8: Sensitivity analysis of treatment effects to alternate models and missing data scenarios

	TOT estimate under alternative models				Sensitivity of TOT to attrition		
					Impute missing dependent variable with mean + (-) X SD for missing control (treatment) individuals		
Outcome	Result from previous tables	Drop baseline covariates	Inverse propensity weighting	Assign first 50% of block to program	0.1 SD	0.25 SD	0.5 SD
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Engaged in agriculture now	0.155	0.15	0.172	0.151	0.270	0.239	0.187
	[0.036]***	[0.038]***	[0.039]***	[0.037]***	[0.080]***	[0.080]***	[0.081]***
Income index (z-score)	0.157	0.135	0.172	0.137	0.151	0.118	0.063
	[0.071]**	[0.080]*	[0.069]**	[0.082]*	[0.065]**	[0.066]*	[0.067]
Hours in illicit activities	-3.697	-3.242	-3.496	-3.814	-3.122	-2.377	-1.134
	[1.593]**	[1.554]**	[1.612]**	[1.729]**	[1.382]**	[1.385]*	[1.405]
Commander relations (z-score)	-0.154	-0.171	-0.228	-0.148	-0.125	-0.092	-0.037
	[0.100]	[0.104]	[0.107]**	[0.094]	[0.095]	[0.095]	[0.096]
Recruitment activities (0-12)	-0.239	-0.311	-0.335	-0.182	-0.215	-0.182	-0.125
	[0.118]**	[0.134]**	[0.158]**	[0.102]*	[0.078]***	[0.077]***	[0.077]***

Notes: Column (1) replicates results from Tables 3 to 7. Column (2) estimates the TOT without baseline covariates but with block fixed effects. Column (3) weights individuals by the inverse probability of assignment within each randomization block instead of controlling for block fixed effects. Column (4) changes the instrument used for attending the program: someone is assigned to treatment if their random number was in the first half of the block (rather than the last random number before the block quota was met). Block fixed effects are omitted as they are no longer need for identification (since probability of assignment does not vary by block). Columns (5) to (7) impute the mean of the control (treatment) group plus (minus) "X" standard deviations of the group's distribution (SD), for X=0.1, 0.25, 0.5. All regressions include a vector of baseline covariates, and all but .

*** p<0.01, ** p<0.05, * p<0.1

D.3 Heterogeneity analysis using expanded indices

Table D.9: Heterogeneity of program impacts by package choice, with expanded economic outcomes

	ITT estimates					
	Impact of assignment to program		Marginal effect of choosing animals package		Program impact on animal choosers (2+4)	
Outcome	Coeff. (1)	Std. Err. (2)	Coeff. (3)	Std. Err. (4)	Coeff. (5)	Std. Err. (6)
Agricultural engagement:	0.127	[0.033]***	-0.046	[0.045]	0.081	[0.041]**
Raising crops/animals†	1.148	[2.360]	2.179	[5.604]	3.327	[5.097]
Acres under cultivation	0.006	[0.016]	0.010	[0.028]	0.016	[0.029]
Thinks farming is a good living	0.075	[0.031]**	0.083	[0.041]**	0.158	[0.043]***
Interested in farming	0.047	[0.020]**	0.014	[0.022]	0.060	[0.023]***
Interested in raising animals	2.502	[2.616]	-8.065	[5.190]	-5.563	[4.757]
Hours worked/week, past month	-2.318	[1.406]	-2.706	[2.490]	-5.024	[2.484]**
Illicit resource extraction	-0.767	[0.695]	-0.842	[1.116]	-1.609	[1.07]
Logging	-1.109	[1.227]	-1.306	[1.923]	-2.415	[1.859]
Mining	-0.442	[0.567]	-0.558	[0.948]	-1.000	[1.041]
Rubber tapping	4.820	[2.599]*	-5.359	[4.337]	-0.539	[4.157]
Farming and animal-raising	4.004	[1.342]***	-4.621	[2.314]**	-0.617	[2.001]
Farming	3.503	[1.198]***	-4.675	[1.884]**	-1.172	[1.571]
Animal-raising	0.501	[0.579]	0.054	[1.321]	0.555	[1.157]
Contract agricultural labor	-0.321	[0.331]	1.086	[1.188]	0.765	[1.104]
Palm, coconut, sugar cutting	0.252	[0.363]	0.066	[0.315]	0.318	[0.376]
Hunting	0.378	[0.352]	-0.862	[0.381]**	-0.484	[0.427]
Non-farm labor and business	0.178	[2.280]	-1.840	[3.196]	-1.662	[2.919]
Other activities	0.330	[0.581]	0.812	[1.079]	1.142	[1.109]
Other illicit activities:						
Any illicit resource extraction	-0.014	[0.032]	-0.057	[0.051]	-0.071	[0.056]
Sells any soft or hard drugs	-0.007	[0.012]	-0.007	[0.015]	-0.013	[0.014]
Stealing activities (z-score)†	0.054	[0.065]	-0.043	[0.082]	0.012	[0.096]

Notes: Column (1) reports the ITT coefficient of program assignment and Column (3) reports the coefficient on an interaction between program assignment and choosing poultry/pigs. Column (5) lists the sum of the coefficients in Columns (1) and (3). The regression includes baseline covariates and regional dummies are used instead of block dummies. Robust standard errors in brackets, clustered by community.

*** p<0.01, ** p<0.05, * p<0.1

Table D.10: Heterogeneity of program impacts by package choice, with expanded economic outcomes

	ITT estimates					
	Impact of assignment to program		Marginal effect of choosing animals package		Program impact on animal choosers (2+4)	
Outcome	Coeff. (1)	Std. Err. (2)	Coeff. (3)	Std. Err. (4)	Coeff. (5)	Std. Err. (6)
Direct recruitment activities (0-12)	-0.157	[0.107]	-0.138	[0.142]	-0.295	[.13]**
Direct recruitment activities excluding "Talked to a commander" (0-11)	-0.072	[0.098]	-0.150	[0.129]	-0.222	[.105]**
Talked to a commander in last 3 months	-0.085	[0.040]**	0.013	[0.068]	-0.073	[.064]
Would go if called to fight for tribe	-0.008	[0.012]	-0.017	[0.015]	-0.025	[.015]
Has been approached about going to CI	0.007	[0.019]	-0.032	[0.027]	-0.025	[.025]
Would go to CI for $250	-0.007	[0.010]	0.013	[0.012]	0.006	[.005]
Would go to CI for $500	-0.012	[0.013]	0.024	[0.019]	0.013	[.014]
Would go to CI for $1000	-0.032	[0.017]*	0.000	[0.027]	-0.031	[.026]
Will move towards CI border area	-0.008	[0.022]	-0.048	[0.027]*	-0.056	[.025]**
Invited to secret meeting on going to CI	0.007	[0.014]	-0.023	[0.020]	-0.016	[.021]
Attended secret meeting on going to CI	-0.007	[0.009]	-0.011	[0.015]	-0.019	[.015]
Was promised money to go to CI	0.008	[0.013]	-0.038	[0.016]**	-0.030	[.014]**
Willing to fight if war breaks out in CI	-0.009	[0.014]	-0.027	[0.024]	-0.036	[.018]**
Has plans to go to CI in the next month	-0.011	[0.009]	0.008	[0.013]	-0.003	[.01]
Indirect recruitment measures (0-4)	-0.080	[0.066]	-0.217	[0.091]**	-0.296	[.097]***
Talks about the CI violence with friends	-0.014	[0.036]	-0.115	[0.054]**	-0.129	[.055]**
Has a partisan preference in CI	-0.080	[0.035]**	-0.049	[0.060]	-0.129	[.06]**
Knows people who went to CI to fight	-0.011	[0.016]	-0.027	[0.027]	-0.038	[.027]
Knows people given money to go to CI	0.025	[0.014]*	-0.025	[0.025]	-0.001	[.024]

Notes: Column (1) reports the ITT coefficient of program assignment and Column (3) reports the coefficient on an interaction between program assignment and choosing poultry/pigs. Column (5) lists the sum of the coefficients in Columns (1) and (3). The regression includes baseline covariates and regional dummies are used instead of block dummies. Robust standard errors in brackets, clustered by community.

*** p<0.01, ** p<0.05, * p<0.1