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# The impact of youth skills training on the financial behaviour, employability and educational choice in Morocco

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# **The impact of skills training on the financial behaviour, employability and educational choice of youth in Morocco**

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## Summary

In the context of global concerns about the economic exclusion of youth, efforts to facilitate youth access to decent jobs and financial services have become a development priority. This is particularly the case in the Middle East and North Africa (MENA) region, where continued growth of the youth population has exacerbated pressures on education systems and labour markets. This has contributed to poor labour market outcomes for young people, increasingly characterised by high unemployment, underemployment and informality. In MENA, nearly 85 per cent of youth are also unbanked; this inadequate access to financial services hampers their ability to prepare for their economic futures.

Aiming to increase the financial inclusion of young people and improve their employability, youth-serving development organisations are increasingly focused on providing interventions centred on skills development training. But despite the proliferation of such training interventions, there is limited evidence of their effectiveness. This impact evaluation report – a collaboration between the ILO's Taqeeem Initiative and MEDA, funded by 3ie – contributes to the growing base of evidence on what works in youth employment and financial inclusion.

### Programme description and research questions

The report evaluates a skills training intervention – '100 Hours to Success' – that targeted youth aged 15 to 25 in Morocco's Oriental region. It consisted of 3 main modules, delivered as 1 training course over 100 hours:

- **Financial education:** Practical tools to manage personal finances, including how to use personal budgeting and the importance and relevance of savings and debt management;
- **Life skills:** Improving personal competencies and conflict management; these units relied heavily on role play and group work; and
- **Business and entrepreneurial skills:** How to assess one's business development abilities, market products and write a business plan.

This study seeks to assess the impact of '100 Hours to Success' on a range of outcomes related to financial inclusion, employability and human capital acquisition. The main research questions included:

- Do training participants demonstrate greater financial knowledge and heightened awareness of banking institutions and their services?
- To what extent has training changed beneficiaries' perception of their own capacities in a broad array of life skills, such as confidence, teamwork and problem solving?
- Has the intervention influenced educational choices?
- Has it placed participants in a better position to enter the labour force?
- Has it increased their chances of securing employment or starting new businesses as youth entrepreneurs?

- Do impacts differ between women and men, younger and older participants and individuals from more and less affluent households? If yes, how?

## **Methodology and data**

To rigorously assess the impact of '100 Hours to Success', the study was designed as a randomised controlled trial (RCT) aiming to create two groups – a treatment and a control group – that, on average, share identical characteristics and only differ with respect to programme exposure. The study sample included 1,815 youth who expressed interest in participating in the training. We collected baseline data in October 2012, when study participants were on average 20 years old; 7% were employed and almost 9 out of 10 (89%) were still enrolled in education. Roughly half (48%) were female.

Randomisation took place in two waves. At first, we offered 600 youth a place in the training. Following low take-up, we randomly drew another 315 from the initial control group 4 months after the first cohort started classes, to increase statistical power. In total, the study assigned 915 youth to the treatment group and 900 to the control group. All training was completed by August 2013 and the follow-up data were collected between August and October 2015.

This 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 sessions or dropping out altogether. As a response, next to intention to treat estimates, the report presents local average treatment effects (LATEs) obtained by instrumenting treatment with assignment status. 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. Models include a set of (time-varying) control variables and individual fixed effects. Second, we could only locate and interview 871 youth (427 from the treatment and 444 from the control group) for the follow-up survey, implying an attrition rate of just over 50 per cent. The remaining sample showed balanced characteristics at baseline, and attrition rates did not differ between the treatment and the control group overall. The results were robust to various sensitivity checks, including inverse probability weighting.

## **Key findings**

This evaluation suggests that the '100 Hours to Success' training affected participants in several ways:

- It had a strong positive and highly significant impact of 27 percentage points on participants' likelihood to maintain a savings account more than 2 years after the end of intervention. This effect was consistent across gender, age groups and household asset levels. The effect on maintaining a savings account was stronger among women (32 percentage points) than men (21 percentage points). This suggests that women without exposure to the

training are less inclined to maintain an independent savings account (due to cultural norms);

- Older individuals also seemed to benefit more from the financial knowledge and awareness training, showing a pronounced and significant impact of 0.6 standard deviations on the financial literacy index. There is weak evidence that this was also true, to a smaller extent, for men and youth from more affluent households;
- There is no evidence that the effects on maintaining a savings account and financial literacy translate into impacts on actual savings, nor is there any statistically significant increase in self-reported use of a budget in maintaining personal finances;
- There is evidence that participants from more affluent households were more likely to have borrowed since the start of the training, perhaps encouraged to see borrowing as a viable option to leverage future earnings and attain financial goals. And although participating may have encouraged youth from less affluent households to seek out loans, a lack of collateral or reputational credit might have constrained higher rates of borrowing;
- The study finds no evidence of long-term effects on participants' self-efficacy and self-reported capacities for leadership, teamwork, problem solving and willingness to take risks. The timing of the follow-up survey (a year later than initially planned) is likely relevant: three years after the baseline survey, any effect is likely to have faded or been overcome by other factors of influence, including the struggle to secure employment or to achieve other long-term goals;
- We find mixed effects on labour market participation and educational choice, with male participants, older participants and those from more affluent households significantly more inclined to stay in education while at the same time remaining outside the labour force. For all three subgroups, the results are driven by two trends: participants tended to remain longer in education and, if they were in education, were less likely to look for a job or to work. This behaviour seems to be consistent with youth investing more in education, both through longer attendance and by devoting less time to labour market activities. There is some rationale for considering that the subgroups' exposure to the training led some of their members to consider that investing in education would help them meet their long-term goals more than entering a difficult labour market; and
- There is no evidence that participating in the training systematically affected long-term labour market outcomes or choices related to educational attainment for women, younger and less affluent training participants.

Interpreting these results, we must bear in mind the major challenges this study faced. While estimating LATEs through an instrumental variable approach addresses the considerable degree of non-compliance and low take-up, it comes at the cost of increased statistical uncertainty and a higher risk of not detecting programme impacts. High attrition in the follow-up survey, which essentially halved the sample

size compared with the baseline survey, further reduces the study's statistical power. This severely limits options to disaggregate findings by relevant socio-economic, demographic and geographical categories.

### **Key recommendations for policy and practice**

This study assesses the impact of a skills training intervention on youth in Morocco. Overall, its findings point to positive, but limited, impacts on financial outcomes: participants were more inclined to maintain a savings account but showed little change in financial behaviour. While there is no evidence of long-term impacts on training participants' life skills, we find that men, younger individuals and youth from more affluent households tend to focus more on post-secondary education and postpone entry into the labour market.

Based on these findings and the evaluation at large, our key recommendations are:

*For programme developers and policymakers:*

- Further investments in financial literacy training would enable greater youth inclusion in the financial system, particularly in Morocco and the MENA region;
- Investing in business training would help sharpen youth investments in education and prepare them as economic actors more broadly;
- Adequate targeting of beneficiaries is key to ensure that training participants can more directly benefit from the training provided to them. Depending on desired outcomes in regard to employment and self-employment, programme implementers might decide to restrict programme access to those at the end of their educational careers; and
- Targeting could include a careful screening on the basis of ambition and aspiration. 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) small grants and access to loans.

*For researchers:*

- Future research to evaluate youth-serving programmes should focus on building evidence on the appropriate mix of training, and validate whether soft skill training produces only short-lived impacts, if any;
- Given that, in this study, impacts on financial literacy and maintaining savings accounts did not result in more financial activity, it seems vital to understand better how relatively short classroom-based trainings can impact financial behaviour more effectively;
- It would also be interesting to identify more precisely the key constraints for a successful school-to-work transition. As such, we recommend evaluating the pay-off from focusing more on formal education and training instead of seeking labour market experience early on, as we observed among men, older participants and youth from more affluent backgrounds;

- Evaluators should anticipate challenges arising from the high mobility of young people, which implies frequent changes of addresses, emails and phone numbers and renders traditional contact information requests relatively ineffective; and
- In designing identification strategies for training and other programme evaluations, researchers should be mindful that youth often enrol in programmes for social reasons, and simple randomisation strategies can be undermined when beneficiaries find their friends excluded as part of the control group. Finding creative ways to randomise by friend groups may promote take-up while providing opportunities to analyse spillovers and other concerns.

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## Abbreviations and acronyms

ANAPEC	Morocco's National Agency for the Promotion of Employment and Skills
ATT	average treatment effect on the treated
DID	difference-in-differences
FE	fixed effects
HH	household
ILO	International Labour Organization
INDH	Morocco's National Human Development Initiative
IPW	inverse probability weighting
ITT	intention-to-treat (effect)
IV	instrumental variable
LATE	local average treatment effect
MAD	Moroccan dirham
MEDA	Mennonite Economic Development Associates
MENA	Middle East and North Africa
MFI	microfinance institution
NEET	not in education, employment or training
NGO	non-governmental organisation
OLS	ordinary least squares
RCT	randomised controlled trial
s.d.	standard deviation
USAID	United States Agency for International Development

# 1. Introduction

## 1.1 Youth economic exclusion in MENA and Morocco

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). While the share of youth among the total population is gradually declining, the maturation of this cohort has ensured that an increasing number are facing the challenges of securing gainful employment as they transition from school to work. At 19.3%, unemployment among this cohort is high – 19.5% for men and 18.1% for women – and close to 90% of young women and about 40% of young men who are not in school are either unemployed or out of the labour force (World Bank 2012).

Beyond the widely reported obstacles they confront in the employment sphere, Moroccan youth face a broader challenge of economic exclusion. They struggle to establish a sound financial foundation and obtain financial services that would empower them more broadly as economic actors, including savings or loans to leverage future earnings (Dhillon *et al.* 2009; World Bank 2012). According to the World Bank, adult Moroccans under the age of 35 have the lowest awareness of financial providers and their services (World Bank 2014). Another World Bank report found that 81.4 per cent of surveyed youth in Morocco in 2009–2010 identified access to enterprise capital as the key obstacle to establishing and running their own business (World Bank 2012).

Governmental and non-governmental organisations (NGOs) have increasingly turned to youth-targeted, supply-side interventions to equip Moroccan young people with the skills and knowledge they need as economic actors and to enter the world of work. Notably, around 70 per cent of the 328 youth employment initiatives included in the Youth Employment Inventory (YEI) for the Middle East and North Africa (MENA) region are training programmes or maintain training as a core element in a mix of youth services.<sup>1</sup>

These programmes are designed to address two things: a reported skills gap between labour market entrants and the needs of employers, and the poor outcomes of previous, demand-side focused efforts to stimulate job creation within the private sector. In response to these issues, the programmes have increased training in soft, basic employment, job searching and, to a lesser extent, financial literacy skills. They also focus more on promoting youth entrepreneurship through training, start-up incubation and access to finance.

This programmatic shift has occurred with little evidence of the effectiveness of such programmes in addressing the wider challenges of economic inclusion among the

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<sup>1</sup> YEI is a comprehensive worldwide database of comparative information on youth employment interventions. It documents programme design, implementation and achieved results. YEI is a joint initiative by the German Federal Ministry for Economic Cooperation and Development, the Inter-American Development Bank, the International Labour Organization and the World Bank. To access the database, visit: [http://www.ilo.org/employment/about/news/WCMS\\_146149/lang--en/index.htm](http://www.ilo.org/employment/about/news/WCMS_146149/lang--en/index.htm)

region's (and Morocco's) youth. It is in this context that we assess the impact of the '100 Hours to Success' training programme in Morocco by Mennonite Economic Development Associates (MEDA).

## 1.2 MEDA's '100 Hours to Success' training

Aiming to provide a more inclusive approach to youth economic engagement in Morocco, MEDA launched its YouthInvest project in Morocco in 2008. Through YouthInvest, MEDA and its local institutional presence MEDA Maroc sought to promote better economic outcomes for Moroccan youth by:

- bolstering access to financial services
- building their capacity to manage their own finances
- improving their job-relevant skills and employability
- providing on-the-job experience through internships
- encouraging youth to create their own employment solutions through entrepreneurship.

MEDA's approach aims to provide youth with a mix of services that will help them bridge a number of interrelated challenges as they transition from school to work.

MEDA's '100 Hours to Success' course, the focus of this impact evaluation study, was a primary component of the larger YouthInvest project. This core training course was tailored to provide participating youth with short, intensive training concentrated on three main topics:

- **Financial services and personal finance management:** to encourage savings, set them on a pathway of responsible personal finance management and prepare them to begin managing their own business finances;
- **Life skills:** to raise self-confidence and enable them to effect positive changes in their own lives and in their communities; and
- **Entrepreneurship:** to develop basic skills and gain experience to start up a business and facilitate the transition into the labour market.

With this mix of training, '100 Hours to Success' aimed to provide eligible youth with a foundation on which to build their careers as economic actors, whether in employment or starting their own businesses. The activity-based modules encouraged participants to learn through experience and applied problem solving, rather than depending on traditional lectures. The goal of this evaluation is to test the efficacy of this training as a means of improving young people's financial inclusion as they transition from school to work and begin to establish their lives as economic actors.

## 1.3 International evidence on job skills and financial capacity building

While evidence on the impact of skills training programmes is growing steadily, not many of these studies have used experimental research methodologies to prove they provide positive employment benefits for young people. There has been a great deal of research on active labour market programmes using non-experimental designs,

including useful overviews by Card, Kluve and Weber (2010) and Betcherman, Olivas and Dar (2004). Although these studies do not identify any one single measure that has had a significant effect across the programmes surveyed, they provide relative rankings of interventions by effectiveness. One meaningful result from this literature is that programmes targeting a specific age group – such as under 25 – are less effective than those targeting the population as a whole.

Available evaluation evidence on skills training is mainly based on programmes in developed countries, although the number of evaluations from developing countries is rapidly growing (see Ibarraran and Shady 2009; Cunningham, Sanchez-Puerta and Wuermli 2010; USAID 2013). A review of impacts across country income levels showed that skills training tends to yield higher impacts for youth in low- and middle-income countries compared with those in high-income countries (Betcherman *et al.* 2007).

Some of the most revealing evidence from developing countries comes from a randomised controlled trial (RCT) in the Dominican Republic, which found that job skills training had a positive (albeit unsustainable) effect on wages but none on employment (Card *et al.* 2011). This training was coupled with an internship; notably, no participants were offered regular employment at the end of their internships.

A more recent randomised trial using a larger sample in Colombia found stronger impacts (Attanasio, Kugler and Meghir 2011). The programme, Jóvenes en Acción, involved three months of classroom training by private agencies, followed by three months of vocational training in a private company. The probability of employment rose and wages were, on average, higher for programme participants, especially women. But we do not know whether this positive effect was due to the vocational training, the internships or the combination of the two. Training may improve productivity, but internships can reveal useful information about the quality of a match.

An assessment of a training programme in Malawi (Cho *et al.* 2013) targeting the self-employed found that both self-assessed profitability and knowledge about business practices increased marginally. But time spent in training came at a cost for participants, who saw no significant gain in earnings and their savings decrease.

The largest evidence gap is in the MENA region. The YEI reveals only six impact evaluations on the topic within the region; and only two of particular importance for skills training:

- Groh *et al.*'s study (2012) on wage subsidies and soft skills training for female community college graduates in Jordan found that, while wage subsidies effectively increased employment in the short term, the soft skills training programme had no impact on average labour outcomes; and
- Premand *et al.*'s evaluation (2012) of an entrepreneurship training programme teaching business planning and leadership skills to Tunisian students in their final year of university found a small effect on the self-employment rate but no change in the wage employment rate. But, as in

many other skills training programmes, the intervention did boost knowledge, optimism and other behavioural skills.

There is less evidence on the link between access to financial services and training, and none of them focus on youth specifically:

- Banarjee *et al.* (2015) reported that a large-scale, multi-country programme providing grants with training on personal finances and life skills effected a small but statistically significant gain for the ultra-poor in terms of household consumption, assets and food security;
- Cole *et al.* (2014) reported that, although a financial literacy programme in South Africa improved knowledge about budgeting, increased self-reported savings and reduced loan applications, they found no significant improvements in other types of financial knowledge or practices; and
- In their meta-analysis of financial literacy interventions Fernandes *et al.* (2014) concluded that, when there are any, the impacts of such interventions on financial outcomes and behaviours are generally small and often come at a high cost.

Overall, the international experience with training suggests that internships, which mix training with job experience, have only marginal positive employment impacts. This less-than-favourable outcome is reflected in training programmes that focus on financial literacy and behaviours. Such outcomes are likely to depend on the type of training available – for example, life skills, vocational skills, financial skills or a mix of such training – as well as its quality and duration. There is not enough international evidence on which to base solid policy recommendations. Given the diversity of approaches and quality, we believe that deeper research is required on the appropriate mix, duration and intensity of training, and on the externalities that affect beneficiaries' use of knowledge and learned skills to improve in their economic well-being.

In our attempt to assess the impact of '100 Hours of Success' in these areas, we seek to contribute to the small but growing body of evidence on skills programmes to help youth enter the labour market and develop as economic actors. Given the significant investments being made in job training and life skills training across the MENA region and the dearth of experimental evidence on their impact, contributing to evidence in the region is particularly important.

But this study goes further:

- We develop evidence around whether providing basic financial literacy to youth fosters greater capacity to secure financial services and early financial empowerment. The MEDA intervention is different from other programmes, in that it combines financial literacy, life skills and entrepreneurship training, while encouraging participants to establish savings accounts;
- Given the short duration of the '100 Hours to Success' training, our study also adds to the evidence around appropriate durations and intensities for effecting real change in economic outcomes for youth;

- The study's strong gender component provides an opportunity to better understand issues that young women face in the economy. A growing body of research suggests that training impacts on income and skills are greater for young men than for young women, although elements of this evidence apply to vocational training programmes where manual trades are more culturally suited to men (see Blattman, Fiala and Martinez 2013; Cho *et al.* 2013). In our study, over half the participants are women and we pay particular attention to outcomes by gender; and
- We also seek evidence on the variability of outcomes among participants' household economic status and age.

We have organised our report as follows. Section 2 contains a more detailed explanation of the intended evaluation questions and research hypotheses that guide the study, along with an elaboration of the '100 Hours to Success' training and the theory of change in which it fits. Section 3 provides more context for the evaluation, describing how we selected the study site and comparing local conditions and the study population with the wider Moroccan context and youth population.

Section 4 provides a description of the timeline followed for both the training rollout and the evaluation, highlighting particular issues that arose with the implementation of the evaluation. Section 5 builds on this timeline to describe the evaluation design and its implementation more fully, and Section 6 describes the implementation of the '100 Hours to Success' training in more detail.

Within each of these sections, we have taken care to highlight factors that may have affected the study's internal and external validity, particularly around the significant non-compliance among our treatment population and the high levels of attrition in our follow-up survey. With these concerns in mind, Section 7 provides our evaluation results, while in Section 8 we offer a deeper analysis of non-compliance and attrition, and discuss any bias that may have been introduced into our results. We close in Section 9 with recommendations for MEDA and other programme developers, policymakers and researchers, to ensure that lessons learned in training can readily be applied.

## **2. Intervention, theory of change and research hypotheses**

### **2.1 Mennonite Economic Development Associates**

MEDA is a Canadian-based NGO established in 1953 as a social investment group. In 2015, the organisation had 281 staff operating in 52 countries. Its mission is to apply a business-oriented approach to economic development, resulting in sustainable enterprises for the benefit of the poor. MEDA serves micro and small entrepreneurs, farmers, small-scale manufacturers, traders, handicraft producers and poor households engaged in a range of income generation activities – in other words, those who could benefit from increased access to capital, markets and technical assistance.

MEDA supports the economically active poor with combinations of services through country-specific programmes. These include:

- **Financial services:** Increasing financial inclusion for marginalised groups – such as women, youth and rural populations – through support for financial institutions. MEDA helps banks and microfinance institutions (MFIs) provide innovative and sustainable access to savings and credit for the poor;
- **Linkages to markets:** Projects, contracts, consulting and products that provide innovative and sustainable access to markets for small producers and/or give the poor better access to products that serve their basic needs;
- **Investment fund development:** Capital funds, consulting services and products that promote profitable direct investment in businesses and institutions; and investments in regional funds that make direct investments in businesses that serve the poor; and
- **Member engagement:** Building opportunities for MEDA supporters to participate in and learn about MEDA's economic development work; raising matched financial contributions for MEDA projects.

## 2.2 YouthInvest project, Morocco

MEDA set up a local office and structure in Morocco in 2008, funded by a multi-million-dollar award from the MasterCard Foundation, and began implementing its YouthInvest project across the country in 2009. Operations covered Casablanca and smaller towns – including Berkane, Boudenib, Ouarzazate, Errachidia and Oujda – and their surrounding largely rural areas in the northeast and southeast (see Figure 1). To implement the project, MEDA developed training partnerships with youth-serving organisations across Morocco and government institutions such as the National Human Development Initiative (INDH).

**Figure 1: Mapping initial rollout of the YouthInvest project, Morocco**



Source: Google maps

YouthInvest was designed to provide a combination of financial and non-financial services to youth aged 15 to 25.<sup>2</sup> This included three training programmes – ‘100 Hours to Success’, computer skills and civic engagement – and a small-scale internship placement intervention, targeting 10 per cent of training participants. MEDA also partnered with local financial service providers to give youth the opportunity to open savings accounts and access credit. YouthInvest’s overall goal was to provide a mixture of services to youth by working closely with local partners – particularly financial institutions – to build their capacity to deliver and integrate such services into the portfolios of existing institutions.

According to its administrative data, MEDA delivered training to 11,900 youth between July 2009 and June 2011: 9,160 received ‘100 Hours to Success’ training; 1,312 computer training; and 253 civic engagement training.<sup>3</sup> During the same period, programme participants opened nearly 7,400 savings accounts. By 2012, 1,890 youth availed loans through the project’s relationships with existing MFIs.

The ‘100 Hours to Success’ programme was YouthInvest’s largest component and there was high demand from youth beneficiaries. Due to programmatic priorities and operational constraints, many of MEDA’s other activities in Morocco under the YouthInvest project – computer skills and civic engagement training, facilitated internships and preferential treatment for financial services – had been suspended by 2012, when our evaluation began. Given the large number of participants in ‘100 Hours to Success’, compared with other YouthInvest components, this report focuses on the implementation of ‘100 Hours to Success’, which continued to be rolled out independently in Morocco’s Oriental region. When we mention training received by beneficiaries, we refer exclusively to ‘100 Hours to Success’, with the understanding that none of the study participants were involved in either of the two other training programmes that had previously been offered as part of the wider YouthInvest project.<sup>4</sup>

### **2.3 Theory of change behind ‘100 Hours to Success’**

In describing the theory of change behind ‘100 Hours to Success’, we are mindful of three things:

- Its position within a broader range of services initially provided by the YouthInvest project (see Appendix A);
- The limitations imposed on securing long-term outcomes with an approach solely focused on training; and
- The motivation MEDA staff gave participants to open a savings account.

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<sup>2</sup> In the original proposal to the donor, the age range was 15 to 25, with acknowledgement that up to 10 per cent of project clients could be outside this age range. MEDA successfully lobbied the donor to extend the age range for financial service activities to 30 due to legal restrictions: in Morocco, you have to be 18 to qualify for a loan; in Egypt, 21.

<sup>3</sup> These numbers are based on unverified monitoring data supplied by MEDA.

<sup>4</sup> For more detail on YouthInvest, its individual components and its overall theory of change, see Appendix A.

With this in mind, we provide a broad overview of the programme's desired objective, outcomes and outputs in Morocco, followed by key assumptions behind this theory of change.

**Objective:** To empower young people to become productive participants in Moroccan economic society.

### **Main outcomes**

1. Youth participants are able to open bank accounts, build up savings and access other financial services, including lending for self-employment and education.
2. Young people invest in quality education to build their own human capital.
3. Young people became more active and employable within the Moroccan labour market.

### **Outputs**

The major output of '100 Hours to Success' centres on the successful completion of the training course. The training was designed to empower participants by building: their confidence, self-efficacy and capacity to manage their personal finances and access financial services; and their knowledge of the fundamentals of starting an entrepreneurial activity. Towards this end, the training had three primary modules:

- **Financial education:** To provide youth with the tools they need to manage their personal finances and, where appropriate, to set up simple financial management systems for a microenterprise. It included modules focused on personal budgeting, savings, debt management, knowledge about banking services and financial negotiations. These were based on materials developed under the Global Financial Education Program, adapted by MEDA staff for youth in the local context and to address the specifics of the local regulatory environment for financial services providers;
- **Community engagement and life skills:** To improve personal competencies, problem solving and conflict management. As with the other modules, MEDA adapted materials (this time developed by the International Youth Foundation) to fit the Moroccan context and better suit the needs identified by local youth. MEDA used role play and group work to help youth understand and manage emotions, develop confidence and assertiveness, manage and reduce stress, deal with problems and conflicts and develop improved abilities to work with teams; and
- **Business and entrepreneurial skills:** MEDA worked with local youth to adapt these modules to the Moroccan context from a curriculum initially developed by Street Kids International and modified by Save the Children. The MEDA modules contained participatory exercises and role play activities to help youth assess their business development abilities, conduct market research for a business idea and plan a business, including developing a pricing strategy, evaluating costs and determining profit margins.

The training was experiential in nature: it relied on participants' personal experiences and worked through live examples and case studies to encourage problem solving and high interaction between participants. Collectively, through the concurrent application of the financial, life skills and business components, the training was designed to provide youth with an empowering learning experience, which would support them as they moved forward to continue in education, enter the labour force or start their own businesses.

The '100 Hours to Success' training was open to applicants aged 15 to 25 and designed to be delivered in a flexible manner, with courses scheduled over a one- to three-month period, depending on youth availability. In practice, most courses ran for nearly three months, with classes held once or twice a week at youth centres and vocational training facilities. MEDA provided trained instructors called youth extension officers. Typically, youth were invited to join the classes as slots became available, and there was a short waiting list to get into a class. Participants were expected to attend all sessions and complete all components of the training, and needed a minimum of 75 per cent attendance to get their certificate of completion.

### **Assumptions**

The key assumptions underlying the theory of change for '100 Hours to Success' are:

1. Morocco's basic education system is not providing young people with the skills they need to be successful members of economic society and needs to be complemented with additional training.
2. Morocco's economy is generating enough jobs on the demand side that are open to young people (MEDA's '100 Hours to Success' did not intervene on the demand side in Morocco).
3. Young men and women face similar challenges and can be trained using the same approach and curriculum.
4. 100 hours (2.5 weeks full time) of training can give young people the skills they need to be successful in economic society.

## **2.4 Research hypotheses**

Building on the theory of change outlined in Section 2.3, this study focuses on the specific hypotheses that relate to the average impact on training participants and impact heterogeneity.

Our hypotheses refer to the effect of training on four main areas:

- **Financial knowledge and behaviours, particularly around savings and loans:** As a result of participating in '100 Hours to Success', youth will be better able to manage their personal finances. In the long term, they will be more likely to maintain a savings account and will show increased levels of activity around saving and borrowing – for example, by securing loans for personal or business use. Participants will also demonstrate heightened understanding regarding the functioning and provision of financial services;

- **Life skill capacities such as confidence, teamwork and problem solving:** Participants will develop a greater sense of confidence in their own abilities, while also gaining a greater sense of personal empowerment within their own family and community – for example, through acquiring problem-solving skills. They will show a more pronounced tendency to sustain interest in, and effort toward, long-term goals and increased levels of self-esteem, and will be more likely to engage with their family and local community;
- **Labour market outcomes:** Participants will be more readily able to secure employment, including jobs that allow them to earn higher incomes, provide better benefits or are more formal; and
- **Further investment in human capital:** Participants will have increased appreciation for the value of training and education, which could lead them to invest further in developing their skills and human capital. They might prolong and intensify their educational career, seeking a higher level of formal education.

Our hypotheses related to individual-level impact heterogeneity fall into three areas:

- **Level of programme participation:** Outcomes will differ from participant to participant, depending on the level of engagement within ‘100 Hours to Success’ and the number of training courses attended (run by MEDA and others);
- **Gender heterogeneity:** Women in the region face a number of barriers to enter the labour force. Understanding the impact of training on the lives of young women is of special interest to MEDA and has significant value in the region; and
- **Initial family income levels and social capital:** Socio-economic standing may affect an individual’s employment prospects in several ways. Those with better socio-economic standing will be more likely to have developed their own human capital, and those with higher economic standing are likely to have social and family networks through which to secure employment. But those from comparably wealthier backgrounds are also more likely to maintain higher reservation wages and be more selective in the jobs they accept.

### 3. Context of the impact evaluation

#### 3.1 ILO’s Taqueem Initiative and 3ie

The research team and MEDA started collaborating in 2009 as part of the International Labour Organization’s (ILO) Taqueem Initiative, which supports organisations delivering labour market interventions for young people in Arab states. From 2009 to 2011, the Taqueem Initiative helped MEDA develop a results measurement system for their YouthInvest project in Morocco. Subsequent discussions between MEDA and the Taqueem Initiative identified ‘100 Hours to Success’ as a relevant entry point for an impact evaluation, given the scale the training had reached and the ability to randomly sort applicants into treatment and control groups. Support and funding from the International Initiative for Impact Evaluation (3ie) allowed them to move forward with this impact evaluation.

### **3.2 Identifying the Oriental region as a focus for the impact evaluation**

The initial concept for this impact evaluation envisioned a national approach through a clustered randomisation. But, as the team moved from conceptualisation to implementation, we refocused on project implementation in the Oriental region – including the city of Oujda and its environs – because of the relative severity of economic constraints facing youth in this region. In 2012, it had Morocco’s highest youth unemployment rates: 41% in urban areas and 21% in rural areas. MEDA’s presence in the region was particularly strong and well organised, which would support the quality of training implementation and the rollout of the evaluation. The geographic focus ensured a solid, more targeted use of scarce resources, for training and the evaluation.

With a population of about 450,000 people, Oujda is Morocco’s twelfth largest city. Its inland location differentiates it from Morocco’s larger port cities – such as Casablanca, Tangier and Agadir, which connect the country to Europe and Sub-Saharan Africa – and it is not a centre for tourism, like Fez and Marrakesh. Being close to the Algerian border, it serves as a hub for trade between Algeria and Morocco. It has some limited industrial capacity and has received government support for special industrial projects. It is also linked to other cities in Morocco by both rail and air.

While the Oriental region has Morocco’s highest youth unemployment rate, lessons from Oujda are applicable to other Moroccan cities and their surrounding rural areas. By and large, Oujda’s youth population is comparable with other Moroccan cities. Given the range of urban and rural youth in our study, and their educational backgrounds, we expect Oujda’s youth to be more reflective of Morocco’s national youth population than that of larger cities such as Casablanca and Rabat. We address the comparability of Oujda and the Oriental region with the rest of Morocco in Section 8, where we discuss the external validity of our findings.

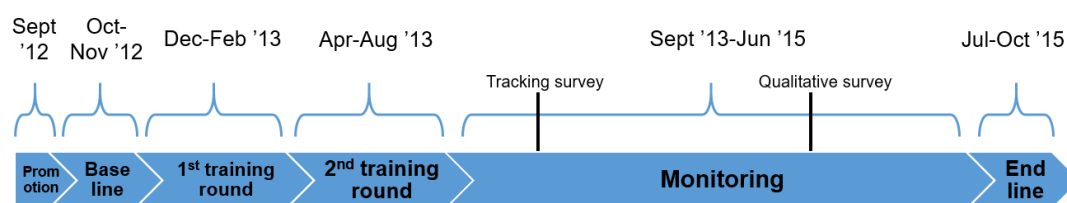
## **4. Timeline**

As we noted previously, our study was subject to numerous delays and challenges to its original design, which affected our timeline and the quality of our data. We discuss in detail the implications for the quality of the study in Sections 6, 7 and 8. Figure 2 charts the key events on a timeline. They are:

- September 2012: initial promotion of ‘100 Hours to Success’ training in the Oriental region; 1,815 young people registered and determined eligible;
- 11–12 October 2012: training of enumerators and baseline survey pilot in Oujda;
- 14 October–7 November 2012: baseline survey of 1,815 young people at youth centres across the Oriental region;
- 15 November 2012: baseline data received by research team; randomisation sorts 600 into the treatment group, 1,215 in the control group;
- 26 November 2012–February 2013: 343 young people enrolled in training;

- March 2013: recognising a high level of no-shows for the first training cohort, second round of randomisation sorts 315 from the control group into a new treatment group, giving 915 in treatment group and 900 in control group;
- April–August 2013: a further 126 youth in the treatment group receive training, bringing the total number of treated to 469;
- December 2013: SMS tracking survey of recent training participants completed to collect initial results and help the research team identify individual's availability for the subsequent follow-up survey, but only 30% of baseline participants respond to the tracking survey or are found, and only 12% complete the survey, despite being given a phone top-up as an incentive; even fewer provide updated contact information;
- September 2014: new data collection firm contracted for follow-up survey;
- 10 September 2014: request for data collection authorisation submitted to Ministry of Interior, as required by law;
- December 2014–January 2015: qualitative study and subsequent report carried out, using focus groups and key informant interviews;
- June 2015: final approval for data collection from Moroccan government;
- 28–30 July 2015: follow-up survey piloted in Oujda;
- August–October 2015: follow-up survey on 427 individuals from the treatment group and 444 from the control group, with proxy data on a few outcome variables obtained from another 200 participants by interviewing a parent, friend or neighbour;
- 18 October 2015: final dataset completed and delivered to research team.

**Figure 2: Timeline of evaluation of '100 Hours to Success': 2012–2015**



## 5. Evaluation design, methods and implementation

Based on our initial planning for the evaluation of MEDA's '100 Hours to Success' training, its past success in recruiting and graduating participants and its willingness to exclude a randomly sorted control group from participation in the programme for the duration of the study, we designed the evaluation as an RCT. This section describes how we designed and implemented the RCT.

### 5.1 Ethical concerns

For our study, there was no review by an ethical review board, as none of the associated organisations maintain such a board, nor do their internal standards require such a review. But, the way we conducted the study was mindful of the ethical issues that such studies can raise:

- We were careful to inform each respondent during the baseline and follow-up surveys about the nature of the study;
- We informed them that participation was voluntary: should they choose to proceed, they could stop at any time or refuse to answer any particular question without risk;
- We made it clear to them that participating in the survey or their responses would not affect whether they were accepted onto the MEDA programme;
- All personal information was carefully controlled so that only members of the research team could access it; and
- MEDA's operational representatives could not access any individual data.

## 5.2 Identification strategy, assignment and sampling

Central to this study's identification strategy is the randomised sorting of applicants to the '100 Hours to Success' training into a treatment and a control group. In principle, randomised sorting allows for an accurate estimate of the counterfactual of training participation. On an aggregate level, the control and treatment groups should be comparable in terms of both observable and non-observable characteristics before the start of the programme; so any differences in outcomes in the averages between the control and treatment groups that we observe after the implementation should be attributable to the training.

The research team first considered randomisation at cluster level – for example, using catchment areas around youth centres – because the training would take place in youth centres and classes would only be able to run with a minimum number of participants. But the relatively small number of potential clusters meant that our study would not have had enough statistical power. Instead, we conducted the randomisation at individual level. In applying the initial random sorting, we found that significant numbers of participants in each youth centre moved forward with the courses.<sup>5</sup> While we did not conduct the randomisation in public, we informed all applicants at baseline that slots were limited and that we would select participants by random assignment.

Figure 3 summarises the different stages of the impact evaluation, which are:

- **Baseline survey:** collecting data from 1,815 youth in October 2012;
- **Randomisation and training:** two rounds from December 2012 to February 2013 and then April to August 2013; and
- **Follow-up survey:** interviewing 871 youth from August to October 2015.

Figure 3 also details how many study participants enrolled in the training programme following the first and second round of randomisation,<sup>6</sup> and how many follow-up

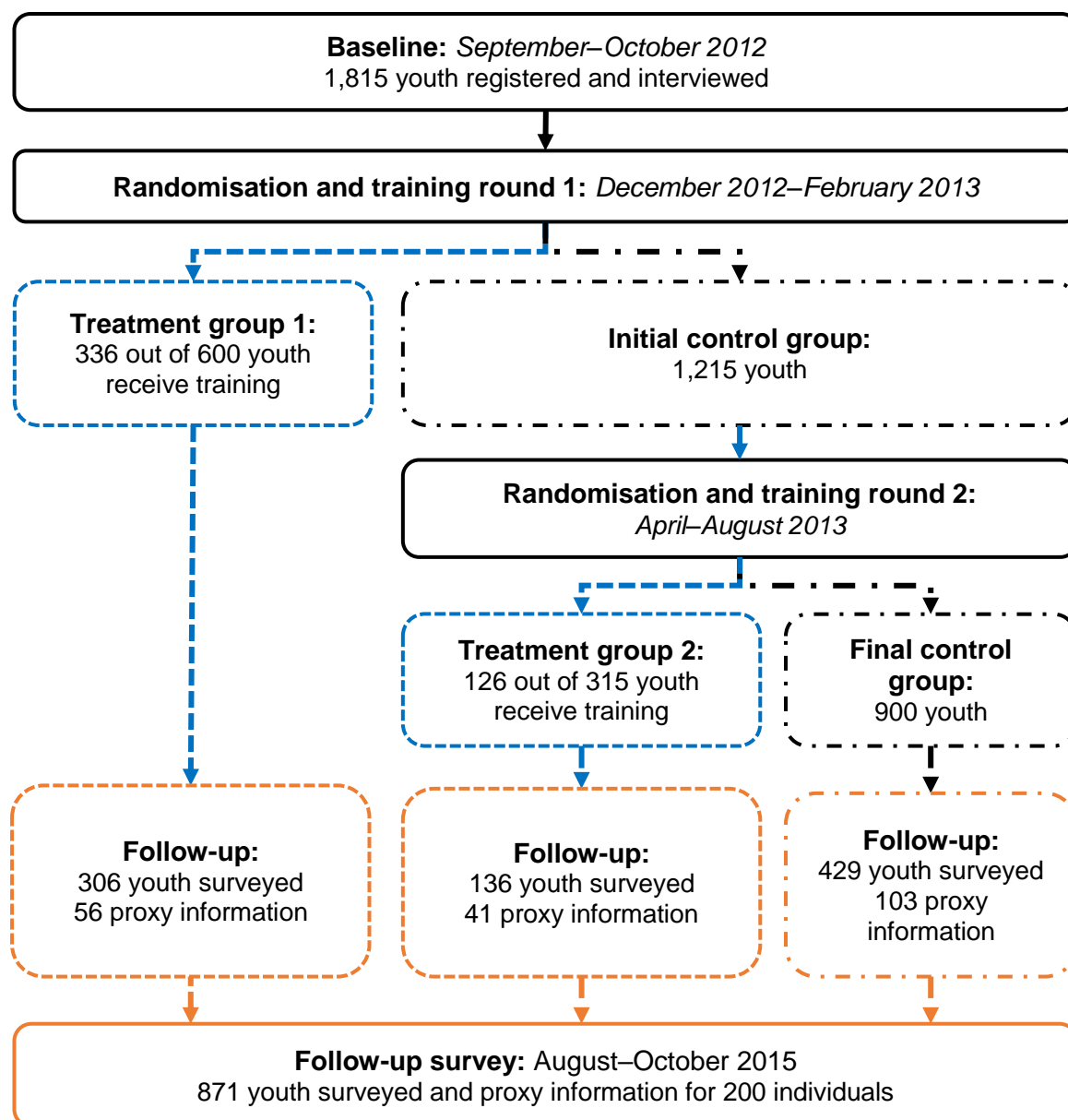
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<sup>5</sup> As a result, the percentage of youth assigned to the treatment group differs slightly across centres. As we explain in Section 7, we control for these small differences by using youth centre as a control variable (one dummy per centre) in our statistical analysis.

<sup>6</sup> 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 the sessions. In

interviews we conducted in each of the three subgroups (first round of training, second round of training, control group). For 200 individuals, we obtained proxy information on a number of outcome variables by interviewing a parent, friend or neighbour (see Section 7.2 for more detail).

**Figure 3: The three stages of the impact evaluation: data collection, randomisation and training**



Section 7, we discuss low overall take-up and its implications on evaluating the impact of the programme.

### **5.3 Sample size determination and sampling design**

Our sample size was determined largely by the resources at hand for delivering the training. MEDA had the capacity to deliver training to around 600 participants over the 2012–2013 period. We based our power calculations on outcomes relating to savings behaviour, employment, self-confidence and empowerment using monitoring data from MEDA, the World Bank (World Bank 2012) and a study of the INJAZ programme in Morocco (Reimers, Dyer and Lettrick 2012). Basing our initial power calculations on an assumption that we would have equitable treatment and control groups in terms of size, we realised that a total sample of 1,200 (600 treatment and 600 control) would not provide us with the power to evaluate outcomes for most of the subgroups we were interested in. We were also concerned with possible attrition. With an envisioned sample of around 1,800 – 600 of whom were in the treatment group – we projected that the study had enough power to detect eventual programme impacts for the whole sample and for a number of subgroups, even in the presence of moderate attrition rates. Detailed power calculations are presented in the baseline report (Dyer *et al.* 2014).

With a target of getting 1,800 self-referred youth participants in the study, MEDA deployed a team in September 2012 to begin promoting the first ‘100 Hours to Success’ training. The team worked with established youth centres, vocational training centres, schools and universities in the wider Oujda area. Applicants had to be aged 15 to 25 and meet basic literacy requirements. They were asked to register their interest in participating before the study launch. MEDA successfully identified 1,815 individuals who registered to participate in the programme and met the criteria for participation. Having reached this goal, MEDA stopped taking applications and we started the study. We did not need to sample the larger population of applicants. Other people came after applications for the study had closed, and many were included in subsequent training groups, but they remained outside of the study.

### **5.4 Assigning treatment and revisiting our randomisation due to poor take-up**

Initially, we were able to do a clear assignment of treatment and control, sorting 600 of the 1,815 survey respondents into the treatment group and the remaining 1,215 into the control group. To achieve this random allocation, we used a list of baseline survey participants in MS Excel and ran a random number generator calculation. We then placed the bottom third of participants into the treatment group.

Soon after training started, it was clear that take-up rates were low and that MEDA was having trouble getting members of the treatment group into training, due to scheduling conflicts and some lack of interest once classes began. A delay in rolling out classes meant that some of the treatment group had moved on to other opportunities or could no longer attend training. Others had expected to take training with their friends and opted out after their friends were put in the control group. So some of the poor take-up was a result of treatment and control strictures.

With only about 336 youth enrolled by February 2013, we made a strategic decision to undertake a second random sorting of individuals from our control group, inviting them to participate in the training. Without this, we would not have had enough power to demonstrate an impact on the desired level of detail and we were worried about potential attrition. We conducted the randomised sorting as described above, moving 315 individuals previously assigned to the control group into the treatment group. The final treatment group totalled 915 and the control group 900.

Limited take-up remained a challenge; only 126 of the 315 individuals allocated to treatment through this second wave of randomisation enrolled in the training. We were aware that those who entered the treatment group in the second wave may have moved on to other things and would no longer be interested in the programme. MEDA had not, however, informed the original control group members that they had been denied services, and there had only been a three-month gap between the initial and the second sorting. While we were concerned that this second randomised sorting would alter the comparability of our findings, our baseline review showed that – on observable characteristics – the treatment and control groups following the second sorting were at least as comparable as those following the initial sorting (see Dyer *et al.* 2014). Our analysis included here uses the full treatment and control group, as delineated by both random sortings.

## **5.5 Data collection and dataset construction**

The primary instruments we used for data collection were a baseline survey and a follow-up survey. We tried to undertake a small, SMS tracking survey immediately after the end of the programme, but the response rate was extremely low and we got no valuable results. We also undertook a qualitative assessment based on structured interviews and focus groups with a small number of participants.

### **Baseline survey**

The research team developed the baseline survey questionnaire, drawing many of its questions from the 2009 World Bank survey of Moroccan youth to give our survey a nationally representative reference point and allow for external validity analysis. The follow-up survey questionnaire included revisions to the baseline – we eliminated household asset questions and added several scales to assess skills, self-efficacy, and household economic well-being. The baseline survey collected detailed contact information on respondents and their friends and family, to help with tracking during the follow-up survey.

The baseline survey was collected by a Moroccan survey fielding company called Sunergia, with a team of young enumerators who were experienced in conducting surveys in Morocco. The research team supervised Sunergia's enumerator training and reviewed the survey instrument with them. The research team was in the field to observe and supervise baseline data collection, which took place over four days at the youth centres where applicants had applied for the '100 Hours to Success' training, with respondents coming to the centres at pre-arranged times. On arrival, they were introduced to their enumerator, who engaged with them in French or the

local Arabic dialect to ensure understanding. The surveys took place in a group setting: each respondent sat with an enumerator one-on-one in a space that provided enough privacy to ensure accurate responses, although several interviews took place in each room at any given time. Although fielding the survey in participants' households would have ensured more privacy, while providing the research team with an opportunity to validate housing and income information, we chose a group setting due to logistical constraints and limited funding.

### **Qualitative assessment**

We undertook a qualitative assessment between December 2014 and January 2015 in partnership with the Swiss Academy for Development in the cities of Oujda and Taourirt in Morocco's Oriental region. We ran 8 focus group discussions with 30 young people from urban and rural areas – 11 male and 19 female – all of whom had completed the training in 2011, 2012 or 2013. We also conducted 13 key participant interviews with: 4 trainers from MEDA; the key partner who provided the training centre in Oujda;<sup>7</sup> and 8 young people who had dropped out of the programme. MEDA staff contacted beneficiaries and programme dropouts for the focus groups through the programme's participant database, and selected the trainers and key partner for interview. The local team, trained by Swiss Academy for Development staff, conducted the discussions and interviews in the local Moroccan Arabic dialect; moderators then translated the transcripts into French for analysis.

### **Follow-up survey**

For the follow-up survey, we hired another survey fielding company, SOLAB, following concerns about Sunergia's capacity to deliver on a data collection effort that would require significant tracking of participants. SOLAB is affiliated with the National Institute of Statistics and Applied Economics, which oversaw the data collection process. SOLAB hired a group of young enumerators from Oujda – some with limited experience in survey fielding – and provided oversight from two experienced field managers. Enumerators used tablets linked to global positioning software, allowing field managers to track where the surveys took place as a control to ensure that reported interviews had taken place. Members of the research team were in Morocco at the start of data collection, meeting with SOLAB and the enumerators in Rabat for three days to train them on the questionnaire and the methodology. Research team members observed the first few days of data collection in the field, to ensure that all was proceeding as intended.

During the follow-up data collection, enumerators tried to call baseline respondents up to four times to arrange meetings. If they could not reach them by phone or had no telephone number for them, enumerators visited the addresses recorded at baseline. If they were unable to find the individual and arrange an interview, the

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<sup>7</sup> MEDA Maroc chose the manager of a community centre in Oujda as the key partner because he played an essential role in providing classrooms for the training, not only in his centre, but also (through his contacts) in other parts of the city. He was well aware of the objectives of the training and was able to observe the evolution of the programme.

enumerator would ask parents, friends or neighbours about the individual's employment and educational status as a proxy measure of these variables. As with the baseline survey, they offered no incentives – monetary or otherwise – to respondents during the follow-up survey.

## **6. Programme design, methods and implementation**

### **6.1 Key project elements and programme activities**

MEDA created '100 Hours to Success' in response to market research at the beginning of the project, where youth and employers identified three main knowledge gaps among young people entering the labour market: financial literacy, life skills, and business and entrepreneurship skills. MEDA's '100 Hours to Success' aimed to deliver a mix of training in these areas in an efficient and flexible structure.

The training was designed to empower young participants by: building their confidence and self-efficacy; their capacity to manage their personal finances and access financial services better; and their knowledge of the fundamentals of starting a business. In Section 2, we provided a detailed description of these three components; here we focus on the implementation of the training in the context of this evaluation.

Within each component, the training is activity-based and experiential, drawing on participants' knowledge and using facilitated problem solving as its key method of imparting information. The training was designed for flexible delivery, with class schedules adjusted around the identified needs of registered participants. Most classes ran over the course of three months, through two-hour sessions, twice a week. A smaller number of courses were delivered over a month-long period. No matter the length of the course overall, participants got 100 hours of training that covered the entire curriculum. And while the training consisted of three primary components, these were delivered in a combined fashion; trainers had flexibility on when and how they delivered specific parts. Participants were not able to opt in or out of any one specific component.

For the period of this evaluation, training was provided at local youth centres – and in some cases, vocational training facilities – that partnered with MEDA around Oujda, its outskirts and surrounding rural towns of Jerada and Taourirt. Participants applied to – and attended the courses at – their closest local centre to ensure consistent attendance. The local centres were mainly financed and managed by government agencies and included *centres de jeunesse et sport*, *centres de formation professionnelle*, and *centres de formation par apprentissage*.

For each participating centre, MEDA provided trained instructors, or youth extension officers. These were young Moroccans certified by MEDA to provide training under '100 Hours to Success'. MEDA master trainers identified core trainers, who were responsible for preparing the extension officers to recruit youth participants and deliver MEDA's '100 Hours to Success' curriculum, teaching them general pedagogical approaches for training youth. Importantly, for the sake of our

evaluation, the youth extension officers deployed in the Oriental region over 2012–2013 were experienced trainers who had worked with MEDA since the start of the programme. So they knew the material well and had demonstrated their capability to deliver the programme. These extension officers were typically of a similar age to the participants; they also spoke the same dialect, lived in the same communities and sometimes knew participants' families. This ensured that they could effectively communicate the lessons of '100 Hours to Success' and engage with local youth to recruit participants.

To enrol in the training, applicants had to complete a short registration form and submit it with a photo and a copy of their national identification documents. They also had to complete an engagement statement highlighting why they wanted to participate in the training. These steps served as a form of commitment to see the programme through and show their desire for new knowledge. Applicants were strongly encouraged to open a savings account before joining the training, but were not excluded if they did not.

Typically, outside of the context of this evaluation, a MEDA-trained youth extension officer would review applicants' information and provide a rapid acceptance response to the applicant. Students were then invited to begin training as slots became available in new '100 Hours to Success' sessions; there was typically a short waiting list to get into a class. But we had to suspend these immediate approvals due to the evaluation's randomised sorting requirements and the need to survey applicants before making offers of enrolment to the treatment group. In some cases, this led to a significant delay, which may have had an impact on the lower-than-expected enrolment rates seen as classes began.

## **6.2 Recruitment strategy and training implementation**

To initiate recruitment for the 2012–2013 round of '100 Hours to Success' in the Oriental region, MEDA began a recruitment drive at youth centres during the summer of 2012. Youth extension officers led this recruitment, requesting interested youth to visit the training institutions in the third and fourth week of October 2012 for registration. Based on this drive, 1,815 young people applied to join the programme.

Following the initial random assignment of applicants into treatment and control groups, MEDA's local team began enrolling members of the treatment group. During the course of the evaluation in the Oriental region, MEDA ran 47 courses at 11 training centres. The first classes started in mid-October 2012, and by mid-December 25 courses had begun meeting. Another five courses began in January and February 2013; seven courses started in March and six in April. The remaining five courses started between May and July. Most (30 of the 47) ran over a 3-month period; 14 over 1–2 months; and 3 were compressed into 1 month. On average, courses had about 18 enrolled participants; a total of 469 individuals from the expanded treatment group enrolled in the training over the evaluation period.

### **6.3 Monitoring project progress and eligibility**

For the sake of assessing compliance among study participants – both in terms of monitoring the depth of participation and ensuring that the random sorting of applicants into treatment and control was not compromised – we put two levels of controls in place.

**Local checks:** After receiving results of the randomised selection, enrolment involved a two-round local vetting process. Local administrative staff would register participants in Oujda, checking against the control group list before completing the registration. MEDA’s regional coordinator in Oujda would then review the lists of registered participants, and verify these against the list of control participants who were not allowed to participate in the training.

**Central office checks:** After completing these local controls, the coordinator sent the final list of course registrations to Casablanca, where central office staff provided final approval of participants, after comparing them against the treatment and control lists.

Each youth extension officer kept detailed attendance records, following specific instructions. All MEDA staff knew about the study, were trained in the methodology and aware that there was a list of youth in the control group. Trainers did not have a copy of the control list, but knew that any youth who showed up for a training session without being on the course attendance list needed to be approved first.

## **7. Impact analysis and results to key evaluation questions**

### **7.1 Primary quantitative specifications**

Obtaining unbiased impact estimates is among the greatest challenges for any evaluation that wants to quantify the causal effect of an intervention on the outcomes of interest. Evaluation designs that simply compare programme beneficiaries with a group of non-participants do not account for differences in important characteristics, some of which may be unobservable. These evaluations are likely to produce incorrect estimates, plagued by what is commonly referred to as endogeneity bias.

In a field experiment, we randomly allocate potential beneficiaries to treatment and control groups precisely to ensure that these groups are comparable in terms of observable and unobservable characteristics that influence outcome variables. In this setting, simply comparing outcome levels from the follow-up survey allows one to identify causal effects. We augment this basic empirical specification in two ways:

1. Because we recorded most outcome variables in both the baseline questionnaire and the follow-up survey, we compare differences in outcomes over time between the control and treatment group. This difference-in-

differences (DID) approach allows us to control for any remaining differences between the control and treatment groups that stay constant over time.<sup>8</sup>

2. We also include a set of (time-varying) control variables. This procedure corrects for remaining small differences in observables and is expected to improve the precision of our estimates.<sup>9</sup>

We thus estimate:

$$y_{it} = \beta_1 Treat_i * Post_t + \alpha_1 + \alpha_2 Post_t + X_{it}\gamma + \eta_i + \varepsilon_{it} \quad (1)$$

Where:

- $y_{it}$  is the outcome variable for individual  $i$  at time  $t$  (0=baseline, 1=follow-up)
- $\alpha_1$  represents the average for the control group at the time of the baseline (conditional on covariates)
- $\alpha_2$  describes the average difference between baseline and follow-up for the whole sample
- $Post_t$  is an indicator taking the value 1 for all observations of the follow-up
- $X_{it}$  captures time-varying, individual and household characteristics
- $\eta_i$  is an individual fixed-effect term that controls for all (observed and unobserved) factors that are time invariant<sup>10</sup>
- $\varepsilon_{it}$  is the idiosyncratic error term that describes variation in the outcome variable that is not explained by the model
- $\beta_1$  is the parameter of interest that captures the effect of the intervention

Note that in (1)  $Treat_i$  is an indicator that takes the value 1 for all treated youth – individuals who actually participated in the programme – and 0 otherwise. Thus, it does not necessarily coincide with treatment assignment as we discuss in more detail in Section 7.2.

This report presents three different impact estimates: average treatment effect on the treated (ATT), intention-to-treat (ITT) effect and local average treatment effect (LATE) (see Section 7.4).

We attempt to obtain the ATT using the ordinary least square (OLS) estimator. ATT is often seen as the most relevant estimator for policymakers and practitioners, because it captures an intervention's average effect on participants. So impact

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<sup>8</sup> The balancing tables in Section 7.3 show that control variables do not differ significantly between the treatment and control groups. Adding them in equation (1) thus first and foremost increases statistical precision.

<sup>9</sup> See Section 7.2 for a description of the control variables.

<sup>10</sup> Note that, as we added an individual fixed effect, we no longer need to include an indicator for participants in the treatment group. In specifications without individual fixed effect, the coefficient of this dummy would amount to the average difference of the outcome variable between treated individuals and the rest of the sample at the time of the baseline. In an experiment where randomisation is successful, we would expect estimates to be close to zero. Indeed, the balancing for outcome variables that we observed during the baseline show that differences are small and statistically insignificant – see the discussion in Section 7.3 and Tables E1 and E2 in Appendix E.

estimates are based on a comparison between the youth who underwent the ‘100 Hours to Success’ training and everyone else. Consequently, the comparison group comprises both youth from the control group and those who were assigned to the treatment group but did not participate. But participation in the training is likely not to be random: participants could, for example, choose (not) to attend the course depending on their (expected) benefit from the intervention. While our OLS estimator controls for a number of observed time-varying and all unobserved time-invariant characteristics, it does seem plausible that the young people who attended the training fundamentally differ from those who did not.<sup>11</sup> Therefore, it seems challenging to obtain accurate OLS-based estimates of ATTs in the context of this evaluation.

This is why we compare the OLS-based ATT results with two other estimators that are supposed to overcome self-selection bias. To this end, we exploit the fact that we randomly assigned youth to a treatment and comparison group. Our second set of impact estimates is based on the equation:

$$y_{it} = \beta_1 \text{Assigned}_i * \text{Post}_t + \alpha_1 + \alpha_2 \text{Post}_t + \mathbf{X}_{it}\boldsymbol{\gamma} + \eta_i + \varepsilon_{it} \quad (2)$$

where  $\text{Assigned}_i$  is a dummy variable taking the value 1 for all individuals randomly placed in the treatment group and 0 for everyone in the comparison group.

Estimating (2) with OLS, we obtain ITT effects. ITT estimates are purely based on a comparison between the assigned treatment and control groups and do not take into account whether individuals did or did not participate. So, in contrast to ATT estimates, ITT effects capture the average effect on individuals from the intended beneficiary population. As we describe in Section 5 (see Figure 3), not all of those assigned to participate took part in the training. Assuming that randomisation was done correctly (see Sections 7.3 and 8 for discussions on internal validity concerns), we would obtain unbiased estimates – in other words, results without systematic errors.<sup>12</sup>

As ITT coefficients do not allow us to gauge the direct effect on treated individuals, we present a third set of impact estimates: the LATE. For these, we instrument actual treatment with treatment assignment using the two-stage least squares estimator where (1) represents the second stage and the first-stage regression is given by:

$$\text{Treat}_i * \text{Post}_t = \delta_0 + \delta_1 \text{Assigned}_i * \text{Post}_t + \mathbf{X}_{it}\boldsymbol{\rho} + \eta_i + \omega_{it} \quad (3)$$

This procedure relies on the assumption that, while participation in ‘100 Hours to Success’ training is likely not random (as it is driven by self-selection), offering the programme to some people at random increased their inclination to participate. For our instrument (programme assignment) to be relevant, youth assigned to the

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<sup>11</sup> “Fundamentally different” in this context implies that treatment and comparison groups on average show different time trends in the outcome variables.

<sup>12</sup> This does not imply that our impact estimates equal exactly the true values. In fact, the statistical margin of error depends, among other things, crucially on the sample size.

treatment group have to be more likely to participate in the training than those in the comparison group. In Section 7.2, we show that this was the case.

We obtain LATE estimates in two steps. First, we calculate differences between all individuals assigned to the treatment and control groups. This corresponds to the ITT effect described above. Second, we scale up this difference with a factor that depends on the compliance rate of all study participants. This factor will be larger (i) the higher the proportion of individuals assigned to the control group who got treated nevertheless (the always-takers); and (ii) the higher the proportion of individuals assigned to the treatment group who did not participate in the training (the never-takers). Importantly, a LATE is an estimate for those individuals who received the treatment because they were assigned to it (the compliers). We discuss compliance among training participants further in Section 7.2.

Taken together, we will present, compare and discuss ATT, ITT effects and LATE for a range of outcome variables:

1. When we mention control and treatment groups, unless explicitly stated, we are referring to individuals who were randomly assigned to be in the control and treatment group, respectively. This does not necessarily correspond to individuals' actual treatment status, which depends on whether they complied with their treatment assignment.
2. When we mention treated individuals – for example, in the context of ATT – we are referring to youth who we consider to have participated in '100 Hours to Success'. We also use the terms comparison group and control group interchangeably.
3. Regarding inference, we present robust standard errors, consistent in the presence of heteroskedasticity.<sup>13</sup>
4. The above equations require data on outcome and control variables before and after the intervention (the DID approach). But we only observed a number of outcome variables in the follow-up questionnaire. Presenting the results in Section 7.4, we clearly flag these variables, for which we base the impact estimates on adapted versions of equations (1), (2), and (3).

## 7.2 Dataset, programme take-up and compliance

Our analysis builds on data from both the baseline and follow-up surveys. After cleaning the baseline data for data entry errors and incomplete responses, we reduced the initial sample of 1,815 to 1,803 respondents. Of these, we assigned 891 to the control group and 912 to the treatment group. We have follow-up data

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<sup>13</sup> An alternative would have been to cluster standard errors at youth centre level, either where study participants completed the baseline survey or where they took part in the training. Cluster robust standard errors take potential intra-cluster correlation into account when assessing the statistical uncertainty that comes with impact estimates. But their consistency (asymptotic properties) is based on a large number of clusters and, as Donald and Lang (2007) show, they do not tend to have desirable properties for a small number of clusters. Our dataset features only 13 centres for baseline data collection, and more than 95 per cent of observations come from 10 of these. This cautions against clustering at this level.

available for 871 individuals who were interviewed in person, amounting to an overall attrition rate of 51.7 per cent. Of those, we had randomly assigned 427 to the treatment group and 444 to the control. The high rate of attrition reflected in these figures is a concern that we address here, and again in Section 8.

From the beginning, we were aware that Moroccan youth often change phone numbers or email addresses.<sup>14</sup> To prepare for this, we made sure to collect detailed contact information on each respondent and for their family members and close friends. When enumerators were unable to contact a study participant directly in the follow-up survey, despite multiple efforts, they attempted to interview another household member, a neighbour or friend instead to obtain rudimentary proxy data on a number of outcome variables relating to educational enrolment and labour market status. We did not include observations from this proxy data in our primary analysis, although we do present findings comparing our 871 respondents with a larger population that includes 200 of these 493 observations (screened for reliability) as a robustness check in Section 8.<sup>15</sup> We were unable to get any follow-up information from the other 439 individuals who participated in the baseline study.

To test our hypotheses about the potential impact of ‘100 Hours to Success’, we chose a number of outcome variables for the three outcome areas, including:

- five to describe financial behaviour, such as saving and borrowing patterns and one to capture financial literacy
- six to measure community engagement and life skills, including the GRIT scale for describing efforts toward long-term goals and Rosenberg’s self-esteem scale
- four related to education and six describing labour market activity and experience.<sup>16</sup>

Importantly, our labour market outcomes analysis takes a bird’s eye view on labour market status and does not distinguish between self- and wage-employment. Of the 871 individuals in the follow-up survey, only 25 identified themselves as self-employed, which was not enough for statistical inference.<sup>17</sup>

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<sup>14</sup> Still, we were surprised and disheartened by the difficulties we faced in tracking down many youth during our SMS tracking survey and the follow-up survey. These problems were increased by an ill-timed (from our perspective) mandate by Moroccan authorities requiring all mobile phone users to register existing numbers with the government or have their number cancelled: many people set up new numbers to avoid registering their original numbers.

<sup>15</sup> Enumerators had to record the provider of the proxy data as parent, family member, friend, neighbour or other. In 293 out of 493 cases, enumerators indicated that they obtained the data from other (in many cases themselves or their own family members). A closer look at these 293 observations reveals that in over 95% of these cases, participants were said to be NEET (compared with 31% of the 871 respondents at the time of the follow-up survey). This seems unlikely to be true. Rather, these data entries are consistent with interrogators mechanically ticking “no” to the few questions in this section.

<sup>16</sup> See Appendix D for a full list of outcome variables.

<sup>17</sup> Of those 25 individuals, 14 were initially assigned to the treatment group and 11 to the control group (p-value for equal means: 0.58).

The control variables we selected consist of basic demographics such as age, gender and whether the respondent lives in an urban area, as well as information about the participant's household (number of siblings, living in a dormitory at time of baseline, father still alive at time of baseline, self-indicated satisfaction with overall household situation) and household head (gender, level of education at time of baseline). We also included a household asset index based on a principal component analysis of 23 self-reported durable household assets, such as number of telephones, cars and availability of a refrigerator. Finally, we added an indicator on whether the respondents already participated in any skills training programmes.

As we have already discussed and as reported in the baseline report, we are concerned about our data on programme take-up. Many of the participants who were randomly sorted to the treatment group did not enrol in the training and, among those who did, a considerable number only attended some of the training sessions. MEDA's initial requirements stated that participants should attend at least 75 per cent of classes to graduate; but they did not hold to this requirement for the cohort of youth participating in the study, given overall poor attendance rates. Thus, many graduated who missed more than a quarter of the classes but showed an interest in continuing the training.

Table 1 summarises attendance information from MEDA's records and the follow-up survey. Column 1 describes how many individuals surpassed or matched certain levels of attendance rates based on administrative data. This reveals a considerable heterogeneity regarding the attendance rate: while 469 persons attended at least 1 training session, only 248 (53%) completed at least 75% of the curriculum. Due to significant attrition, we were only able to interview 136 of those in the follow-up survey (column 2). In our analysis below, we consider those who participated in at least half of the classes and did not show evidence of dropping out of the programme (column 3) at its end as treated. This leaves us with 173 individuals whom we consider as having been treated for our analysis (column 4). However, coding of treatment status only matters for ATT and LATE estimates as ITT estimates only rely on treatment assignment, not on programme participation. In this report, we refer to this group as treated individuals; they are not to be confused with the treatment group, which consists of all individuals who were randomly assigned to receive the treatment.<sup>18</sup>

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<sup>18</sup> Similarly, when we mention the comparison group we refer to individuals who were randomly assigned to not receive the training. Note that we use the terms comparison group and control group interchangeably.

**Table 1: Attendance and treatment status**

	(1)	(2)	(3)	(4)
Attendance rate	Administrative data	Follow-up survey	Early dropouts	Remaining observations
>0%	469	234	43	191
25%+	422	208	21	187
50%+	<b>360</b>	<b>176</b>	<b>3</b>	<b>173</b>
75%+	248	136	0	136
100%	43	25	0	25

Importantly, not all individuals assigned to participate in ‘100 Hours to Success’ took part in the programme. Table 2 allows us to assess the degree of non-compliance in our evaluation by comparing assignment status to programme participation status. For the 444 assigned to participate in the project, 171 enrolled in and remained in training, a compliance rate of 38.5 per cent. Consulting administrative records shows that the average attendance rate<sup>19</sup> among these 171 is 83.6 per cent, a fairly high level of engagement in the training. By contrast, the average attendance rate among the non-compliers – those who were assigned to treatment but we do not consider as treated – is six per cent, although some within this group attended nearly half of the classes.<sup>20</sup> But we believe that this is unlikely to significantly bias our ATT/LATE estimates, as the treatment intensity in the whole comparison group (non-compliers in the treatment group plus compliers in the control group) is only 2.3 per cent.

**Table 2: Compliance with treatment assignment**

Baseline treatment assignment	Follow-up treatment status		
	Not-treated	Treated	Total
<i>Control group</i>	425 99.53%	2 0.47%	427 100%
<i>Treatment group</i>	273 61.49%	171 38.51%	444 100%
<b>Total</b>	698 80.14%	173 19.86%	871 100%

For those assigned to the control group, compliance is fairly clean. Of the 427 individuals in our control group (for whom we have baseline and follow-up data), 425 were not treated – a compliance rate of 99.5 per cent. According to MEDA’s administrative records, none of the 425 attended any training and only two found their way into training rolls. This is a valuable finding, because it allows us to directly

<sup>19</sup> The attendance rate is the percentage of classes relative to all classes offered to the respective individual.

<sup>20</sup> To be precise, the group of non-compliers who were initially assigned to treatment consists of two subgroups: the 212 individuals who did not receive any training at all and the 61 individuals who signed up for the programme but did not complete enough classes to be considered treated.

compare OLS estimates for ATT and the instrumental variable (IV) estimates for LATE. In general, the difference between the two concepts is that LATE estimates only inform us about the effect on those individuals who received the treatment because they were assigned to it (compliers), but tell us nothing about what happened to those who received the treatment regardless of being assigned to a treatment group. As there are virtually zero of these always-takers in the sample, ATT and LATE conceptually coincide. So substantial differences in the estimates of these effects could be seen as an indicator that one (or both) techniques produce biased results.<sup>21</sup>

Investigating compliance rates is important for several reasons when carrying out an impact evaluation:

- It informs us how well the programme attracted prospective beneficiaries and motivated them to complete (most parts of) the training;
- It indicates the extent to which statistical power got reduced, which has implications for the internal validity of the evaluation as a whole; and
- Importantly, the higher the degree of compliance, the better we can exploit the study's experimental design – in other words, its random placement of participants in treatment and control groups – to estimate programme impact.

When estimating LATE, we use programme assignment as an instrument for treatment status. For credible LATE results, any instruments must be both valid and relevant. To be valid, the randomisation procedure needs to have worked, in the sense that treatment and control groups on average share similar characteristics. We partially test this requirement by comparing both groups with respect to relevant (observable) control and outcome variables in Section 7.3. To be relevant, our instrument – treatment assignment – needs to be sufficiently correlated with the potentially endogenous regressor we want to replace (actual treatment). We can test whether we can consider an instrument relevant by analysing the first stage regression of the two-stage least square estimation method.

We obtained the results in Table 3 by regressing actual treatment status (see Table 2) on an indicator capturing random treatment assignment. In Table 3, column 1 represents a specification with only treatment and assignment status, while the results in columns 2–4 subsequently add control variables and fixed effects. Controlling for a number of household and individual background variables and individual effects, we find that assignment to the treatment group increased the probability of participating in the training by 38 percentage points.<sup>22</sup> As a rule of thumb, instruments are considered relevant if the F-statistic on the joint significance exceeds 10. The F-statistic for the specification in column 4 equals about 254.

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<sup>21</sup> Section 8.2.1 describes to what extent our OLS and IV estimates differ from each other.

<sup>22</sup> This is in line with Table 3, which shows that 38.5% of those assigned to treatment received the intervention compared with roughly 0.5% of those assigned to the control group. Subtracting these two numbers from each other yields conceptually the same result as column 1 (Table 3), which includes neither control variables nor fixed effects.

Assigned to treatment or control thus seems to be a relevant instrument, and weak instrument bias is likely not to be of concern for this evaluation.<sup>23</sup>

**Table 3: First-stage regression**

	(1)	(2)	(3)	(4)
<b>Random assignment</b> (treatment /comparison)	<b>0.380***</b>	<b>0.384***</b>	<b>0.380***</b>	<b>0.381***</b>
	0.029	0.024	0.024	0.024
<b>Constant</b>	<b>0.005</b>	<b>-0.214*</b>	<b>0.000</b>	<b>-0.088</b>
	0.017	0.120	0.008	0.088
<b>Control variables</b>	No	Yes	No	Yes
<b>Individual fixed effects</b>	No	No	Yes	Yes
<b>F-statistic – instrument</b>	<b>255.56</b>	<b>260.93</b>	<b>255.56</b>	<b>254.18</b>
# individuals	871	871	871	871
# observations	871	871	1,742	1,742

Note: Standard errors below estimates; \*/\*\*/\*\* statistically significant at 90/95/99% confidence level.

### 7.3 Balancing tables

Tables 4 and 5 provide a comparison of the treatment and control groups for the full sample and those included in both the baseline and follow-up surveys, allowing for an assessment of how well the randomised sorting worked and how attrition may have affected the sample. We do not attempt to provide a full descriptive analysis of the sample here. This can be found in the baseline report (Dyer *et al.* 2014). We discuss any differences between the baseline and the final sample that might have arisen due to the considerable attrition rate in Section 8.

Table 4 presents the results of our review of data on various control variables for the full baseline sample of 1,803 individuals. Note that behind the names of variables in this and all following tables, we can find information about the range of potential values.<sup>24</sup> There are no significant differences between participant and comparison groups, with p-values in all cases considerably larger than five per cent; this indicates that control variables for the whole sample are balanced between participants assigned to the treatment and the control group.

<sup>23</sup> Note that, while the first stage regression might include slightly fewer observations for some outcomes where not all study participants replied to the respective questions, the instrument remains highly relevant in all these cases.

<sup>24</sup> Indicator variables that either take the value one or zero are displayed as varname (0/1), while variables that can take continuous values from zero to one are shown as varname (0–1). Finally, variables that capture a (discrete) scale are listed as varname (lower bound to upper bound).

**Table 4: Balancing table, control variables (full sample)**

	<b>Full group: mean N=1,803</b>	<b>Control group: mean N=891</b>	<b>Δ (Treat- control)</b>	<b>p-value</b>
Female (0/1)	0.525	0.535	-0.020	0.395
Age	19.984	20.068	-0.167	0.200
Number of siblings	3.818	3.865	-0.093	0.349
Urban (0/1)	0.750	0.756	-0.013	0.523
Living in dormitory (0/1)	0.192	0.192	0.001	0.954
Household (HH) members	4.903	4.910	-0.013	0.888
Female HH head (0/1)	0.119	0.125	-0.011	0.490
Education level, HH head (0–6)	1.655	1.662	-0.014	0.865
Father alive (0/1)	0.915	0.914	0.003	0.814
HH assets (1–10)	4.024	4.050	-0.053	0.216
Satisfied with HH situation (1–4)	2.863	2.843	0.040	0.205
Attended training (0/1)	0.138	0.145	-0.014	0.378

Note: \*\*\*/\*\*\* statistically significant at 90/95/99% confidence level; number of observations: after quality checks, we included 1,803 out of 1,815 in the analysis.

We might be concerned that the same does not hold true when restricting our analysis to those interviewed for both baseline and follow-up surveys, given the high attrition rate. As Table 5 (which focuses on the restricted sample of 871 individuals) reveals, this concern is generally unfounded. In this regard, there are only two exceptions: individuals in the participant group are less likely to reside in urban areas and less likely to live in a female-headed household (both significant at the 10 per cent level).

**Table 5: Balancing table, control variables (follow-up sample)**

	<b>Full group: mean N=871</b>	<b>Control group: mean N=427</b>	<b>Δ (Treat- control)</b>	<b>p-value</b>
Female (0/1)	0.475	0.489	-0.028	0.413
Age	19.979	20.014	-0.068	0.720
Number of siblings	3.744	3.703	0.081	0.558
Urban (0/1)	0.781	0.808	-0.053	0.057*
Living in dormitory (0/1)	0.164	0.157	0.014	0.571
HH members	4.912	4.972	-0.118	0.397
Female HH head	0.126	0.148	-0.042	0.064*
Education level, HH head (0–6)	1.642	1.644	-0.004	0.971
Father alive (0/1)	0.902	0.895	0.015	0.448
HH assets (1–10)	4.055	4.071	-0.030	0.605
Satisfied with HH situation (1–4)	2.863	2.836	0.054	0.236
Attended training (0/1)	0.133	0.136	-0.005	0.822

Note: \*\*\*/\*\*\* statistically significant at 90/95/99% confidence level.

Tables E1 and E2 in Appendix E show the similarities between assigned treatment and control groups with respect to all outcome variables included in our analysis and already observed during the baseline survey. With one exception out of 16 outcome variables (reservation wage), there are no significant differences either for the whole sample or for participants who were interviewed both for the baseline and the follow-up survey.

Table 6 shows attrition rates by assignment and take-up status. While the value for the comparison group (52.1%) is slightly higher than the treatment group (51.3%), this difference is not significant at any conventional level (p-value: 0.75). Likewise, attrition rates only differ slightly (and insignificantly) when splitting the sample according to treatment. So, although the study faces a comparably high attrition rate overall, it does not seem plagued by differential attrition. While we might see this as indication that randomisation worked – in the sense that individuals in the treatment and control groups are on average comparable – we discuss the potential implications of our study’s high rate of attrition in further detail in Section 8, where we also provide a number of robustness checks.

**Table 6: Attrition rates among the treatment and control groups**

	Baseline	Follow-up	Attrition rate (%)	p-value
<b>Number of observations</b>	1,803	871	51.69	
<b>By random assignment</b>				
<i>Comparison</i>	891	427	52.08	0.747
<i>Offered treatment</i>	912	444	51.32	
<b>By take-up status</b>				
<i>Comparison</i>	1,451	698	51.90	0.726
<i>Treated</i>	352	173	50.85	

## 7.4 Evaluation outcomes

We now turn to the presentation of results addressing the three main areas of interest linked to the three main hypotheses about the impact of the ‘100 Hours to Success’ training: financial literacy and behaviour; community engagement and life skills; and education and labour market status. The main outcome tables report impact estimates for ATT, ITT and LATE. To allow us to contextualise the magnitude of the estimated impact coefficients, the tables also indicate the mean and standard deviation of the comparison group at the time of the follow-up survey. They also show whether the variable in question was observed before and after the intervention or only recorded in the follow-up questionnaire: column 2 shows whether we estimated a DID model for this outcome variable; in those where we did estimate the DID model, data for both the baseline and the follow-up survey were available. DID specifications include individual fixed effects and a set of (time-varying) control

variables.<sup>25</sup> For all other variables, where we made no DID estimations, we estimated a simple-difference model. As well as the set of (potentially) time-varying and time-invariant control variables that we described in Section 7.3, we include a full set of 13 youth centre dummies (for those where the baseline survey took place) to control for small differences in geographical origin.

After showing the main results for the whole observed study population (Table 7), we disaggregate the sample by gender (Table 8), age (Table 9) and affluence (Table 10). This subgroup analysis aims to uncover potential heterogeneous impacts in line with our assumptions and expectations about the training (see Section 2). This analysis is severely limited by the high attrition rate and the considerable degree of non-compliance among those assigned to treatment, greatly diminishing the power of the exercise. As a result, we limit the number of dimensions along which we split our sample and avoid disaggregation criteria that would lead to a very uneven split. For example, geographical origin would leave us with a sample of 191 rural youth (51 treated individuals). We also avoid disaggregating the sample along two subgroup criteria at the same time – for example, young and female; more affluent and male).

Finally, when we split the sample into subgroups, we only report LATE estimates. As we mentioned in Section 7.2, their interpretation comes close to an ATT, as non-compliance is almost exclusively present in the treatment group. We also believe LATE estimates to be more credible than OLS impact coefficients, which are likely plagued by selection bias. With all these limitations in mind, we should regard results for subgroups as an indication of how potential treatment effects might vary along gender, age and household asset dimensions but not expect to obtain precisely identified point estimates.

#### *7.4.1 Outcomes related to financial literacy and behaviour*

Table 7 displays the results related to the first of the three primary hypotheses, documenting impacts on participants' (practical) knowledge about saving and lending and their financial behaviour. To measure financial literacy, participants were presented with four statements that related to saving and lending services and asked to rate them as right or wrong; they were also asked to self-assess whether they could identify two saving and lending institutions. These six survey items constitute the financial literacy index that is scaled such that it describes the combined percentage of correct (knowledge items) and positive answers (self-assessed knowledge).<sup>26</sup> Table 7 shows a moderate positive effect on the financial literacy index, but this is only significant in the ATT specification that is prone to self-selection bias.

Next, we turn to the analysis of actual financial behaviour, considering survey responses to several questions. The first is whether the individual has a savings

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<sup>25</sup> Potentially time-varying control variables are indicators for living in an urban area and whether respondents already participated in any skills training programmes; number of persons living in the same household; and satisfaction with overall household situation.

<sup>26</sup> Note that we therefore based the financial literacy index on six binary survey items (either yes/no or right/wrong) and calculated it by taking the average value of the six items for every individual. For more details, see Appendix D.

account.<sup>27</sup> It is worth noting here that the rate for having a bank account among our control group – 35.6 per cent – is quite high, given the youth of our target population and the lack of financial services for youth across Morocco. The World Bank found that only about 12 per cent of Moroccan youth maintained savings accounts in a formal financial institution (World Bank 2012). Participants in ‘100 Hours to Success’ are estimated to be about 15 percentage points (ATT) more likely to maintain a savings account than those who did not participate in the programme. Column 3 shows that this is likely to be an underestimation as, according to the LATE estimator, the causal impact of the training on the probability of maintaining a savings account is around 27 percentage points – an estimate that is significant at the one per cent level.<sup>28</sup> At the same time, we should emphasise that participants were asked to open a savings account upon registering for the training programme. However, it is worth noting that, two years after participants opened their bank accounts, they still seem to have them.

Importantly, although they have maintained these accounts, they do not show a statistically significant increase in savings behaviour over individuals in our control group, nor do they report any observable increase in self-reported use of a budget in maintaining their personal finances. While approximately 12 per cent of our study population in the control group has borrowed money since October 2012, there is no statistically significant difference in borrowing between the treatment and control populations. If insignificant, LATE impact estimates are also relatively small in absolute size: around 0.1 standard deviations.

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<sup>27</sup> These impact estimates are based on estimating equations (1), (2) and (3) (see Section 7.1). MEDA strongly encouraged prospective participants to open a bank account, but only after the baseline survey and randomisation were complete. As for other outcome variables, data for whether individuals have savings accounts come from the baseline and follow-up survey, which both rely on self-reported answers.

<sup>28</sup> A likely explanation for this discrepancy is that the comparison group for the ATT estimate consists not only of youth randomly assigned not to participate in the programme, but also of individuals who enrolled in the training but did not attend enough sessions to be considered as treated (at a minimum, they had to attend 50 per cent of the trainings offered to them – see Table 1). Indeed, a closer inspection of the data reveals that about 48% of this subgroup have a savings account, compared with only around 36% of those assigned to the comparison group. In fact, all youth were strongly encouraged to open a savings account before or right at the beginning of the training sessions.

**Table 7: Impact estimates for financial literacy and behaviour**

	DID	N	Control group: mean (s.d.)	(1) ATT	(2) ITT	(3) LATE
<b>Financial literacy</b>						
Financial literacy index (0–1)	No	871	0.499	0.064***	0.016	0.042
			0.285	0.024	0.020	0.052
<b>Financial behaviour</b>						
Has savings account (0/1)	Yes	871	0.356	0.148***	0.102***	0.269***
Saves (0/1)	Yes	871	0.479	0.054	0.039	0.103
			0.486	0.055	0.044	0.115
Participates in savings group (0/1)	No	871	0.035	-0.010	-0.007	-0.018
Maintains a budget (0/1)	No	871	0.184	0.015	0.013	0.033
			0.500	0.043	0.034	0.089
Borrowed since Oct 2012 (0/1)	No	871	0.124	-0.008	0.013	0.035
			0.330	0.029	0.023	0.060

Note: Robust standard errors below impact estimates; \*/\*\*/\*\* statistically significant at 90/95/99% confidence level; s.d. = standard deviation.

Table 8 shows outcomes in financial literacy and behaviour by gender. The data reveal considerable gender disparities in our sample. Looking at the average in the comparison group, men show more active financial behaviour across all outcome variables. This does not seem to be driven by knowledge differences: if anything, women in the comparison group display stronger financial literacy results than their male counterparts. This points to other constraints that prevent young Moroccan women from participating in economic and financial activities – for example, socio-cultural perceptions of women in the highly conservative Oriental region limit the sectors and types of work they can engage in, favouring public sector jobs or home-based enterprises.

The moderate positive impact on the financial literacy index that we estimate for the whole sample seems to be the result of an improvement among male participants, even though – perhaps as a result of the reduced sample – the estimate fails to show statistical significance. But this does not translate into greater financial activity among men. On the contrary, across outcomes, impact estimates for financial behaviour are more pronounced (more positive) for women. Notably, programme participation increased the likelihood of women holding a savings account by 32.3 percentage points. This effect was not nearly as strong among men (20.9 percentage points) and – likely due to the now much smaller sample – loses statistical significance. Estimates for saving and lending behaviour and maintaining a budget show positive signs for women and negative signs for men. While none of these results is backed up by statistical significance, gender disparities generally appear smaller among programme participants compared with gender differences in the comparison group.

**Table 8: Impact estimates for financial behaviour, by gender**

	DID	WOMEN			MEN		
		N women	Control group: mean (s.d.)	LATE	N men	Control group: mean (s.d.)	LATE
<b>Financial literacy</b>							
Financial literacy index (0–1)	<b>No</b>	<b>414</b>	<b>0.515</b>	<b>-0.016</b>	<b>457</b>	<b>0.483</b>	<b>0.096</b>
			0.293	0.065		0.278	0.084
<b>Financial behaviour</b>							
Has savings account (0/1)	<b>Yes</b>	<b>414</b>	<b>0.306</b>	<b>0.323**</b>	<b>457</b>	<b>0.404</b>	<b>0.209</b>
			0.462	0.130		0.492	0.164
Saves (0/1)	<b>Yes</b>	<b>414</b>	<b>0.292</b>	<b>0.131</b>	<b>457</b>	<b>0.463</b>	<b>-0.282</b>
			0.456	0.141		0.500	0.187
Participates in savings group (0/1)	<b>No</b>	<b>414</b>	<b>0.014</b>	<b>0.044</b>	<b>457</b>	<b>0.055</b>	<b>-0.076</b>
			0.119	0.034		0.229	0.066
Maintains a budget (0/1)	<b>No</b>	<b>414</b>	<b>0.392</b>	<b>0.070</b>	<b>457</b>	<b>0.560</b>	<b>-0.142</b>
			0.489	0.106		0.498	0.148
Borrowed since Oct 2012 (0/1)	<b>No</b>	<b>414</b>	<b>0.077</b>	<b>0.066</b>	<b>457</b>	<b>0.170</b>	<b>-0.004</b>
			0.267	0.065		0.376	0.112

Note: Robust standard errors below impact estimate; \*/\*\*/\*\* statistically significant at 90/95/99% confidence level.

Table 9 displays estimates on financial outcomes by age group: younger (19 years or below at the time of the baseline survey in 2012) and older (20+). Generally, we do not observe differences between younger and older individuals in the comparison group. In this subgroup analysis, the impact on financial literacy is strongly positive and highly significant for older youth while the coefficient for younger participants turns negative (but is statistically insignificant). Maintaining a savings account seems concentrated among younger individuals with a significant positive impact in both age groups (at the 10 per cent level), but estimates for other outcome variables are insignificant and do not show a clear pattern.

**Table 9: Impact estimates for financial behaviour, by age at baseline**

	DID	YOUTH AGED 20+			YOUTH AGED UNDER 20		
		N 20+	Control group: mean (s.d.)	LATE	N under 20	Control group: mean (s.d.)	LATE
<b>Financial literacy</b>							
Financial literacy index (0–1)	<b>No</b>	<b>481</b>	<b>0.476</b>	<b>0.166***</b>	<b>390</b>	<b>0.529</b>	<b>-0.129</b>
			0.281	0.064		0.289	0.089
<b>Financial behaviour</b>							
Has savings account (0/1)	<b>Yes</b>	<b>481</b>	<b>0.333</b>	<b>0.214*</b>	<b>390</b>	<b>0.385</b>	<b>0.336*</b>
			0.472	0.128		0.488	0.171
Saves (0/1)	<b>Yes</b>	<b>481</b>	<b>0.354</b>	<b>0.073</b>	<b>390</b>	<b>0.412</b>	<b>-0.243</b>
			0.479	0.139		0.493	0.202
Participates in savings group (0/1)	<b>No</b>	<b>481</b>	<b>0.033</b>	<b>-0.015</b>	<b>390</b>	<b>0.037</b>	<b>0.023</b>
			0.180	0.041		0.190	0.059
Maintains a budget (0/1)	<b>No</b>	<b>481</b>	<b>0.467</b>	<b>-0.035</b>	<b>390</b>	<b>0.492</b>	<b>0.000</b>
			0.500	0.108		0.501	0.147
Borrowed since Oct 2012 (0/1)	<b>No</b>	<b>481</b>	<b>0.125</b>	<b>0.055</b>	<b>390</b>	<b>0.123</b>	<b>-0.012</b>
			0.331	0.076		0.329	0.096

Note: Robust standard errors below impact estimates; \*/\*\*/\*\* statistically significant at 90/95/99% confidence level.

Table 10 shows differences in financial outcomes by economic background: those within the top and bottom halves of the asset ranking. Using principal component analysis, we calculated a scaled score for each respondent based on self-reported household assets. There are only small differences when considering the control group: individuals from the lower socio-economic status show slightly lower financial literacy indicators, but approximately the same financial activity levels.

We find that the improvement in financial literacy observed for the whole sample concentrates entirely among individuals from less advantageous economic backgrounds, but again, the estimate fails to show statistical significance. There is a sizeable programme effect on maintaining a savings account and a strong association between treatment and borrowing among those of higher socio-economic status (as measured by household assets). While 9.7 per cent of the control group

had borrowed since October 2012, when asked in the follow-up survey, those who attended the training were estimated to be 19.8 percentage points more likely to have borrowed during this time. Among more marginalised people, if there is an association with programme participation, it is negative. This is suggestive perhaps of the familiarity that individuals from comparatively more affluent households might have already had with issues related to financial services.

**Table 10: Impact estimates for financial behaviour, by household assets**

	DID	HH ASSETS BOTTOM 50%			HH ASSETS TOP 50%		
		N bottom 50%	Control group: mean (s.d.)	LATE	N top 50%	Control group: mean (s.d.)	LATE
<b>Financial literacy</b>							
Financial literacy index (0–1)	No	436	0.454	0.096	435	0.542	-0.008
			0.283	0.064		0.281	0.082
<b>Financial behaviour</b>							
Has savings account (0/1)	Yes	436	0.343	0.218*	435	0.369	0.329*
			0.476	0.126		0.484	0.171
Saves (0/1)	Yes	436	0.376	-0.158	435	0.382	0.063
			0.486	0.140		0.487	0.191
Participates in savings group (0/1)	No	436	0.019	0.070*	435	0.051	-0.107**
			0.137	0.043		0.220	0.052
Maintains a budget (0/1)	No	436	0.505	-0.128	435	0.452	0.080
			0.501	0.109		0.499	0.145
Borrowed since Oct 2012 (0/1)	No	436	0.152	-0.092	435	0.097	0.198**
			0.360	0.077		0.296	0.098

Note: Robust standard errors below impact estimates; \*/\*\*/\*\* statistically significant at 90/95/99% confidence level.

#### 7.4.2 Outcomes related to community engagement and life skills

In this section, we review outcomes related to two distinct but interlinked categories:

- **Engagement in the community:** Whether the respondent ever volunteers for a charity, how satisfied they are with their role in the community and self-perceived capacity to resolve problems in the community (Flanagan, Syversten and Stout 2007); and

- **Self-reported perspectives on life skills and self-efficacy:** We use the GRIT scale (Duckworth *et al.* 2007) to measure the tendency to sustain interest in and effort towards long-term goals; the Risk Propensity Scale (Meertens and Lion 2008) to assess respondents' willingness to take on risks; and Rosenberg's Self-Esteem Scale to capture respondents' own perceptions of self-esteem (Rosenberg 1965).

In both areas, we find largely insignificant increases that are small in absolute size and do not show a clear pattern. The same applies when we break down the data along gender, age and household asset lines (see Tables E5, E6 and E7 in Appendix E). But we should emphasise that we collected the follow-up survey data nearly three years after the programme began. It is quite likely that the programme had had a more immediate impact in these areas, but the retention of this information had faded. We discuss this further in Section 8.

**Table 11: Impact estimates for community engagement and life skills**

	DID	N	Control group: mean (s.d.)	(1) ATT	(2) ITT	(3) LATE
<b>Community engagement</b>						
Ever volunteered (0/1)	Yes	871	0.152 0.360	0.067* 0.038	0.017 0.032	0.044 0.084
Satisfied with role in community (1–5)	Yes	871	2.520 0.733	-0.089 0.084	-0.056 0.067	-0.147 0.176
Community problem-solving scale (0–4)	No	870	3.366 0.842	-0.014 0.069	-0.105 0.056	-0.276 0.147
<b>Life skills</b>						
Risk scale (1–5)	No	871	2.224 0.500	0.001 0.039	0.031 0.034	0.083 0.089
GRIT scale (1–5)	No	871	3.338 0.486	0.008 0.040	0.007 0.033	0.018 0.086
Rosenberg's self-esteem scale (0–30)	No	870	20.850 3.173	-0.165 0.262	-0.289 0.208	-0.754 0.540

Note: Robust standard errors below impact estimates; \*/\*\*/\*\* statistically significant at 90/95/99% confidence level.

### 7.4.3 Education, entrepreneurship and employment

In this section, we document the impacts of '100 Hours to Success' in education and the labour market. Irrespective of the programme, we would expect to see considerable changes in our outcome variables, given the study's three-year time horizon. Indeed, comparing baseline and follow-up averages of key educational and labour market indicators confirms that the intervention reached a population in the

transition between school and work. So, while enrolment in education dropped from 89% to 51%, labour market participation increased from 13% to 47% over the course of this study.

Closer inspection of the data reveals that almost three-quarters of those in the comparison group who were still in education (72 per cent) were enrolled in a university programme. Data on the highest level of education attained so far show that most respondents in the control group had a post-secondary degree (40% a professional diploma and 32% a university degree). Only 7% of participants were employed at the time of the baseline and 67% had no work experience. Three years later, slightly less than a third (29%) were employed and two-thirds (67%) had acquired some work experience (on average around one year).<sup>29</sup>

ATT estimates on educational enrolment are positive and statistically significant (see Table 12). Their corresponding LATE coefficients are much smaller in size (and lose significance), which again suggests that the decision to take part in the programme was selective. The LATE estimates indicate that treated participants are slightly more likely to remain in education, although none reaches statistical significance.

We also observe six different labour market-related outcome variables:

- labour market status: whether a respondent is employed, unemployed (without employment in the past seven days, but available and actively looking for work) or inactive
- whether respondents are NEET<sup>30</sup>
- whether they have any work experience
- whether there was a proxy reservation wage
- number of jobs held so far
- number of months worked so far.<sup>31</sup>

We do not distinguish between self-employment and wage employment, as only 25 (out of 871) individuals in the follow-up survey identified as entrepreneurs, which does not allow a meaningful quantitative analysis.

Table 12 documents that we find no statistically significant impact of the '100 Hours to Success' training in five out of six labour market outcomes. The exception is labour market status: according to the LATE estimates, programme participation decreases the likelihood of being employed by 17.2 percentage points. This result 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

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<sup>29</sup> Most of the statistics presented in this paragraph are from Table 12 (follow-up) and Table E2 in Appendix E (baseline). To ensure comparability, the averages presented at the time of the baseline are restricted to the comparison group only. Table E2 also shows that the differences between comparison and treatment group at the time of the baseline are marginal.

<sup>30</sup> Enrolment in education includes secondary schools, vocational schools and universities.

<sup>31</sup> The number of months worked covers the current job (if any) and up to the last three jobs: 95 per cent of respondents indicated that they had held three jobs or less in the follow-up survey.

(18.3 percentage points) with unemployment being virtually unaffected. Although they lack statistical significance, the results for the other five outcome indicators are in line with the observed decrease in employment probability. Training participants on average have worked fewer months and show a slightly greater risk of being NEET.

The result that programme participation reduces – rather than increases – labour market activity appears counterintuitive. To understand the dynamics at work better, a joint analysis of educational and labour market outcomes proves insightful. We record three labour market outcomes – employed, unemployed or inactive – and whether respondents are still or no longer enrolled in education. Combining both dimensions results in six mutually exclusive outcome categories, and each youth belongs to exactly one of these. For example, at the time of the baseline survey, 79% of youth (from the restricted sample of 871 observations that we are using for impact analysis) were still enrolled in education and inactive in the labour market; 6% were not in education and inactive; 5% were enrolled in education while looking for work, and so on.

Figure 4 presents LATE impact estimates based on both baseline and follow-up data for these six categories. Note that as each youth belongs to exactly one category, the six impact estimates necessarily add up to zero. Figure 4 documents three important findings:

- Almost three-quarters of the increase in inactivity can be attributed to individuals who are staying in education: the increase in inactivity and being enrolled in education is 13.2 percentage points, compared with 18.3 percentage points when disregarding educational enrolment as shown in Table 12, column 3;
- Some programme participants seem to stay longer in education (even though, as Table 12 shows, this effect is not significant); and
- Among all of those enrolled in education, fewer are currently employed or looking for work.

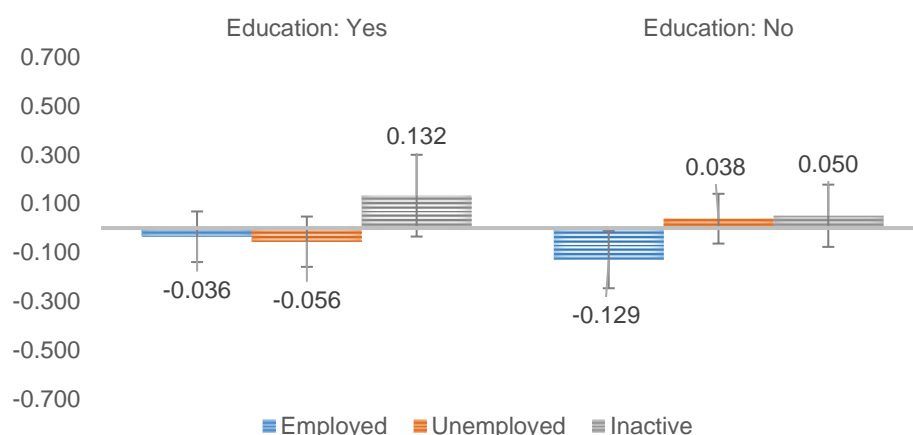
These findings are consistent with programme participants who invest and focus more in human capital accumulation through (formal) education and training rather than acquiring early labour market experience.

**Table 12: Impact estimates for education and employment outcomes**

	DID	N	Control group: mean (s.d.)	(1) ATT	(2) ITT	(3) LATE
<b>Education</b>						
Currently enrolled (0/1)	Yes	871	<b>0.513</b>	<b>0.124***</b>	<b>0.016</b>	<b>0.041</b>
			0.500	0.046	0.036	0.094
Enrolment level (0–4)	Yes	871	<b>1.862</b>	<b>0.329*</b>	<b>0.078</b>	<b>0.204</b>
			1.874	0.184	0.139	0.363
Attainment level (1–6)	Yes	871	<b>3.974</b>	<b>-0.129</b>	<b>0.028</b>	<b>0.075</b>
			1.006	0.088	0.071	0.186
Aspiration level (1–6)	Yes	869	<b>4.431</b>	<b>0.060</b>	<b>0.018</b>	<b>0.050</b>
			1.545	0.132	0.107	0.300
<b>Labour market</b>						
NEET (0/1)	Yes	871	<b>0.290</b>	<b>-0.029</b>	<b>0.034</b>	<b>0.089</b>
			0.454	0.046	0.035	0.091
Employed (0/1)	Yes	871	<b>0.286</b>	<b>-0.081*</b>	<b>-0.063*</b>	<b>-0.165*</b>
			0.452	0.040	0.034	0.088
Unemployed (0/1)	Yes	871	<b>0.180</b>	<b>0.007</b>	<b>-0.007</b>	<b>-0.018</b>
			0.385	0.042	0.031	0.082
Inactive (0/1)	Yes	871	<b>0.534</b>	<b>0.074</b>	<b>0.070*</b>	<b>0.183*</b>
			0.499	0.050	0.039	0.102
Any work experience (0/1)	Yes	871	<b>0.665</b>	<b>-0.056</b>	<b>-0.018</b>	<b>-0.046</b>
			0.473	0.053	0.040	0.104
Reservation wage (MAD)	Yes	871	<b>3,652</b>	<b>213</b>	<b>87.851</b>	<b>224</b>
			1,852	198	150.333	382
Number of jobs so far	No	871	<b>1.007</b>	<b>-0.141</b>	<b>-0.065</b>	<b>-0.173</b>
			1.388	0.108	0.091	0.235
Number of months worked	No	790	<b>12.000</b>	<b>-0.622</b>	<b>-1.704</b>	<b>-4.488</b>
			22.001	1.883	1.490	3.873

Note: Robust standard errors below impact estimates; \*/\*\*/\*\* statistically significant at 90/95/99% confidence level; MAD=Moroccan dirhams.

**Figure 4: Labour market status and education**



Note: N=871; 90 per cent confidence intervals based on robust standard errors plotted together with impact estimates; education refers to current enrolment in a secondary school, vocational school or university.

Turning to a detailed subgroup analysis, Table 13 disaggregates results on employment prospects by gender. Averages in the comparison group reveal that women are more likely than men to be enrolled in education or training (57% compared with 46%). Having attained essentially the same education as men to date, women are also more ambitious with respect to their aspired level of education (4.7 on a scale of 1–6, compared with 4.2). But this does not translate into better labour market outcomes: in fact, women are much less likely than their male counterparts to be employed (12% compared with 44%) and much more likely to be outside of the labour force (73% compared with 34%).

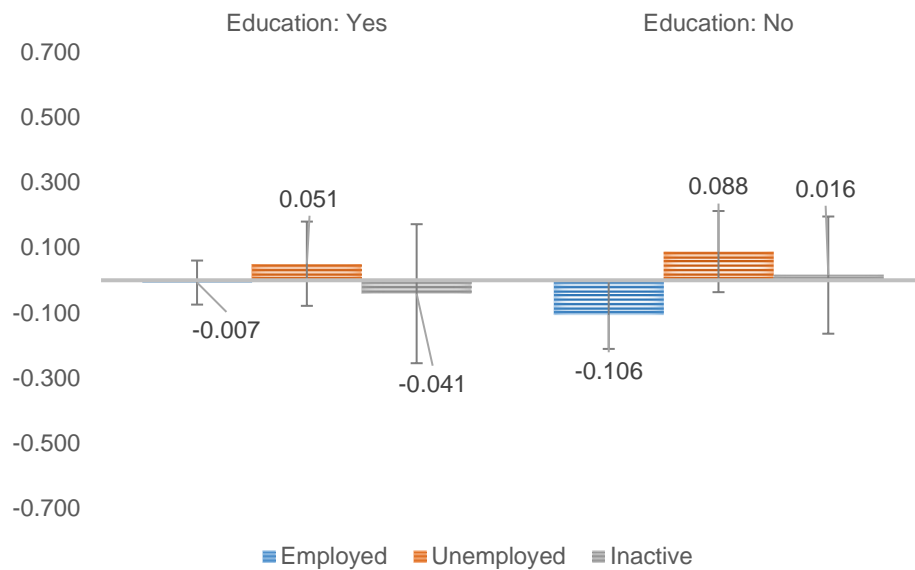
Compared with the overall impact estimates, two interesting trends emerge when we disaggregate by gender. First, women show less tendency to remain in education, while the impact on labour market outcomes are more conflicting. Importantly, none of the coefficients reaches statistical significance. Second, the tendencies we observe for the whole sample seem to come entirely from the men. Impacts for men remaining in education are stronger (but still insignificant), while there is an enormous drop from the labour force (seen through a rise in inactivity). This is around twice as large among men as it is among the overall sample (and highly significant). Figures 5 and 6, which combine labour market status and educational enrolment, confirm these observations. They also mirror the analysis we presented in Figure 4 for the whole sample. Coefficients for women vary around zero (with large confidence intervals). For men, the pronounced rise in labour market inactivity is strongly associated with enrolment in education. As Figure 6 shows, the overall estimate of an increase in the probability of inactivity (48 percentage points) is broken down into increased inactivity while staying in education (39 percentage points) and inactivity while not pursuing further education (9 percentage points). These trends seem to be driven by both men staying longer in education and men who, while staying in education, reduce their labour market activity.

**Table 13: Impact estimates for education and employment outcomes, by gender**

	DID	WOMEN			MEN		
		N women	Control group: mean (s.d.)	LATE	N men	Control group: mean (s.d.)	LATE
<b>Education</b>							
Currently enrolled (0/1)	Yes	414	<b>0.569</b>	<b>0.003</b>	457	<b>0.459</b>	<b>0.117</b>
			0.496	0.116		0.499	0.150
Enrolment level (0–4)	Yes	414	<b>2.105</b>	<b>0.108</b>	457	<b>1.628</b>	<b>0.412</b>
			1.888	0.473		1.835	0.558
Attainment level (1–6)	Yes	414	<b>3.957</b>	<b>0.300</b>	457	<b>3.991</b>	<b>-0.273</b>
			1.111	0.226		0.895	0.290
Aspiration level (1–6)	Yes	413	<b>4.678</b>	<b>0.216</b>	457	<b>4.197</b>	<b>-0.149</b>
			1.486	0.396		1.566	0.453
<b>Labour market</b>							
NEET (0/1)	Yes	414	<b>0.335</b>	<b>0.104</b>	457	<b>0.248</b>	<b>0.071</b>
			0.473	0.119		0.433	0.143
Employed (0/1)	Yes	414	<b>0.124</b>	<b>-0.113</b>	457	<b>0.440</b>	<b>-0.274</b>
			0.331	0.074		0.498	0.169
Unemployed (0/1)	Yes	414	<b>0.144</b>	<b>0.139</b>	457	<b>0.216</b>	<b>-0.210</b>
			0.351	0.102		0.412	0.135
Inactive (0/1)	Yes	414	<b>0.732</b>	<b>-0.025</b>	457	<b>0.344</b>	<b>0.484***</b>
			0.444	0.117		0.476	0.171
Any work experience (0/1)	Yes	414	<b>0.560</b>	<b>0.011</b>	457	<b>0.766</b>	<b>-0.103</b>
			0.498	0.122		0.424	0.173
Reservation wage (MAD)	Yes	414	<b>3,800</b>	<b>-23</b>	457	<b>3,512</b>	<b>503</b>
			2,174	521		1,475	556
Number of jobs so far	Yes	414	<b>0.622</b>	<b>0.137</b>	457	<b>1.376</b>	<b>-0.532</b>
			0.978	0.242		1.608	0.459
Number of months worked	No	414	<b>4.308</b>	<b>0.259</b>	457	<b>19.080</b>	<b>-11.019</b>
			8.146	2.316		27.670	8.476

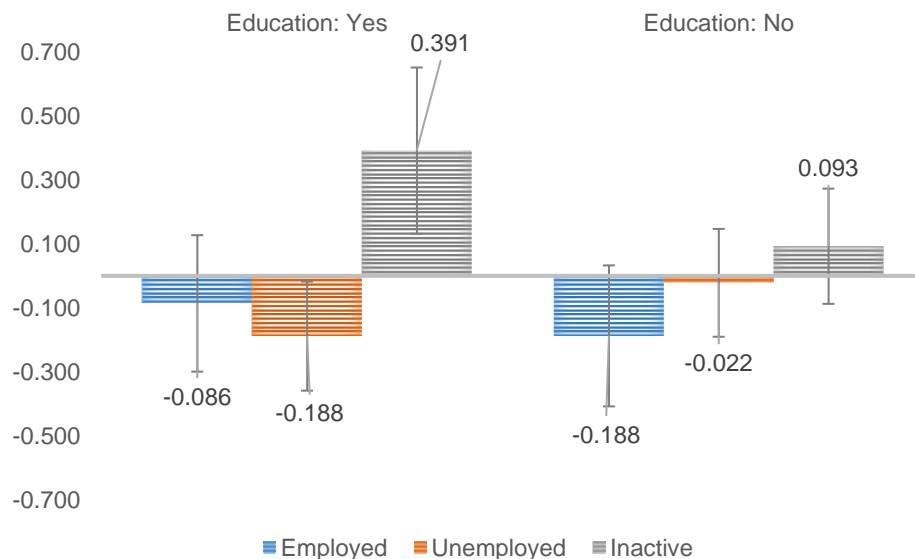
Note: Robust standard errors below impact estimates; \*/\*\*/\*\* statistically significant at 90/95/99% confidence level.

**Figure 5: Labour market status and education: women**



Note: N=414; 90 per cent confidence intervals based on robust standard errors plotted together with impact estimates; education refers to current enrolment in a secondary school, vocational school or university.

**Figure 6: Labour market status and education: men**



Note: N=457; 90 per cent confidence intervals based on robust standard errors plotted together with impact estimates; education refers to current enrolment in a secondary school, vocational school or university.

Next, we disaggregate the sample into two age groups: older (average age 25 at follow-up stage) and younger (average age 20 at follow-up). Considering averages in the control group, older youth were – as we might expect – less likely to be still

enrolled in education. They would likely have attained a higher level of education than their younger study participants. But this is not associated with better labour market outcomes – in terms of employment, number of jobs or months of accumulated work experience. Disregarding programme impact, those who are on average five years older (25 as opposed to 20) fail to perform better on the labour market. This is a telling sign of a long and difficult school-to-work transition period for Moroccan youth.

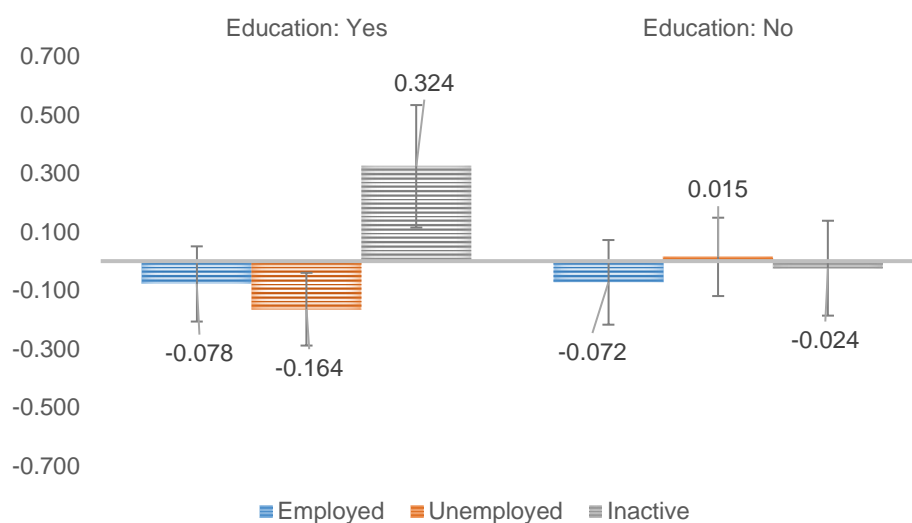
Considering impact, coefficients shown in Table 14 generally lack statistical significance and add little to the tendencies observed for the overall sample. Combining employment and labour market outcomes (Figure 7) reveals that older youth seem to be driven away from labour market activity and towards focusing on and staying in education. In fact, for older youth, almost all impact on labour market activity seems to be combined with a prolonged stay in education or reduced labour market activity while staying in education. By contrast, impact coefficients for younger youth (Figure 8) do not reveal a clear pattern and come with huge confidence intervals, as they fail to show statistical significance.

**Table 14: Impact estimates for education and employment outcomes, by age at baseline**

	DID	YOUTH AGED 20+			YOUTH AGED UNDER 20		
		N aged 20+	Control group: mean (s.d.)	LATE	N under 20	Control group: mean (s.d.)	LATE
<b>Education</b>							
Currently enrolled (0/1)	Yes	481	<b>0.488</b>	<b>0.082</b>	390	<b>0.545</b>	<b>-0.017</b>
			0.501	0.117		0.499	0.162
Enrolment level (0–4)	Yes	481	<b>1.792</b>	<b>0.107</b>	390	<b>1.952</b>	<b>0.357</b>
			1.890	0.448		1.856	0.613
Attainment level (1–6)	Yes	481	<b>4.158</b>	<b>-0.350</b>	390	<b>3.738</b>	<b>0.774**</b>
			0.989	0.218		0.979	0.346
Aspiration level (1–6)	Yes	480	<b>4.464</b>	<b>0.071</b>	390	<b>4.389</b>	<b>-0.014</b>
			1.575	0.394		1.507	0.450
<b>Labour market</b>							
NEET (0/1)	Yes	481	<b>0.321</b>	<b>-0.010</b>	390	<b>0.251</b>	<b>0.233</b>
			0.468	0.115		0.435	0.149
Employed (0/1)	Yes	481	<b>0.279</b>	<b>-0.150</b>	390	<b>0.294</b>	<b>-0.188</b>
			0.450	0.111		0.457	0.144
Unemployed (0/1)	Yes	481	<b>0.208</b>	<b>-0.150</b>	390	<b>0.144</b>	<b>0.221*</b>
			0.407	0.105		0.352	0.133
Inactive (0/1)	Yes	481	<b>0.513</b>	<b>0.300**</b>	390	<b>0.561</b>	<b>-0.033</b>
			0.501	0.130		0.498	0.166
Any work experience (0/1)	Yes	481	<b>0.654</b>	<b>-0.008</b>	390	<b>0.679</b>	<b>-0.090</b>
			0.477	0.129		0.468	0.176
Reservation wage (MAD)	Yes	481	<b>3,595</b>	<b>810*</b>	390	<b>3,724</b>	<b>-743</b>
			1,653	453		2,079	694
Number of jobs so far	No	481	<b>1.004</b>	<b>-0.184</b>	390	<b>1.011</b>	<b>-0.086</b>
			1.401	0.291		1.376	0.399
Number of months worked	No	434	<b>11.586</b>	<b>-0.787</b>	356	<b>12.520</b>	<b>-10.229</b>
			21.707	4.625		22.418	6.933

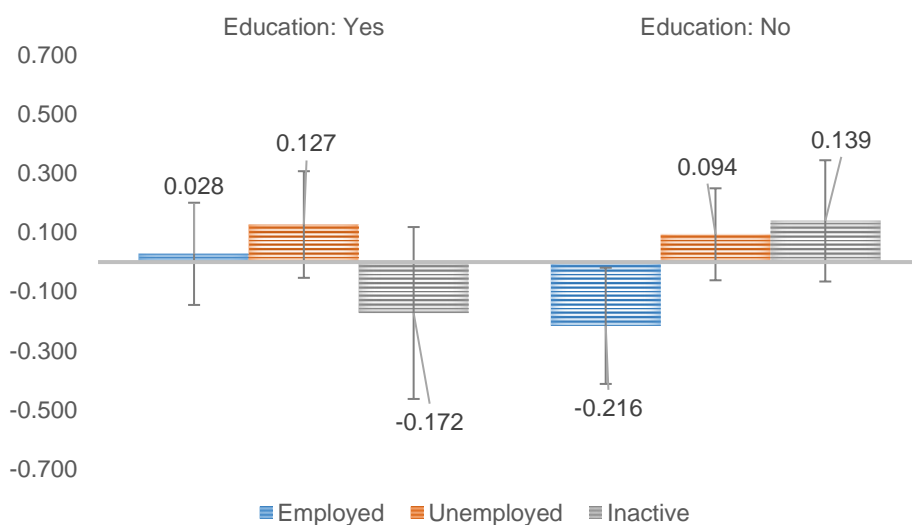
Note: Robust standard errors below impact estimates; \*/\*\*/\*\* statistically significant at 90/95/99% confidence level.

**Figure 7: Labour market status and education: age 20+**



Note: N=481; 90 per cent confidence intervals based on robust standard errors plotted together with impact estimates; education refers to current enrolment in a secondary school, vocational school or university.

**Figure 8: Labour market status and education: age <20**



Note: N=390; 90 per cent confidence intervals based on robust standard errors plotted together with impact estimates; education refers to current enrolment in a secondary school, vocational school or university.

In Table 15, we divide the results on educational and labour market outcomes by level of household socio-economic status. We found no considerable differences when comparing control group averages for youth from more and less affluent backgrounds. But there is an interesting difference with respect to impact estimates.

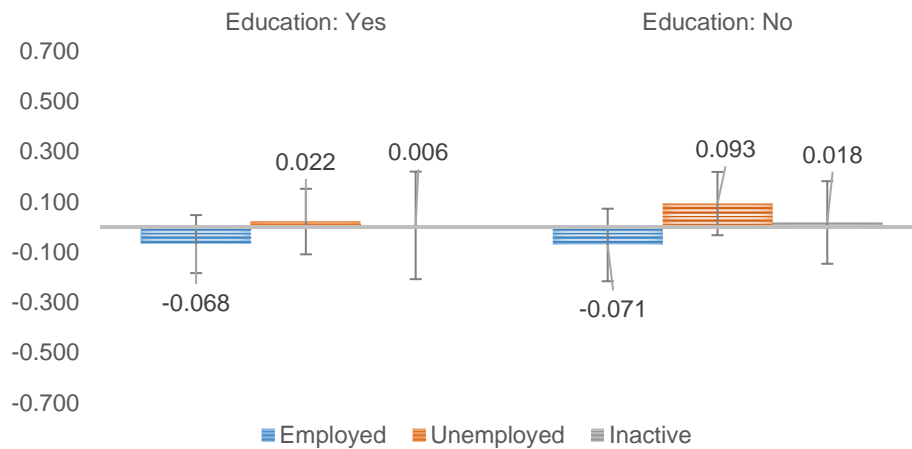
Youth from less well-off backgrounds do not seem to react systematically to the intervention; this is also apparent from the combined educational and labour market outcome categories displayed in Figure 9. Study participants from more affluent households, on the other hand, show a pronounced increase in labour market inactivity. As Figure 10 reveals, this is largely accompanied by a prolonged stay in education.

**Table 15: Impact estimates for education and employment outcomes, by household asset**

	DID	HH ASSETS BOTTOM 50%			HH ASSETS TOP 50%		
		N bottom 50%	Control group: mean (s.d.)	LATE	N top 50%	Control group: mean (s.d.)	LATE
<b>Education</b>							
Currently enrolled (0/1)	Yes	436	0.524	-0.040	435	0.502	0.141
			0.501	0.118		0.501	0.155
Enrolment level (0–4)	Yes	436	1.905	-0.037	435	1.820	0.509
			1.879	0.441		1.873	0.608
Attainment level (1–6)	Yes	436	3.995	0.042	435	3.954	0.096
			0.981	0.232		1.031	0.303
Aspiration level (1–6)	Yes	435	4.351	0.063	435	4.512	-0.017
			1.584	0.374		1.504	0.492
<b>Labour market</b>							
NEET (0/1)	Yes	436	0.276	0.111	435	0.304	0.087
			0.448	0.115		0.461	0.147
Employed (0/1)	Yes	436	0.290	-0.139	435	0.281	-0.218
			0.455	0.104		0.451	0.152
Unemployed (0/1)	Yes	436	0.162	0.115	435	0.198	-0.176
			0.369	0.105		0.400	0.134
Inactive (0/1)	Yes	436	0.548	0.025	435	0.521	0.394**
			0.499	0.126		0.501	0.173
Any work experience (0/1)	Yes	436	0.600	0.026	435	0.728	-0.148
			0.491	0.134		0.446	0.164
Reservation wage (MAD)	Yes	436	3,344	248	435	3,948	199
			1,828	458		1,831	642
Number of jobs so far	No	436	0.871	-0.357	435	1.138	0.178
			1.326	0.266		1.437	0.412
Number of months	No	391	11.723	-7.851*	399	12.263	0.106
			20.570	4.291		23.328	6.598

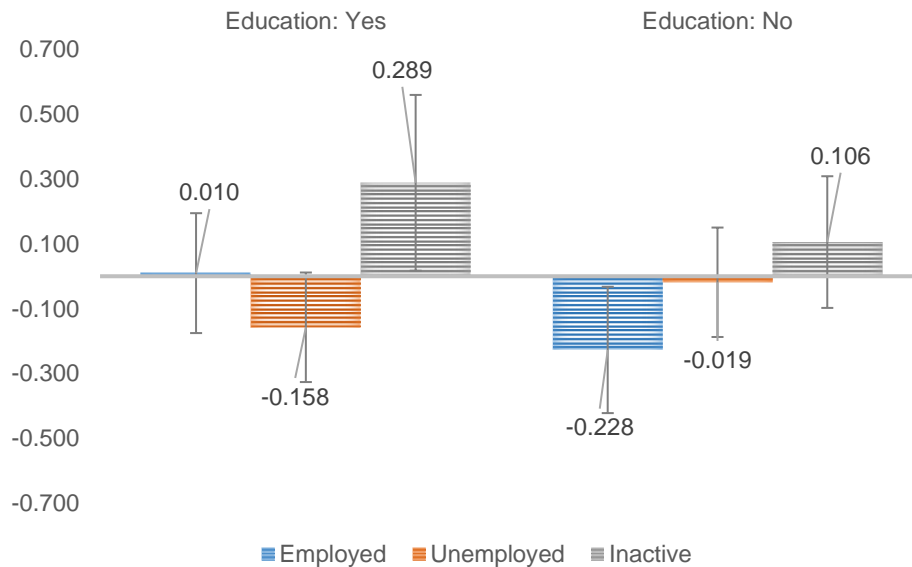
Note: Robust standard errors below impact estimates; \*/\*\*/\*\* statistically significant at 90/95/99% confidence level.

**Figure 9: Labour market status and education: bottom 50 per cent of assets**



Note: N=436; 90 per cent confidence intervals based on robust standard errors plotted together with impact estimates; education refers to current enrolment in a secondary school, vocational school or university.

**Figure 10: Labour market status and education: top 50 per cent of assets**



Note: N=435; 90 per cent confidence intervals based on robust standard errors plotted together with impact estimates; education refers to current enrolment in a secondary school, vocational school or university.

Taken together, the findings on education and labour market activity suggest that the ‘100 Hours to Success’ training drew participants towards remaining in and focusing on formal education before entering the labour market and trying to secure employment. These tendencies seem to be concentrated almost entirely among men, older participants and individuals from more affluent households.

## 8. Discussion

### 8.1 Review and discussion of outcomes

Overall, the results we have presented so far suggest that ‘100 Hours to Success’ training affected participants along several dimensions. The training has a strong positive and highly significant impact of 27 percentage points on participants’ likelihood to maintain a savings account for more than two years after the end of intervention. This effect is consistent across gender, age groups and different household asset levels. For women, the effect on maintaining a savings account is stronger (32 percentage points) than for men (21 percentage points). This suggests that women without exposure to the training are less inclined to open and maintain an independent savings account. Our findings also suggest that older individuals benefited from the financial knowledge and awareness training, showing a pronounced and significant impact of 0.6 standard deviations on the financial literacy index. Perhaps this age group has a stronger need to learn to achieve financial independence through financial management. There is weak evidence that financial literacy also improved – to a smaller extent – for men and youth from more affluent households.

**Financial behaviour:** There is no evidence that the effects on maintaining a savings account and financial literacy translate into impacts on actual savings behaviour, nor does the report find any statistically significant increase in self-reported use of a budget in maintaining personal finances. We do find that youth from more affluent households are more likely to have borrowed since the start of the programme, perhaps encouraged to see borrowing as a viable option to leverage future earnings and attain financial goals. And while youth from less affluent households may also have been encouraged to seek out loans, they may have met external barriers and found that they lack the collateral or reputational credit to secure initial loans from formal financial institutions. Some MFIs in Morocco accept savings as collateral if those savings reach a certain percentage of the loan; this should make such loans more accessible to those from less affluent households. But if youth are not saving (or able to save), then this requirement remains a barrier to borrowing. While our results are merely suggestive, these potential barriers to accessing microfinance for youth from poorer backgrounds are worthy of additional investigation. Alternatively, it may be that these young people have decided that loans are not appropriate for them at this time and they have chosen not to indebt themselves.

**Life skills:** We found no evidence of long-term effects on participants’ self-efficacy and self-reported capacities for leadership, teamwork, problem solving and willingness to take risks. The timing of the follow-up survey, which was administered a year later than initially planned, is likely relevant. It is plausible that participants experience a buoying effect immediately after participating in a programme designed to help them feel empowered and emboldened, whether in regard to their job search or their engagement in the community. It is also likely that such effects are temporary in nature. If our follow-up survey had been delivered according to the original timeline – one year after programme delivery – we may have more readily observed this

effect. But with our follow-up survey implemented some three years after the baseline survey, any such effect is likely to have dissipated or been overcome by other factors of influence, including the struggle to secure employment or achieve other long-term goals.

We should also note that life skills have been a general focus of private sector concerns around hiring youth. So focusing training programmes on life skills and entrepreneurship training (which often promotes life skills) is a logical approach for organisations like MEDA, seeking to empower youth as economic actors. MEDA designed its curriculum in coordination with representatives of both the private sector and youth to develop a demand-driven intervention.

**Employment:** Employers' hiring decisions depend on a number of factors over which MEDA and other youth-serving organisations have no control, such as how rigid hiring and firing policies are and overall labour demand. This is surely important when interpreting our findings for employment, where we document one fairly strong and consistent effect, which is that men, older participants and individuals from more affluent households are more inclined to stay outside the labour force, having neither secured employment, nor actively been seeking work.<sup>32</sup> Women, younger individuals and persons from less well-off backgrounds show a tendency for increased unemployment rates at the expense of reduced employment rates. These effects, however, generally fail to reach conventional levels of statistical significance.

To shed light on these counterintuitive results, we combine labour market status with an indicator for current enrolment in education (or training). This reveals a striking pattern for male, older and programme participants from more affluent households. The pronounced drop in labour market activity in these subgroups is combined with a prolonged stay in education. In fact, we find that these subgroups are both more likely to remain longer in education and, if still in education, less likely to look for a job or be employed.

While we have little data available on the exact reasons for these outcomes, this behaviour seems consistent with individuals investing more in education, not only through longer attendance, but also by devoting less time to labour market activity. There is some rationale for considering that exposure to the '100 Hours to Success' training led some in these subgroups to consider that further investment in education would be preferable to entering a difficult, largely informal labour market to help them meet long-term goals. Our data for women, younger and less affluent programme participants suggests that training participation had little effect on labour market outcomes and decisions to pursue further education. Here, the much reduced power of the study proves a considerable drawback: even though impact estimates sometimes reach a substantial magnitude, they come at the same time with large confidence intervals and statistically insignificant estimates.

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<sup>32</sup> We must bear in mind that our results are derived by splitting the sample into women and men; younger and older; and more and less affluent individuals. Due to a lack of observations (power) we do not restrict our analysis to individuals who combine several of those characteristics – for example, women from more affluent households.

Any interpretation of these results must take into account data availability and the study's final sample size. Although the original study design allowed for over 1,800 respondents, we had a much reduced final sample due to an attrition rate of just over 50 per cent. This limits our ability to elaborate on the nuances of financial behaviour, self-efficacy and labour market outcomes. Non-compliance among some of the treated population further diluted the power of our study. We deal with the influence of these factors on our results and issues related to internal validity in Section 8.2. We also draw important lessons around the nuances of studying youth populations in Section 9.

Finally, in the context of the results presented above, it is perhaps important to step back and review the experience of participants in the '100 Hours to Success' training as a whole, highlighting participants' reactions and views on the impact of the programme. Importantly, when asked whether they would recommend the training to a friend, 99.5 per cent reported that they would. This is a fairly strong proxy for how worthwhile they considered programme participation to be. Nearly 85% of the treated population also reported that participating in '100 Hours to Success' influenced their decisions around personal investments in education; 77% reported that participating influenced their choices about employment. So, although measured impacts on long-term outcomes for financial behaviour, self-efficacy and life skills, and employment are mixed, participants felt that the programme was beneficial to them overall and helped them in their transition to adulthood, whether with educational or employment choices.

The results of the qualitative study that took place in parallel with this evaluation reinforced this impression of a positive learning environment (see Section 5.5). Our focus group participants felt that they had gained many useful skills for everyday situations, including:

- **Life skills:** Self-confidence, active listening, persistence, dealing with criticism, working with groups, how to succeed in an interview and how to present themselves to an employer;
- **Financial behaviour:** Lessons about saving, using debit cards and cheques, how to open lines of credit and obtain loans. Those who had not made active use of savings accounts noted that they planned to apply their skills for saving once they had more income; and
- **Work and self-employment:** Those who were employed learned how to deal with clients and family members they work with and said that the job-searching skills they learned had helped them to secure their current positions.

## 8.2 Internal validity

Throughout this study, we have highlighted concerns around compliance and attrition. In this section, we explore the effect of these concerns on the internal validity of our findings in more detail, report the results of a number of robustness checks, and address issues related to John Henry effects, Hawthorne effects and other sources of potential bias.

### 8.2.1 Non-compliance with treatment assignment

We highlighted the fairly high rate (around 62 per cent) of non-compliance among those assigned to treatment (see Table 2). This is driven by two groups: individuals who never showed up to the training, despite being assigned to the treatment group, and study participants who signed up for the intervention but did not attend enough classes to be considered as treated (dropouts).

We note that the scheduling of the intervention within the context of the study – given requirements for monitoring adherence to the randomised sorting – may have delayed the rollout of particular training sessions, leading to some individuals dropping the programme from consideration. We also found that some of those assigned to the treatment group failed to enrol in ‘100 Hours to Success’ after finding that their friends had been randomly sorted into the control group and were not eligible for training. This was a circumstance we had not foreseen in our original modelling of the randomised selection. It is also important to mention that, while MEDA strongly encouraged prospective participants to open a bank account, at no point was this a criterion that barred individuals from starting the programme.

These non-compliance issues pose concerns for the rollout of interventions like ‘100 Hours to Success’ and the structure of their evaluations. In this study, we try to address non-compliance by presenting estimates for three different effect types:

- ATT, often considered as most relevant to policymakers, but in our context prone to self-selection bias when estimated by OLS
- ITT, which describes the average effect on the group that was randomly assigned to treatment
- LATE, which allows us (at best) to obtain unbiased causal estimates for the so-called group of compliers.

The compliers are individuals who did or did not participate in the training precisely because they were randomly allocated to the treatment or control groups. Because non-compliance in this study almost exclusively occurs among individuals of the treatment group, we can for all practical purposes interpret LATE coefficients as ATT.<sup>33</sup> We should also keep in mind that individuals had to attend at least 50 per cent of classes offered to them to be considered as treated.<sup>34</sup>

LATEs attribute all observed differences between individuals assigned to the control and treatment groups to those that are considered treated. This includes any potential programme effect from youth who participated in a few training classes, but not enough to qualify as treated. So LATE estimates should be regarded as an upper bound for ATTs.<sup>35</sup>

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<sup>33</sup> For more details, please see Section 7.2.

<sup>34</sup> Of 234 youth who attended at least one class, only 176 fulfilled this criterion (see Table 1).

<sup>35</sup> This depends on whether treatment effects are proportional to class attendance. Dropping out after attending a few classes could also be interpreted as a lack of interest and/or (a perceived) lack of ability to benefit from the training content. In this case, we would expect the

Three interesting insights emerge when comparing ATT estimates, obtained through OLS, with LATE coefficients, estimated through an IV approach (see Tables 7, 11 and 12, columns 1 vs 3):

- The results of both approaches qualitatively point in the same direction: OLS and IV estimates always show the same sign for variables where at least one coefficient reaches statistical significance;
- In almost all of these cases, OLS estimates indicate a more positive impact than the IV approach, which could be explained by a selection bias in programme take-up: it seems that individuals who would have had better outcomes in financial literacy and behaviour, life skills and labour market status anyway were more likely to sign up for and complete the '100 Hours to Success' training; and<sup>36</sup>
- While LATE coefficients seem more reliable in terms of bias, the IV approach comes at the cost of reduced power. Intuitively, this is because our statistical design now focuses on exploiting variation among compliers.<sup>37</sup> We observe that standard errors of impact estimates are consistently around twice as large when comparing the standard OLS estimates with the IV approach.<sup>38</sup> This implies that we only obtain statistically significant impact coefficients for considerable strong treatment effects.

### 8.2.2 Attrition

Attrition is always a concern for an evaluation's internal validity, especially when attrition rates are as high as those we observe here. It is even more worrisome when we see differential attrition, which is not balanced between the control and treatment groups. In this case, attrition might invalidate the randomised research design by making treatment and control groups incomparable, rendering the identification of the true intervention effects impossible. The results we present in Section 7 are encouraging in that there seem to be no observable differences at baseline between the participant and comparison groups when restricting the sample to those who were observed twice (N=871). We document that differential attrition between treatment and control group is small (<1 percentage point) and not statistically significant (see Table 6).

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training to have very limited or no impact for those below the threshold of attending at least 50 per cent of the classes.

<sup>36</sup> The only exception is the savings account indicator. Given the design of the intervention, it seems plausible that many of the individuals who received only very few training sessions and are therefore considered as not treated did open a bank account, which leads in this exceptional case to a downward bias in the OLS estimate. See Section 7.4.1 for a more detailed discussion.

<sup>37</sup> Still, for the analysis we use all available observations. Note that, when estimating LATEs, it is impossible to tell whether an individual participant qualifies as a complier. If they were assigned to the treatment (control) group and did (not) receive the training, they might as well be an always-taker or never-taker. But we did identify averages for compliers, always-takers and never-takers.

<sup>38</sup> To see this, compare the standard errors of the (likely biased) ATT estimates from column 1 to the standard error of the LATE estimate in column 3 in Tables 7, 11 and 12.

In this section, we further investigate whether individuals who attrited from the sample differ from those who were observed twice (baseline and follow-up), based on suggestions found through literature review.<sup>39</sup> We explored two ways of doing this – producing balancing tables that compare control variables and outcome variables between both groups one-by-one or estimating a multivariate regression. But, while the former method offers variables that can predict attrition, it does not take into account potential correlations between these variables. So we focused on the latter approach of estimating a multivariate regression to predict attrition by taking into account all included variables simultaneously.<sup>40</sup>

Table 16 displays the results of regressing a dummy, wherein all individuals who were observed twice (non-attriters on assignment) receive the value 1. It is particularly worrisome if attrition correlates with assignment status, as this might indicate that the treatment itself changed individuals' propensity to leave the study. But we found no relationship between assignment to the treatment or comparison group and the probability of being interviewed in the follow-up survey. This holds true in a simple linear probability model (column 1) and when subsequently adding controls (column 2). The fact that there are four variables (being female, living in an urban area, living in a dormitory and father being alive) that are correlated with whether a study participant could be interviewed during the follow-up survey does not generally affect the study's internal validity. 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. As a result, the sample observed at the follow-up is no longer fully comparable with the baseline sample.

But this only leads to biased estimates if the propensity to leave the study differs between treatment and control groups. So in columns 3 and 4, we present results for specifications including interaction terms between those four variables that are informative in predicting attrition and assignment status.<sup>41</sup> We find that female participants who were part of the treatment group are not significantly less likely to be observed during the follow-up compared with women that were assigned to the control group. In fact, the only interaction term that is significant at the 10 per cent level relates to individuals assigned to the treatment group who live in urban areas. The coefficients in column 4 imply that (only) study participants in the control group who lived in urban areas were less likely to be interviewed during the follow-up survey. With this one exception, we did not find further indications that selective attrition might have affected the internal validity of the study.

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<sup>39</sup> See Duflo, Glennerster and Kremer (2008).

<sup>40</sup> See Appendix E for balancing tables that compare individuals who were observed twice (non-attriters) with those who were only interviewed for the baseline survey (attriters) – Table E3 for control variables and Table E4 for outcome variables.

<sup>41</sup> The full set of control variables includes all control variables listed in Table 4 that are not already included in the specifications in columns 2 and 3. None of these additional variables show an impact that is significant at conventional confidence levels.

We attempted to correct for potential biases due to the difference in probability of being observed twice for urban study participants in the control group by using inverse probability weighting (IPW). To do this, we:

- estimate a probit model including all variables and interaction terms listed in column (4) of Table 16
- predict for every individual the probability of being included in the follow-up survey
- use the inverse of this probability as sampling weight when estimating our models, as represented by the equations (1), (2) and (3) (see Section 7.1).

Intuitively, we give more weight to individuals who have a comparably high probability of not being followed up and so are presumably more similar to persons who were not included in the follow-up survey.

**Table 16: Attrition regressions:**

	(1)	(2)	(3)	(4)
<b>Full set of controls included</b>	<b>No</b>	<b>No</b>	<b>No</b>	<b>Yes</b>
Assignment: treatment/control	<b>0.008</b> 0.024	<b>0.007</b> 0.023	<b>0.014</b> 0.095	<b>0.013</b> 0.095
Female		<b>-0.084***</b> 0.024	<b>-0.069**</b> 0.034	<b>-0.066*</b> 0.034
Urban		<b>0.064**</b> 0.027	<b>0.115***</b> 0.039	<b>0.112***</b> 0.040
Living in dormitory		<b>-0.070**</b> 0.030	<b>-0.087**</b> 0.043	<b>-0.087**</b> 0.043
Father alive		<b>-0.086**</b> 0.042	<b>-0.126**</b> 0.059	<b>-0.125**</b> 0.060
Female*Assign			<b>-0.029</b> 0.048	<b>-0.033</b> 0.048
Urban*Assign			<b>-0.099*</b> 0.055	<b>-0.096*</b> 0.055
Dormitory*Assign			<b>0.036</b> 0.060	<b>0.038</b> 0.060
(Father alive)* Assign			<b>0.083</b> 0.084	<b>0.085</b> 0.084
Constant	<b>0.479***</b> 0.018	<b>0.568***</b> 0.049	<b>0.561***</b> 0.069	<b>0.449***</b> 0.135

Note: N=1,803; the dependent variable is a dummy that takes the value one for individuals that participated in both the baseline and the endline survey and zero for those that could not be included in the follow-up data collection, standard errors below impact estimates; \*/\*\*/\*\* statistically significant at 90/95/99% confidence level.

Tables 17, 18 and 19 contain the results reporting the normal (non-weighted) and IPW estimates for ITT and LATE. Overall, the estimates are very similar, and all results are robust to using IPW when considering statistical significance.<sup>42</sup>

<sup>42</sup> Observe that the ITT estimates in column 1 of the three tables exactly correspond to the coefficients presented in Tables 7, 11 and 12. For technical reasons, the models combining instrumental variable techniques with IPW (column 4 of Tables 17, 18 and 19) have been

**Table 17: Impact estimates for financial literacy and behaviour (standard and IPW)**

	DID	N	Control group: mean (s.d.)	(1) ITT standard	(2) ITT IPW	(3) LATE standard	(4) LATE IPW
<b>Financial literacy</b>							
Financial literacy index (0–1)	No	871	<b>0.499</b>	<b>0.016</b>	<b>0.016</b>	<b>0.042</b>	<b>0.042</b>
			0.285	0.020	0.020	0.052	0.051
<b>Financial behaviour</b>							
Has savings account (0/1)	Yes	871	<b>0.356</b>	<b>0.102***</b>	<b>0.105***</b>	<b>0.271**</b>	<b>0.274**</b>
			0.479	0.039	0.040	0.111	0.110
Saves (0/1)	Yes	871	<b>0.379</b>	<b>-0.021</b>	<b>-0.017</b>	<b>-0.064</b>	<b>-0.054</b>
			0.486	0.044	0.044	0.122	0.121
Participates in savings group (0/1)	No	871	<b>0.035</b>	<b>-0.007</b>	<b>-0.002</b>	<b>-0.018</b>	<b>-0.006</b>
			0.184	0.013	0.012	0.033	0.032
Maintains a budget (0/1)	No	871	<b>0.478</b>	<b>-0.019</b>	<b>-0.018</b>	<b>-0.051</b>	<b>-0.046</b>
			0.500	0.034	0.034	0.089	0.087
Borrowed since Oct 2012 (0/1)	No	871	<b>0.124</b>	<b>0.013</b>	<b>0.010</b>	<b>0.035</b>	<b>0.026</b>
			0.330	0.023	0.023	0.060	0.058

Note: Robust standard errors below impact estimates; \*/\*\*/\*\* statistically significant at 90/95/99% confidence level.

estimated without including individual fixed effects (but including all time-varying and time-invariant control variables described in Section 7). To allow direct comparability, the coefficients displayed in column 3 therefore also omit individual fixed effects. Differences are marginal and for all practical purposes negligible.

**Table 18: Impact estimates for community engagement and life skills (standard and IPW)**

	DID	N	Control group: mean (s.d.)	(1) ITT standard	(2) ITT IPW	(3) LATE standard	(4) LATE IPW
<b>Community engagement</b>							
Ever volunteered (0/1)	Yes	871	<b>0.152</b>	0.017	0.015	<b>0.050</b>	<b>0.043</b>
			0.360	0.032	0.032	0.092	0.090
Satisfied with role in community (15)	Yes	871	<b>2.520</b>	-0.056	-0.053	<b>-0.138</b>	<b>-0.127</b>
			0.733	0.067	0.068	0.183	0.183
Community problem-solving scale (0–4)	No	870	<b>3.366</b>	-0.105**	-0.106**	<b>-0.276**</b>	<b>-0.272**</b>
			0.842	0.056	0.057	0.147	0.146
<b>Life skills</b>							
Risk scale (1–5)	No	871	<b>3.338</b>	0.031	0.028	<b>0.083</b>	<b>0.073</b>
			0.486	0.034	0.034	0.089	0.086
GRIT scale (1–5)	No	871	<b>2.224</b>	0.007	0.008	<b>0.018</b>	<b>0.021</b>
			0.500	0.033	0.033	0.086	0.083
Rosenberg’s self-esteem scale (0–30)	No	870	<b>20.850</b>	0.031	0.028	<b>0.083</b>	<b>0.073</b>
			3.173	0.034	0.034	0.089	0.086

Note: Robust standard errors below impact estimates; \*/\*\*/\*\* statistically significant at 90/95/99% confidence level.

**Table 19: Impact estimates for education and employment outcomes (standard and IPW)**

	DID	N	Control group: mean (s.d.)	(1) ITT standard	(2) ITT IPW	(3) LATE Standard	(4) LATE IPW
<b>Education</b>							
Currently enrolled (0/1)	Yes	871	0.513	0.016	0.008	0.042	0.020
			0.500	0.036	0.037	0.103	0.103
Enrolment level (0–4)	Yes	871	1.862	0.078	0.060	0.217	0.158
			1.874	0.139	0.142	0.392	0.392
Attainment level (1–6)	Yes	871	3.974	0.028	0.022	0.081	0.062
			1.006	0.071	0.071	0.230	0.227
Aspiration level (1–6)	Yes	869	4.431	0.018	0.043	-0.031	-0.021
			1.545	0.107	0.109	0.322	0.314
<b>Labour market</b>							
NEET (0/1)	Yes	871	0.290	0.034	0.040	0.090	0.106
			0.454	0.035	0.035	0.094	0.095
Employed (0/1)	Yes	871	0.286	-0.063*	-0.064**	-0.172**	-0.169**
			0.452	0.034	0.032	0.086	0.080
Unemployed (0/1)	Yes	871	0.180	-0.007	-0.004	-0.016	-0.009
			0.385	0.031	0.032	0.083	0.084
Inactive (0/1)	Yes	871	0.534	0.070*	0.068*	0.189*	0.178*
			0.499	0.039	0.039	0.105	0.102
Any work exp. (0/1)	Yes	871	0.665	-0.018	-0.019	-0.045	-0.045
			0.473	0.040	0.040	0.115	0.114
Reservation wage (MAD)	Yes	871	3,652	88	74	326	268
			1,852	150	154	424	420
Number of jobs so far	No	871	1.007	-0.065	-0.064	-0.173	-0.165
			1.388	0.091	0.086	0.235	0.220
Number of months worked	No	790	12.000	-1.704	-1.630	-4.488	-4.201
			22.001	1.490	1.378	3.873	3.510

Note: Robust standard errors below impact estimates; \*/\*\*/\*\* statistically significant at 90/95/99% confidence level.

Next, we carry out a second robustness check to evaluate the extent to which attrition might have invalidated our study. As mentioned in Section 7.2, we obtained proxy data for 200 additional study participants by interviewing a household member,

neighbour or friend. These proxy data contain answers to a few simple questions about that respondent's current educational and employment status.<sup>43</sup>

From this information, we know whether study participants were still enrolled in an educational programme and their labour market status (NEET, employed, unemployed or inactive). We add these observations to the sample of 871 individuals we have analysed so far. Comparing the results from the smaller sample (individuals followed up in person) and the larger sample (including the 200 individuals not observed in person) can be seen as a further test of attrition. Considerably different results would be an indication that attrition from the study would be correlated with outcomes and treatment assignment; and that adding some of the otherwise unobserved participants would bring our estimates closer to impact estimates for the whole sample. As Table 20 reveals, this is not the case. Impact estimates from the normal sample are close to those we obtained using the enlarged sample. This holds not only for LATE estimates, but also for the ITT specification (see Table E8, Appendix E). Figure E1 (Appendix E) – which plots LATE impact estimates for a combination of labour market status and education – shows results very similar to those we presented in Section 7 (Figure 4).

Summing up the discussion about the attrition rate in the follow-up survey and its implications for this study's internal validity, we need to remember five points:

1. 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, we do not observe overall differential attrition rates in the treatment and comparison groups.
2. Our remaining sample of 871 looks balanced with respect to control and outcome variables for the baseline survey. This is reassuring, but we should take it with a grain of salt because the small sample we are left with makes it difficult to identify differences precisely.
3. Comparing our remaining sample with those who could not be observed for the follow-up survey, we observe some stark differences with respect to observable characteristics. With the exception of urban youth, this does not correlate with treatment assignment. Our results are robust when correcting observable differences in our remaining sample through inverse probability weighting.
4. Analysing a larger sample that includes proxy data for an additional 200 individuals, which reduces attrition from 52% to 41%, does not alter the impact estimates in a meaningful way.
5. We are aware that our analysis cannot take unobservable factors into account that might have influenced attrition rates selectively.<sup>44</sup>

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<sup>43</sup> See discussion in Section 8 for how these data were obtained, screened and checked for quality and reliability.

<sup>44</sup> Recall that attrition overall is balanced between those assigned to the control and treatment groups. 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

In summary, we do not find convincing evidence that the high rate of attrition systematically biases the obtained results, but attrition still represents a significant challenge for this study. Together with the considerable degree of non-compliance, it increases the statistical uncertainty of our estimates and severely constrains our efforts to disaggregate findings by relevant socio-economic, demographic and geographical subgroups.

**Table 20: Impact estimates of intervention, including proxy data (normal and large samples)**

	NORMAL SAMPLE			LARGE SAMPLE		
	N	Control group: mean (s.d.)	LATE	N	Control group: mean (s.d.)	LATE
<b>Education</b>						
Currently enrolled (0/1)	871	0.513	0.044	1,071	0.463	0.082
		0.500	0.095		0.499	0.090
<b>Labour market</b>						
NEET (0/1)	871	0.290	0.086	1,071	0.315	0.075
		0.454	0.092		0.465	0.087
Employed (0/1)	871	0.286	-0.170*	1,071	0.266	-0.132
		0.452	0.088		0.442	0.081
Unemployed (0/1)	871	0.180	-0.017	1,071	0.185	-0.042
		0.385	0.082		0.389	0.077
Inactive (0/1)	871	0.534	0.188*	1,071	0.545	0.173*
		0.499	0.101		0.498	0.096

Note: Robust standard errors below impact estimates; \*/\*\*/\*\* statistically significant at 90/95/99% confidence level.

### 8.2.3 Other risks to internal validity

In interpreting our results, we are mindful of the potential influence of other sources of introduced bias, including spillover, Hawthorne and John Henry effects. But overall, we find little reason to believe that such factors would have a significant effect on outcomes, given that incentives behind such potential biases are low in the context of '100 Hours to Success'.

**Spillover effects:** We note that the '100 Hours to Success' intervention was delivered within fairly small, tightly knit communities. And with members of our treatment and control groups living in the same neighbourhoods, attending the same youth centres, often being friends or even siblings, we assume that the treatment and control groups would have shared some knowledge, potentially in an informal manner and without context. Given that the '100 Hours to Success' curriculum

assigned to treatment (relative to the attrition rate in the control group). And the outcomes of these two subgroups would have to be negatively correlated with each other. An encouraging sign that this might not be of major importance is that attrition does not differ between those who received some treatment and those who did not (see Table 6).

depends heavily on role play and experiential learning, we also posit that any spillovers would have a minor effect in terms of actual behaviours or sustained knowledge after the intervention.

**Hawthorne effects:** Where respondents modify or improve an aspect of their behaviour in response to being observed. In particular, Hawthorne effects might influence responses to some of the household income questions and self-reported capacities on life skills and self-efficacy issues. A contributor to these effects may have been the enumerators engaged in both the baseline and follow-up surveys, given that they were by and large from the respondents' peer group. But importantly, given the random sorting of respondents, any bias in this regard should be equal in both the treatment and control groups.

**John Henry effects:** When members of the control group perceive themselves at a disadvantage to the treatment group and work harder to overcome the perceived deficiency. We do not think that the weight of the intervention or the perceived loss of opportunity from being placed in the control group had such an influence on the mindset of respondents that it introduced any significant biases in terms of follow-up outcomes, particularly given that follow-up was nearly three years after treatment assignment.

### **8.3 External validity**

Equally important to the consideration of internal validity issues are the factors we need to take into account when drawing lessons from this study for a larger context. We focused on the external validity of the study, both in the larger Moroccan context and within broader international efforts to inform solutions for youth economic inclusion. The '100 Hours to Success' training was developed and delivered to fulfil the particular needs of the youth population in Oujda and Morocco's Oriental region while meeting a variety of identified needs around financial literacy and behaviour, self-efficacy and support for youth in their efforts to secure gainful employment. It was a fairly simple intervention that broadly reflects many of the supply-side interventions being deployed for young people in Morocco and globally, and it would make a natural component of many youth-oriented interventions. The relevant external validity issues here are the scale of the economic challenges facing a particular youth population and the broader institutional context in which they are making economic decisions.

As Salehi-Isfahani and Dhillon (2008) highlight, the normative institutional context that shapes market signals and governs youth market behaviour, financial institutions, employers and other economic actors has a dominating influence over outcomes for young people as they transition to adulthood. Where labour market regulations are particularly inflexible and raise employers' perceived risks around hiring decisions, supply-side interventions like the '100 Hours to Success' training may not have the same impact as they would in regulatory structures that enable employers to experiment more freely with new hires. In terms of financial services, banking regulations or internal institutional rules determine certain requirements such

as minimum deposits and collateral requirements.<sup>45</sup> These can pose barriers that cannot be overcome by improved understanding of financial services and facilitated introductions of clients to institutions. As such, the broader impact of relatively simple supply-side interventions that are successful in terms of knowledge generation might be limited through regulatory barriers, especially when it comes to access to finance.

Although such institutional concerns may be evident within the Moroccan context, the institutional context should be fairly consistent across the country. So we would expect similar outcomes for a training like '100 Hours to Success'. Importantly, while youth economic exclusion might be starker in Oujda than other parts of the country, if we consider the Oriental region's fairly high rates of unemployment, the macro-economic factors that affect job creation and other economic outcomes might dissipate positive economic outcomes for participants. But, broadly speaking, we find that Oujda provides a fairly representative environment within the broader Moroccan context, while institutional norms would be consistent across the nation. As such, we would expect outcomes to follow those demonstrated here throughout the country. In the MENA context, outcomes would depend, among other things, on the respective programme implementer's ability to adapt to nuances in the institutional context.

As part of the baseline report (Dyer *et al.* 2014), we assessed the extent to which the youth from our sample might be representative of youth from Morocco's Oriental region, comparing our results with findings from the 2009 World Bank survey of young people in Morocco (World Bank 2012). The World Bank study surveyed 2,000 households (1,216 urban and 784 rural) across Morocco, using a nationally representative sample, collecting more detailed data from 2,883 young people aged 15–29. This included information on economic inclusion, aspirations, notions of leisure, community participation, and access to and use of services. When comparing aggregate indicators from our survey with aggregate figures from the World Bank study for both Morocco as a whole and the Oriental region, we should bear in mind that, while national aggregates from the World Bank data are representative of the country, figures at the regional level are not necessarily representative of the Oriental sub-population, where the World Bank survey only includes 185 observations.

In regard to key demographic factors, it is noteworthy that our survey population tends to be younger than the World Bank survey population: while the span of the surveyed population is comparable, those aged 15–21 are more heavily represented in the MEDA pool of applicants. This is natural, given that applicants are – for the most part – just beginning the transition from school to work. Our sample is fairly reflective of the World Bank study's national population on gender, but more likely to include urban dwellers, better educated youth and those on the pathway to becoming better educated. But overall, we find that results from our study are generally applicable to the broader Moroccan youth population.

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<sup>45</sup> The '100 Hours to Success' financial education module was designed to improve youth knowledge and financial decision-making, including whether or how to take a loan or open a savings account. Alongside this, the YouthInvest project worked with partners to change collateral requirements for youth loans, broadening access to youth who completed a version of '100 Hours to Success'. But this aspect of the YouthInvest project lay outside the bounds of our evaluation.

## 9. Specific findings for policy and practice

In the context of global concerns regarding economic outcomes for youth, identifying interventions that can successfully increase the inclusion of youth in economic markets is a mounting priority. In this context, this study makes a number of important contributions to the global evidence base on skills training, youth employment and financial inclusion. These are particularly relevant for the Moroccan context and the MENA region as a whole.

1. The findings of this evaluation point to positive impacts on outcomes related to youth financial behaviour, shown through the establishment and maintenance of savings accounts. While increases in savings could not be demonstrated, the establishment of a savings account is a first step towards improved economic security.
2. This type of training has made participants postpone entry into the labour market in preference for continuing their post-secondary education. While this may seem a zero-sum situation, further investment in education overall is proven to lead to more promising, economically secure futures.
3. We can conclude that, while skills training may play a role in helping youth improve their own economic circumstances, the skills and knowledge gaps filled by such training are probably not the binding constraints that keep youth from securing jobs and establishing themselves as independent economic actors. Skill training can help prepare young people for job search and employment.
4. While these results may be surprising to few, this evaluation represents the first of its kind to measure the effects of skills training for a youth population in Morocco, and will therefore go a long way to opening up the way for further impact research on this important theme.

From the MEDA perspective, our outcome findings – particularly those related to employment – may not come as a surprise. In the broader context of the YouthInvest project, MEDA saw its expected outcomes of improved financial behaviour and access to services and improved job prospects as the culmination of a mix of youth services. These would include training for youth as well as partnerships with financial institutions, designed not only to facilitate access to financial services for youth but to provide technical support to such institutions. Still, ‘100 Hours to Success’ provided a unique intervention offering youth a mix of training, which covered life skills, entrepreneurship and financial literacy.

Even with carefully noted caveats about the potential impact of ‘100 Hours to Success’ outside the broader context of MEDA’s YouthInvest project, we feel that there are relevant lessons from the implementation of the intervention for programme implementers, policymakers and researchers. We conclude our report with specific recommendations on each of these fronts.

## 9.1 MEDA and other programme implementers

From the perspective of our findings, financial literacy training and the promotion of saving, as delivered through MEDA's '100 Hours to Success', encourage greater youth inclusion in the financial system. Such interventions are a first step to engaging youth in the financial system in places where so many remain unbanked, and we need to understand better how these first steps – training, opening a savings account – can lead to more profound behavioural changes among youth.

But even when such training does not have the immediate impact of increasing savings among participants, the knowledge they gain about the financial system and how to use it provides youth with an important foundation for the future. This can help them with future investments in education and job searching and to build collateral that can open up other financial services, including mortgage loans and business start-up loans.

The combined approach to business and life skills training provides youth with an important tool for weighing future educational and employment options. While there was little evidence of improving their economic outcomes in terms of employment, the experience and learning provided by '100 Hours to Success' seems to help youth better gauge the merits of employment and self-employment and what is required to be successful in these endeavours.

There is also some rationale for considering that their exposure to the training led many participants – particularly among the men – to consider that further investment in education is preferable to entering a difficult, largely informal labour market. Staying in education would help them meet long-term goals, with a positive impact on their future outcomes as economic actors.

Overall, careful targeting of beneficiaries is key to ensuring that participants can more directly benefit from the training. In the case of '100 Hours to Success', the targeting of beneficiaries was intentionally broad, in an effort to provide core services to a wide community of Moroccan youth. Many of MEDA's targeted population were not in a position to apply lessons learned from training directly to specific behaviours as economic actors. Given the age range and the fact that most were still enrolled in school or university during the intervention, many were not prepared to build on the programme's job skill and job-search skills.

This is also reflected in the limited impact that programme participation had on economic activity, including employment and self-employment. Entrepreneurship training poses a particular targeting challenge. As with life skills training, many organisations engage in entrepreneurship training in a broadly targeted context with the understanding that most beneficiaries will not become entrepreneurs. Still, the training provides valuable basic knowledge about how businesses and business financing work while engaging youth in an experience that teaches a broad set of life skills. MEDA's broad targeting of youth implies that, for most participants, expected outcomes followed this broader expectation. Organisations aiming to facilitate higher rates of self-employment among its participants, would need to ensure careful

targeting from the beginning. This might include using short entrepreneurship boot camps or similar activities as screening mechanisms before identifying participants for more extensive training.

Skills training like that provided through '100 Hours to Success' might provide the most impact when it is embedded in a larger programme of youth services that allow participants to apply their learning and gain relevant experience. For MEDA in particular, we are mindful of their efforts to deploy the '100 Hours to Success' training within a programmatic set of services that included on-the-job experience through internships, direct experience with financial services and – for those demonstrating the intention to start their own business – linkages to credit-focused financial services provided by partnering MFIs. This addition of services beyond training would arguably secure higher take-up of training and decrease the dropout rate. But the necessary dependence on partnerships with financial service providers, other NGOs and government agencies could prove a significant challenge and increase the service provider's costs per beneficiary, as they did with MEDA's early experience with YouthInvest in Morocco.

In terms of desired employment outcomes, we feel that youth-serving organisations may benefit from offering participants direct job experience in the form of internships and apprenticeships. While our evaluation does not provide evidence on this issue, international evidence suggests that skills training matched with practical experience is more impactful than training alone. In making this suggestion, we do not seek to overemphasise the potential impact of short internships and are mindful that, in providing firms with free labour, internships can have unintended consequences. We are also mindful of the costs and complexities (particularly in dealing with partners) that such an approach can impose on programme design and implementation.

Finally, the variability of grant support – an issue that NGOs understand well – is a pressing concern. A dependence on donors and their susceptibility to rapid shifts in priorities or project design can undermine or upset any project. In the case of MEDA's YouthInvest, changing donor priorities redirected the project's focus towards structural support for MFIs, effectively ending MEDA's ability to deliver '100 Hours to Success' in Morocco.

## **9.2 Policymakers**

For policymakers, particularly those in Morocco and the broader MENA region, long-standing developmental challenges related to growing youth populations remain a policy priority. But efforts to resolve the economic challenges facing youth have focused on immediate active labour market programmes and support for training interventions. The results of this evaluation provide further evidence that skills training needs to be part of a comprehensive policy package that addresses not only young people's skills deficits but also broader socio-economic challenges.

With this in mind, we build on our evidence to provide the following recommendations for policymakers.

1. Across Morocco and the wider MEDA region, there is a significant need for fostering youth inclusion in the financial system. We encourage policymakers to focus on and expand the provision of financial literacy training, coupled where possible with encouraging young people to establish savings accounts. Although such investments may not lead to short-term gains in youth savings, the knowledge and experience they will gain – especially those from more marginalised, unbanked families – can enable early positive financial behaviours. This can help youth to make better investments in education and job search. It also supports longer-term goals, including an improvement in access to other financial services such as mortgages and business start-up loans.
2. Providing business and life skills training within the context of other youth support services can have a positive impact on their economic well-being. Our study suggests that providing such training is beneficial to youth clients. And, although it may not produce benefits in immediate employment or self-employment gains, it does widen young people’s understanding of their own capacities and the investments they need to make to secure their economic goals, often fostering continued investments in education. In a more targeted environment, such training is likely to improve the efficacy of youth efforts to bridge the school-to-work transition or establish successful micro and small businesses, particularly when the training is matched with other services, such as microfinance lending or job experience.
3. In terms of job creation, too much dependence on supply-side interventions (like training) are unlikely to produce the long-term results sought by implementers, whether governmental or non-governmental actors. Active labour market programmes can be effective in some cases. At the same time, however, policymakers should be aware that such programmes generally do not yield the expected success rates because of barriers imposed by restrictive labour market regulations in Morocco and other MENA countries. And in the context of slow economic growth and lack of demand, the economy’s overall ability to create new jobs is limited. We therefore want to emphasise the need for policymakers to continue pushing towards more business-friendly policies to encourage job-creating economic growth.

### **9.3 Evaluators: research priorities and practical advice**

Within the context of the broader international development agenda, there is an increasing focus on evaluation of youth programmes, particularly in regard to interventions focused on youth employment and economic inclusion. Training is one of the core areas of investment, particularly in life or soft skills and entrepreneurship. This area also remains in need of evaluation research to demonstrate what approaches work (and work best) for youth as they transition to adulthood.

#### *9.3.1 Implications for future research*

In the context of continued investments in youth training, many information gaps remain around the quality of training and how best to structure it to improve its

impact. Building on our own experience and the international record, we provide evaluators with a reflection on the important needs for future research.

1. Restructuring the approach of our study could have achieved additional learning around the right mix of skills training. Given that the overall approach to '100 Hours to Success' included three components, we could have randomised treatment in a way that allowed us to assess each component, with some participants receiving no training, some receiving only one component, and others receiving the total training package. But this would have required significant changes to MEDA's training structure and placed significant impositions on the training organisations. It would also have increased sample size requirements and costs of the training rollout and the study. So, while bearing in mind the challenges that such an approach presents, we highlight the need for studies that are designed to measure various levels of training, which could capture the impact of life and soft skills training as distinct from other training.
2. Given the uncertainties around impact as it relates to training with job experience, particularly in regard to internships, we identify a need for research that focuses on training with internships, apprenticeships and other means of securing direct job experience for beneficiaries. Research on the appropriate duration and intensity of various kinds of training would also be useful.
3. Because our study found that impacts on financial literacy and maintaining savings accounts do not result in more financial activity, we recommend further research to understand how – if at all – relatively short classroom-based trainings can impact financial behaviour more effectively. Research questions could include: How can training programmes and financial institutions incentivise savings behaviour over the short term, beyond providing information about such services and encouraging youth to open accounts? How can changes in minimum initial deposit requirements or other bank requirements incentivise savings? Building on international experience, can training programmes foster savings through more innovative classroom approaches?
4. Another practical area where researchers can support youth-serving organisations – particularly those providing training – is around resolving poor take-up and high dropout rates. The experience of MEDA's '100 Hours to Success' is not unique. And while organisations tend to view the issue through a programmatic rather than a research lens, building evidence on an effective means to increase take-up and limit dropouts, through a randomised sorting of incentives for take-up or rewards for completing training, would help shape more effective programmes.
5. From a broader policy perspective, it is important to pinpoint the key constraints for a successful school-to-work transition. It would be useful to evaluate the pay-off of focusing on formal education and training instead of seeking early labour market experience. Rigorous approaches to assessing training versus labour market experience versus continued education could

provide important evidence that could impact on policymakers' focus on education and employment. Such research would naturally pose a particular challenge in study design.

### *9.3.2 Practical advice for evaluators*

Engaging youth in evaluation studies can present unique challenges around follow-up timing; mobility and tracking youth after the baseline survey; and dealing with attrition issues related to project implementation. In terms of timing, there is always a tension between securing fairly rapid results and demonstrating a programme's more long-term effects. Programme implementers need timely results that allow them to test assumptions about and improve upon a programme's approach within the project's life (and donor) cycle. Some issues may also benefit from a shorter follow-up period – for example, our evaluation might have captured more of an impact in terms of life skills and self-efficacy if we had followed a shorter follow-up schedule, while the training was still fresh in participants' minds. But undertaking a follow-up assessment too early brings with it the risk of missing longer-term impacts, which are particularly important when considering employment-related outcomes.

No matter where the decision on follow-up timing falls within a particular evaluation, it is important for evaluators to realise that final schedules may be entirely outside of their control. This is particularly so in countries like Morocco, where surveys need approval from the national statistics agency or other government bodies. In the case of our evaluation, securing government approval delayed our follow-up by eight months. While careful preparation and early engagement with government agencies may prove helpful in reducing delays, securing of approval will often be subject to unforeseen hurdles.

With regard to securing follow-up interviews with youth, our team was aware of the challenges arising from the high mobility of young people: frequent changes of address, email and phone number, render traditional contact information requests relatively ineffective. With these factors in mind, it is important to expand and update the types of contact information collected for follow-up to include social networks. Given the busy schedules youth maintain and their (increasing) resistance to answering survey questionnaires, evaluators should consider budgeting for and providing an incentive such as small cash payments or phone credits, while making sure to field-test the effectiveness of such incentives.

Finally, many of the youth who were sorted into our treatment group failed to enrol in the training because their friends were in the control group and unable to join the programme. In designing impact evaluations, evaluators should bear in mind that youth participate in programmes like this for many reasons. One important factor is the social outlet provided, and they often join to be with friends. Sorting without considering this factor can raise issues with attrition and influence a programme's demonstrated impact by biasing attrition – for example, through spillover effects. When designing an evaluation for youth, evaluators might want to consider finding creative ways to randomise by friend groups to promote take-up while providing opportunities to analyse spillovers and other concerns.

## Appendix A: MEDA's YouthInvest project and theory of change

MEDA's YouthInvest was designed to provide a combination of financial and non-financial services to underserved youth aged 15–25,<sup>46</sup> including:

- life skills, entrepreneurship and financial literacy training through '100 Hours to Success'
- computer skills training through 'Click to Success'
- civic education
- internship placements.

YouthInvest also partnered with local financial service providers to provide youth with opportunities to open savings accounts and access credit. The goal of the project was not only to provide a mixture of services to youth, but to work closely with local partners – especially financial institutions – to build their capacity to deliver and to incorporate such services into the mainstream of their portfolios.

MEDA began implementing the YouthInvest project across Morocco in 2009. Operations covered Casablanca and smaller towns and their surrounding largely rural areas in the country's northeast and southeast regions. These anchoring towns included Berkane, Boudenib, Ouarzazate, Errachidia and Oujda. To implement the project, MEDA developed training partnerships with youth service organisations across Morocco, as well as government institutions such as the INDH.

MEDA also worked with financial service providers Banque Populaire and Al Barid (the national postal banking service) to provide youth with access to savings accounts. To open a savings account, banks require an initial deposit of 100 dirhams (US\$12); the post office bank requires 20 dirhams (US\$2.40).<sup>47</sup> At first, MEDA required its training programme participants to establish savings accounts before they could enrol in other YouthInvest components, but it softened this requirement after partner financial institutions suspended an understanding with MEDA for lower initial deposits. After this, MEDA strongly encouraged, but no longer required, its programme participants to establish savings accounts.

MEDA also developed relationships with several MFIs to facilitate access to microfinance loans for young entrepreneurs. This third component of the YouthInvest project in Morocco developed more slowly than had been anticipated, and by mid-2012 MEDA had reoriented it to focus on building the capacity of MFIs rather than youth service provision. Following this significant programmatic change, capacity-

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<sup>46</sup> In the original proposal to the donor, the age range was 15 to 25, with acknowledgement that up to 10 per cent of project clients could be outside this age range. MEDA later lobbied the donor to extend the age range for financial service activities because of legal restrictions: in Morocco, the lower age limit for loans is 18; in Egypt it is 21. So YouthInvest financial activities extended up to age 30, but '100 Hours to Success' training targeted 15 to 25-year-olds.

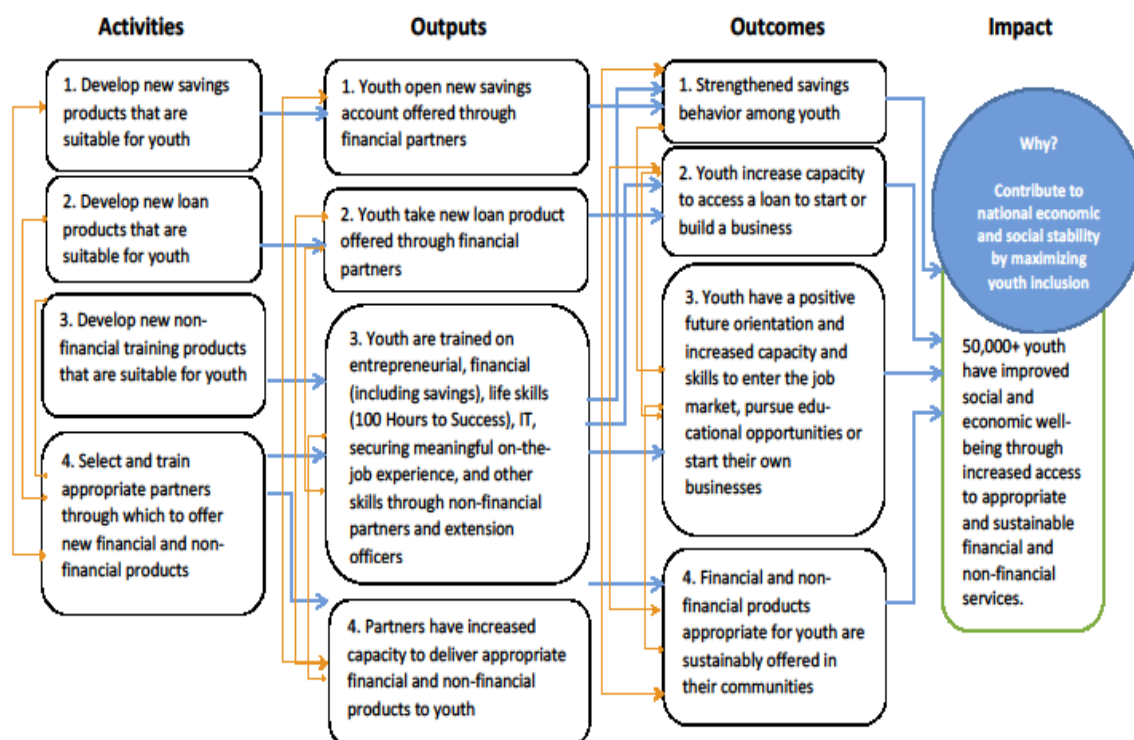
<sup>47</sup> This US\$–MAD exchange rate refers to the rate during the baseline survey period.

building activities with MFIs started in early 2013, leaving a small window to develop youth-friendly products before the project’s close in mid-2014.

MEDA also tried to match training participants with month-long internships in local firms, in coordination with the National Agency for the Promotion of Employment and Skills, ANAPEC. In the absence of an agreement with ANAPEC, MEDA sought direct agreements with Moroccan firms, asking them to commit to provide internships to trainees on the basis of identified interests and skills (as determined by local trainers). Under the plan, MEDA would provide each intern with a 500 dirham (US\$70) stipend to cover transport costs. MEDA received some initial pushback from firms, which were reluctant to make the required payments into the social insurance scheme for each intern. MEDA aimed to place 10 per cent of training participants in internships in their local communities.

It is important to highlight that, when initially conceived, ‘100 Hours to Success’ was embedded in the larger YouthInvest project. In its early stages of implementation, ‘100 Hours to Success’ gave participants an entry point into a larger range of services. MEDA also used ‘100 Hours to Success’ to screen participants interested in entrepreneurship and self-employment, steering them towards YouthInvest’s partnerships with MFIs to support business start-ups. Together, these interventions collectively aimed to provide youth with a more solid foundation of training and services to establish themselves as economic actors.

**Figure A1: Theory of change for MEDA’s YouthInvest**



Source: MEDA.

Following a mid-term evaluation in 2011, the project donor encouraged a significant programmatic shift that emphasised MEDA's institutional capacities in technical support for financial service providers and youth product development. With this shift, MEDA sharpened its focus on engaging financial service partners, increasing institutional capacity building and technical support to MFIs and their staff. To do this, MEDA had to limit its investments in workforce development activities, reducing (and eventually eliminating) direct training for youth on business skills, financial education and entrepreneurship ('100 Hours to Success'). In 2012–13, MEDA only continued with its training activities in Oujda and its rural environs, to meet its commitment to this evaluation.

## Appendix B: ‘100 Hours to Success’ training components

‘100 Hours to Success’ component	Background and purpose	Content
Business and entrepreneurial skills	<p>To develop entrepreneurial competencies and personal behaviours that facilitate the creation and the management of the new enterprises.</p> <p>MEDA selected modules first developed by Street Kids International and modified by Save the Children.</p> <p>The MEDA team adapted the materials to the Moroccan context and supplemented them with locally appropriate activities.</p>	<p>This unit has three components:</p> <p><b>What is a business?</b> Youth will be able to assess and strengthen their personal abilities with the goal of designing a small enterprise.</p> <p><b>Conducting market research:</b> Youth will be able to describe what market research entails and its importance in creating a business. They will also conduct a preliminary market study for their future enterprises.</p> <p><b>Business planning:</b> Youth will be able to evaluate costs associated with creating a business, determine profit margins and choose a pricing strategy.</p>
Financial education	<p>To provide tools with which youth can manage their money in an informed and rational way.</p> <p>MEDA staff adapted materials developed by Microfinance Opportunities, to create these modules to train youth in Morocco.</p>	<p>The unit has six modules:</p> <p><b>Budgeting:</b> “Use money wisely”.</p> <p><b>Saving:</b> “You can do it”.</p> <p><b>Debt management:</b> “Handle with care”.</p> <p><b>Bank services:</b> “Know your options”.</p> <p><b>Financial negotiations:</b> “Communicate with confidence”.</p> <p><b>Revision and evaluation:</b> Added by MEDA, this final unit allows participants to review important themes in the context of their own lives.</p>

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Life skills	Based on materials developed by the International Youth Foundation.	The unit has three components:  <b>Personal competencies:</b> Youth will learn to understand and manage emotions; develop and increase confidence in self and others; be assertive; and manage stress and life situations.  <b>Problem solving and conflict management:</b> Youth will learn techniques to manage conflicts, solve problems and gain team-building skills.  <b>Effective work habits:</b> Youth will learn tools for career assessment, job interview skills, how to be a good employee, how to manage money and time.
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## **Appendix C: Field notes and other information from formative work (in French and in English)**

### **Formation pour les Enumérateurs de l'enquête MEDA 100 Heures pour Réussir**

#### **Attributs d'un bon énumérateur**

Le champ succès énumérateur

Vous avez déjà un grand nombre des caractéristiques qui rendent un énumérateur réussi – la preuve, vous avez été embauché. Il est rare, cependant, pour quelqu'un d'être «parfait» dès le premier jour. Il devez travailler et essayer d'améliorer en continuité.

Demandez à votre superviseur, si vous voulez de l'aide ou d'encadrement supplémentaire.

#### **Caractéristiques d'un énumérateur réussi**

- Le bon sens
- La capacité de comprendre des instructions complexes
- La discipline pour travailler de façon autonome
- La capacité de convaincre et de mobiliser la coopération
- Bonne apparence et le comportement

#### **Préparez-vous pour l'entrevue**

##### 1. Organisation

- Être à l'heure!!
- Apprenez à connaître votre calendrier et tenir un journal de vos entretiens
- Gardez questionnaires propre et avoir suffisamment de fournitures

##### 2. Physiquement

- Le travail de terrain est un travail difficile!!
- Avoir suffisamment de repos. La concentration est limité quand vous êtes fatigué
- Portez des vêtements appropriés et une étiquette de nom

#### **Préparez-vous pour l'entrevue**

##### 3. Mentalement

- Être positif (nature de ce travail)
- Soyez flexible: Différentes personnes ont des idées différentes, veulent de l'information différente, et interagissent différemment
- Connaître l'objet de l'enquête (avantages pour les participants et la communauté)
- Connaître la questionnaire: se familiariser non seulement avec des questions, mais aussi avec des réponses appropriées

- **Utilisez une introduction efficace**

Une introduction à l'enquête efficace:

1. Il doit être court.
2. Inclure
  - Nom
  - Commanditaire
  - But
  - Temps nécessaire

### **Utilisez une introduction efficace**

Exemple

Un exemple d'une bonne introduction:

Bonjour / Bonjour. Mon nom est <nom>.

<Nom du répondant >? Je collecte de données pour MEDA MAROC et SOLAB. Nous faisons une enquête pour recueillir des informations sur l'employabilité des jeunes. La participation à cette enquête est entièrement volontaire. Cette enquête devrait prendre environ 45 minutes et je vais poser des questions sur vous-même, votre famille et vos activités sociales.

### **Présentation inefficaces**

Un mauvais exemple:

Bonjour. Êtes-vous <nom de l'intimé>? Je mène la <nom de l'enquête>. Avez-vous le temps de me dépanner?

### **Ecoutez, écoutez, écoutez**

Mauvaises habitudes d'écoute:

1. Pas concentré
2. Ne pas répondre à des préoccupations, parce que vous ne l'avez pas vraiment entendu l'intimé
3. Changer le sujet de discussion
4. Sentiment défensive
5. Interruption
6. À l'écoute des points de désaccord

### **Ecoutez, écoutez, écoutez**

Les bonnes habitudes d'écoute:

1. Parlez fort
2. Ecoutez jusqu'à la fin
3. Ecouter avant d'évaluer

4. Répondre aux préoccupations avec des réponses spécifiques
5. Paraphrase ce qu'il a entendu
6. Regarder indices non verbaux (distrait, effrayé, agressif, malade, en état d'ébriété)

### **Ecoutez, écoutez, écoutez**

Certaines préoccupations typiques:

1. «Qui êtes-vous?» (Vendez vous quelque chose, etc.?)
2. «Quel genre d'information que vous voulez?» (Combien de temps cela prendra? Sera t il compliqué de répondre?)
3. «Pourquoi appelez-vous pas quelqu'un d'autre?» (Pourquoi suis-je demandé de faire? Qu'est-ce que je vais sortir de ce?)

### **Comment répondre à la réticence**

- Nous avons absolument besoin un taux de réponse élevée
- Les jeunes qui seront interviewés représentent autres jeunes
- Nous ne pouvons pas trouver des substituts. Seules les jeunes qui ont postulé pour le programme sont admissibles à l'enquête

### **Comment faire face à des refus?**

- Les répondants doivent être libres de refuser à toutes les questions qui sont posées ou de participer dans l'enquête
- Le énumérateur doit toujours essayer d'obtenir une réponse / participation, sauf si l'intimé est particulièrement difficile ou agressif
- Signalez immédiatement refus de participer à votre superviseur

### **Comment faire face à des refus?**

Cette enquête ne soit pas obligatoire, mais il est préférable.

*«J'ai entendu que les enquêtes sont utilisées par le gouvernement pour lever des impôts.»*

L'information sera gardée confidentielle et utilisée seulement pour les fins de cette étude.

*«Je ne lis jamais les rapports.»*

Vous faites directement ou indirectement. Beaucoup de publicité dans les journaux, magazines, radio.

### **Comment faire face à des refus?**

*«Je suis seulement une personne. Il y a beaucoup plus de participants. Pourquoi pensez-vous contactez pas quelqu'un d'autre?»*

Vous représentez d'autres jeunes qui vous ressemblent. Comment bonnes décisions peuvent être prises sans une bonne information?

### **Comment faire face à des refus?**

- Certaines personnes ne seront pas d'accord de participer, même après nos meilleurs efforts. Ne vous découragez pas
- Laissez toujours avec une attitude positive
- Pourquoi un énumérateur devrait essayer d'obtenir la participation?
- Intimé représente beaucoup d'autres
- Des données plus précises
- Vous ne voulez pas une tendance de non participation

### **Comment faire face à des refus?**

- Devrions-nous essayer à nouveau si un participant a refusé dans le passé? OUI! Sauf refus agressif Pourquoi?
- Montrer le dévouement et la valeur
- Les attitudes des gens changent
- Une forte participation est la clé de l'étude

### **Le travail d'un enquêteur sur le terrain**

L'agent énumérateur champs est responsable des tâches suivantes:

- Mise en place des rendez-vous (??) et les garder
- Mener les entretiens en personne
- Poser des questions et écouter les réponses sans "dominer" la conversation
- Avoir un téléphone qui fonctionne
- Représenter SOLAB comme un professionnel en tout temps

### **La planification des entrevues**

- Planifier avec le participant SEULEMENT!
- Vérifier le nom de chaque répondant
- Donnez votre nom et de dire pour qui vous travaillez
- Nom de l'enquête
- Temps nécessaire pour une entrevue
- Date et l'heure annexe
- Gardez en temps le temps qu'il faut pour se déplacer
- Essayez de planifier des entrevues consécutives dans la même zone

### **Logistique du travail sur le terrain**

- Chaque énumérateur est affecté à un superviseur, et rends des rapports directement à ce superviseur. Les superviseurs sont ultimement responsables de distribuer des affectations et évaluer la performance d'un énumérateur
- Les énumérateurs sont responsables de signaler tous les incidents (peu importe leur taille) au superviseur dans les 24 heures de l'événement

### **Logistique du travail de terrain – Honnêteté**

- Les dossiers doivent être exactes. L'honnêteté est d'une importance capitale
- Enregistrer les données en temps opportun. Ne pas essayer de mémoriser des réponses
- La falsification des données est cause de licenciement immédiat
- Si vous croyez que le défendeur est falsifié réponses, signalez-le à votre superviseur

### **Logistique du travail sur le terrain – Confidentialité**

- SOLAB garantie répondants que l'information qu'ils donnent sera strictement confidentielles et ne seront utilisées qu'à des fins statistiques
- Les informations que vous collectez sera diffusée uniquement dans les formes de totaux lorsque les résultats sont publiés. Aucune information concernant une personne ou une famille sera publié ou affiché à toute personne ou organisme

### **Logistique du travail de terrain – Règles**

- Afin de protéger cette confiance du public, vous devez être d'accord avec les règles suivantes avant de commencer votre travail comme un agent énumérateur
- Les données recueillies ne doivent jamais être partagés ou discuté avec quelqu'un qui est pas un superviseur SOLAB
- Les données ne peuvent être utilisées à d'autres fins!

### **Logistique du travail de terrain – Règles**

- Les énumérateurs doivent pas discuter des données entre eux, avec les autres participants, la famille, des amis ou des collègues
- Toutes les données recueillies et des documents de l'enquête sont la propriété de SOLAB et doivent être retournés au superviseur ou le bureau SOLAB
- Aucune violation de la confidentialité ne sera tolérée. Une telle violation est une cause de licenciement immédiat

### **Logistique du travail de terrain – Règles**

- Sondages et les entrevues ne peuvent pas être déléguées à toute autre personne et doivent être remplis par le énumérateur assigné
- En travaillant sur cette enquête, un agent énumérateur doit afficher des badges d'identification appropriées. Aucune autre entreprise, l'identification religieuse ou politique peut être distribué

### **Résumé pour le interviews**

- Le énumérateur doit passer par toutes les sections du questionnaire avec le répondant

- Sans se précipiter, gardez à l'esprit du temps, car les gens obtiennent habituellement fatigué après un certain temps
- Ne pas essayer de reformuler des questions puisque vous pouvez modifier par inadvertance le sens
- Capture réponses immédiatement
- Remerciez les participants lorsque vous avez terminé

### **Interview**

Enregistrer le temps de l'entrevue

- Rédiger des commentaires sur le questionnaire
- Si nécessaire, signaler tout problème ou omissions graves à votre superviseur
- Ecrire RA en face de toute question les participants ont refusé de répondre

### **Data collection protocol for the MEDA Maroc follow-up survey and pilot survey**

1,815 sample population

#### **Available tracing data (of 1,815 sample):**

- 1,000 with addresses and phone number
  - 800 with street address
  - 200 with approximate location and proximity indicators/makers
- 200 with address only
- 300 with phone number only (no address)
- 115 with phone number of respondent and phone number of family member

#### **Tracing plan**

1. Assess quality of tracing data
  - a. Of the 1,815 respondents, tracing data should be categorised as sufficient, weak or unusable.
  - b. Tracing data categorised as sufficient means that the location address of the respondent is clear.
  - c. Tracing data categorised as weak:
    - i. No address has been given OR
    - ii. Address is unclear or incomplete
    - iii. Only phone number is present and no or unclear address.
  - d. Tracing data categorised as unusable:
    - i. No address has been given AND
    - ii. No phone number has been given OR
    - iii. Phone number is wrong or disconnected.
2. For those respondents with sufficient tracing data (with clear addresses), surveyors will call individual or family to verify address and set up meeting time.

- a. If no telephone number available OR no answer, surveyors will visit the location spontaneously and attempt survey.
    - i. If respondent is not present or unavailable, surveyor will:
      1. Confirm meeting time/availability with family member, friend or neighbour AND/OR
      2. Visit household up to four times to attempt interview
    - ii. If, after spontaneous visit, we learn that youth has moved within the Oriental region, surveyor will:
      1. Confirm new address and phone number with family member or neighbour
      2. Call and set up interview time
      3. Survey youth (up to four visits if necessary)
  - b. If, after calling, we find out that respondent has moved within Oriental region, surveyor will:
    1. Confirm address and phone number with youth during the call or by reaching family member or friend
    2. Set up time for interview
    3. Visit household up to four times to attempt interview.
  - c. If respondent has moved out of Oriental region, surveyor will:
    - i. Attempt a call survey, attempting to reach the respondent at least four times on different days.
3. For those respondents with weak tracing data, field supervisors will attempt calling the respondent.
- a. If successful, supervisor will verify address and availability for meeting; then coordinate with enumerators.
    - i. If unsuccessful, supervisor will attempt to call family members' phone numbers, if available.
    - ii. If possible, we will verify the respondent's new phone number and address, confirming phone number and address twice with family member.
  - b. We will attempt each number up to four times on two different days.
  - c. For those respondents with new verified addresses, we will try up to four visits to conduct interview.
  - d. For those respondents and/or family members who could not be reached, record and flag as unsuccessful for later follow-up.
4. Phone surveys should be attempted only in the following circumstances:
- a. Phone number has been verified.
  - b. Respondent has moved out of Oriental region.
  - c. Respondent is unavailable for face-to-face interview and/or prefers phone interview.

### **Pilot test: Oujda, Monday 27 July to Wednesday 29 July 2015**

*Pre-test:* Interviewers should go to the field to administer the full questionnaire to a small number of households (outside the study sample). This shouldn't focus on major adjustments to the questionnaire, but rather simulate the administration of the questionnaire under normal circumstances. By the end of the pilot test, the interview teams will be able to:

- correctly list, sample and interview households in the enumeration area
- understand their roles
- understand, and correctly follow interviewing protocols
- successfully collect data from 25 households (outside of the study area), which is supervised for quality without major data entry programme problems.

After the pilot survey, any changes to the questionnaire should be integrated and survey back-translated from Arabic to French for verification.

*Evaluation:* Following the pilot, data will be transmitted to the principal investigators. Interviewers and supervisors should be evaluated on their understanding of the questionnaire and their ability to record data correctly using the same test scenarios as used in classroom practice. The pilot period should only conclude once the field teams have demonstrated mastery of the designated tasks.

### **To be supplied by SOLAB**

SOLAB will supply all the data entry protocol needed, which includes:

- dataflow:
  - surveyor verification
  - supervisor verification
  - upload data to server (internet connection required – where provided?)
  - project manager verification and corrections
  - Principal Investigator verification
- range and consistency checks
- validation of data: concentration on open-ended questions
- procedure for dealing with tablet and enumeration errors
- procedures for addressing data inconsistencies/misreporting when identified
- spot-checking plans to ensure adherence to data collection protocols and confirming quality of data collection and entry, including a minimum [%] of re-visits to a random sample of the evaluation sample to confirm the validity of the data.

The following need to be requested from SOLAB:

- workplan
- data treatment plan
- back translation of survey
- pilot: data file and surveyor evaluation
- weekly data and narrative reports.

Figure C1: Data collection protocol schema

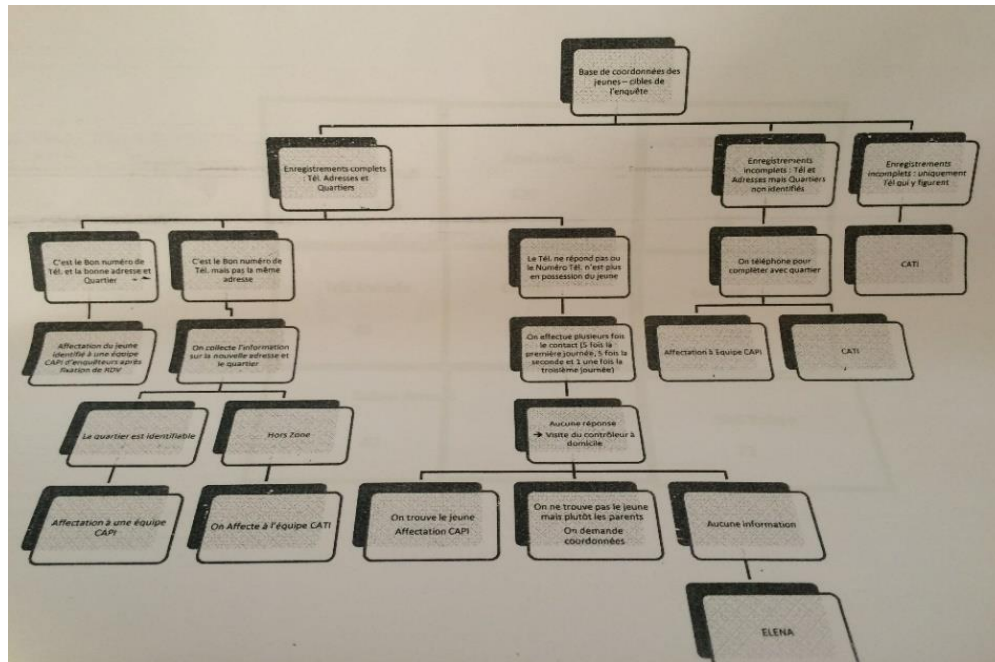


Figure C2: Survey divided by subregions/counties

Trik Al Gharb 126	Koulouch 129	Nacer 58
Trik Jerrada 48	Centre 38	Lazaret 137
Salam Armod 81	Qods 54	Sidi Yahya 71

## **Appendix D: Description of outcome and control variables from survey instrument**

This appendix includes a detailed description of all control and outcome variables we use in the main text of this report (a subset of variables recorded in the surveys). We indicate if coding differs from the labels presented in the main text and whether a variable was observed during the baseline, follow-up survey or both surveys.

### **1. Control variables**

#### **Age (both)**

Age in years

#### **Gender (both)**

1=female; 0=male

#### **Number of siblings (baseline)**

Number of brothers and sisters

#### **Urban**

1=urban; 0=rural

#### **Living in dormitory – Dormitory (baseline)**

Does individual live in student dormitory?

1=yes; 0=no

#### **Number of household members – HHSIZE (both)**

Number of persons in household

#### **Female household head – HH1GEN (baseline)**

Gender of household head

1=female; 0=male

#### **Education level household head – HH1EDUC (baseline)**

Education of household head

0=no

1=around primary

2=primary

3=primary – secondary

4=secondary

5=vocational training

6=university

**Father alive – FATHLIVE (baseline)**

Is your father still alive?

1=yes; 0=no

**Household assets – ASSETS (baseline)**

Score of 1–10, based on a principal component analysis including 23 household assets.

**Satisfied with household situation – SATISFIED (both)**

How satisfied are you with your household well-being (all the things you can buy and do)?

1=highly dissatisfied

2=dissatisfied

3=satisfied

4=very satisfied

**Attended training – TRAINING (baseline)**

With exception of MEDA, have you participated in any skills training programmes?

1=yes; 0=no

**2. Outcome variables**

**2.1 Financial literacy and behaviour**

**Financial literacy index (FI):**

$$FI_i = \frac{1}{J} \sum_{j=1}^J ITEM_{ij}, j \in \{SAVINGINST, LOANID, MEDABUDGET, MEDAINSTALL, MEDASAVCURR, MEDABANKRISK\}$$

*Self-assessed knowledge questions:*

**Two saving institutions – SAVINGINST (follow-up)**

Do you know at least two local institutions that offer savings services?

1=yes; 0=no

**Two lending institutions – LOANID (follow-up)**

Can you identify at least two institutions or associations that provide loans in your area?

1=yes; 0=no

*True/false knowledge questions:*

**Budget useful tool – MEDABUDGET (follow-up)**

“A budget is a useful tool to achieve short-term goals.”

1=true (correct); 2=false (wrong); 3=don't know (wrong)

**Paying back loans – MEDAINSTALL (follow-up)**

“When taking on a loan, you must pay the loan instalments when you have enough money.”

1=true (wrong); 2=false (correct); 3=don't know (wrong)

**Interest savings accounts – MEDASAVCURR (follow-up)**

“Savings accounts offer greater benefit from interest than current accounts.”

1=true (correct); 2=false (wrong); 3=don't know (wrong)

**Banks facing risk – MEDABANKRISK (follow-up)**

“Banks do not face any risk in lending the money that they receive from deposits.”

1=true (wrong); 2=false (correct); 3=don't know (wrong)

*Questions with yes/no answers:*

**Has savings account – SAVINGSACCT (both)**

Do you have a savings account?

1=yes; 0=no

**Saves – SAVINGS (both)**

Do you save money?

1=yes; 0=no

**Participates in savings group – SAVINGSGROUP (follow-up)**

Do you participate in a savings group?

1=yes; 0=no

**Maintains a budget – BUDGET (follow-up)**

Do you maintain a budget for your personal finances?

1=yes; 0=no

**Borrowed since Oct 2012 – LOANFU (follow-up)**

Since October 2012, have you borrowed any money?

1=yes; 0=no

## 2.2 Life skills and aspirations outcomes

### Ever volunteered – VOLUNTEER (both)

Do you ever volunteer your time to charities, NGOs or similar organisations?

1=yes; 0=no

### Satisfied with role in community – COMMUNSATIS (both)

How satisfied are you with your level of participation in the life of your community?

1=not at all satisfied

2=somewhat satisfied

3=satisfied

4=very satisfied

### Riskscale (follow-up)

Riskscale=(sum RISK A-RISK F)/6

Please indicate the extent to which you agree or disagree with the following statement that best reflects the way you personally feel. Please do not think too long before answering; usually your first inclination is also the best one.

1=strongly agree; 2=agree; 3=neutral; 4=disagree; 5=strongly disagree

\*5=strongly agree; 4=agree; 3=neutral; 2=disagree; 1=strongly disagree

RISK A Safety first.

RISK B I do not take risks with my health.

RISK C I prefer to avoid risks.

RISK D\* I take risks regularly.

RISK E I really dislike not knowing what is going to happen.

RISK F\* I usually view risks as a challenge.

### GRIT scale (follow-up)

GRITSCALE=(sum of GRIT-ITEMS)/12

Here are a number of statements that may or may not apply to you. For the most accurate score, when responding, think of how you compare with most people – not just the people you know well, but most people in MOROCCO. There are no right or wrong answers, so just answer honestly! All questions include the following scale:

5=very much like me; 4=mostly like me; 3=somewhat like me; 2=not much like me;

1=not at all like me

\*1=very much like me; 2=mostly like me; 3=somewhat like me; 4=not much like me,

5=not at all like me

GRIT A I overcome difficulties to meet a significant challenge.

GRIT B\* New ideas and projects sometimes distract me from previous ones.

GRIT C\* My interests vary from year to year.

GRITD	Setbacks don't discourage me.
GRITE*	I have been obsessed with a certain idea or project for a short time but later lost interest.
GRITF	I am a hard worker.
GRITG*	I often set a goal but later choose to pursue a different one.
GRITH*	I have difficulty maintaining my focus on projects that take more than a few months to complete.
GRITI	I finish whatever I begin.
GRITJ	I've achieved a goal that required years of effort.
GRITK*	My interests change often.
GRITL	I am diligent.

### Community problem-solving scale – COMPROBSCALE (follow-up)

COMPROBSCALE =(sum of items)/9

If you found out about a problem in your community that you wanted to do something about – for example, that illegal drugs were being sold near a school or high levels of lead were discovered in local drinking water – how well do you think you would be able to do each of the following?

0=I definitely can't; 1=I probably can't; 2=maybe; 3=I probably can; 4=I definitely can

COMPROBA	Create a plan to address the problem.
COMPROBB	Get other people to care about the problem.
COMPROBC	Organise and run a meeting.
COMPROBD	Express your views in front of a group of people.
COMPROBE	Identify individuals or groups who could help you with the problem.
COMPROBF	Write an opinion letter to a local newspaper.
COMPROBG	Call someone on phone that you had never met before to get their help with the problem.
COMPROBH	Contact an elected official about the problem.
COMPROBI	Organise a petition.

### Rosenberg self-esteem scale (0–30) (follow-up)

Self-esteem scale=(sum of SELFa-SELFj) – 10

Below is a list of statements dealing with your general feelings about yourself. Please indicate how strongly you agree or disagree with each statement.

1=strongly disagree; 2=disagree; 3=agree; 4=strongly agree

\*4=strongly disagree; 3=disagree; 2=agree; 1=strongly agree

SELFA	On the whole, I am satisfied with myself.
SELFB*	At times, I think I am no good at all.
SELFC	I feel that I have a number of good qualities.
SELFD	I am able to do things as well as most other people.
SELFE*	I feel I do not have much to be proud of.

SELFF*	I certainly feel useless at times.
SELFG	I feel that I'm a person of worth, at least on an equal plane with others.
SELFH*	I wish I could have more respect for myself.
SELFI*	All in all, I am inclined to feel that I am a failure.
SELFJ	I take a positive attitude towards myself.

### **2.3 Education and employment outcomes**

#### ***Education***

##### **Currently enrolled – EDUCENROLLYN (both)**

Are you currently enrolled in a secondary school, vocational school or university?

1=yes; 0=no

##### **Enrolment level – EDUCENROLL (both)**

Are you currently enrolled in a secondary school, vocational school or university?

0=no

1=college

2=lycee

3=professional

4=university

##### **Attainment level – EDUCATTAIN (both)**

1=primary

2=college (intermediate)

3=lycee (high school)

4=professional degree

5=university

6=masters or higher

##### **Education aspiration level – EDUCFUTURE (both)**

What is the highest level of education you intend to complete in the future? (Coding ensures comparability with EDUCATTAIN)

2=do not intend to finish lycee

3=lycee

5=professional institution

5=university

6=masters or higher

##### **NEET – not in employment, education or training (both)**

1=yes; 0=no

##### **EMPLOYED (both)**

Have you worked in the past seven days?

1=yes; 0=no

**UNEMPLOYED (both)**

*Individuals are considered as unemployed when they answer “yes” to both questions below:*

Have you been available to work during the past seven days?

Did you search for work in the last seven days?

1=yes; 0=no

**INACTIVE (both)**

*Individuals who are neither employed nor unemployed are considered as inactive (residual category)*

1=yes; 0=no

**Any work experience – WKEXPDUM (both)**

Does individual have any work experience?

1=yes; 0=no

**Reservation wage – SALARYMIN (both)**

What is the minimum salary you would accept for a job, below which you would rather not work?

Enter amount (monthly – MAD)

**Number of jobs so far – JOBTOT (follow-up)**

How many jobs have you had in the past (including the current job if any)?

Enter number

**Number of months worked – WKDURATIONTOT (follow-up)**

Duration of up to the last three and current jobs/self-employment.

Enter number of months

## Appendix E: Results tables referenced in text

Table E1: Balancing table, outcome variables (full sample)

	Mean full	Mean control	$\Delta$ (Treat- control)	p-value
<b>Financial behaviour</b>				
Has savings account (0/1)	0.211	0.212	-0.003	0.889
Does save (0/1)	0.484	0.501	-0.032	0.169
<b>Life skills and aspirations</b>				
Ever volunteered (0/1)	0.156	0.146	0.020	0.250
Satisfied with role in community (1–5)	2.688	2.655	0.064	0.074*
<b>Education</b>				
Currently enrolled (0/1)	0.861	0.861	0.000	0.996
Enrolment level (0–4)	2.796	2.783	0.026	0.689
Attainment level (1–6)	2.463	2.457	0.010	0.823
Aspiration level (1–6)	5.050	5.030	0.039	0.426
<b>Labour market</b>				
NEET (0/1)	0.110	0.112	-0.005	0.746
Employed (0/1)	0.075	0.067	0.016	0.199
Unemployed (0/1)	0.074	0.072	0.005	0.690
Inactive (0/1)	0.850	0.861	-0.021	0.214
Any work experience (0/1)	0.418	0.409	0.017	0.467
Reservation wage (MAD per month)	2,562	2,653	-180	0.023**

Note: \*/\*\*/\*\* statistically significant at 90/95/99% confidence level.

**Table E2: Balancing table, outcome variables (endline sample)**

	Full group: mean	Control group: mean	$\Delta$ (Treat- control)	p-value
<b>Financial behaviour</b>				
Has savings account (0/1)	0.215	0.211	0.008	0.782
Does save (0/1)	0.493	0.496	-0.008	0.819
<b>Life skills and aspirations</b>				
Ever volunteered (0/1)	0.164	0.157	0.014	0.571
Satisfied with role in community (1–5)	2.687	2.658	0.056	0.292
<b>Education</b>				
Currently enrolled (0/1)	0.886	0.890	-0.007	0.744
Enrolment level (0–4)	2.851	2.874	-0.045	0.612
Attainment level (1–6)	2.471	2.475	-0.009	0.889
Aspiration level (1–6)	5.048	5.016	0.063	0.361
<b>Labour market</b>				
NEET (0/1)	0.088	0.084	0.008	0.677
Employed (0/1)	0.070	0.066	0.009	0.613
Unemployed (0/1)	0.075	0.066	0.018	0.319
Inactive (0/1)	0.855	0.869	-0.027	0.267
Any work experience (0/1)	0.425	0.429	-0.007	0.825
Reservation wage (MAD per month)	2,607	2,741	-260	0.026**

Note: \*\*/\*\*\*\* statistically significant at 90/95/99% confidence level.

**Table E3: Balancing table (attrition) control variables (full sample)**

	<b>Full group: mean  N=1,803</b>	<b>Attrited group: mean  N=932</b>	<b>Δ (Stayed- attrited)</b>	<b>p-value</b>
Female (0/1)	0.525	0.572	-0.097	0.000***
Age	19.984	19.988	-0.009	0.946
Number of siblings	3.818	3.887	-0.143	0.150
Urban (0/1)	0.750	0.721	0.060	0.003***
Living in dormitory (0/1)	0.192	0.219	-0.055	0.003***
HH members	4.903	4.896	0.016	0.868
Female HH head (0/1)	0.119	0.113	0.014	0.372
Education level, HH head (0–6)	1.655	1.667	-0.026	0.758
Father alive (0/1)	0.915	0.927	-0.025	0.061*
HH assets (1–10)	4.024	3.994	0.061	0.152
Satisfied with HH situation (1–4)	2.863	2.863	0.001	0.982
Attended training (0/1)	0.138	0.142	-0.008	0.603

Note: \*/\*\*/\*\* statistically significant at 90/95/99% confidence level.

**Table E4: Balancing table (attrition), outcome variables (full sample)**

	Full group: mean N=1,803	Attrited group: mean N=932	$\Delta$ (Stayed- attrited)	p-value
<b>Financial behaviour</b>				
Has savings account (0/1)	0.211	0.207	0.008	0.692
Does save (0/1)	0.484	0.476	0.016	0.493
<b>Life skills and aspirations</b>				
Ever volunteered (0/1)	0.156	0.148	0.016	0.346
Satisfied with role in community (1–5)	2.688	2.689	-0.002	0.949
<b>Education</b>				
Currently enrolled (0/1)	0.861	0.837	0.049	0.002***
Enrolment level (0–4)	2.796	2.746	0.105	0.103
Attainment level (1–6)	2.463	2.455	0.016	0.731
Aspiration level (1–6)	5.050	5.051	-0.003	0.944
<b>Labour market</b>				
NEET (0/1)	0.110	0.130	-0.041	0.005***
Employed (0/1)	0.075	0.080	-0.010	0.402
Unemployed (0/1)	0.074	0.074	0.001	0.962
Inactive (0/1)	0.850	0.845	0.010	0.559
Any work experience (0/1)	0.418	0.411	0.014	0.546
Reservation wage (MAD per month)	2,562.101	2,518.956	88.533	0.266

Note: \*\*\*/\*\*\* statistically significant at 90/95/99% confidence level.

**Table E5: Impact estimates for community engagement and life skills, by gender**

	DID	WOMEN			MEN		
		N women	Control group: mean (s.d.)	LATE	N men	Control group: mean (s.d.)	LATE
<b>Community engagement</b>							
Ever volunteered (0/1)	<b>Yes</b>	<b>414</b>	<b>0.134</b>	<b>0.048</b>	<b>457</b>	<b>0.170</b>	<b>0.029</b>
			0.341	0.102		0.376	0.141
Satisfied with role in community (1–5)	<b>Yes</b>	<b>414</b>	<b>2.512</b>	<b>-0.136</b>	<b>457</b>	<b>2.528</b>	<b>-0.182</b>
			0.701	0.219		0.763	0.285
Community problem-solving scale (0–4)	<b>No</b>	<b>413</b>	<b>3.213</b>	<b>-0.227</b>	<b>457</b>	<b>3.512</b>	<b>-0.278</b>
			0.868	0.193		0.791	0.227
<b>Life skills</b>							
Risk scale (1–5)	<b>No</b>	<b>414</b>	<b>2.116</b>	<b>0.091</b>	<b>457</b>	<b>2.326</b>	<b>0.059</b>
			0.443	0.100		0.531	0.158
GRIT scale (1–5)	<b>No</b>	<b>414</b>	<b>3.344</b>	<b>0.004</b>	<b>457</b>	<b>3.333</b>	<b>0.017</b>
			0.507	0.109		0.467	0.137
Rosenberg’s self-esteem scale (0–30)	<b>No</b>	<b>413</b>	<b>21.144</b>	<b>-0.986</b>	<b>457</b>	<b>20.569</b>	<b>-0.818</b>
			3.330	0.707		2.996	0.890

Note: Robust standard errors below impact estimates; \*/\*\*/\*\* statistically significant at 90/95/99% confidence level.

**Table E6: Impact estimates for community engagement and life skills, by age at baseline**

	DID	YOUTH AGED 20+			YOUTH AGED UNDER 20		
		N aged 20+	Control group: mean (s.d.)	LATE	N under 20	Control group: mean (s.d.)	LATE
<b>Community engagement</b>							
Ever volunteered (0/1)	<b>Yes</b>	<b>481</b>	<b>0.129</b>	<b>0.099</b>	<b>390</b>	<b>0.182</b>	<b>-0.044</b>
			0.336	0.103		0.387	0.148
Satisfied with role in community (1–5)	<b>Yes</b>	<b>481</b>	<b>2.529</b>	<b>-0.192</b>	<b>390</b>	<b>2.508</b>	<b>-0.077</b>
			0.731	0.211		0.736	0.306
Community problem-solving scale (0–4)	<b>No</b>	<b>480</b>	<b>3.370</b>	<b>-0.116</b>	<b>390</b>	<b>3.360</b>	<b>-0.448</b>
			0.790	0.173		0.907	0.260
<b>Life skills</b>							
Risk scale (1-5)	<b>No</b>	<b>481</b>	<b>2.265</b>	<b>-0.010</b>	<b>390</b>	<b>2.170</b>	<b>0.211</b>
			0.497	0.112		0.501	0.146
GRIT scale (1-5)	<b>No</b>	<b>481</b>	<b>3.312</b>	<b>0.049</b>	<b>390</b>	<b>3.373</b>	<b>-0.054</b>
			0.468	0.105		0.508	0.146
Rosenberg’s self-esteem scale (0–30)	<b>No</b>	<b>481</b>	<b>20.575</b>	<b>-0.317</b>	<b>389</b>	<b>21.204</b>	<b>-1.803*</b>
			3.125	0.682		3.207	0.927

Note: Robust standard errors below impact estimates; \*/\*\*/\*\* statistically significant at 90/95/99% confidence level.

**Table E7: Impact estimates for community engagement and life skills, by household assets**

	DID	HH ASSETS BOTTOM 50%			HH ASSETS TOP 50%		
		N bottom 50% HH assets	Control group: mean (s.d.)	LATE	N top 50% HH assets	Control group: mean (s.d.)	LATE
<b>Community engagement</b>							
Ever volunteered (0/1)	<b>Yes</b>	<b>436</b>	<b>0.148</b>	<b>0.003</b>	<b>435</b>	<b>0.157</b>	<b>0.103</b>
			0.356	0.099		0.364	0.147
Satisfied with role in community (1–5)	<b>Yes</b>	<b>436</b>	<b>2.514</b>	<b>-0.111</b>	<b>435</b>	<b>2.525</b>	<b>-0.177</b>
			0.707	0.217		0.758	0.289
Community problem-solving scale (0–4)	<b>No</b>	<b>436</b>	<b>3.339</b>	<b>-0.380**</b>	<b>434</b>	<b>3.391</b>	<b>-0.166</b>
			0.848	0.187		0.838	0.228
<b>Life skills</b>							
Risk scale (1–5)	<b>No</b>	<b>436</b>	<b>2.222</b>	<b>0.192*</b>	<b>435</b>	<b>2.225</b>	<b>-0.065</b>
			0.505	0.111		0.497	0.142
GRIT scale (1–5)	<b>No</b>	<b>436</b>	<b>3.290</b>	<b>0.071</b>	<b>435</b>	<b>3.385</b>	<b>-0.075</b>
			0.465	0.102		0.503	0.143
Rosenberg’s self-esteem scale (0–30)	<b>No</b>	<b>436</b>	<b>20.424</b>	<b>-0.407</b>	<b>434</b>	<b>21.264</b>	<b>-1.352</b>
			3.225	0.693		3.073	0.898

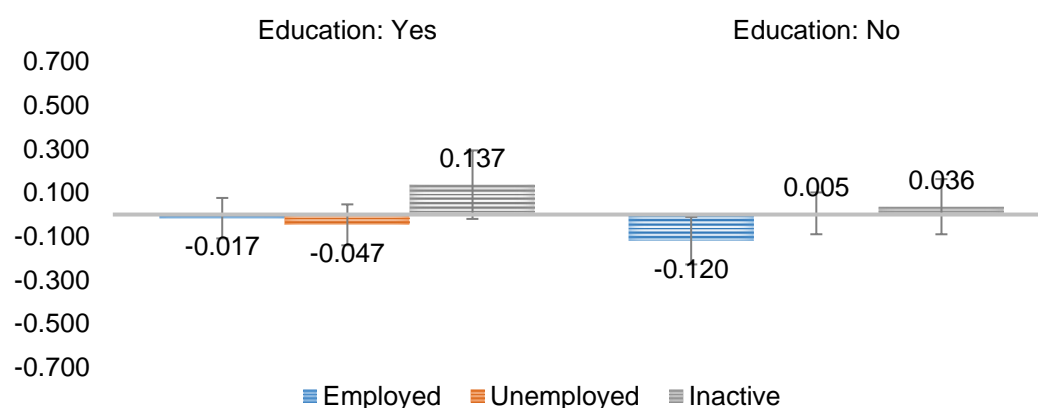
Note: Robust standard errors below impact estimates; \*/\*\*/\*\* statistically significant at 90/95/99% confidence level.

**Table E8: Impact estimates of intervention, including proxy data (normal and large samples)**

	NORMAL SAMPLE			LARGE SAMPLE		
	N normal sample	Control Group: mean (s.d.)	LATE	N large sample	Control Group: mean (s.d.)	LATE
<b>Education</b>						
Currently enrolled (0/1)	<b>871</b>	<b>0.513</b> 0.500	<b>0.017</b> 0.036	<b>1,071</b>	<b>0.463</b> 0.499	<b>0.030</b> 0.033
<b>Labour market</b>						
NEET (0/1)	<b>871</b>	<b>0.290</b> 0.454	<b>0.033</b> 0.035	<b>1,071</b>	<b>0.315</b> 0.465	<b>0.028</b> 0.032
Employed (0/1)	<b>871</b>	<b>0.286</b> 0.452	<b>-0.065*</b> 0.033	<b>1,071</b>	<b>0.266</b> 0.442	<b>-0.048</b> 0.030
Unemployed (0/1)	<b>871</b>	<b>0.180</b> 0.385	<b>-0.007</b> 0.031	<b>1,071</b>	<b>0.185</b> 0.389	<b>-0.015</b> 0.028
Inactive (0/1)	<b>871</b>	<b>0.534</b> 0.499	<b>0.071*</b> 0.039	<b>1,071</b>	<b>0.545</b> 0.498	<b>0.064*</b> 0.035

Note: Robust standard errors below impact estimates; \*/\*\*/\*\* statistically significant at 90/95/99% confidence level.

**Figure E1: Labour market status and education, large sample**



Note: N=1,071; 90 per cent confidence intervals based on robust standard errors plotted together with impact estimates.

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Development organisations in the Middle East and North Africa (MENA) region are increasingly focused on skills training and financial education to help youth find employment and improve their financial habits. This report examines the impact of one such training intervention, 100 Hours to Success, implemented by the Mennonite Economic Development Associates (MEDA) among youth aged between 15 to 25 years in Morocco's Oriental Region.

Researchers conclude that training increased the likelihood of participants maintaining a savings account for more than two years after the end of the intervention. The effects were stronger for women than for men. There was no evidence to support the assumption that maintaining an account indicated more savings. There was also no evidence of long-term effects on participants' belief in

their abilities to succeed in specific situations or accomplish specific tasks, or on their self-reported capacities with respect to leadership, team work, problem solving and willingness to take risks.

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