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The impact of skills training on the financial behaviour, employability and educational choices of rural young people

Findings from a Randomized Controlled Trial in Morocco

TAQEEM INITIATIVE

WHAT WORKS In Youth Employment

The impact of skills training on the financial behaviour, employability and educational choices of rural young people

Findings from a Randomized Controlled Trial in Morocco

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International Labour Office Geneva

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Preface

In June 2012, the International Labour Conference of the International Labour Organization (ILO) resolved to take urgent action to tackle the unprecedented crisis in youth employment. The aim was to achieve this through a multi-pronged approach geared towards employment growth and the creation of decent jobs. The resolution "The youth employment crisis: A call for action" contains a set of conclusions that constitute a blueprint for shaping national strategies for youth employment. In 2016, the Global Initiative on Decent Jobs for Youth was launched to facilitate increased impact and expanded country-level action on decent jobs for young people. This will be accomplished through multi-stakeholder partnerships, the dissemination of evidence-based policies and the scaling up of effective and innovative interventions.

The ILO has responded to this challenge by investing more in understanding "what works" in youth employment and supporting governments and social partners to translate evidence into integrated employment policy responses. In 2010 the ILO set-up the Fund for Evaluation in Youth Employment, followed in 2013 by the "Area of Critical Importance: What Works in Skills and Youth Employment" to foster knowledge sharing and provide financial and technical assistance for the rigorous assessment of youth employment interventions. Regional approaches have also been established, including the "Taqeem Initiative: What Works in Youth Employment", which targets ILO constituents in the Arab States and Africa region. Taqeem (meaning "evaluation" in Arabic) applies an iterative cycle of capacity development, impact research and policy influence to improve evidence and help youth employment policy-makers take evidence-based decisions for better resource allocation and programme design.

The "Impact report" series disseminates research reports from Taqeem-supported impact evaluations. Reports include baseline, endline and qualitative studies which describe the impact estimates of evaluations of youth employment interventions based on experimental and quasi-experimental designs.

This report assesses a youth-focused skills training programme in rural and semi-rural Morocco. The intervention delivered financial, life and entrepreneurial skills training, aiming to assist young people with the challenges they face during their transition from school to work. Using a randomized controlled trial, the study explores the medium- to long-term impacts on a range of outcomes related to financial inclusion, employability and human capital accumulation. This report is authored by Jonas Bausch (ILO), Paul Dyer, Drew Gardiner (ILO), Jochen Kluve (Humboldt-University Berlin, RWI – Leibniz Institute for Economic Research) and Sonja Kovacevic (ILO).

The paper was produced in partnership with IFAD as part of an IFAD-financed project, titled "Strengthening gender monitoring and evaluation in rural employment in the Near East and North Africa." Through rigorous impact research, this capacity development and learning grant project aims to understand "what works" in the promotion of gender mainstreaming, with the ultimate goal of reaching gender equality in rural employment outcomes across the region. Co-financing was also provided by the International Initiative for Impact Evaluation, Inc. (3ie).

It is not an easy time to be a young person in the labour market. The topic of youth employment remains a global challenge and a key policy concern for the ILO, governments, trade unions, employers' organizations and the UN system as a whole. Through active participation and collaboration between these groups and young people themselves, we can provide the support needed to help young women and men succeed in the future world of work.

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Executive summary

Background and context

Amid global concerns about the economic exclusion of young people, 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. With young people also facing low labour demand, this has led to poor youth labour market outcomes, increasingly characterized by high unemployment, underemployment and informality. In MENA, close to 90 per cent of young women and about 40 per cent of young men who are not in school are either unemployed or out of the labour force (World Bank, 2012). Moreover, as nearly 85 per cent of young people in the region remain unbanked, inadequate access to financial services is hampering their ability to prepare for their economic future.

This paper evaluates a youth-focused skills training programme in rural and semi-rural Morocco – 100 Hours to Success – using a randomized controlled trial (RCT). The intervention delivered financial, life and entrepreneurial skills training, aiming to assist young people with the challenges they face during their transition from school to work. Youth-focused development organizations throughout MENA increasingly concentrate on providing skills development training to promote financial inclusion and the labour market integration of young people. Despite the proliferation of such training, evidence is still scarce on its effectiveness in addressing the wider challenges of economic inclusion among young people in MENA. As the first RCT in Morocco to examine a skills training programme, this impact evaluation therefore contributes to closing the knowledge gap on what works in youth employment in the region.

Programme description and research questions

The training intervention we assess targets young people between the ages of 15 and 25 living in Morocco's Oriental Region. It focused on young people from rural areas as well as as small semi-rural towns surrounding Oujda, the main city in the region. Its curriculum consists of three main modules, delivered as one training programme over 100 hours of instruction. It focuses on financial education, providing participants with practical tools to help them manage their personal finances. Moreover, it includes sessions on personal competencies and conflict management skills. Responding to low labour demand in the region, the training also covers business and entrepreneurial skills, for example through developing a business plan.

In this paper, we explore the impact of the training on a range of outcomes related to financial inclusion, employability and human capital accumulation. As a direct outcome, we are interested in whether participants demonstrate greater financial knowledge and heightened awareness of banking institutions and their services. As a more demanding outcome, we study whether the intervention influences educational choices, and whether it places participants in a better position to enter the labour force. Across all outcome types, we investigate whether and how impacts differ between women and men, younger and older participants and individuals from more and less affluent households. This allows us to develop additional guidance concerning challenges related to individual background characteristics.

Methodology and data

To rigorously assess the impact of 100 Hours to Success, the study was designed as an RCT aiming to create two groups – a treatment group and a control group – that, on average, have identical characteristics and only differ in terms of exposure to the programme. The study sample includes 1,815 young people who expressed an interest in participating in the training. Baseline data were collected in October 2012, when study participants were on average 20 years old, 7 per cent were employed and almost nine out of ten (89 per cent) were still enrolled in education. Roughly half of these young people (53 per cent) are female. The study assigned 915 individuals to a treatment group and 900 individuals to a control group. All the training was completed by August 2013 and the follow-up data were collected between August and October 2015.

The study employs state-of-the-art econometrical techniques to address two significant challenges. First, take-up rates remained low, with only 469 individuals eventually starting the course and many not attending all the sessions or dropping out altogether. Given the low take-up rates, the report presents local average treatment effects (LATE) obtained by instrumenting treatment with assignment status, in addition to intention to treat (ITT) estimates. As non-compliance is almost exclusively limited to the treatment group, the interpretation of these estimates comes close to an average treatment effect on the treated (ATT). Second, only 871 young people (427 from the treatment group and 444 from the control group) could be located and interviewed for the follow-up survey, implying an attrition rate of just over 50 per cent. The study finds that the remaining sample shows balanced characteristics at baseline. Furthermore, attrition rates do not differ between the treatment and control group overall, and the results are robust to various sensitivity checks, including inverse probability weighting.

However, attrition and non-compliance still represent significant challenges for this study: through the reduced power of the analysis, the statistical uncertainty of our estimates increases and severely constrains efforts to disaggregate findings by relevant socio-economic, demographic and geographical subgroups.

Key findings in implications for policy and research

The analysis suggests that the training programme affected participants in several areas of their lives. Studying financial behaviour, we find that programme participants are considerably more likely to have established a savings account and maintained it more than two years after the end of the intervention. While this effect is robust across socio-demographic subgroups, it does not generally translate into increased use of financial services, such as taking out a loan or saving. However, participants from more affluent backgrounds are more likely to obtain a loan. There is evidence that the effects of the training are conditional on an enabling environment that allows young people to put their new knowledge into practice: when it comes to financial literacy and borrowing behaviours, the effects for young people with fewer assets and for women are much smaller compared with those for men and young people from more affluent backgrounds. Restricted access to loans and other financial services for young people from low-asset households and a lack of autonomy in educational and occupational choices for women are examples of the barriers participants may face.

Turning to more demanding outcomes, we document the impact of the programme on the educational choices of participants. Our results show that men, older participants and young people from more affluent family backgrounds are more likely to postpone their entry into the labour market in favour of continuing their education. Given that investment in education is associated with more promising, economically secure futures, this is a positive although unexpected outcome. However, the effect can only be observed for these three subgroups of participants. We therefore show that skills training and greater awareness of one's own situation might affect life choices, such as increased investment in education, conditional on the set of choices that is financially and socially available. At the same time, there is no evidence that participation in the training programme systematically affected the long-term labour market outcomes or choices related to educational attainment of women, younger and less affluent training participants.

Our findings imply that, by addressing the constraints faced by specific socio-economic groups, skills training interventions could become more effective. Moreover, key barriers to the successful economic integration of young people need to be closely analysed. For example, access to loans could be tackled alongside training, so that young people can leverage the knowledge they gain to pursue their economic interests. Investigating which constraints are most salient is important from a research perspective but also relevant for programme implementers and policy-makers.

Another implication of our analysis is that constructive targeting of programme participants can further augment the effects of the training. We find 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 participants are at a different stage of their transition from school to work. Young people who 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 have learned during the training. Through restricting access to the programme to young people at the end of their education, programme managers and policy-makers could not only leverage scarce resources but also make programmes more effective – in terms of both impact and take-up.

Section 1: Introduction

As of 2015, young people aged 15 to 29 made up 27 per cent of Morocco's total population (Morocco Haut Commissariat au Plan, 2015). The maturation of this cohort has meant that an increasing number are facing the challenges of securing gainful employment as they transition from school to work. With labour demand also low in Morocco, this has led to poor labour market outcomes for the country's young people. At 19.3 per cent, unemployment among this cohort is high – and close to 90 per cent of young women and about 40 per cent 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 per cent of young people remain unbanked – inadequate access to financial services is hampering the ability of young people to prepare for their economic future. These young people face obstacles in establishing a sound financial foundation and obtaining financial services that would empower them more broadly as economic actors, including beginning to save and 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 semi-rural and rural Morocco using an RCT. The training targeted financial, life and entrepreneurial skills to help young people overcome the challenges they face during their transition from school to work. The Youth Employment Inventory (YEI), a database of youth employment interventions, shows that skills training is the most prevalent component of the Active Labour Market Programme (ALMP) portfolios of governmental and non-governmental organizations, designed to equip young people with the skills and knowledge required to enter the world of work. In fact, of the 328 youth employment programmes included in the YEI for the MENA region, around 70 per cent are training programmes 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 programmes 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 majority of skills training evaluation is performed in industrialized countries, the scarce evidence available on low and middle-income countries suggests that skills training is in fact more effective in these very countries (Betcherman et al., 2007; Kluve et al., 2016). As such, investment in skills training for young people in low and middle-income countries might be particularly worthwhile, but the lack of credible evidence prevents governmental and non-governmental organizations from

[1] The YEI is a comprehensive database of comparative information on youth employment interventions worldwide, documenting programme design, implementation and results achieved. It is a joint initiative by the German Ministry of Economic Cooperation and Development, the Inter-American Development Bank, the ILO and the World Bank. The YEI is available at: http://www.youth-employment-inventory.org/.

confidently advising the expansion of these programmes. Moreover, while the evidence base for ALMPs in developing countries has been growing over recent years (Cunningham et al., 2010; Ibarraran and Shady, 2009; USAID, 2013), wide regional and methodological gaps remain. Unfortunately, much of the research is non-experimental (Betcherman et al., 2004; Card et al., 2010; ILO, 2015b), raising doubts as to whether the observed effects can be attributed to the intervention in question. The few available experimental studies that cover youth-focused skills training rarely focus on MENA countries (Attanasio et al., 2011; Card et al., 2011; Cho et al., 2013).

Therefore, the MENA region has by far the largest evidence gap regarding what works in youth employment. 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 has no impact on average labour market outcomes. Second, Premand et al. (2012) evaluate an entrepreneurship training programme for Tunisian students in their final year of university, focusing on skills in business planning and leadership. They found a small effect on the self-employment rate, while the wage employment rate remained unchanged. Nevertheless, as with many other examples of skills training programmes, the intervention succeeded in boosting knowledge, optimism and other behavioural skills.

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

This paper contributes to the global evidence base on skills training, 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 training can enable young people to use opportunities when these are present, but does not fundamentally alter the opportunity structure itself. General programme impacts are concentrated on intermediate outcomes, while more pertinent outcomes are only observable for certain socio-demographic groups. Our second main contribution concerns a new aspect of the effects of skills training. While skills training is widely believed to increase knowledge and might on this causal pathway also impact employment opportunities, we show that greater awareness of one’s own situation also affects life choices, such as increased investment in education.

The intervention we assess targets young people between the ages of 15 and 25 living in Morocco’s Oriental Region. It focused on young people from rural areas as well as as small semi-rural towns surrounding Oujda, the main city in the region. Its curriculum consists of three main modules, delivered as one training course over 100 hours of instruction. It focuses on financial education, providing participants with practical tools to help them manage their personal finances. In addition, personal competencies and conflict management skills are addressed. Given the low labour demand in the region, business and entrepreneurial skills are also covered, for example through developing a business plan.

In this paper, we explore the impact of the training on a range of outcomes related to financial inclusion, employability and human capital accumulation. As a basic outcome, we test whether participants demonstrate greater financial knowledge and heightened awareness of banking institutions and their services. As a more demanding outcome, we study whether the intervention influences educational choices, and whether it places participants in a better position to enter the labour force and increases their prospects when they do so. Moreover, we investigate whether and how impacts differ between women and men, younger and older participants and individuals from more and less affluent households. This allows us to develop additional guidance concerning specific challenges related to individual background characteristics.

Our analysis is based on a sample of up to 1,815 young people (53 per cent female) for whom baseline and endline data were collected in 2012 and 2015 respectively. The evaluation makes use of the random allocation of study participants to a treatment and a control group to identify the causal effects of the programme. We present and compare different estimators, including LATE, to account for imperfect take-up. Extensive statistical and econometrical checks suggest that even though attrition in the follow-up survey is around 52 per cent, it appears to be non-differential between the treatment and control groups and is unlikely to systematically affect our impact estimates.

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

Our second main finding concerns the effects of the training on educational choices. Our estimates of labour market outcomes and educational choices show that men, older participants and young people from more affluent family backgrounds are more likely to continue their education 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 (if unexpected) outcome. However, our first finding on constraints still holds: only those participants whose financial and cultural backgrounds allow for it end up continuing their education. In summary, the study not only explores the conditions under which skills training has substantial impacts but also shows that these interventions may indeed have more far-reaching effects than increasing knowledge in a specific area.

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

Section 2: Intervention

Morocco is a middle-income country with a GDP per capita of around 7,800 US dollars (purchasing power parity) in 2015 (World Bank, 2016). Like other countries in the MENA region, its economy is plagued by slow job growth and high youth unemployment, which stood at 17.5 per cent as of 2012. The youth labour market in Morocco is marked by substantial regional disparities. As of 2012, unemployment rates among young people in the Oriental Region were the highest of any region in Morocco: 41 per cent in urban areas and 21 per cent in rural areas. Economically, its inland location differentiates it from larger port cities such as Casablanca, Tangier and Agadir, which tie Morocco to Europe and sub-Saharan Africa. In addition, it is not a centre for tourism like Fez and Marrakesh. However, given its proximity to the Algerian border, the region serves as a hub for trade between Algeria and Morocco. All this contributes to the Oriental Region being an area in which young people face significant labour market barriers.[2]

Aiming to promote 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 young Moroccan people by bolstering their access to financial services, building their capacity to manage their own finances, improving their job-relevant skills and employability, and encouraging them 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 participants with short but intensive training – totalling around 100 hours of engagement – and was open to all applicants between the ages of 15 and 25 who met basic literacy requirements. The training aimed to address three major skills areas, which 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: financial literacy, life skills and business and entrepreneurship skills. In 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 access financial services more effectively, as well as the fundamentals of starting an entrepreneurial activity,

2 MEDA implemented 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 north-east and south-east of the country. Due to programmatic priorities and operational constraints, many of MEDA's other activities in Morocco under the YouthInvest project had been suspended by 2012, but the 100 Hours to Success training continued to be rolled out in the Oriental Region. None of the study participants were involved in any other activities or programmes offered by MEDA as part of the wider YouthInvest project. More details on YouthInvest, its individual components and its overall theory of change are provided in the 3ie Final Grantee Report of this impact evaluation, available at: http://www.3ieimpact.org/en/evidence/impact-evaluations/details/2503/.

were central. The training had three primary modules that were delivered in a combined fashion, with trainers given flexibility about the order in which they delivered specific parts of each component. Young people were not able to opt in or out of any specific components.

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 micro-enterprise. It included modules on personal budgeting, savings, debt management, banking services and financial negotiations. Teaching was based on materials developed under the Global Financial Education Program, adapted by local staff for young people 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 young people understand and manage emotions, develop confidence and assertiveness, manage and reduce stress, deal with problems and conflicts, and improve their ability 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 inputs from the young people 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 to the identified needs of registered participants. Most classes ran over the course of three months. The normal timeframe 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 partnerships with youth-focused organizations across Morocco, as well as government institutions such as the *Initiative nationale pour le développement humain*. It also provided trained instructors called youth extension officers. During the period of this evaluation, training was provided at local youth centres around Oujda and its outskirts, as well as at several centres located in more rural towns around Oujda (Jerrada and Taourirt). Individual participants applied through the centres closest to them and, upon enrolment, attended courses at the closest local centre to ensure consistent participation.

Overall, the intervention aimed to provide young people with a mix of services to guide them through the transition from school to work. Envisioned outcomes included participants being 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 more active and better able to find employment within the Moroccan labour market. The logic of the intervention rested on three main assumptions: (i) Morocco's basic education system is not providing young people with the skills they need to be successful members of the workforce and needs to be complemented with additional training; (ii) Morocco's economy is generating enough jobs on the demand side for young people with the appropriate skillset; and (iii) 100 hours of training can improve the work-related skills of young people and allow them to find success in the labour market.

Section 3: Research design

Building on the logic of the intervention described above, this study examines specific hypotheses related to the average impact on training participants in two principal areas: first, financial knowledge and behaviour and, second, the labour market outcomes and educational choices of participants. With regard to the first area, the study aims to understand better whether the young people who participated in the training could demonstrate heightened understanding of the functioning and provision of financial services, were better able to manage their personal finances, were 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 regard to the second area, the study explores whether the combined approach to training provided by the intervention (life skills, financial literacy and entrepreneurship training) increased the likelihood of (self-)employment and led to longer spells of employment. Furthermore, the study documents whether and how the training impacted the educational choices of participants (for example through shortening or prolonging their education, seeking a higher level of formal education or trying to enter the labour market as quickly as possible).

The study also examines whether the training had different impacts on selected sub-groups. It seeks to understand the impact of gender as well as of differences associated with socio-economic status. For example, those with higher socio-economic standing might have social and family networks that they can employ more effectively to secure employment than participants from poorer families. At the same time, however, those from comparably wealthier backgrounds are more likely to require higher wages and, as such, to be more selective in the types of jobs that they seek and accept.

3.1 Evaluation design

Obtaining unbiased impact estimates is one of the greatest challenges for any evaluation that seeks to quantify the causal effect of an intervention on the outcomes of interest. Evaluation designs that simply compare programme beneficiaries with 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 and innate ability – may be hard or even impossible to observe. These evaluations are likely to produce incorrect estimates, badly affected 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 of both observable and non-observable characteristics before the start of the programme. Therefore, any differences in outcome averages between the control and treatment groups 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 relied primarily on monitoring data from MEDA, World Bank data (World Bank, 2012) and an evaluation of the INJAZ entrepreneurship programme in Morocco (Reimers et al., 2012). With an envisioned sample of around 1,800 young people, 600 of whom were in the treatment group, the study was expected 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. Aiming to recruit 1,800 individuals for the study from applications submitted by young people, MEDA started promoting 100 Hours to Success in September 2012. The company 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. As a result of 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 young people were randomly sorted into a treatment group, with the 1,215 remaining young people being placed 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 the selection of participants would be executed at random. At the end of November 2012, young people in the treatment group were invited to enrol in the training.

By March 2013, it had become clear that take-up rates were low. Only 336 (out of 600) young people had enrolled in the training.[5] Among other things, this was ascribed to scheduling conflicts and some lack of interest once classes began. Also, a delay in rolling out the classes meant that some members of the treatment group had moved on to other opportunities. In addition, some young people within the treatment group had expected to take the training with their friends; once they found out that the training was not available to these 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.

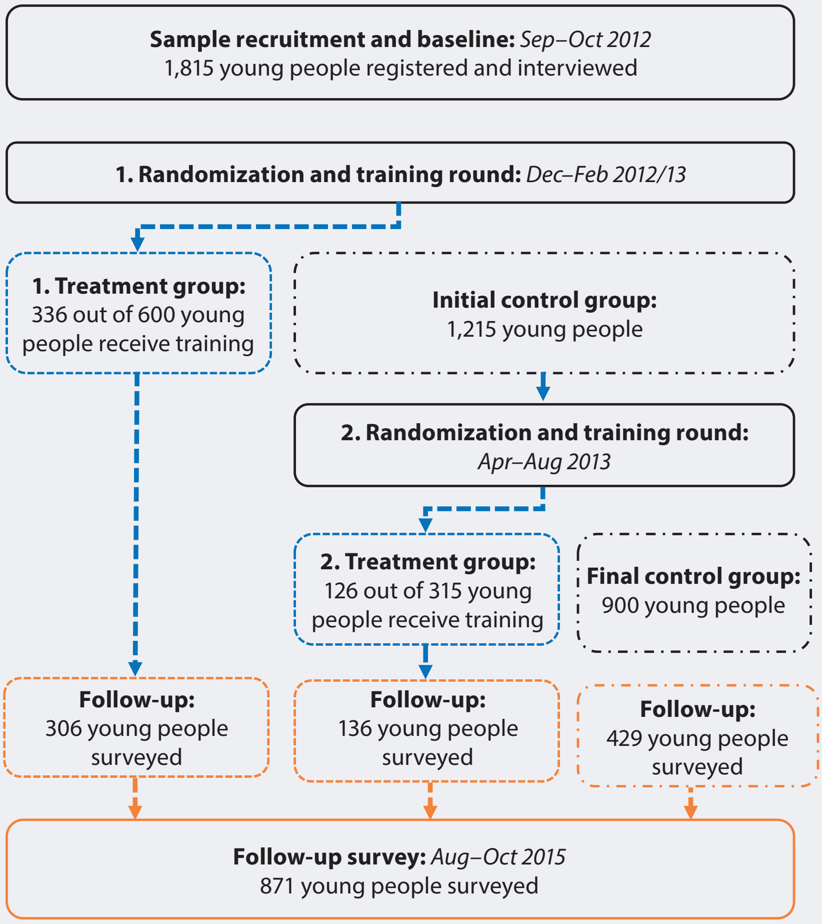
Understanding that the study would not have enough power to demonstrate an impact to the desired level of detail, the research team undertook a second random sorting of individuals from the control group and invited them to participate in the training. More precisely, the

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

4 Randomization was carried out at the level of the individual. Initially, the research team considered randomization at the cluster level (for example, by using catchment areas around youth centres) because training would take place at youth centres and classes at each youth centre would only be able to run with a minimum number of participants. Due to the 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 sufficient numbers of participants in each youth centre to move forward with the courses. As the percentage of young people 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).

5 Herein, by enrolment, we mean that they attended at least one training session. In fact, a considerable number of those who enrolled in the training did not attend all of the sessions. Section 4.2 discusses the low overall take-up and its implications for evaluating the impact of the programme.

Figure 3.1 Evaluation design



team placed 315 randomly chosen individuals, previously assigned to the control group, in the treatment group, with the final treatment group totalling 915 individuals and the control group totalling 900 individuals. Limited take-up remained a challenge, however, as only 126 of the 315 individuals allocated to treatment through this second wave of randomization actually 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 addition, 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]

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

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

For the follow-up survey, which took place from August to October 2015, another survey 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 survey methodology. Members of the research team also observed data collection during the first few days of fieldwork. During data collection for the follow-up survey, 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 young people were interviewed in person during the follow-up survey, which took place more than two years after the intervention ended.

When enumerators were not able to contact a study participant directly during 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 their current labour market status. Enumerators obtained proxy data for 493 additional individuals. However, due to serious concerns about the validity of these data, we limit our analysis to the 871 observations obtained through face-to-face interviews with study participants.[7]

[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 had obtained the data from “someone else” (in many cases themselves or their own family members). According to these 293 observations, only 1 per cent of study participants were enrolled in education or training, compared with 52 per cent of the 871 respondents interviewed for the follow-up survey. This seems unlikely to be true. Rather, these data entries appear to be consistent with interrogators mechanically ticking “no” for the few questions in this section. This also brings into question the data quality for the remaining 200 cases where a parent, family member, friend or neighbour supposedly answered. Inspection of these data reveal very large differences between this small group (i.e., the remaining 200 individuals for whom proxy information are available) and all the young people who responded to the survey questions in person. In particular, they are dramatically less likely to be enrolled in education or training (51 per cent vs. 25 per cent) or to be employed (26 per cent vs. 20 per cent), while they are significantly more likely to be NEET (31 per cent vs. 41 per cent). These results are not just compositional effects, as cited differences barely change when covariates such as gender, age and household assets are included.

Section 4: Description of study sample

Our analysis builds on data from both the baseline and follow-up surveys. After we cleaned the baseline data for data entry errors and incomplete responses, 1,803 out of 1,815 observations remained (referred to hereafter as the baseline sample). Of these 1,803 individuals, 891 were assigned to the control group and 912 to the treatment group. We have follow-up data available for 871 individuals (referred to hereafter as the endline sample) who were interviewed in person, amounting to an overall attrition rate of 51.7 per cent. Of those, 427 had been randomly assigned to the treatment group, while 444 had been assigned to the control group. The high rate of attrition reflected in these figures is a concern that we address in detail below (see Sections 4.1 and 5.3).

In this paper, we present baseline characteristics only for those individuals for whom endline data are also available.[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 semi-rural (vs. rural) area, as well as information about the participant's household (number of siblings, whether the participant is living in a dormitory, whether their father is still alive, self-reported satisfaction with overall household situation) and, in particular, information on the head of the household (gender and level of education). Furthermore, we include a household asset index based on a principal component analysis of 23 self-reported durable household assets, such as number of telephones and cars and availability of a refrigerator.[9] Finally, we include details of whether the respondent has participated in any other skills training programmes.

As Table 4.1 shows, 48 per cent of study participants reached in the follow-up survey were women, and participants were on average 20 years old at baseline. Overall, 16 per cent were living in a dormitory – reflecting a substantial share of young students or workers living outside their parental home. The average study household size was around five people. Only 13 per cent of respondents had previously participated in a skills training programme outside the formal education system. Table 4.1 also displays differences between the control and treatment groups. These differences are generally small and none are significant at the 95 per cent confidence level. However, individuals in the control group are less likely to reside in semi-rural areas and less likely to live in a household headed by a woman (both significant at the 90 per cent level).

Table A.2 in the appendix shows the baseline values for several outcome variables for the young people interviewed as part of the follow-up survey. Almost nine out of ten (89 per

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

9 First, we conduct a principal component analysis using all 23 self-reported durable household assets. Next, 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 a mean of 4.02 (median: 3.94), with a standard deviation of 0.91.

cent) of these young people were still enrolled in education at the time of the baseline survey; roughly one in five (21 per cent) said they had their own savings account; and almost half (49 per cent) said they regularly saved money. The differences between the 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 groups appear to be roughly similar with regard to observable characteristics. This is a strong sign that – for the full sample at baseline, at least – randomized sorting did indeed result in two groups that are sufficiently similar in terms of characteristics. Table A.1 and Table A.3 also display background characteristics and outcome variables for the baseline sample.

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
Semi-rural	0.781	0.808	-0.053	0.057*
Living in dormitory	0.164	0.157	0.014	0.571
No. of household members	4.912	4.972	-0.118	0.397
Female head of household	0.126	0.148	-0.042	0.064*
Education level – head of household (0–6)	1.642	1.644	-0.004	0.971
Father alive	0.902	0.895	0.015	0.448
Household assets (1–10)	4.055	4.071	-0.030	0.605
Satisfied with household 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

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

Finally, when comparing our study sample with the overall youth population in Morocco's Oriental Region – using a representative World Bank survey[10] – we find that our survey population is 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 better educated in the future.

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

4.1 Attrition

Attrition is always a concern for the 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 groups: so-called *differential attrition*. In this case, attrition might invalidate the randomized research design by making the treatment and control groups incomparable. However, attrition rates in the control group (52.1 per cent) and the treatment group (51.3 per cent) are almost identical, with the difference being non-significant (p-value: 0.75).[11] Therefore, even though this study faces a high overall attrition rate, it does not seem badly affected by differential attrition.

In this section, we further investigate whether individuals who dropped out of the sample differ from those who were observed twice (at 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 the control and outcome variables of both groups one by one. While this gives a first impression of whether there are variables able to predict attrition, it does not consider the potential correlations between these variables. Therefore, we focus on the second approach, estimating a multivariate regression that tries to predict attrition by taking into account all the included variables simultaneously.

Table 4.2 displays the results of regressing a binary variable, whereby all the 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 worrying 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 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 being 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 follow-up is no longer fully comparable with the baseline sample. This affects the external validity of the findings: the endline sample differs from the baseline sample and can therefore not capture the population represented at baseline. However, this is of little importance in this study, as the baseline sample was not selected at random from the general Moroccan youth population. Still, the study sample might well reflect the characteristics of those young Moroccan people who are interested in skills training.

[11] Note also that when splitting the sample according to actual treatment status, the difference remains small and insignificant: 50.9 per cent among all individuals who 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 4.2.

Table 4.2 Determinants of inclusion in follow-up survey (N = 1,803)

	Dependent variable: Individual observed in follow-up			
	Probit models (average marginal effects)			
	(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
Semi-rural		**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*Assigned			**-0.028**	**-0.031**
			0.047	0.047
Semi-rural* Assigned			**-0.100***	**-0.097***
			0.054	0.055
Dormitory* Assigned			**0.037**	**0.039**
			0.060	0.060
Father alive*Assigned			**0.084**	**0.086**
			0.084	0.084
Full set of controls	**No**	**No**	**No**	**Yes**
N (persons)	1,803	1,803	1,803	1,803

Note: 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. This is threatened only if estimates of the propensity to leave the study differ between the treatment and control groups. Thus, in column (3) and (4) we interact the four variables that are associated with dropping out in the endline survey (gender, semi-rural vs. rural, staying in a dormitory and whether the father is still alive) with treatment 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 who were assigned to the control group. In fact, the only interaction term significant at the 10 per cent level relates to individuals assigned to the treatment group who live in semi-rural areas. The coefficients in column (4) imply that (only) study participants in the control group who live in semi-rural

[12] The full set of control variables includes all the 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 shows an impact significant at conventional confidence levels.

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 semi-rural study participants in the control group by using inverse probability weighting as a robustness check for our results (see Section 5.3).

4.2 Take-up

As stated above, low programme take-up is of concern in this study. Out of the 900 young people who 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 attended only some of the training sessions. Consequently, the average attendance rate for a person initially assigned to the treatment group is around 35 per cent (36 per cent when restricted to those included in the follow-up survey). For our analysis, we consider all young people who attended at least half of the sessions as treated, provided they attended at least one session in the second half of the course.[13] As per these criteria, 352 young people (39 per cent of those initially assigned to the treatment group) are considered treated.[14]

Low take-up rates do not introduce bias into the analysis but limit the interpretation of results in two ways. First, rather than observing the effects of the treatment, we estimate the average **intended** impacts of the programme on the study population (as well as the average impacts on those considered treated). Second, low take-up reduces the power of the study, making it harder to detect smaller impacts across outcome categories. Although low take-up rates hamper the precise estimation of the programme's impacts, studying them facilitates meaningful interpretation of the study's results. Differential take-up by background can also shed light on how and why the effects of the treatment differ.

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 participation. These results hold both when treatment is modelled with a binary dummy as per the above definition (i.e. having attended at least half the classes offered) and for specifications where the intensity of treatment is taken as a 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: first, it seems that training is most appreciated by those individuals who are older and therefore more likely to apply their knowledge through financial activities and entrepreneurship. Although

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13 In this paper, we refer to this group as treated individuals, not to be confused with the **treatment group**, which consists of all the individuals randomly assigned to receive the treatment.

14 More precisely, 360 young people attended at least 50 per cent of the sessions offered to them, and eight are considered drop-outs due to non-attendance of the second half of the course. According to MEDA's own initial requirements, participants were expected to attend at least 75 per cent of classes to successfully graduate. However, it should be noted that MEDA did not necessarily stick to this requirement for the cohort of young people participating in this study, given the poor overall attendance rates. Therefore, many young people graduated who might have missed more than a quarter of the classes, but still showed an interest in continuing the training.

we cannot determine which individuals in our sample are at the end of their education – and therefore likely to decide in the near future on potential entrepreneurial activities and options for employment – it seems that offering training to these groups would be especially worthwhile. For a short training course like 100 Hours to Success in particular, it seems overly optimistic to think that individuals with several years of schooling to complete before they become economically active will remember and apply the knowledge they acquire so much later in their lives. Second, women are around 10 percentage points more likely to take up training across a variety of specifications. Finally, programme take-up seems not to be driven primarily by initial labour market status or enrolment in education. For both variables, estimated coefficients are negative – perhaps suggesting that young people in full-time education or work are less able or willing to commit to an additional training course – but not statistically significant. Taking these patterns together, our analysis suggests that subgroup dynamics play an important role in our sample, and we pay special attention to exploring differential impacts across the 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
Semi-rural	**-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**

Note: "Full set of controls" also includes whether participants are living in a dormitory, number of siblings, whether the father is alive, whether the head of the household is female, education level of the head of the household, and self-reported satisfaction with the household situation. In the first two columns we use the binary definition of "drop-out" where more than 50% participation counts as treated; in the last 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 are displayed below impact estimates. */**/*** = statistically significant at 90%/95%/99% confidence level.

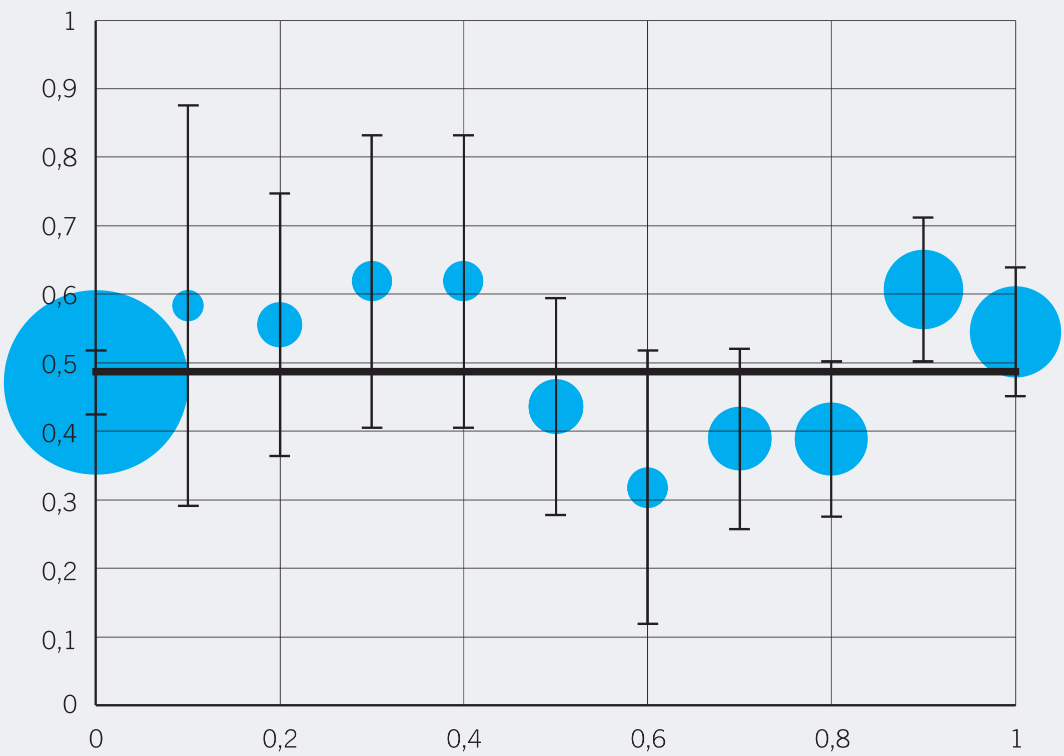
Table 4.4 Compliance in follow-up sample (N = 871)

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 are shown in brackets.

As mentioned in Section 4.1, non-differential attrition is crucial to obtain unbiased estimates. Whether an individual receives treatment should not affect whether they drop out. Figure 4.1 explores this relationship between attrition and take-up. For each level of attendance (x axis), the corresponding percentage of young people included in the follow-up survey (y axis) is depicted.[15] Figure 4.1 clearly demonstrates that young people

Figure 4.1 Observation rate in follow-up survey by treatment intensity and treatment group (N = 912)



Note: We plot the likelihood of being included in the follow-up survey (y axis) against treatment intensity by groups. The size of the circles is proportional to the number of observations in the respective groups. 95% confidence intervals are plotted for all groups. The black line represents the average follow-up inclusion rate (48.7%) for the treatment group.

15 This is 1 minus the attrition rate.

who attend more classes do not show a significantly higher propensity to complete the follow-up questionnaire. Attrition rates vary unsystematically around the average (51.3 per cent) 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.

Section 5: Results

When sorting in the treatment group is randomized, it is sufficient to compare average outcome levels between the treatment and control groups 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 groups.[16] Through this difference-in-differences (DiD) approach, we control for any differences between the control and treatment groups that stay constant over time. Second, 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 therefore estimate:

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

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 survey (conditional on covariates and fixed effects) and α_2 describes the average difference between baseline and follow-up for the whole sample. In addition, $\boldsymbol{X_{it}}$ 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 1 for all young people who participated in the programme – and zero otherwise. To the extent that individuals do not comply with their initial treatment status, it differs from treatment **assignment**.[20]

16 We use the terms "comparison group" and "control group" interchangeably.

17 The balancing tables presented in Section 4 show that control variables do not differ significantly between the treatment and comparison groups. Adding them in equation (1) therefore first and foremost increases statistical precision.

18 These are: dummy for living in an semi-rural/rural area, household size, self-assessed satisfaction with household situation and dummy for whether the individual has 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 any more. In specifications without an individual fixed effect, the coefficient of this dummy would amount to the average difference in the outcome variable between treated individuals and the rest of the sample at the time of the baseline survey. 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 at 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 who were randomly assigned to the control group and the treatment group, respectively. When mentioning young people who we consider to have participated in the training, we refer to "treated young people".

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 1,800 respondents, our ability to elaborate on the nuances of financial behaviour and labour market outcomes is limited by an attrition rate of just over 50 per cent. Additionally, non-compliance among some of the treated population has diluted the power of the study. Keeping in mind the considerable non-compliance rate, we present and compare three different effect types.

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. Therefore, impact estimates are based on a comparison between all the young people who underwent the training and everyone else, irrespective of initial random assignment. However, actual participation in the training is likely not to be random, meaning that this estimate is likely to be biased. Participants could, for example, choose (not) to attend the course depending on whether they expected to benefit from it. While our OLS estimator controls for several observed time-varying and all unobserved time-invariant characteristics, we cannot exclude the possibility that young people who chose to attend the training differ fundamentally from those who 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 by 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 groups and do not consider whether individuals actually did (not) participate. Therefore, unlike ATT estimates, ITT effects capture the average effect on individuals from the population who 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 a two-stage least squares estimator where (1) represents the second stage and the first stage regression is given by:

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

This procedure relies on the (testable) assumption that while participation in 100 Hours to Success is likely not to be random (but driven by self-selection), offering the programme to some people at random increased their inclination to participate. Despite a substantial share of study participants who did not comply with treatment assignment (see Section 4.1), random assignment status easily passes conventional tests for instrument relevance. Young

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

people assigned to the treatment group were on average 38 per cent more likely to participate in the training programme than individuals in the control group.[22]

In general, LATE have a restricted interpretation: they are estimates of the average effects on so-called **compliers**, individuals who participated in the training **because** they were assigned to the treatment group.[23] For this study, as there is close to zero non-compliance in the control group, our LATE conceptually coincide with ATT. Therefore, substantial differences between ATT and LATE estimates could be an indicator that one (or both) techniques produce biased results.

We will therefore present, compare and discuss (i) ATT, (ii) ITT effects and (iii) LATE for a range of outcome variables. Before presenting and discussing detailed results on a range of outcomes, we note that 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 these cases, OLS estimates indicate a more positive impact than the instrumental variable (IV) approach, which is likely to be due to selection bias in programme take-up: it seems that those individuals who would have had better outcomes with respect to financial literacy and behaviour and labour market status were more likely to take up the intervention. Third, while LATE coefficients appear conceptually 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 standard OLS estimates with the IV approach. This implies that we only obtain statistically significant impact coefficients for considerably 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 (which are clearly flagged in the results tables[25]), impact

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 stages. First, we calculate differences between all the individuals **assigned** to the treatment group and the control group. This corresponds to the ITT effect described above. Second, this difference is scaled up by 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 who were treated nevertheless – so-called **always-takers**; and (ii) the higher the proportion of individuals assigned to the treatment group who 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, at 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, cluster standard errors do not tend to have desirable properties for small numbers of clusters. Our dataset features only 13 distinct youth centres serving as centres for baseline data collection, with more than 95 per cent of observations coming from only ten centres, which cautions against clustering at this level.

25 Tables display whether the variable in question was observed before and after the intervention or recorded only 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.

estimates are based on adapted versions of equations (1) and (2) that also include 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 entire 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. Therefore, we limit the number of dimensions along which we split our sample and avoid disaggregation criteria that would lead to a very uneven split. 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 report only LATE estimates. As mentioned above, their interpretation comes close to an average treatment effect on the treatment group. Moreover, we believe LATE estimates to be more credible than OLS impact coefficients, which are likely to be negatively affected 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 lines, but not expect to obtain precisely identified point estimates.

5.1 Financial behaviour

When studying the financial literacy and behaviour of participants in 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 the outcomes that are likely to change, as participants were strongly encouraged (but not mandated) to open a savings account upon enrolling in the training. Notably, the rate of young people with a bank account among our control group is quite high (35.6 per cent), given the age of our target population and the lack of financial services for young people across Morocco. The World Bank found in its 2009–10 survey of young Moroccans that about 12 per cent maintained savings accounts in a formal financial institution (World Bank, 2012). Relative to the overall youth population, there is therefore 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 of maintaining a savings account. The ATT specification (Table 5.1) estimates that participants are 15 per cent more likely to have a savings account than those who did not participate. The LATE is even larger at 27 percentage points, an estimate that is significant at the 1 per cent level. For this particular outcome, the substantial difference between the two estimates is likely to be driven by the definition of treatment: only young people who participated in more than 50 per cent of classes were considered as treated, while those who attended only a few classes might also have been encouraged to open a bank account. A closer inspection of the data reveals that, among young people who attended at least one training session,

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

about 54 per cent have a savings account, compared with only around 36 per cent of 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, the 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 of the programme on opening a bank account are almost automatic, as relatively little initiative from participants is required for them to materialize. However, the strong effects show that young people participating in the training did comply with the recommendations and encouragement they were given. Furthermore, it is worth noting that most young people still had the bank account they opened two years after the end of the training.

Table 5.1 Impact estimates for financial literacy and behaviour

		Comp. group:	(1)	(2)	(3)
	DiD	Mean (st. dev.)	ATT	ITT	LATE
Financial literacy index	No	**0.499**	**0.064*****	**0.016**	**0.042**
		0.285	0.024	0.020	0.052
Has savings 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**

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

When studying standard measures of financial behaviour, such as saving and borrowing, effects are generally small and insignificant. Importantly, all these measures of financial behaviour are self-reported and might therefore be subject to perception bias. One possible avenue for validation is to complement them with side-effects of this very same behaviour. If individuals maintain a budget, for example, 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 related to saving and lending services, which 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 shows the combined percentage of correct answers (knowledge items) and positive answers (self-assessed knowledge).[27] 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 adopt more active financial behaviours, although the effects are limited.

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
Financial literacy index	No	**0.515**	**-0.016**	**0.483**	**0.096**	**0.241**
		0.293	0.065	0.278	0.084	
Has savings 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**	

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

To check whether the relatively small impacts 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 participants and negative for male participants. However, estimates fall outside the bounds of statistical significance due to the small sub-samples, despite substantial effect sizes. It is also noteworthy that the already large effect for maintaining a savings account 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

[27] Note that the financial literacy index is therefore 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.

account. Together, these observations might indicate that women were more responsive to the training than men. This – at least in part – could be a catching-up effect, as women in the control group show lower levels of financial activity across various measures 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 and/or knowledge but also on the opportunities individuals are given because of their socio-economic backgrounds. Although the training may increase participants' 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, as shown by credit ratings). Our findings suggest that socio-economic background, approximated by household assets, does indeed shape the effects of the training. Based on 23 self-reported household assets, we computed a scale for each respondent.[28] To capture heterogeneity by background, we divided respondents into two categories: those in the top half of the asset ranking and those in the bottom half.

Table 5.3 Impact estimates for financial behaviour by household assets

		Household assets lower 50%		Household assets top 50%		
	DiD	Comp. group: Mean (st. dev.)	LATE	Comp. group: Mean (st. dev.)	LATE	p-value
Financial literacy index	No	**0.454**	**0.096**	**0.542**	**-0.008**	**0.305**
		0.283	0.064	0.281	0.082	
Has savings 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**	

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

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

Table 5.3 shows the estimated impacts of the training on financial literacy and financial behaviour outcomes for both subgroups. Overall, participants from more affluent households are more likely to significantly alter their financial behaviour. Although participants from both more and less affluent households are significantly more likely to maintain a savings account after the training, the effect is substantially larger for the former group (32 compared with 21 percentage points). Moreover, this group is 20 percentage points more likely to have borrowed compared with similar young people in the less affluent group. These wealthier young people might be encouraged through the training to see borrowing as a viable option, helping them to leverage future earnings to attain their financial goals. Young people from less affluent households may also be encouraged to seek out loans; however, this encouragement may be met with external barriers. Given their socio-economic status, they may not have the collateral or credit reputation to secure initial loans from financial institutions. It should be noted that some microfinance institutions in Morocco accept savings as collateral if those savings reach a certain percentage of the loan, which should make such loans more accessible to people from less affluent households. However, if young people are not saving (or able to save), then this requirement remains a barrier to borrowing. Alternatively, these young people may have decided that loans are not appropriate for them at this point in time, and chosen not to put themselves in debt.

Table 5.4 Impact estimates for financial behaviour by age group at baseline

		Aged 20+		Under age 20		
	DiD	Comp. group: Mean (st. dev.)	LATE	Comp. group: Mean (st. dev.)	LATE	p-value
Financial literacy index	No	**0.476**	**0.166*** **	**0.529**	**-0.129**	**0.004**
		0.281	0.064	0.289	0.089	
Has savings 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**	

Note: The "DiD" column indicates whether the outcome variable was observed only in the follow-up survey ("No") or also at baseline ("Yes"). The next column shows the mean and standard deviation in the control group at the time of the follow-up survey. The last column shows the p-value for a test of equal impact coefficients between subgroups (in a specification that allows different coefficients for covariates). Robust standard errors are displayed 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 that poorer participants, although stimulated by the training and motivated to work on financial issues, cannot realize their potential due to the financial constraints in their background. One 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 training can be translated into higher levels of financial activity.

Finally, Table 5.4 displays estimates of financial outcomes, splitting the sample into younger participants (19 years or below at the time of the baseline survey in 2012) and older participants (20 years and older). In this subgroup analysis, the impact on financial literacy is strongly positive and highly significant for older participants (a standard deviation of around 0.6), while the coefficient for younger participants even turns negative (but is statistically insignificant). Conversely, maintaining a savings account seems to be concentrated among younger individuals, with a significant positive impact on both age groups (at the 10 per cent 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 in the areas of education and the labour market. Comparing key educational and labour market indicators at baseline with follow-up data three years later, we can see that the intervention reached a population who were in the middle of the transition from school to work. While labour market participation (in the control group) increased from 13 to 47 per cent 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 per cent.

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

Regarding educational outcomes, ATT estimates on educational enrolment are positive and statistically significant (as shown in Table 5.5). The fact that their corresponding LATE coefficients are much smaller (and lose significance) once again suggests that the decision to take part in the programme was selective. The LATE specification indicates that treated participants are slightly more likely to remain in education, even though the estimates lack statistical significance.

29 Most of the statistics presented in this paragraph are taken from Table 5.5 and Table A.2 in the appendix. To ensure comparability, the averages presented at the time of the baseline survey are restricted to the comparison group only. However, differences between the comparison and treatment groups at baseline are marginal.

We also 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 a NEET (not in education, employment or training) indicator, capturing individuals who are inactive in the labour market and not enrolled in education,[30] and record whether respondents have any work experience to date and, if so, the number of months worked.[31] 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 for a meaningful quantitative analysis.

Table 5.5 Impact estimates for education and employment outcomes

	DiD	Comp. group:	(1)	(2)	(3)
		Mean (st. dev.)	ATT	ITT	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**

Note: The “DiD” column indicates whether the outcome variable was observed only in the follow-up survey (“No”) or also at baseline (“Yes”). The next column shows the mean and standard deviation in the control group at the time of the follow-up survey. Robust standard errors are displayed 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 shows that, for four out of six labour market outcomes, we do not find a statistically significant impact of the 100 Hours to Success programme. Labour market status is the exception, where the LATE specification indicates that programme participation reduced

30 Enrolment in education also includes secondary schools, vocational schools and universities.

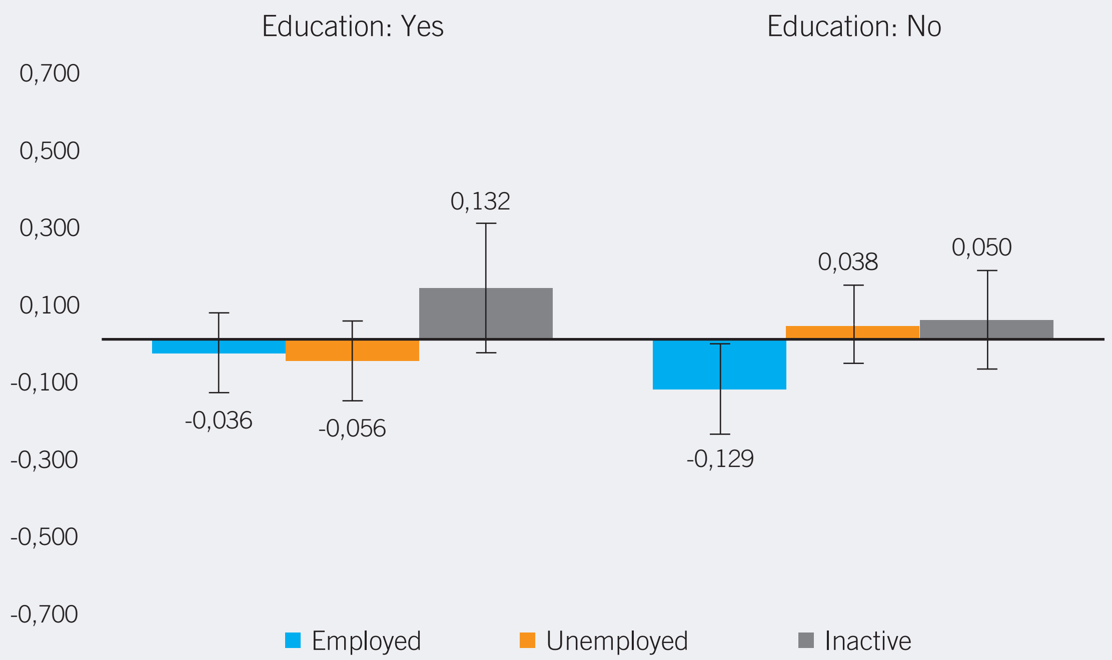
31 Work experience (number of months) captures the current job (if any) and up to the last three jobs of respondents (95% of respondents indicated in the follow-up survey that they had held three jobs or fewer).

participants' likelihood of being employed by 16.5 percentage points – a result that is statistically significant at the 90 per cent 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. Although they lack statistical significance, the results for the other three outcome indicators are in line with this observed decrease in the probability of employment. Training participants on average have less work experience and a (slightly) higher risk of being classified as NEET.

The observation that programme 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 (secondary school, vocational school or university) or no longer enrolled. Combining both dimensions results in six mutually exclusive outcome categories. For example, at the time of the baseline survey, 79 per cent of young people (from the restricted sample of 871 observations used for impact analysis) were still enrolled in education and inactive in the labour market; 6 per cent were not in education and inactive in the labour market; and 5 per cent were enrolled in education while at the same time looking for work.

Figure 5.1 presents LATE impact estimates based on both baseline and follow-up data for these six categories. Note that as each young person belongs to exactly one category, the six impact estimates automatically add to zero. Figure 5.1 documents three important findings.

Figure 5.1 Impact estimates for educational enrolment and labour market status, full sample (N = 871)



Note: We estimate impacts on six mutually exclusive categories, combining educational enrolment (yes/no) with labour market status (employed/unemployed/inactive). Impact estimates deliberately sum to zero. 90% confidence intervals based on robust standard errors are displayed.

First, almost three-quarters of the increase in inactivity can be attributed to individuals who have remained in education: the increase in inactivity and being enrolled in education is 13.2 percentage points, compared with 18.3 percentage points when disregarding educational enrolment (as shown in Table 5.5). Second, some programme participants seem to stay longer in education, even though this effect is not significant. Third, among all of those enrolled in education, fewer are currently employed or looking for work. These findings are consistent with programme participants who invest in and focus more on human capital accumulation through (formal) education and training, rather than acquiring early labour market experience.

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 be enrolled in education or training (57 per cent vs. 46 per cent). At the same time, women are much less likely to be employed (12 per cent vs. 44 per cent) and much more likely to find themselves outside the labour force (73 per cent vs. 34 per cent) than their male counterparts.

Compared with the impact estimates above, two interesting trends emerge from this disaggregation. First, women show less of a tendency to remain in education, while impact on labour market outcomes shows conflicting results. Importantly, none of the coefficients reaches statistical significance. Second, the tendencies observed for the whole sample seem to be coming entirely from men. Impacts on remaining in education are stronger (but still insignificant), while there is an enormous exit from the labour force (rise in inactivity) – around twice as large as for the overall sample (and highly significant). These observations are confirmed by Figures 5.2 and 5.3, which combine labour market status and educational enrolment and mirror the analysis presented in Figure 5.1 for the whole sample. Coefficients for women are 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 for an increase in the probability of being inactive (48 percentage points) is broken down into increased inactivity while remaining in education (39 percentage points) and being inactive while not pursuing further education (9 percentage points). Moreover, this seems to be driven both by men who stay longer in education and men who, while remaining in education, reduce their labour market activity.

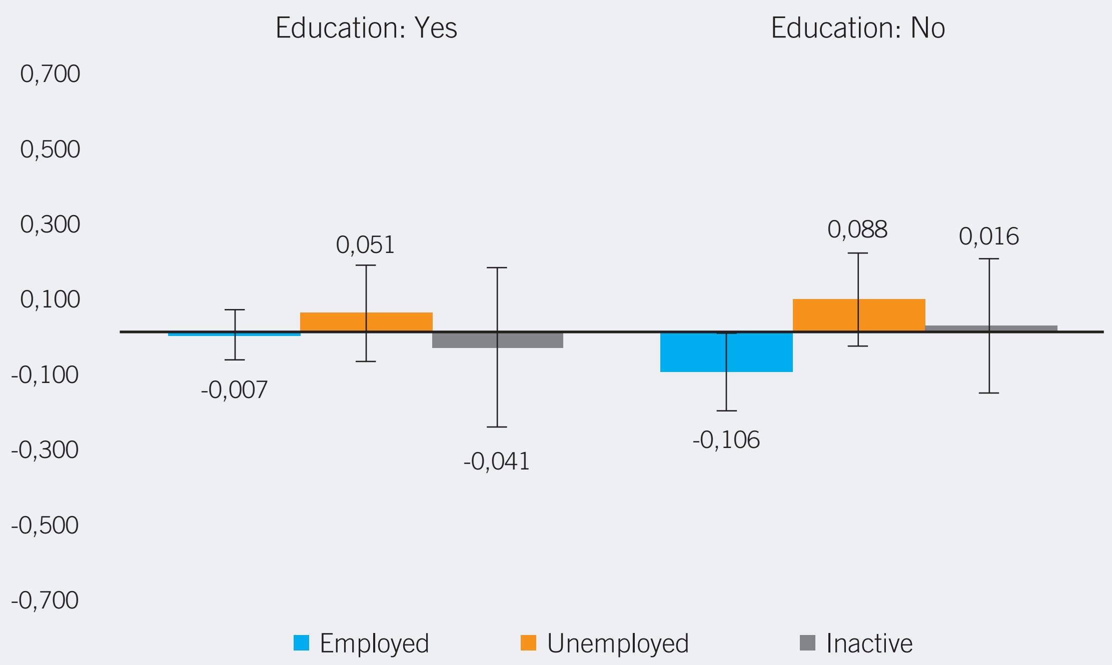
Table 5.6 Impact estimates for education and employment outcomes by gender

		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	

		Women		Men		
	DiD	Comp. group: Mean (st. dev.)	LATE	Comp. group: Mean (st. dev.)	LATE	p-value
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**	

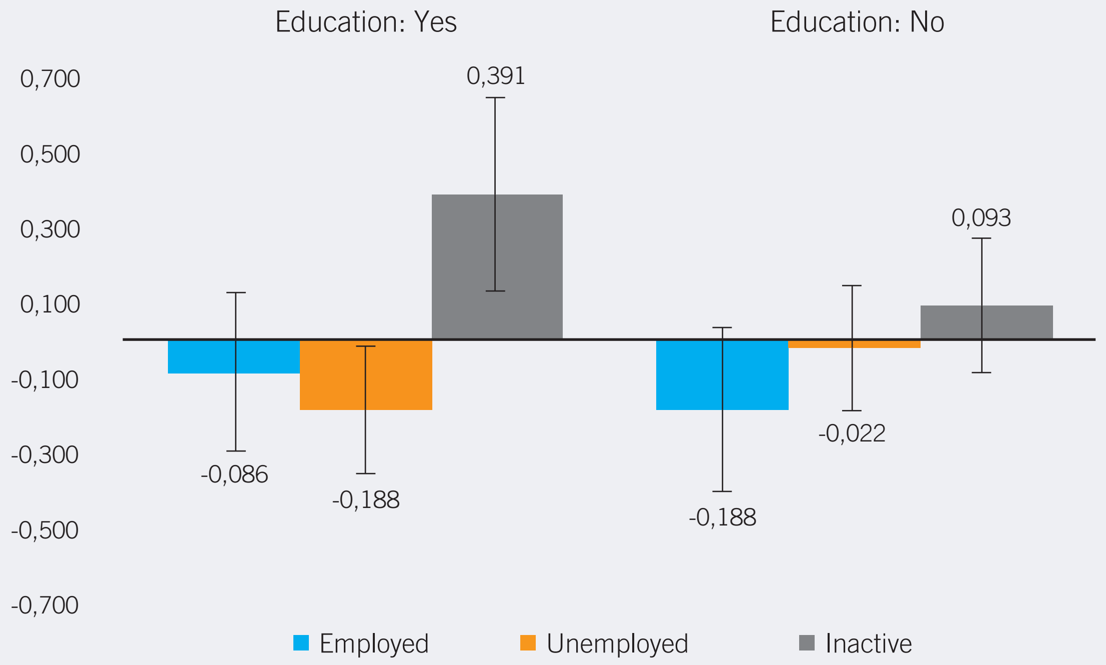
Note: The "DiD" column indicates whether the outcome variable was observed only in the follow-up survey ("No") or also at baseline ("Yes"). The next column shows the mean and standard deviation in the control group at the time of the follow-up survey. The last column shows the p-value for a test of equal impact coefficients between subgroups (in a specification that allows different coefficients for covariates). Robust standard errors are displayed 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 observations for men.

Figure 5.2 Impact estimates for educational enrolment and labour market status, women (N = 414)



Note: We estimate impacts on six mutually exclusive categories, combining educational enrolment (yes/no) with labour market status (employed/unemployed/inactive). Impact estimates deliberately sum to zero. 90% confidence intervals based on robust standard errors are displayed.

Figure 5.3 **Impact estimates for educational enrolment and labour market status, men (N = 457)**



Note: We estimate impacts on six mutually exclusive categories, combining educational enrolment (yes/no) with labour market status (employed/unemployed/inactive). Impact estimates deliberately sum to zero. 90% confidence intervals based on robust standard errors are displayed.

Next, in Table 5.7 we report the results of the training on educational and labour market outcomes by level of household assets. There are no major differences when comparing the averages for young people in the control group from more and less affluent backgrounds. However, we find an interesting difference with respect to impact estimates. Young people

Table 5.7 **Impact estimates for education and employment outcomes by household assets**

		Household assets lower 50%		Household 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	

		Household assets lower 50%		Household assets top 50%		
	DiD	Comp. group: Mean (st. dev.)	LATE	Comp. group: Mean (st. dev.)	LATE	p-value
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**	

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

from less well-off backgrounds do not seem to react systematically to the intervention, which is also true when looking at the combined educational and labour market outcome categories displayed in Figure 5.4. By contrast, study participants from more affluent households show a pronounced increase in labour market inactivity which is – as Figure 5.5 reveals – accompanied by a prolonged stay in education.

Finally, we disaggregate the sample into older participants – 25 years old on average at the time of the follow-up survey – and younger participants, who were on average 20 years old at the time of the follow-up survey (see Table 5.8). Older participants in the control group are – as we might expect – less likely to still be enrolled in education, and tend to have attained a higher level of education than younger participants. However, this is not associated with better labour market outcomes, either in terms of employment or months of accumulated work experience. The fact that – disregarding programme impact – participants who are on average five years older fail to perform better in the labour market is a telling sign of the long and difficult school to work transition period that many young Moroccans seem to face. In terms of impact, coefficients generally lack statistical significance and add little to the tendencies observed for the overall sample. Combining employment and labour market outcomes (Figures 5.6 and 5.7) reveals that older participants seem to be focusing on, and staying in, education rather than trying to enter the labour market.

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 participants to conclude that further investment in education would be preferable to entering a difficult, largely informal labour market in helping them meet their long-term goals. For women, younger and less

Figure 5.4 **Impact estimates for educational enrolment and labour market status, bottom 50% of household assets (N = 436)**

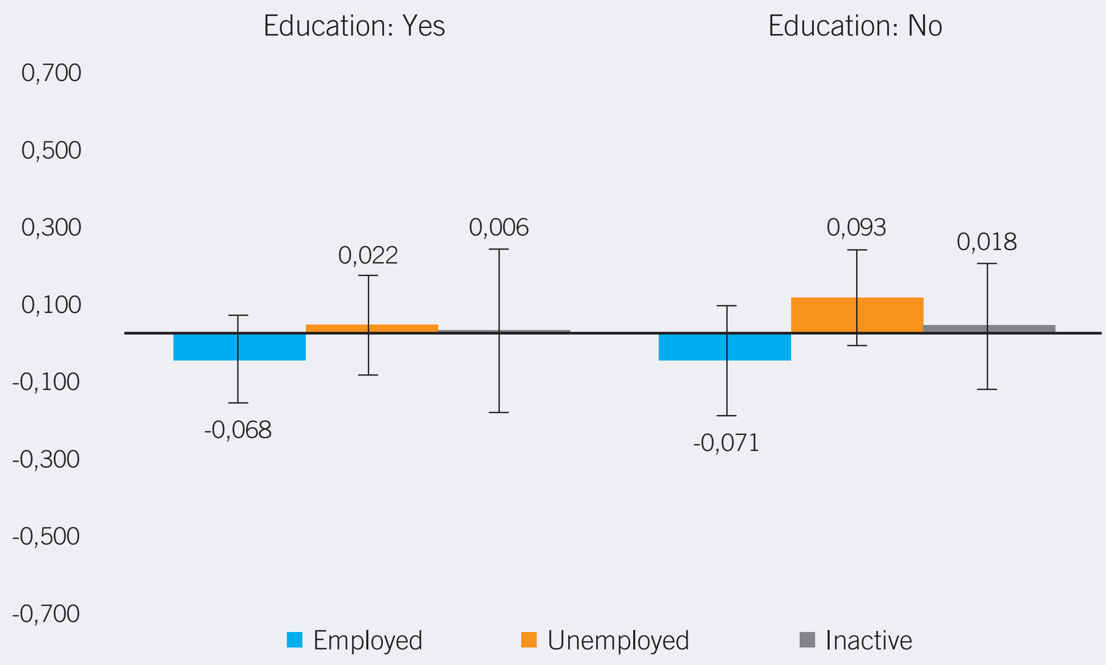
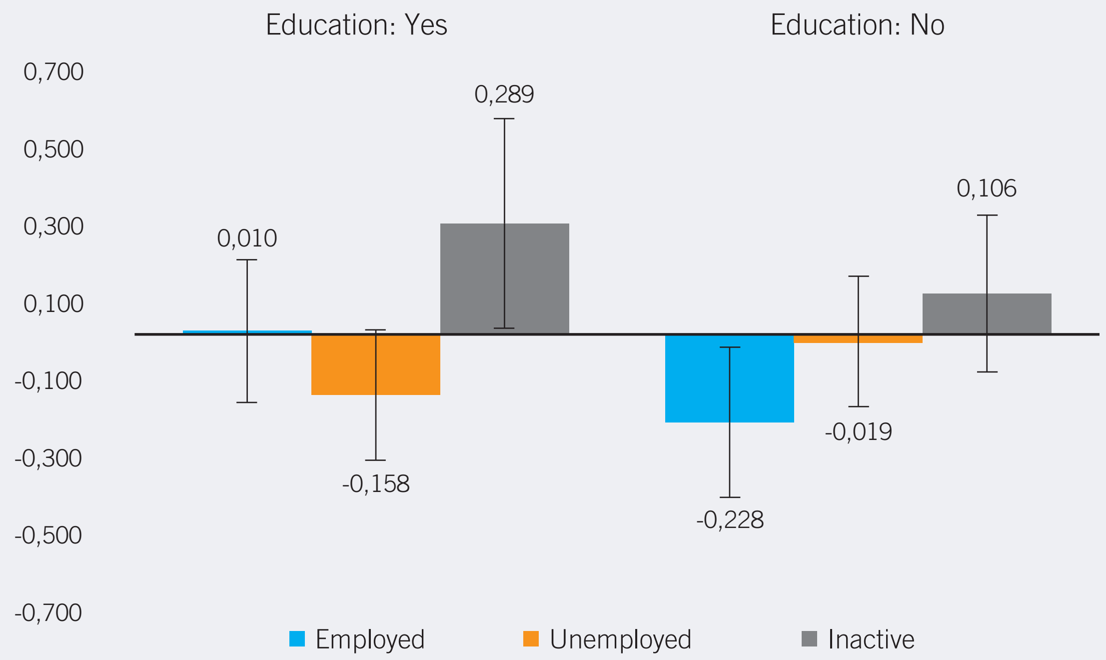


Figure 5.5 **Impact estimates for educational enrolment and labour market status, top 50% of household assets (N = 435)**



Note: We estimate impacts on six mutually exclusive categories, combining educational enrolment (yes/no) with labour market status (employed/unemployed/inactive). Impact estimates deliberately sum to zero. 90% confidence intervals based on robust standard errors are displayed.

Figure 5.6 **Impact estimates for educational enrolment and labour market status, age <20 (N = 390)**

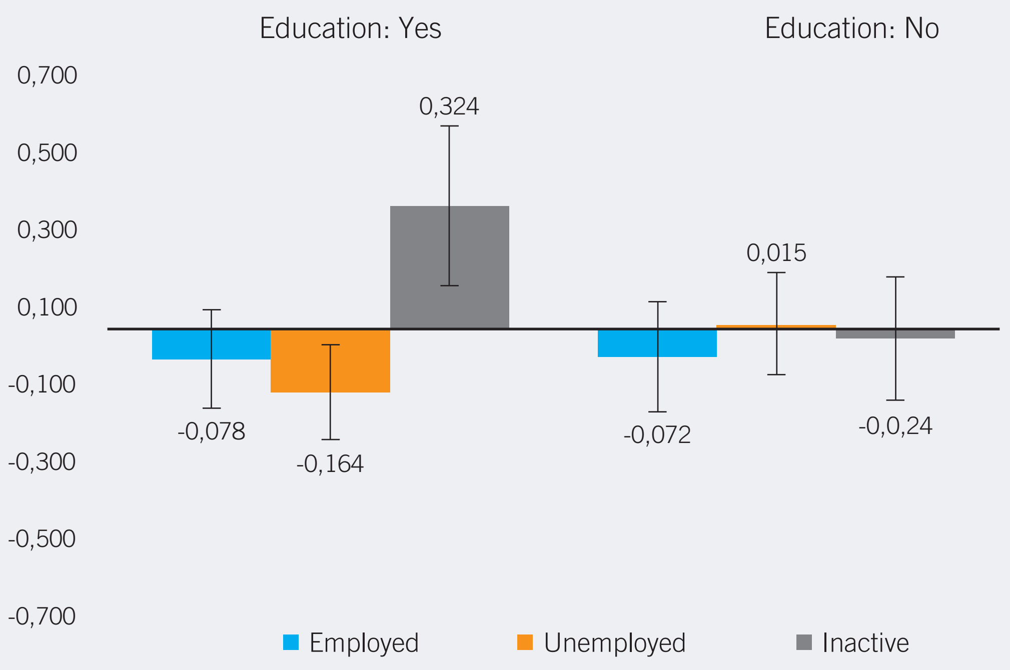
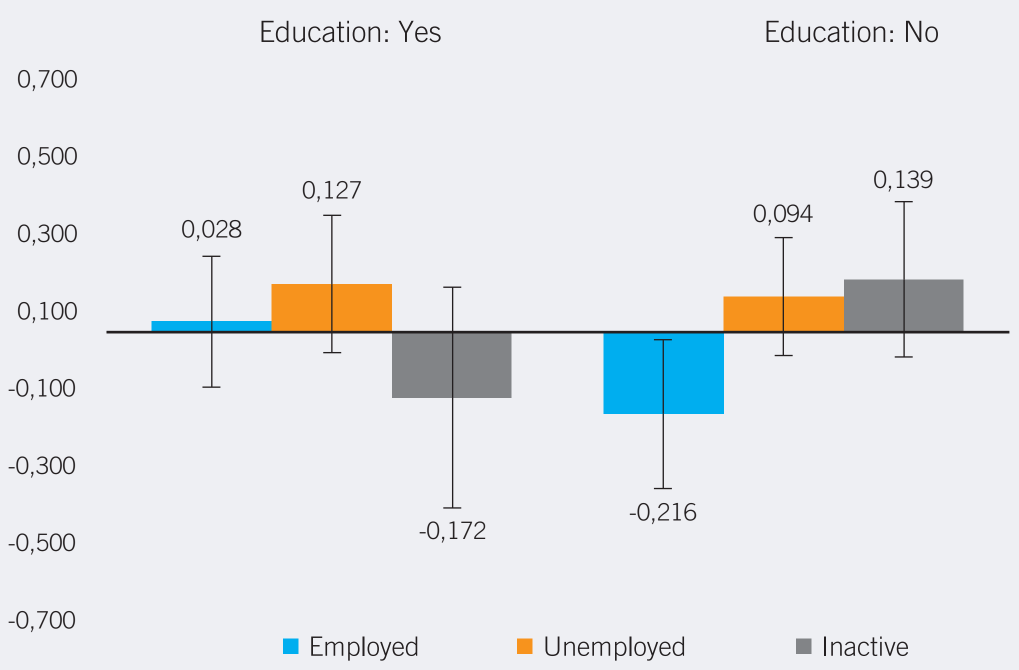


Figure 5.7 **Impact estimates for educational enrolment and labour market status, age 20+ (N = 481)**



Note: We estimate impacts on six mutually exclusive categories, combining educational enrolment (yes/no) with labour market status (employed/unemployed/inactive). Impact estimates deliberately sum to zero. 90% confidence intervals based on robust standard errors are displayed.

affluent participants, data suggest that the training might have left their labour market outcomes and decisions on pursuing further education largely unaffected.

Table 5.8 **Impact estimates for education and employment outcomes by age group at baseline**

		Aged 20+		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**	

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

5.3 Robustness check: inverse probability weighting

We attempt to correct for potential biases due to differences in the probability of semi-rural study participants in the control group being observed twice by using **inverse probability weighting (IPW)**. In doing so, we first estimate a probit model including all variables and interaction terms listed in column (4) of Table 4.2. Second, we predict for every individual the probability of being included in the follow-up survey. Third, we use the inverse of this probability as a 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, and who are therefore presumably more like the people who were not included in the follow-up survey.

Table 5.9 Impact estimates for financial literacy and behaviour – 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 savings 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**

Note: The "DiD" column indicates whether the outcome variable was observed only in the follow-up survey ("No") or also at baseline ("Yes"). The next column shows 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, and columns (3) and (4) compare LATE estimates. Robust standard errors are displayed below impact estimates. */**/*** = statistically significant at 90%/95%/99% confidence level.

Tables 5.9 and 5.10 display the normal (non-weighted) estimates in column (1) ITT and column (3) LATE, and the IPW estimates in column (2) ITT and column (4) LATE. Overall, the estimates are very similar, and all results are robust to using inverse probability weights when considering statistical significance.[32]

In summing up the discussion about the attrition rate in the follow-up survey and its implications for the internal validity of the 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 per cent, overall we do not observe differential attrition rates in the treatment and comparison groups. 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 pinch of salt because the small sample we are left with makes it difficult to precisely identify differences.

[32] Note 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 IV techniques with IPW (column 4 of Tables 8.2, 8.3 and 8.4) have been estimated without including individual fixed effects (but including all the time-varying and time-invariant control variables described in Section 7). To allow direct comparability, the coefficients displayed in column (3) also omit individual fixed effects. Differences are marginal and for all practical purposes negligible.

Table 5.10 Impact estimates for education and employment outcomes – IPW

			ITT		LATE	
	DiD	Comp. group: Mean (st. dev.)	(1) Standard	(2) IPW	(3) Standard	(4) IPW
(i) Education						
Enrolled	Yes	**0.513**	**0.016**	**0.008**	**0.042**	**0.020**
		0.500	0.036	0.037	0.103	0.103
(ii) Labour market						
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**

Note: The "DiD" column indicates whether the outcome variable was observed only in the follow-up survey ("No") or also at baseline ("Yes"). The next column shows 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, and columns (3) and (4) compare LATE estimates. Robust standard errors are displayed below impact estimates. */**/*** = statistically significant at 90%/95%/99% confidence level. ♦: for months of work experience, the models are based on 790 observations.

Third, comparing our remaining sample with participants who could not be observed for the follow-up survey, we see some stark differences with respect to observable characteristics. However, with one exception (semi-rural young people) this does not correlate with treatment assignment. Our results are robust when correcting observable differences in our remaining sample through IPW. Fourth, we are aware that our analysis cannot take into account unobservable factors that might have influenced attrition rates selectively, but it is encouraging that within the cohort of young people assigned to the treatment group, attrition does not vary by actual treatment status and varies unsystematically by treatment intensity (see Figure 4.1).[33]

[33] It is worth restating that attrition overall is balanced between those assigned to the control group and those assigned to the 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, the outcomes of these two subgroups would have to be negatively correlated with one other.

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

Section 6: Conclusion

In light of a persistent global youth employment challenge, identifying interventions that can effectively increase the inclusion of young people in economic markets is a mounting priority. The impact evaluation of MEDA's 100 Hours to Success skills training programme, the first youth-focused RCT in Morocco, 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 in several ways which have important implications for future programming, in policy as well as in 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 in financial behaviour, such as increased borrowing or saving. The effects of the training differ substantially depending on participants' age, social background and gender. Smaller effects for financial literacy and borrowing behaviours for young people from households with fewer assets and for women suggest that the beneficial impact of skills training can only unfold in an enabling environment that allows beneficiaries to put their new knowledge into practice. Restricted access to loans and other financial services for young people from low-asset households and a lack of autonomy in educational and occupational choices for women are examples of the barriers participants may face.

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

Further research is needed to better understand the constraints young people 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 young people who receive the training stay longer in education and postpone their entry into the labour market. Again, impacts vary across subgroups as these effects are driven by older participants, young people from more affluent family backgrounds and men. Scrutinizing heterogeneous outcomes more closely is hugely valuable for policy-makers. When exploring and addressing constraints such as financial limitations on continuing with education or family obligations, the greater effects found for male, older and more affluent participants could be extended to the whole sample, thereby increasing the average effect. These differences also indicate knowledge gaps about the effects along the results chain, which 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 the training itself might motivate young people to extend their knowledge.

Another implication of our analysis is that better targeting of programme participants can further augment the effects of the training. For programme implementers, this deduction is especially useful, because it allows them 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 participants find themselves at a different stage of the transition from school to work. Young people who will still be in education for a couple of years before starting to search for a job will not be able to apply what they learned in the training straight away. Therefore, any gains in terms of knowledge or skills relevant to the labour market are likely to have dissipated by the time they leave school or university.

For programmes that target (self-)employment outcomes, it might therefore be advisable to restrict access to those at the end of their education. This could, furthermore, include a careful screening process 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. More careful targeting might also lead to higher take-up rates, and dedicated studies of the take-up of skills training programmes could be particularly worthwhile. Studying whether and how rewards for completing training can increase take-up rates – and what the determinants of dropping out are (such as distance to the training location) – can contribute to an improvement both in the evaluation of training and the training itself.

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

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Appendix

Table A.1 Control variables at baseline, full sample (N = 1,803)

	Mean			
	Full, N = 1,803	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
Semi-rural	0.750	0.756	-0.013	0.523
Living in dormitory	0.192	0.192	0.001	0.954
No. of household members	4.903	4.910	-0.013	0.888
Female head of household	0.119	0.125	-0.011	0.490
Education level – head of household (0–6)	1.655	1.662	-0.014	0.865
Father alive	0.915	0.914	0.003	0.814
Household assets (1–10)	4.024	4.050	-0.053	0.216
Satisfied with household 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

Note: The first column presents averages for all observations, the second column presents averages for the control group and the third column shows differences between the treatment and control groups. The last column contains p-values for a two-sided test of equal means between the treatment and control groups. After quality checks, 1,803 out of 1,815 observations 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				
Has savings account	0.215	0.211	0.008	0.782
Does save	0.492	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

	Mean			
	Full, N = 871	Control, N = 427	Δ (Treat–control)	p-value
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

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

Table A.3 Outcome variables at baseline, full sample (N = 1,803)

	Mean			
	Full, N = 1,803	Control, N = 891	Δ (Treat–control)	p-value
Financial behaviour				
Has savings 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

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

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