The Impact of Skills Training on Financial Behaviour, Employability, and Educational Choice of Youth:

Findings from a Randomized Controlled Trial in Morocco

Jonas Bausch, Paul Dyer, Drew Gardiner, Jochen Kluve, Sonja Kovacevic*

15 December 2016

Abstract

This paper reports results of a randomized controlled trial that evaluates a skills training programme for youth – 15 to 25 years of age – implemented until 2013 in Morocco's Oriental region. The training course focused on financial, life and entrepreneurial skills aiming to help youth face challenges as they attempt to bridge the school-to-work transition. We find strong positive effects on participants' propensity to establish a savings accounts and maintaining it more than two years after the end of the intervention. While the effect is robust across socio-demographic subgroups, it does not generally translate into increased financial behaviour, such as taking a loan or saving activity. Still, participants from more affluent backgrounds are more likely to access loans. Estimates for labour market outcomes and educational choice show that men, older participants and youth from more affluent family backgrounds are more likely to continue staying in education while postponing entry into the labour market. Extensive robustness checks confirm these results and carefully address imperfect take-up as well as high survey attrition rates that appears to be non-differential. Our study implies that through effective targeting towards youth at the end of their educational careers, programme managers and policy makers could increase the effectiveness of programmes. At the same time, the heterogeneous outcomes across social background and gender shows that the constraints faced by these groups need to be explored more closely. Addressing barriers that prevent participants from fully making use of what skills training can offer has the potential to scale up the effect of these youth employment interventions.

* Address correspondence to bausch@ilo.org. Bausch, Gardiner, Kovacevic: International Labour Organization, Dyer: Independent consultant, Kluve: Humboldt-University Berlin, RWI – Leibniz Institute for Economic Research.
Financial support from the International Initiative for Impact Evaluation, Inc. (3ie) is gratefully acknowledged. Importantly, the views expressed are not necessarily those of 3ie or its members, or of the International Labour Organization.

1. Introduction

Amid global concerns about the economic exclusion of youth, efforts to facilitate youth access to decent jobs and financial services have become a developmental priority. In the Middle East and North Africa (MENA) region, where growth of the youth population in past decades has exacerbated pressures on the educational system and labour markets, this issue is particularly salient. Morocco is no exception where, as of 2015, young people (aged 15 to 29) made up 27 percent of the total population (Morocco Haut Commissariat au Plan 2015). Although fertility rates are gradually declining, the current large youth cohort leads to challenges in the transition from school to work.

In combination with the chronically low labour demand, this has led to poor labour market outcomes for young people. In MENA, youth unemployment rates stand at 19.3 percent (19.5 percent for men and 18.1 percent for women) and close to 90 percent of young women and about 40 percent of young men who are not in school are either unemployed or out of the labour force (World Bank 2012). Moreover, in MENA – where nearly 85 percent of youth remain unbanked – inadequate access to financial services hampers the ability of youth to prepare for their economic futures. These youth face obstacles in establishing a sound financial foundation and obtaining financial services that would empower them more broadly as economic actors, including beginning saving or accessing loans that would allow them to leverage future earnings (Dhillon et al. 2009; World Bank 2012).

This paper evaluates a youth-focused skills training programme in Morocco using a Randomized Controlled Trial (RCT). The intervention delivered financial, life and entrepreneurial skills training, aiming to assist youth with the challenges they are facing during their school-to-work transitions. The Youth Employment Inventory (YEI), a database of youth employment interventions, shows that skills training are the most prevalent component within Active Labour Market Programme (ALMP) portfolios of governmental and non-governmental organizations, designed to equip youth with the skills and knowledge required to enter the world of work. In fact, of the 328 youth employment programs included for the MENA region in the Youth Employment Inventory, around 70 percent are training programs or maintain training as a core element in a mix of youth services.[1] However, despite their prevalence, little is known about the effectiveness of such programs in addressing the wider challenges of economic inclusion among the youth population in MENA.

The global evidence base for youth skills training is characterized by a remarkable mismatch: While the overwhelming majorities of skills training evaluations is performed in industrialized countries, the scarce evidence available on low and middle-income countries suggests that skills training are more effective in these very countries (Betcherman et al. 2007, Kluve et al. 2016). Thus, investments in skills trainings for youth in low- and middle-income countries might be particularly worthwhile, but lack of credible evidence prevents from confidently advising to expand these programmes. Moreover, while the evidence base for ALMPs in developing countries has been growing over the past years (see Cunningham et al. 2010; Ibarraran and

[1] The Youth Employment Inventory (YEI) is a comprehensive database to provide comparative information on youth employment interventions worldwide documenting programme design, implementation and achieved results. YEI is a joint initiative by the German Ministry of Economic Cooperation and Development, the Inter-American Development Bank, the International Labour Organization (ILO), and the World Bank. To access the database: http://www.youth-employment-inventory.org/.

Shady 2009; USAID 2013), both wide regional and methodological gaps remain. Unfortunately, much of the research is non-experimental (see for example Betcherman et al. 2004; Card et al. 2010; ILO 2015b; Kluve and Weber 2010), raising doubts whether the observed effects can be attributed to the intervention in question. Studies on youth-focused skills trainings that employ experimental designs were often not focused on MENA countries (e.g. Attanasio et al. 2011; Card et al. 2011; Cho et al. 2013).

Thus, by far the largest evidence gap regarding what works in youth employment remains in the MENA region. The YEI reveals that only two impact evaluations in the region focus on skills training. First, Groh et al. (2012) present evidence from Jordan on the effectiveness of wage subsidies and soft skills training in helping female community college graduates find employment. The study shows that wage subsidies are effective in increasing employment in the short term, but the accompanying soft skills training programme had no impact on average labour outcomes. Second, Premand et al. (2012) evaluate an entrepreneurship training program, focusing on skills in business planning and leadership, for Tunisian students in their final year of university. They found a small effect on the self-employment rate, while the wage employment rate remained unchanged. Nevertheless, as in many other examples of skills training programs, the intervention did succeed in boosting knowledge, optimism and other behavioural skills.

Reviewing existing research points to a difficulty observed for establishing the effects of skills training: Active Labour Market Programmes (ALMPs) often combine skills provision with one or more other interventions types such as wage subsidies, internships or access to loans. This might be indeed preferable from a programmatic viewpoint. In a recent global systematic review of youth employment interventions, Kluve et al. (2016) find that multi-pronged interventions tend be more effective. Still, when evaluating multidimensional interventions, problems arise in attributing the effects to any single component. The study we present in this paper is a pure skills training, so we can unambiguously link the observed effects to that particular type of ALMP.

This paper contributes to the global evidence base on skills trainings, youth employment and financial inclusion. Being the first study of its kind in Morocco, it is particularly relevant for the local context as well as for the MENA region as a whole. Our findings show that skills trainings can enable youth to use opportunities when present but do not fundamentally alter the opportunity structure itself. Programme impacts are concentrated on intermediate outcomes or only observable for certain socio-demographic groups. Our second main contribution concerns a new dimension of skills training effect. While skills trainings were believed to increase knowledge and might on this causal pathway also impact employment opportunities, we show that the increased awareness of one’s own situation also affect life choices, such as increased investment in education.

The intervention we assess targets youth between the ages of 15 and 25 living in Morocco’s Oriental Region. Its curriculum consists of three main modules, delivered as one training over 100 hours of instruction. It focuses on financial education providing participants with practical tools to help them manage their personal finances. Alongside, personal competencies and conflict management skills, were addressed. Given the low labour demand in the region, also business and entrepreneurial skills were covered, for example through developing a business plan. We explore impacts on a range of outcomes related to financial inclusion, employability and human capital acquisition. As a direct outcome we are interested in whether training participants demonstrate greater financial knowledge and heightened awareness of banking institutions and their services. As a more demanding outcome, we study if the intervention influences educational choices, and whether it places participants in a better position to enter the labour force and increases their prospects when entering the labour market. Moreover, we investigate whether and how impacts differ between women and men, younger and older participants and individuals from more and less affluent households to develop additional guidance concerning specific challenges related to individual background characteristics.

Our analysis is based on a sample of up to 1815 youth (53 percent women) for which baseline and endline date were collected in autumn 2012 and 2015, respectively. The evaluation exploits the random allocation of study participants to a treatment and control group to identify causal programme effects. We present and compare different estimators, including local average treatment effects to account for imperfect take-up. Extensive statistical and econometrical checks suggest that even though attrition in the follow-up survey is around 52 percent it appears to be non-differential between treatment and control group and unlikely to systematically affect our impact estimates.

We find positive impacts on outcomes related to financial behaviour of youth, shown through the establishment and maintenance of savings accounts. However, increases in savings could not be demonstrated. This seemingly discrepant pattern delivers a valuable insight in the nature of changes that can be realized through skills training: It helps youth seizing existing opportunities but does not alter the opportunity structure itself. Where youth are already preparing for employment or entrepreneurial activities because they are about to finish their educational cycles, the training gives an additional edge in terms of knowledge and access to financial instruments. Where youth are still involved in education or face severe (gendered) constraints, the training does not fundamentally alter behaviour. Skills trainings can therefore be characterized as contributory and auxiliary. These findings help to shed light on patterns of mixed evidence observed in previous studies: for trainings addressing a specific knowledge gap in an otherwise enabling environment, huge positive effects can be observed. But if participants are facing severe external constraints, the training alone cannot substantially alter their economic opportunities.

Our second main finding concerns the side effect of the training on educational choices. Our estimates of labour market outcomes and educational choice show that men, older participants and youth from more affluent family backgrounds are more likely to choose continuing their educational careers while (partly) postponing entry into the labour market. Given that investment in education is associated with more promising, economically secure futures, this is a positive although unexpected outcome. Our first finding on constraints still holds: only those participants whose financial and cultural backgrounds allow for it end up continuing their education. In sum, the study not only explores the conditions under which skills training have substantial impacts but also shows that these interventions may indeed have much more far reaching effects than increasing knowledge in a specific area.

The remainder of the paper is organized as follows: Section 2 describes the evaluated skills training programme and sketches its theory of change. Section 3 elaborates on the research questions, the evaluation design and data collection. Section 4 provides descriptive statistics of the study sample and in-depth analysis of attrition patterns in the follow-up survey as well as of factors that determine take-up of the intervention. Section 5 contains results before Section 6 discusses the main findings and concludes.

2. Intervention

Morocco is a middle-income country with a GDP per capita of around 7'800 USD (PPP) in 2015 (World Bank 2016). Similar to other countries in the MENA region its economy is plagued by slow job growth and high youth unemployment which stood at 17.5 percent as of 2012. The youth labour market in Morocco is marked by substantial regional disparities. As of 2012, unemployment rates among youth in the Oriental Region were the highest of any region in Morocco: 41 percent in urban areas and 21 percent in rural areas. Its most populous city, Oujda, has about 450,000 inhabitants making it the 12th largest city in Morocco. Economically, its in-land location differentiates it from larger port cities in Morocco, such as Casablanca, Tangier, and Agadir, which tie Morocco to Europe and Sub-Saharan Africa. Also, it is not a centre for tourism like Fez and Marrakesh. However, given Oujda's proximity to the Algerian border, it serves as a hub for trade between Algeria and Morocco. All this contributes to the Oriental Region being an area in which youth are facing significant labour market barriers.[2]

Aiming to provide a more inclusive approach to youth economic engagement in Morocco, Mennonite Economic Development Associates (MEDA) launched its YouthInvest project in Morocco in 2008. The YouthInvest project sought to promote better economic outcomes for Moroccan youth by bolstering access to financial services, building their capacity to manage their own finances, improving their job-relevant skills and employability, and encouraging youth to create their own employment solutions through entrepreneurship. The primary component of the larger YouthInvest project was MEDA's "100 Hours to Success" skills training course, the focus of this impact evaluation study.

The course was tailored to provide participating youth with a short but intensive training – totalling around 100 hours of engagement – and was open to all applicants between the ages of 15 and 25 that met basic literacy requirements. The training aimed to address three major skills areas that were identified through market research conducted at the beginning of the project with young people and employers. During this process, three knowledge gaps, prevalent among young people entering the labour market, emerged, namely financial literacy, life skills, and business and entrepreneurship skills. As a response, the training was designed to empower young participants by providing an experience that builds confidence and self-efficacy. On the technical side, learning to manage personal finances and to better access financial services as well as the fundamentals of starting an entrepreneurial activity were central. The training had three primary modules that were delivered in a combined fashion,

[2] MEDA implementing its YouthInvest project across Morocco from 2009 to 2014. Operations covered Casablanca, as well as smaller towns and their surrounding largely rural areas in the Northeast and Southeast regions of the country. Due to programmatic priorities and operational constraints, many of MEDA's other activities in Morocco under the YouthInvest project were suspended by 2012 but the 100 Hours to Success training continued to be rolled out in Morocco's Oriental Region. None of the study participants were involved in other activities or programmes offered by MEDA from 2009-2012 in Morocco as part of the wider YouthInvest project. More detail on YouthInvest, its individual components and its overall theory of change is provided the 3ie Final Grantee Report of this impact evaluation, available here: http://www.3ieimpact.org/en/evidence/impact-evaluations/details/2503/.

with trainers given flexibility about the order in which they delivered specific parts of each component. Youth were not able to opt in for any one specific component.

The first component focused on financial education providing participants with practical tools to help them manage their personal finances and, where appropriate, to understand how to set up simple financial management systems for a microenterprise. It included modules focused on personal budgeting, savings, debt management, knowledge about banking services, and financial negotiations. Teaching was based on materials developed under the Global Financial Education Program, adapted by local staff for youth in the Moroccan context, as well as to address the specifics of the local regulatory environment for financial services providers.

Second, the community engagement and life-skills component, adapted from materials developed by the International Youth Foundation, focused on improving personal competencies, problem solving, and conflict management. Built on role play and group work, the units aimed to help youth understand and manage emotions, develop confidence and assertiveness, manage and reduce stress, deal with problems and conflicts, and develop improved abilities to work with teams.

Third, the business and entrepreneurial skills component included modules based on a curriculum initially developed by Street Kids International, and modified by inputs from Save the Children. These modules were adapted to the Moroccan context by local staff based on input from youth themselves. Participants were guided through participatory exercises and role play activities designed to allow them to assess their own abilities regarding business development, to conduct market research for a business idea, and to plan a business (including the development of a pricing strategy, how to evaluate costs, and how to determine profit margins).

Within each component, the training was implemented in an activity-based manner, drawing on participants' experiences and knowledge. Rather than depending on traditional lectures the course relied on applied problem solving, working through live examples and case studies as its key method of imparting information. The training curriculum was designed to be delivered in a flexible manner, with specific class schedules adjusted around the identified needs of registered participants. Most classes ran over the course of three months. The normal time line for course delivery was two-hour sessions, twice a week for three months. A smaller number of courses were delivered over a month-long period.

To implement the training, MEDA developed training partnerships with youth-serving organizations across Morocco, as well as government institutions such as the Initiative nationale pour le développement humain. MEDA provided trained instructors called youth extension officers. For the period of this evaluation, training was provided at local youth centres around Oujda and its outskirts, as well as several centres located in more rural towns around Oujda (Jerrada and Taourirt). Individual participants applied through the centres closest to them, and upon enrolment, they attended courses at the closest local centre to ensure consistent attendance.

Overall, the intervention aimed to provide youth with a mix of services that will help them to steer through the school-to-work transition period. Envisioned outcomes for this objective include youth participants to be able to open bank accounts, build up savings and access other financial services, including lending for self-employment and education. At a later stage, young people would be enabled to be more active and find employment within the Moroccan labour market. This said, the intervention logic also rests on three main assumptions: (i) Morocco's basic education system is not providing young people with skills needed to be successful members of economic society and needs to be complemented with additional training; (ii) Morocco's economy is generating enough jobs on the demand side which are open to young people with the appropriate skills set; (iii) 100 hours of training can improve young peoples' skills that are relevant for being successful in economic society.

3. Research Design

Building on the above described intervention logic, this study examines specific hypotheses that relate to the average impact on training participants on two principal areas: first, financial knowledge and behaviours and, second, labour market outcomes and educational choices of participants. Regarding the first area, the study aims to understand better whether youth that participated in the training could demonstrate heightened understanding regarding the functioning and provision of financial services, were better able to manage their personal finances, more likely to maintain a savings account, and showed increased levels of activity with respect to saving and borrowing – for example through securing loans for personal or business use.

With respect to the second outcome area, the study explores whether the combined approach to training (life skills, financial literacy, and entrepreneurship training) provided by the intervention increased the likelihood of (self-)employment and led to longer employment spells. Furthermore, the study documents whether and how the training impacted educational choices of participants (for example through shortening or prolonging their educational career, seeking a higher level of formal education or trying to integrate as quickly as possible in the labour market).

Moreover, the study examines whether the training had differential impacts on selected sub-groups. The study seeks to understand gender impacts as well as differences associated with socio-economic backgrounds. For example, those with better socio-economic standing might have social and family networks that they could better employ to secure employment. At the same time, however, those from comparably wealthier backgrounds are also more likely to maintain higher reservation wages and, as such, to be more selective in the job offerings that they seek and accept.

3.1 Evaluation design

Obtaining unbiased impact estimates is among the greatest challenges for any evaluation that wants to quantify the *causal* effect of an intervention on the outcomes of interest. Evaluation designs that simply compare programme beneficiaries to a group of non-participants do not account for the fact that these groups are often very different from each other. Some of the differences – for example motivation, skills or innate ability – may be hard or impossible to observe. These evaluations are likely to produce incorrect estimates, plagued by what is commonly referred to as endogeneity bias (Cameron and Trivedi 2005; Imbens and Wooldridge 2009).

The evaluation of “100 Hours to Success” was designed as an RCT. In principle, randomized sorting allows for an accurate estimate of the counterfactual of training participation. On an aggregate level, control and treatment groups should be comparable in terms both of observable and non-observable characteristics before the start of the programme. Thus, any differences in outcome averages between the control and treatment group that are observed after the implementation should be attributable to the training.

Power calculations for this study were based on outcomes relating to savings behaviour and employment. They primarily relied on monitoring data from MEDA, World Bank data (World Bank 2012), and an evaluation of the INJAZ programme in Morocco (Reimers et al. 2012). With an envisioned sample of around 1,800 youth, 600 of whom in the treatment group, the study was projected to have sufficient power to detect eventual programme impacts for the whole sample as well as for a number of subgroups even in the presence of moderate attrition rates.[3]

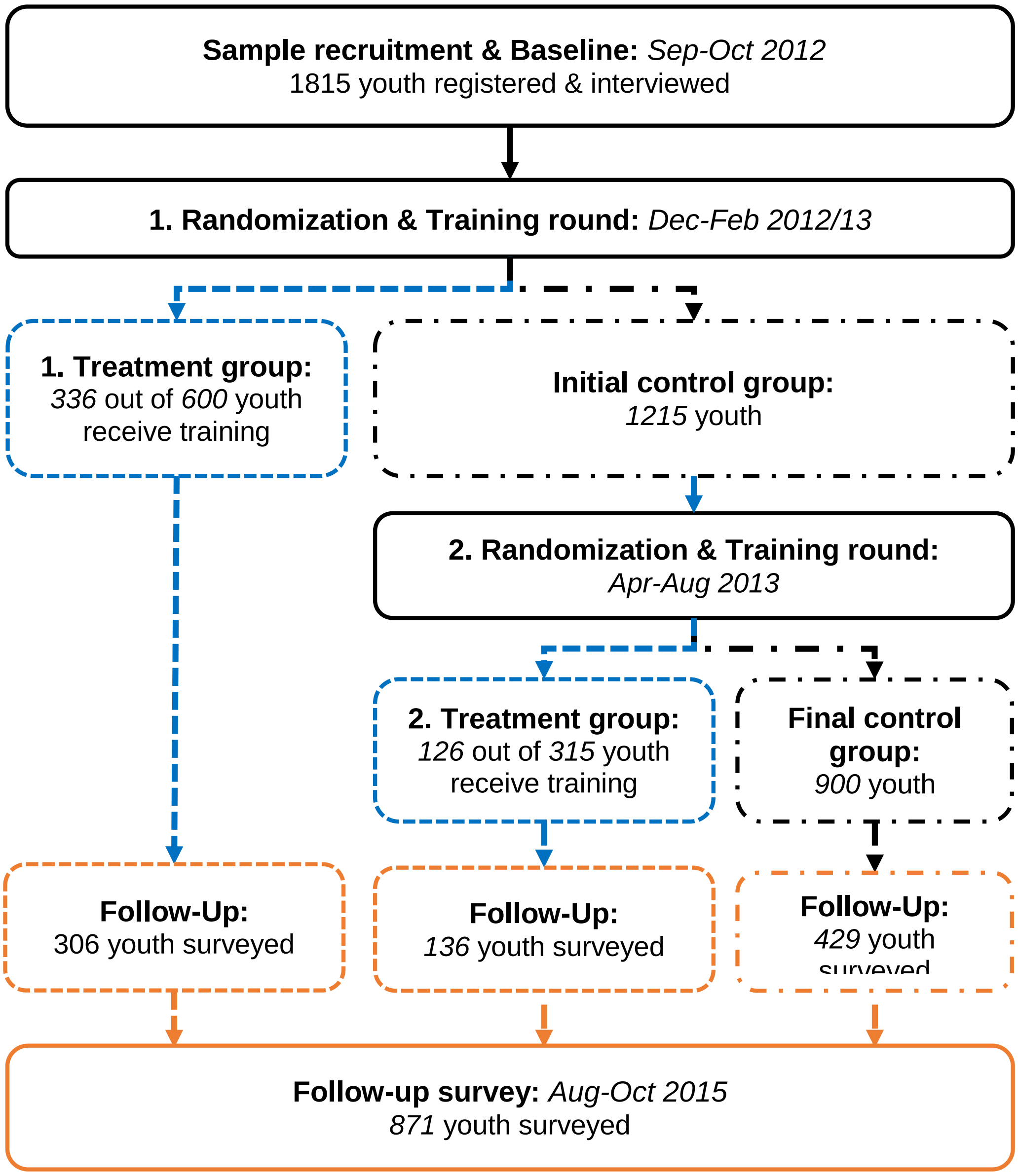
Figure 3-1 summarizes the different stages of the impact evaluation. Targeting the inclusion of 1,800 individuals in the study through applications submitted by youth, the promotion of “100 Hours to Success” started in September 2012. MEDA worked with established youth centres, vocational training centres, schools and universities in the Oriental Region. Applicants could register their interest in participating prior to the launch of the study. Through this recruitment drive, 1’815 young people applied for the training and participated in a baseline survey.

After the completion of the baseline survey in November 2016, 600 youth were randomly sorted into a treatment group, with the 1’215 remaining youth being in the initial control group.[4] While the randomization was not carried out in public, applicants interested in the training were informed at baseline that slots were limited and that selection of participants would be carried out by random assignment. Starting at the end of November 2012, youth in the treatment group were invited to enrol in the training.

[3] Detailed power calculations are presented in the baseline report (Dyer et al. 2015).

[4] Randomization was done on the level of the individual. Initially, the research team considered randomization at the cluster level (e.g., by using catchment areas around youth centres) due to the fact that training would take place in youth centres and that classes at each youth centre would only be able to run with a minimum number of participants from each centre. Due to a relatively small number of potential clusters, this would have resulted in our study not having enough statistical power. In applying the initial random sorting, we found that there were significant numbers of participants in each youth centre to move forward with the courses. As the percentage of youth assigned to the treatment group in each youth centre differs slightly across centres, we control for these small differences by using youth centre as a control variable (one dummy per centre) in our statistical analysis (see also Section 5).

Figure 3-1: Evaluation design



By March 2013 it became clear that take-up rates were low. Only 336 (out of 600) youth enrolled in the training.[5] Among others this was ascribed to scheduling conflicts and some lack of interest once classes began. Also, a delay in rolling out classes meant that some members of the treatment group had moved on to other opportunities. Also, some youth within the treatment population had expected to take training with their friends; having found that training was not made available to their friends (in the control group), they opted out. As such, some of the poor take-up might have been a result of the imposition of treatment and control structures.

Understanding that the study would not have enough power to demonstrate an impact on the desired level of detail, the research team undertook a second random sorting of individuals

[5] Herein, by enrolment, we mean that they attended at least one training session. In fact, a considerable number of those that enrolled in the training did not attend all sessions. Section 0 discusses low overall take-up and its implications on evaluating the impact of the program.

from the control group and invited them to participate in the training. More precisely, the research team placed 315 randomly chosen individuals, previously assigned to the control group, in the treatment group, with the final total treatment group totalling 915 individuals and the control group totalling 900 youth. Limited take-up remained a challenge as only 126 of the 315 individuals allocated to treatment through this second wave of randomization enrolled in the training.

3.2 Data collection

The primary instruments used for data collection in our study are a baseline and a follow-up survey. During both data collection rounds, no incentives – monetary or otherwise – were provided to respondents. In additional a qualitative assessment based on structured interviews and focus groups was conducted with a small number of participants between December 2014 and January 2015.[6]

The baseline survey collected detailed contact information on respondents, as well as friends and family of the respondents to help with tracking during the follow-up survey. A Moroccan data collection company, called Sunergia, administered the baseline survey under supervision of the research team and after having been trained on the survey instruments. The research team was also in the field to observe and supervise field operations. Baseline data was collected at the youth centres to which applicants had applied for the "100 Hours to Success" training. Respondents were asked to come to the centres at pre-arranged times and enumerators engaged respondents in either French or in the local dialect of Arabic to ensure understanding.

For the follow-up survey, taking place from August-October 2015, another survey fielding company called SOLAB was hired. SOLAB is affiliated with the director of the National Institute of Statistics and Applied Economics who oversaw the data collection process. Members of the research team were present in Morocco at the start of data collection, meeting in Rabat with SOLAB and the enumerators for three days to train them on the questionnaire and the methodology. Members of the research team also observed data collection during the first few days of fieldwork. During data collection for the follow-up, enumerators attempted to call baseline respondents up to three times to arrange meetings. For those not reachable by phone or for whom no telephone number had been recorded at baseline, enumerators would visit the addresses recorded at baseline. All in all, 871 youth were interviewed in person during the follow-up survey that took place more than two years after the intervention ended.

When enumerators were not able to contact a study participant directly in the follow-up survey, despite multiple efforts, they attempted to interview a household member, neighbour or friend instead to obtain proxy data on whether the study participant was enrolled in education or training and on current labour market status. Enumerators obtained proxy data for 493 additional individuals. However, due to serious concerns about the validity of these data, we

[6] The qualitative study was conducted using focus group discussions with beneficiaries and interviews with trainers, one key partner (the manager of a community centre in Oujda) and young people who dropped out of the program. A total of eight focus group discussions were held (involving 30 participants, of whom 11 were male and 19 were female). All the participants had completed the training in 2011, 2012 or 2013. Thirteen interviews with key participants were also conducted with four trainers from MEDA, the key partner who provided the training centre in Oujda, and eight young people who dropped out of the program. The study was undertaken in partnership with the Swiss Academy for Development and the results were published online, see ILO (2015a).

limit our analysis to the 871 observations that were obtained through face-to-face interviews with the study participants.[7]

4. Description of study sample

Our analysis builds on data from both the baseline and the follow-up survey. After cleaning the baseline data for data entry errors and incomplete responses, 1803 out of 1815 observations remain (referred to hereafter as the baseline sample). Of these 1803 individuals, 891 were assigned to the control group and 912 to the treatment group. We have follow-up data available for 871 individuals (refereed to hereafter as the endline sample) who were interviewed in person, amounting to an overall attrition rate of 51.7 percent. Of those, 427 had been randomly assigned to treatment while 444 had been assigned to control. The high rate of attrition reflected in these figures is a concern that we address in great detail below (see Sections 0 and 5.3).

In this paper, we present baseline characteristics only for those individuals for which endline data are available as well.[8] Table 4-1 presents baseline values for a range of background variables, namely demographics such as age, gender and whether the respondent lives in an urban area, as well as information about the participant's household (number of siblings, indicator for living in a dormitory at the time of the baseline, indicator whether the father is still alive at the time of the baseline, self-reported satisfaction with overall household situation) and, in particular, the household head (gender, level of education at time of the baseline). Furthermore, we include a household asset index that is based on a principal component analysis of 23 self-reported durable household assets, such as number of telephones, cars, and availability of a refrigerator.[9] Finally, we add an indicator whether the respondent already participated in any skills training programs.

As Table 4-1 shows, 48 per cent of study participants reached in the follow-up survey were women and at baseline on average 20 years old. Overall, 78 per cent were living in urban areas and 16 per cent were living in a dormitory – reflecting a substantial share of young

[7] Enumerators had to record from whom they obtained these proxy data with "parent", "family member", "friend", "neighbour" or "someone else (other)" as options to select from. In 293 out of 493 cases where proxy data were collected, enumerators indicated that they obtained the data from "someone else" (in many cases themselves or their own family members). Taking a closer look at these 293 observations reveals that in only 1 percent of cases, study participants are said to be enrolled in education or training compared with 52 percent in the sample of 871 respondents at the time of the follow-up survey. This seems unlikely to be true. Rather, these data entries appear consistent with interrogators ticking mechanically "no" to the few questions in this section. This also question the data quality for the remaining 200 cases where a parent, friend or supposedly answered. Inspection of the data reveal very large differences between this small group (i.e. the remaining 200 individuals for which proxy information are available) and all youth that respondent to survey questions in persons. In particular, youth are dramatically less likely said to be enrolled in education or training (51 percent vs. 25 percent), or to be employed (26 percent vs. 20 percent), while being significantly more likely to be NEET (31 percent vs. 41 percent). These results are not just compositional effects as cited differences barely change upon inclusion of covariates such as gender, age and household assets.

[8] This section does not attempt to provide a complete descriptive analysis of the full sample which can be found in the baseline report (see Dyer 2015 et al.).

[9] First, we conduct a principal component analysis using all 23 self-reported durable household assets. Then, we predict individual scores that are then scaled through a linear transformation such that the highest score assumes the value 10 and the lowest score the value 1. The resulting asset scale has mean 4.02 (median: 3.94) with a standard deviation of 0.91.

students or workers living outside their parental household. Still, the average study household size was around five persons. Only 13 per cent of respondents had previously participated in a skills training programme outside the formal educational system in the past. Table 4-1 also displays differences between control and treatment groups. Differences are generally small and none are significant at the 95 per cent confidence level. However, individuals in the participant group are less likely to reside in urban areas and less likely to live in a female-headed household (both significant at the 90 percent level).

Table A-2 in the appendix shows baseline values for the sample that was surveyed as part of the follow-up survey for several outcome variables. Almost nine out of ten (89 per cent) of youth were still enrolled in education at the time of the baseline survey, roughly one out of five (21 per cent) indicated to have an own savings account and almost half of the survey respondents said that they regularly saved money (49 percent). Differences between control and treatment groups are small and not significant at any conventional level. While this is only a preliminary test, it shows that even given substantial attrition rates, the treatment and control group appear roughly similar regarding observable characteristics. In addition, Table A-1 and Table A-3 describe characteristics for background and outcome variables for the baseline sample. This is a strong sign that – for at least the full sample at baseline – randomized sorting indeed resulted in two groups that are sufficiently similar on observables.

Table 4-1: Control variables at baseline, endline sample (N=871)

	Mean			
	Full (N=871)	**Control (N=427)**	**Δ(Treat-Control)**	**p-value**
Gender (1=female)	0.475	0.489	-0.028	0.413
Age	19.979	20.014	-0.068	0.720
No. of Siblings	3.744	3.703	0.081	0.558
Urban	0.781	0.808	-0.053	0.057*
Living in dormitory	0.164	0.157	0.014	0.571
No. of HH members	4.912	4.972	-0.118	0.397
Female HH head	0.126	0.148	-0.042	0.064*
Education level – HH head (0-6)	1.642	1.644	-0.004	0.971
Father alive	0.902	0.895	0.015	0.448
HH assets (1-10)	4.055	4.071	-0.030	0.605
Satisfied w/ HH situation (1-4)	2.863	2.836	0.054	0.236
Attended other skills training in past	0.133	0.136	-0.005	0.822

Notes: the first column presents averages for all observations, the second column for the control group and the third column differences between the treatment and the control group. The last column contains p-values for a two-sided test of equal means between the control and treatment group; */**/*** statistically significant at 90%/95%/99% confidence level.

Finally, when comparing our study sample to the overall youth population in Morocco's Oriental region – using a representative World Bank survey[10] - our survey population tends to be younger. Those aged 15–21 are more heavily represented in the pool of study participants, which is natural given that applicants are – for the most part – just beginning the transition from school to work. While our survey population is generally younger than the population included in the World Bank sample, they are better educated – and on the pathway to becoming even more educated in the future.

4.1 Attrition

Attrition is always a concern for internal validity of evaluations, especially when attrition rates are as high as those observed here. It is even more problematic when attrition is not balanced between the control and treatment group, so-called *differential attrition*. In this case, attrition might invalidate the randomized research design by making treatment and control groups incomparable. However, attrition rates in the control (52.1 per cent) and treatment group (51.3 per cent) are almost identical with the difference being non-significant (p-value: 0.75).[11] Thus, even though this study faces a comparably high attrition rate overall it does not seem plagued by differential attrition.

In this section, we further investigate whether individuals who drop out of the sample differ from those who were observed twice (baseline and follow-up), based on suggestions from the literature (Duflo et al. 2008). This can be done in at least two ways. First, we can produce balancing tables that compare control- and outcome-variables between both groups one by one. While this gives a first impression about whether there are variables able to predict attrition, it does not consider potential correlations between these variables. Therefore, we focus on the second approach, estimating a multivariate regression that tries to predict attrition taking into account all included variables simultaneously.

Table 4-2 displays the results of regressing a binary variable, wherein all individuals who were observed twice ("non-attriters" on assignment) receive the value 1, on several background variables and treatment assignment status (in a probit model). Attrition correlating with assignment status would be particularly worrisome as it might indicate that the treatment itself changed individuals' propensity to leave the study. However, there is no relationship between *assignment* to the participant or comparison group and the probability of being interviewed in the follow-up survey. This holds true in a simple model (column 1) and when subsequently adding controls (column 2). Importantly, the fact that there are four variables that are correlated with whether a study participant could be interviewed during the follow-up survey does not generally affect the internal validity of the study. For example, all else equal, a female participant is – depending on the specification – between 6.6 and 8.4 percentage points less likely to be part of the follow-up survey than a typical male participant. Consequently, the sample observed at the follow-up is no longer fully comparable to the baseline sample. This affects the external validity of the findings: The endline sample differs from the baseline

[10] The baseline report (Dyer et al. 2015) assessed to what extent the youth from our sample might be representative of youth from the Oriental Region in Morocco. To this end, it compared characteristics of this study sample with data from the 2009 World Bank survey of young people in Morocco (World Bank 2012).

[11] Note that also when splitting the sample according to actual treatment status the difference remains small and insignificant: 50.9 per cent among all individuals that are considered treated and 51.9 per cent among all other study participants (p-value: 0.73). For the definition of treatment and a discussion around treatment status compliance see also Section 0.

sample and can therefore not capture the population represented at baseline. However, this is of little importance in this study, as the baseline sample of the study was not representative in any case. While the study sample might reflect well characteristics of those Moroccan youth that are interested in skills trainings, it cannot be claimed that it is representative for the entire Moroccan youth population.

Table 4-2: Determinants of inclusion in follow-up survey (N=1803)

	Dependent variable: Individual observed in follow-up			
	Probit models (average marginal effect)			
	(1)	**(2)**	**(3)**	**(4)**
Assignment (Treatment =1)	**0.008**	**0.007**	**0.013**	**0.011**
	0.024	0.023	0.095	0.095
Female		**-0.084*** **	**-0.069** **	**-0.066* **
		0.023	0.034	0.034
Urban		**0.064** **	**0.115*** **	**0.113*** **
		0.027	0.039	0.040
Living in dormitory		**-0.070** **	**-0.088** **	**-0.088** **
		0.030	0.043	0.043
Father alive		**-0.086** **	**-0.127** **	**-0.126** **
		0.042	0.060	0.059
Female*Assign			**-0.028**	**-0.031**
			0.047	0.047
Urban*Assign			**-0.100* **	**-0.097* **
			0.054	0.055
Dormitory*Assign			**0.037**	**0.039**
			0.060	0.060
Father Alive* Assign			**0.084**	**0.086**
			0.084	0.084
Full set of controls	**No**	**No**	**No**	**Yes**
N - Persons	1803	1803	1803	1803

Notes: we estimate probit models, computing average marginal effects and corresponding robust standard errors, displayed below impact estimates; */**/*** statistically significant at 90%/95%/99% confidence level.

The main concern in this study is therefore internal validity. It is threatened only if estimates if the propensity to leave the study differs between treatment and control group. Thus, in column (3) and (4) we present results for specifications including interaction terms between those four variables that are informative in predicting attrition an assignment status.[12] We find that female participants who were part of the treatment group are *not* significantly less likely to be observed during the follow-up compared with women that were assigned to the control group. In fact, the only interaction term significant at the 10 percent level relates to individuals

[12] The full set of control variables include all control variables listed in Table 4-1 that are not already included in the specifications in columns (2) and (3). None of these additional variables show an impact significant at conventional confidence levels.

assigned to the treatment group that live in urban areas. The coefficients in column (4) imply that (only) study participants in the control group that live in urban areas are less likely to be interviewed during the follow-up survey. With this one exception, we do not find further indications that selective attrition might have affected the internal validity of the study.

We attempt to correct for potential biases due to differences in the probability of being observed twice for urban study participants in the control group by using inverse probability weighting as a robustness check for our results in Section 5.3.

4.2 Take-up

As stated above, low programme take-up is of concern in this study. Out of the 900 youth that were randomly chosen to form the treatment group, 469 individuals started the training programme (see Figure 3-1). However, administrative data show that among this group a considerable number did only attend some of the training sessions. Consequently, the average attendance rate for a person initially assigned to the treatment is around 35 per cent (36 percent when restricting to those included in the follow-up survey). For our analysis, we consider all youth attending at least half of the sessions as treated, provided they attended at least one session of the second half of the course.[13] Per these criteria 352 youth (39 per cent of those initially assigned to the treatment group) are considered treated.[14]
Low take-up rates do not introduce bias in the analysis but limit the interpretation results in two aspects. Firstly, rather than observing the effect of the treatment, average *intended* impacts of the programme on the study population are estimated (as well as average impacts on those considered treated). Secondly, low take-up reduces the power of the study making it harder to detect smaller impacts across outcome categories. Although low take-up rates are hampering the precise estimation of programme impacts, studying them facilitates a meaningful interpretation of the study's results. Differential take-up by background can shed light on how and why the effects of the treatment differ as well.

Table 4-3 shows that female, older and more affluent individuals were more likely to take-up the intervention while coming from a household with more assets reduced the likelihood of participations. These results hold both when treatment is modelled with a binary dummy per the above definition (i.e. having attended at least half of the classes offered) and for specifications where the treatment intensity is taken as dependent variable. We report only significant effects, although all other background factors are included for the estimation.

When looking at differential take-up, the following patterns emerge: Firstly, it seems that trainings are most appreciated by those individuals who are older and thus more likely to apply their knowledge in financial activity and entrepreneurship. Although we cannot determine in our sample which individuals are at the end of their educational cycles and likely to decide about potential entrepreneurial activities and explore options of employment, it seems that

[13] In this paper, we refer to this group as *treated individuals,* not to be confused with the *treatment group* that consists of all individuals who were *randomly assigned* to receive the treatment.
[14] More precisely, 360 youth attended at least 50 per cent of the sessions offered to them and 8 are considered as dropouts due to non-attendance in the second half of the course. According to MEDA's own initial requirements, participants were expected to attend at least 75 percent of classes to successfully graduate. However, it should be noted that MEDA did not necessarily hold to this requirement for the cohort of youth participating in the study given overall poor attendance rates. Thus, many graduated who may have missed more than a quarter of the classes, but still showed an interest in continuing the training.

offering training to these groups would be especially worthwhile. Especially for a short training like "100 Hours to Success" it seems overly optimistic that individuals still having to complete several years of schooling before becoming economically active, will remember and apply this knowledge such much later in their lives. Secondly, women are around 10 percentage points more likely to take up training across a variety of specifications. Finally, programme take-up seems not primarily driven by initial labour market status or enrolment in education. For both variables estimated coefficients are negative – perhaps suggesting that youth following full time education or working are less able or willing to commit to an additional training course – but not statistically significant. Taken together, our analysis suggests that subgroup dynamics play an important role in our sample and we will pay special attention to exploring differential impacts across main socio-demographic groups in Section 5.

Table 4-3: Take-up determinants, treatment group (N=912)

	Dependent variable:			
	Binary take-up **Probit**		Continuous take-up **OLS**	
Gender	**0.109*** **	**0.099*** **	**0.074*** **	**0.065** **
	0.031	0.033	0.026	0.027
Age	**0.016*** **	**0.016*** **	**0.011** **	**0.010** **
	0.006	0.006	0.005	0.005
Urban	**-0.039**	**-0.032**	**-0.045**	**-0.040**
	0.036	0.039	0.031	0.032
Assets	**-0.042** **	**-0.042** **	**-0.036** **	**-0.036** **
	0.020	0.020	0.015	0.015
Attended other skills training in past	**0.084* **	**0.081* **	**0.065**	**0.061**
	0.047	0.047	0.041	0.041
Enrolled in education		**-0.043**		**-0.057**
		0.050		0.042
Employed		**-0.054**		**-0.036**
		0.061		0.049
Full set of controls	Yes	Yes	Yes	Yes
N	**912**	**912**	**912**	**912**

Notes: Full set of controls contain also whether participants are living in a dormitory, number of siblings, whether the father is alive, whether the household head is female, the educational level of the household head, self-reported satisfaction with household situation; in the first two columns we use the binary definition of drop-out where more than 50% percent participation counts as treated; in the second two columns we use the actual participation rates (#classes attended / #classes offered) as the dependent variables; for the probit model we report average marginal effects; robust standard errors below impact estimates; */**/*** statistically significant at 90%/95%/99% confidence level.

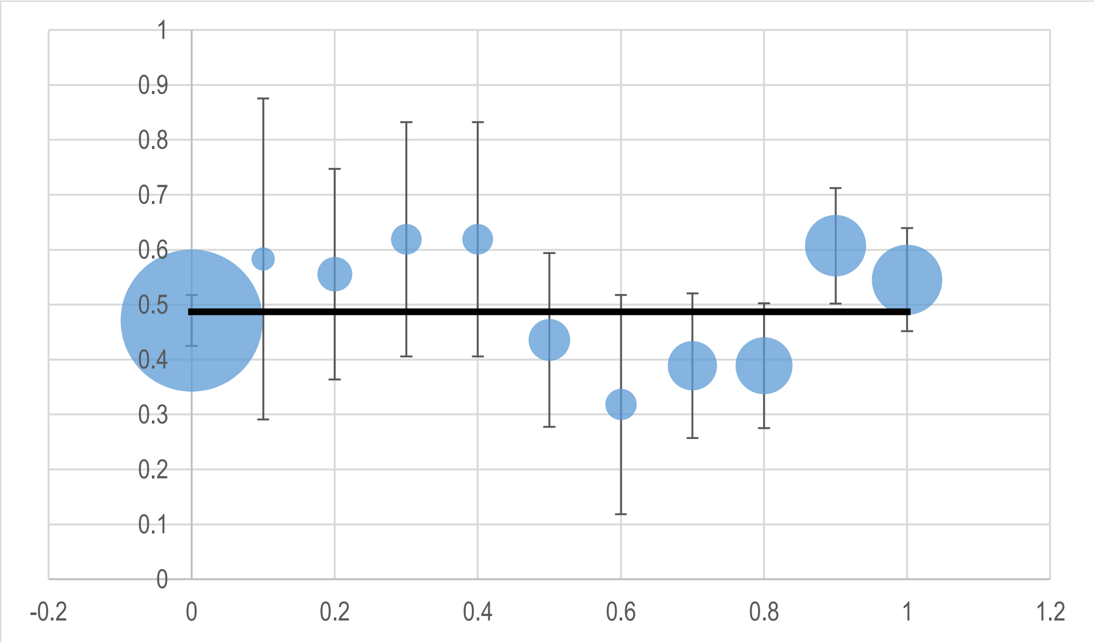
Table 4-4: Compliance in follow-up sample (N=871)

Treatment assignment	**Actual treatment status**		
	Not-Treated	Treated	Total
Assigned to control	99.53%	0.47%	100.00%
	(425)	(2)	(427)
Assigned to treatment	61.49%	38.51%	100.00%
	(273)	(171)	(444)
Total	80.14%	19.86%	100.00%
	(698)	(173)	(871)

Note: absolute numbers in each group in brackets

As mentioned in the section on attrition, non-differential attrition is crucial to obtain unbiased estimated. Whether an individual receives treatment should not affect whether they drop out. Figure 4-1 explores this relation between attrition and take-up. For each level of attendance (X axis), the corresponding percentage of youth included in the follow-up survey (Y axis) is depicted.[15] It clearly shows that youth attending more classes do not show a significantly higher propensity to complete the follow-up questionnaire. Attrition rates vary unsystematically around the average (51.3 percent) for all those assigned to the treatment group. Participation in the follow-up survey seems unlikely to be driven by treatment status or treatment intensity.

Figure 4-1: Observation rate in follow-up survey by treatment intensity, treatment group (N=912)



Notes: we plot the likelihood to be included in the follow up survey (Y axis) against treatment intensity by groups; the size of the circles represent are proportional to the number of observations in the respective group; 95 percent confidence intervals are plotted for all groups; the black line represents the average follow-up inclusion rate (48.7 percent) for the treatment group.

[15] This is one minus the attrition rate.

5. Results

When sorting in the treatment group is randomized, it is sufficient to compare average outcome levels between the treatment and control group to identify causal effects (Angrist and Pischke 2009). We augment this basic empirical specification in two ways. First, as we recorded most outcome variables not only during the follow-up survey but also in the baseline questionnaire, we compare differences in outcomes over time between the control and treatment group.[16] Through this difference-in-differences approach (DID) we control for any differences between control and treatment group that stay constant over time. Secondly, we include a set of (time-varying) control variables to correct for remaining small differences in observables which is expected to improve the precision of our estimates.[17]

We thus estimate:

$$y_{it} = \beta\, Treat_i * Post_t + \alpha_1 + \alpha_2 Post_t + \boldsymbol{X_{it}}\gamma + \eta_i + \varepsilon_{it} \quad (1)$$

where y_{it} is the outcome variable for individual *i* at time *t* (0: baseline, 1: follow-up). The coefficient α_1 represents the average for the control group at the time of the baseline (conditional on covariates and fixed effects), α_2 describes the average difference between baseline and follow-up for the whole sample. Moreover, $\boldsymbol{X_{it}}$ captures time-varying, individual and household characteristics[18], while η_i is an individual fixed-effect term that controls for all (observed and unobserved) factors that are time-invariant,[19] and ε_{it} is the idiosyncratic error term that describes variation in the outcome variable which is not explained by the model. Finally, β is the parameter of interest, capturing the average effect of the intervention. Note that in equation (1) $Treat_i$ is an indicator that takes the value one for all youth who participated in the program – and zero otherwise. To the extent that individuals are not complying with their initial treatment status it differs from treatment *assignment.*[20]

Any interpretation of impact coefficients – for the whole sample and when disaggregating by subgroups – must consider the study's final sample size. While the original study design allowed for over 1800 respondents, our our ability to elaborate on the nuances of financial behaviour and labour market outcomes are limited by an attrition rate of just over 50 percent. Additionally, non-compliance among some of the treated population diluted the power of the study. Keeping in mind the considerable non-compliance rate, we present and compare three different effect types.

[16] We use the expression *comparison group* and *control group* interchangeably.
[17] The balancing tables presented in Section 4 show that control variables do not differ significantly between treatment and comparison group. Adding them in equation (1) thus first and foremost increases statistical precision.
[18] These are: dummy for living in an urban/rural area, household size, self-assessed satisfaction of household situation and a dummy for whether the individuals participated in another skills training programme in the past.
[19] Note that as we added an individual fixed effect, we do not need to include an indicator for participants in the treatment group anymore. In specifications without individual fixed effect, the coefficient of this dummy would amount to the average difference of the outcome variable between treated individuals and the rest of the sample at the time of the baseline. In an experiment where randomization "worked", we would expect estimates to be close to zero. Indeed, the balancing for outcome variables that were already observed during the baseline show that differences are small and statistically insignificant – see Table A-2 in the appendix.
[20] When mentioning "control" and "treatment" groups, unless explicitly stated, we refer to individuals that were randomly assigned to be in the control group and treatment group, respectively. When mentioning youth that we consider as having participated in the training, we refer to "treated youth".

First, we estimate *average treatment effects on the treated (ATT)* using ordinary least squares (OLS). Often, the ATT is the estimator most relevant for policy makers and practitioners: it captures the average effect of an intervention on actual participants. Thus, impact estimates are based on a comparison between all youth that underwent the training and everyone else, irrespective of initial random assignment. However, actual participation in the training is likely not to be random, so that this estimate is likely to be biased. Participants could, for example, choose (not) to attend the course depending on their expected benefit from the intervention. While our OLS estimator controls for several observed time-varying and all unobserved time-invariant characteristics, we cannot exclude the possibility that youth that chose to attend the training fundamentally differ from those that did not.[21]

As obtaining accurate OLS-based estimates of ATTs in our setting appears unlikely, we also present two other estimators that are supposed to overcome self-selection bias and exploit the random treatment assignment. Our second set of impact estimates is based on a modified version of equation (1) where the dummy representing actual treatment status is replaced with a variable representing initial (random) treatment assignment. Simple OLS estimation delivers *Intention-to-Treat (ITT)* effects. ITT estimates are based on a comparison between the assigned treatment and control group and do not consider whether individuals actually did (not) participate. Thus, different from ATT estimates, intention-to-treat effects capture the average effect on individuals from the population that were *intended* beneficiaries.

Because ITT coefficients do not allow for a gauging of the direct effect on individuals who were treated, we present a third set of impact estimates: we estimate *local average treatment effects (LATE)* by instrumenting actual treatment with treatment assignment using the two-stage least squares estimator where (1) represents the second stage and the first-stage regression is given by:

$$Treat_i * Post_t = \delta_0 + \delta_1 Assigned_i * Post_t + \boldsymbol{X_{it}\rho} + \eta_i + \omega_{it} \ (2)$$

This procedure relies on the (testable) assumption that while participation in 100 Hours to Success training is likely not random (but driven by self-selection), offering the program to some people at random increased their inclination to participate. Despite a substantial share of study participants that did not comply with treatment assignment (Section 4.1) random assignment status easily passes conventional tests for instrument relevance. Youth assigned to the treatment group were on average 38 percentage points more likely to participate in the training programme than individuals in the control group.[22]

In general, local average treatment effects have a restricted interpretation: they are estimates for the average effects on the so-called *compliers*, individuals that participated in the training *because* they were assigned to the treatment.[23] For this study, as there is close to zero non-

[21] "Fundamentally different" in this context implies that conditional on control variables treatment and comparison group on average show different *time trends* in the outcome variables under consideration.

[22] We formally test for instrument relevance by regressing actual treatment status on random assignment status as well as a full set of control variables and individual fixed effects. Individuals assigned to the treatment group are 38.1 percentage points more likely to participate in the training (p-value: <0.001). As a rule of thumb, instruments are considered relevant if the F-statistic on the joint significance exceeds 10. The F-statistic for this specification equals about 254.

[23] Intuitively, LATE estimates are obtained in two steps. First, we calculate differences between all individuals *assigned* to the treatment and control group. This corresponds to the ITT effect described above. Second, this

compliance in the control group, our local average treatment effects conceptually coincide with average treatment effects on the treated. Thus, substantial differences between ATT and LATE estimates could be an indicator that one (or both) techniques produce biased results.

Taken together, we will thus present, compare and discuss (i) average treatment effects on the treated, (ii) intention to treat effects, and (iii) local average treatment effects for a range of outcome variables. Before presenting and discussing detailed results on a range of outcomes, there are three more general findings that should be taken into account when comparing ATT and LATE estimates. First, ATT and LATE estimates always show the same sign for variables where at least one coefficient reaches statistical significance. Second, in almost all of these cases, OLS estimates indicate a more positive impact than the IV approach, which is likely due to selection bias in program take-up: it seems that those individuals who would have had better outcomes with respect to financial literacy and behaviour and labour market status where more likely to take up the intervention. Third, while LATE coefficients conceptually appear more reliable in terms of bias, the IV approach does come at the cost of reduced power as this statistical design focuses on exploiting variation among compliers. We observe that standard errors of impact estimates are consistently around twice as large when comparing the standard OLS estimates with the IV approach. This implies that we only obtain statistically significant impact coefficients for considerable strong treatment effects.

Regarding inference, we present standard errors that are robust with regard to heteroskedasticity.[24] Moreover, several outcome variables were only observed in the follow-up questionnaire. For these outcomes (that are clearly flagged in the result tables[25]) impact estimates are based on adapted versions of equations (1) and (2) that also include also a full set of time invariant control variables[26] as well as 13 youth centre dummies (for centres where the baseline survey took place) to control for small differences in geographical origin.

Following the main results for the whole observed study population, we disaggregate the sample by gender, older and younger participants, and study participants from more and less affluent households. This subgroup analysis aims to uncover potential heterogeneous impacts. It is important to highlight that this analysis is severely limited by the high attrition rate and the considerable degree of non-compliance among those assigned to treatment, which greatly diminishes the power of this exercise. Thus, we limit the number of dimensions along which we split our sample and avoid disaggregation criteria that would lead to a very uneven

difference is scaled up with a factor that depends on the compliance rate of all study participants. This factor will be larger (i) the higher the proportion of individuals assigned to the control group that got treated nevertheless – so-called *always-takers,* and (ii) the higher the proportion of individuals assigned to the treatment group that did not participate in the training – so-called *never-takers.*

[24] An alternative would have been to cluster standard errors to take potential intra-cluster correlation into account when assessing the statistical uncertainty of impact estimates. This could be done for example on the level of the youth centre (i) where study participants completed the baseline survey or (ii) where they took part in the 100 Hours to Success training. However, consistency (asymptotic properties) of cluster stand errors is based on a large number of clusters and, as Donald and Lang (2007) show, they do not tend to have desirable properties for small number of clusters. Our dataset only features 13 distinct youth centres serving as centres for baseline data collection, with more than 95 percent of observations coming from only 10 centres, which cautions against clustering at this level.

[25] tables display whether the variable in question was observed before and after the intervention or only recorded in the follow-up questionnaire: the column labelled "DiD" indicates whether a difference-in-differences model was estimated for this outcome variable in which case data for both the baseline and the follow-up survey were available

[26] That is all variables listed in the balancing tables in Section 4.

split.[27] Furthermore, we avoid altogether disaggregating the sample along two subgroup criteria at the same time (i.e., young *and* female or more affluent household *and* male).

Finally, when splitting the sample into subgroups, we only report LATE estimates. As mentioned above, their interpretation comes close to an average treatment effect on the treatment. Moreover, we believe LATE estimates to be more credible than OLS impact coefficients that are likely plagued by selection bias. With all these limitations in mind, we should regard results for subgroups as an indication of how potential treatment effects might vary along gender and household asset dimensions but not expect to obtain precisely identified point estimates.

5.1 Financial behaviour

When studying financial literacy and behaviour of participants of 100 Hours to success, it is important to note that for some outcomes the threshold for change is lower than for others. Whether an individual has a bank account is one of these outcomes that are likely to change, as participants were strongly encouraged (but not mandated) to open a saving account, upon enrolling in the training. Notably, the rate of youth having a bank account among our control group is quite high (35.6 percent) given the youth of our target population and the lack of financial services for youth across Morocco. The World Bank found in its 2009-2010 survey of Moroccan youth that about 12 percent of youth maintained savings accounts in a formal financial institution (World Bank 2012). Relative to the overall youth population, there is thus less room for improvement due to the already high access to bank accounts in the sample.

We estimate a large and highly significant effect of the training on the probability to maintain a saving account. The ATT specification (Table 5-1, column 1) estimates that participants are 15 percentage points more likely to have a savings account than those who did not participate. The LATE is even larger with 27 percentage points, an estimate that is significant at the 1 percent level. For this particular outcome, the substantial difference between the two estimates is likely to be driven by the definition of treatment: only youth that participated in more than fifty percent of the classes were considered as treated, while also those that attended only a few classes might have been sufficiently encouraged to open a bank account. A closer inspection of the data reveals, that among youth that attended at least one training session about 54 percent have a saving account compared to only around 36 among those assigned to the comparison group.
For this broad definition of actual treatment status, the LATE estimate reaches 19.7 percentage points (p-value: 0.009). As this approach reduces exposure to the training to a minimal extent this latter estimate can be regarded as a conservative lower bound relative to the 27 percentage points reported in Table 5.1.

One could argue that the effects on opening a bank account are almost mechanical, as relatively little initiative from participants is required for them to materialize. Still, the strong effects show that youth participating in the training complied with the recommendations and encouragement given. Furthermore, it is worthwhile to note that youth seem to keep the banks account they once opened even two years after the implementation of the training was completed.

[27] For example, geographical origin would leave us with a sample of 191 rural youth (51 treated individuals)

Table 5-1: Impact estimates for financial literacy and behaviour

	DiD	Comp. group: Mean (st. dev)	(1) ATT	(2) ITT	(3) LATE
<u>(i) Financial Literacy:</u>					
Financial Literacy Index	No	**0.499**	**0.064*** **	**0.016**	**0.042**
		0.285	0.024	0.020	0.052
<u>(ii) Financial Behaviour:</u>					
Has saving account	Yes	**0.356**	**0.148*** **	**0.102*** **	**0.269*** **
		0.479	0.054	0.039	0.103
Does save	Yes	**0.379**	**-0.055**	**-0.021**	**-0.055**
		0.486	0.055	0.044	0.115
Maintains budget	No	**0.478**	**0.015**	**-0.019**	**-0.051**
		0.500	0.043	0.034	0.089
Borrowed since Oct 2012	No	**0.124**	**-0.008**	**0.013**	**0.035**
		0.330	0.029	0.023	0.060
N (#persons)			**871**	**871**	**871**

Notes: "DiD" column indicates whether outcome variable was only observed in the follow-up survey ("No") or also at the baseline ("Yes"); next column contains the mean and standard deviation in the control group at the time of the follow-up survey; robust standard errors below impact estimates; */**/*** statistically significant at 90%/95%/99% confidence level

However, when studying standard measures of financial behaviour, such as saving and borrowing, effects are small and insignificant. Importantly, all these measures of financial behaviour are self-reported and might therefore be subject to perception bias. A possible avenue for validation is to complement them with side-effects of this very same behaviour. If individuals maintain a budget, their financial skills are likely to improve. Therefore, we also report the effect of the training on financial literacy. We construct financial literacy as an index composed of four statements that related to saving and lending services participants had to classify as “right” or “wrong”. Additionally, individuals were asked to identify two saving and lending institutions. Using these six items, the index is scaled such that it describes the combined percentage of correct (knowledge items) and positive answers (self-assessed knowledge).[28] We find a moderate positive impact on financial literacy across all specifications, although only the ATT effect is significant. Overall, this gives the impression that the training programme is indeed encouraging participants to more active financial behaviour even though effects are limited.

[28] Note that the financial literacy index, thus, is based on six binary survey items (either yes/no or right/wrong) and is calculated by taking the average value of the six items for every individual.

Table 5-2: Impact estimates for financial behaviour by gender

		Women		Men		
	DiD	Comp. group: Mean (st. dev)	LATE	Comp. group: Mean (st. dev)	LATE	p-value
(i) Financial Literacy:						
Financial Literacy Index	No	**0.515**	**-0.016**	**0.483**	**0.096**	0.241
		0.293	0.065	0.278	0.084	
(ii) Financial Behaviour:						
Has saving account	Yes	**0.306**	**0.323****	**0.404**	**0.209**	0.587
		0.462	0.130	0.492	0.164	
Does save	Yes	**0.292**	**0.131**	**0.463**	**-0.282**	0.079*
		0.456	0.141	0.500	0.187	
Maintains budget	No	**0.392**	**0.058**	**0.560**	**-0.142**	0.227
		0.489	0.106	0.498	0.148	
Borrowed since Oct 2012	No	**0.077**	**0.061**	**0.170**	**-0.004**	0.701
		0.267	0.065	0.376	0.112	
N (#persons)			**414**		**457**	

Notes: "DiD" column indicates whether outcome variable was only observed in the follow-up survey ("No") or also at the baseline ("Yes"); next column contains the mean and standard deviation in the control group at the time of the follow-up survey; last column contains the p-value for a test of equal impact coefficients between subgroups (in a specification that allows different coefficients for covariates); robust standard errors below impact estimates; */**/*** statistically significant at 90%/95%/99% confidence level.

To check whether the relatively small effects on saving and borrowing behaviour are driven by heterogeneous effects for different subgroups of study participants, we disaggregate the sample by gender and household assets. When considering the effects on female and male participants separately (see Table 5-2), it is noticeable that all three effects for measures of financial behaviour are positive for female and negative for male participants. However, estimates fall outside the bounds of statistical significance due to the small sub-samples and despite substantial effect sizes. It is also noteworthy that the already large effect for maintaining saving accounts is even more pronounced for women (32 percentage points) than for men (21 percentage points), suggesting that women without exposure to the training were less inclined to see the need to open and maintain an independent savings account. Together this might indicate that women were more responsive to the training. This – at least in part – could be a catching-up effect as women in the control group, across various measures, show lower levels of financial activity than men. Interestingly, this does not seem to be driven by knowledge differences. If anything, women in the comparison group display stronger results with respect to financial literacy than their male counterparts.

When considering financial behaviour, there are reasons to believe that this does not only depend on training or knowledge but also on the opportunities individuals are given because of their socio-economic backgrounds. Although the training may increase financial literacy and entrepreneurial aspirations, other outcomes such as borrowing might depend crucially on the relative social and financial wealth of their families (for example through credit ratings). Our findings suggest that socio-economic background, approximated by household assets, does indeed shape the effect of the training. Based on 23 self-reported household assets, we computed a scale for each respondent.[29] To capture heterogeneity by background, we divide respondents into two categories, those within the top half of the asset ranking and those at the bottom half.

Table 5-3: Impact estimates for financial behaviour by household assets

		HH assets lower 50%		HH assets top 50%		
	DiD	**Comp. group:** **Mean** (st. dev)	**LATE**	**Comp. group:** **Mean** (st. dev)	**LATE**	**p-value**
(i) Financial Literacy:						
Financial Literacy Index	No	**0.454**	**0.096**	**0.542**	**-0.008**	0.305
		0.283	0.064	0.281	0.082	
(ii) Financial Behaviour:						
Has saving account	Yes	**0.343**	**0.218***	**0.369**	**0.329***	0.603
		0.476	0.126	0.484	0.171	
Does save	Yes	**0.376**	**-0.158**	**0.382**	**0.063**	0.353
		0.486	0.140	0.487	0.191	
Maintains budget	No	**0.505**	**-0.130**	**0.452**	**0.070**	0.242
		0.501	0.109	0.499	0.145	
Borrowed since Oct 2012	No	**0.152**	**-0.078**	**0.097**	**0.196****	0.022**
		0.360	0.077	0.296	0.098	
N (#persons)			**436**		**435**	

Notes: "DiD" column indicates whether outcome variable was only observed in the follow-up survey ("No") or also at the baseline ("Yes"); next column contains the mean and standard deviation in the control group at the time of the follow-up survey; last column contains the p-value for a test of equal impact coefficients between subgroups (in a specification that allows different coefficients for covariates); robust standard errors below impact estimates; */**/*** statistically significant at 90%/95%/99% confidence level.

Table 5-3 reports the estimated impacts on financial literacy and financial behaviour outcomes for both subgroups. Overall, participants from more affluent household are more likely to significantly alter their financial behaviour. Although participants from both more and less affluent households significantly increase their likelihood of maintaining a savings account, the effect is substantially larger in the for the former group (32 compared to 21 percentage points). Moreover, they are 20 percentage points more likely to have borrowed compared to similar youth in the control group. These individuals might be encouraged to see borrowing as a viable option, helping them to leverage future earnings to attain financial goals. It may also encourage youth from less affluent households to seek out loans; however, this encouragement may be met with external barriers. Given their socioeconomic status, they may not have the collateral or reputational credit to secure initial loans from financial institutions. It should be noted that some microfinance institutions in Morocco accept savings as a collateral if those savings reach a certain percentage of the loan, which should make

[29] See Section 4, footnote 9 for more details on how the household asset index was calculated.

such loans more accessible to those from less affluent households. However, if youth are not saving (or able to save), then this requirement remains a barrier to borrowing. Alternatively, these youths may decide that loans are not appropriate for them at this point in time and they choose not to indebt themselves.

Table 5-4: Impact estimates for financial behaviour by age groups at baseline

		Youth aged 20+		Youth under age 20		
	DiD	Comp. group: Mean (st. dev)	LATE	Comp. group: Mean (st. dev)	LATE	p-value
(i) Financial Literacy:						
Financial Literacy Index	No	**0.476**	**0.166*** **	**0.529**	**-0.129**	0.004
		0.281	0.064	0.289	0.089	
(ii) Financial Behaviour:						
Has saving account	Yes	**0.333**	**0.214***	**0.385**	**0.336***	0.568
		0.472	0.128	0.488	0.171	
Does save	Yes	**0.354**	**0.073**	**0.412**	**-0.243**	0.196
		0.479	0.139	0.493	0.202	
Maintains budget	No	**0.467**	**-0.035**	**0.492**	**0,000**	0.852
		0.5	0.108	0.501	0.147	
Borrowed since Oct 2012	No	**0.125**	**0.055**	**0.123**	**-0.012**	0.600
		0.331	0.076	0.329	0.096	
N (#persons)			**481**		**390**	

Notes: "DiD" column indicates whether outcome variable was only observed in the follow-up survey ("No") or also at the baseline ("Yes"); next column contains the mean and standard deviation in the control group at the time of the follow-up survey; last column contains the p-value for a test of equal impact coefficients between subgroups (in a specification that allows different coefficients for covariates); robust standard errors below impact estimates; */**/*** statistically significant at 90%/95%/99% confidence level.

Taking together the smaller effect on savings accounts for participants from less affluent backgrounds and the negative (though insignificant) effects on saving and borrowing, this forms a picture that is instructive for the design of further interventions. It seems to be that poor participants, although stimulated by the training and motivated to work on financial issues, cannot realize their potential due to financial constraints in their background. A way to make the programme more efficient could therefore be to combine it with access to loans and financial institutions, so that the motivational effects of the trainings can be translated into higher financial activity.

Finally, Table 5-4 displays estimates on financial outcomes splitting the sample into younger (19 years or below at the time of the baseline survey in 2012) and older (20 years and older) study participants. In this subgroup analysis, the impact on financial literacy is strongly positive and highly significant for older youth (around 0.6 standard deviations) while the coefficient for younger participants even turns negative (but statistically insignificant). Conversely, maintaining a savings account seems concentrated among younger individuals with a significant positive impact in both age groups (at the 10 percent level). Estimates for other outcome variables are insignificant and do not show a clear pattern.

5.2 Labour market outcomes

We now turn to outcomes the areas of education and the labour market. Comparing baseline of key educational and labour market indicators with follow-up data three years later, shows that the intervention reached a population in the middle of transition from school to work. While labour market participation (in the control group) increased from 13 to 47 percent over the course of the study, enrolment in education – meaning current enrolment in either a secondary school, vocational school or university – dropped from 89 to 51 percent.

Almost three-quarters (72 percent) of those in the comparison group remaining in education at the time of the follow-up survey were enrolled in a university program. Furthermore, data on the highest level of education attained so far show that most respondents in the control group already hold a post-secondary degree (40 percent a professional diploma and 32 percent a university degree). Regarding labour market status, only 7 percent of participants were employed at the time of the baseline and 67 percent did not have any work experience at all. Three years later, slightly less than a third (29 percent) find themselves in employment while two-thirds (67 percent) at least acquired some work experience (on average around one year).[30]

Regarding educational outcomes, ATT estimates on educational enrolment are positive and statistically significant as Table 5-5 shows. The fact that their corresponding LATE coefficients are much smaller in size (and lose significance) once again suggests that the decision to take part in the program was selective. The LATE specification indicates that treated participants are slightly more likely to remain in education even though the estimates lacks statistical significance.

Moreover, we observe six different labour market-related outcome variables. The first three relate directly to labour market status that is whether an individual is employed, unemployed (i.e., without employment in the past seven days but available and actively looking for work) or inactive, the residual category. We also measure impacts on an NEET (*Not in Education, Employment or Training*) indicator capturing individuals that are inactive on the labour market and not enrolled in education,[31] whether respondents had any work experience so far and their job experience, measured in number of months worked.[32] We do not distinguish between self-employment and wage employment as only 25 (out of 871) individuals in the follow-up survey identify themselves as entrepreneurs, which does not allow a meaningful quantitative analysis.

[30] Most of the statistics presented in this paragraph are taken from Table 5-5 (follow-up) and Table A-2 in the appendix for the baseline survey. To ensure comparability, the averages presented at the time of the baseline are restricted to the comparison group only. However, differences between comparison and treatment group at the time of the baseline are marginal.
[31] Enrolment in education also includes secondary schools, vocational school and universities.
[32] Work experience (number of months) captures the current job (if any) and up to the last three jobs of respondents (95% of respondents indicated that they held three jobs or less in the follow-up survey).

Table 5-5: Impact estimates for education and employment outcomes

	DiD	Comp. group: Mean (st. dev)	(1) ATT	(2) ITT	(3) LATE
(i) Education					
Enrolled	Yes	**0.513**	**0.124*** **	**0.016**	**0.041**
		0.500	0.046	0.036	0.094
(ii) Labour Market					
Employed	Yes	**0.286**	**-0.081***	**-0.063***	**-0.165***
		0.452	0.040	0.034	0.088
Unemployed	Yes	**0.180**	**0.007**	**-0.007**	**-0.018**
		0.385	0.042	0.031	0.082
Inactive	Yes	**0.534**	**0.074**	**0.070***	**0.183***
		0.499	0.050	0.039	0.102
NEET	Yes	**0.290**	**-0.029**	**0.034**	**0.089**
		0.454	0.046	0.035	0.091
Any work exp.	Yes	**0.665**	**-0.056**	**-0.018**	**-0.046**
		0.473	0.053	0.040	0.104
Months work exp.♦	No	**12.000**	**-0.622**	**-1.704**	**-4.488**
		22.001	1.883	1.490	3.873
N (#persons)			**871**	**871**	**871**

Notes: "DiD" column indicates whether outcome variable was only observed in the follow-up survey ("No") or also at the baseline ("Yes"); next column contains the mean and standard deviation in the control group at the time of the follow-up survey; robust standard errors below impact estimates; */**/*** statistically significant at 90%/95%/99% confidence level.

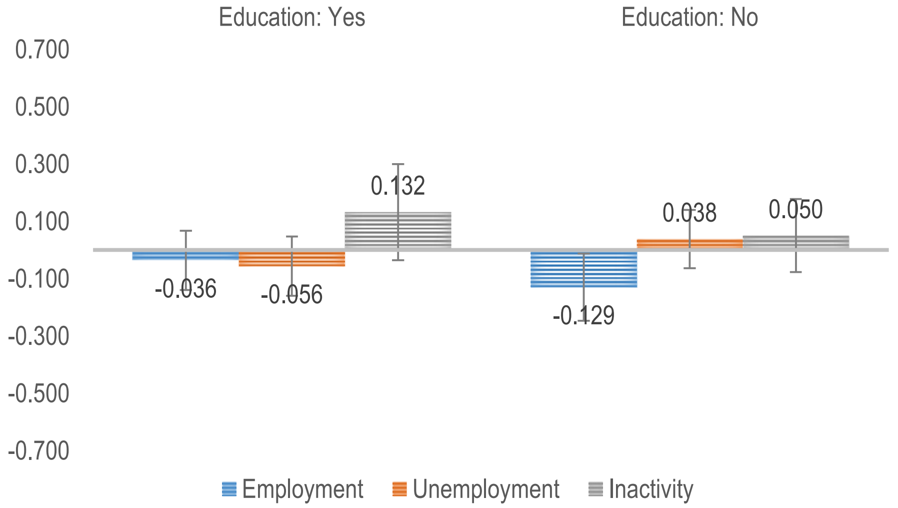
♦: for months of work experience the models are based on 790 observations.

Table 5-5 documents that for four out of six labour market outcomes we do not find a statistically significant impact of the “100 Hours to Success” training. Labour market status is the exception where the LATE specification indicates that program participation decreased participants’ likelihood of being employed by 16.5 percentage points – a result that is statistically significant at the 90%-confidence level. This decrease in the probability of being employed is accompanied by an increase in inactivity of almost the same size (18.3 percentage points) with unemployment being virtually unaffected. Albeit lacking statistical significance, the results for the other three outcome indicators are in line with the observed decrease in the employment probability. Training participants on average have less work experience and a (slightly) higher risk of being classified as NEET.

The result that program participation reduces – rather than increases – labour market activity appears counterintuitive. To better understand the dynamics at work, a joint analysis of educational and labour market outcomes proves insightful. We record three labour market outcomes: study participants are either employed, unemployed or inactive. Furthermore, each individual is either still enrolled in education (either secondary school, vocational school or university) or not enrolled anymore. Combining both dimensions, results in six mutually exclusive outcome categories. For example, at the time of the baseline survey, 79 percent of youth (from the restricted sample of 871 observations that is used for impact analysis) were still enrolled in education while at the same time inactive in the labour market, six percent were not in education and inactive, five percent were enrolled in education while at the same time looking for work, and so on.

Figure 5-1 presents LATE impact estimates based on both baseline and follow-up data for these six categories. Note that as each youth belongs to exactly one category, the six impact estimates necessarily add up to zero. Figure 5-1 documents three important findings. First, almost three-quarters of the increase in inactivity can be attributed to individuals who are staying in education: the increase in inactivity and being enrolled in education is 13.2 percentage points, compared to 18.3 percentage points when disregarding educational enrolment as shown in Table 5-5, column (3). Second, some program participants seem to stay longer in education (even though this effects is not significant). Third, among all of those enrolled in education, fewer are currently employed or looking for work. These findings are consistent with program participants who invest and focus more in human capital accumulation through (formal) education and training rather than acquiring early labour market experience.

Figure 5-1: Impact estimates for educational enrolment and labour market status, full sample (N=871)



Notes: we estimate impacts on six mutually exclusive categories combining educational enrolment (yes/no) with labour market status (employed/unemployed/inactive); by construction impact estimates sum up to zero; 90 percent confidence intervals based on robust standard errors are displayed.

Turning to a detailed subgroup analysis, Table 5-6 presents results on employment prospects disaggregated by gender. Averages in the comparison group reveal that women are more likely to still be enrolled in education or training (57 percent vs. 46 percent). At the same time, women are much less likely to be employed (12 percent vs. 44 percent) and much more likely to find themselves outside of the labour force (73 percent vs. 34 percent) than their male counterparts.

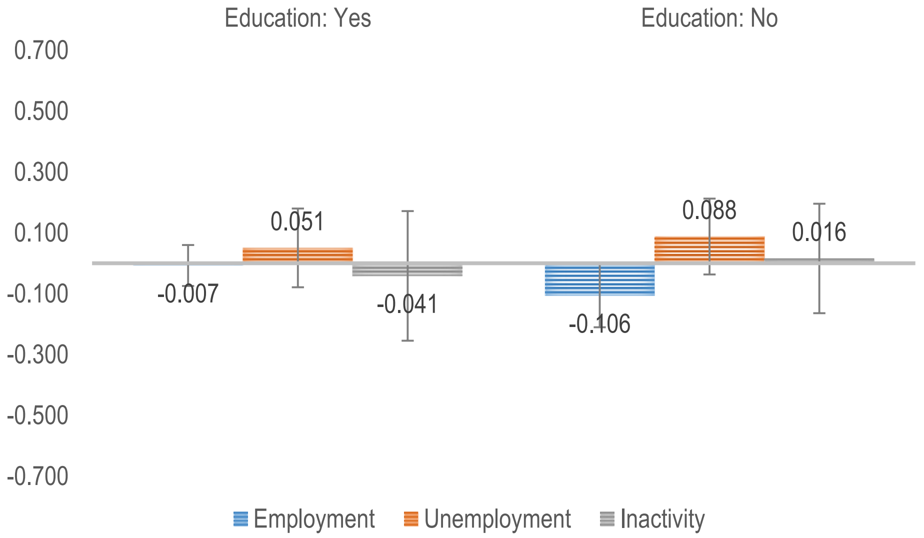
Compared with the impact estimates in the previous section, two interesting trends emerge from this disaggregation. First, women show less of a tendency to remain in education while impact on labour market outcomes show conflicting signs. Importantly, none of the coefficients reaches statistical significance. Second, the tendencies observed for the whole sample seem to be entirely coming from men. Impacts for remaining in education are stronger (but still insignificant) while there is an enormous drop from the labour force (rise in inactivity) around twice as large as for the overall sample (and highly significant). These observations are confirmed by Figure 5-2 and Figure 5-3 which combine labour market status and educational enrolment and mirror that analysis presented in Figure 5-1 for the whole sample. Coefficients for women vary around zero (with large confidence intervals). For men, the pronounced rise in labour market inactivity is strongly associated with enrolment in education. As Figure 5-3 shows the overall estimate of an increase in the probability of inactive (48 percentage points) is decomposed into increased inactivity while staying in education (39 percentage points) and inactive while not pursuing further education (9 percentage points). Moreover, this seem to be driven by both men that stay longer in education and male individuals who, while staying in education, reduce their labour market activity.

Table 5-6: Impact estimates for education and employment outcomes by gender

		Women		Men		
	DiD	**Comp. group: Mean** (st. dev)	**LATE**	**Comp. group: Mean** (st. dev)	**LATE**	**p-value**
(i) Education						
Enrolled	Yes	**0.569**	**0.003**	**0.459**	**0.117**	0.547
		0.496	0.116	0.499	0.150	
(ii) Labour Market						
Employed	Yes	**0.124**	**-0.113**	**0.440**	**-0.274**	0.385
		0.331	0.074	0.498	0.169	
Unemployed	Yes	**0.144**	**0.139**	**0.216**	**-0.210**	0.039**
		0.351	0.102	0.412	0.135	
Inactive	Yes	**0.732**	**-0.025**	**0.344**	**0.484*** **	0.014**
		0.444	0.117	0.476	0.171	
NEET	Yes	**0.335**	**0.104**	**0.248**	**0.071**	0.859
		0.473	0.119	0.433	0.143	
Any work exp.	Yes	**0.560**	**0.011**	**0.766**	**-0.103**	0.590
		0.498	0.122	0.424	0.173	
Months work exp.♦	No	**4.308**	**0.259**	**19.080**	**-11.019**	0.228
		8.146	2.316	27.670	8.476	
N (#persons)			**414**		**457**	

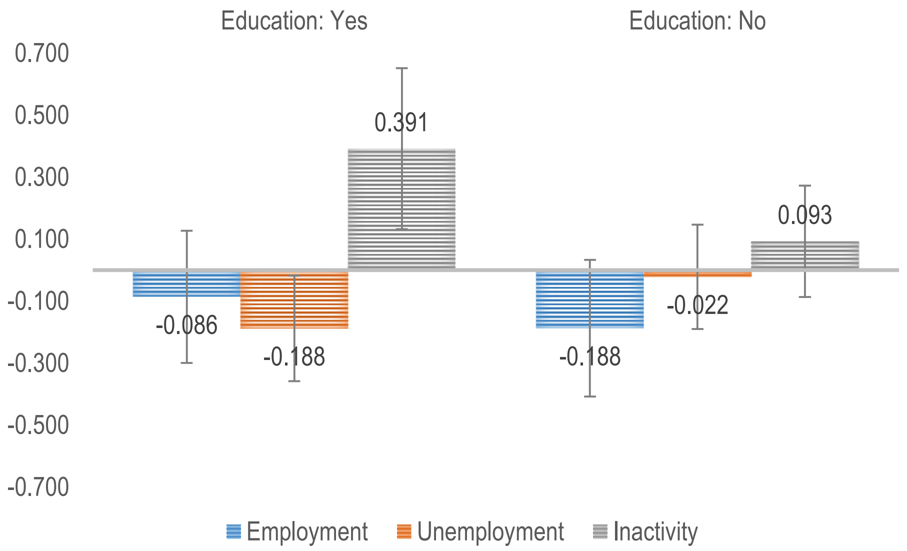
Notes: "DiD" column indicates whether outcome variable was only observed in the follow-up survey ("No") or also at the baseline ("Yes"); next column contains the mean and standard deviation in the control group at the time of the follow-up survey; last column contains the p-value for a test of equal impact coefficients between subgroups (in a specification that allows different coefficients for covariates); robust standard errors below impact estimates; */**/*** statistically significant at 90%/95%/99% confidence level.
♦: for months of work experience the models are based on 370 observations for women and 420 observation for men.

Figure 5-2: Impact estimates for educational enrolment and labour market status, women (N=414)



Notes: we estimate impacts on six mutually exclusive categories combining educational enrolment (yes/no) with labour market status (employed/unemployed/inactive); by construction impact estimates sum up to zero; 90 percent confidence intervals based on robust standard errors are displayed.

Figure 5-3: Impact estimates for educational enrolment and labour market status, men (N=457)



Notes: we estimate impacts on six mutually exclusive categories combining educational enrolment (yes/no) with labour market status (employed/unemployed/inactive); by construction impact estimates sum up to zero; 90 percent confidence intervals based on robust standard errors are displayed.

Next, we report results on educational and labour market outcomes when dividing the sample by the level of household socioeconomic status in Table 5-7. There are no considerable differences when comparing averages of the control group between youth from more and less affluent backgrounds. However, we find an interesting difference with respect to impact estimates. Youth from less well-off backgrounds do not seem to react systematically to the intervention, which is also true when looking at combined educational and labour market outcome categories that are displayed in Figure 5-4. On the contrary, study participants from more affluent households show a pronounced increase in labour market inactivity which is – as Figure 5-5 reveals – accompanied with a prolonged stay in education.

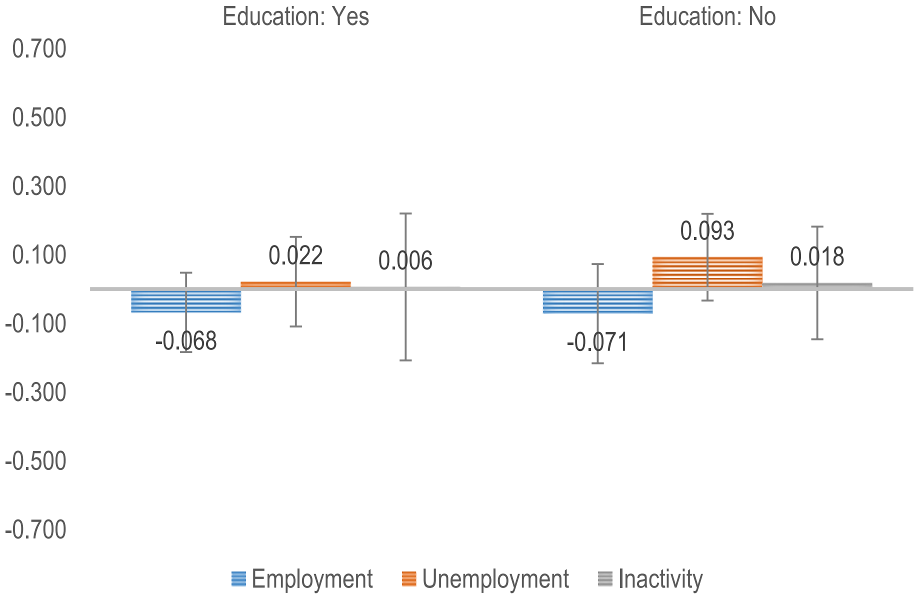
Table 5-7: Impact estimates for education and employment outcomes by household assets

		HH assets lower 50%		HH assets top 50%		
	DiD	Comp. group: **Mean** (st. dev)	**LATE**	Comp. group: **Mean** (st. dev)	**LATE**	**p-value**
(i) Education						
Enrolled	Yes	**0.524**	**-0.040**	**0.502**	**0.141**	0.354
		0.501	0.118	0.501	0.155	
(ii) Labour Market						
Employed	Yes	**0.290**	**-0.139**	**0.281**	**-0.218**	0.669
		0.455	0.104	0.451	0.152	
Unemployed	Yes	**0.162**	**0.115**	**0.198**	**-0.176**	0.087
		0.369	0.105	0.400	0.134	
Inactive	Yes	**0.548**	**0.025**	**0.521**	**0.394****	0.084*
		0.499	0.126	0.501	0.173	
NEET	Yes	**0.276**	**0.111**	**0.304**	**0.087**	0.896
		0.448	0.115	0.461	0.147	
Any work exp.	Yes	**0.600**	**0.026**	**0.728**	**-0.148**	0.411
		0.491	0.134	0.446	0.164	
Months work exp.♦	No	**11.723**	**-7.851***	**12.263**	**0.106**	0.230
		20.570	4.291	23.328	6.598	
N (#persons)			**436**		**435**	

Notes: "DiD" column indicates whether outcome variable was only observed in the follow-up survey ("No") or also at the baseline ("Yes"); next column contains the mean and standard deviation in the control group at the time of the follow-up survey; last column contains the p-value for a test of equal impact coefficients between subgroups (in a specification that allows different coefficients for covariates); robust standard errors below impact estimates; */**/*** statistically significant at 90%/95%/99% confidence level. ♦: for months of work experience the models are based on 391 observations for youth from less affluent background and 399 observation for youth from more affluent backgrounds.

Finally, we disaggregate the sample into older youth – on average around 25 years old at the time of the follow-up survey – and younger youth that were on average 20 years old at the time of the follow-up survey (see Table 5-8). Older youth in the control group are – as we might expect – less likely to be still enrolled in education and have attained a higher level of education than younger study participants. However, this is not associated with better labour market outcomes – neither in terms of employment, nor months of accumulated work experience. The fact that – disregarding program impact – youth that are on average five years older do fail to perform better on the labour market is a telling sign of a long and difficult school to work transition period many Moroccan youth seem to face. Considering impact, coefficients generally lack statistical significance and add little to the tendencies observed for the overall sample. Combining employment and labour market outcomes (Figure 5-6 and Figure 5-7) reveals that older youth seem to be focusing on and staying in education rather than trying to enter the labour market.

Figure 5-4: Impact estimates for educational enrolment and labour market status, bottom 50% of HH assets (N=436)



Notes: we estimate impacts on six mutually exclusive categories combining educational enrolment (yes/no) with labour market status (employed/unemployed/inactive); by construction impact estimates sum up to zero; 90 percent confidence intervals based on robust standard errors are displayed.

Figure 5-5: Impact estimates for educational enrolment and labour market status, top 50% of HH assets (N=435)

Notes: we estimate impacts on six mutually exclusive categories combining educational enrolment (yes/no) with labour market status (employed/unemployed/inactive); by construction impact estimates sum up to zero; 90 percent confidence intervals based on robust standard errors are displayed.

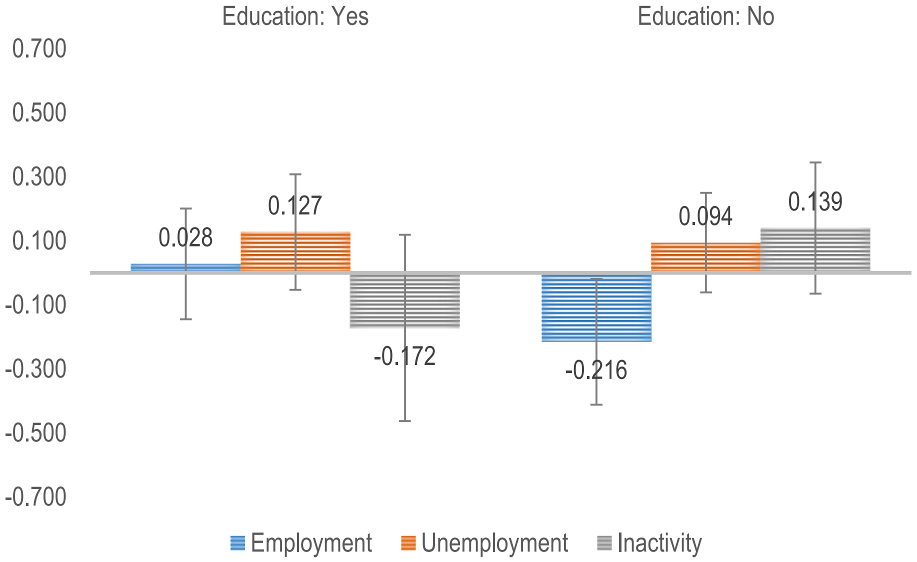
Taken together, the findings on education and labour market activity suggest that individuals invest more in education not only through longer attendance but also by devoting less time to labour market activity. It appears that the training led some to consider that further investments in education would be preferable to entering a difficult, largely informal labour market in helping them meet long-term goals. For women, younger and less affluent program participants, data suggest that training participation might have left labour market outcomes and decision on pursuing further education largely unaffected.

Table 5-8: Impact estimates for education and employment outcomes by age groups at baseline

		Youth aged 20+		**Youth under age 20**		
	DiD	**Comp. group: Mean** (st. dev)	**LATE**	**Comp. group: Mean** (st. dev)	**LATE**	**p-value**
(i) Education						
Enrolled	Yes	**0.488**	**0.082**	**0.545**	**-0.017**	0.618
		0.501	0.117	0.499	0.162	
(ii) Labour Market						
Employed	Yes	**0.321**	**-0.010**	**0.251**	**0.233**	0.836
		0.468	0.115	0.435	0.149	
Unemployed	Yes	**0.279**	**-0.150**	**0.294**	**-0.188**	0.029
		0.450	0.111	0.457	0.144	
Inactive	Yes	**0.208**	**-0.150**	**0.144**	**0.221***	0.114
		0.407	0.105	0.352	0.133	
NEET	Yes	**0.513**	**0.300****	**0.561**	**-0.033**	0.198
		0.501	0.130	0.498	0.166	
Any work exp.	Yes	**0.654**	**-0.008**	**0.679**	**-0.090**	0.706
		0.477	0.129	0.468	0.176	
Months work exp.♦	No	**11.586**	**-0.787**	**12.520**	**-10.229**	0.305
		21.707	4.625	22.418	6.933	
N (#persons)			**481**		**390**	

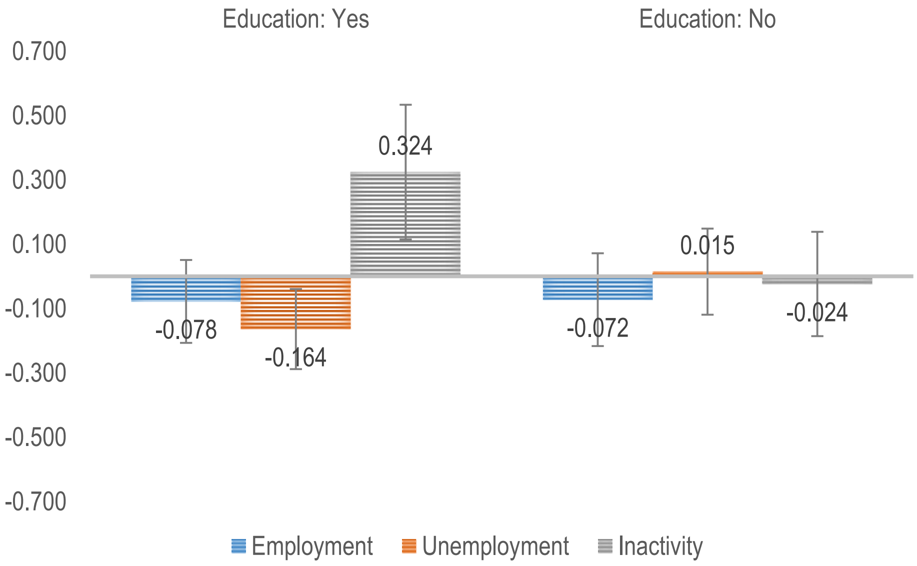
Notes: "DiD" column indicates whether outcome variable was only observed in the follow-up survey ("No") or also at the baseline ("Yes"); next column contains the mean and standard deviation in the control group at the time of the follow-up survey; last column contains the p-value for a test of equal impact coefficients between subgroups (in a specification that allows different coefficients for covariates); robust standard errors below impact estimates; */**/*** statistically significant at 90%/95%/99% confidence level. ♦: for months of work experience the models are based on 434 for older youth and 356 observations for younger youth.

Figure 5-6: Impact estimates for educational enrolment and labour market status, age <20 (N=390)



Notes: we estimate impacts on six mutually exclusive categories combining educational enrolment (yes/no) with labour market status (employed/unemployed/inactive); by construction impact estimates sum up to zero; 90 percent confidence intervals based on robust standard errors are displayed.

Figure 5-7: Impact estimates for educational enrolment and labour market status, age 20+ (N=481)



Notes: we estimate impacts on six mutually exclusive categories combining educational enrolment (yes/no) with labour market status (employed/unemployed/inactive); by construction impact estimates sum up to zero; 90 percent confidence intervals based on robust standard errors are displayed.

5.3 Robustness check: Inverse probability weighting

We attempt to correct for potential biases due to differences in the probability of being observed twice for urban study participants in the control group by using *inverse probability weighting*. In doing so, first, we estimate a probit model including all variables and interaction terms listed in column (4) of Table 4-2. Secondly, we predict for every individual the probability of being included in the follow-up survey. Thirdly, we use the inverse of this probability as sampling weight when estimating our models (as represented by the equations (1), (2) and (3)). Intuitively, we give more weight to individuals who have a comparably high probability of not being followed up with and thus are presumably more like persons that were not included in the follow-up survey.

Table 5-9: Impact estimates for financial literacy and behaviour – Inverse Probability Weighting (IPW)

			ITT		**LATE**	
	DiD	**Comp. group:**	**(1)**	**(2)**	**(3)**	**(4)**
		Mean (st. dev)	**Standard**	**IPW**	**Standard**	**IPW**
(i) Financial Literacy:						
Financial Literacy Index	No	**0.499**	**0.016**	**0.016**	**0.042**	**0.042**
		0.285	0.020	0.020	0.052	0.051
(ii) Financial Behaviour:						
Has saving account	Yes	**0.356**	**0.102*** **	**0.105*** **	**0.271** **	**0.274** **
		0.479	0.039	0.040	0.111	0.110
Does save	Yes	**0.379**	**-0.021**	**-0.017**	**-0.064**	**-0.054**
		0.486	0.044	0.044	0.122	0.121
Maintains budget	No	**0.478**	**-0.019**	**-0.018**	**-0.051**	**-0.046**
		0.500	0.034	0.034	0.089	0.087
Borrowed since Oct 2012	No	**0.124**	**0.013**	**0.010**	**0.035**	**0.026**
		0.330	0.023	0.023	0.060	0.058
N (#persons)			**871**	**871**	**871**	**871**

Notes: "DiD" column indicates whether outcome variable was only observed in the follow-up survey ("No") or also at the baseline ("Yes"); next column contains the mean and standard deviation in the control group at the time of the follow-up survey; columns (1) and (2) compare ITT estimates for a standard (OLS) and IPW specification, columns (3) and (4) compare LATE estimates; robust standard errors below impact estimates; */**/*** statistically significant at 90%/95%/99% confidence level.

Table 5-9 and Table 5-10 contain the results reporting the normal (non-weighted) estimates in column (1) ITT and (3) LATE, and the IPW-estimates in column (2) ITT and (4) LATE. Overall, the estimates are very similar, and all results are robust to using inverse probability weights when considering statistical significance.[33]

[33] Observe that the ITT estimates presented in column (1) of the respective tables exactly correspond to the coefficients presented in Tables 7.7, 7.11 and 7.12. For technical reasons the models combining instrumental variable techniques with inverse probability weighting (column 4 of Tables 8.2, 8.3 and 8.4) have been estimated without including individual fixed effects (but including all time-varying and time-invariant control variables described in Section 7). To allow direct comparability, the coefficients displayed in column (3) therefore also omit individual fixed effects. Differences are marginal and for all practical purposes negligible.

Table 5-10: Impact estimates for education and employment outcomes – Inverse Probability Weighting (IPW)

			ITT		LATE	
	DiD	Comp. group:	(1)	(2)	(3)	(4)
		Mean (st. dev)	Standard	IPW	Standard	IPW
<u>(i) Education</u>						
Enrolled	Yes	**0.513**	**0.016**	**0.008**	**0.042**	**0.020**
		0.500	0.036	0.037	0.103	0.103
<u>(ii) Labour Market</u>						
Employed	Yes	**0.290**	**0.034**	**0.040**	**0.090**	**0.106**
		0.454	0.035	0.035	0.094	0.095
Unemployed	Yes	**0.286**	**-0.063***	**-0.064****	**-0.172****	**-0.169****
		0.452	0.034	0.032	0.086	0.080
Inactive	Yes	**0.180**	**-0.007**	**-0.004**	**-0.016**	**-0.009**
		0.385	0.031	0.032	0.083	0.084
NEET	Yes	**0.534**	**0.070***	**0.068***	**0.189***	**0.178***
		0.499	0.039	0.039	0.105	0.102
Any work exp.	Yes	**0.665**	**-0.018**	**-0.019**	**-0.045**	**-0.045**
		0.473	0.040	0.040	0.115	0.114
Months work exp.♦	No	**12.000**	**-1.704**	**-1.630**	**-4.488**	**-4.201**
		22.001	1.490	1.378	3.873	3.51
N (#persons)			**871**	**871**	**871**	**871**

Notes: "DiD" column indicates whether outcome variable was only observed in the follow-up survey ("No") or also at the baseline ("Yes"); next column contains the mean and standard deviation in the control group at the time of the follow-up survey; columns (1) and (2) compare ITT estimates for a standard (OLS) and IPW specification, columns (3) and (4) compare LATE estimates; robust standard errors below impact estimates; */**/*** statistically significant at 90%/95%/99% confidence level. ♦: for months of work experience the models are based on 790 observations.

Summing up the discussion about the attrition rate in the follow-up survey and its implications for the internal validity of this study, four points should be remembered. First, treatment assignment and treatment status do not generally correlate with the probability of being included in the follow-up survey. So, while attrition just surpasses 50 percent, we do overall not observe differential attrition rates in the treatment and comparison group. Second, our remaining sample of 871 looks balanced with respect to control and outcome variables for the baseline survey. This is reassuring but should be taken with a grain of salt because the small sample we are left with makes it difficult to precisely identify differences. Third, comparing our remaining sample with those who could not be observed for the follow-up survey, we observe some stark differences with respect to observable characteristics. However, with one exception (urban youth) this does not correlate with treatment assignment. Our results are robust when correcting observable differences in our remaining sample through inverse probability weighting. Fourth, we are aware that our analysis cannot take unobservable factors into account that might have influenced attrition rates selectively but it is encouraging that within the group of youth assigned to the treatment group, attrition does not very by actual treatment status and varies unsystematically by treatment intensity (see Figure 4-1).[34]

All in all, we do not find convincing evidence that the high rate of attrition present in this study systematically biases the obtained results. However, attrition still represents a significant challenge for this study. In combination with the considerable degree of non-compliance, it depresses the power of the analysis by increasing the statistical uncertainty of our estimates and severely constraints efforts to disaggregate findings by relevant socio-economic, demographic and geographical subgroups.

6. Conclusion

In light of an enduring global youth employment challenge, identifying interventions that can effectively increase inclusion of youth in economic markets is a mounting priority. As the first youth-focused RCT in Morocco, the impact evaluation of MEDA's "100 Hours to Success" skills training programme contributes to closing the knowledge gap on what works in youth employment in the MENA region where rigorous evidence is still extremely limited. The analysis suggests that the training programme affected participants along several dimensions which have important implications for future programming, policy as well as research.

Regarding financial outcomes, we find substantial and significant impacts of the training on establishing a savings account and keeping it more than two years after the end of the intervention. However, there is little evidence that this effect and further impacts on financial literacy generally translate into changes of financial behaviour, such as increased borrowing or saving activity. The effects of the training differ substantially by age, social background and between the genders. Smaller effects for financial literacy and borrowing behaviours for youth from households with fewer assets and for women suggest that the beneficial impact of a skills training can only unfold when participants have an environment that allows them to put their new knowledge into practice. Restricted access to loans and other financial services for youth from low-asset households and a lack in autonomy for educational and occupational choices for women are examples of the barriers participants may face.

Therefore, our findings imply that skills training should be combined with programmes to address constraints faced by these groups in order to tap the full potential of these interventions. Moreover, key barriers to a successful economic integration of youth need to be closely analysed. For example, access to financial services should be tackled alongside with training, so that youth can leverage the knowledge gained to pursue their economic interests. Investigating which constraints are most salient is both important from a research perspective but also relevant for programme implementers and policy makers.

[34] Recall that attrition overall is balanced between those assigned to control and treatment group. To introduce bias in the results, we would need a higher attrition propensity for one subgroup of those assigned to treatment and a lower attrition propensity for another subgroup assigned to treatment (relative to the attrition rate in the control group). Second, outcomes of these two subgroups would have to be negatively correlated with each other.

Further research is needed to better understand the constraints youth are facing in their transition from school to work. This could shed more light on the effects this study documents regarding labour market activity. We find that youth receiving the training stay longer in education and postpone their entry into the labour market. Again, impacts vary across subgroups as the above effects are driven by older participants, youth from more affluent family background and men. Scrutinizing heterogeneous outcomes more closely has a huge potential for policy makers. When exploring and addressing constraints such as financial means to continue education or family obligations, the larger effects found for male, older and affluent youth could be extended to the whole sample, thus increasing the average effect. These differences also indicate knowledge gaps about the effects along the results chain that should be addressed by further research. It would be useful to investigate whether decisions to pursue post-secondary education are driven by strategic economic choices to increase future earnings or whether training might have led youth to uncover knowledge gaps that they are seeking to fill.

Another implication of our analysis is that adequate targeting of programme participants can additionally augment the effects of the training. For programme implementers, this is especially useful, because it allows to leverage resources. Our findings imply that older participants benefit more directly from the training provided to them. Most likely, this is not a question of age as such but because older youth find themselves in a different stage of their school-to-work transition periods. Young people that will still be in school for a couple of years before starting to search for a job will not be able to apply what they learned in the training. Thus, any gains in terms of labour market relevant knowledge or skills are likely to have dissipated by the time youth leave school or university.

For programmes that target (self-)employment outcomes it might therefore be advisable to restrict programme access to those at the end of their educational careers. This could, furthermore, include a careful screening based on ambition and aspirations. Narrowing down the pool of beneficiaries might, for example, allow better alignment between the training's ability to identify potential entrepreneurs and the provision of additional support services, including (in some cases) grants and access to loans. For instance, given the age range of those involved in the training, and the fact that most youth were still enrolled in school or university during the intervention, many are not prepared to build on the job- and job-search-relevant skills provided by the training program. A more careful targeting might also lead to higher take-up rates and dedicated studies of take-up of skills training programmers could be particularly worthwhile. Studying whether and how rewards for training completion can increase take-up or determinants of drop-outs such as distance to the training location can contribute to an improvement both of training evaluations and the training itself.

Looking ahead, long-standing developmental challenges related to growing youth populations and youth labour market inclusion will remain a policy priority in Morocco as well as the MENA region more broadly. So far, efforts to resolve the economic challenges facing youth have focused on immediate ALMPs such as subsidized work programmes and skills trainings. The results of this study provide further evidence that skills training, although useful in themselves, should be part of a comprehensive policy package that addresses not only young people's skills deficits but also broader socio-economic challenges.

Bibliography

Angrist, JD, and Pischke, J-S. 2009. *Mostly Harmless Econometrics: An Empiricist's Companion.* Princeton University Press.

Attanasio, O, Kugler, A and Meghir, C. 2011. *Subsidizing Vocational Training for Disadvantaged Youth in Colombia: Evidence from a Randomized Trial, Applied Economics 3, July, pp. 188–220*.

Betcherman, G, Olivas, K and Dar, A. 2004. *Impacts of Active Labor Market Programs: New Evidence from Evaluations with Particular Attention to Developing and Transition Countries, Social Protection Discussion Paper Series.* Washington, DC: World Bank.

Betcherman, G, Godfrey, M, Puerto, S, Rother, F and Stavreska, A. 2007. *Global Inventory of Interventions to Support Young Workers: Synthesis Report*. Washington, DC: World Bank.

Cameron, AC, and Trivedi, PK. 2005. *Microeconometrics: Methods and Applications.* Cambridge University Press.

Card, D, Kluve, J, and Weber, A. 2010. *Active Labor Market Policy Evaluations: A Meta-Analysis, NBER Working Paper No. 16173. July.*

Card, D, Ibarraran, P, Regalia, F, Rosas-Shady, D, Soares, Y. 2011. *The Labor Market Impacts of Youth Training in the Dominican Republic, Journal of Labor Economics 29, February.*

Cho, Y, Kalomba, D, Mobarak, AM and Orozco, V. 2013. *Gender differences in the effects of vocational training: Constraints on women and drop-out behaviour, IZA Discussion Paper No. 7408*. Bonn: Institute for the Study of Labor.

Cunningham, W, Sanchez-Puerta, ML, and Wuermli, A. 2010. *Active Labor Market Programs for Youth: A Framework to Guide Youth Employment, World Bank Employment Policy Primer No. 16.* Washington, DC: World Bank.

Dhillon, N, Salehi-Isfahani, D, Dyer, P, Yousef, T, Fahmy, A, and Kraetsch, M. 2009. *Missed by the Boom, Hurt by the Bust: Making Markets Work for Young People in the Middle East.* Washington, DC: Middle East Youth Initiative.

Donald, SG, Lang, K. 2007. *Inference with Difference-in-Differences and Other Panel Data, The Review of Economics and Statistics, vol. 89(2), pp. 221-233.*

Duflo, E, Glennerster, R, Kremer, M. 2008. *Using Randomization in Development Economics Research: A Toolkit, Handbook of Development Economics, Volume 4, Chapter 61.*

Dyer, P, Gardiner, G, Kluve, J, Mizrokhi, E. 2015. *Boosting Youth Employability in Morocco – II, Randomized Controlled Trial Baseline Report*. International Labour Organization.

Groh, M, Krishnan, N, McKenzie, D and Vishwanath, T. 2012. *Soft skills or hard cash? The impact of training and wage subsidy programs on female youth employment in Jordan, Policy Research Working Paper No. 6141*. Washington, D.C.: World Bank.

Ibarraran, P and Shady, DR. 2009. *Evaluating the impact of Job Training Programme in Latin America: Evidence from IDB-Funded Operations, Journal of Development Effectiveness, Taylor & Francis Journals, vol. 1(2), pp. 195-216.*

Imbens, GW, and Wooldridge, JM. 2009. *Recent Developments in the Econometrics of Programme Evaluation.* Journal of Economic Literature, 47(1): 5-86.

International Labour Organization. 2015a. *Boosting Youth Employability in Morocco – I: Qualitative assessment of MEDA Maroc's 100 Hours to Success programme.* Geneva: International Labour Organization.

International Labour Organization. 2015b. *Synthesis Review of ILO Experience in Youth and Women's Employment in the MENA Region: Summary version.*

Kluve, J., S. Puerto, D. Robalino, J. Romero, F. Rother, J. Stöterau, F. Weidenkaff, and M. Witte. 2016. *Interventions to Improve the Labour Market Outcomes of Youth: A Systematic Review of Training, Entrepreneurship Promotion, Employment Services, and Subsidized Employment Interventions.* International Labour Organization.

Morocco Haut Commissariat au Plan. 2015. *Indicators and Aggregates.* Available online at: http://www.hcp.ma/.

Premand, P, Brodmann, S, Almeida, R, Grun, R, Barouni, M. 2012. *Entrepreneurship training and self-employment among university graduates: evidence from a randomized trial in Tunisia, World Bank Policy Research Working Paper Series No. 6285.* Washington, DC: World Bank.

Reimers, F, Dyer, P, and Lettrick, S. 2012. *Unlocking Arab Youth Entrepreneurship Potential: An Evaluation of the INJAZ Al-Arab Company Program*. Available online at: http://injazalarab.org/wp-content/uploads/2014/08/An-Evaluation-of-the-INJAZ-Al-Arab-Company-Program2.pdf.

USAID. 2013. *Examining the Evidence in Youth Workforce Development: USAID Youth Research, Evaluation and Learning Project, USAID State of the Field Report.* Washington, DC: USAID.

World Bank. 2012. *Morocco: Promoting youth opportunities and participation*. Washington, DC: World Bank.

World Bank. 2016. *International Comparison Programme database,* available online (http://data.worldbank.org/).

Appendix

Table A-1: Control variables at baseline, full sample (N=1803)

	Mean			
	Full (N=1803)	**Control (N=891)**	**Δ(Treat-Control)**	**p-value**
Gender (1=female)	0.525	0.535	-0.020	0.395
Age	19.984	20.068	-0.167	0.200
No. of Siblings	3.818	3.865	-0.093	0.349
Urban	0.750	0.756	-0.013	0.523
Living in dormitory	0.192	0.192	0.001	0.954
No. of HH members	4.903	4.910	-0.013	0.888
Female HH head	0.119	0.125	-0.011	0.490
Education level – HH head (0-6)	1.655	1.662	-0.014	0.865
Father alive	0.915	0.914	0.003	0.814
HH assets (1-10)	4.024	4.050	-0.053	0.216
Satisfied w/ HH situation (1-4)	2.863	2.843	0.040	0.205
Attended other skills training in past	0.138	0.145	-0.014	0.378

Notes: the first column presents averages for all observations, the second column for the control group and the third column differences between the treatment and the control group. The last column contains p-values for a two-sided test of equal means between the control and treatment group; the last column contains p-values for a two-sided test of equal means between the control and treatment group. Number of observations: After quality checks 1803 out of 1815 observation were included in the analysis; */**/*** statistically significant at 90%/95%/99% confidence level.

Table A-2: Outcome variables at baseline, endline sample (N=871)

	Mean			
	Full, N= 871	Control, N = 427	Δ(Treat-Control)	p-value
Financial behaviour	**Financial behaviour**			
Has saving account	Has saving account	0.211	0.008	0.782
Does save	Does save	0.496	-0.008	0.819
Education				
Currently enrolled	0.886	0.890	-0.007	0.744
Labour Market				
NEET	0.088	0.084	0.008	0.677
Employed	0.070	0.066	0.009	0.613
Unemployed	0.075	0.066	0.018	0.319
Inactive	0.855	0.869	-0.027	0.267
Any work exp.	0.425	0.429	-0.007	0.825

Notes: the first column presents averages for all observations, the second column for the control group and the third column differences between the treatment and the control group. The last column contains p-values for a two-sided test of equal means between the control and treatment group; */**/*** statistically significant at 90%/95%/99% confidence level.

Table A-3: Outcome variables at baseline, full sample (N=1803)

	Mean			
	Full, N= 1803	Control, N = 891	Δ(Treat-Control)	p-value
Financial behaviour				
Has saving account	0.211	0.212	-0.003	0.889
Does save	0.484	0.501	-0.032	0.169
Education				
Currently enrolled	0.861	0.861	0.000	0.996
Labour Market				
NEET	0.110	0.112	-0.005	0.746
Employed	0.075	0.067	0.016	0.199
Unemployed	0.074	0.072	0.005	0.690
Inactive	0.850	0.861	-0.021	0.214
Any work exp.	0.418	0.409	0.017	0.467

Notes: the first column presents averages for all observations, the second column for the control group and the third column differences between the treatment and the control group. The last column contains p-values for a two-sided test of equal means between the control and treatment group; */**/*** statistically significant at 90%/95%/99% confidence level.