

Short- and Medium-term Impacts of Employability Training: Evidence from a Randomised Field Experiment in Rwanda

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Abstract

We use a randomised field experiment to study short-term 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 outcomes of the training on attitudes towards work, employability and labour market outcomes. We also consider unintended social impacts of the training. The outcomes of the intervention are modest. While on the short term the training positively impacted ‘work readiness’ and networking, we do not document significant effects on employment status or income. In the medium term, we do not find any significant effect. Non-compliance and attrition reduced statistical power of our analysis.

Keywords: training intervention, Akazi Kanoze, employability, underemployment

JEL classification: I25, J24, J46, O17

1. Introduction

Interventions to increase human capital are common in low-income countries and have received widespread financial support in recent years—the World Bank alone invests up to one billion dollars per year in training programs (Blattman and Ralston, 2015). The standard training focuses on ‘hard’ skills such as financial literacy, book-keeping, marketing and sales (Bulte *et al.*, 2016). Other programs include vocational training, on-the-job training and more recently several pay attention to investments in non-cognitive life skills or the so-called soft

skills (McKenzie, 2017a). These interventions are more psychology-based and include efforts not only to improve the representation (appearance) and health of participants but also to build character skills such as self-control, sociability and a proactive mindset (Heckman and Kautz, 2013). While a large body of literature examines the impact of ‘hard’ skills human capital interventions (e.g., Blattman and Ralston, 2015 and Kluge *et al.*, 2019), the evidence regarding the impact of trainings combining ‘hard’ and ‘soft’ skills is limited, especially for developing countries.

The objective of this paper is to evaluate the impact of an employability training for underemployed youths that includes emphasis on soft and hard skills, in rural Rwanda. The literature distinguishes between demand-side and supply-side programs. Our study focuses on the supply side. The main contributions are as follows. First, we examine the impact of a program that combines vocational training, soft skills training and an internship. The combination of components makes the program rather expensive. Second, we use a randomised field experiment to evaluate the impacts of participating in the training. Third, we attempt to distinguish between effects on employability, work readiness, employment status and income. However, 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 also evaluate potential unintended social outcomes (e.g., honesty, anti-social behaviour) of the training. Evidence suggests that unemployment has social and moral consequences and may contribute to social instability—perhaps especially in fragile states (see, e.g., Barr *et al.*, 2016 and Bruck *et al.*, 2016).¹ The training could have two effects, working in opposite directions. If it is successful in promoting employment, it could foster respect for other people’s entitlements and citizenship in general. If not, then frustration with the process and the personal costs incurred (e.g., opportunity cost of time) might foster resentment and cause people to behave worse. The impact of the training on anti-social behaviour is therefore an empirical question. The analysis is exploratory as this was not explicitly part of the intervention’s design. Fourth, we focus on the dynamics of impacts by considering both short-term outcomes as well as medium-term impacts. It is not obvious when to measure the effects of the training. Short-term effects may taper off and disappear over time, but it may also take some time before positive treatment effects are observed (e.g., if employment benefits materialise due to reduced churning).

The study suffers from non-compliance and attrition. Nevertheless, we document positive effects of the intervention on several ‘readiness outcomes’ in the short term. However, we do not find significant effects as we try to measure impacts further down the causal chain—employment status, income or savings. Moreover, after 2 years, we do not find any significant effect. Although the statistical power of our analysis is rather low, the analysis rules out that the employment impacts of the program are big—a discouraging result, given the high costs

1 Considering impacts on social instability is also interesting in the light of the growing literature that focuses on the impact of aspirations on behaviour. This literature argues that future-oriented behaviour is affected by the difference between the standard of living that somebody aspires and the current standard of living—the so-called aspirations gap (Ray, 2006). However, if it turns out that the aspirations gap cannot be closed, aspirations failures may result. The intervention we study may increase underemployment by altering participants’ expectations. Participants who may have been satisfied with a particular informal job may become frustrated and be unwilling to continue in that position after undergoing training.

of the program. Moreover, to the extent that subjects in the treatment group displaced ‘other job-seekers’ on the labour market, the social returns of the project are lower than the private returns for the treatment group.

Blattman and Ralston (2015) review the evidence regarding technical and vocational training and conclude it is disappointing.² Across the board, labour market outcomes were small or absent, and dropout rates were high. McKenzie (2017a) reviews nine vocational study interventions and finds that only three had a significant impact on employment (with an average increase in employment of 2.3 percentage points) and two find a statistically significant impact on earnings. Fox and Kaul (2018) argue that an important reason for the lack of success of employment programs is that these programs try to improve employability for jobs that do not exist. Other studies are a bit more positive. Chakravarty *et al.* (2019) show that a skills training in Nepal had positive impacts on self-employment activities of women. Shonchoy *et al.* (2016) find positive impacts if the employment training is combined with a stipend and a 1-month internship.³ Maitra and Mani (2017) study a subsidised vocational training program targeting low-income women in India. They document positive effects on employment and earnings, both in the short term (after 6 months) and medium term (18 months). Since we document zero or very small impacts on employment and income in Rwanda, our findings are consistent with the earlier set of dismal results.

While most evaluations focus on the impact of ‘hard’ skills, evaluations considering programs that combine ‘hard’ and ‘soft’ skills, the aim of our study, are limited. Soft skills and mental abilities are likely important determinants of labour market outcomes. Groh *et al.* (2015) show, using data on Jordan’s labour market for educated youth, that psychometric testing is predictive of subsequent labour market outcomes and facilitates matching on the labour market, reducing labour market frictions by attenuating information asymmetries. Importantly, this study tests personality traits, mental abilities and soft skills but does not seek to improve them through a training. Others have evaluated the impact of soft skills trainings. Campos *et al.* (2017, 2018) compare the impact of a personal initiative training program with traditional business training in Togo. The personal initiative program has significant effects on sales and profits, even after a 2-year period, while the traditional training only affects business practices. Ubfal *et al.* (2019) evaluate a similar program in Jamaica. Although the soft skills training has significant effects on business outcomes for men in the short run, 12 months after the training, there are no significant impacts anymore. These two studies differ from ours as they deal with established microentrepreneurs and do not consider

- 2 See also McKenzie and Woodruff (2014), who discuss the literature evaluating the impact of entrepreneurship trainings and conclude that problematic design of many studies precludes the drawing of strong conclusions.
- 3 Another literature evaluates the impact of capital-centric approaches to employment generation. The marginal return to capital tends to be high in developing countries (De Mel *et al.*, 2008), which has inspired many initiatives involving physical capital injections. These include (unconditional) cash transfers to individuals or groups of people (Blattman *et al.*, 2014, 2016), or the provision of valuable assets such as livestock (e.g., Bandiera *et al.* 2018). Blattman and Ralston (2015) and Filmer *et al.* (2014) review the evidence and conclude there is considerable support for the hypothesis that such interventions ‘work’. However, Blattman *et al.* (2019) are critical about the long-term impacts of a cash grant intervention in Ethiopia—positive impacts of the capital injection (and job offer intervention) had dissipated after 5 years.

employment effects.⁴ Another related study is [Blattman et al. \(2017\)](#), who consider cognitive behavioural therapy among young high-risk urban men in Liberia. 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 behaviour) but has no significant effect on employment prospects.⁵ Our study does not distinguish between the effects of the hard and soft skills components of the training, but the absence of a positive overall effect is consistent with the findings of [Ubfal et al. \(2019\)](#) and [Blattman et al. \(2017\)](#).

The paper is organised as follows. In Section 2, we sketch the Rwandan context and introduce the intervention. Section 3 summarises and scrutinises the data, paying attention to measurement, balance between treatment and comparison group, attrition and non-compliance. It also outlines the identification strategy. Section 4 contains the results, distinguishing between various types of outcomes and short- and medium-term results. The conclusions and discussion ensue.

2. The training intervention in Rwanda

We study the effects of employability training in the context of rural Rwanda. While the Rwandan economy has grown steadily since the late 1990s, it has failed to create sufficient jobs to create employment for the growing number of (increasingly well-educated) Rwandan youths. According to the [Rwanda-African Economic Outlook \(2012\)](#), approximately 200,000 job seekers enter the labour 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 17.9%, and the official unemployment rate among youths is 22.4% ([RLFS, 2020](#)).

Unlike many 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 nationwide—top-down style. To curb concerns about unemployment and underemployment, the government has launched several initiatives. The Rwandan National Employment Policy shows how the government is committed to developing the private sector by creating a favourable 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

- 4 [Steinert et al. \(2018\)](#) examine the impact of a financial literacy training combined with a psychosocial parenting intervention for 552 households in South Africa and show that the combined program has substantial positive effects on financial behaviour. However, this study also does not consider labour market effects.
- 5 [Adoho et al. \(2014\)](#) evaluate bundled interventions for adolescent girls in Liberia incorporating vocational training and other components including business skills training and empowerment activities. While the impact on employment is positive, it requires approximately 12 years to earn back the costs of the job skills training.

and technical and vocational education and training to encourage graduates to generate their own employment instead of being job seekers.⁶

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 and implemented by Education Development Center between October 2009 and September 2016. The project targeted Rwandan ‘youths’, aged between 14 and 35 years, 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 AK project provided education and training in nineteen 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 emphasises both technical (vocational/skills) 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. The underlying idea is that participating in the training helps participants to build technical and soft skills and that mentoring and internships help to build a professional network. We examine whether this translates into higher productivity, improved entrepreneurship, a greater chance of obtaining paid employment, 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.

The following are the core trainings and support activities that the treatment group in this study received.

1. **Work-Readiness Curriculum:** this curriculum includes 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 by certified trainers. Depending on youths’ schedules, trainers would hold class between 4 and 6 hours per day, Monday through Friday. All youths took a completion exam and passing students were awarded a nationally recognised certificate by the Workforce Development Authority, the arm of the Ministry of Education in charge of vocational and technical education.

6 Recent structural reforms in the education sector established the Rwanda Technical and Vocational Education and Training Board whose main mission is to promote the quality of education in technical and vocation education and training from level one (1) to five (5) aimed at fast tracking socio-economic development of the country.

2. Entrepreneurship Curriculum: this 35-hour curriculum was taught after the foundational Work-Readiness Curriculum finished and consisted of five modules: (a) getting ready for entrepreneurship, (b) finding a good business idea, (c) establishing a business, (d) operating a business and (e) developing a business plan.
3. Complementary trainings: AK trained participants in forming savings groups and encouraged the cohorts to continue saving together. Furthermore, youth received 2–3 months of technical training in one of the following activities (chosen by the student): 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 organisations were required to find all participants an internship or apprenticeship, if they had not yet secured their own job or began a business. This 3-month 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 AK trainers.

On average, the marginal costs of the training amounted to USD 450 per youth (this does not include the fixed cost component associated with design and management).

3. Measurement, data and identification

We use a randomised field experiment in two districts in Southern Province (*Huye*) and Western Province (*Nyamasheke*) to evaluate the impact of the AK training.⁷ Aided by community leaders, implementing organisations recruited vulnerable out-of-school youths. Field officers interviewed interested youths individually and administered a standardised rapid literacy assessment to ensure they had at least fourth-grade reading level. From the final list of 400 interested and eligible youths, equally divided over both districts, all youths were invited to participate in a short baseline survey (September 2013) and were randomly assigned to treatment and control groups. Assignment to treatment implies that youths were invited and encouraged to participate in the training, and assignment to control implies no such encouragement. This randomisation was stratified by district, so that in each district, 100 youth were assigned to treatment.⁸ An *ex ante* power analysis revealed that a sample of 400 youths would be enough to detect an impact with 80% power if the training would actually reduce the unemployment rate among youths by 13% points (versus a control

- 7 Youths were recruited from rural areas and small towns. Cultural imperatives, for example related to sharing within kin networks, likely vary across localities and affect incentives to receive training and engage in employment. However, stratified randomisation implies this does not compromise the internal validity of our analysis. Cultural sharing norms of course are relevant when considering the external validity of analyses like this one—transporting our impact estimates to other regions with different norms. However, the external validity of individual studies is debatable anyway since the impact of supply-side interventions (such as our training program) will always be conditional on demand-side factors—what sort of employment opportunities is locally available?
- 8 The initial sample for the experiment included 600 interested youths. However, the initial randomisation files were lost for one of the three implementing organisations, so we no longer have access to the assignment status of 200 youths (even if we know their actual treatment status). In light of considerable non-compliance (see below), which seems to be driven by unobservables (see [Table A1](#)), we continue our analysis with the remaining 400 youths. We explore power concerns below.

group unemployment rate of 45%; see Table 1). However, this computation is based on zero attrition and full compliance—assumptions that proved to be overly optimistic (see below). To start with, baseline data are available for (only) 392 individuals.

The intervention took place between September 2013 and May 2014. After the intervention, we organised three follow-up data collection waves. First, in October 2014, 6 months after the intervention finished, 323 participants participated in the first follow-up survey (for the analysis of short-term effects). We refer to this as the midline survey. Second, in June 2016, more than 2 years after the intervention, 310 youths participated in an endline survey (for the analysis of medium-term effects). Third, the data available at endline were complemented by lab-in-the-field games to measure certain preferences and beliefs in an incentive compatible fashion in December 2016 and January 2017. For this final round of data collection, referred to as endline games, we managed to track 317 participants. We allocated extra effort to retracing individuals from the control group, as these were somewhat under-represented in the midline and endline survey data waves.

The survey instrument for the first follow-up was created to evaluate livelihood outcomes, including wage labour acquisition and self-employment. The tool is composed of three parts: (i) employment and income information, (ii) financial management and (iii) work readiness and business skills. For endline surveys, we used a more elaborate tool with additional variables measuring business attitudes and behavioural change, networking and anti-social behaviour.

3.1. Dependent variables

We distinguish between (i) readiness outcomes, (ii) employment and financial outcomes and (iii) social outcomes. Many of our outcome variables are index numbers, aggregating the information of different survey questions. All indexes are standardised, 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 definition of variables and indices are provided in Appendix 1.

First, consider the intervention's readiness 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 (five self-reported knowledge questions) and a slightly more elaborate one at the endline (10 knowledge questions). For the endline, we use a validated locus of control index, based on twenty-one questions that measure whether the respondent believes he/she is able to control his/her own (economic) fate. The locus-of-control is defined as the belief individuals have about the degree to which they have control over factors influencing their lives (Rotter, 1954).⁹ Our business attitude index, based on seven questions, represents attitudes towards entrepreneurship and owning a business. To approximate the breadth and depth of someone's professional network we asked six 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 our variables for measuring employment and financial outcomes. We focus on employment status, which is comprised of formal and non-formal work for wages.

9 *Ex-ante*, it is not clear whether the training increases or decreases an individuals' sense of autonomy. Thus, the impact of the training on the locus of control is ambiguous.

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*, which indicates whether the respondent has been employment in at least one of these categories. As financial outcomes we collected data on income, consumption and savings levels.

Finally, turn to social outcomes. We asked sixteen questions about the frequency of engaging in socially undesirable behaviour (refusing to repay debts, drinking, fighting, use of drugs, etc.). The information collected in these questions was collapsed in one anti-social behaviour index. We also use two measures based on the play in lab-in-the-field games as additional social impact variables. First, we played a simple cheating game where we asked respondents 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 that was reported, the respondent 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 their outcomes truthfully or not. However, as the number of die rolls increases, the share of fives and sixes should approach 33%. For the subsamples of treated and control respondents, therefore, we can draw conclusions about the prevalence of cheating (over-reporting of fives and sixes). We also organised a common pool game where subjects decide 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 not only measures the propensity to cooperate but also picks up trust and a preference for efficiency. Game protocols are included in Appendix 2. The correlation between the anti-social behaviour index and the dishonesty measure from the laboratory experiment is surprisingly low: only -0.0283 .

Because we did not want to overburden the respondents, different questions were spread across different survey waves. Hence, while we collected repeated data for some variables across rounds (e.g., having a mentor and employment status), other questions were asked only once to respondents. For example, we only measured social outcomes at the endline and during the experimental games—not at the baseline or the first follow-up. Moreover, for some variables (work readiness, income and savings) we used different measures at endline than at baseline and first follow-up. In our tables, we use labels I and II to indicate when variables are based on a different measure.

3.2. Balance test at baseline

First, we examine whether random assignment of respondents to treatment and control arms created two comparable groups. Table 1, Panel A summarises key baseline variables for the two groups and reports the differences in sample means and the *p*-values associated with these differences as well as normalised differences (Imbens and Wooldridge, 2009). The list of variables includes simple individual characteristics like age and gender and the district where people live, as well as baseline values of our dependent variables.¹⁰ As indicated by

10 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).

the number of observations, there are missing values for some variables. On average, the two groups are not statistically different at baseline. Since the normalised differences are all small, we do not worry that linear regression methods are sensitive to the specification.

3.3. Attrition

Considerable attrition is not uncommon in field experiments (Ghanem *et al.*, 2020). As we work with underemployed youths, who are spatially mobile and move around searching for opportunities to work, this study forms no exception. In his review of the impact of training programs McKenzie (2017a) identifies attrition as the most major concern. All but one study have attrition rates of 18% (or more), and the highest attrition rate was no less than 46% (Cho *et al.*, 2013). This attrition is unlikely to be random and therefore likely to bias estimates of impact. For example, impact estimates are biased upwards if attritors from the treatment group are ‘low-quality’ participants who dropped out of the training and cannot be found afterwards or if attritors from the control group are ‘high-quality’ workers too busy to participate because they found good jobs independently of the training. Conversely, if the most successful individuals from the treatment group drop out of the sample because they are too busy in their new jobs, then impact estimates are biased downwards.

We, too, suffer from this problem and have up to 22.5% attrition in our follow-up data collection waves. *A priori* we do not know how this affects our impact estimates—upward or downward. During the endline game wave, we spent extra efforts to track youth, so that attrition is highest in the endline survey wave. Importantly, Panel B of Table 1 shows that for none of these waves attrition significantly differs across treatment and control. Nonetheless, to explore to what extent unbalanced attrition could explain our results, we report Lee (2009) bounds around treatment effects in our Appendix 3, where we report intent to treat (ITT) estimates. To obtain Lee bounds, we impose a monotonicity assumption and drop either the highest or the lowest values from the group with the lowest attrition rate until the attrition rates are equal. For most binary outcome variables, the difference between the lower and upper Lee bounds is relatively small compared with the width of the confidence interval, thus suggesting that unbalanced attrition does not explain our results.

Because samples of surveyed individuals differ across waves and because we are interested in comparing results across waves, we show two types of regression results based on different samples. We analyse the largest sample that is available for a particular model, but also show results for the sub-sample of 248 respondents that appear across all four data collection waves. We will henceforth call the latter population our common sample.

3.4. Identification strategy

Our experimental design was based on encouragement of randomly selected youths to participate in the training. This implies we can compare outcomes for the treatment and control groups and obtain unbiased estimates of the impact of being offered the program for a subsample of the respondents—the compliers. Panel C of Table 1 reveals that our subjects only partially complied with the random assignment in our experiment. Specifically, only 75% of the interested youths assigned to treatment actually participated in the trainings and no less than 19% of the control group youths managed to enter the program (eligible subjects

were never refused treatment by the implementing NGOs). From this perspective, it makes most sense to think about our experiment as an encouragement design. To study the impact of the treatment on the compliers, we therefore estimated a local average treatment effect (LATE). The second stage of the model reads as

$$y_{it} = \alpha + \beta \hat{P}_i + (\gamma y_{i0}) + \delta D_i + u_{it}, \quad (1)$$

where y_{it} denotes an outcome variable for respondent i , \hat{P}_i is predicted participation in the training and u_{it} is an error term. For several dependent variables, but not all, we have access to baseline values y_{i0} . If baseline values are available, they will be included on the RHS and we estimate an analysis of covariance specification to increase the power of the test. We also include baseline values if the baseline variable is defined differently, as our econometric specification is robust to this. D_i is a district dummy, as we stratified on the district.¹¹ 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.

We instrument P_i by the randomised treatment assignment and estimated the following first-stage equation:

$$P_i = \kappa + \lambda T_i + (\pi y_{i0}) + \rho D_i + \omega_{it}, \quad (2)$$

where P_i is a dummy variable equal to 1 if the respondent participated in the training and T_i equals 1 if the respondent is randomly assigned to the treatment group. Coefficient β in (1) is our estimator of interest and represents the LATE for the compliers in our sample. To test the strength of the instrument, we test $H_0 : \lambda = 0$.¹² Depending on the sample, the resulting F-statistics vary from 141.239 ($p = 0.000$) in the common sample to 179.131 ($p = 0.000$) for the first follow-up. We can reject the null hypothesis that the instrument is weak (also see Panel C of Table 1).

For completeness, we also report the results of the so-called ITT effects, in Appendix 3 (See Tables A2–A5). This implies estimating (1), but now using random assignment T as the main regressor (instead of predicted participation \hat{P}_i). In a qualitative sense, these results are rather similar, even if estimated coefficients are obviously smaller.

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. Therefore, within each wave and category of variables (readiness outcomes, employment and financial outcomes and social outcomes), we create a standardised summary index of the dependent variables within this wave-category pair. This allows us to focus on the treatment effects on indexes, which reduces the number of hypothesis considered and thus the probability of type I errors.¹³

3.5. Statistical power

When designing this experiment we did an ex ante power test, but the outcomes of this test are uninformative because we underestimated non-compliance and attrition. The power of

11 Since there are many missing values in baseline data, we do not control for other baseline values.

12 Without including the baseline value of the dependent variable.

13 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 standardised summary index.

Table 1: Balance Across Treatment and Control

| | Assigned to treatment | | Assigned to control | | Difference | | | | |
|--|-----------------------|----------|---------------------|-----|------------|------------|---------|-------|------------|
| | N | Mean | SD | N | Mean | SD | Mean | p | Normalised |
| <i>Panel A: baseline characteristics</i> | | | | | | | | | |
| Female | 198 | 0.49 | (0.50) | 194 | 0.43 | (0.50) | 0.06 | 0.220 | 0.124 |
| Age | 196 | 20.49 | (3.48) | 193 | 20.83 | (3.77) | -0.34 | 0.348 | -0.095 |
| District Huye | 198 | 0.49 | (0.50) | 194 | 0.52 | (0.50) | -0.03 | 0.686 | -0.041 |
| District Nyamasheke | 198 | 0.51 | (0.50) | 194 | 0.48 | (0.50) | 0.03 | 0.686 | 0.041 |
| Has a mentor | 179 | 0.29 | (0.46) | 171 | 0.31 | (0.46) | -0.02 | 0.693 | -0.042 |
| Work readiness index I | 165 | 0.03 | (1.03) | 156 | 0.00 | (1.00) | 0.03 | 0.799 | 0.028 |
| Employed | 198 | 0.58 | (0.50) | 194 | 0.55 | (0.50) | 0.03 | 0.630 | 0.049 |
| Self-employed | 198 | 0.29 | (0.45) | 194 | 0.30 | (0.46) | -0.01 | 0.810 | -0.024 |
| Wage-employed | 198 | 0.36 | (0.48) | 194 | 0.31 | (0.46) | 0.05 | 0.302 | 0.104 |
| Income I (Rwf/day) | 197 | 335.82 | (762.47) | 194 | 308.91 | (716.39) | 26.91 | 0.719 | 0.036 |
| Savings accumulated I (Rwf) | 175 | 10405.23 | (27723.52) | 174 | 6833.10 | (13431.70) | 3572.13 | 0.127 | 0.164 |
| <i>Panel B: sample inclusion</i> | | | | | | | | | |
| Midline sample | 198 | 0.85 | (0.36) | 194 | 0.80 | (0.40) | 0.05 | 0.200 | 0.130 |
| Endline sample | 198 | 0.81 | (0.39) | 194 | 0.77 | (0.42) | 0.04 | 0.397 | 0.086 |
| Game sample | 198 | 0.80 | (0.40) | 194 | 0.81 | (0.39) | -0.01 | 0.775 | -0.029 |
| Common sample | 198 | 0.67 | (0.47) | 194 | 0.59 | (0.49) | 0.08 | 0.106 | 0.164 |
| <i>Panel C: compliance</i> | | | | | | | | | |
| Participation | 198 | 0.75 | (0.43) | 193 | 0.19 | (0.39) | 0.56 | 0.000 | 1.122 |

Table 2: Short-term Impacts (LATE)

| | Readiness outcomes | | | | Employment and financial outcomes | | | | |
|--------------------------------|-------------------------------------|------------------------|-------------------------------------|---|-----------------------------------|--------------------------|--------------------------|------------------------------|--|
| | (1) Readiness outcome index I | (2) Has a mentor | (3) Work readiness index I | (4) Employment and financial outcome index I | (5) Employed | (6) Self- employed | (7) Wage- employed | (8) Income I (Rwf/day) | (9) Savings accumulated I (Rwf) |
| <i>Panel A: largest sample</i> | | | | | | | | | |
| Treatment | 0.633*** | 0.224** | 0.422** | 0.006 | 0.099 | 0.086 | 0.025 | -212.378 | -1090.920 |
| SE | (0.191) | (0.088) | (0.181) | (0.170) | (0.093) | (0.088) | (0.085) | (310.217) | (8514.728) |
| 95% upper bound | 0.948 | 0.369 | 0.720 | 0.286 | 0.252 | 0.231 | 0.166 | 299.369 | 12955.611 |
| Observations | 298 | 321 | 317 | 323 | 323 | 323 | 323 | 323 | 322 |
| Mean of control group | -0.000 | 0.643 | -0.000 | -0.000 | 0.555 | 0.297 | 0.284 | 879.042 | 22438.710 |
| <i>Panel B: common sample</i> | | | | | | | | | |
| Treatment | 0.757*** | 0.322*** | 0.428** | 0.018 | 0.091 | 0.121 | -0.029 | -399.667 | 4868.050 |
| SE | (0.220) | (0.100) | (0.198) | (0.181) | (0.103) | (0.100) | (0.096) | (375.450) | (8516.337) |
| 95% upper bound | 1.121 | 0.488 | 0.756 | 0.317 | 0.260 | 0.287 | 0.129 | 220.247 | 18930.009 |
| Observations | 230 | 247 | 243 | 248 | 248 | 248 | 248 | 248 | 247 |
| Mean of control group | -0.060 | 0.596 | -0.003 | 0.050 | 0.591 | 0.313 | 0.313 | 1010.467 | 21114.783 |
| ANCOVA | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Panels A and B show LATE estimates. The dependent variables are indicated in the column title. Readiness outcome index I is the standardised index of the variables in columns (2) and (3). Employment and financial outcome index I is the standardised index of the variables in columns (5), (8) and (9). See Appendix 1 for the definition of the other variables. Panel A shows results for all youth in our sample. Panel B shows results for youth that participated in all four data rounds. All regressions control for the baseline value of the dependent variable and for the respondent's district at baseline. We report robust standard errors and 95% upper bounds.

* $p < 0.10$,

** $p < 0.05$,

*** $p < 0.01$.

Table 3: Medium-term Impacts (LATE)

| | | Readiness outcomes | | | | | |
|--------------------------------|--|----------------------------|--------------|-------------------------|------------------------|-------------------------|------------------|
| | | (1) | (2) | (3) | (4) | (5) | (6) |
| | | Readiness outcome index II | Has a mentor | Work readiness index II | Locus of Control index | Business attitude index | Networking index |
| <i>Panel A: largest sample</i> | | | | | | | |
| Treatment | | 0.147 | 0.208 | 0.023 | 0.055 | -0.329 | 0.249 |
| SE | | (0.203) | (0.131) | (0.181) | (0.186) | (0.202) | (0.183) |
| 95% upper bound | | 0.483 | 0.425 | 0.322 | 0.362 | 0.004 | 0.550 |
| Observations | | 291 | 156 | 310 | 310 | 310 | 308 |
| Mean of control group | | 0.000 | 0.577 | 0.000 | 0.000 | -0.000 | -0.000 |
| <i>Panel B: common sample</i> | | | | | | | |
| Treatment | | 0.199 | 0.208 | 0.074 | 0.028 | -0.321 | 0.378* |
| SE | | (0.228) | (0.146) | (0.203) | (0.207) | (0.213) | (0.200) |
| 95% upper bound | | 0.576 | 0.450 | 0.408 | 0.370 | 0.031 | 0.708 |
| Observations | | 230 | 129 | 248 | 248 | 248 | 246 |
| Mean of control group | | 0.024 | 0.589 | -0.024 | 0.064 | 0.051 | -0.040 |
| ANCOVA | | Yes | Yes | Yes | No | No | No |

Notes: Panels A and B show LATE estimates. The dependent variables are indicated in the column title. Readiness outcome index II is the standardised index of the variables in columns (2)-(6). See Appendix 1 for the definition of the other variables. Panel A shows results for all youth in our sample. Panel B shows results for youth that participated in all four data rounds. Where possible, regressions control for the baseline value of the dependent variable, although the baseline value is sometimes defined differently. All regressions control for the respondent's district at baseline. We report robust standard errors and 95% upper bounds.

* $p < 0.10$,

** $p < 0.05$,

*** $p < 0.01$.

Table 4: Medium-term Impacts (LATE)

| | Employment and financial outcomes | | | | | | | |
|--------------------------------|--|-----------------|----------------------|----------------------|--------------------------------|----------------------------------|--|--------------------------------|
| | (1) Employment and financial outcome index II | (2) Employed | (3) Self-employed | (4) Wage-employed | (5) Income II (Rwff/day) | (6) Consumption (Rwff/day) | (7) Savings accumulated II (Rwff) | (8) Employed (game wave) |
| <i>Panel A: largest sample</i> | | | | | | | | |
| Treatment | -0.205 | 0.011 | 0.012 | -0.017 | -515.896* | -458.266 | -19345.538 | 0.098 |
| SE | (0.171) | (0.086) | (0.084) | (0.095) | (304.830) | (382.237) | (15527.382) | (0.092) |
| 95% upper bound | 0.077 | 0.154 | 0.151 | 0.140 | -12.973 | 172.362 | 6273.582 | 0.251 |
| Observations | 340 | 310 | 310 | 310 | 310 | 310 | 306 | 317 |
| Mean of control group | -0.000 | 0.713 | 0.260 | 0.487 | 988.438 | 1832.785 | 44323.630 | 0.646 |
| <i>Panel B: common sample</i> | | | | | | | | |
| Treatment | -0.244 | -0.017 | 0.058 | -0.084 | -603.345* | -507.083 | -21145.879 | 0.126 |
| SE | (0.198) | (0.094) | (0.094) | (0.105) | (340.930) | (412.091) | (16867.100) | (0.097) |
| 95% upper bound | 0.083 | 0.138 | 0.213 | 0.090 | -40.428 | 173.320 | 6705.091 | 0.286 |
| Observations | 248 | 248 | 248 | 248 | 248 | 248 | 246 | 248 |
| Mean of control group | 0.041 | 0.730 | 0.261 | 0.504 | 1075.735 | 1885.761 | 47299.558 | 0.635 |
| ANCOVA | Yes | Yes | Yes | Yes | Yes | No | Yes | Yes |

Notes: Panels A and B show LATE estimates. The dependent variables are indicated in the column title. Employment and financial outcome index II is the standardised index of the variables in columns (2) and (5)–(8). See Appendix 1 for the definition of the other variables. Panel A shows results for all youth in our sample. Panel B shows results for youth that participated in all four data rounds. Where possible, regressions control for the baseline value of the dependent variable, although the baseline value is sometimes defined differently. All regressions control for the respondent's district at baseline. We report robust standard errors and 95% upper bounds.

* $p < 0.10$,

** $p < 0.05$,

*** $p < 0.01$.

Table 5: Medium-term Impacts (LATE)

| | Social outcomes | | | |
|--------------------------------|-----------------------------------|--|---|---|
| | (1) Social outcome index | (2) Anti-social behaviour index | (3) Number of 5 or 6 obtained rolling a dice 10 times | (4) Amount withdrawn from the shared resource |
| <i>Panel A: largest sample</i> | | | | |
| Treatment | -0.001 | -0.017 | 0.720* | -117.831* |
| SE | (0.089) | (0.214) | (0.378) | (62.124) |
| 95% upper bound | 0.564 | 0.336 | 1.353 | -13.592 |
| Observations | 339 | 304 | 317 | 317 |
| Mean of control group | -0.000 | 0.000 | 7.582 | 408.861 |
| <i>Panel B: common sample</i> | | | | |
| Treatment | 0.145 | 0.040 | 0.865** | -79.479 |
| SE | (0.074) | (0.239) | (0.411) | (64.848) |
| 95% upper bound | 0.613 | 0.434 | 1.554 | 29.331 |
| Observations | 248 | 244 | 248 | 248 |
| Mean of control group | -0.032 | 0.036 | 7.609 | 372.174 |
| ANCOVA | No | No | No | No |

Notes: Panels A and B show LATE estimates. The dependent variables are indicated in the column title. Social outcome index is the standardised index of the variables in columns (2)–(4). See Appendices 1 and 2 for the definition of the other variables. Panel A shows results for all youth in our sample. Panel B shows results for youth that participated in all four data rounds. All regressions control for the respondent's district at baseline. We report standard errors clustered at the session level (columns (1), (3) and (4)), robust standard errors (column (2)) and 95% upper bounds.

* $p < 0.10$,

** $p < 0.05$,

*** $p < 0.01$.

the analysis is lower than expected, and as a consequence, we are underpowered to detect small or even moderate effects of the training. Our approach to dealing with power-related issues is twofold.

First, for several key dependent variables we provide the minimum detectable effect (MDE) size that follows from an ex-post power computation, based on the realised experiment with its imperfections. This is easy but does not add much additional information because the ex-post MDE is simply $t_{0,2} + t_{0,025} = 2.8$ times the standard error (Duflo *et al.*, 2007). We only report MDEs for variables with a straightforward interpretation, to facilitate comparison across studies (not for our index variables). Second, and more importantly, we ask whether the training has effects that are large enough to justify its costs. To probe this issue, we report the one-sided 95% upper-bound on the treatment effects of all variables (in a separate row). We use the upper bound for key variables—especially employment and income—to ask whether there is any chance, with the most extreme assumption, that this intervention is worth its cost.

4. Estimation results

In this section, we report short-term and medium-term impacts of the human capital intervention. To structure our discussion of the findings, we distinguish between readiness outcomes, employment and financial outcomes and social outcomes.¹⁴

4.1. Short-term effects: Half a year after the training

Data collection in 2014 focused on employability, work readiness and employment status, and there was no effort to collect data on civic or anti-social behaviour. In other words, we only have data for several readiness, employment and financial outcomes (and not for social outcomes). Table 2 summarises our results for short-term results. The variable Treatment refers to participating in the training program. As readiness outcome variables, we use a measure indicating whether the subject has a mentor, and the work readiness index. These separate variables have been collapsed into a single outcome index as well. The results indicate that, as expected, the training had a positive effect on readiness outcomes: 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.

None of the employment and financial outcomes are statistically significant. Since the point-estimate of the impact on employment is positive, and since the precision is relatively low, however, the one-sided 95% upper-bound on the impact on employment is considerable (25.2 percentage points). To explore whether the training could still be worth its costs, we now compare the costs and benefits per extra created job. If true impact on employment would be equal to the upper bound, then the cost per job created would equal USD 1800,¹⁵ or the equivalent of nearly 24 months of wages for the control group.¹⁶ However, the 95% upper bound on income (RWF 299/day \approx USD 0.5/day) suggests that the training at best helps youth to find part-time or low paying jobs, in which case the benefits need to sustain much longer to justify the costs of the training. Even for the most optimistic scenario, the costs of the training thus seem to be high compared with the short-term benefits, especially given the concern that people may lose their jobs within 2 years—an issue we return to below.

As mentioned, it is easy to compute MDEs for key variables (Duflo *et al.*, 2007). The MDE for the probability of finding a job is 0.26 ($=0.093*2.8 = 0.093*[t_{0.2} + t_{0.025}]$) percentage points—approximately ten times the average impact as measured in other studies (see McKenzie, 2017a). The MDE for the daily income variable is USD 1.4—approximately the effect of 3 hours of work. Observe that our coefficient of the income variable is negative. The MDE for savings is nearly USD 40 or almost equal to the average savings level in the control group. The MDEs are therefore, as argued, very high.

14 We have analysed whether the effects vary by gender but we are under-powered to pick up meaningful differences between men and women.

15 Note that the cost of the training per participant equals approximately USD 450. If for each participant, the training generates 0.252 job (the 95% upper bound of the impact on employment), the cost of generating one job equals $450/0.252 = 1786$ (= approximately USD 1800 per job).

16 The minimum wage for plantation work in Rwanda is less than USD 1 per day, but the minimum wage for construction work is in the range of USD 2.50–5.00. We assume that the average wage of youths who are fortunate enough to find a job is USD 3.50. (USD 1 = Rwf 600.)

4.2. Medium-term effects: 2 years after the training

We also evaluate effects after a period of 2 years has elapsed and also consider impacts beyond the labour market. Table 3 summarises results for a vector of readiness outcomes. In addition to the mentor and a work readiness variable, we now also consider indexes of the locus of control, business attitudes and networking. For the largest sample, we now find that assignment to treatment never enters significantly. While this may suggest that the treatment effect attenuated over time, the comparison between short-term and medium-term effects is limited by missing answers on the mentoring question and the use of a more elaborate, but different work-readiness index on the medium term. For the common sample, we also find very little evidence of impact. Only the networking index is marginally significant.

Table 4 reports the medium-term impacts on employment and financial outcomes. We now find no statistically significant results at a 5% significance level. This is true for both the employment variables, and the income and savings variables. For both samples and across all variables, we now find very small coefficients.

Observe that we find weak evidence that participating in the training reduces income (at the 10% level). It is unclear whether this finding reflects a type I error or a genuine impact of the training. If the latter, we are unable to test the underlying mechanism.¹⁷

The 95% upper bound for the probability of finding employment due to the training is 15 percentage points. This means that the upper bound estimate has fallen by some 10 percentage points or that in the upper bound scenario the effect disappeared for 40% of the training participants who found a job due to the training. Since the earn-back period of the training was 24 months, we conclude that the (private) benefits of the training in terms of extra wages do not compensate for the cost of the training—even for the most optimistic estimate of the training's effect on the probability of finding a job. The 95% upper bound on the income impact is slightly negative, which suggests that we can rule out even small effects on income on the medium term. The ex-post MDEs for income, consumption levels and savings levels are, respectively, USD 1.4, USD 1.8 and USD 72. As before, these numbers can be compared with the average income, consumption and savings levels in the control group.

Finally, Table 5 summarises our results on social outcomes. These appear mixed, and some are marginally significant. While we document no difference between the treated and control for the variable based on survey-based questions on anti-social behaviour, we find some impact on our experimental variables. Specifically, respondents assigned to treatment are more likely to lie about the number of fives and sixes thrown. However, there is also weak support for the hypothesis that these respondents extract less from the common pot, suggesting stronger pro-social preferences. Both these effects, however, do not survive standard adjustments for multiple hypothesis testing. Aggregating across the variables into one social impacts index suggests there is no evidence that the training helps to convert subjects into more civic-minded individuals or that frustration about lack of impact of the training causes the youths in our sample to behave 'worse'.

17 A few mechanisms are possible. It could be due to time spent in the training, which came at the opportunity cost of developing other skills or making different contacts that are more helpful in generating future income or it could be that the training increases reservation wages, decreasing employment and income in the medium term.

5. Discussion and conclusions

We evaluate a range of effects of broad employability training in Rwanda. The training combines elements of entrepreneurship training and technical (vocational) training, with an emphasis on ‘soft skills’ to promote employability or work readiness, and an extensive internship. Our short-term findings are mixed. We detect a significant increase in work readiness and support but fail to pick up an overall impact on employment or income. The situation does not seem to improve over time, as we do not find any significant effect after 2 years. In terms of rural livelihoods, the differences in income, consumption or savings remain also 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. The results echo those of [Blattman et al. \(2019\)](#) who also find that 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. There exists a potential downside to these disappointing results that extend beyond the variables that we measure. If the training altered participants’ expectations, they may now be less satisfied with their (mainly informal) jobs and perhaps be frustrated about the lack of change in their lives. However, we measure ‘unintended social impacts’ of the training with a survey-based measure of engaging in anti-social behaviour and experimental measures of cheating and pro-social preferences and do not provide robust support for the latter hypothesis.

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 capital injections) to have transformative impacts. Alternatively, there may be scope to complement the supply-side approach that aims to increase the employability of workers with demand-side measures. For example, subsidies can be given to firms to encourage them to hire more workers (e.g., [De Mel et al., 2016](#)). However, while wage subsidies may be useful for temporary employment creation, their effectiveness tends to drop after the subsidy stops ([McKenzie, 2017a](#)). Alternatively, governments can try to implement policies to help firms to create jobs (e.g., [McKenzie, 2017b](#)), or overcome regulations and labour laws that restrict growth and employment creation by firms.

Finally, our study has important limitations, so the results should be treated with caution. First, due to significant non-compliance our experiment resembles an encouragement design. This implies we are able to report LATEs that are representative for those that complied with the randomised treatment status (and participated in data collection). Non-compliance and attrition may limit the external validity of our results. Second, we wish to emphasise that our analysis suffers from low statistical power. The implication is that the intervention needs to be (highly) successful for us to be able to reject the null hypotheses with sufficient power. For example, we have computed the MDE size for key variables, aiming for 80% power with a 5% significance level. In the absence of non-compliance and attrition, we would be able to pick up a treatment-induced increase in the probability of employment of less than 13%-point. Non-compliance and attrition push up this MDE to 24%-point.

Nevertheless, our evaluation study suggests that it is highly unlikely that the program is economically efficient. The intervention we study is relatively expensive, and to justify its cost, we would need to document ‘big’ impacts. To err on the very optimistic side, we compute the

95% upper bounds of our results—a considerable adjustment in light of our large standard errors. Most importantly, this 95% upper bound for employment effects reveals that impacts are almost certainly too small to earn back the training cost. Future research should consider why the training was unsuccessful. Perhaps, it was too short to meaningfully change the skills and competences of participants (but increasing the training's length would also add to its cost). Alternatively, it may be the case that addressing human capital constraints alone to promote employment is not a cost-effective strategy to promote employment. This outcome is reinforced if we consider that any gains to subjects in the treatment group may partly be due to displacement of 'other job-seekers' on the labour market—the social returns of the project are lower than the private returns to subjects from the treatment group.

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Supplementary material

Supplementary material is available at *Journal of African Economies* online.

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Appendix 1: Definition of variables and indices

Work-readiness index I (baseline and first follow-up survey) is the standardised 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.

Employed is a dummy that equals 1 if the individual works for himself or herself (self-employment), with others in a cooperative (joint ownership), for his family (with payment) or for someone not in his or her family (as casual or contract) and 0 otherwise.

Self-employed is a dummy that equals 1 if the individual works for himself or herself (self-employment) or with others in a cooperative (joint ownership) and 0 otherwise.

Wage-employed is a dummy that equals 1 if the individual works for his family (with payment) or for someone not in his or her family (as casual or contract) and 0 otherwise.

Income I (baseline and first follow-up survey) is the total of income in the following categories:

- income from work for your family, for yourself or with others on average per day;
- income from work for someone not in your family per payment period, scaled to average per day.

Missing values in these categories are assumed to equal zero if the individual reports income in the other category.

Savings accumulated I (baseline and first follow-up survey) is the self-reported total of savings.

Work-readiness index II (endline survey) is the standardised 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, Prioritising 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: 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 the standardised 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 me have a little chance in protecting our personal interests when they are in conflict with those of strong pressure groups. (-)
12. Regarding money, there is not much you can do for yourself when you are poor. (-)
13. Politicians can do very little to prevent poverty. (+)
14. It is not always wise for me to save because many things turn out to be a good matter of good fortune or 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 labelled as Entirely disagree, Mostly disagree, Somewhat disagree, Neither agree nor disagree, Somewhat agree, Mostly agree and Entirely agree.

Business Attitude index (endline survey) is the standardised 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 labelled as Entirely disagree, Mostly disagree, Somewhat disagree, Neither agree nor disagree, Somewhat agree, Mostly agree and Entirely agree.

Networking index (endline survey) is the standardised 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.

Income II (endline survey) is the total of income in the following categories:

- income from sales of agricultural output in the past 12 months, scaled to average per day;
- income from agricultural (off-farm) wage labour in the past 7 days, scaled to average per day;
- income from non-agricultural wage labour in the past 7 day, scaled to average per day;
- income from micro-enterprise or other self-employment in the past 7 day, scaled to average per day.

One individual from the control group, who actually participated in the treatment, reported income and accumulated savings more than five times higher than all other individuals in our sample. As (i) our IV estimation assumes that the outcomes of non-compliers are unaffected by the treatment, (ii) and we wanted to limit the effect of this outlier on our findings and (iii) we wanted to be conservative in ruling out large treatment effects, we have set the income of this individual equal to the second-highest income in the sample.

Consumption (endline survey) is the total of consumption in the following categories.

- Consumption of ... in the past 12 months, scaled to average per day:
 - wrap around cloth for women
 - men's garments
 - women's garments
 - children's clothing
 - women's tailoring
 - men's footwear
 - women's footwear
 - children's footwear
 - international travel
 - building repairs (supplies)
 - building repair labour
 - mattresses
 - bed sheets and pillows
 - local travel
 - health insurance
- Consumption of ... in the past 4 weeks, scaled to average per day:
 - disinfectant and cleaners
 - shoe brushes and polish
 - household (domestic) help
 - spare parts of motorcycle or bicycle
 - film and developing equipment
 - passport photos
 - men's haircuts (barber)
 - women's haircuts (stylist and treatment)
 - hair products
 - toothpaste (and accessories)
 - sanitary napkins
 - beauty/cosmetic products
 - vehicles
 - labour for repair of motorcycle or bicycle
 - labour for vehicle repair
 - bicycles/motorcycles
 - generators
 - bail/fines
 - sound systems
 - fishing equipment

- mills/grinding equipment
- mobile phones
- sewing machines
- solar panels
- pumps
- wheelbarrows
- batteries
- Consumption of ... in the past 7 days, scaled to average per day:
 - local rice
 - imported rice
 - corn (flour)
 - sorghum (flour)
 - carbonated soft drinks
 - beef meet
 - peanut oil
 - palm oil
 - cooking bananas
 - tomato
 - Irish potato
 - sweet potato
 - sugar (local)
 - fresh milk
 - local commercial beer
 - bar drinks
 - restaurant food and drinks

Missing values in each of these categories are assumed to equal zero if the individual reports consumption in other categories.

Accumulated savings II (endline survey) is the total of accumulated savings in the following categories:

- savings via savings group
- microfinance savings
- savings via Savings and Credit Cooperative Organization
- bank savings
- cash savings
- informal savings (savings via Rotating Credit and Savings Association, other)
- value of crops in storage
- mobile savings
- other savings

One individual from the control group, who actually participated in the treatment, reported income and accumulated savings more than five times higher than all other individuals in our sample. As (i) our IV estimation assumes that the outcomes of non-compliers are unaffected by the treatment, (ii) and we wanted to limit the effect of this outlier on our findings and (iii) we wanted to be conservative in ruling out large treatment effects,

we have set the accumulated of this individual to the second-highest accumulated savings in the sample.

Anti-social behaviour index (endline survey) is the standardised mean of questions to what extend the respondent has been engaged in each of the following behaviours 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.
9. Cursed at someone.
10. Acted rudely towards someone.
11. Publicly embarrassed someone.
12. Felt so sad or hopeless almost every day for 2 weeks or more in a row that you stopped doing some usual activities.
13. Engaged in heavily drinking of beer.
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 labelled as Never, Once a year, Twice a year, Several times a year, Monthly, Weekly and Daily.

Appendix 2: 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 consists of
 - a short survey
 - a peeling garlics task
 - four games.¹⁸
- 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.

18 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.

- 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 2 hours.
- Do you have any questions? Please ask the enumerator if you have any questions.
- If you have questions later, you may contact researcher XXX at XXX.
- 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 ten 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 three times a 5 or 6, and another seven times, you roll a 1, 2, 3 or 4, then you will earn Rwf 600.

Example 2

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

Example 3

If you roll seven times a 5 or 6, and another three 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.

| | | 1. Yes | 2. No |
|------|---|---------------|-------|
| 2.1 | Roll the dice for the first time. Does the number of dots equal 5 or 6? | | |
| 2.2 | Roll the dice for the second time. Does the number of dots equal 5 or 6? | | |
| 2.3 | Roll the dice for the third time. Does the number of dots equal 5 or 6? | | |
| 2.4 | Roll the dice for the fourth time. Does the number of dots equal 5 or 6? | | |
| 2.5 | Roll the dice for the fifth time. Does the number of dots equal 5 or 6? | | |
| 2.6 | Roll the dice for the sixth time. Does the number of dots equal 5 or 6? | | |
| 2.7 | Roll the dice for the seventh time. Does the number of dots equal 5 or 6? | | |
| 2.8 | Roll the dice for the eighth time. Does the number of dots equal 5 or 6? | | |
| 2.9 | Roll the dice for the ninth time. Does the number of dots equal 5 or 6? | | |
| 2.10 | Roll the dice for the tenth time. Does the number of dots equal 5 or 6? | | |
| | | <i>Answer</i> | |
| 2.11 | How many times did you answer yes on the above ten questions? | | |
| 2.12 | How much would you have earned from rolling the dice these ten times? | Rwf | |

- 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.

| | | 1. Yes | 2. No |
|------|---|---------------|-------|
| 2.13 | Roll the dice for the first time. Does the number of dots equal 5 or 6? | | |
| 2.14 | Roll the dice for the second time. Does the number of dots equal 5 or 6? | | |
| 2.15 | Roll the dice for the third time. Does the number of dots equal 5 or 6? | | |
| 2.16 | Roll the dice for the fourth time. Does the number of dots equal 5 or 6? | | |
| 2.17 | Roll the dice for the fifth time. Does the number of dots equal 5 or 6? | | |
| 2.18 | Roll the dice for the sixth time. Does the number of dots equal 5 or 6? | | |
| 2.19 | Roll the dice for the seventh time. Does the number of dots equal 5 or 6? | | |
| 2.20 | Roll the dice for the eighth time. Does the number of dots equal 5 or 6? | | |
| 2.21 | Roll the dice for the ninth time. Does the number of dots equal 5 or 6? | | |
| 2.22 | Roll the dice for the tenth time. Does the number of dots equal 5 or 6? | | |
| | | <i>Answer</i> | |
| 2.23 | How many times did you answer yes on the above ten questions? | | |
| 2.24 | How much do you earn from rolling the dice these ten times? | Rwf | |

- 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

| | | |
|------------------------------|----------------------------------|----------------------------|
| Suppose | Then | So |
| Player A/E withdraws Rwf 200 | The remaining shared resource is | Player A/E earns Rwf 1,000 |
| Player B/F withdraws Rwf 300 | Rwf 1,600 | Player B/F earns Rwf 1,100 |
| Player C/G withdraws Rwf 500 | Doubling this shared resource | Player C/G earns Rwf 1,300 |
| Player D/H withdraws Rwf 600 | gives Rwf 3,200 | Player D/H earns Rwf 1,400 |
| | So all players receive another | Group total is Rwf 4,800 |
| | Rwf 800 | |

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

Example 2

| | | |
|------------------------------|----------------------------------|--------------------------|
| Suppose | Then | So |
| Player A/E withdraws Rwf 800 | The remaining shared resource is | Player A/E earns Rwf 800 |
| Player B/F withdraws Rwf 800 | Rwf 0 | Player B/F earns Rwf 800 |
| Player C/G withdraws Rwf 800 | Doubling this shared resource | Player C/G earns Rwf 800 |
| Player D/H withdraws Rwf 800 | gives Rwf 0 | Player D/H earns Rwf 800 |
| | So all players receive another | Group total is Rwf 3,200 |
| | Rwf 0 | |

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

Example 3

| | | |
|----------------------------|----------------------------------|----------------------------|
| Suppose | Then | So |
| Player A/E withdraws Rwf 0 | The remaining shared resource is | Player A/E earns Rwf 1,600 |
| Player B/F withdraws Rwf 0 | Rwf 3,200 | Player B/F earns Rwf 1,600 |
| Player C/G withdraws Rwf 0 | Doubling this shared resource | Player C/G earns Rwf 1,600 |
| Player D/H withdraws Rwf 0 | gives Rwf 6,400 | Player D/H earns Rwf 1,600 |
| | So all players receive another | Group total is Rwf 6,400 |
| | Rwf 1,600 | |

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.

| | | <i>Answer</i> |
|-----|---|---------------|
| 5.1 | What amount do you withdraw from the shared resource? | Rwf |

- 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.

Appendix 3: Appendix tables

Table A1: Differences Between Participants and Non-participants

| | Participants | | | Non-participants | | | Diff | |
|--|--------------|---------|------------|------------------|---------|------------|---------|-------|
| | N | Mean | SD | N | Mean | SD | Mean | p |
| <i>Panel A: baseline characteristics</i> | | | | | | | | |
| Female | 186 | 0.49 | (0.50) | 205 | 0.44 | (0.50) | 0.05 | 0.321 |
| Age | 185 | 20.23 | (3.43) | 203 | 21.01 | (3.74) | -0.78 | 0.032 |
| District Huye | 186 | 0.51 | (0.50) | 205 | 0.50 | (0.50) | 0.01 | 0.954 |
| District Nyamasheke | 186 | 0.49 | (0.50) | 205 | 0.50 | (0.50) | -0.01 | 0.954 |
| Has a mentor | 167 | 0.28 | (0.45) | 182 | 0.32 | (0.47) | -0.04 | 0.449 |
| Work readiness index I | 154 | 0.05 | (1.03) | 166 | -0.02 | (1.00) | 0.07 | 0.495 |
| Employed | 186 | 0.59 | (0.49) | 205 | 0.54 | (0.50) | 0.05 | 0.376 |
| Self-employed | 186 | 0.33 | (0.47) | 205 | 0.26 | (0.44) | 0.07 | 0.107 |
| Wage-employed | 186 | 0.33 | (0.47) | 205 | 0.33 | (0.47) | 0.00 | 0.973 |
| Income I (Rwf/day) | 185 | 322.68 | (641.02) | 205 | 318.97 | (819.49) | 3.71 | 0.960 |
| Savings accumulated I (Rwf) | 165 | 9452.85 | (26003.56) | 183 | 7876.26 | (17375.86) | 1576.59 | 0.511 |

Table A2: Short-term Impacts (ITT)

| | Readiness outcomes | | | | Employment and financial outcomes | | | | |
|--------------------------------|---------------------------|--------------|------------------------|--|-----------------------------------|---------------|---------------|--------------------|-----------------------------|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| | Readiness outcome index I | Has a mentor | Work readiness index I | Employment and financial outcome index I | Employed | Self-employed | Wage-employed | Income I (Rwf/day) | Savings accumulated I (Rwf) |
| <i>Panel A: largest sample</i> | | | | | | | | | |
| Assigned to treatment | 0.359*** | 0.131** | 0.250** | 0.004 | 0.058 | 0.051 | 0.015 | -125.349 | -643.528 |
| SE | (0.107) | (0.051) | (0.108) | (0.100) | (0.055) | (0.052) | (0.050) | (182.676) | (5021.702) |
| 95% upper bound | 0.536 | 0.215 | 0.428 | 0.169 | 0.149 | 0.136 | 0.098 | 176.001 | 7640.647 |
| Lee lower bound | 0.235 | 0.109 | 0.134 | -0.079 | 0.034 | 0.008 | -0.023 | -314.725 | -7769.229 |
| Lee upper bound | 0.437 | 0.172 | 0.390 | 0.080 | 0.096 | 0.070 | 0.039 | -74.452 | 1183.524 |
| Observations | 298 | 321 | 317 | 323 | 323 | 323 | 323 | 323 | 322 |
| Mean of control group | -0.000 | 0.643 | -0.000 | -0.000 | 0.555 | 0.297 | 0.284 | 879.042 | 22438.710 |
| <i>Panel B: common sample</i> | | | | | | | | | |
| Assigned to treatment | 0.433*** | 0.194*** | 0.262** | 0.011 | 0.055 | 0.073 | -0.018 | -242.530 | 2945.112 |
| SE | (0.122) | (0.058) | (0.122) | (0.110) | (0.062) | (0.060) | (0.058) | (226.599) | (5157.789) |
| 95% upper bound | 0.634 | 0.290 | 0.463 | 0.192 | 0.158 | 0.173 | 0.078 | 131.613 | 11461.522 |
| Observations | 230 | 247 | 243 | 248 | 248 | 248 | 248 | 248 | 247 |
| Mean of control group | -0.060 | 0.596 | -0.003 | 0.050 | 0.591 | 0.313 | 0.313 | 1010.467 | 21114.783 |
| ANCOVA | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Panels A and B show ITT estimates. The dependent variables are indicated in the column title. Readiness outcome index I is the standardised index of the variables in columns (2) and (3). Employment and financial outcome index I is the standardised index of the variables in columns (5), (8) and (9). See Appendix 1 for the definition of the other variables. Panel A shows results for all youth in our sample. Panel B shows results for youth that participated in all four data rounds. All regressions control for the baseline value of the dependent variable and for the respondent's district at baseline. We report robust standard errors and 95% upper bounds. ANCOVA indicates analysis of covariance.

* $p < 0.10$,
 ** $p < 0.05$,
 *** $p < 0.01$.

Table A3: Medium-term Impacts (ITT)

| | Readiness outcomes | | | | | |
|--------------------------------|---|------------------------|--------------------------------------|-------------------------------------|--------------------------------------|----------------------------|
| | (1) Readiness outcome index II | (2) Has a mentor | (3) Work readiness index II | (4) Locus of Control index | (5) Business attitude index | (6) Networking index |
| <i>Panel A: largest sample</i> | | | | | | |
| Assigned to treatment | 0.083 | 0.126 | 0.014 | 0.033 | -0.196 | 0.149 |
| SE | (0.115) | (0.077) | (0.108) | (0.111) | (0.119) | (0.109) |
| 95% upper bound | 0.274 | 0.254 | 0.191 | 0.216 | 0.001 | 0.329 |
| Lee lower bound | 0.019 | 0.064 | -0.066 | -0.035 | -0.196 | 0.092 |
| Lee upper bound | 0.178 | 0.237 | 0.020 | 0.120 | -0.045 | 0.399 |
| Observations | 291 | 156 | 310 | 310 | 310 | 308 |
| Mean of control group | 0.000 | 0.577 | 0.000 | 0.000 | -0.000 | -0.000 |
| <i>Panel B: common sample</i> | | | | | | |
| Assigned to treatment | 0.114 | 0.127 | 0.045 | 0.017 | -0.194 | 0.231* |
| SE | (0.130) | (0.086) | (0.123) | (0.126) | (0.128) | (0.121) |
| 95% upper bound | 0.329 | 0.269 | 0.247 | 0.225 | 0.018 | 0.431 |
| Observations | 230 | 129 | 248 | 248 | 248 | 246 |
| Mean of control group | 0.024 | 0.589 | -0.024 | 0.064 | 0.051 | -0.040 |
| ANCOVA | Yes | Yes | Yes | No | No | No |

Notes: Panels A and B show ITT estimates. The dependent variables are indicated in the column title. Readiness outcome index II is the standardised index of the variables in columns (2)–(6). See Appendix 1 for the definition of the other variables. Panel A shows results for all youth in our sample. Panel B shows results for youth that participated in all four data rounds. Where possible, regressions control for the baseline value of the dependent variable, although the baseline value is sometimes defined differently. All regressions control for the respondent's district at baseline. We report robust standard errors and 95% upper bounds.

* $p < 0.10$,

** $p < 0.05$,

*** $p < 0.01$.

Table A4: Medium-term Impacts (ITT)

| | Employment and financial outcomes | | | | | | | |
|--------------------------------|--|-------------------|--------------------------|--------------------------|-------------------------------|---------------------------------|---|--------------------------------|
| | (1) Employment and financial outcome index II | (2) Employed | (3) Self- employed | (4) Wage- employed | (5) Income II (Rwf/day) | (6) Consumption (Rwf/day) | (7) Savings accumulated II (Rwf) | (8) Employed (game wave) |
| <i>Panel A: largest sample</i> | | | | | | | | |
| Assigned to treatment | -0.116 (0.095) | 0.007 (0.052) | 0.007 (0.050) | -0.010 (0.057) | -306.512* (175.433) | -272.650 (226.296) | -11359.898 (8970.630) | 0.056 (0.052) |
| 95% upper bound | 0.040 | 0.092 | 0.090 | 0.083 | -17.075 | 100.701 | 3441.030 | 0.142 |
| Lee lower bound | -0.111 | -0.007 | -0.024 | -0.035 | -494.527 | -528.897 | -23816.348 | 0.050 |
| Lee upper bound | -0.056 | 0.038 | 0.021 | 0.010 | -265.764 | -199.857 | -7700.325 | 0.064 |
| Observations | 340 | 310 | 310 | 310 | 310 | 310 | 306 | 317 |
| Mean of control | -0.000 | 0.713 | 0.260 | 0.487 | 988.438 | 1832.785 | 44323.630 | 0.646 |
| <i>Panel B: common sample</i> | | | | | | | | |
| Assigned to treatment | -0.148 (0.117) | -0.010 (0.057) | 0.035 (0.057) | -0.051 (0.063) | -366.127* (199.263) | -307.529 (247.992) | -12714.771 (9977.117) | 0.076 (0.059) |
| 95% upper bound | 0.045 | 0.083 | 0.129 | 0.054 | -37.119 | 101.930 | 3759.453 | 0.173 |
| Observations | 248 | 248 | 248 | 248 | 248 | 248 | 246 | 248 |
| Mean of control | 0.041 | 0.730 | 0.261 | 0.504 | 1075.735 | 1885.761 | 47299.558 | 0.635 |
| group | | | | | | | | |
| ANCOVA | Yes | Yes | Yes | Yes | Yes | No | Yes | Yes |

Notes: Panels A and B show ITT estimates. The dependent variables are indicated in the column title. Employment and financial outcome index II is the standardised index of the variables in columns (2) and (5)–(8). See Appendix 1 for the definition of the other variables. Panel A shows results for all youth in our sample. Panel B shows results for youth that participated in all four data rounds. Where possible, regressions control for the baseline value of the dependent variable, although the baseline value is sometimes defined differently. All regressions control for the respondent's district at baseline. We report robust standard errors and 95% upper bounds.

* $p < 0.10$,

** $p < 0.05$,

*** $p > 0.01$

Table A5: Medium-term Impacts (ITT)

| | (1) | (2) | Social outcomes | |
|--------------------------------|-------------------------|-----------------------------------|---|---|
| | Social outcome index | Anti-social behaviour index | (3) Number of 5 or 6 obtained rolling a dice 10 times | (4) Amount withdrawn from the shared resource |
| <i>Panel A: largest sample</i> | | | | |
| Assigned to treatment | -0.000 | -0.010 | 0.408* | -66.752* |
| SE | (0.051) | (0.126) | (0.219) | (34.705) |
| 95% upper bound | 0.319 | 0.197 | 0.775 | -8.519 |
| Lee lower bound | -0.032 | -0.187 | 0.311 | -75.673 |
| Lee upper bound | 0.045 | 0.037 | 0.452 | -64.313 |
| Observations | 339 | 304 | 317 | 317 |
| Mean of control group | -0.000 | 0.000 | 7.582 | 408.861 |
| <i>Panel B: common sample</i> | | | | |
| Assigned to treatment | 0.088 | 0.024 | 0.525** | -48.202 |
| SE | (0.046) | (0.143) | (0.247) | (39.037) |
| 95% upper bound | 0.381 | 0.260 | 0.940 | 17.300 |
| Observations | 248 | 244 | 248 | 248 |
| Mean of control group | -0.032 | 0.036 | 7.609 | 372.174 |
| ANCOVA | No | No | No | No |

Notes: Panels A and B show ITT estimates. The dependent variables are indicated in the column title. Social outcome index is the standardised index of the variables in columns (2)–(4). See Appendices 1 and 2 for the definition of the other variables. Panel A shows results for all youth in our sample. Panel B shows results for youth that participated in all four data rounds. All regressions control for the respondent's district at baseline. We report standard errors clustered at the session level (columns (1), (3) and (4)), robust standard errors (column (2)) and 95% upper bounds.

* $p < 0.10$,

** $p < 0.05$,

*** $p < 0.01$.