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The Returns to Microenterprise Support among the Ultrapoor: A Field Experiment in Postwar Uganda†

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We show that extremely poor, war-affected women in northern Uganda have high returns to a package of $150 cash, five days of business skills training, and ongoing supervision. Sixteen months after grants, participants doubled their microenterprise ownership and incomes, mainly from petty trading. We also show these ultrapoor have too little social capital, but that group bonds, informal insurance, and cooperative activities could be induced and had positive returns. When the control group received cash and training 20 months later, we varied supervision, which represented half of the program costs. A year later, supervision increased business survival but not consumption. (*JEL* I38, J16, J23, J24, L26, O15, Z13)

The World Bank, the United Nations, and the United States government have made the eradication of extreme poverty by 2030 a central development goal.[1] Since the world's poor often live in economies with few firms, anti-poverty programs often try to foster self-employment. This includes farm enterprises such as raising livestock for sale, and nonfarm enterprises such as trading or retail. But can the extreme poor be expected to start and sustain such microenterprises? And what constraints hold them back?

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[1] "Extreme poverty" refers to earning less than the $1.25 per day international poverty line. See Burt, Hughes, and Milante (2014) for a discussion of the goals.

Two in five of the world's extreme poor are projected to live in fragile and conflict-affected states by 2030, yet rigorous evidence on what works in these settings is sparse.[2] To help fill this gap, this paper studies a relatively common approach to relieving extreme poverty—transfers of human and physical capital—but to a postwar population: the most marginalized people living in small villages in northern Uganda, following a 20-year war.

A humanitarian organization, the Association of Volunteers in International Service (AVSI), identified 1,800 poor people, mostly women, in 120 war-affected villages, and tried to help them start very small but sustainable retail and trading enterprises. AVSI's Women's INncome Generating Support (WINGS) program provided people grants of $150 (about $375 in purchasing power parity, or PPP, terms), along with five days of business skills training and planning, plus ongoing supervision to help implement the plan. The grant was 30 times larger than the beneficiaries' baseline monthly earnings.

An abundance of evidence argues that the average poor person has high returns to capital and is held back in part by poor access to credit and insurance, and that capital transfers and insurance products help grow microenterprises and incomes.[3] Most of this evidence, however, comes from people who already have businesses or were selected for their business aptitude.[4] It's not clear if it applies to the most marginalized and "ultrapoor"—the people with the lowest incomes, no capital, and limited social networks—especially after war.[5]

The WINGS program has parallels to "graduation" style programs delivered to hundreds of thousands of ultrapoor households globally. Graduation programs give a bundle of temporary income support, livestock, livestock training, access to microfinance, supervision, and life-skills education. On balance, these programs have been successful: several studies show substantial shifts from casual labor to farm self-employment, and 10 to 40 percent increases in household consumption or earnings compared to control groups, lasting at least two to four years (Bandiera et al. 2013; Banerjee et al. 2015). The WINGS program differed from these other ultrapoor programs in several dimensions, however, including: the postwar setting; fewer program components; the focus on petty business; and providing cash rather than livestock.[6] WINGS was also focused on young women.

[2] See Burt, Hughes, and Milante (2014) for population projections. For reviews of the evidence see Blattman and Miguel (2010) and Puri et al. (2014).

[3] For example, see Udry and Anagol (2006); de Mel, McKenzie, and Woodruff (2008); Banerjee and Duflo (2011); Karlan, Knight, and Udry (2015); Fafchamps et al. (2014); Blattman, Fiala, and Martinez (2014).

[4] For example, Blattman, Fiala, and Martinez (2014) see high returns to a group-based cash transfer in northern Uganda. But the program targeted young adults with much higher levels of education and existing business plans for relatively high-skill microenterprises. That program also excluded the two most conflict-affected districts, where WINGS was implemented.

[5] On the one hand, returns to capital or other inputs could be greater on the extensive margin than the intensive one. Indeed there is growing evidence that poor households use cash to start new enterprises and earn high returns, although little of this evidence comes from the poorest of the poor (Macours, Premand, and Vakis 2012; Gertler, Martinez, and Rubio-Codina 2012; Blattman, Fiala, and Martinez 2014; Bianchi and Bobba 2013). Returns could also be high in a newly stable political equilibrium, as neoclassical models of growth predict (Blattman and Miguel 2010). On the other hand, the ultrapoor could have low returns to capital, for instance because they lack crucial inputs such as education or business experience, or because they are vulnerable to expropriation within or outside the family.

[6] Many in the aid community fear cash can be seized, wasted, or cause harm. They could be right. Besides the lack of other important inputs, extreme poverty has also been associated with cognitive deficits that impede

We evaluated WINGS by assigning the targeted people to either receive the program immediately or a year-and-a-half later, randomizing at the village level. Given the extreme setting, AVSI was reluctant to have a permanent control group—a common concern in humanitarian settings, and one reason humanitarian evaluations are rare. Thus, our design evaluates impacts a few months before the 60 control villages entered the program.

We also tested the role of social capital in business success: could social capital be fostered, and would it increase the returns to grants? In poor rural villages, social networks are a main source of business advice, cooperation, and informal finance.[7] For instance, in microcredit, growing evidence suggests that group lending is helpful not because of joint liability, but rather because it builds social capital and promotes risk-pooling.[8]

To test this, in half of the treatment villages, AVSI returned a couple of months after the grants (after individual businesses had already been started) to encourage the participants to form self-help groups, and offered three additional days of training in working together. The curriculum focused on developing organizational structures, decision-making processes, leadership, and helping them form a rotating savings and credit association (ROSCA).

Sixteen months after grants, the standard WINGS program (without group encouragement) led to large changes in occupation and incomes. Thirty-nine percent of the control group had a nonfarm business, and this rose to 80 percent among WINGS participants. Employment rose from 15 to 24 hours per week, and cash earnings rose about PPP $1 a day. Since the average person in the control group earned less than $1 a day, the program doubled earnings. As a result, a conservative estimate of household consumption rose by almost a third, to roughly PPP $1.25 per day. Annualized, this impact corresponds to a PPP $465 increase in nondurable consumption—about a quarter of the PPP $1,946 standard program cost.

For program participants, the gains were mainly economic. There was little evidence of changes in physical health, mental health, financial autonomy, or domestic violence. Outside the household, however, the program increased self-reported social support and community participation. Participants also reported an increase in resentment and verbal abuse from some neighbors, however, perhaps due to jealousy, or because they posed competition for preexisting traders.

The group encouragement, meanwhile, increased the frequency and intensity of group activities. We see no impact on consumption after 16 months, but program participants who received group encouragement reported double the earnings of those that did not. Interestingly, this was not because their petty trading businesses were larger, more likely to survive, or more profitable. Rather, the evidence suggests that groups spurred informal finance as well as labor-sharing and cooperative cash

investment and raise the risk of temptation spending (e.g., Bertrand, Mullainathan, and Shafir 2004). Among the poorest women, moreover, traditional norms could pressure them to share cash, make it easy to expropriate them, or hinder their business growth (Field, Jayachandran, and Pande 2010; Duflo 2012). This is the fundamental reason that AVSI designed WINGS to include training and supervision.

[7] See Fafchamps (1992), Foster and Rosenzweig (1995), Murgai et al. (2002).

[8] Feigenberg, Field, and Pande (2013) show that encouraging social interaction via group meetings reduces default on individual loans in India. Giné and Karlan (2014) also show individual liability has little impact on default in the Philippines.

cropping. Group formation also seems to have mitigated the resentment and abuse from neighbors.

Ideally, we could have randomly varied all program components and measured their returns. This was not possible. But, following the main evaluation, we used the entry of the control group into the program to investigate the marginal effect of the most expensive component: supervision. The supervisory visits provided substantive advice as well as pressure to implement the business plan, but were more than twice as costly as the grant. When the control group received WINGS after 20 months, we randomized them individually to receive the business training and grant plus: no supervisory visits; two visits (to provide commitment to invest); or five visits for both commitment and substantive business advice.

A month after grants, but before any potential visits, expecting a visit had an ambiguous effect on business investment: those assigned to supervision increased investment by some measures and decreased by others. A year later, the effect of supervision on incomes is also ambiguous: nondurable consumption is marginally lower among those assigned to visits, earnings are marginally higher, but neither effect is statistically significant. Supervision, however, did increase business survival.

Altogether, these results come with caveats. First, the control group knew they were on the waitlist, and so anticipation of treatment could affect their behavior. Second, all measures are self-reported. Experimenter demand is a risk, but given the size of impacts (and the absence of noneconomic impacts, where we might expect experimenter demand) the bias seems unlikely to drive our results. Third, there is mild randomization imbalance and attrition. Treatment effects, however, are robust to corrections and to missing data scenarios. Finally, these are 16-month impacts and given the fact that the control group entered the program, we cannot say whether they persist.

Nonetheless, WINGS illustrates that the poorest may be able to start and sustain small enterprises, even in very small, fairly poor communities. Moreover, the 16-month consumption impacts of WINGS are almost identical to the one-year or two-year impacts of livestock-based ultrapoor programs, although WINGS was about half as costly. So far the livestock programs have longer term evidence in their favor, and the sustainability of cash-centric programs to the very poorest is an open question.[9] Even so, studies of cash transfers to the non-extreme poor show sustained or growing impacts after four to six years (Blattman, Fiala, and Martinez 2014; de Mel, McKenzie, and Woodruff 2012b).

Cash should be much cheaper and easier to deliver than livestock or capital goods, so if it stimulates employment as well as the accumulation of income-generating assets it could affect how ultrapoor programs are designed and scaled. This warrants more investigation, especially in humanitarian settings where cash is becoming more common as it can be difficult to provide in-kind capital. Also important to investigate are cost-effective forms of supervision and training.

[9] Livestock programs sustain gains after two to four years, while ultrapoor cash transfer studies have 1 to 2 years of evidence so far (e.g., Haushofer and Shapiro 2013; Macours, Premand, and Vakis 2012).

Finally, the results support the view that social interactions encourage cooperation, and that such social capital delivers economic returns. Most social capital is endogenously formed, and it's unusual to have experimental variation in local bonds. Echoing Feigenberg, Field, and Pande (2013) on microcredit, we see that a program that simply encourages group and ROSCA formation can increase social interactions, enhance social capital, increase risk-pooling and cooperation, and perhaps even raise incomes. What's striking is that these profitable social bonds did not form in the absence of encouragement, and yet were provoked by a relatively short training. It implies the poor may be social capital constrained as well as credit constrained, and external intervention seems to help overcome barriers to collective action.

I. Setting and Study Participants

Uganda as a whole is a poor but stable and growing country. National income grew roughly 6.5 percent per year for the two decades prior to this study (Government of Uganda 2007). A long-running, low-level insurgency in northern Uganda, however, meant that most of the north was left out.

From 1987 to 2006, small bands of rebels conscripted, abused, and stole from civilians in northern Uganda, especially the Kitgum and Gulu districts. Equally devastating was the Ugandan government's decision to fight the insurgency by forcibly moving nearly the entire rural population of Kitgum and Gulu—about two million people—into dozens of displacement camps. The camps were often no more than a few miles from people's rural homes, but people generally could not access their farmland during the war. Most households lost everything—livestock, homes, savings, and household durables—as a result.

By 2006 the rebels were mostly defeated or pushed out of the country, and by 2007 the government permitted displaced people to return home and rebuild. The north's economy began growing quickly, aided by an increase in demand from a newly peaceful Sudan. The government started a number of large-scale development programs to help the north catch up to the rest of the country. Even so, northern Uganda had some of the lowest standards of living in the world. By 2007, two-thirds of households were unable to meet basic needs and lived mainly on food aid (Government of Uganda 2007).

By 2009, when this study began, most people had rebuilt their homes and had begun farming again. Food distribution and other emergency relief had ended. Most rural villagers, however, were still desperately impoverished.

A. *Study Sites and Participants*

AVSI identified 120 villages in the two most war-affected districts, Kitgum and Gulu. Most villages ranged in size from 350 to 1,000 people, with an average population of 699 (about 100 households). The study villages represented about a quarter of the population of the six rural subcounties where AVSI worked.[10]

[10] AVSI actively worked in six subcounties—Odek, Lakwana, and Lalogi in Gulu and Omiya Anyima, Namokora, and Orom in Kitgum. These have 252 total villages: 84 in Gulu; 168 in Kitgum. AVSI excluded from

AVSI held community meetings to describe the program and asked communities to nominate 20 marginalized villagers, asking that three-quarters be women aged 14 to 30. AVSI staff interviewed all nominees and selected 10 to 17 participants per village, excluding relatives of leaders and the least poor.

Table 1 describes the 120 villages and all 1,800 study participants, based on a baseline survey of participants and each community leader. Twenty-six percent of villages had a weekly market, and while on average there were three shops or kiosks selling goods, the median village had none. Most goods were imported from the district capital and retailed by a handful of shop owners.

Outside of traditional occupations (e.g., subsistence agriculture and some casual labor), main work opportunities came from petty trade and retail, cottage production (e.g., bricks, charcoal), livestock rearing, and cash crops. These farm and nonfarm microenterprises required few new skills or education, but they were capital-intesive and had fixed costs of entry.

The average participant in the program was female, 27 years old, and had 2.8 years of education. Half were married or partnered. They reported an average of 15 hours of work a week in the past month, mainly in their own agriculture. Just 3 percent did any petty trade or business.

In general they were poor with no access to finance. Average cash earnings in the month before the survey were 8,940 Ugandan shillings (UGX) ($4.47 at 2009 market exchange rates). Formal insurance was unknown, and almost no formal lenders were present in the north at the outset of this study in 2008. Only 9 percent of the sample were members of a village savings and loans group. On average they had UGX 4,860 ($2.42) in cash savings and a nearly equal amount in debts, usually from family and friends. Just 4 percent said they could get a loan of $50, which is unsurprising because of high transaction costs and the near absence of informal or formal lending institutions. Formal loan terms seldom extended beyond three months, moreover, with annual interest rates of 100 to 200 percent. Because of high fees, real interest rates on savings were typically negative. Given the startup costs of microenterprise, this absence of credit and insurance was a major barrier to entry.

Effects of the War.—The war affected and displaced everyone in the study sample, impoverishing all. Until about a year before the program, everyone in the village had lived in a nearby displacement camp for at least three years, with no access to farmland, during which their lands became overgrown and their houses destroyed. At the time of the WINGS program, households were reestablishing agriculture for the first time since at least 2003.

One in five people in our sample were abducted into the armed group at some point, usually only for a few days to carry looted goods. Long stays with the armed group were less common, and only 5 percent of the sample became fighters or were forced to marry a rebel commander. Abduction and conscription, however, were not

the sample villages with fewer than 80 households. AVSI then chose program villages proportional to parish population, whereby more populous parishes would have a higher number of program villages (A parish is an administrative unit within the subcountry with five to ten villages). Official population figures did not exist and estimates were based on 2008 data from AVSI and the United Nations. We estimate participant households in treatment villages were less than 2 percent of households in the subcounty.

TABLE 1—DESCRIPTIVE STATISTICS AND RANDOMIZATION BALANCE FOR SELECT COVARIATES

Variable	Means, full sample		Balance test	
	Treatment (Observations = 896)	Control (Observations = 904)	Difference	*p*-value
	(1)	(2)	(3)	(4)
Demographics				
Age	27.02	27.63	−0.62	0.17
Female	0.86	0.86	−0.01	0.72
Married or living with partner	0.46	0.50	−0.05	0.26
Single-headed household	0.51	0.47	0.04	0.17
Highest grade reached at school	2.82	2.75	0.07	0.70
Forcibly recruited into rebel group	0.20	0.25	−0.05	0.03
Carried gun within rebel group	0.03	0.04	−0.01	0.45
Forcibly married within rebel group	0.03	0.03	−0.00	0.63
Lagged outcomes				
Any nonfarm self-employment	0.03	0.04	−0.01	0.17
Average work hours per week	14.57	16.19	−1.62	0.12
Agricultural	11.27	13.36	−2.09	0.02
Nonagricultural	3.29	2.83	0.46	0.25
Average hours of chores per week	34.88	34.25	0.63	0.68
No employment hours in past month	0.23	0.18	0.05	0.07
Monthly cash earnings, 000s UGX	8.54	9.32	−0.78	0.26
Durable assets (consumption), *z*-score	−0.67	−0.59	−0.07	0.05
Durable assets (production), *z*-score	−0.53	−0.50	−0.02	0.48
Number of community groups	0.48	0.58	−0.10	0.04
Member of a savings group	0.08	0.11	−0.03	0.07
Total savings, 000s UGX	4.24	5.47	−1.23	0.20
Total debts, 000s UGX	4.24	4.08	0.15	0.82
Activities of daily life, *z*-score	−0.06	−0.04	−0.02	0.75
Symptoms of distress, *z*-score	0.09	−0.09	0.18	0.02
Quality of family relationships, *z*-score	−0.09	0.09	−0.19	0.00
Any community maltreatment, past year	0.19	0.16	0.03	0.11
Village-level covariates (*Observations = 120*)				
Village population	749.62	649.05	100.58	0.34
Inverse distance to all villages	0.51	0.58	−0.07	0.34
Inverse distance to treatment villages	0.56	0.47	0.09	0.43
Distance to capital (km)	46.21	44.72	1.48	0.58
Accessible by bus	0.98	0.91	0.08	0.05
Village has a market	0.18	0.34	−0.16	0.05
Number of shops in village	1.23	1.75	−0.52	0.30
Total NGOs in village	7.13	7.42	−0.29	0.68
p-value from joint significance of 76 covariates				< 0.01

Notes: All variables denominated in UGX and hours were top-censored at the nintey-ninth percentile to contain outliers. The durable asset indexes (*z*-scores) are calculated so that they have mean zero and unit standard deviation for the full sample over all survey waves, and hence the sign is negative at baseline. The differences in columns 3 and 4 come from OLS regressions of baseline covariates on an indicator for treatment plus a district fixed effect, with robust standard errors clustered by village.

necessarily a source of relative poverty. Annan et al. (2011) used exogenous variation in conscription to identify the long-term effects. Social acceptance of former conscripts was high, and most people were psychologically resilient.[11] These findings held even for the longest-serving females and those who were forcibly married

[11] Psychological distress is commonplace, but serious symptoms are concentrated in the minority exposed to the most violence and with the least social support.

or bore children. Conscription also had little effect on women's schooling and labor market outcomes. Women's options outside the armed group were not much better than inside the armed group, since most would not have been schooled or accumulated capital. Conscripted men, however, were well behind their peers after the war, because they missed out on opportunities to accumulate physical and human capital.

In short, the war was so destructive that few young people emerged with any assets or schooling. At the time of the WINGS program, they were rebuilding their livelihoods from almost nothing.

B. *Comparison to Nonparticipating Villagers*

In general, the program successfully targeted the villages' poorest, but it's worth noting that almost all villagers were very poor by any measure. We do not have data on nonparticipants at baseline. Twenty months after the start of the program, however, we surveyed 2,836 nonparticipant households in treatment and control villages (about 25 from each village), and sought to interview two working age adults per household, in order to measure spillovers.[12] Table 2 reports summary statistics for participants and nonparticipants in the control villages only, in order to compare people in the absence of direct treatment effects. We distinguish between households that were and were not traders at baseline.

If we look at similar-aged adults in "nonparticipant" households, participants have similar cash earnings, but 24 percent lower consumption, 0.63 standard deviations fewer durable consumption assets (e.g., house quality, furniture, and household items), and 0.22 standard deviations fewer production assets (e.g., livestock or farm tools). Participants also have about half the education and 63 percent of nonparticipants' work hours. About a third of nonparticipant households have at least one adult engaged in trading at baseline, and these tend to be wealthier than average.

II. Intervention, Experimental Design, and Data

The WINGS program had four components:

Basic Skills Training.— Participants received five days of business skills training, ending with the preparation of a simple business plan. Training was designed for the illiterate and focused on business planning, sales, marketing, record-keeping, and budgeting (see online Appendix A for the curriculum). Trainers reviewed plans with the participants and returned unsatisfactory plans for revision. They encouraged participants to consider high cash flow activities that would diversify their income sources, especially petty trading and retailing.

[12] Shortly before Phase 2 disbursement, we created village household lists, randomly sampled 25 nonparticipant households from each village (excluding the roughly 15 participant households), and sought to interview two working age adults per household on their economic activities, plus household data on assets and expenditures. We also collected village prices on a variety of goods. Nonresponse to the survey was only 5.5 percent.

TABLE 2—PARTICIPANTS VERSUS NONPARTICIPANTS (*control villages at Phase 1 endline*)

Covariate	Participants	Nonparticipants ages 17–40, control villages		
		Traders	Non-traders	All
	(1)	(2)	(3)	(4)
Age	28.10	29.35	28.55	28.71
Years of education	2.81	5.58	4.48	4.70
Average weekly work hours	15.02	31.08	21.93	23.78
Agricultural weekly hours	9.68	21.11	16.80	17.67
Nonagricultural weekly hours	5.47	9.98	5.13	6.11
Reports any hours in petty business	0.16	0.26	0.07	0.11
Monthly cash earnings, 000s of UGX	15.76	23.45	10.14	12.82
Monthly household consumption, 000s of UGX	108.38	175.05	134.04	142.30
Durable assets (consumption), *z*-score	−0.45	0.64	0.06	0.18
Metal roof	0.00	0.03	0.00	0.01
Number of goats	0.97	1.62	1.22	1.30
Number of bicycles	0.39	0.77	0.60	0.63
Number of mobile phones	0.14	0.58	0.35	0.39
Durable assets (production), *z*-score	−0.21	0.30	−0.07	0.01
Observations	917	360	1,427	1,787

Notes: For work hours and income, we report household averages in nonparticipant households, restricting data to adults aged 17–40. A household is coded as a trading household if at least one household respondent says he or she regularly sold an item a year ago, and did not obtain that item from his or her own production, for any items in a list of 35. Individual-level covariates come from a self-reported survey of all respondents. All variables denominated in UGX and hours were top-censored at the ninety-nineth percentile to contain outliers.

Cash.—Once a plan was approved, the participant received a grant of 300,000 UGX or $150 at 2009 market exchange rates. The grant was framed as funds to implement the business plan. AVSI delivered cash in two equal installments about two and six weeks after training.

Supervision.—AVSI trainers traveled four to five times to the villages in the six months after the grant to provide one-on-one advising and supervision.

Group Formation.—About two months after grants were disbursed, AVSI also offered a three-day group dynamics training that encouraged participants in the village to form self-help groups that would exchange ideas for improving their petty business and agriculture, organize savings and credit, and (to a lesser extent) collaborate or cooperate in activities such as marketing their produce or buying inputs.

The vast majority of the three days focused on providing information and skills related to working effectively as a group, including: leader selection, group decision making, communication and listening skills, and conflict resolution methods. It applied these skills to the same topics that were the focus of the five-day business skills training: business planning, saving, and record-keeping. The difference is that the group dynamics training mostly focused on how they could adapt these skills when working as a group. The final day focused on how to organize a savings group, including best practices and record-keeping. Other forms of business cooperation, such as joint purchasing and collaborative marketing, were mentioned in passing as advantages of working in groups, but these production economies of scale received very little attention. Indeed, AVSI deliberately did not encourage

participants to form firms or cooperatives. This is one reason AVSI offered the group training some weeks after the individual business plans, grants, and initial investment decisions.

Groups decided on their own aims and organization, however, and at the end of the training AVSI helped groups write a constitution that formalized the aims, leadership, and decision-making structure of the group. Online Appendix A describes the curriculum.

On average, WINGS cost $860 per person at market exchange rates: $150 for grants; $125 for targeting and disbursement; $124 for business training; $82 for group dynamics training; $348 for five supervisory visits; and $31 in other costs. This is equivalent to PPP $2,150.

A. *Phase 1 Research Design*

In Phase 1, we randomized 60 of the 120 villages to receive the WINGS program. The other 60 were randomized to a waitlist group (Phase 2) to be treated at least 18 months later. The participants in the waitlist villages were aware of this treatment status.

Within these 60 treatment villages in Phase 1, we randomized 30 to receive the group dynamics training and 30 to no group encouragement. Participants in the 60 control villages were told they would receive the intervention in about 18 to 24 months, called Phase 2. Figure 1 illustrates the sample, design, and timing.

We randomized by public draw, to ensure village buy-in and transparency.[13] The draw resulted in a slight imbalance in baseline covariates, reported in Table 1.[14] Treatment participants were slightly worse off, with lower durable assets, employment, literacy, savings group memberships, participation in armed groups, and family and community support. The villages they lived in were also less likely to have a market. A test of joint significance of all covariates has a p-value of < 0.001. If anything, this may lead us to understate treatment effects. To account for possible bias, we will control for these covariates in all treatment effects estimates and show robustness to difference-in-differences measures.

To evaluate Phase 1, we attempted to survey all 1,800 participants 20 months after baseline, 16 months after the first grant (at the median). Attrition was minimal; we tracked migrants to their current location and found 96.3 percent (not including three who died). Attrition is generally not significantly correlated with treatment or baseline covariates (see online Appendix B).[15]

[13] We gathered village leaders in each district. They drew village names without replacement to be assigned to Phase 1 or 2. The authors were present for the draw to ensure its validity. We randomized group dynamics training via computer.

[14] See online Appendix B for all 76 covariates, as well as balance tests for the group dynamics randomization. In total, 24 percent of the (nonindependent) covariates have $p < 0.10$. In general, the group dynamics randomization was balanced.

[15] In addition to these survey data, we collected formal qualitative data to better understand program experiences, constraints, and mechanisms. Two Ugandan research assistants interviewed 32 randomly selected participants in eight villages three times during and after the program. They followed semi-scripted questionnaires and recorded, transcribed, and translated all interviews and notes.

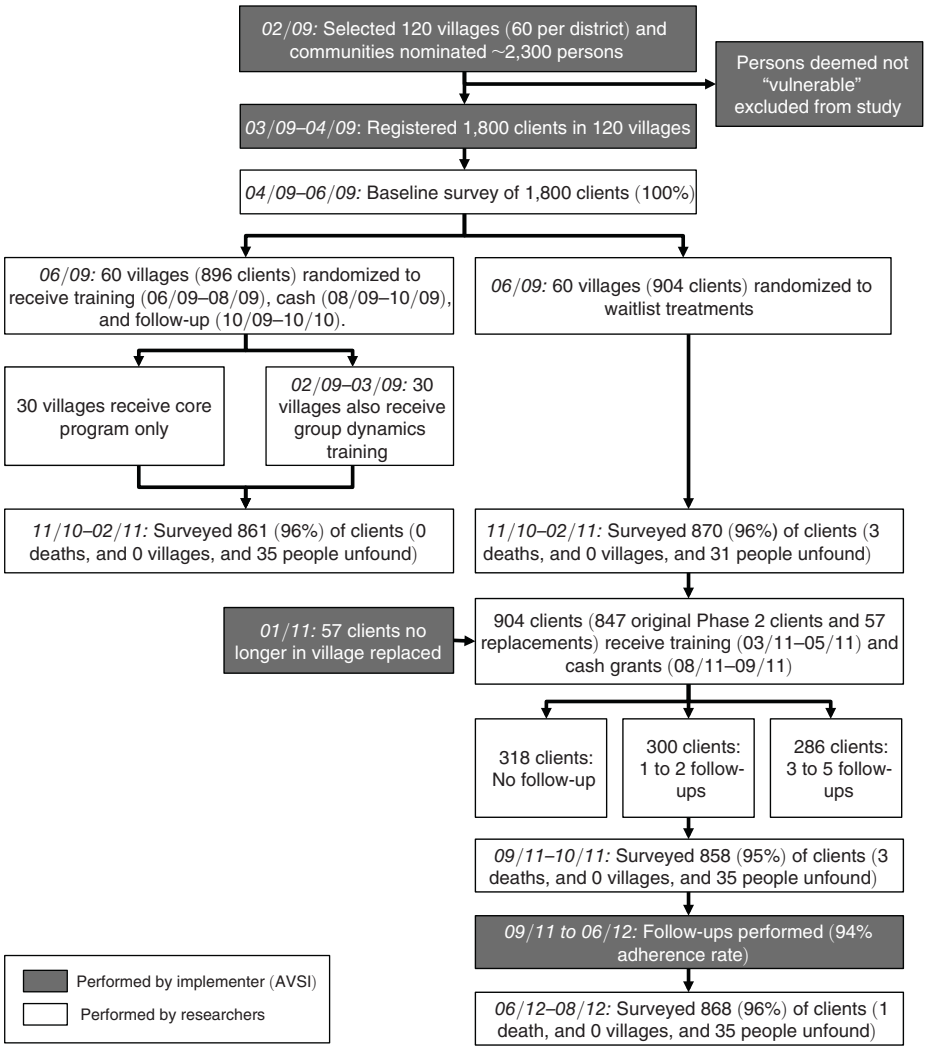

FIGURE 1. DESCRIPTION OF THE STUDY SAMPLE AND EXPERIMENTAL DESIGN

B. *Phase 2 Research Design*

In Phase 2, participants in the 60 control villages received the WINGS program.[16] We used this as an opportunity to evaluate the marginal impact of the highest-cost component, supervision.[17] The first supervisory visit or two was mainly to hold grantees accountable for implementing their business plan. The later visits were primarily to provide advice. Of the 904 people in Phase 2, we randomly assigned

[16] Program changes were minor. AVSI increased grants to 360,000 UGX to account for inflation, and disbursed grants in a single tranche for efficiency.

[17] Of the 60 villages, 30 were also randomized to have spouses included in the training, and present at the grant disbursement, described in a companion paper (Green et al. 2015).

individuals to receive the cash and basic training with one of three treatments: no supervisory visits; one to two supervisory visits, focused principally on commitment to invest; or all five supervisory visits, to provide commitment but also substantive advice on business management and household bargaining.[18]

To evaluate impacts, we first surveyed Phase 2 participants about a month after the grant, shortly before the first follow-up. We intended this short-run survey to assess how participants' actions and investments varied with expectations of any supervision. We surveyed them again about a year after the grants to study the impacts of actual supervision. Again, attrition was low; we found 95 percent at the one-month survey and 96 percent at the one-year survey.

III. Empirical Strategy

We estimate intent-to-treat (ITT) effects via the ordinary least squares (OLS) regression:

$$Y_{ij} = \theta T_j + \delta_T D_j^T + \delta_{\bar{T}} D_j^A + \mathbf{X}_{ij}\beta + \varepsilon_{ij}, \tag{1}$$

where Y is an outcome for participant i in village j, T is an indicator for treatment (e.g., assignment to Phase 1 versus Phase 2), and $\mathbf{X}$ is a vector of controls that includes a district fixed effect and all baseline covariates, including lagged outcomes.[19] Robust standard errors are clustered by village.

The terms D_j^T and D_j^A are weighted measures of distance from the village to other treatment villages and all other evaluation villages. We include them to account for and estimate potential spillovers from participants in treatment villages to those in wait-list villages, which could otherwise bias treatment effects. The average wait-list village has at least five treatment villages within ten kilometers (km), and many villages share markets. We can identify and control for cross-village externalities using exogenous variation in the local density of treatment villages generated by the randomization, conditional on the density of all evaluation villages in the sample (Miguel and Kremer 2004).[20]

There are two other threats to identification. One is anticipation of treatment. If participants delayed investments by one to two years in expectation of a grant, this would lead us to overstate treatment effects. Alternatively, the expectation of a future grant could act as a form of insurance and increase investment, leading us to understate impacts. For instance, Bianchi and Bobba (2013) show that expectation of conditional cash transfers in Mexico increase microenterprise investment by

[18] We randomized via computer at the individual level, blocking by village. The groups are relatively balanced, with balance tests reported in online Appendix B. Before Phase 2 began, 57 of the 904 participants assigned to Phase 2 had died or left the village and were no longer eligible. In February 2011, AVSI replaced these participants with others from the same village following the same nomination and screening procedures described above.

[19] See online Appendix B for all baseline covariates. Also, note that baseline and endline variables denominated in UGX or hours can have a long upper tail. Extreme values will be influential in any treatment effect, and we therefore top-code them at the nintey-ninth percentile.

[20] That is, D_j^T is a random variable conditional on D_j^A. Previous papers estimate distance measures as the number of villages within a fixed radius. We use a slightly less dichotomous measure, described in Appendix D, using road network distances. In practice these spillovers appear to be negligible and so they do not materially change our conclusions (online Appendix D).

relaxing an insurance rather than a liquidity constraint. Our design does not allow us to say what these anticipation effects might be.

A second threat is that, since outcomes are self-reported, we will overestimate the impact if experimenter demand leads treated subjects to overreport well-being, or if the controls underreport outcomes in the (mistaken) belief that they could be dropped from eligibility. We feel these biases are unlikely to drive our results for a few reasons. Most of all, as we will see below, we observe no impact on variables such as empowerment or health, meaning that incentives to misreport would have to be confined to economic outcomes alone. Related to this, misreporting would have to be highly systematic within economic outcomes: income and employment was collected through more than 100 questions across 25 activities, and assets and expenditures were calculated from 150 questions.

IV. Results

A. *Impacts of the Full Intervention*

Ninety-five percent of the treated made initial business plans for the buying and selling of goods, and the remainder made plans for butchery, livestock, or other business.[21] As we will see, however, over time their investments diversified into more nonfarm businesses and also livestock.

While the capital was seen as crucial to starting their enterprises, most participants also reported that the training, business planning, and supervision were valuable. Sixteen months after the grants, 98 percent said that AVSI staff were important in planning their business, 77 percent of people said the supervisory visits made them somewhat anxious, and 94 percent would recommend the supervisory visits. Yet this advising was not too constricting: 95 percent said they felt free to spend the grant as they saw fit.

Table 3 reports impacts 16 months after grants.[22] We start by focusing on the program without group dynamics training.

At baseline, only 3 percent of the sample reported any nonfarm business, a category that includes mainly petty trading and cottage production, but excludes agricultural enterprise and livestock-related work (Table 1). These nonfarm businesses grew substantially in the 20 months after the baseline survey. In control villages, half of the participants eligible for the program say they attempted to start some nonfarm business since baseline, and 39 percent have some form of nonfarm business at the endline survey—an increase of 36 percentage points. This investment could be an anticipation effect of the future grant (since they will enter Phase 2 a few months after the endline, reducing the risk of investments) in which case treatment effects understate the effect of the WINGS program. Alternatively, the investment and enterprise start-up could reflect the rise in business we would expect as people

[21] Ninety-six percent of people assigned to treatment received the training and grant in Phase 1. The main reasons for not receiving a grant were death or migration, in which case someone outside the study sample received the program instead.

[22] We focus on occupational choice, employment levels, and incomes. This focus is in line with the model in online Appendix C.

TABLE 3—ECONOMIC IMPACTS OF WINGS PROGRAM AND GROUP FORMATION

Outcome	Control mean (1)	ITT estimates, 16 months after grants (Observations = 1,734) No group training (2)	Group training (3)	Difference (4)
Occupational choice				
Any nonfarm self-employment	0.39	0.401	0.409	0.008
		[0.030]***	[0.033]***	[0.034]
Started enterprise since baseline	0.50	0.487	0.485	−0.002
		[0.025]***	[0.025]***	[0.018]
Average work hours per week	14.98	9.391	9.877	0.486
		[1.608]***	[1.794]***	[2.029]
Agricultural	9.52	3.496	4.002	0.506
		[1.389]**	[1.300]***	[1.524]
Nonagricultural	5.342	5.895	5.875	−0.020
		[0.893]***	[0.916]***	[1.217]
Average hours of chores per week	39.75	0.305	1.416	1.111
		[1.013]	[1.203]	[1.301]
Income and food security				
Index of income measures, *z*-score	−0.22	0.464	0.616	0.151
		[0.068]***	[0.080]***	[0.087]*
Monthly cash earnings, 000s UGX	15.53	10.372	23.390	13.018
		[3.443]***	[4.607]***	[5.512]**
Durable assets (consumption), *z*-score	0.12	0.327	0.384	0.056
		[0.067]***	[0.068]***	[0.073]
Monthly nondurable consumption, 000s UGX	107.74	31.031	33.439	2.408
		[5.010]***	[5.227]***	[6.049]
Durable assets (production), *z*-score	−0.02	0.402	0.397	−0.005
		[0.064]***	[0.058]***	[0.064]
Times went hungry, past week	0.19	−0.098	−0.084	0.013
		[0.039]**	[0.036]**	[0.043]
Usual number of meals per day	1.76	0.057	0.078	0.021
		[0.028]**	[0.031]**	[0.031]

Notes: All variables denominated in UGX and hours were top-censored at the nintey-ninth percentile to contain outliers. Columns 2 and 3 report the coefficients and standard errors on indicator for assignment to Phase 1 without and with the group dynamics component in an OLS regression of each outcome on treatment indicators, a Gulu district (strata) fixed effect, and baseline covariates. Column 4 reports the difference between the two treatment groups. Standard errors are robust and clustered at the village level.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

recover from an adverse shock. In principle, the control group could delay some investment in anticipation of the grant, though this would mean delaying for nearly two years. If so, we overestimate the effect of the WINGS program.

Nonetheless, there are even greater changes in treatment villages. Nearly every participant tried to start a nonfarm enterprise, and 79 percent have one at endline—a doubling compared to the control group. We see a large shift in occupation choice towards nonfarm enterprise, mainly wholesale and retail trade, kiosks, and shops, but also including some services.

As a result, nonfarm hours of work in the treatment group double compared to controls, rising from about 5 to 11 per week. But there is no crowding out of other activities, as the participants were generally underemployed beforehand.

Farm hours actually rise compared to controls, from about 9.5 hours per week in the control group to about 13 with treatment. Most of this increase comes from increased hours caring for livestock, as ownership of cattle, sheep, goats, and pigs more than doubles.

We have three measures of income. The first is cash earnings in the past month, in 2009 UGX. Cash earnings are noisy, subject to seasonality, and will understate income by omitting home production. Thus, we also emphasize two measures of permanent income. One is an index of 33 durable assets for consumption purposes (including housing quality, furniture, and household items), standardized to have mean zero across all survey rounds, and hence measures asset changes over time. This is commonly used as a proxy for consumption (Filmer and Scott 2012). Unfortunately, we do not have asset values.

The other measure is estimated total household consumption of 57 goods in the past month, including food, small household items, communications, transport, and so forth.[23] This is a partial list of goods, and may understate true consumption, especially because we do not have durable asset prices and cannot impute consumption of durables. Of course it is possible that the consumption measure overstates the value of home-produced items because of quality differences. We also consider a family index of all three measures.

The baseline was run at a time of intensive planting and weeding, shortly before the lean season began. The Phase 1 endline was run at a time of planting and harvesting at the outset of a dry season, during which dry season crops are produced and nonfarm activities such as brick-making are common. Major activities include the sale of crops and animals for festivals and payment of school tuition fees. We see incomes rise from baseline to endline partly because of this seasonality, but also because the 20 months from baseline to endline were a time of increasing incomes and productivity.

Durable assets in control villages rose 0.75 standard deviations since baseline. Their cash earnings rose by two thirds, from UGX 9,320 per month (about $4.70 at 2009 market exchange rates) to UGX 15,530 ($7.72), while hours of work stayed steady. We do not have nondurable consumption data at baseline.

The WINGS program leads to large increases in all three income measures, by 0.46 standard deviations overall. This includes a 66 percent increase in monthly cash earnings relative to the control group, though this is just UGX 10,372 ($5.19) in absolute terms. Durable consumption assets rise by 0.33 standard deviations. Nondurable consumption rises 29 percent relative to the control group, UGX 31,031 per month, or about $15.50 at market exchange rates.[24] Note this is a total household level measure of consumption, and there are 6.9 members in the average household.

This consumption treatment effect is three times the earnings treatment effect, which could reflect a number of factors besides measurement error: low seasonal earnings; earnings that are reinvested in other household members' enterprises or

[23] This abbreviated approach follows Beegle et al. (2012). Some items (such as food) are asked with a three-day recall period, some items (such as communications and transport) are asked with a one or two week period, and larger nondurable spending (such as festivals, etc.) are asked with a recall of several months. All items are then adjusted to monthly totals and added.

[24] We cannot reject equality of treatment effects by gender (not shown).

home-produced forms of consumption (e.g., products from livestock); or the consumption of some of the saved grant.

Because no income measure is perfect, this introduces uncertainty into the true effect on poverty. But if we use $5.16 per month as a lower bound and $15.50 as an upper one (about $13 and $39 in PPP terms) this is indisputably a large impact on income in absolute and relative terms.

We also have an index of 19 production-related durables, mainly livestock, farm equipment, and vocational tools (e.g., sewing machines). These are not included in the consumption durables. These investments rise 0.4 standard deviations, primarily from livestock purchases—0.27 more cattle, two more fowl, and two more other animals such as goats, sheep, or pigs (online Appendix D). Hence, profits are being reinvested in productive assets.

Food security improves slightly as a result of this increase in incomes and enterprise. Times going hungry in the past week fall from 0.2 to 0.1, and the number of meals per day rises from 1.76 to 1.82. Since the endline was not run in the lean season, the effects may be muted. The bulk of earnings seems to go into assets, savings, and non-food consumption. For instance, savings more than tripled, increasing to UGX 107,344 ($54).

Two concerns are bias arising from baseline imbalance and systematic attrition. The results, however, are robust to exclusions of the baseline covariates, a difference-in-differences ITT estimate controlling for other baseline covariates, and also relatively extreme attrition bounds (see online Appendix D).

Comparison to Nonparticipants.—We can also see how WINGS affects the position of program participants in the community by comparing them to nonparticipants in control villages. Figure 2 depicts the distribution of the standardized income index for participants in treatment villages, participants in control villages (i.e., those nominated for the program), and adults from nonparticipant households in the control villages, divided into households that did and did not engage in petty trading at baseline. Comparing distributions, we see that treatment brings the ultrapoor nearly past the income levels of nontrading households, but not past the income levels of traders. In control villages, means are −0.36 among participants, −0.06 among nontrading nonparticipants, and 0.59 among trading nonparticipants. Participants in treatment villages have a mean of 0.02.

The income index is scaled to have zero mean and unit standard deviation across all participants and nonparticipants. We restrict the sample to 17 to 40 year olds.

Within-Village Spillovers.—About 10 percent of village households received WINGS, meaning WINGS provided a large cash influx into the village. Most participants entered petty trading, and so we might wonder about the effect of new entrants on prices or preexisting traders in treatment villages (especially given that the median village has no shops).[25] Comparing treatment and control villages,

[25] If the local retail market is not perfectly competitive (e.g., due to liquidity constraints serving as entry barrier), a program that promotes petty trading should drive down the price of imported goods by increasing competition. Cunha, De Giorgi, and Jayachandran (2011) find some evidence that imperfect competition partly explains

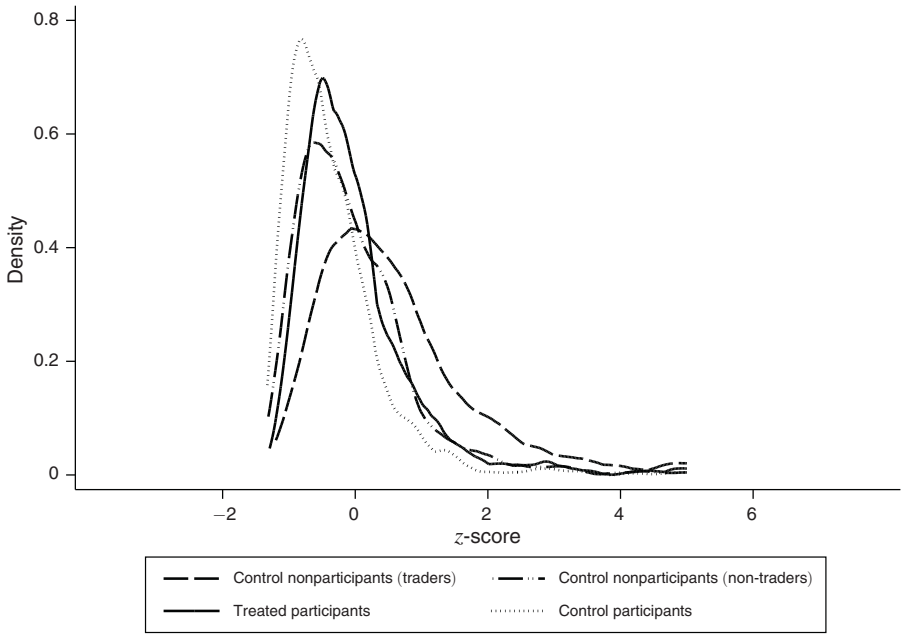

FIGURE 2. INCOME INDEX DENSITY, WORKING-AGE ADULT PARTICIPANTS AND NONPARTICIPANTS

Notes: The income index is scaled to have zero mean and unit standard deviation across all participants and nonparticipants. We restrict the sample to 17 to 40 years olds.

prices of imported and produced/exported goods both fell a slight amount (0.05 and 0.09 standard deviations, not statistically significant), potentially because increased trade decreased the market power of existing traders and brought prices closer to the competitive equilibrium. We present these results in detail in online Appendix D5.

We can also compare nonparticipants in treatment villages to nonparticipants in control villages (with the caveat that within villages participants were not randomly selected). Overall, despite the size of the program, there are limited spillovers to markets or nonparticipant households. We see no effect on the incomes or occupational choice of nonparticipant households, although if we look only at preexisting traders we see a slight shift from petty trading to casual and agricultural work, with incomes holding more or less steady.

the price effects of transfers in rural Mexico. This would benefit consumers, but potentially decrease the profits of existing retail sellers, leading them to shift occupations and potentially lose income. Also, as in Buera, Kaboski, and Shin (2012), new entrepreneurs could supply less labor to the market, more labor to their own business, and may even demand labor from the market instead. This will push up market wages. Given that most people in northern Ugandan villages are not fully employed, most enterprises are owner-operated, and there is a limited market for casual labor, we did not expect to see a significant change in wages. At the same time, to the extent that higher earnings increases participants' consumption, it could create a countervailing effect on prices, diminishing any decline. Studies of Mexican conditional cash transfer programs find that the transfers increase consumption of both cash recipients and program-ineligible households (Hoddinott and Skoufias 2004, Angelucci and De Giorgi 2009). Higher demand can increase prices as a result, especially in more remote locales (Cunha, De Giorgi, and Jayachandran 2011).

B. *Impacts of Group Formation*

Columns 3 and 4 of Table 3 report the impacts of receiving the group encouragement. There is no evidence of an impact on business start-up, occupational choice, and levels of work. By one measure, though, group formation increased incomes. With group formation and training, the earnings treatment effect is more than double the size of the WINGS program without the group intervention. Durable assets and nondurable consumption, however, are not much higher. Overall, because of the earnings impact, the income index is 0.15 standard deviations greater but significant only at the 10 percent level.

We thus treat the earnings effect with caution. The magnitude is large enough, however, that it's worth exploring potential reasons. The evidence suggests that informal insurance and cooperative activities (especially farming) play a role.

Table 4 reports ITT estimates for various group interactions. Two-thirds of the control group are members in a community group of some kind, from water and school committees to savings and farming groups. Being in a treatment village without group encouragement increases group membership by 12.9 percentage points, and the group formation treatment nearly doubles this effect. The people in group formation villages go from being in 1.7 groups to 2.9.

Treatment without group encouragement increases the frequency of meeting with a group. People in control villages meet with their "most important group" (not necessarily the WINGS-identified group) 1.4 times a month. This rises to about 1.8 times a month with the standard WINGS program, and to 2.4 times with the group dynamics training. Meetings are principally for savings-related activities or communal farming, and seldom for petty business-related activities.

The group is more likely to save together and lend to one another—in effect providing a form of informal insurance. The group dynamics training leads to an increase in loans to and from other households, but no significant change in transfers (which do not have to be repaid). Total debts rise as a result of group dynamics to almost double the control level of debts.

Communal farming is one of the most commonplace forms of economic cooperation—people pool their labor and either assist each other on one another's plots, or farm a new plot collectively for cash or own consumption. Control villagers meet 1.3 times a month for farming, and this rises by almost two-thirds with WINGS and group dynamics training. We do not have a measure of total cooperative farming hours, but note that with WINGS alone, earnings from the last harvest falls by UGX 33,211 (about $18) compared to the control group, perhaps because petty trading crowds out these activities. But there is no decrease in the group dynamics trained villages.

C. *Health, Social Relations, and Empowerment*

Health.—Table 5 reports non-economic program impacts. There is little change in an index of physical health measures.[26] But of the three people who died between

[26] It is a standardized index of days ill, a subjective "overall" health question, and three activities of daily living (walking a distance, carrying a heavy load, and working on a farm). See online Appendix D for component treatment effects.

TABLE 4—PARTICIPATORY FINANCIAL IMPACTS OF GROUP FORMATION

Outcome	Control mean (1)	ITT estimates, 16 months after grants (Observations = 1,734) No group training (2)	Group training (3)	Difference (4)
Group engagement				
Member of any community group	0.668	0.129	0.245	0.116
		[0.028]***	[0.026]***	[0.025]***
Number of community groups	1.721	0.448	1.169	0.720
		[0.138]***	[0.155]***	[0.173]***
Of your "most important" group:				
Times you meet per month	1.409	0.362	0.954	0.592
		[0.156]**	[0.147]***	[0.175]***
For communal farming	1.297	0.415	0.843	0.428
		[0.327]	[0.294]***	[0.375]
For savings	1.037	0.767	1.555	0.788
		[0.196]***	[0.148]***	[0.217]***
For social support	0.117	0.051	0.201	0.150
		[0.032]	[0.040]***	[0.043]***
For business	0.142	0.089	0.142	0.053
		[0.104]	[0.062]**	[0.116]
Financial access and inter-HH transfers				
Transfers to other HH, 000s UGX	8.495	7.269	9.126	1.857
		[2.225]***	[1.961]***	[2.802]
Transfers from other HH, 000s UGX	19.809	44.663	40.858	−3.805
		[6.900]***	[7.078]***	[9.641]
Loans to other HH, 000s UGX	6.317	4.487	10.457	5.970
		[1.876]**	[1.997]***	[2.450]**
Loans from other HH, 000s UGX	9.215	1.408	6.578	5.170
		[1.634]	[2.336]***	[2.511]**
Member of a savings group	0.272	0.271	0.517	0.246
		[0.048]***	[0.038]***	[0.052]***
Total savings, 000s UGX	37.374	107.607	115.467	7.860
		[12.312]***	[12.544]***	[16.934]
Total debts, 000s UGX	5.232	0.974	4.141	3.167
		[1.007]	[1.270]***	[1.514]**
Other outcomes				
Access to business advice in village, *z*-score	−0.081	0.145	0.213	0.068
		[0.062]**	[0.064]***	[0.063]
Earnings in last harvest, 000s UGX	152.768	−33.211	2.204	35.415
		[14.844]**	[14.022]	[16.514]**

Note: See notes to Table 3.
***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

TABLE 5—IMPACTS OF WINGS ON HEALTH, SOCIAL RELATIONS, AND EMPOWERMENT

Outcome	Control mean (1)	ITT estimates, 16 months after grants: No group training (2)	Group training (3)	Difference (4)
Health				
Physical health index, *z*-score	0.002	0.020	−0.013	−0.033
		[0.070]	[0.069]	[0.085]
Died since baseline	0.003	−0.005	−0.005	0.000
		[0.002]*	[0.003]*	[0.002]
Social relationships				
Quality of family relationships, *z*-score	0.018	0.034	0.011	−0.023
		[0.057]	[0.052]	[0.067]
Social support received, *z*-score	−0.084	0.195	0.159	−0.037
		[0.069]***	[0.063]**	[0.081]
Community participation, *z*-score	−0.086	0.159	0.345	0.187
		[0.055]***	[0.062]***	[0.070]***
Community hostility index, *z*-score	−0.070	0.164	−0.018	−0.182
		[0.073]**	[0.050]	[0.076]**
Empowerment				
Autonomy in purchases, *z*-score[a]	−0.026	0.082	0.089	0.007
		[0.059]	[0.062]	[0.075]
Physical and emotional abuse, *z*-score[a]	−0.030	0.066	−0.046	−0.113
		[0.079]	[0.078]	[0.088]
Degree of partner control, *z*-score[a]	−0.110	0.170	0.129	−0.041
		[0.082]**	[0.079]	[0.086]
Partner relationship quality, *z*-score[a]	−0.086	0.180	0.201	0.021
		[0.085]**	[0.111]*	[0.119]
Female and lives with partner at endline	0.536	0.046	0.072	0.026
		[0.020]**	[0.022]***	[0.024]

Notes: See notes to Table 3. Means and ITTs for index components are in online Appendix D.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

[a]Women with partners at endline only ($n = 961$).

baseline and endline, all were control group members. This translates to a 0.5 percent decrease in mortality, significant at the 10 percent level.[27]

Social and Community Participation and Status.—Increased income and employment is associated with greater social support, community participation, but also community hostility, according to several index measures.[28] We see little change in an index of three questions on the quality of family relationships, but we see a 0.2 standard deviation increase in seven forms of social support received in the past month (such as someone comforting you when you are feeling sad); and a

[27] Also, as shown by Green et al. (forthcoming), there is also no change in mental health, as measured by an index of 35 symptoms of depression and distress.

[28] Means and treatment effects for individual components are available in online Appendix D.

0.16 standard deviation increase in five forms of community participation (such as speaking out at community meetings or being a community leader).

But program participants (at least those who did not receive the group dynamics training) were more likely to report disputes with neighbors or verbal abuse from others. Our qualitative interviews, however, suggest they were targets of jealousy or resentment from a small number of other households in the village, rather than the village at large. Group formation and training may have insulated marginalized women from such abuse.

Empowerment.—Finally, we see little evidence that enhanced business activity and incomes increased reports of empowerment. We ask all subjects about their autonomy in household financial decisions, such as whether they can decide how to spend their pocket money, use their earnings to buy clothes without permission, or have a say in the purchase of large assets in the household.[29] This autonomy measure increases by 0.08 standard deviations (not statistically significant).[30] We also asked women with a partner at endline about aspects of their relationship.[31] There is almost no change in self-reported physical and emotional abuse by the partner.[32] And, if anything, women actually report an increase in the degree of control their spouse asserts over their finances and freedoms of movement and association. The husband increases tendencies to control contact outside the home and also demands or seizes some of the women's newfound earnings (see online Appendix D). At the same time, women report a 0.18 standard deviation increase in the quality of the relationship, feeling more free to express their opinions and reporting a healthier relationship. Overall, these results paint a picture of husbands who encourage but then control their wife's business earnings, in return for weak increases in purchasing autonomy.

D. *Impacts of Supervision*

In Phase 2, when control villages received the program, we surveyed participants about grant use and future expectations a month after they received it (a few weeks before the first follow-up visits), and again a year later. Table 6 reports one-month treatment effects of expecting any follow-up, and Table 7 reports 12-month treatment effects of two visits (supervision without advice) and five visits (supervision with extended advice).

[29] We adapted empowerment questions from the Uganda Demographic and Health Survey.

[30] For women alone the result is significant at the 10 percent level.

[31] Treated women were more likely to answer these questions because they were slightly more likely to be married at endline, principally because of new marriages rather than any change in divorce rates. This could introduce positive or negative selection from "marginal marriages." We are interested in spousal abuse and relations as an outcome, and so the current results including selection are relevant. Alternatively, we could confine our analysis to the subset of women reporting partners at baseline. These results are not shown, but in general abuse and marital control are lower (though not significantly so) implying the marginal marriages are slightly better quality on average than baseline ones.

[32] See Green et al. (2015) for a more detailed study of intimate partner violence in this setting. Note, however, that abuse is reported by fewer than a quarter of women, and so is probably underreported. Even so, the effect of treatment is close to zero and so even significant underreporting is unlikely to affect the basic conclusion, so long as it is not significantly correlated with treatment.

Table 6—Impacts of Expecting Supervision, 1 Month after Grant

Outcome	Mean, no supervision (1)	ITT estimates, any supervision (Observations = 858) Coefficient (2)	Standard error (3)
Expects AVSI staff to visit them in future	0.097	0.878	[0.021]***
Autonomy in grant planning and spending, *z*-score	0.082	−0.170	[0.072]**
Grant money diverted to others, 000s UGX	1.290	0.281	[1.293]
Proportion of grant spent on			
Business investments and expenditures	0.269	0.050	[0.022]**
Large assets or home improvements	0.119	−0.038	[0.018]**
Food, clothing, or personal items	0.018	−0.007	[0.002]***
Gifts, contributions, or celebrations	0.002	0.000	[0.001]
Health or education	0.035	−0.007	[0.005]
Saved or unspent	0.537	0.002	[0.022]
Expenditure data (*000s UGX*)			
Nondurable consumption	47.821	−0.509	[2.211]
Total business investments since grant	27.481	−1.841	[3.539]

Notes: Columns 2 and 3 report the coefficients and standard errors on assignment to either two or five visits from an OLS regression of each outcome on this treatment indicator, a stratum fixed effect, and baseline covariates. Standard errors are robust and clustered by village.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

One-Month Impacts.—People appear to have believed their treatment assignment: only 10 percent of the "no follow-up" group said they expected a visit, whereas 98 percent of those assigned to any follow-ups did.[33] As in Phase 1, participants also expressed independence and control over the funds. For instance, 94 percent of the no supervision group said they felt free to spend the grant how they wished, and 80 percent said they could deviate from the business plan. An index of seven such measures of autonomy fell by 0.17 standard deviations with supervision. With or without supervision, however, there is little evidence of diversion of the grant or social pressure to share it.[34]

Expecting supervision has a modest impact on investment, at least by some measures. First, we asked participants to categorize how they spent the grant. Second, we sum all investment-related expenses in the last month from an expenditure module. Using the grant allocation measure, people have saved or not spent 54 percent of the grant and reported almost no spending on celebrations and gifts, whether they expected supervision or not. But those expecting a visit increased their share of the grant spent on business investment by 5 percentage points, reducing the share spent on durables (e.g., homes or livestock).

By the expenditure survey, however, follow-up has little effect on the amount reported spent on business items. One reason could be that money is fungible, and

[33] AVSI followed through on this assignment: none of the "no follow-up" people were visited, and 91 percent of those assigned to two or five follow-ups reported receiving the correct amount.

[34] We asked participants how much of the grant they had to give to household members and other community members. In total this was typically less than 1 percent of the grant, and expecting follow-up had little impact on the amount.

TABLE 7—ECONOMIC IMPACTS OF SUPERVISION, 12 MONTHS AFTER GRANTS

Outcome	Mean, no supervision (1)	ITT estimates (Observations = 868) 2 visits (2)	5 visits (3)	Difference (4)
Occupational choice				
Any nonfarm self-employment	0.58	0.110	0.152	0.042
		[0.039]***	[0.044]***	[0.037]
Started enterprise since baseline	0.92	0.045	0.061	0.016
		[0.019]**	[0.018]***	[0.014]
Average work hours per week	31.49	1.105	4.764	3.659
		[2.229]	[2.187]**	[2.453]
Agricultural	26.28	−1.168	1.588	2.757
		[2.213]	[2.168]	[2.230]
Nonagricultural	5.206	2.274	3.176	0.902
		[0.907]**	[0.892]***	[1.137]
Average hours of chores per week	33.42	1.698	1.797	0.099
		[1.718]	[1.481]	[1.570]
Income and food security				
Index of income measures, *z*-score	−0.06	0.021	0.036	0.015
		[0.082]	[0.068]	[0.073]
Monthly cash earnings, 000s UGX	13.06	3.308	2.170	−1.138
		[2.523]	[2.010]	[2.637]
Durable assets, *z*-score	0.81	−0.065	0.052	0.117
		[0.089]	[0.088]	[0.082]
Nondurable consumption, 000s UGX	132.57	−1.008	−3.022	−2.014
		[4.968]	[5.464]	[4.781]
Times went hungry, past week	0.16	−0.019	−0.077	−0.058
		[0.043]	[0.030]**	[0.036]
Usual number of meals per day	1.73	0.029	0.043	0.014
		[0.041]	[0.038]	[0.037]

Notes: All variables denominated in UGX and hours were top-coded at the ninety-ninth percentile to contain outliers. Standard errors are robust and clustered at the village level.

***Significant at the 1 percent level.

**Significant at the 5 percent level.

*Significant at the 10 percent level.

while expenditures in the days following the grant might have fallen more heavily on durables, the participants may have made up the investment shortfall by spending other earned income on the relevant materials for their business.

One-Year Impacts.—Table 7 reports the impacts of supervision a year after grants. We conducted the survey during a time of wet season planting and the beginning of the main harvest, at the closing of a lean season.

Two supervisory visits increase the likelihood someone is operating a business by 11 percentage points (19 percent), and hours of nonfarm work per week by 2.3 hours (44 percent). The three further visits lead to slight, not statistically significant increases.

Incomes are 0.021 standard deviations higher as a result of the two visits, and 0.036 standard deviations higher from five visits. Neither result is statistically

significant.[35] Compared to those with no visits, those with two visits have nearly a quarter higher cash earnings. Overall, cash earnings are low because of the season. This is one reason we focus on measures of permanent income, including durable assets and nondurable consumption, where we see no significant increase with supervision.

We also tested whether supervision has greater impact on the most present-biased or least autonomous individuals. We measured future orientation using incentivized games and self-reported survey questions. We also use a composite measure of three self-reported financial autonomy questions. We interact these baseline measures with treatment in online Appendix D. The future orientation level and interaction have the expected sign (i.e., more investment and earnings) though the autonomy measure does not. None are statistically significant, but the coefficients on treatment—which represents the effect of treatment on the present-biased and less autonomous—are larger and more statistically significant than in Table 7.

E. *Rate of Return on the Program*

We lack the long term data to do a full cost-effectiveness analysis, but in Table 8 we imagine the simple case where the increase in income is permanent, and calculate a simple internal rate of return (IRR) given program costs. Our preferred measure is our estimate of nondurable consumption. We also report IRR calculations for the lower earnings treatment effect, although earnings probably understate returns because it ignores other household members, non-cash earnings, and the proportion of the grant that went into savings or durable assets.

The full WINGS program has an internal rate of return of roughly 24 percent using total household nondurable consumption. Since we used an abbreviated consumption measure, this is probably an underestimate of the true effect. This consumption-based return is similar with or without group encouragement, since the impact and cost rise more or less proportionally.[36] If we use the social discount rate of 5 percent commonly used by the World Bank and International Monetary Fund (IMF), the present value of the consumption treatment effect is nearly five times the cost of the program.

By way of comparison, the six livestock-based graduation programs evaluated by Banerjee et al. (2015) had IRRs ranging from 2 percent to 24 percent two years after the program, with an average IRR of 8 percent. Total consumption benefits were about 2.3 times as great as total costs of that program. The graduation programs have an additional year of data, however, and so we can be more confident in the sustainability of its gains.

Finally, despite the fact that supervisory visits represent about half of program costs, we cannot reject a negative or zero return. The sign of the IRR for supervisory visits is ambiguous, however, because the treatment effects of supervision

[35] Given the sample size, we estimated we would require income increases of 0.15 or greater to have 80 percent statistical power.

[36] If we use the earnings treatment effects, the IRRs are lower but still large and positive: 8 percent without group encouragement and 16 percent with it.

TABLE 8—PROGRAM COSTS AND SIMPLE INTERNAL RATE OF RETURN (IRR) CALCULATIONS, 2009 PPP USD

Phase	Program	Consumption ITT		Program cost	Hypothetical perpetuity		
		Monthly	Annualized		IRR	Present value at 5% discount rate	Proportion of program cost
Panel A. Using aggregate consumption treatment effect							
1	Without group training	38.79	465	1,946	24%	9,309	478%
	With group training	41.80	502	2,150	23%	10,302	467%
2	Two supervisory visits	−1.26	−15	348	−4%	−302	−87%
	Five supervisory visits	−3.78	−45	871	−5%	−907	−104%

Phase	Program	Earnings ITT		Program cost	Hypothetical perpetuity		
		Monthly	Annualized		IRR	Present value at 5% discount rate	Proportion of program cost
Panel B. Using monthly earnings treatment effect							
1	Without group training	12.97	156	1,946	8%	3,112	80%
	With group training	29.24	351	2,150	16%	7,017	163%
2	Two supervisory visits	4.14	50	348	14%	992	142%
	Five supervisory visits	2.71	33	871	4%	651	37%

Notes: The internal rate of return is calculated as the discount rate r at which the net present value (NPV) of the program returns are equal to the program cost. Since $NPV = \sum_{t=1}^{\infty} (ITT \times 12) \times (1 - r)^t = \frac{ITT \times 12}{r}$, $IRR = \frac{ITT \times 12}{Program\ Cost}$.

on earnings and consumption run in different directions, and were in neither case statistically significant. The IRR is negative for the preferred consumption measure. It's possible, however, that supervision pays off in the longer term since business survival rates are higher.

V. Discussion and Conclusions

These results show that a package of $150 cash ($375 in PPP terms), five days of business training, and ongoing supervision led to a doubling of new nonfarm enterprises and a significant rise in incomes among extremely poor and conflict-affected villagers, most of whom were women who had never operated such an enterprise before.

Second, the results show that simply encouraging people to form self-help groups—centered around communal savings, lending, and work—was enough to boost incomes. By some measures (such as consumption) this benefit was proportional to the cost. By other measures (monthly earnings) the benefit was more than proportional. Either way, it suggests that the poorest were not simply constrained by a lack of capital, credit, and insurance. Social interactions among the marginalized were simple to encourage and this stimulated valuable labor-sharing and risk-sharing.

Finally, by some measures, supervising how participants spent the grant strengthened their incentives and commitment to invest. We see no evidence that supervision increased long-run performance, however, since the impact on income by some measures (such as consumption) was negative, and by other measures (earnings) was positive, but in neither case was it statistically significant after one year. We were not able to test the effects of a grant with or without supervision against a

pure control group in Phase 1 of the study, which is an unfortunate limitation. We also only have one-year data on the impacts of supervision. Nonetheless, given that supervision costs two to three times as much as the grant itself, the impacts are surprisingly ambiguous.

The returns to streamlining programs such as WINGS are potentially large, possibly allowing more marginalized people to benefit for the same level of aid spending. For instance, targeting and disbursement were nearly as costly as the grant itself, yet participants were not much different than nonparticipants in the village who were not already traders. Moreover, mobile banking technologies are cheaper and becoming available in the region. Finally, the last three to five visits cost more than the grant itself, but the returns to additional supervision appear to be diminishing at best.

Reducing these costs could have high returns. For instance, a hypothetical program that delivered similar impacts for half to two-thirds the cost could have a 35–55 percent internal rate of return.[37] It's impossible to say what would happen to program impacts from such cost-cutting, but given the estimates in Table 8 it is difficult to imagine that impacts would fall proportionally to costs. Given the growing amount of cash-based programming in humanitarian and development settings, this proposition demands testing.

A. *Comparisons to Other Ultrapoor Programs*

It's difficult to compare this program to the alternatives without randomized, head-to-head comparisons, but the impacts of WINGS compare favorably to evidence on transfers of livestock and unconditional cash, at least within the timeframe we can evaluate with our design.

"Graduated" Ultrapoor Programs.—These provide in-kind capital, such as livestock, along with training and other services, such as temporary income support. The fact that the WINGS participants invested much of their microenterprise profits in livestock suggests that animals are important investments and stores of value. Yet the fact that the poorest chose to make these investments on their own, plus the relatively high returns to petty business, also suggest that the ultrapoor can make forward-looking, profitable investment choices with some basic assistance.

Graduated programs have more and longer-term evidence in their favor than cash-based programs, however, with studies showing 10 to 40 percent increases in consumption or earnings over two to four years (Banerjee et al. 2011, Bandiera et al. 2013, Banerjee et al. 2015). The 16-month impacts of WINGS are comparable to the upper end of this range using an abbreviated consumption measure. We cannot say if the WINGS impacts persist, but several cash transfer studies with non-extreme

[37] For instance, if targeting and disbursement costs could be limited to 10 percent of the cost of grants (a cost reached in the Kenya GiveDirectly experiment, as discussed by Haushofer and Shapiro 2013) and if there were merely one to two supervisory visits, the average program cost would be half of the current amount.

poor demonstrate steady or increasing four to six year impacts (Blattman, Fiala, and Martinez 2014; de Mel, McKenzie, and Woodruff 2012b).[38]

Unconditional Cash Transfers.—These lie at the other extreme. This is the approach taken by GiveDirectly in Kenya. Results from a randomized control trial of grants of one-time transfers of $400 and $1,500 (in PPP terms) show high returns to investment in household durables after an average of about seven months, and large short-term increases in consumption (Haushofer and Shapiro 2013). It remains to be seen if these returns persisted, as there is some indication they decline over the seven months.

B. *Mechanism*

Why might WINGS have had such large impacts on income? To generate investment and high returns, programs such as WINGS must help overcome some constraint. Otherwise the ultrapoor in our sample would already be operating at their efficient scale, and a cash influx would not be invested in such a way as to generate high returns.[39] Four of the most common constraints in the literature are: lack of credit, imperfect insurance, low business knowledge (or skill) levels, and present bias. Most anti-poverty interventions implicitly or explicitly target one or more of these.

While all these constraints undoubtedly play a role, the circumstantial evidence points to the importance of cash and group encouragement relieving a credit constraint. First, credit constraints in northern Uganda were extreme. As we noted in Section I, participants' ability to borrow before the program was almost nonexistent. Just 4 percent of our sample said they could get a loan of $50, and those loans were short term and typically carried interest rates in excess of the business returns we observe. The cash relieved this constraint directly, obviously, but group formation also had significant effects on the formation of ROSCAs and the incidence of borrowing. Group formation may also have increased informal insurance (though since the main effect of group formation was on inter-household loans rather than transfers, insurance is difficult to separate from increased access to credit).[40]

[38] Note that, like WINGS, none of these cases are truly unconditional cash transfers, as most cash transfers are labelled or involve some selection, such as preparation of a business plan.

[39] See online Appendix C for a formal model of occupational choice under various constraints, and a proof of the efficient scale argument.

[40] It's unlikely that a single cash transfer relieved the insurance constraint. Bianchi and Bobba (2013) show that knowledge of future cash transfers can increase current entrepreneurship because future transfers insure against risky enterprise income. In the program they study, however, households knew they would receive regular transfers over several years, whereas in our case people receive a grant only once and in the very near future. Furthermore, relieving an insurance constraint is only consistent with high returns to capital if the petty business is significantly more risky than traditional occupations. We asked respondents about expected variance in incomes, and found that even though treated people expect more volatile incomes in the future, their lowest expected income after the program is twice as high as in the control group. We asked people to estimate their highest, median, and lowest incomes in the coming year. The difference between highest and lowest expectation increases by two thirds but their lower bound doubles (online Appendix Table D2). Qualitatively, our interviews also suggested that subsistence farming is perceived to be as or more risky than petty trade, if only because petty trade has higher cash flow and plentiful local markets.

The success of the group encouragement program component is intriguing. The impacts on income are ambiguous, but the effects on behavior are not. It illustrates that social capital of various forms was important and may not always form without third-party encouragement. Our design probably understates the importance of group encouragement, moreover, since some of the success of the core intervention, without formal group encouragement, was surely due in part to the group encouragement implicit in the group-based training and other elements of the program design. There are close parallels to findings by Feigenberg, Field, and Pande (2013), who show that encouraging social interaction via group meetings reduced default on individual loans in India. It seems that encouraging group and ROSCA formation can increase social interactions, enhance social capital, increase risk-pooling and cooperation, and raise incomes.

Finally, the training and follow-up provided business advice and information. Most of the people in our sample had little experience in microenterprises in general or petty trading in particular. Moreover, the positive effect of two initial supervisory visits (by some measures, at least) is consistent with the hypothesis that at least a subset of people are time-inconsistent. Thus, skills training and supervision cannot be separated from the effects of credit constraints and low social capital. Even so, the training and supervision cost four times as much to deliver as the grant, and six times as much as the group dynamics training. Also, a review of more than a dozen evaluations of business skills training fails to find that it passes a cost benefit test, at least on its own (McKenzie and Woodruff 2014). Given the prevalence of training and supervision components in other programs, and their obvious cost, a clear experimental test of their efficacy with cash (or other capital transfers) is important.

C. *Generalizability*

How generalizable are these results? The post-conflict context in which WINGS was implemented could mean that returns to capital are higher than elsewhere: low levels of initial competition, scarce capital, and low financial development, all within a national economy that is growing as it adjusts to a politically stable equilibrium.[41] On the other hand, we observe similar impacts in nonconflicted regions, whether from the graduation program evaluations mentioned above, or microenterprise programs elsewhere in Uganda.[42]

In the event that post-conflict arenas do offer higher returns, however, there is unfortunately no shortage of refugees, displaced persons, disaster victims, and conflict-affected populations. Our results suggest that cash-centric ultrapoor programs could reduce extreme poverty in such settings, in addition to the stable contexts where they are more familiar. This is useful to know, since with logistical and funding challenges, the UN and other donors are turning to cash as a main form of

[41] A study in post-tsunami Sri Lanka finds a slow return of firms to pre-disaster capital, that firms receiving grants recovered sooner, that returns to capital doubled in damaged areas, and that capital had the largest impacts on retail firms (de Mel, McKenzie, and Woodruff 2012a).

[42] See Bandiera et al. (2012); Blattman, Fiala, and Martinez (2014).

support for complex emergencies and humanitarian settings, such as cash support to the millions of Syrian refugees across the Middle East.

The matter is unsettled. Given the potential scalability and cost-effectiveness of cash, and the huge number of extreme poor, what is needed most is a multi-country trial, ideally one that evaluates the returns to additional components (such as training) and pits cash transfers head-to-head with other asset transfer strategies.

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