Short and Medium-Term Impacts of an Entrepreneurship Training on Employment and Employability: Experimental Evidence from Rwanda

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Abstract: We use a field experiment to study the short- and medium term impacts of a training intervention that aims to increase employability of Rwandan (underemployed) youths. The training includes networking and mentorship, as well as modules on developing entrepreneurship, technical skills and ‘soft’ social skills. We evaluate intended impacts of the training on attitudes towards work, employability, and labor market outcomes such as employment and income. We also consider unintended ‘deep impacts’ on honesty and social behavior. The outcomes of the intervention are mixed. While the training has positive effects on employability and employment in the short term, these effects are attenuated over time. Specifically, after two years we only find modest effects of the training on network size and self-employment (marginally significant) of participants.

Keywords: Underemployment, entrepreneurship, Akazi Kanoze, training intervention.

JEL Codes: I25, J24, J46, O17.

Acknowledgements: We would like to thank workshop participants in Kigali, Groningen and Berlin for comments and suggestions. This work was supported by the Netherlands Foundation for Scientific research (N.W.O. grant # W 08.01.101).
1. Introduction

Employment is an important source of income and provides people with an identity and sense of self-esteem. Particularly in developing countries, employment takes many forms – often simultaneously for the same individuals. Many people pursue a portfolio of activities to earn a living, including self-employment, salaried jobs and (more or less informal) activities on a casual basis. While few people can afford the luxury to be truly unemployed, there are widespread concerns about “underemployment,” especially in Africa. Underemployment refers to the situation where individuals cannot work as much as they prefer to do, and consequently earn less than what they would want to earn. This paper studies the impact of a human capital training intervention that aims to improve “employability”, promote entrepreneurial activities, and reduce underemployment in Rwanda.

Underemployment throughout large parts of Africa has its roots in the failure of the structural transformation to take off. The productivity of labor is too low, relative to wages, for the manufacturing sector to be competitive on international markets and absorb large numbers of workers from the agricultural sector (Frankema and Waaijenberg 2018). Demographic factors compound the challenge. The African population is, on average, young, and projected to grow very rapidly the coming decades. Underemployment is therefore expected to become a growing concern, and problems associated with underemployment will manifest themselves more clearly in years to come.

The most immediate problem of underemployment is the unfulfilled potential of individuals, resulting in low incomes or even outright poverty. But there are other problems as well. The lack of a perspective on satisfying future employment might contribute to social instability, especially in fragile states. For example, Barr et al. (2016) document that unemployment has adverse moral consequences, reducing the propensity of individuals to
acknowledge others’ earned entitlements. Donors and governments are concerned that large groups of young men who lack the prospect of decent jobs or a stake in local development might engage in anti-social behavior (e.g. fighting, crime) or even engage in rioting and rebelling. A large literature has probed the complex inter-relationships between employment and income on the one hand, and anti-social behavior or insurgency on the other (e.g., for a recent review, refer to Bruck et al. 2016).

In light of these concerns, it is not surprising that promoting employment and entrepreneurship has emerged as a top priority throughout Africa. In the long run, private sector development should probably emerge as the main engine of employment growth, requiring a supportive policy and institutional environment. In the short run, however, complementary initiatives might help. Blattman and Ralston (2015) distinguish between two common approaches to increase employment. So-called demand-side programs aim to directly increase the demand for (unskilled) labor, for example via workfare initiatives associated with the building of infrastructure. In contrast, supply-side programs provide an important input or service to reduce external constraints that are perceived to constrain the growth of employment. Would-be or fledgling entrepreneurs as well as unskilled youths may be targeted for support and receive, for example, injections of capital or access to a training that aims to generate specific “skills” which would, among other things, stimulate entrepreneurship. Supply-side approaches are based on the (implicit) assumption that markets fail to generate and allocate inputs and services effectively. Relaxing this “binding constraint” should unleash productivity growth and foster employment.
Evidence regarding the impact of supply-side interventions is now slowly accumulating. In light of the focus of our study we consider interventions to increase human capital. These interventions are common and have received widespread financial support in recent years – the World Bank alone invests up to one billion dollars per year in training programs. Human capital interventions come in different forms and types. While some programs focus on entrepreneurship and business skills, others aim to improve non-cognitive skills, or provide technical and vocational training. Entrepreneurship trainings cover issues such as financial literacy and book-keeping, marketing and sales, and prepare potential entrepreneurs to start up a business or existing micro-entrepreneurs to increase the scale and profitability of their ongoing operations. Investments in non-cognitive or so-called “soft skills” include efforts to improve the representation (appearance) and health of participants, but also to build character skills such as self-control and sociability. Vocational and technical training aims to raise the productivity of labor across a broad range of activities, from hairdressing to carpentry, by providing respondents with practical skills and sometimes with an internship as well (assisting networking and building experience). Many interventions combine these approaches, and some combine investments in human capital with injections of physical capital.

While a large body of literature examines the impact of human capital interventions in developed countries (see, for example, Kluve et al. 2019), rigorous empirical work based on credible counterfactuals remains scant in the context of developing countries. McKenzie and Woodruff (2014) critically discuss the literature evaluating the impact of entrepreneurship

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1 A number of studies suggest that the marginal return to capital tends to be high in developing countries (De Mel et al. 2008), which has inspired a plethora of initiatives involving physical capital injections. These include (unconditional) cash transfers to individuals or groups of people (e.g., Blattman et al. 2014, 2016), business plan competitions (e.g., Fafchamps and Quin 2015), or the provision of valuable assets such as livestock (e.g., Bandiera et al. 2013). Blattman and Ralston (2015) and Filmer et al. (2014) review the available evidence and conclude there is considerable support for the hypothesis that capital-centric approaches to employment generation “work” in the sense of creating employment. However, more recent evidence reported in Blattman et al. (2018) is critical about the long-term impacts of a cash grant intervention in Ethiopia – all positive impacts of the capital injection (and a job offer intervention) had dissipated after five years.
trainings, and conclude that problematic design of many studies precludes the drawing of strong conclusions (not enough statistical power, “timing” of the evaluation, etc.). There is only limited evidence of sustained impact on firm profit or income, and evidence is especially scanty for female entrepreneurs.² Blattman and Ralston (2015) review the evidence regarding technical and vocational training, and conclude it is disappointing—especially for men (exceptions include Maitra and Mani (2017) and Adoho et al. (2014)). Across the board, labor market outcomes were small or absent, and (young, male) dropout rates were high.³ An important reason for the lack of success of many employment programs is that they aim to prepare candidates for jobs that do not exist (Fox and Paul, 2018).

However, not all outcomes are negative. Chakravarty et al. (2019) evaluate a skills training in Nepal, which did not improve the paid employment status of participants, but had positive impacts on self-employment activities of women, and income of men. Even if wage employment opportunities are scarce or do not exist, skills trainings can have positive effects on employment. Shonchoy et al. (2018) do not find positive impacts of a standard employment training, but if a stipend is added to the training (lowering credit constraints) or an ‘on the job’ training module is included, the training has positive impacts—even 18 months after the intervention. This suggests traditional interventions focusing exclusively on knowledge or skill transfer  may not be enough, and that details of the training intervention matter.

While standard entrepreneurship trainings focus on business practices and technical skills, some recent studies evaluate interventions that try to change so-called “soft” skills. These interventions focus on issues such as a proactive mindset, and are generally more psychology-based. Early examples of such work are studies by Campos et al. (2017, 2018). They use a

³ For related evidence from developed countries, refer to Heckman et al. (1999) and Hirschleifer (2014).
randomized controlled trial design to compare the impact of a personal initiative training program, with a traditional business training in Togo. The personal initiative program has sizeable and significant effects on sales and profits, even after a 2 years period, while the traditional training only affected the uptake of business practices. Ubfal et al. (2019) evaluate a similar program for Jamaica, and find that positive short-term effects have dissipated 12 months after the training. The studies by Campos et al. (2017, 2018) and Ubfal et al. (2019) focus on existing micro-entrepreneurs rather than underemployed youths, and don’t consider employment effects. A final study related to ours is Blattman et al. (2017), who consider cognitive behavioral therapy among young high-risk urban men in Liberia. They find therapy successfully affects the identity and character of participants (e.g. it increased self-control and reduced the propensity to engage in criminal or violent behavior) but has no significant effect on employment prospects.

The objective of this paper is to evaluate the impact of an entrepreneurship training that includes efforts to improve soft and technical skills, in rural Rwanda. We use a field experiment to evaluate the impacts of participating in the training on a range of outcome indicators—based on the implementing agency’s theory of change. The ultimate aim of the intervention is transformative; it seeks to prepare “youths” who dropped out of school with the right set of skills and attitudes to find and retain gainful employment. We therefore consider the effects of the training on various measures of outcomes (e.g., “work readiness”, size of the participant’s professional network), and impacts (e.g., employment status and income). In addition to considering intended effects of the training, we also evaluate potential unintended deep impacts (e.g., honesty, anti-social behavior). These variables may be associated with social stability, and are therefore presumably of interest to policy makers. We consider both short-term and medium-term effects and, where possible, evaluate whether treatment effects vary with gender.
This study contributes to the small literature on the impact of training interventions by analyzing a traditional entrepreneurship and technical skills training that also includes a soft-skills component, in a low-income context. It fits in the body of work that considers the supply side approach to curb underemployment. The paper’s contribution rests upon (i) the use of a field experiment to evaluation in a novel context; (ii) an attempt to unravel the various types of effects according to the implementer’s theory of change (augmented with measures of social and anti-social behavior); (iii) an explicit focus on how impacts evolve over time by considering both short-term as well as medium-term effects; and (iv) the integration of surveys and incentive-compatible lab-in-the-field games to measure preferences of participants.

We document positive impacts of the intervention on many dependent variables in the short-term, including work readiness and employment status. The “signal-to-noise ratio” deteriorates as we measure effects further down the causal chain, and consider effects on savings and consumption. Perhaps not surprisingly, we also find that positive effects taper off over time. Two years after the training participants and non-participants are statistically indistinguishable for most of our dependent variables, with network size (an outcome) and self-employment status (an impact) as positive exceptions. More surprisingly, we also document a negative effect of the training on one of our ‘deep impact’ proxies. Specifically, trained respondents are more likely to cheat in an honesty experiment than their peers in the control group.

The paper is organized as follows. In section 2 we sketch the Rwandan context and introduce the intervention. Section 3 summarizes and scrutinizes the data, paying attention to measurement, balance between treatment and comparison group, and attrition. It also outlines the identification strategy. Section 4 contains the results, distinguishing between short- and
medium-term effects, and between outcomes and (deep) impacts. Section 5 present the results of various robustness analyses. The conclusions and discussion ensue.

2. The training intervention in Rwanda

We study the effects of an entrepreneurship training in the context of rural Rwanda. While the Rwandan economy has grown steadily since the late 1990s, this growth has failed to create sufficient jobs to create employment for the growing number of (increasingly well-educated) Rwandan youths. According to a recent African Economic Outlook, approximately 200,000 job seekers enter the labor market each year in Rwanda, and only 74,000 new jobs are created. As in other African countries, finding employment is particularly difficult for young people, who constitute no less than 67% of the Rwandan population. The official unemployment rate in Rwanda is 13.2%, and the underemployment rate is much higher. This is not unlike many other African countries.

Unlike most other African countries, the Rwandan government is well-informed about socio-economic conditions across the country, and has the capacity to design and implement policies nation-wide – top-down style. Perhaps the legacy of the genocide has created a more urgent sense of the importance of social stability than in other places. To curb concerns about unemployment and underemployment, the government has launched several initiatives. The Rwandan National Employment Policy (NEP) shows how the government is committed to developing the private sector by creating a favorable environment for business, helping firms to overcome constraints that hinder the growth of the private sector (MIFOTRA, 2014). It documents strategies to give entrepreneurs technical, material and financial support to establish or strengthen their enterprises, and recently introduced entrepreneurship courses in higher
learning institutions and Technical and Vocational Education and Training (TVET) to encourage graduates to generate their own employment instead of being job seekers.\textsuperscript{4}

The Akazi Kanoze (AK) Youth Livelihoods Project fits within the ambition to create employment and improve entrepreneurship in Rwanda through training interventions. It was a 7-year project financed by the United States Agency for International Development (USAID) and implemented by Education Development Center (EDC) between October 2009 and September 2016. The project targeted Rwandan “youths”, aged between 14 and 35, the majority of whom had dropped out of school without a diploma. The aim was to build youths’ capacity to seek out or create their own sustainable livelihood. This was accomplished through trainings and internship placement (job placement was not part of the program), aiming to provide youths with increased access to opportunities for productive engagement in Rwandan society – either as an employee or through self-employment. This was achieved by providing them with market-relevant life and work-readiness training and support, hands-on training opportunities, and internships. The Akazi Kanoze project provided education and training in 19 districts to over 21,000 Rwandan youths, 45% of whom resided in rural areas. A small sub-sample of these youths participated in the current study (see below).

The project’s theory of change emphasizes both technical and entrepreneurship training as well as the development of soft skills. The aim is to increase “work-readiness and employability,” where employability is defined as “a set of achievements, skills, understandings and personal attributes that make a person more likely to gain and retain employment, be successful in their chosen occupation and be professionally and personally satisfied with their employment” (Knight and Yorke 2002). The theory of change is that

\textsuperscript{4} The Rwandan “Workforce Development Authority (WDA)” works under the Ministry of Education (MINEDUC) and has a mandate to support entrepreneurship in TVETs in Rwanda.
participating in the training helps participants to build technical and soft skills, and that mentoring and internships help to build a professional network. This should translate into higher productivity, improved entrepreneurship, a greater chance of obtaining and keeping paid jobs, higher incomes, and eventually improved livelihoods. While not an explicit goal of the intervention, it is interesting to explore whether these projected effects also help participants to transform into respectable and honest citizens – contributing to social stability (for further information see Appendix 2).

A standard AK training consists of the following elements: (i) two months of Work Readiness and Entrepreneurship Curriculum covering topics such as personal awareness, communication, professional conduct, financial literacy, personal health, rights and responsibilities, and entrepreneurship; (ii) three months of in-class lessons and technical training focusing on income-generating activities such as hair dressing, hospitality, masonry, tailoring, carpentry and welding; (iii) a three-month internship, providing participants with on-the-job experience and in some cases leading to full-time employment. Local organizations were responsible for implementing the following activities with capacity building and oversight from EDC. Parts of the training component were offered as a full-time activity, and others as a half-day activity. Internships were full-time, and participants received compensation for travel costs, if necessary. On average, the full costs of the training amounted to $400-450 USD per participant.

3. Measurement, data and identification

We use a field experiment in two districts in Southern Province (Huye) and Western Province (Nyamasheke), to evaluate the impact of the AK training. Implementing organizations over-recruited interested youth meeting the demographic criteria of the project, with help of community leaders in identifying vulnerable out-of-school youths with basic literacy skills.
Consistent with standard practice, field officers interviewed interested youths individually to administer a standardized rapid literacy assessment to ensure they had at least 4th grade reading level. From the final list of 600 interested eligible youths, participants were randomly assigned to treatment and control groups and participated in a short baseline survey (September 2013). Baseline data are available for 590 individuals. This study compares respondents participating in the training with respondents that did not participate. The intervention took place between September 2013 and May 2014. After the intervention we organized three follow-up data collection waves.

- Six months after the intervention finished, in October 2014, 468 participants were surveyed for the analysis of short-term effects. Considerable attrition is of course not uncommon in studies working with underemployed youths, who are spatially mobile and move around searching for opportunities to work.
- The end-line survey for the analysis of medium-term effects was conducted in June 2016, when 449 youths were surveyed (this is slightly more than two years after the intervention).
- Follow-up lab-in-the field games were organized in December 2016 and January 2017 to measure certain preferences and beliefs in an incentive compatible fashion. For this final round of data collection we managed to track 455 participants. We allocated extra effort to retracing individuals from the control group, as these were somewhat under-represented in the follow-up survey data collection waves (see below).

The survey instrument for the first follow-up survey wave was created to evaluate livelihood outcomes, including wage labor acquisition and self-employment. The tool consists of three parts, collecting information on: (i) employment and income; (ii) financial management; and (iii) work readiness and business skills. This survey tool was supplemented
at end-line with additional variables measuring business attitudes and behavioral change, networking, and anti-social behavior.

3.1 Dependent variables

We distinguish between three ‘types’ of dependent variables: outcomes, impacts and ‘deep impacts.’ Outcomes are produced by a combination of the efforts of the implementing agency and the participants. Examples include changes in knowledge and (employment) attitudes caused by the training – requiring input or effort by both the trainer and the trainee. Impacts capture effects on the livelihoods of participants, such as employment and income effects. Finally, ‘deep impacts’ pick up the combined effects of outcomes and impacts on the beliefs, preferences and social behavior of participants. We focus on variables associated with (anti)social behavior and citizenship. We collected data across many different dimensions, and to avoid overburdening respondents we spread different questions over different survey waves. Hence, while we collected repeated data for some variables across rounds (e.g. income and employment status), other questions were asked only once to respondents. For example, we only measured ‘deep impacts’ at the end-line and during the experimental games – not at the baseline or the first follow-up. Much of the analysis below is therefore based on cross-section estimators.

Many of our outcome variables are index numbers, aggregating the information of different individual survey questions. All indexes are standardized, using the mean and standard deviation of the control group, so the index for the control group has mean zero and a standard deviation of one. Details of the survey questions and indices are provided in Appendix 1. First consider ‘outcomes.’ Our Work Readiness index seeks to gauge whether respondents possess sufficient knowledge to apply for a job or start a business. We use a simple index at baseline and the first follow-up (5 self-reported knowledge questions), and a slightly more elaborate
one at the end-line (10 knowledge questions). For the end-line we also use a validated *Locus of Control* index, based on 21 questions that measure whether the respondent believes he is able to control his own (economic) fate. Our *Business Attitude index*, which is based on 7 questions, represents attitudes towards entrepreneurship and owning a business. To approximate the breadth and depth of someone’s professional network we asked 6 questions about contacts that may be instrumental in the acquisition of information or attracting customers, etc. These questions are aggregated in our *Networking index*.

Next, consider the additional variables we use to measure the ‘impact’ of the training. We include employment status, which is comprised of formal and non-formal work for wages. Categories of employment include: working for a family member, non-family member, a cooperative, or being self-employed. These variables are combined in the dummy *Employed*, indicating whether the respondent is employment in at least one of these categories. As auxiliary impact measures we have collected data on income, consumption and savings levels.

Finally, to measure ‘deep impacts’ we asked 16 questions about the frequency of engaging in socially undesirable behavior (refusing to repay debts, drinking, fighting, use of drugs etc.). The information collected in these questions was collapsed in one *Anti-Social behavior index*. We also use two measures based on the play of respondents in lab-in-the-field games as additional ‘deep impact variables.’ First, we played a simple cheating game where respondents were asked to roll a die ten times in private, and report the number of times a five or six came up. For every five or six reported, respondents received a monetary reward (Rwf 100). We did not check the number of fives or sixes actually thrown, so we cannot verify whether individual respondents are reporting outcomes truthfully, or not. However, as the number of die rolls increases, the share of fives and sixes should approach 33%. For subsamples of treated and control respondents, therefore, we can draw conclusions about the prevalence of cheating.
also played a common pool game where subjects decided how much to withdraw from a common pot that was shared with three other respondents. The Nash prediction is to withdraw the maximum amount, and the social optimum is to extract nothing and share the surplus. This variable measures the propensity to cooperate, but also picks up trust and a preference for efficiency. Game protocols are included in Appendix 2.

3.2 Balance test at baseline

We now examine whether random assignment of respondents to treatment and control arms succeeded in creating two comparable groups. Table 1 summarizes key baseline variables for the two groups separately, and reports the differences in sample means as well as the p-values associated with these differences. We also report how many respondents returned in subsequent data collection rounds. The list of variables includes (potential) controls as well as baseline values of dependent variables that will be used later. We evaluate basic individual characteristics like age and gender, and several variables measuring asset ownership at baseline (land and livestock). We also consider potential outcome variables of interest like having a mentor, the average number of customers per month, customer care variables (offering packaging and discounts), employment variables (self-employed, work for money, unemployed), earnings, and savings. As is evident; across nearly all dimensions we find that, on average, the groups are not statistically different at baseline. The exceptions are whether or not the respondent owns livestock and whether she works for a cooperative.6

5 Our partner NGO (Akazi Kanoze) did not collect standard individual characteristics like marital status, family size, and schooling at baseline. We collected these data later, but do not include these variables in Table 1 as their values may have changed between the baseline and subsequent waves (and these changes may be correlated with treatment status). Nevertheless, when considering these “additional demographics” we also do not find significant differences between treatment and control groups (not shown).

6 The analysis is based on actual treatment status and not on assignment to treatment as the randomized treatment assignment file unfortunately was lost. However, it is highly unlikely that subjects from the control group attended the training as this was monitored and enforced. Some non-compliance by subjects invited to the training did occur. By comparing the number of subjects assigned to control and the number of subjects not partaking in the training, we know that between 14 and 24 subjects invited to the training did not actually attend it. Because we
3.3 Main sample

Because the number of respondents varies across data collection waves, we had to decide about the sample frame for which to report regression results. We use a two-pronged approach. First, the main sample that we consistently use for all models is based on the sub-sample of 357 respondents that appeared across all four data collection waves. The final column of Table 1 reports the \( p \)-values for differences between treated and control households for this main sample at baseline, and we now find that all variables are “balanced.” Since many available observations are dropped for specific models when using the main sample, we lose statistical power. Second, to assess the extent to which this is a problem we re-estimate all models using the largest sample for each variable (see below), so that sample size for the Largest Sample (LS) estimates varies across models.

3.4 Identification strategy

We estimate a series of Ordinary Least Squares (OLS) models for our continuous dependent variables—regressing them on a training dummy and baseline controls. Specifically, we estimate the following model to gauge short-term effects (based on data from the first follow-up survey) and medium-term effects (based on end-line variables):

\[
y_{it} = \alpha + \beta T_i + \delta x_i + u_{it}, \tag{1}
\]

where \( y_{it} \) denotes an outcome variable for respondent \( i \), \( T_i \) is a dummy variable equal to 1 if the respondent received the training, \( x_i \) is a vector of covariates measured at baseline, and \( u_{it} \) is an
error term (we however report robust standard errors). Vector $x_i$ also includes dummies capturing the identity and location of the implementing NGO. Coefficient $\beta$ is our coefficient of interest, measuring the average difference in value of the dependent variable between subjects from the treated and comparison group. We use robust standard errors, and for dependent variables derived from lab-in-the-field games we cluster standard errors at the level of the experimental session.

To analyze gender differential effects, we add an interaction term to the model:

$$ y_{it} = \alpha + \beta T_i + \gamma T_i F_i + \delta x_i + \omega_{it}, \quad (2) $$

where $F_i$ is a dummy variable equal to 1 if the respondent is female. This dummy is also included in vector $x_i$.

For several dependent variables, but not all, we also have access to baseline values $y_{i0}$. For these dependent variables we include baseline variables in vector $x_i$, and effectively estimate an Analysis of Covariance (ANCOVA) specification. As some of our dependent variables are of a binary nature (such as employment status), we also estimate non-linear probit models to explain variation in such variables as a robustness check.

As we test multiple hypotheses and accept a type I error of $\alpha = 0.05$ for each individual hypothesis, we risk rejecting true null hypotheses. We correct for multiple hypothesis testing in two ways. First, as mentioned, within each wave and category of variables (outcomes, impacts and deep impacts), we create a standardized summary index of the dependent variables within this wave-category pair. This allows us to focus on the treatment effects on indexes, greatly reducing the number of hypothesis considered (and the probability of type II errors). When some, but not all, values were missing for a respondent, we estimated these missing
values by the mean of observed values of other respondents with the same treatment status before aggregating in the standardized summary index.

Second, the use of indexes may obscure the impacts on specific variables, perhaps the variables we are most interested in (such as employment status). Lumping variables together is therefore somewhat uninformative. For that reason we also report the impact of the training on various individual variables. To attenuate concerns about multiple hypotheses testing, we calculate family-wise error rate (FWER) adjusted p-values using the Westfall and Young (1993) free step-down resampling method as outlined by Anderson (2008) with 10,000 replications for individual variables. Inference based on FWER-adjusted p-values limit the probability of rejecting at least one true null hypothesis of a family of hypotheses to a specific significance level. When we choose as family the null hypotheses within a wave-category pair, this is an alternative for creating a standardized category index that allows evaluating the whole subgroup of hypotheses.

It is an open question whether the FWER adjusted p-values should be preferred or the unadjusted p-values so we report both for completeness. Standard individual p-values are most relevant if one aims to consider impacts on specific (index) variables, especially if these variables follow naturally from the theory of change of the implementing agency (and are not the result of *ex post* cherry picking). In contrast, the more conservative FWER-adjusted p-values and the p-value for the consolidated index are more relevant if one is interested in picking up over-all effects of the intervention across a range of potential variables.

### 3.5 Attrition

Table 1, rows 2-5, shows that significantly fewer representatives of the control group participated in follow up survey waves (and, hence, in the main sample). This was remedied in
the game round as we allocated extra time and effort to retrieve members of the control group. However, our survey-based results may suffer from non-random attrition, which may bias our estimates of impact. The abovementioned result that for the main (post-attrition) sample all variables are balanced provides some early evidence that attrition does not invalidate the randomization. To further assess the severity of the differential attrition problem, we first check whether attrition is related to baseline characteristics. More specifically we estimate the following nonlinear least squares model:

\[ Main_i = \alpha_T(\beta x_i) + \gamma x_i T_i + \varepsilon_i \]  

(3)

where \( Main_i \) is a dummy variable indicating that the individual has participated in all four data collection waves, \( T_i \) is a dummy variable equal to 1 if the respondent received the training, \( x_i \) is a vector of \( j \) covariates measured at baseline, and \( \varepsilon_i \) is an error term. We test whether \( \gamma = 0 \) and, comfortingly, find no indication for differential attrition (LR = 17.84, \( p = 0.3334 \)).

Given that the randomization worked well and that differential attrition is not related to baseline characteristics, we expect no structural differences in baseline characteristics across treatment status for individuals from the main sample. This was also evident from the final column of Table 1.

Further, we check whether respondents that are present in one follow-up wave, but absent in at least one other follow-up wave, are systematically different in outcomes/impacts from respondents present in all samples. We do this by adding the dummy \( Main_i \) as independent variable to our main specification (2) above, and use the largest subsample for the estimation. Respondents missing in some waves only differ significantly from the main sample for one outcome variable; the Locus of Control. Given the number of outcome and impact variables considered and the probability of Type I errors tolerated (\( \alpha = 0.05 \)), we conclude that there is
no evidence of systematic differences in outcomes/impacts across surveyed and non-surveyed respondents in any wave. (Details available on request.)

As mentioned above, we also vary the sample frame across models and re-estimate all models using the “largest sample” that can support a specific analysis. This implies adding all the observations that are dropped from the main sample because they were missing in any of the data collection waves.\(^7\) Results are presented in the bottom panels of the tables (see rows “Treatment Status LS”). As will become evident, the results for the “largest sample” are very similar to the ones reported below – coefficients hardly change and standard errors are slightly smaller (but not to the extent that they warrant a change in interpretation of the results). The similarity of the results across samples provides further indication that biased attrition does not invalidate our analysis.

4. Estimation results

In this section we report short-term and medium-term impacts of the human capital intervention. We will structure our discussion of the findings in accordance with the theory of change of EDC, and distinguish between outcomes, impacts and deep impacts.

a. Short-term effects

Data collection in 2014 focused on employability, work readiness and employment status. There was no effort to collect data on civic or anti-social behavior, so we only have data for several outcome and impact measures (and not for deep impact variables). Table 2 summarizes our results for short-term results. As outcome variables we use a measure indicating whether the subject has a mentor, and the work readiness index. These separate variables have been

\(^7\) Observe that, since there is no differential attrition in the game wave, this solves the differential attrition problem for all variables based on data collected in this round.
collapsed into a single outcome index as well. The results indicate that, as expected, the training had a positive effect on outcomes in the short term: respondents self-report to be better able and equipped to look for a job, and are more likely to have a mentor to assist them (marginally significant).

<< Insert Table 2 about here >>

In light of these positive outcomes, it is perhaps not surprising that impacts also tend to be positive. This follows from the other columns in Table 2. According to the (perhaps naïve) estimation results where we do not control for multiple hypotheses testing, trained individuals are more likely to be self-employed or work elsewhere (for wages or in a cooperative). They are less likely to be employed by a family member, which probably reflects crowding-out of underemployment within kin networks by more highly-valued employment elsewhere. The net effect is that the employment status has improved, and this result survives adjustments for multiple hypothesis testing, using the largest sample. The coefficients associated with the income and savings variables are both positive but imprecisely estimated. The income variable is therefore not significant.

It is important to place these results in perspective. The intervention took place during a (global) economic downturn, and employment conditions throughout the country deteriorated. In our main sample, the share of subjects that was actually employed declined from 65% at baseline to 58% during the first follow-up. Our results imply that while employment remained roughly constant among treated subjects, employment dropped significantly for the control group. This suggests that the training helped Rwandan youths to obtain new jobs, or retain existing ones, in times of economic crisis.

**4.2 Medium-term effects**
While the short-term effects are encouraging, the ambitions of the Akazi Kanoze training extend further. The objective is to provide youths with the tools and capabilities to improve their livelihoods in the long term, and therefore it is important to evaluate whether positive effects can be sustained over a period of approximately two years. Moreover, we wish to evaluate whether effects of the training extend beyond labor market outcomes.

<< Insert Table 3 here >>

In terms of outcomes we now have access to a broader set of variables than before. Table 3 summarizes the outcome results for the main sample of respondents. These are weaker than before. Specifically, while we still find significant effects for Work Readiness and Networking, none of the other outcome variables is significantly different across experimental arms. After correcting for multiple hypotheses testing, the Work Readiness variable also becomes insignificant. Even variables that relate directly to the theory of change of the implementing agency, such as business attitudes or the locus of control, have not significantly improved when measured two years after the training.

Compared to the short-term impacts, the impact results summarized in Table 4 are also weaker. Importantly, all effects are “of the right sign” and we still document a positive effect on the economic impact index. But many of the underlying variables are no longer significant. Specifically, while we continue to find an overall effect on employment status in column 2, the value of the coefficient is smaller than before and the effect on employment appears to be driven by self-employment.\(^8\) Both effects do not survive the correction for multiple hypotheses testing, but the effect for employment does marginally survive adjustments for multiple hypothesis testing for the largest sample, where we have more statistical power. The effects on

\(^8\) Half a year later, we again asked for the employment status during the game wave. The coefficient for this variable is even smaller and insignificant.
employment working for others and cooperatives have disappeared, and it seems as if a certain fraction of these former employees have again started working for family members. We do not find significant effects on income, consumption or savings levels. We still pick up a marginal significant effect on the standardized summary index, as this index reduces “noise” by taking the mean of standardized individual variables.

<< Insert Table 4 here >>

Finally, Table 5 summarizes our medium-term “deep impacts.” These are decidedly mixed. While trained individuals report to engage less in anti-social behavior, on average they are more likely to lie about the number of fives and sixes thrown. The number of fives and sixes reported by respondents from the control group is significantly lower than that of the treatment group. Observe that an increased propensity to lie might also (partly) explain the decrease in (self-reported) anti-social behavior. Another explanation might be potential experimenter demand effects as trained individuals presumably “know better” which behaviors are socially desirable, and which ones are not (through the soft skills part of the training). We do not find that trained individuals behave differently from their peers in the control group when playing a common pool game – extraction levels are statistically indistinguishable. Aggregating across the individual variables into one social impacts index reveals that the training does not help to convert subjects into more civic-minded individuals.

<< Insert Table 5 here >>

4.3 Heterogeneity by gender

We do not find significant differences between males and females, except for two variables. First, we find a positive income effect for females at endline ($\beta = -5288.586, p = 0.279; \gamma = 22467.179, p = 0.024$). Secondly, we find that the lying effect in the die game is mainly driven
by males ($\beta = 0.956, p = 0.001; \gamma = -0.883, p = 0.025$). However, these gender differences are not significant in the standardized summary indexes and also do not survive $p$-value adjustments for multiple hypothesis testing. Overall, we do not find strong evidence that the impact of the training is mediated by gender.

As a final robustness analysis we have estimated probit models for binary dependent variables (not reported). These models show similar results as the linear models discussed above, although $p$-values tend to be somewhat smaller in probit models. Using a probit model, we find a marginally significant treatment effect on employment measured during the game wave ($p = 0.097$).

5. Discussion and conclusions

We evaluate the effects of an ambitious and broad entrepreneurship training in rural Rwanda, distinguishing between more or less direct “outcomes” of the training and more distant and elusive impacts and deep impacts. We measure impacts shortly after the training (6 months) and after a period of two years has elapsed. The training intervention we consider combines different elements. First, an entrepreneurship component, providing trainees with knowledge about starting up and running their own business. Second, a technical (and vocational) training component, focusing on skills transfer. And third, a “soft skills” component, aiming to promote employability or work readiness. The training also involves an extensive internship, linking subjects to the labor market.

Human capital interventions have become tremendously popular in recent years, but concerns have been voiced that youths are trained to seek jobs that are not available. Confronted with a fledgling manufacturing sector that in many countries is not competitive on international markets and a burgeoning population, many African governments seek to curb
underemployment and invest in social stability by promoting self-employment and the start-up of microenterprises. Youths who dropped out of school are a particularly relevant target group for many governments since they are perceived to be “at risk” of engaging in anti-social and potentially destabilizing behavior. According to one estimate the annual “investment” in vocational and technical trainings reaches a billion dollars per year.

What do such investments buy? We know disappointingly little about this issue in the context of developing countries and particularly fragile states. There have been few rigorous analyses based on a comparison of treated individuals and a credible counterfactual. The results that are available paint a mixed picture. Generally speaking, the impact of technical training interventions has been modest – especially for men. The effect of entrepreneurship trainings is also mixed but slightly more positive, and as yet very little is known about investments in soft skills and cognitive therapy. The early evidence on human capital interventions provides hardly cause for celebration.

It is important to expand this body of literature to guide priority setting in future policy making. Our field experiment in Rwanda brings new evidence to the fore. As is well-known, the situation in Rwanda is a-typical because of the very high population density and extensive involvement of a relatively potent central government. We believe the case of a (state-endorsed) human capital intervention in the context of the rapidly growing and urbanizing Rwandan economy provides a setting where trainings have the best chance to succeed.

Our short-term findings are encouraging. We detect a significant increase in work readiness and networking, and we also pick up an improvement of the employment status of the treated. But not all of these effects are not sustained in the longer term. Our medium-term findings are rather sobering, revealing regression to the mean. In terms of outcomes, while trained individuals continue to have an edge over their peers from the comparison group in terms of
work readiness and networking, the difference between treated and control is smaller than before. We still find that trained individuals are more likely to be employed, but the difference between treated and control has shrunk and seems to be exclusively driven by increased self-employment. This supports the notion that preparing youths for (non-existing) jobs is difficult, and that promoting entrepreneurship and self-employment may hold more promise. The risk of such a strategy may be the development of a “flea-market economy” where potential economies of scale are under-exploited. In terms of rural livelihoods, the differences in income, consumption or savings between treated and untreated are too small to be statistically significant.

Our findings suggest it is sensible to revisit subjects in RCTs as the measured impacts of interventions may evolve markedly over time. Our findings echo those of Blattman et al. (2018) who also find that rather positive short-term effects of employment interventions (in their case in the form of cash grants and random job offerings in the manufacturing sector in Ethiopia) taper off over time and eventually dissipate completely.

This analysis does not provide the final word on the impact of human capital interventions – presumably not even for the case of Rwanda. Our sample is relatively small and the analysis suffers from attrition. Additional evidence is necessary, and especially the evaluation of elusive “deep impacts” would benefit from much larger samples. Nevertheless, our findings are consistent with evidence from other supply-side interventions aimed at promoting entrepreneurship via the accumulation of human capital. It appears as if the impacts of human capital interventions in developing countries is modest, and perhaps such interventions should be augmented with additional components such as physical or financial capital injections to have transformative impacts. It also remains worthwhile to explore how the establishment of small and medium sized enterprises can be promoted—increasing demand for (skilled) labor.
References


McKenzie, D. & Woodruff, C. What are we learning from business training and entrepreneurship evaluations around the developing world? *World Bank Research Observer 29*(1), 48-82

MIFOTRA, R. (2014). *Design of Five-Year National Employment Programme (NEP) for Rwanda.* Retrieved from Kigali, Rwanda:


Table 1: Balance at Baseline

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<th></th>
<th>Treated</th>
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<th>Control</th>
<th></th>
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<th>Diff</th>
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<th>p</th>
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<td></td>
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<td>mean</td>
<td>sd</td>
<td>N</td>
<td>mean</td>
<td>sd</td>
<td>p</td>
<td>p</td>
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<td>314</td>
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<td>(0.00)</td>
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<td>(0.46)</td>
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<td>0.76</td>
<td>(0.43)</td>
<td>0.414</td>
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<td>(0.50)</td>
<td>0.000</td>
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</tr>
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<td>(0.50)</td>
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<td>(0.46)</td>
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<td>0.497</td>
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<td>(0.48)</td>
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<td>(0.47)</td>
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<td>(0.34)</td>
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<td>0.296</td>
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<td>(0.43)</td>
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<td>(0.42)</td>
<td>0.702</td>
<td>0.732</td>
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<td>(0.40)</td>
<td>314</td>
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<td>(0.34)</td>
<td>0.057</td>
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<td>0.993</td>
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<td>(0.49)</td>
<td>0.352</td>
<td>0.620</td>
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<td>Income from self/fam/coop</td>
<td>224</td>
<td>292.73</td>
<td>(576.09)</td>
<td>271</td>
<td>242.66</td>
<td>(537.05)</td>
<td>0.322</td>
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<td>Income from other</td>
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<td>(261.59)</td>
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<td>(653.21)</td>
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<td>Has land access</td>
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<td>0.42</td>
<td>(0.49)</td>
<td>314</td>
<td>0.30</td>
<td>(0.46)</td>
<td>0.004</td>
<td>0.227</td>
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<td>Has savings</td>
<td>257</td>
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<td>(0.50)</td>
<td>284</td>
<td>0.51</td>
<td>(0.50)</td>
<td>0.291</td>
<td>0.572</td>
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<td>10234.60</td>
<td>(30883.48)</td>
<td>274</td>
<td>7034.58</td>
<td>(15530.85)</td>
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<td>0.273</td>
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<td>Workreadiness index (baseline/midline)</td>
<td>208</td>
<td>0.02</td>
<td>(1.01)</td>
<td>227</td>
<td>0.00</td>
<td>(1.00)</td>
<td>0.807</td>
<td>0.894</td>
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<tr>
<td>Has skills to find a job</td>
<td>249</td>
<td>3.09</td>
<td>(0.83)</td>
<td>273</td>
<td>3.08</td>
<td>(0.85)</td>
<td>0.877</td>
<td>0.230</td>
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<tr>
<td>Knows how to apply for jobs or get promoted</td>
<td>246</td>
<td>2.13</td>
<td>(0.90)</td>
<td>261</td>
<td>2.08</td>
<td>(0.84)</td>
<td>0.555</td>
<td>0.640</td>
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<tr>
<td>Has skills to start and grow a successful business</td>
<td>236</td>
<td>2.66</td>
<td>(0.93)</td>
<td>269</td>
<td>2.60</td>
<td>(0.94)</td>
<td>0.485</td>
<td>0.443</td>
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<tr>
<td>Knows about formal elements of a business plan</td>
<td>244</td>
<td>1.83</td>
<td>(0.86)</td>
<td>268</td>
<td>1.76</td>
<td>(0.79)</td>
<td>0.336</td>
<td>0.528</td>
</tr>
<tr>
<td>Knows about marketing and how to attract customers</td>
<td>240</td>
<td>2.14</td>
<td>(0.95)</td>
<td>268</td>
<td>2.25</td>
<td>(0.95)</td>
<td>0.198</td>
<td>0.294</td>
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<td>Has a mentor</td>
<td>228</td>
<td>0.27</td>
<td>(0.45)</td>
<td>266</td>
<td>0.33</td>
<td>(0.47)</td>
<td>0.155</td>
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<td>Attract customers index</td>
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<td>(1.26)</td>
<td>94</td>
<td>-0.03</td>
<td>(1.09)</td>
<td>0.398</td>
<td>0.216</td>
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Counts, means, standard deviations and the first p-value are calculated for the baseline sample. The second p-value is calculated for the main sample.
Table 2: Short-term Outcomes and Impacts

<table>
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<th>Dependent variables</th>
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<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
<th>(5)</th>
<th>(6)</th>
<th>(7)</th>
<th>(8)</th>
<th>(9)</th>
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<td>Outcome index</td>
<td>0.408</td>
<td>0.100</td>
<td>0.440</td>
<td>0.321</td>
<td>0.176</td>
<td>-0.037</td>
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<td>0.031</td>
<td>0.086</td>
<td>87.958</td>
<td>10120.857</td>
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<td>Has a mentor</td>
<td>0.104</td>
<td>0.053</td>
<td>0.109</td>
<td>0.106</td>
<td>0.054</td>
<td>0.020</td>
<td>0.050</td>
<td>0.013</td>
<td>0.048</td>
<td>156.617</td>
<td>4665.792</td>
</tr>
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<td>0.060*</td>
<td>0.000***</td>
<td>0.033***</td>
<td>0.001***</td>
<td>0.061*</td>
<td>0.044**</td>
<td>0.021**</td>
<td>0.072*</td>
<td>0.575</td>
<td>0.031**</td>
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<td>Economic impact index</td>
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<td>0.000***</td>
<td>-</td>
<td>0.005***</td>
<td>0.132</td>
<td>0.132</td>
<td>0.115</td>
<td>0.132</td>
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<td>0.132</td>
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<td>356</td>
<td>352</td>
<td>357</td>
<td>357</td>
<td>357</td>
<td>357</td>
<td>357</td>
<td>357</td>
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<td>356</td>
</tr>
<tr>
<td>You work for your family (with payment)</td>
<td>0.119</td>
<td>0.081</td>
<td>0.110</td>
<td>0.186</td>
<td>0.148</td>
<td>0.077</td>
<td>0.125</td>
<td>0.095</td>
<td>0.113</td>
<td>0.083</td>
<td>0.163</td>
</tr>
<tr>
<td>You work yourself (self-employment)</td>
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<td>0.624</td>
<td>0.029</td>
<td>0.000</td>
<td>0.513</td>
<td>0.044</td>
<td>0.297</td>
<td>0.000</td>
<td>0.203</td>
<td>602.833</td>
<td>18792.025</td>
</tr>
<tr>
<td>You work with others in a cooperative (joint)</td>
<td>0.438</td>
<td>0.098</td>
<td>0.492</td>
<td>0.329</td>
<td>0.193</td>
<td>-0.026</td>
<td>0.100</td>
<td>0.029</td>
<td>0.099</td>
<td>130.651</td>
<td>10107.363</td>
</tr>
<tr>
<td>You work for someone not in your family (as casual or</td>
<td>0.000**</td>
<td>0.032**</td>
<td>0.000***</td>
<td>-</td>
<td>0.000***</td>
<td>0.000***</td>
<td>0.000***</td>
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<td>0.044**</td>
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<td>0.024**</td>
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<td>466</td>
<td>462</td>
<td>468</td>
<td>468</td>
<td>468</td>
<td>468</td>
<td>468</td>
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<td>465</td>
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<tr>
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<td>0.081</td>
<td>0.110</td>
<td>0.186</td>
<td>0.148</td>
<td>0.077</td>
<td>0.125</td>
<td>0.095</td>
<td>0.113</td>
<td>0.083</td>
<td>0.163</td>
</tr>
<tr>
<td>Mean of control group</td>
<td>0.000</td>
<td>0.624</td>
<td>0.029</td>
<td>0.000</td>
<td>0.513</td>
<td>0.044</td>
<td>0.297</td>
<td>0.000</td>
<td>0.203</td>
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<td>18792.025</td>
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<td>0.492</td>
<td>0.329</td>
<td>0.193</td>
<td>-0.026</td>
<td>0.100</td>
<td>0.029</td>
<td>0.099</td>
<td>130.651</td>
<td>10107.363</td>
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<tr>
<td>(0.091)</td>
<td>(0.046)</td>
<td>(0.093)</td>
<td>(0.082)</td>
<td>(0.046)</td>
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<td>yes</td>
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Top panel shows main sample results, bottom panel shows largest sample results
Robust standard errors in parentheses
Standard p-values listed first, FWER-adjusted p-values (calculated over columns 2-3 and 5-11, respectively) listed second
Outcome index is standardized index of the variables in columns 2 and 3
Economic impact index is standardized index of the variables in columns 5, 10 and 11
Employed is a dummy indicating whether the respondent has employment in the categories of columns 6-9
*p<0.10, **p<0.05, ***p<0.01
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<th>Has a mentor</th>
<th>Work readiness index</th>
<th>Locus of Control index</th>
<th>Business attitude index</th>
<th>Networking index</th>
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<td>0.209</td>
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</tr>
<tr>
<td>(0.110)</td>
<td>(0.083)</td>
<td>(0.104)</td>
<td>(0.112)</td>
<td>(0.118)</td>
<td>(0.109)</td>
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<td>0.185</td>
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<td>0.185</td>
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<td>(0.074)</td>
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</table>

Top panel shows main sample results, bottom panel shows largest sample results.
Robust standard errors in parentheses.
Standard p-values listed first, FWER-adjusted p-values (calculated over columns 2-6) listed second.
Outcome index is standardized index of variables in columns 2-6.
* p<0.10, ** p<0.05, *** p<0.01
<table>
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<th>(1) Economic impact index</th>
<th>(2) Employed</th>
<th>(3) You work for your family (with payment)</th>
<th>(4) You work yourself (self-employment)</th>
<th>(5) You work with others in a cooperative (joint)</th>
<th>(6) You work for someone not in your family (as casual or game wave)</th>
<th>(7) Total income</th>
<th>(8) Total consumption</th>
<th>(9) Total savings accumulated</th>
<th>(10) Employed (game wave)</th>
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<td>(0.050)</td>
<td>(0.025)</td>
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<td>0.029**</td>
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Top panel shows main sample results, bottom panel shows largest sample results.
Robust standard errors in parentheses.
Standard p-values listed first, FWER-adjusted p-values (calculated over columns 2-10) listed second.
Economic index is standardized index of variables in columns 2 and 7-10.
Employed is a dummy indicating whether the respondent has employment in the categories of columns 6-9.
*p<0.10, ** p<0.05, *** p<0.01
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<th>Anti-social behaviour index</th>
<th>Number of 5 or 6 obtained rolling a dice 10 times</th>
<th>Amount withdrawn from the shared</th>
<th>(1)</th>
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<th>(3)</th>
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<td>0.106</td>
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<td>(0.094)</td>
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<td>0.092</td>
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</table>

Top panel shows main sample results, bottom panel shows largest sample results
Robust standard errors in parentheses
Standard errors clustered at session level for column 4
Standard p-values listed first, FWER-adjusted p-values (calculated over columns 2-3) listed second
Social impact index is standardized index over variables in columns 2-4

* p<0.10, ** p<0.05, *** p<0.01
Appendix 1: Definition of Indices

Workreadiness index (baseline and first follow-up survey) is the standardized mean of the following questions:

1. Do you feel you have the skills to find a job/livelihood?
2. Do you know how to apply for jobs or improve your work position/status (or get promoted)?
3. Do you feel you have the skills to start and grow a successful business on your own?
4. Do you know about the formal elements of a business plan?
5. Do you know about marketing and how to attract customers?

These questions are answered at a four-point scale with the answer options labelled as Not at all, A little bit, Yes and Very much.

Note that this is a very different index than the Work readiness index based on endline data.

Work readiness index (endline survey) is standardized mean of the number of correct answers on the following questions, with the correct answer underlined:

1. When dealing with a difficult customer, you: Ignore the person, Remain calm, Tell them you will not talk to them anymore, Tell them to please leave, Yell at them so they understand your point of view
2. Someone who works well in a team: Interrupts only when offering new ideas, Pays attention only to those who have the same opinions, Provides ideas and seeks the ideas of others in the group, Creates conflict to make the discussion interesting, All of the above
3. Good time management involves: Planning, Prioritizing what needs to be done, Avoiding distractions, All of the above, None of the above
4. To be successful, team need: Different goals, Vague roles and tasks, Trusting relationships among team members, All of the above, None of the above
5. Voluntary Counselling and Testing (VCT): Is required by law, Is public information, Includes advice and support given by a qualified person, Tests you on your knowledge of HIV, It does not assure that the process will be confidential
6. In Rwanda, an employer does not need to: Provide regular breaks for employees, Maintain a safe workplace, Pay employees the agreed upon wage on time, Pay for employees' transportation to work, Give the employee the agreed work
7. The Rwandan work week consists of: 35 hours, 50 hours, 45 hours, 40 hours, None of the above
8. As a worker, you should have: One day off per week, 1.5 days paid leave per month, Access to safety equipment if required to do job safely, All of the above, None of the above
9. Which of the following is not a very good use of business money: Buying additional products because you expect an increase in demand, Borrowing from the business to pay for a new television for your family, Repairing a broken light in the production space or retail shop, Hiring an extra person for a short while during a busy period, Giving bonuses to employees who work better than others.

10. Selling on credit is: Never a good idea because people might not pay back, Always a good idea because you can sometimes charge higher prices, Can be a good idea but you should limit how often you sell on credit and do so carefully, Always a good idea because customers pay much attention to prices as a purchase priority, Never a good idea because it never increases revenues.

Note that this is a very different index than the Work readiness indices based on baseline and first follow-up data.

Locus of Control index (endline survey) is standardized mean of the following questions, with the sign indicating whether questions are included positively or negatively in the index:

1. Saving and careful investing is a key factor in becoming rich. (+)
2. Whether or not I become wealthy depends mostly on my ability. (+)
3. People’s poverty results from their own idleness. (+)
4. I feel that my finances are mostly determined by powerful people. (-)
5. There is little I can do to prevent poverty. (-)
6. The seriousness of poverty is overstated. (+)
7. When I get what I want, it is usually because I am lucky. (-)
8. In the long run, people who take care of their finances stay wealthy. (+)
9. Although I might have the ability, I will not become better off without appealing to those in positions of power. (-)
10. Becoming rich has nothing to do with luck. (+)
11. People like myself have little chance in protecting our personal interests when they are in conflict with those of strong pressure groups. (-)
12. Regarding money, there isn’t much you can do for yourself when you are poor. (-)
13. Politicians can do very little to prevent poverty. (+)
14. It’s not always wise for me to save because many things turn out to be a good matter of good fortune of bad fortune. (-)
15. If I become poor, it is usually my own fault. (+)
16. Getting what I want financially requires pleasing those people above me. (-)
17. I am usually able to protect my personal interests. (+)
18. When I get what I want, it is usually because I worked hard for it. (+)
19. My life is determined by my own actions. (+)
20. It is chiefly a matter of fate whether I become rich or poor. (-)
21. Only those who inherit or win money can possibly become rich. (-)
These questions are answered on a 7-point Likert scale with the answer options labeled as Entirely disagree, Mostly disagree, Somewhat disagree, Neither agree nor disagree, Somewhat agree, Mostly agree and Entirely agree.

**Business Attitude index (endline survey)** is standardized mean of the following questions:

1. I always work hard in order to be among the best in my field.
2. I believe that concrete results are necessary in order to judge business success.
3. I get my biggest thrills when my work is among the best.
4. I will make every effort to start and keep my own firm.
5. Starting and managing my own business entails great satisfactions for me.
6. I personally consider entrepreneurship to be a highly desirable career alternative for people with my professional and education background.
7. Overall, I consider starting your own business as the best one can do.

These questions are answered on a 7-point Likert scale with the answer options labeled as Entirely disagree, Mostly disagree, Somewhat disagree, Neither agree nor disagree, Somewhat agree, Mostly agree and Entirely agree.

**Networking index (endline survey)** is standardized mean of the following questions:

1. Do you have people involved in business activities in your area that you go to for help or advice whenever necessary?
2. Do people that you are in contact with in the business help you to find employees to run your business
3. Do people that you are in contact with in the business help you to get knowledge to run your business
4. Do people that you are in contact with in the business help you to find more customers for your business
5. Do people that you are in contact with in the business provide valuable information to run your business
6. Among these people, is there any one of them that you can invite or have invited for celebrations or party?

These questions are answered by Yes or No.

**Anti-social behavior index (endline survey)** is standardized mean of questions to what extend the respondent has been engaged in each of the following behaviors in the past 12 months:
1. Engaged in drinking heavily at a social function.
2. Cheated by a significant amount on your income tax return or any other large payment.
3. Forged somebody’s signature or used fake documents.
4. Used somebody else’s assets without permission.
5. Refused to repay an overdue debt.
6. Failed to attend savings group meeting or any other community meeting three times in a row.
7. Made fun of someone at your workplace.
8. Said something hurtful to someone.
10. Acted rudely towards someone.
11. Publicly embarrassed someone.
12. Felt so sad or hopeless almost every day for two weeks or more in a row that you stopped doing some usual activities.
14. Engaged in consumption of illegal drugs.
15. Was involved in a physical fight.
16. Was involved in a physical fight in which you were injured and had to be treated by a doctor or nurse.

These questions are answered on a 7-point frequency scale with the answer options labeled as Never, Once a year, Twice a year, Several times a year, Monthly, Weekly, Daily.

Appendix 2: Akazi Kanoze intervention description

The Akazi Kanoze Youth Livelihoods Project, financed by the United States Agency for International Development (USAID) and implemented by Education Development Center (EDC) had two result areas that guided implementation:

**Result 1: Increase Livelihood Opportunities for Youth**

- Akazi Kanoze empowers youth with the necessary tools and resources to enter a positive development pathway that leads to increased livelihood opportunities;

**Result 2: Develop a Thriving Youth Livelihood Support System**

- Akazi Kanoze builds capacity and creates linkages between youth, the Rwandan economy, and public and private institutions so youth gain access to increased opportunities to engage productively in Rwandan society. Akazi Kanoze was designed to enable youth to be more capable of earning a livelihood, through connections to life and work readiness trainings, the development of technical skills,
and linkages to market actors. The following are the core youth trainings and support activities that the treatment group in this study received:

1. **Work Readiness Curriculum**: All youths participated in a work readiness curriculum that included eight modules on personal development, interpersonal communication, work habits and conduct, leadership, safety and health at work, worker and employer rights and responsibilities and financial literacy. This 100-hour curriculum was taught consecutively by certified trainers who had completed a teacher training conducted by EDC master trainers. Depending on youths’ schedules and other responsibilities, trainers would hold class between 4-6 hours per day, Monday through Friday. All youth took a completion exam and passing students were awarded a nationally recognized certificate by the Workforce Development Authority, the arm of the Ministry of Education in charge of vocational and technical education.

2. **Entrepreneurship Curriculum**: This 35-hour curriculum was taught after the foundational Work Readiness Curriculum finished. This training consisted of five modules: 1.) Getting ready for entrepreneurship, 2.) Finding a good business idea, 3.) Establishing a business, 4.) Operating a business and 5.) Developing a business plan.

3. **Complementary Trainings**: Akazi Kanoze trained participants in forming savings groups and encouraged the cohorts to continue saving together. Furthermore, youth received two to three months of technical training in hair dressing, hospitality, masonry, carpentry, welding and tailoring. The technical trainings were conducted by local craftsmen and experts from within the community.

4. **On the job Training**: The local implementing organizations were required to find all participants an internship or apprenticeship, if they had not yet secured their own job or began a business. This on-the-job training provided youths with real world experience and sometimes resulted in full-time employment. Youth who started businesses directly out of training received mentoring and business support from their Akazi Kanoze trainers.

**Appendix 3: Lab in the field game protocols**

**General information and consent form**

- Welcome. We are here to collect data for a study of the University of Rwanda. The study analyses how Rwandese people make decisions. We hope that this research will benefit the people of Rwanda in the future.

- This session consist of:
  - A short survey.
  - A peeling garlics task.
  - Four games.  

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9 This paper only studies the results of two relatively simple games in this paper. The results of two more complicated games will be discussed in another paper.
You will receive Rwf 3,000 to cover your travel costs.
Besides, you will receive Rwf 1,000 as compensation for your time.
In addition, you can earn up to Rwf 11,400 in the games.
In any case, you will earn money in the games.
After finishing the session, we will tell you and pay you the amount you have earned.
Taking part in this study is completely voluntary. No one has to participate.
The researchers will keep your responses confidential and will not inform other respondents about your choices unless we tell you in advance that we do so.
You are not obliged to answer questions if you do not want to and you are free to leave at all times without any penalties.
There will be no negative consequences to anyone if you do not want to participate.
The information we collect will go to a secure locations at the University of Rwanda and Wageningen University. The researchers will use the information we collect in articles that might be published, as well as in academic presentations.
Participation takes around two hours.
Do you have any questions? Please ask the enumerator if you have any questions.
If you have questions later, you may contact researcher Aussi Sayinzoga at XXXXXXXXXX.
We ask you to sign this informed consent form if you have read and understand the above information, your questions have been answered and you agree to participate in this study.

Round 2: Rolling a dice

Instructions

- In this round, you can earn a minimum of Rwf 0 and a maximum of Rwf 2,000.
- We will ask you to roll a dice 10 times.
- On each side, the dice has a number of dots. The number of dots varies from 1 to 6.
- For each roll, we ask you to indicate whether the number of dots equals 5 or 6.
- For each time the number of dots equals 5 or 6, you will earn Rwf 200.
- For each time the number of dots equals 1, 2, 3 or 4, you will earn nothing.
- No one will watch you when you will roll the dice.
- You can roll the dice behind your hand, so that no one else can see.

Example 1

If you roll 3 times a 5 or 6, and another 7 times you roll a 1, 2, 3 or 4, then you will earn Rwf 600.

Example 2
If you roll 5 times a 5 or 6, and another 5 times you roll a 1, 2, 3 or 4, then you will earn Rwf 1,000.

Example 3

If you roll 7 times a 5 or 6, and another 3 times you roll a 1, 2, 3 or 4, then you will earn Rwf 1,400.

➢ Do you have any questions about this round? Please ask the enumerator if you have any questions.

Example 4

Now, we play a last example with you. We will tell you when you can roll the dice. Please fill in the right answers on the questions.

<table>
<thead>
<tr>
<th>2.1</th>
<th>Roll the dice for the first time. Does the number of dots equal 5 or 6?</th>
</tr>
</thead>
<tbody>
<tr>
<td>2.2</td>
<td>Roll the dice for the second time. Does the number of dots equal 5 or 6?</td>
</tr>
<tr>
<td>2.3</td>
<td>Roll the dice for the third time. Does the number of dots equal 5 or 6?</td>
</tr>
<tr>
<td>2.4</td>
<td>Roll the dice for the fourth time. Does the number of dots equal 5 or 6?</td>
</tr>
<tr>
<td>2.5</td>
<td>Roll the dice for the fifth time. Does the number of dots equal 5 or 6?</td>
</tr>
<tr>
<td>2.6</td>
<td>Roll the dice for the sixth time. Does the number of dots equal 5 or 6?</td>
</tr>
<tr>
<td>2.7</td>
<td>Roll the dice for the seventh time. Does the number of dots equal 5 or 6?</td>
</tr>
<tr>
<td>2.8</td>
<td>Roll the dice for the eighth time. Does the number of dots equal 5 or 6?</td>
</tr>
<tr>
<td>------</td>
<td>-------------------------------------------------------------------------</td>
</tr>
<tr>
<td>2.9</td>
<td>Roll the dice for the ninth time. Does the number of dots equal 5 or 6?</td>
</tr>
<tr>
<td>2.10</td>
<td>Roll the dice for the tenth time. Does the number of dots equal 5 or 6?</td>
</tr>
<tr>
<td>2.11</td>
<td>How many times did you answer yes on the above ten questions?</td>
</tr>
<tr>
<td>2.12</td>
<td>How much would you have earned from rolling the dice these ten times?</td>
</tr>
</tbody>
</table>

- An enumerator will check your answer on the last two questions.
- Do you have any questions about this round? Please ask the enumerator if you have any questions.

For real

Now, we play for real money. We will tell you when you can roll the dice. Please fill in the right answers on the questions.

<table>
<thead>
<tr>
<th>2.13</th>
<th>Roll the dice for the first time. Does the number of dots equal 5 or 6?</th>
</tr>
</thead>
<tbody>
<tr>
<td>2.14</td>
<td>Roll the dice for the second time. Does the number of dots equal 5 or 6?</td>
</tr>
<tr>
<td></td>
<td>Roll the dice for the third time. Does the number of dots equal 5 or 6?</td>
</tr>
<tr>
<td>---</td>
<td>-------------------------------------------------------------------</td>
</tr>
<tr>
<td>2.15</td>
<td>Roll the dice for the fourth time. Does the number of dots equal 5 or 6?</td>
</tr>
<tr>
<td>2.16</td>
<td>Roll the dice for the fifth time. Does the number of dots equal 5 or 6?</td>
</tr>
<tr>
<td>2.17</td>
<td>Roll the dice for the sixth time. Does the number of dots equal 5 or 6?</td>
</tr>
<tr>
<td>2.18</td>
<td>Roll the dice for the seventh time. Does the number of dots equal 5 or 6?</td>
</tr>
<tr>
<td>2.19</td>
<td>Roll the dice for the eighth time. Does the number of dots equal 5 or 6?</td>
</tr>
<tr>
<td>2.20</td>
<td>Roll the dice for the ninth time. Does the number of dots equal 5 or 6?</td>
</tr>
<tr>
<td>2.21</td>
<td>Roll the dice for the tenth time. Does the number of dots equal 5 or 6?</td>
</tr>
<tr>
<td>2.22</td>
<td>How many times did you answer yes on the above ten questions?</td>
</tr>
<tr>
<td>2.23</td>
<td>How much do you earn from rolling the dice these ten times?</td>
</tr>
<tr>
<td>2.24</td>
<td>Rwf</td>
</tr>
</tbody>
</table>

➢ An enumerator will check your answer on the last two questions and collect the dices.

➢ You will receive the money you earned at the end of the session.
Round 5: Common pool

Instructions

- In this round, you can earn a minimum of Rwf 400 and a maximum of Rwf 2,000.
- In this round, you are again in a group of four players.
- The group together receives a shared resource of Rwf 3,200.
- We will ask all players individually to choose an amount between Rwf 0 and Rwf 800 that he/she withdraws from the shared resource. When choosing your amount to withdraw, you do not know the amount other players withdraw.
- The remaining shared resource will be doubled and equally shared by all players.
- This implies that if you withdraw more, then you will earn more yourself, but the total earnings of the group will be lower.
- We will show the examples with real money.

Review questions

- What is the size of the shared resource?
- What is the maximum someone can withdraw from the shared resource?
- What will happen after all four players have withdrawn something?

Example 1

<table>
<thead>
<tr>
<th>Suppose:</th>
<th>Then:</th>
<th>So</th>
</tr>
</thead>
<tbody>
<tr>
<td>Player A/E withdraws Rwf 200</td>
<td>The remaining shared resource is Rwf 1,600</td>
<td>Player A/E earns Rwf 1,000</td>
</tr>
<tr>
<td>Player B/F withdraws Rwf 300</td>
<td>Doubling this shared resource gives Rwf 3,200</td>
<td>Player B/F earns Rwf 1,100</td>
</tr>
<tr>
<td>Player C/G withdraws Rwf 500</td>
<td>So all players receive another Rwf 800</td>
<td>Player C/G earns Rwf 1,300</td>
</tr>
<tr>
<td>Player D/H withdraws Rwf 600</td>
<td></td>
<td>Player D/H earns Rwf 1,400</td>
</tr>
</tbody>
</table>

Group total is Rwf 4,800

So the players that withdraw more, earn more, than the players that withdraw less.

Example 2
Suppose:

Player A/E withdraws Rwf 800
Player B/F withdraws Rwf 800
Player C/G withdraws Rwf 800
Player D/H withdraws Rwf 800

Then:

The remaining shared resource is Rwf 0
Doubling this shared resource gives Rwf 0
So all players receive another Rwf 0

So:

Player A/E earns Rwf 800
Player B/F earns Rwf 800
Player C/G earns Rwf 800
Player D/H earns Rwf 800

Group total is Rwf 3,200

So if everybody withdraws Rwf 800, then nothing will be doubled.

Example 3

Suppose:

Player A/E withdraws Rwf 0
Player B/F withdraws Rwf 0
Player C/G withdraws Rwf 0
Player D/H withdraws Rwf 0

Then:

The remaining shared resource is Rwf 3,200
Doubling this shared resource gives Rwf 6,400
So all players receive another Rwf 1,600

So:

Player A/E earns Rwf 1,600
Player B/F earns Rwf 1,600
Player C/G earns Rwf 1,600
Player D/H earns Rwf 1,600

Group total is Rwf 6,400

So if nobody withdraws, then everything will be doubled, so everybody earns more.

The group total is highest if no participant withdraws any money, lower if participants withdraw something and lowest if all participants withdraw Rwf 800.

➢ Do you have any questions about this round? Please ask the enumerator if you have any questions.
For real

Now, we play for real money.

<table>
<thead>
<tr>
<th>Question</th>
<th>Answer</th>
</tr>
</thead>
<tbody>
<tr>
<td>5.1 What amount do you withdraw from the shared resource?</td>
<td>Rwf</td>
</tr>
</tbody>
</table>

- An enumerator will check whether your answer on the last question is an amount between Rwf 0 and Rwf 800.
- We will calculate what you earned and you will receive the money you earned at the end of the session.