I Z A Institute
of Labor Economics
Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 12793

Direct and Indirect Effects of Subsidized Dual Apprenticeships

Bruno Crépon
Patrick Premand

NOVEMBER 2019

I Z A Institute
of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 12793

Direct and Indirect Effects of Subsidized Dual Apprenticeships

Bruno Crépon
Crest and IZA

Patrick Premand
World Bank and ETH Zürich

NOVEMBER 2019

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Direct and Indirect Effects of Subsidized Dual Apprenticeships*

Traditional apprenticeships based on private arrangements are widespread in developing countries. Public interventions have attempted to address failures in the apprenticeship markets to expand access or improve training quality. Subsidized dual apprenticeships have the potential to address financial constraints for youths and firms' inability to commit to provide general skill training. This paper analyzes the impact of subsidized dual apprenticeships combining on-the-job and theoretical training in Côte d'Ivoire. We set up an experiment that simultaneously randomized whether interested youths were assigned to a formal apprenticeship, and whether apprenticeship positions opened by firms were filled with formal apprentices. We document direct effects for youths and indirect effects for firms, such as whether they substitute between traditional and subsidized apprentices. In the short run, youths increase their human capital investments and we observe a net entry of apprentices into firms. Substitution effects are limited: the intervention creates 0.74 to 0.77 new position per subsidized apprentice. The subsidy offsets forgone labor earnings. Four years after the start of the experiment, treated youths perform more complex tasks and their earnings are higher by 15 percent. We conclude that subsidized dual apprenticeships expand access to training, upgrade skills and improve earnings for youths without crowding out traditional apprentices.

JEL Classification: D22, J23, J24, O12, C93

Keywords: employment, apprenticeship, wage subsidy, training, direct and indirect effects, equilibrium effects, micro and small enterprises, field experiment, Africa

Corresponding author:
Bruno Crépon
Bureau 4099
CREST
5 Avenue Henri Le Châtelier
91764 Palaiseau Cedex
France

* This paper is the result of a collaboration with the government of Côte d'Ivoire. We are particularly thankful to Adama Bamba, Hermann Toualy, Ismahel Abdoul Barry and Cesar Gbeugré Toassa at BCPE; Traoré Bamoudien and Ouattara Abdoul Kadher at AGEFOP; as well as Hamoud Wedoud Abdel Kamil at the World Bank. Funding from the Skills, DIME i2i and Jobs Umbrella Trust Funds at the World Bank, as well as the JPAL PPE initiative are gratefully acknowledged. We thank Sondo Eloi Somtinda for outstanding field coordination throughout the study, Joël Farronato for excellent baseline survey coordination, as well as Nicolo Tomaselli, Henriette Hanicotte, Dimanche Allo and Angui Kacou for leading follow-up data collection. Marina Tolchinsky, Matthew Olckers, Fatine Guedira and Eva Lestant provided excellent contributions to data analysis. Johanne Buba, Deon Filmer, Leonardo Iacovone and seminar participants in Bern, Cairo, the Bank of Italy, ETH Zürich, Florence, Geneva (Graduate Institute), ILO, Fribourg, Oxford, Lugano, Mannheim, Neuchatel, Zürich, and the World Bank provided useful comments and suggestions. All errors and omissions are our own. The findings, interpretations, and conclusions of the paper are those of the authors and do not necessarily represent the views of the World Bank or the government of Côte d'Ivoire. The original design of the experiment is filed in AEARCTR-0002726. This paper's computational reproducibility was checked by DIME analytics.

1 Introduction

Traditional apprenticeships are one of the most common types of training in the developing world. They typically involve on-the-job training in small informal firms and often last several years. In Africa, there might be up to 4 times more traditional apprentices than youths attending vocational education (Filmer et al., 2014). Most traditional apprenticeships are based on informal private arrangements between youths (or their parents) and firm owners. While traditional apprenticeships have developed over time through a market-based system with little public intervention, the optimality of the model has been questioned. The improvement of apprenticeship systems has become an important policy objective in many countries (OECD/ILO, 2017; UNESCO, 2015), for example in Africa (Walther, 2008; ILO, 2012) or Latin America (Fazio et al., 2016). Reforms are often justified on the basis that they can expand access to apprenticeship while at the same time improving training quality and returns for youths. Some policies focus on improving training quality. For instance, dual apprenticeship schemes combine practical and theoretical training. They are modeled after institutions from high-income countries, such as Germany or Switzerland. Other policies primarily seek to expand access, for instance through wage subsidies or enhanced intermediation mechanisms.

The effectiveness of public interventions in the market for apprenticeship depends on successfully addressing market failures faced by youths or firms. Several important features of the market for training have been highlighted in the literature. On the one hand, firms may not be willing to cover the costs of general training in skills that are transferable. In this case, youths have to incur general training costs as full residual claimants (Becker, 1962). On the other hand, youths may not invest in training that provides only firm-specific skills. Two potential obstacles may thus lead to under-investment in training. First, youths may be unable to pay for training. In Becker's model, the cost of training includes direct costs as well as forgone earnings while trainees are not engaged in other types of work. In traditional apprenticeships, youths pay these costs by accepting a low wage. In presence of capital market imperfections, youths incur a reduction of utility during the training period. Second, firms may be unable to credibly commit to provide general skill training in absence of complete contracts (Acemoglu and Pischke, 1998, 1999; Dustmann and Schönberg, 2012). The issue may be particularly salient in developing countries where small firms with limited

technology are common and formal contracts are rare. Youths might not invest in training by accepting a reduction of utility during training without a guarantee to learn general skills that are valuable in the labor market. Labor market frictions can mitigate this problem by allowing firms to retain trained workers at low wages. Consistent with this, traditional apprentices in Sub-Saharan Africa often spend many years in firms before being able to leave. However, as Dustmann and Schönberg (2012) emphasize, these arrangements imply that training provision remains below the optimal level. Acemoglu and Pischke (2000) show that external certification or regulatory frameworks can help workers obtain returns from general training. In addition to these issues, information asymmetries and imperfect intermediation mechanisms may also hinder matches between youths and firms (Hardy and McCasland, 2015).

In this paper, we analyze the impact of subsidized dual apprenticeships through a randomized experiment in Côte d'Ivoire. The program offered a subsidy of 30,000 FCFA per month (approximately 54 USD, or half the formal minimum wage), paid directly to apprentices for 12 or 24 months (depending on occupations). As such, the intervention tackles financial constraints for youths to invest in training. The program also included dual training, with practical on-the-job learning complemented by theoretical courses. Training curricula were developed to ensure apprentices learned general skills. A basic apprenticeship contract and certification mechanisms were put in place. The dual approach can resolve potential commitment failures in firms by ensuring that formal apprentices receive general skill training in vocational centers.

A first contribution of the paper is to document how subsidized dual apprenticeships expand youths' access to apprenticeship and training. Youths who decide to participate in formal apprenticeships may have entered traditional apprenticeships absent the program. We call this substitution effect on the youth side a *windfall* effect. In addition, we also test whether the program increases overall access to apprenticeship and the number of apprentices in firms. Fostering youths' demand for apprenticeship only expands overall access if firms are able to absorb an additional inflow of apprentices. Otherwise, subsidizing youths to enter apprenticeship might crowd out other potential apprentices. We thus also analyze the indirect impact of the program on firms to estimate potential *substitution* effects, i.e. whether firms replace traditional apprentices with formal apprentices.

To simultaneously measure direct effects among individuals and indirect (substitution) effects in firms, we set up a double-sided experiment. Specifically, we randomize whether interested youths were assigned to a formal apprenticeship, and whether apprenticeship positions opened by firms were filled with formal apprentices. A simple theoretical framework shows that this design is tailored to estimate direct impacts such as *windfall* effects for youths, as well as indirect impacts such as *substitution* effects in firms hosting apprentices. The framework also shows how those estimates combine to provide bounds for the net number of apprenticeship positions created by the program.

Results show that the program substantially expands access to apprenticeship. We collected surveys of youths and firms *during* the program. While windfall and substitution effects are significant, they are moderate in magnitude. The share of youths in formal apprenticeship increases by 71.2 percentage points, but there are significant windfall effects: 26 percent of formal apprentices substitute out of traditional apprenticeships. On the side of firms, the program leads to an inflow of 1.4 formal apprentices per firm. Substitution effects are also observed: for each formal apprentice placed, 0.23 traditional apprentices is displaced. By showing how the windfall and substitution effects combine, we find that the net number of new apprenticeship positions created is between 0.74 (1- the windfall effect) and 0.77 (1- the substitution effect) percent of the number of formal apprentices placed.

We also assess forgone earnings while youths are in the program. We show that youths forgo employment opportunities in wage and self-employment, which implies a large opportunity cost from participation in apprenticeship. The subsidy contributes to offset these losses in labor earnings, so that the program has no significant impact on youths' total earnings in the short-term. The inflow of apprentices also sheds light on mechanisms in firms. We find that firms see a substantial increase in the value of the work provided by apprentices net of their compensation during the program. Firms pay formal apprentices less than traditional apprentices, despite higher productivity. This is consistent with firms indirectly receiving compensation for the direct cost of training provision, such as the time spent teaching apprentices.

The second main contribution of this paper is to document how subsidized dual apprenticeships affect youths' skills, employment and earnings in the medium term. We collected a second youth follow-up survey four years after the start of the experiment, and approximately

two years after the end of the intervention. Besides gathering information on employment and earnings, the survey was also designed to build an index of routine task intensity for each worker, following approaches in Autor et al. (2003), Autor and Handel (2013) or Dicarlo et al. (2016). We can estimate whether participation in formal apprenticeship impacted skills by analyzing whether treated youths perform more complex tasks.

Results show substantial improvements in earnings among youths assigned to formal apprenticeship. In low-income settings such as Sub-Saharan Africa, formal unemployment is low but most individuals are employed in low-productivity occupations (Filmer et al., 2014). Echoing results in Bertrand et al. (2017) in the Côte d'Ivoire context, we find limited impacts on the number of activities or total hours worked among youths. However, we find that treated youths have higher total earnings by a significant 15 percent. We also show that youths assigned to formal apprenticeship are more involved in non routine analytical tasks. This is consistent with an improvement in skills and in average productivity. Lastly, we also observe a substantial increase in the share of youths receiving training certification, which constitutes a mechanism to signal they have acquired general skills.

Our paper contributes to several strands of research, starting with the literature on the demand for human capital investments. As discussed above, potential market failures can lead to under-investments in training, including for on-the-job training in firms. Capital market imperfections may prevent youths to cover the direct and opportunity costs of training (Becker, 1962). There might be commitment failures in firms so that youths may not be willing to pay for training without guarantees that they will acquire general skills (Acemoglu and Pischke, 1999, 1998; Dustmann and Schönberg, 2012). The program we study was designed to address these potential market failures. Subsidized dual apprenticeships provide wage subsidies to address financial constraints, as well as dual training and a basic certification scheme to ensure youth acquire general skills. Based on a randomized experiment, our results show that subsidized dual apprenticeships strongly raised youths' participation in apprenticeship. These results are consistent with financial constraints and commitment failures in firms being prevalent, although we cannot formally separate the relative contribution of the program in addressing each of these two market failures. In contrast, Hardy et al. (2019) analyze a placement intervention that improves the matching between youths and firms and reduces entry costs. They only find a small increase in youths' participation

in apprenticeship. This suggests that information asymmetries and entry costs alone may not be the most binding constraint to expand access to apprenticeship among youths.

Our study also relates to research on training and apprenticeship programs. Overall evidence on training program effectiveness is limited (Bertrand et al. (2013); Blattman and Ralston (2015); McKenzie (2017)). There is a general debate on whether in-class vocational training or on-the-job training is more effective. On the one hand, vocational training can equip youths with general skills and provide certification.[1] Yet vocational training has several potential drawbacks. It might not provide the skills that firms are looking for on the labor market. Participants may not acquire work habits and related soft skills either. On the other hand, on-the-job training may allow participants to acquire a range of skills and accumulate work experience. However, these skills might be firm-specific and limit mobility across firms. Traditional apprenticeships in developing countries mostly involve on-the-job training, but they tend to be longer that typical on-the-job training programs that last a few months. In a recent paper, Alfonsi et al. (2019) compare vocational training with traditional apprenticeship. They find that the two types of training equip youths with similar technical skills. In the medium-term, vocational training improves youths' labor market prospects. Vocational trainees are better able to signal their skills and benefit from greater labor mobility. In contrast, traditional apprenticeship only has short-term effects. Youths quickly exit the firm in which they were placed, and employment effects fade out approximately a year after the start of the intervention. A few other studies estimate the returns to traditional apprenticeship and also find mixed results (Frazer, 2006; Cho et al., 2013), including negative effects on earnings (Hardy et al., 2019). Given these results and the large number of youths in traditional apprenticeship in developing countries, there are important questions on whether apprenticeships can be upgraded and made more effective. In this paper, we consider a setting where traditional apprenticeships are prevalent. We analyze the introduction of subsidized dual apprenticeships which combine theoretical training and a certification scheme with on-the-job training. The combination of vocational and on-the-job training can ensure individuals acquire both firm-specific and general skills.[2] Despite this

[1] A recent literature documents the returns of improving the ability of youths to signal their skills. See Abebe et al. (2018); Abel et al. (forthcoming); Bassi and Nansamba (2018); Carranza et al. (2019) for studies in Sub-Saharan Africa.

[2] The combination of the two types of training has been studied mostly in the context of short on-the-job training programs. For instance, Attanasio et al. (2011, 2017) analyze the effectiveness of vocational training

potential, the literature on the effectiveness of dual apprenticeship is thin, both in developed and developing countries (Wolter and Ryan, 2011). To our knowledge, we present the first experimental estimates of the impacts of dual apprenticeships on youths and firms. We document positive impacts on youths' earnings four years after the start of the program as well as improvements in skills consistent with gains in productivity.

Finally, our paper also links to the literature on the identification of indirect and equilibrium effects in program evaluation. Various strategies have been used to identify indirect and equilibrium effects. Some recent papers use tailored experimental designs to identify indirect effects on the non eligible population (Angelucci and De Giorgi, 2009; Bandiera et al., 2017) or related mechanisms (Cunha et al., 2018). The double randomization design proposed by Moffitt (2001) has been used to identify impacts on the eligible population that is not participating (Crépon et al., 2013; Akram et al., 2017). In these cases, the identification of indirect effects requires powerful experiments and adapted designs (Baird et al., 2018). While we do not measure general equilibrium effects, our double-sided randomized design is tailored to estimate direct impacts on youths as well as potential substitution effects in firms. As such, we study potential indirect and equilibrium effects of a specific kind. Our study takes place in a setting where a market for traditional apprenticeship exists. We analyze how the introduction of subsidized dual apprenticeships affects the equilibrium on that market, and whether there are indirect effects such as substitution effects in firms. Analyzing such indirect effects is an important but understudied question (see for example the discussion in McKenzie (2017)). Most empirical research on employment and wage subsidy programs has focused on individual participants (for reviews, see Almeida et al. (2014); Card et al. (2018), with a few studies analyzing impacts on labour demand in firms (e.g. de Mel et al. (2019)). It is rare for studies to simultaneously analyze direct and indirect impacts for youths and firms. Another example is the recent paper on different types of training by Alfonsi et al. (2019), who also use a double-sided randomization to measure impact on youths and firms.

The paper is structured as follows. Section 2 describes the intervention and experimental design. Section 3 presents the conceptual framework. Section 4 discusses the data and estimation strategy. Section 5 tests program impact on access to apprenticeship. It documents windfall and substitution effects for youths and firms. Section 6 analyzes impacts on youth

complemented by a short internship and placement services.

employment and earnings in the short and medium-term. Section 7 presents impacts on youths' human capital investments and skills. Section 8 discusses short-term impacts on the net value of work by apprentices in firms. Section 9 concludes. The appendix contains supplementary material.

2 Intervention and Experimental Design

2.1 Apprenticeships in Developing Economies

Traditional apprenticeships are widespread in the developing world, including in Africa (Filmer et al., 2014). The vast majority of apprentices are in traditional apprenticeship. Traditional apprenticeships are one of the few sources of training accessible to the large number of youths who exited the education system without completing primary or secondary school. They are also one of the main sources of skill acquisition for informal operators. Despite their prevalence, traditional apprenticeships remain poorly understood and documented. Traditional apprenticeships are private arrangements between youths (or their families) and private sector firms. Although their form can vary, traditional apprenticeships share a range of characteristics (Walther, 2008). They take place in micro and small firms, many of which operate in the informal sector. With the help of their family, youths are often placed with master craftsmen identified through connections. A fee (in-kind or in cash) is paid for the placement. Arrangements are rarely formalized through a contract. Youth learn the trade through practical, on-the-job training by working in enterprises under the mentoring of a master craftsman, either an experienced worker or the enterprise owner. Over time, youths start being paid. Traditional apprenticeships can last many years, and often do not lead to certification, although master craftsmen typically need to grant departure to mark the completion of an apprenticeship. After completing traditional apprenticeships, youths transition either as an employee in the host firm, as a wage worker in another firm, or in self-employment. Most youths remain in the informal sector, in part due to the scarcity of formal wage jobs suitable for workers with limited education.

2.2 The Côte d'Ivoire Formal Apprenticeship Program

Côte d'Ivoire is a lower middle-income country, which had a GDP per capita of 770 000 FCFA per person (approximately USD 1350) in 2015. Employment is heavily concentrated in self-employment, with 46.9 percent of employed individuals in agricultural self-employment and 29.3 percent in non-agricultural self-employment. In contrast, only 17.4 percent of individuals who are working hold wage jobs, and formal unemployment was 6.7 percent in 2015 (Christiaensen and Premand, 2017). After steady economic development through the mid-1990s, Côte d'Ivoire entered a period of conflict, punctuated by a post-electoral crisis in 2010-11. Stability returned after the institution of a new government in 2011, and growth has been steady since then. A range of public investments and programs were launched in 2011. They included an emergency youth employment and skills development project (PEJEDEC), which had an objective to improve access to temporary employment and skills development opportunities for youths in Côte d'Ivoire. Among other interventions, the project included an apprenticeship component.[3]

The PEJEDEC apprenticeship component is overseen by the office coordinating employment programs (BCP-Emploi)[4], with the national training agency as implementing agency (AGEFOP).[5] The program puts in place a formal apprenticeship scheme lasting 12 or 24 months, depending on occupations. The program initially aimed to cover 5,000 youths, and is in the process of being expanded to approximately 14,000 youths. Low-skilled youths between 18 and 24 years old are placed in firms, where they receive on-the-job training under the supervision of a master craftsman. Youths sign a contract with the implementation agency (AGEFOP) and are paid a monthly subsidy of 30,000 FCFA (approximately USD 54, or half the formal minimum wage), which is aimed to cover meals and transport costs. They receive an insurance coverage and work equipment. The apprenticeship is dual, since on-the-job practical training is complemented by theoretical training (approximately 180 hours per year) tailored to the needs of apprentices and delivered by local training institutions. AGEFOP defined the key general skills apprentices needed to learn in each trade and developed related training curricula. The program also introduced basic regulations through

[3]PEJEDEC: Projet Emploi Jeune et Développement des Compétences (www.pejedec.org). See Bertrand et al. (2017) for evidence on the effectiveness of a public works program supported by the same project.

[4]BCP-Emploi: Bureau de Coordinnation des Programmes d'Emploi.

[5]AGEFOP: Agence de la Formation Professionnelle.

standardized apprenticeship contracts. AGEFOP apprenticeship counselors regularly visit master craftsmen and apprentices to monitor the relationship and learning. They also have the authority to suspend subsidies in case there are issues with youths' participation or performance. Formal apprenticeships end with an assessment of youths' skills, with a possibility of certification. Firms are not compensated for taking on apprentices, though they receive a small toolkit of material to facilitate practical learning. Moreover, employers commit not to request the payment of tuition fees at the start of the apprenticeship, in contrast to the traditional apprenticeship model in West Africa (Walther, 2008).

The average program cost is estimated at FCFA 1,135,030 (approximately USD 2,045) per youth for a 24 months apprenticeship. This includes FCFA 720,000 (or USD 1,297) for subsidies for youths, FCFA 330,000 (or USD 595) of other direct costs (toolkit, theoretical training, equipment,...), and FCFA 85,030 (approximately USD 153) for indirect costs (selection, counseling, and so forth).

2.3 Enrollment Process and Experimental Design

One objective of the experiment is to measure simultaneously direct effects among youth (including windfall effects) and indirect effects in firms (including substitution effects). This requires a specific design that randomly assigns both youth and firms to treatment and control groups. In this section, we present the key features of the experimental protocol. Appendix A1 provides a more detailed description. Figure 1 illustrates the design. In the next section, we show that the experiment identifies parameters consistent with a simple theoretical framework, and discuss how windfall and substitution effects combine.

The experiment was implemented in 7 urban areas in the interior of the country. The design was stratified by micro markets, defined as a trade in a given locality. As a first step, the implementing agency identified a set of firms that were interested to host program apprentices and the number of their open apprenticeship positions. This gave a number of positions to be potentially filled in a given micro market. The second step was to register interested and eligible youths in the experiment. In each micro market, as many youths were registered as there were open positions.

Third, we randomly assigned firms to treatment and control groups, in order to have an equal number of treatment and control positions in each micro market. One practical

complication was that firms did not offer the same number of positions in each micro market and that firms could open positions in several (closely linked) micro markets.[6] To address this, we paired firms according to the structure of their open positions in the set of micro markets, and then performed randomized assignment within each pair. Once treatment firms were drawn, this gave us the number of positions to fill in any given micro market. This was usually half the number of positions registered in the first step, but not always, given variations in the portfolio of open positions in firms.

The fourth step of the experimental protocol was to randomly assign exactly the same number of youths to treatment in each micro market as the number of positions to fill. Figure A1 presents the distribution of the ratio of assignment of youths by micro market.[7] The rate of assignment of youths to the program is specific to the micro market, so that we use weights in the youth level analysis (see Section 4). In the fifth and final step, counsellors from the implementing agency matched selected youths with selected firms offering positions in the same trade. The matching took place based on criteria such as distance between the firm and the youth place of residence.

Across the 7 localities covered by the study, 731 firms offered apprenticeship positions. Approximately half of them (361), were randomly selected to host program apprentices. 1,842 young applicants were eligible. 911 eligible and motivated youths were assigned to the program and 921 to the control group.[8] Most firms offered several positions, and on average treatment firms were assigned 2.52 apprentices.

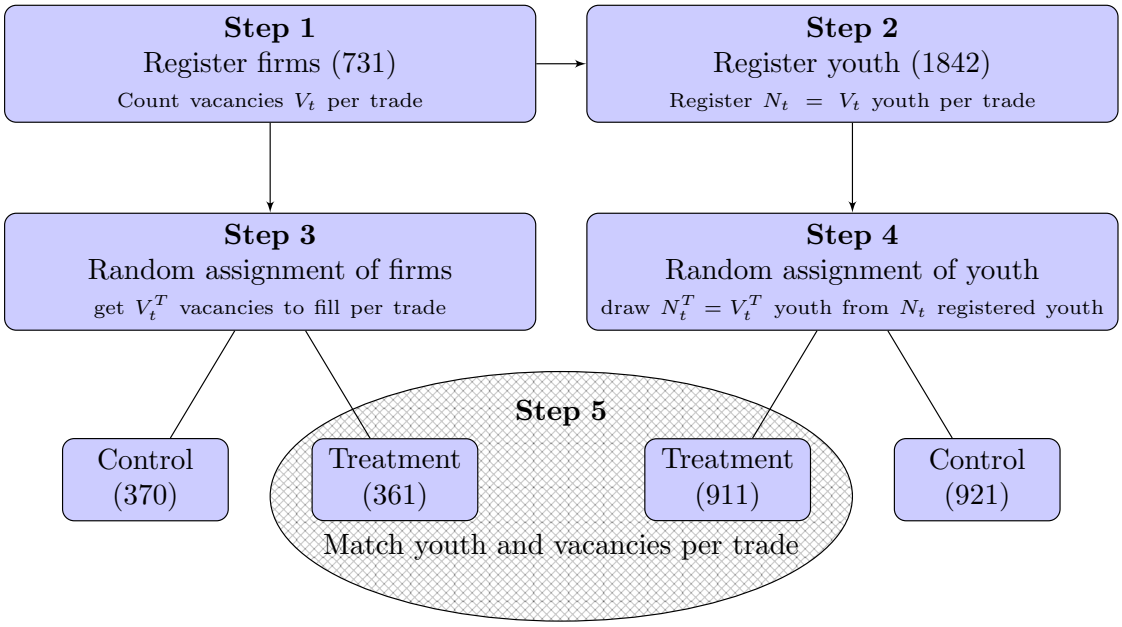
The randomization protocol implies that youths and firms in the treatment and control groups are statistically similar (as we discuss further in Section 4). However, one potential concern is whether youths and firms from the control groups are affected by the experiment, for instance through spill-over effects driven by changes in the tightness of the apprenticeship market (the chance of firms and potential apprentices to match). The conceptual framework in Section 3 discusses these issues and analyzes the sources of adjustment in the tightness of

[6] In some cases, firms in a given sector are active in several trades. For example, the garage sector includes apprenticeship positions in several trades: coach builder, car mechanic, car electrician, or car painter.

[7] When a small number of positions is offered in some trades, and when those positions are offered together with positions in other trades, the firm randomization process can lead to all the positions in a given trade assigned to treatment or to control. In such a case, the youth assignation probability is either 0 or 1. We kept the firms in the data set, but the corresponding youths were not included in the sample for youth regressions. The case arises for 10 youths.

[8] 10 youths were removed from the sample because of the special case mentioned in footnote 7.

Figure 1: Experimental design



The figure describes the five steps of the experimental design. The design was implemented separately in each of the 7 localities in the experiment. The numbers in parenthesis provide the number of observations across the 7 localities. 10 youths were removed from the analysis sample because of the special case mentioned in footnote 7, so that the youth sample has 1832 observations.

the apprenticeship market. It shows that there are various reasons to expect the adjustment to be small.

The size of the experiment is one factor that drives the size of the potential adjustment in the tightness of the apprenticeship market. It is useful to gauge the size of the experiment compared to the market for apprentices. As mentioned above, apprenticeship is the main source of training for youths in sub-Saharan Africa. Based on the Côte d'Ivoire 2013 national employment survey and 2014 population census, we can show that the share of treated youths in the apprenticeship market is limited. Table 1 shows the estimated share of youths in the treatment group relative to the number of youths in apprenticeship in the study localities. The order of magnitude of this size is less than 10%. (See Appendix A1 for details.)

The experiment induces negative shocks on both the demand and the supply of traditional apprentices. Within each micro market, note that the experimental design maintains a similar ratio of apprentices and available vacancies in the treatment and control groups. This also ensures that the chances of a match between firms and youths in the control group are unchanged.

3 Conceptual Framework

We develop a simple matching model for apprenticeship positions to describe the interaction between treated and control units among both youths and firms. The objective of the model is to show how the introduction of subsidized dual apprenticeships affects the equilibrium in the market for traditional apprenticeship. This is useful to analyze potential substitution effects on the two sides of the market. The framework is exposed here, and Appendix A3 provides additional details.

3.1 Supply and demand of traditional apprentices

The model makes a distinction between formal and traditional apprentices. We consider a population of N_y youths searching for traditional apprenticeship positions. The intervention offers A youths (representing a share $\sigma_a = A/N_y$) to participate in a new formal apprenticeship program.[9] N_{form} of these youths will enter formal apprenticeships. The model also considers a population of N_f firms searching for traditional apprentices. The program offers a share σ_f of firms to host formal apprentices. We assume that all firms are willing to do so and hire n_f formal apprentices.

The model shows how the entry of N_{form} formal apprentices changes the equilibrium in the market for traditional apprentices. The adjustment variable is the tightness of the apprenticeship market (θ), i.e. the ratio of available vacancies to the number of youths searching for a position. This is primarily because the main purpose of the model is to describe how the intervention affects the chances of youths and firms to match, which is directly related to changes in the tightness. In the traditional apprenticeship market, arrangements are rarely formalized in a contract. Wages are paid in an informal way, including different components for meals, transportation, clothing, and "motivation".

Consider the supply of traditional apprentices $S^{trad}(\theta)$. By removing a share σ_a of youths from the traditional apprenticeship market, the program causes a downward shift in the supply by $\sigma_a S^{trad}(\theta)$. This shift can be written as ωN_{form}. ω is a key parameter corresponding to the "windfall" effect. It measures the share of youths in formal apprenticeship who would have entered traditional apprenticeship absent the program. (This is estimated for the value

[9] We assume all youths prefer formal apprenticeships to traditional apprenticeships.

of the market tightness with the program, as we discuss further in Section 3.3.1).

At the firm level, we consider the partial demand for traditional apprentices once n_f formal apprentices have been hired. We model it as $d(\theta) - \psi n_f$. ψ is another key parameter. It relates to the potential substitution between traditional apprentices and formal apprentices. If $\psi = 1$, there is full substitution: each entry of a formal apprentice is associated with one less entry of a traditional apprentice in the firm, so that the total number of apprentices remains unchanged. On the other hand, if $\psi = 0$, there is no substitution: the entry of one formal apprentice has no effect on the number of traditional apprentices hired. $d(\theta)$ is the demand for traditional apprentices absent the program. It is a decreasing function of θ since the cost of hiring one additional apprentice increases with the tightness in the traditional apprenticeship market. The aggregate demand for traditional apprentices sums the demand across all firms (including those offered formal apprentices or not). It writes as $D^{trad}(\theta) - \psi N_{form}$, where $D^{trad}(\theta)$ is the aggregate demand for traditional apprentices absent the program. ψN_{form} is the shock on the demand for traditional apprentices induced by the program.

3.2 Change in equilibrium

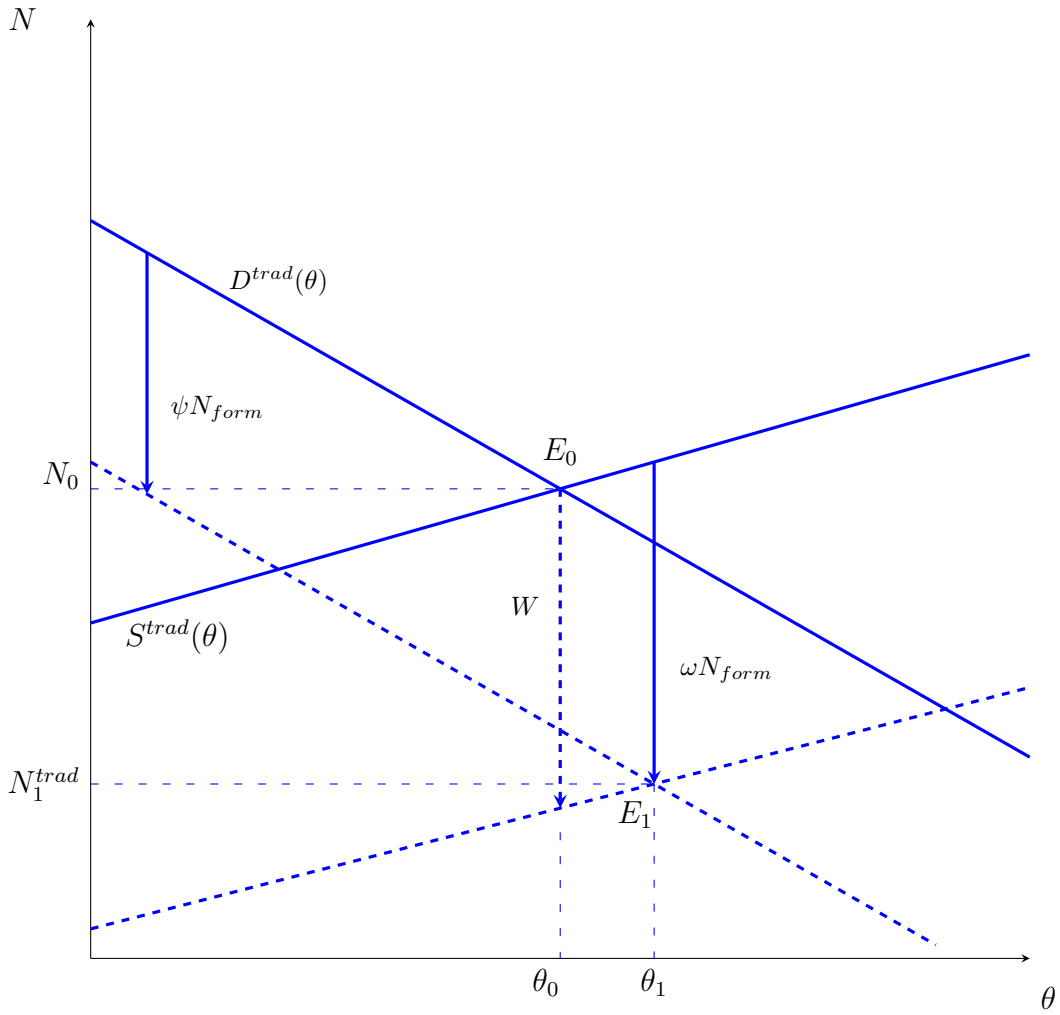
Absent the program, the equilibrium number of traditional apprentices is N_0 and the tightness of the market is θ_0 (see point E_0 in Figure 2). With the program, there is a new equilibrium in the number of traditional apprentices entering firms (N_1) and the apprenticeship market tightness (θ_1) (see point E_1 in Figure 2). The model describes changes in the apprenticeship market tightness, which indicates the changes in opportunities for potential apprentices on the market.

We can express the change in the number of traditional apprentices $N_1 - N_0$ and in the tightness $\theta_1 - \theta_0$ as function of the two shift parameters ψ and ω. This can be done by expanding the supply and demand functions around the final equilibrium value θ_1:

$$\frac{N_1^{trad} - N_0}{N_{form}} = -\frac{A_s\omega + A_d\psi}{A_s + A_d} \quad (1)$$

$$\theta_1 - \theta_0 = (\omega - \psi)\frac{N_{form}}{A_s + A_d} \quad (2)$$

Figure 2: Equilibrium employment of traditional apprentices and tightness



The figure shows the adjustment of the traditional apprenticeship market when N_{form} formal apprentices are hired. Point E_0 corresponds to the equilibrium absent any intervention. Point E_1 corresponds to the equilibrium after the intervention is introduced.

The figure shows the downward shift in firms' demand function ψN_{form} (which captures the substitution effect). It also shows the downward shift in the supply function on the youth side (which captures the windfall effect). It can be measured at initial market conditions θ_0 (W), or at market conditions θ_1, once a new equilibrium has been reached (ωN_{form}).

where A_s and A_d are the slope parameters of the supply and demand functions.[10]

As equation 1 shows, the overall reduction in the number of traditional apprentices on the market is an average of the windfall effect ω on the supply side and the substitution effect ψ on the demand side. This has two implications. First, in our double sided experiment, the net employment effects can be bounded: $|N_1^{trad} - N_0|/N_{form} \in [min(\omega, \psi), max(\omega, \psi)]$. The validity of these bounds does not depend on an assumption that the size of the experiment is small, or that the labor market tightness adjustments are small. Second, one-sided experiments that are limited to either the demand or supply side of the labor market would not provide sufficient information to estimate the net number of positions created by the program.

Equation 2 can be used to assess the magnitude of the labor market tightness adjustments. Although the sign of $(\theta_1 - \theta_0)$ is unknown, there are various reasons to expect the adjustment to be small. First, the windfall and substitution parameters ω and ψ are expected to be positive, so that they would tend to cancel each other in the adjustment of θ. In the empirical analysis, we will estimate the two parameters. As such, we will also be able to get a sense of the magnitude of the tightness adjustment ex post. Second, A_s and A_d are the derivatives of the aggregate supply and demand functions, so that their order of magnitude is of the size of the market (say M). The order of magnitude of N_{form} is of the size of the experiment (say E), thus the order of magnitude of the second term in equation 1 is the share σ of the experiment relative to the market (E/M).[11] Going back to the adjustment in the tightness, we get the following approximation:

$$|\theta_1 - \theta_0| \propto |\omega - \psi|\sigma \tag{3}$$

As discussed in Section 2.3, the relative size of the experiment is small. This is another reason to expect the adjustment in market tightness to be small.

[10] ω is defined as $\sigma_a S^{trad}(\theta_1)/N_{form}$. It involves market conditions with the program θ_1. In Section 3.3.1, we discuss how we can identify the parameter ω from the data.

[11] The size of the intervention can be characterized by the share of youths enrolled in the experiment σ_a and the share of firms hosting formal apprentices σ_f.

3.3 Implications for estimation

3.3.1 Intention to Treat parameters

The framework derives the theoretical parameters to be estimated in the empirical analysis and provides insights on how to interpret the empirical results. We consider Intention to Treat (ITT) parameters that are obtained by comparing means between the treatment and control groups, as Section 4.2 will detail. Consider for example participation in either traditional or formal apprenticeships. The entry rate in (any) apprenticeship in the treatment group is $\tau_1 = N_{form}/A$. The ITT parameter we measure compares this entry rate with the entry rate in apprenticeship in the control group ($\tau_0 = \omega N_{form}/A$). Thus our estimated ITT parameter is : $ITT_{youth} = N_{form}/A - \omega N_{form}/A$. Note that the entry rate in the control group is measured at prevailing market conditions θ_1, but the "true" ITT is based on the entry rate in apprenticeship absent the program at market condition θ_0. Figure 2 shows that the "true" ITT would involve W, while the estimated quantity is ωN_{form}. However, the difference between the two ($W/A - \omega N_{form}/A$) is of the order of magnitude of the adjustment in the tightness (see details in the appendix). As already discussed and shown in Equation 3, $|\theta_1 - \theta_0| \propto |\omega - \psi|\sigma$, the adjustment in the tightness is small under two conditions: when the shifts in supply and demand are of the same order of magnitude, and when the size of the experiment is small.

A similar analysis applies to the ITT parameter for the net number of apprentices hired by firms. The total number of apprentices hired is $n_f + d(\theta_1) - \psi n_f$ in treatment firms, and $d(\theta_1)$ in control firms. The estimated ITT parameter on the side of firm identifies $ITT_{firm} = (1-\psi)E(n_f)$. Here again, the true ITT would compare the entry rate in treatment firms with what their hiring rate of apprentices would have been absent the program at market condition θ_0 (instead of θ_1). This introduces a difference between the "true" and estimated ITT parameters, which writes: $E(d(\theta_1) - d(\theta_0))$. As before, the difference is of the order of magnitude of the adjustment in market tightness, and is expected to be small.

This analysis extends to other parameters obtained by comparing youths or firms in the treatment and control groups. Overall, changes in market condition from θ_0 to θ_1 are the only reason why youths in the control group could be affected. The change in θ is likely to be small, and we can assess ex post that it is indeed the case.

3.3.2 Instrumental variable estimation of ω and ψ

As discussed previously, our framework involves two key parameters: the windfall parameter ω and the substitution parameter ψ. Thanks to our double-sided experiment, we can identify both parameters using instrumental variable regressions.

Consider for example the regression equation of being an apprentice (of any type) on being a formal apprentice, using the assignment variable as an instrument. It is well-known that this Wald estimate is simply the ratio of the OLS estimates of youth entry into any apprenticeship and youth entry into formal apprenticeship on the assignment to treatment variable. The numerator is the ITT estimate described in the previous section $N_{form}/A - \omega N_{form}/A$. The denominator is the entry rate of youths in formal apprenticeship in the treatment group N_{form}/A. As a result, the instrumental variable estimate is $1 - \omega$.

Similarly, consider running an instrumental variable regression of the number of apprentices in firms on the the number of formal apprentices using the assignment variable as an instrument. The parameter is defined as the ratio of the ITT parameter for the number of apprentices to the average average number of formal apprentice per firm in the treatment group. Again, the formulas in the previous section show that the instrumental variable estimator identifies $1 - \psi$.

4 Data and Estimation Strategy

4.1 Data

The program was rolled out sequentially, locality after locality. Baseline data was collected in each locality as part of the enrollment process. Specifically, after the apprenticeship positions offered by firms were validated by program staff, a comprehensive baseline survey was implemented in each firm with confirmed positions. Separately, baseline data were collected among youth deemed eligible after they successfully passed a motivation interview. Baseline data collection among firms and youths took place in each locality before the randomization was performed. The baseline and enrollment phase took place between July 2014 and October 2014. The selection of firms and youths took place shortly after, and placements were mostly completed by January 2015.

The first follow-up (midline) survey took place between March 2016 and June 2016. The follow-up youth survey was collected by phone, and the follow-up firm survey in person. The surveys were collected on average 20 months after the start of the program.[12] Since most apprenticeships last 24 months, results based on the first follow-up survey should be interpreted as providing short-term impacts while apprentices are still in the program.[13]

A second follow-up (endline) survey took place between May 2018 and October 2018, approximately 4 years after the start of the experiment or 18-24 months after the end of the program. The follow-up survey focused on youths, with the objective to estimate impacts on employment and earnings after formal apprenticeships were completed. The survey was collected by phone for most of the sample. Youths who could not be contacted by phone were tracked and interviewed in person.

Substantial efforts were made to minimize attrition during the follow-up surveys.[14] As a result, 1,661 youth were surveyed in the first follow-up survey, implying a response rate of 90.7%.[15] Similarly 674 firms were surveyed, leading to a response rate of 92.2%.[16] 1686 youth were surveyed in the second follow-up survey, for a response rate of 91.5%.[17] The response rates in the follow-up surveys are balanced across the treatment and control groups.[18]

Tables A2 and A3 present baseline characteristics and balance checks for youths, respectively firms. Both tables have the same structure. The left panel is devoted to the analysis of the baseline sample (including on the last row the share of youth or firms with available baseline data). The right panel presents baseline characteristics of follow-up respondents and related balance checks. For youths, balance checks are presented separately for youths

[12]Figure A2 documents precisely the timing of surveys as a function of the randomization date.

[13] 754 of the 914 youths in the treatment group (or 82%) were in trades where the apprenticeship contract lasted 24 months.

[14]An unfortunate IT issue with the online server used for electronic data collection led to the loss of baseline data for 26% of youths (475 youths, 250 in Treatment and 225 in Control) and 5% of firms (37 firms, 18 in Treatment and 19 in Control). The problem was concentrated in two localities. The loss of some baseline data limited availability of contact information to track youths (and firms) at follow-up. This contributed to a lower response rate among youths in localities where IT issues occurred.

[15]The attrition rate is 9.3%, with 171 youths not surveyed, 84 in Treatment and 87 in Control.

[16]The attrition rate is 7.8%, with 57 firms not surveyed, 26 in Treatment and 31 in Control. Note that part of this attrition is due to firm closure. We designed a specific data collection instrument for registered employers whose firm had closed by the time of the follow-up survey. 12 cases were identified. "True" attrition is limited to 6.2% (45 firms, 23 in Treatment and 22 in Control).

[17]The attrition rate was 8.5%, with 156 youths not surveyed, 61 in treatment and 95 in control.

[18]The last row of Table A2 presents the balance check for the response rate among youths in the first and follow-up surveys (see last row, with p values of 0.92 for the first follow-up survey, respectively 0.44 for the second follow-up surveys). The last row of Table A3 contains the balance check for the response rate among firms (see last row, p-val=0.48).

in the first and second follow-up surveys.

Table A2 shows that youths interested in formal apprenticeships are 20.7 years old on average, and mostly men (87 percent). They have some (but limited) education, as 63 percent have completed primary school and 17 percent lower secondary school. 46 percent of applicants aspire to a wage job, and 54 percent to become self-employed. There are few significant differences between the treatment and control group of youths, who are largely comparable and well-balanced at baseline. As can be seen from the table, the share of available baseline data (see footnote 14) is not perfectly balanced, but the response rates at both follow-ups are well-balanced, which is what matters most since it is the follow-up sample that is used for empirical estimation.

Table A3 highlights that most firms offering apprenticeship positions are informal micro and small enterprises. 84 percent have no formal legal status and 67 percent do not keep books. Firms have 6.4 permanent employees on average (counting the owner), of which 3.4 are apprentices. Traditional apprentices therefore constitute more than half the workforce in these micro and small firms. Traditional apprentices are mostly hired through private channels, 81 percent based on a request from their parents. About half of the apprentices in firms at baseline pay fees to the master craftsmen. Table A3 documents that the experiment led to good baseline balance between the treatment and control firms: the few significant differences are marginal and of small magnitude.

In the first and second follow-up youth surveys, we collected data on youth activities at the moment of the survey, as well as hours and earnings in those activities. Based on a detailed employment module covering primary and secondary activities, we can distinguish between occupations as wage worker, self-employed or apprentice. We use this variable to build a measure of participation in apprenticeship. In the first follow-up survey, we also measured entry into apprenticeship since the start of the experiment. This comes from a module of the survey where we collected detailed data on human capital investments, including participation in apprenticeship, vocational training and schooling. We can then distinguish between formal and traditional apprenticeship based on a question about enrollment in formal government-supported apprenticeship programs. The list of possible formal apprenticeship programs includes the subsidized dual apprenticeship program offered by PE-JEDEC, as well as a smaller program offered by AGEFOP. The two programs can at times

be hard for youths to distinguish since the PEJEDEC program was also implemented by AGEFOP. We thus consider a formal apprenticeship variable capturing youths who reported being enrolled in a formal apprenticeship supported by either PEJEDEC or AGEFOP.[19]

In the second follow-up survey, we complement this data with detailed information on youths' human capital investments since the start of the experiment. This allows us to assess whether the intervention had an impact on human capital investments after the end of the program. Importantly, we also collect data on the type of tasks realized by youths in their primary occupation. This is based on the "task approach" to estimate the skill-content of occupations developed by Autor et al. (2003) and Acemoglu and Autor (2011). Autor and Handel (2013) applied this approach to measure the content of tasks at the worker level. This was adapted to developing country contexts and implemented in several large-scale surveys (see Dicarlo et al. (2016)). We apply this approach to test whether the intervention affected youths' skills based on the complexity of tasks realized in their primary occupations. For each worker, we build an index of routine task intensity. The index aggregates routine and non-routine (analytical or interpersonal) tasks. We then estimate whether participation in formal apprenticeship impacted skills by increasing the frequency of non-routine tasks, and decreasing routine task intensity.

The follow-up firm survey collected data on the characteristics of firms, their workforce, sales and profits. It compiled a listing of all apprentices who entered or left the firm since the start of the experiment (i.e. on or after the randomization date in each locality), and collected additional information on each apprentice, both from enterprise owners and from apprentices themselves. This employer-employee type of data enables us to accurately measure the flows of apprentices in and out of firms, as well as their contribution to firm activity. For example, we can compute the number of apprentices working in firms at the moment of the survey, but also various interesting flows: the number of apprentices who entered firms since randomization, and among them those who left firms and those still in firms. We can measure all these variables separately for formal and traditional apprentices. We can identify whether each apprentice was enrolled through a formal apprenticeship program. (Appendix A2 provides additional details on this disaggregation.) We can then obtain the number of

[19]More details about the formal apprenticeship variables can be found in Appendix A2, Table A6 and Table A5. Appendix A2 shows that the formal apprenticeship variable is consistent with administrative data and a process evaluation survey.

traditional and formal apprentices who entered the firm since the start of the experiment, and who were present in firms at the time of the follow-up survey.

For each apprentice, the follow-up firm survey also asked about the number of days worked in the last seven days and the number of hours worked in the last business day. In order to measure apprentices' contribution to firm activity, we asked employers about the amount he would have had to pay to hire a casual worker to perform the same tasks. This in turn allows us to compute the value of the work performed by each apprentice. We also asked employers about the compensation paid to each apprentice. These measures can be aggregated at the firm level.[20] In addition, we collected several measures of sales and profits, addressing concerns about the measurement of these variables raised by De Mel et al. (2009). Following the procedure recommended in De Mel et al. (2009), we obtained direct measures of total profits and revenues. We then asked firm owners to recall all sales over the last month and related expenses. On that basis, we collected another (updated) measure of profits and revenues. Moreover, we implemented near-systematic back-checks of key variables, including sales and profit. Thus, for most firms in the sample, we have four measures of sales and profits.

4.2 Estimation Strategy

Given the double-sided randomization design, intent-to-treat (ITT) program impacts on firms can be estimated by comparing outcomes between firms assigned to treatment (i.e. where formal apprentices were assigned by the program to fill open positions), and firms assigned to control (i.e. where open apprenticeship positions were not filled by the program). The ITT analysis at the firm level is performed using OLS regressions with the 667 firm-level observations at follow-up:

$$y_i = a + bT_i + \sum_v \gamma_v 1_v + \sum_s \delta_s 1_s + u_i \tag{4}$$

[20] We made a distinction between various forms of compensation. Employers usually provide meals and cover expenses such as transportation and clothes. They also provide some money for the work done by youths in order to "motivate" them. We measure each of these payments and aggregate them by youth and by firm to get a total wage bill for apprentices.

We compute White-Huber robust standard errors. T is the variable capturing assignment to treatment, v stands for the locality and s for the sector.[21]

In parallel, intent-to-treat program impacts on youths can be estimated by comparing outcomes between youths assigned to treatment (i.e. offered a formal apprenticeship position in a treatment firm), and control. We account for the fact that youths were assigned to treatment and control with probabilities that were specific to each trade in each locality, producing a set of corresponding strata S_t. We compute the empirical assignment rate in each stratum $\widehat{\pi_m}$ and estimate inversely propensity weighted regressions. The weights are simply defined as $T_i/\widehat{\pi}_m + (1-T_i)/(1-\widehat{\pi}_m)$.[22] To obtain accuracy gains from stratification, we run an inversely propensity weighted regression with strata dummies on the 1,661 youth observations:

$$y_i = a + bT_i + \sum_{St} \mu_{St} 1_{St} + u_i \tag{5}$$

We compute White-Huber robust standard errors.

As a robustness check, we also implement permutation tests for the main ITT estimates for youths and firms. The null hypothesis is that the program has no effect on any individual: $y_i(0) = y_i(1)$. The permutation test provides the exact p-value of a given test under this null hypothesis. The test is implemented first with actual data, and then it is implemented again after randomly assigning units to a fake treatment group. The p-value is the share of draws for which a statistic is above the one obtained with actual data. This type of test is particularly useful when samples are small and the distribution of the outcome variable is skewed. As such, while we implement the test for each of our main ITT estimates, we are most interested in the results for variables such as the value of apprentices' work in firms or youth earnings.

[21] Sectors are broader than trade. Sectors refer to firm activities and "trades" to youths' occupations. The two concepts are often the same, but in some cases firms in a given sector are active in several trades. See Appendix A1 for more details.

[22] The empirical assignment rates are defined on the sample used in the regression. There are 1,676 observations for which we were able to collect follow-up data. However, there are 15 youths for whom the empirical assignment probability within their stratum is either 0 or 1, and for whom we have follow-up data. These are discarded from the youth regressions.

5 Short-term Windfall and Substitution Effects

5.1 Youth Entry into Apprenticeship and Windfall Effects

In this section, we document how the subsidized dual apprenticeship program affected youths' entry into apprenticeship, and the extent to which it induced windfall effects as youths switched between traditional and formal apprenticeships. The left panel of Table 2 (titled "Youth") presents ITT estimates for youth participation in apprenticeship. Participation in apprenticeship is decomposed between traditional apprenticeship, i.e. private arrangements that exist independently of the program, and formal apprenticeship of the type promoted by the program. We also distinguish between participation at the moment of the survey and over the course of the experiment.

The top panel of Table 2 documents impacts on participation in apprenticeship at the time of the first follow-up survey. Column (3) shows a large increase in the share of youths in apprenticeship approximately 20 months after the start of the program. While only 17.9% of youths in the control group are in apprenticeship, the proportion in the treatment group is larger by 36.5 percentage points, reaching 54.4%. This points to a large increase in participation in apprenticeship.

Column (1) and (2) of Table 2 present estimates for some of the key parameters from the conceptual framework in Section 3. Column (1) documents a large increase in participation in formal apprenticeship: the share of youths in formal apprenticeship is 49.0 percentage points larger in the treatment group than in the control group. The estimated increase in the participation in formal apprenticeship corresponds to the parameter τ_1 of the conceptual framework: $\tau_1 = 0.49$.

Participation in formal apprenticeship is in part associated with substitution out of traditional apprenticeships. Column (2) shows that being offered participation in formal apprenticeship leads to a substantial reduction in participation in traditional apprenticeship. 16.1 percent of youths in the control group participate in traditional apprenticeships but this proportion is reduced by 12.5 percentage points in the treatment group. This corresponds the windfall effect τ_0 in our framework ($\tau_0 = 0.125$). By construction the net impact of offering participation in the program on entry into apprenticeship directly follows from τ_1 and τ_0: $\tau_1 - \tau_0 = 0.365$. This is the estimate in column (3). Despite the significant windfall

effect, the program still leads to a substantial impact on access to apprenticeship among youths.

Note that there is a small non-compliance with the experimental protocol. We observe a very low, but non-zero share of control youths (1.8 %) participating in formal apprenticeship. This is primarily due to the fact that our definition of "formal" apprenticeship includes both AGEFOP and PEJEDEC programs, as discussed in Section 4.1.

Table A7 in appendix presents the results from permutation tests for our main ITT estimates (Imbens and Rubin (2015)). The first three columns of the top panel correspond to participation in apprenticeship among youths. There is a remarkable concordance between asymptotic results and results from permutation tests.

So far, we have focused on impacts on participation in apprenticeship at the time of the follow-up survey. Our surveys also enable us to measure inflows into apprenticeships over the duration of the experiment. ITT estimates for impacts on entry into apprenticeship are presented in the bottom panel of Table 2. They confirm previous findings of increased participation but also significant windfall effects. The share of youths entering formal apprenticeship over the course of the experiment increases by 71.2 percentage points. The share of youths entering any type of apprenticeship also increases by 52.8 percentage points. This is substantial but still lower than the net impact on entry into formal apprenticeship. The difference between the two estimates shows that 18.5 percent of treated youths would have entered traditional apprenticeship in the absence of the intervention, a significant windfall effect.[23]

Differences between results on the share of youths in apprenticeship at the time of the follow-up survey (top panel) and over the course of the experiment (bottom panel) are related to drop-out. Drop-out is a common issue in many employment or training programs, including in apprenticeship (e.g. Cho et al. (2013)). We observe drop-out from formal apprenticeship as well as from traditional apprenticeship. Out of the 75% of treated youths who enter formal apprenticeship, 50.8% are in formal apprenticeship at the time of the follow-up survey. Similarly, out of the 22.5% of control youths who enter traditional apprenticeship, 16.1% are still in apprenticeship at the moment of the survey. The drop-out rate is of similar

[23]There is also a small non-compliance when analyzing the inflow of apprentices. The share of control youths in formal apprenticeship is 3.8 %. This is due to our definition of "formal" apprenticeship including both AGEFOP and PEJEDEC programs, as mentioned above and in Section 4.1.

magnitude in formal and traditional apprenticeship (respectively 32.4% and 28.4%).

5.2 Intake of Apprentices and Substitution Effects in Firms

We now analyze how the subsidized dual apprenticeship program affects firms' demand for apprentices in the short-term. This includes the key question of whether the program induced indirect effects in firms, for instance through substitution between traditional and formal apprentices.

Table 2 (right upper panel, titled "Firm") documents the impact of the program on the number of apprentices who are still in firms at the time of the follow-up survey. Column (6) shows that the program led to an increase in the total number of apprentices by 0.613 apprentice per firm at the time of the follow-up survey. This is a key result of the experiment: offering youths to participate in subsidized dual apprenticeships leads to an increase in the total number of apprentices in firms. We can decompose this effect between changes in the number of formal and traditional apprentices. Column (4) shows that the total number of formal apprentices in firms increases by 0.787. Columns (6) and (4) provide estimates for key parameters of the conceptual framework (Section 3.3.1): $E(n_f - \psi n_f) = 0.613$, and $E(n_f) = 0.787$. The difference between the two parameters is the estimated impact on the entry of traditional apprentices $E(-\psi n_f)$, which is directly related to the substitution effect. Column (5) shows that 0.174 traditional apprentice is displaced per formal apprentice placed. This effect is moderate in magnitude and it is not statistically significant.

The intermediate panel of appendix Table A7 presents the results from permutation tests for the main ITT estimates on entry of apprentices in firms (first 3 columns). There is again a remarkable concordance between asymptotic results and results from permutation tests.

As explained in Section 4.1, our firm survey asked employers to list all apprentices who have worked in the firm over the course of the experiment, including those who left the firm since randomization. We can thus measure flows of apprentices in and out of firms between the start of the experiment and the follow-up survey. Table 2 (lower panel) documents net program impacts on the number of apprentices who entered since randomization. Results show that there are 1.080 additional apprentices per firm that entered since the date of randomization, and 1.398 additional formal apprentices per firm.[24] Over the course of the

[24]Figure A3 provides additional information about the impact of the program on the inflow of apprentices

experiment, the estimated substitution effect shows that 0.318 traditional apprentice did not enter firms per formal apprentice placed. This is a substitution effect of moderate magnitude and statistically significant at the ten percent level.

As can be seen in Table 2, there is also some non-compliance with the experimental protocol on the side of firms. 0.058 formal apprentices were present in control firm at the moment of the survey (upper panel), and 0.188 formal apprentices entered control firms since the start of the experiment (lower panel). This may be due in part to some recall errors, especially for measures of flows over the course of the experiment, but we cannot fully rule out that a few program youths were matched with control firms.

One feature of our experiment is that we registered as many youths as there were open positions in firms. We can thus compare the total number of formal apprentices in the youth and firm surveys. These comparisons are not straightforward and we provide additional details in Appendix A2. Overall, the flows of formal apprentices since the start of the experiment are broadly consistent in the firm and youth surveys. At the moment of the follow-up surveys, however, there are more formal apprentices in the youth survey than in the firm survey. Appendix A2 details how these numbers can be reconciled. The difference may in part be explained by the timing of the surveys. The two surveys were not collected exactly at the same time. In addition, the surveys were implemented toward the end of the program, at a time when theoretical training was ongoing. Some formal apprentices attending theoretical training may not have been listed as present at the time of the follow-up firm survey, although they were still listed in flow variables covering the period of the experiment.

In this section, we have documented an increase in the entry of apprentices over the course of the experiment and in the number of apprentices that remained in firms at follow-up. We obtain these results based on rich data listing all apprentices and asking a detailed set of questions about each of them. The follow-up firm survey also provides information about the total workforce in firms, including apprentices and other types of employees. This data simply counts employees, ignoring their date of entry. Appendix Table A8 presents the estimated impact on those variables aggregated at the firm level. While there is a significant

into firms. The figure shows monthly inflows of apprentices in treated and control firms by date (with zero being the randomization date). The figure makes a distinction between inflows of formal apprentices and traditional apprentices in treated firms. The figure clearly shows a spike of entry of formal apprentices in treated firms shortly after randomization.

impact on the inflow of apprentices who entered firms since the beginning of the experiment, there is no significant impact on the overall number of apprentices in firms at follow-up. The estimated impact is 0.464 with a standard error of 0.362. The impact on flows are not large enough to affect stocks significantly, which may be due in part to large standard errors in the stock variables.[25]

5.3 Net Impact on Apprenticeship Positions Created

So far, we have discussed ITT estimates of offering youth to enter formal apprenticeships, and of assigning formal apprentices to firms with open positions. Results show that there are significant windfall effects for youths as well as substitution effects in firms. These effects imply that the net number of positions created by the program is smaller than the number of formal apprentices placed. Consistent with the conceptual framework in Section 3, we now discuss more precisely what is the overall impact of the intervention on the net number of apprenticeship positions created. To do so, we estimate the two main parameters from the framework, ω and ψ, and consider how windfall and substitution effects combine.

5.3.1 Instrumental variable estimation of ω and ψ

As discussed in Section 3.3.2, we can estimated the two parameters ω and ψ using instrumental variables.

We first consider the regression equation of the entry into any form of apprenticeship on the entry into formal apprenticeship using the youth assignment variable as an instrument:

$$a_i = \alpha_y + \beta_y f_i + \sum_{St} \mu_{St} 1_{St} + u_i \tag{6}$$

where a_i stands for having started an apprenticeship since the beginning of the experiment and f_i for having started a formal apprenticeship. The conceptual framework shows that the instrumental variable estimate identifies $1 - \omega$, where ω relates to the windfall effect and captures the proportion of formal apprentices who would have started an apprenticeship

[25]For completeness, the impact of 0.464 on the stock of apprentice combines the previous impact of 0.613 on the total number of apprentices who entered over the course of the experiment and are still in firms at follow-up, with the impact on the total number of apprentices who were in the firm before the randomization and are still in the firm at the moment of the survey, -0.154 (with a standard error of 0.239).

absent the program.

We also consider the regression equation of the total number of entries of apprentices in firms on the entries into formal apprenticeships using the firm assignment variable:

$$n_{tot,i} = \alpha_f + \beta_f n_{f,i} + \sum_v \gamma_v 1_v + \sum_s \delta_s 1_s + u_i \quad (7)$$

where $n_{tot,i}$ is the total number of youths entering apprenticeship in firms and $n_{f,i}$ is the total number of formal apprentices entering firms. As highlighted in the conceptual framework, this instrumental variable estimate identifies $1-\psi$. ψ is the substitution parameter: for each formal apprentice entering firms, there is ψ less traditional apprentice. The instrumental variable approach also accounts for the small non-compliance mentioned above.

Table 3 presents the results. We consider participation in traditional or formal apprenticeship since the start of the experiment. The fist two columns contain the reduced form, which are the ITT estimates presented above. The third column presents IV estimates for (1-ω) and (1-ψ), which are simply the ratio of the first two columns. The last column provides the estimated substitution and windfall parameters ψ and ω. As can be seen from the table, the estimated windfall parameter for youths is 0.259, with a standard error of 0.022. On the firm side, there is 0.773 youth entering firms per formal apprentice placed, thus leading to an estimated substitution parameter of 0.227, with a standard error of 0.128.

In Table 3, we use the inflow of apprentices since the start of the experiment, including those who dropped-out, with the first stage estimates corresponding to the lower panel of Table 2. Instead, we could have estimated the windfall and substitution parameters using the upper panel of the table, which focuses on youths who are still apprentices at the moment of the follow-up survey. The estimated ω and ψ would have been very close, with $\omega = 0.125/0.490 = 0.255$ and $\psi = 0.174/0.787 = 0.221$. This highlights the robustness of the estimated windfall and substitution effects.

These findings have important implications. As shown in Equation 1, the reduction in the total number of traditional apprentices per formal apprentice placed is a weighted average of ψ and ω. We cannot determine the weights precisely, because they are function of demand

and supply parameters that we are unable to estimate. However, we can provide bounds:

$$\frac{A_s\psi + A_d\omega}{A_s + A_d} \in [min(\psi, \omega), max(\psi, \omega)] = [0.227, 0.259] \tag{8}$$

Since the two parameters ψ and ω are very close, the interval is rather narrow. Overall, the net number of apprenticeship positions created by the program is thus estimated between 74.1 and 77.3 percent of the number of formal apprentices placed.[26] While results confirm that there are windfall and substitution effects, the effects are of small to moderate magnitude. Overall, the subsidized dual apprenticeship program does expand access to apprenticeship by increasing the net number of apprenticeship positions.

We can obtain Imbens-Manski confidence intervals for partially identified parameters (Imbens and Manski, 2004). The 95% confidence interval we obtain is $[-0.0098, 0.2997]$.[27] In other words, the 95% confidence interval for the net number of positions created by the program is between 70.3% and 101% of the number of formal apprentices placed.

5.3.2 Adjustment in market tightness

We can also assess the magnitude of market tightness adjustments. Note that estimates of the two parameters ω and ψ are of the same order of magnitude. As shown in Equation A8, the order of magnitude of the change in the market tightness is $\sigma(\psi - \omega)$, where σ is the size of the experiment. The experiment was designed in a setting where σ is small, which is a first reason to expect the market tightness adjustment to be small. In addition, as equation A8 clearly shows, the adjustment also depends on the difference between the two estimated parameters ψ and ω. Again, this difference is small.

This result is important for the interpretation of the ITT estimates. The experiment was designed so that the share of youths and firms enrolled was small. We were thus initially confident that ITT estimates would not be affected by changes in market tightness. The results confirm this feature of the experiment: the tightness adjustment is very small in

[26] As for the calculation of the windfall and substitution effect, rather than estimating the net number of apprenticeship positions created by the program over the course of the experiment, we could have estimated the net number of apprenticeship positions created at the time of the follow-up survey. In this case, with an estimated substitution effect of 0.221 and an estimated windfall effect of 0.255, the net number of positions created by the program would be estimated between 74.5 and 77.9 percent of the number of formal apprentices placed.

[27] This interval is defined as $[\psi - C\sigma_\psi, \omega + C\sigma_\omega]$, with C satisfying $\Phi(C - r) - \phi(-C) = 0.95$, where $r = (\omega - \psi)/max(\sigma_\psi, \sigma_\omega) = 0.25$. The value of C satisfying the equation is found to be 1.850.

practice. Control firms and youths are thus unlikely to be affected by spill-overs due to changes in market conditions.

6 Short and Medium-Term Impact on Youth Earnings

So far, we have focused on results on youth participation in apprenticeships, and indirect effects related to the number of new apprenticeship positions in firms. The discussion has shed light on the presence and magnitude of windfall and substitution effects, respectively among youths and firms. It has showed that the program expanded access to apprenticeship and increased the inflow of apprentices into firms. We now turn to analyzing the short and medium-term impacts of subsidized dual apprenticeships on youth employment and earnings. This provides additional information on short-term opportunity costs from participation in apprenticeship, as well as medium-term benefits and returns for youths.

6.1 Short-term Youth Employment and Earnings

We first analyze short-term impacts on youth employment, activities and earnings during the program. Results show that there are substantial opportunity costs for youths to participate in apprenticeship. Table 4 documents ITT estimates (equation 5) for employment, hours worked and earnings by type of employment.

The upper panel of the table presents results on youths' activities. It shows that youths in the control group are mostly active, as 91 percent are engaged in some economic activity. Moreover, the average number of activities in the control group is larger than one, indicating that some youths have several activities. The program only induces a small increase in participation (by 3.4 percentage points) and in the average number of activities (by 0.05). However, the program induces youths to reorganize their portfolio of activities and forgo some employment opportunities. Specifically, individuals in the treatment group are less likely to hold wage jobs (by 13.5 percentage points) or to be self-employed (by 12.9 percentage points), and more likely to be apprentices (by 36.5 percentage points).

The intermediate panel shows similar effects for hours worked. Total hours of work only marginally increase (by 3.7 hours per week). The increase in hours worked as apprentices (+18.2 hours per week) is offset by a decrease in hours worked in wage employment (-6.5

hours per week) and in self-employment (-7.7 hours per week).[28]

The third panel presents estimates of program impact on total earnings, earnings by source of employment, and non-labor earnings. Overall, the program has no short-term impact on average earnings for youths. Results show that labor earnings decrease by FCFA 10 494 (or 25 percent), while non-labor earnings increase by 10 213 FCFA (or 135 percent). The decrease in labor earnings is driven by a decrease in earnings in wage employment (-FCFA 6 414) and self-employment (-FCFA 6 381), which is only partly offset by an increase in apprenticeship earnings paid by employers (+ FCFA 3 238). The program subsidy, which is paid by the implementing agency (and not the firm), is included in non-labor income. The increase in non labor earnings in the treatment group (by FCFA 10 213) is driven by the subsidy.[29] As such, it is only after accounting for the program subsidy that forgone labor earnings are fully compensated. Overall, although the total number of hours worked slightly increases, employment earnings decrease, and total earnings remain stable. The top panel of Table A7 in appendix presents the results from permutation tests for the ITT estimates on total hours worked per week and total earnings (last 2 columns). Asymptotic results and results from permutation tests are very close.

It is also possible to compute average hourly earnings in the different occupations across the treatment and control groups. Those average hourly earnings are simply obtained by dividing earnings in a given occupation by the number of hours in that occupation. Comparisons of hourly earnings between activities and across groups are informative, although they should not be interpreted as causal, since there is selection into different occupations. In the control group, youths involved in apprenticeship earn on average FCFA 628 per hour from their employers, far lower than hourly earnings in wage employment or self-employment (respectively FCFA 1020 and FCFA 1083). This suggests large opportunity costs of apprenticeships. Interestingly, average hourly labor earnings of apprentices in the treatment group (FCFA 310) is far lower than average hourly earnings of apprentices in the control group

[28]These results are broadly consistent with the overall employment situation in Côte d'Ivoire, where unemployment is relatively low, and most youth are engaged in some type of employment, often in agriculture, non-agricultural self-employment or informal wage jobs (Christiaensen and Premand, 2017). In this context, Bertrand et al. (2017) also find that the employment impacts of a public works program mostly take the form of a reorganization of economic activities (instead of an increase in overall employment rates).

[29]Non labor income is built from three sources: "Stipends", "Remittances" and "Other sources of income". While the estimated effect on stipends is large and significant (10672), no effect is detected on the two other variables. They however account for most of the mean in the control group, with the mean stipend being only 458.

(FCFA 628). However, accounting for the program subsidy, the average hourly earnings of apprentices in the treatment group (FCFA 706) is larger, although it remains lower than earnings in wage employment or self-employment. This illustrates how the subsidy changes the structure of payments made by employers to apprentices: the provision of the subsidy leads employers to pay formal apprentices less.

Overall, results show that the opportunity costs of participating in apprenticeship are quite large in the short-term. Individuals forego earnings in wage jobs and in self-employment, and the program subsidy contributes to balancing the financial costs of undertaking apprenticeships.

The estimated average treatment effect of offering participation in formal apprenticeship on earnings during the program is close to zero. However, consistent with our framework, we expect some heterogeneity in impacts on earnings due to variations in the employment situation of participants absent the program. For some youths with limited outside opportunities, participation in formal apprenticeship might lead to an increase in earnings, for example because of the subsidy. For other youths with better opportunities, the impact on earnings might be smaller, and even possibly negative if gains expected in the future are large. The left panel of Figure 3 presents estimates of the cumulative distribution of hours and earnings during the program in the treatment and control groups.[30] As can be seen from the figure, the cumulative distribution of earnings in the treatment group is first below and then above the cumulative distribution in the control group, meaning that there is no stochastic dominance of one distribution over the other. This is important as it implies that at least some youths who entered formal apprenticeships see a reduction in earnings compared to what they would have earned absent the program. This in turn implies that these youths expect an increase in future earnings due to formal apprenticeships.

To formally test the null hypothesis of a same distribution of potential outcomes, we also implement Mann-Whitney ranksum tests of a same average rank of observations in treatment and control groups. This test has the advantage of being robust to outliers. We present exact p-values obtained using permutation tests with 10,000 permutations. Results are shown below Figure 3. The null hypothesis is clearly rejected. Out of 10,000 replications, the computed statistics is above the statistic obtained with the true assignment in only 58

[30] See notes in figure 3 for more details on its construction.

cases for earnings, leading to an estimated p-value of 0.006. Results on hours are also robust: out of 10,000 replications, the computed statistics is never above the true statistic.

6.2 Medium-term Youth Employment and Earnings

We now turn to analyzing program impact on youth employment and earnings 4 years after the start of the experiment. Table 5 presents estimates of medium-term impacts on youth employment. Effects on labor market participation (top panel) are limited to small changes in the types of jobs youths are working in. As in the first follow-up survey, almost all individuals in the control group (98.2 percent) are working at the time of the second follow-up survey. As such, the intervention only has a small impact on the share of individual employed (+1 percentage point) or on the number of activities youths are engaged in (+0.082 activities per youth). The share of youths in self-employment is stable (at approximately 46 percent in the control group). However, the intervention increases the share of youths employed as apprentices (+9.4 percentage points), while decreasing the share of youths in wage employment (-3.6 percentage points). While youths in the treatment group exited formal apprenticeships as the program ended and their contract expired, approximately 26.6 percent remain employed as (traditional) apprentices in firms.[31] These findings also show that a substantial share of youths remain in their occupation nearly two years after the end of the intervention.

Results on time worked (Table 5, middle panel) show similar patterns as effects on participation. No effects are found on total time worked. The slight increase observed while youths were in the program has vanished. A small increase in the number of hours worked as an apprentice (+4.4 hours per week) offsets a small decrease in the number of hours worked as wage employed (-2.6 hours per week). Time worked in self-employment remains unchanged.

Importantly, Table 5 (bottom panel) documents positive medium-term impacts on youths' earnings. Total labor earnings increase by 8987 FCFA per month (or 17 percent) and total earnings by 9394 FCFA per month (or 15 percent). This is particularly noteworthy since youths no longer receive any subsidy. Consistent with the program being completed, no im-

[31] Note that the share of control youths in apprenticeship is stable between the first follow-up (17.9 percent) and second follow-up survey (17.2 percent). In contrast, the share of treated youths in apprenticeship decreases from 54.4 percent to 26.6 percent. As such, the intervention did accelerate exits from apprenticeship, though not all treated youths fully exited.

pact is detected on non-labor earnings. Despite limited employment effects, youth earnings in apprenticeship increase by 3593 FCFA on average, or 62 percent. Earnings in wage employment remain stable, despite slightly lower participation among treated youths. Earnings in self-employment increase by 4512 FCFA on average, or 23 percent, even as treated youths are not more likely to be active in self-employment. These results show that, even as treated youths are slightly more likely to remain apprentices as opposed to wage employed, the net earnings gains are positive on average. In addition, since changes in participation and time worked are limited, these results point to gains in workers' productivity.

The right panel of Figure 3 presents estimates of cumulative distributions of medium term hours and earnings. The figure shows that the proportion of youths who earn less than a given amount is uniformly and significantly lower in the treatment group than in the control group. This suggests robust positive program impacts on earnings across the distribution. Looking at estimates of the cumulative distribution function is also important because the underlying regressions to measure impact on the share of youths who earn less than a given amount are robust to outliers. The results clearly show that the intention to treat parameter on earnings are not driven by outliers. We also implement Mann-Whithey ranksum test and compute p-values using permutation tests using 10,000 replications. Result are displayed below the figures. Out of 10,000 replications, the computed statistics is above the statistic obtained with the true assignment in only 5 cases for earnings, leading to an estimated p-value of 0.0005. Results also confirm the absence of an effect on hours: out of 10,000 replications, the computed statistics is above the computed statistic in 2166 cases, leading to a p-value of 0.2166. Here again, we implemented permutation tests for the estimated ITT on total hours and earnings. Results are presented in Appendix Table A7. Asymptotic results and results from permutation tests are very close.

7 Youth Human Capital Investments and Skills

7.1 Human Capital Investments

Results from the first follow-up survey clearly show that the intervention boosts the entry of youths into apprenticeship. We can also assess impacts on training participation after the end of the intervention. This is important to test whether human capital investments by

youths in the control group catch up over time. In the second follow-up survey, we collected retrospective data on all training started by youths since the launch of the experiment. For each training, the data include the start date, its type, and the end date. We analyze the share of youths in training, the number of trainings attended, the total duration of training, the share of youths who received certification and the reported direct costs of training.

Table 6 presents ITT estimates of human capital investments for youths. The upper panel presents impacts on training during the experiment (2014-2016). The lower panel presents results on training after the intervention (2017-2018). The program leads to a strong increase in human capital investments during the program. The share of youths participating in training increases by 48.4 percentage points, from 21.6 to 70 percent (column (1)). On average, treated youths participate in 10.83 additional months of training compared to the control group. This is a large increase since youths in the control group only spend 2.93 months in training on average (column (3)). The training taken by treated youths was also longer. In the control group, conditional on participation, youths spent 2.93/0.216=13.6 months, while in the treatment group the length of training was in average (10.83+2.93)/(0.484+0.216)=19.7 months. In contrast, the number of months spent in training started after the end of the program is very small (lower panel). In the control group, it is only 0.27 months. Importantly, the treatment effect on training participation after the program is zero. This means that control youths are not catching up after the end of the intervention. The program is not just shifting training participation over time, it is truly (and strongly) increasing youths' human capital investments.

The intervention also leads to more youths obtaining training certification (+15.1 percentage point). This is a substantial increase compared to the control group were only 9.5 percent of youths obtained certification. This is noteworthy because certification signals that general skills have been acquired. At the same time, the increase may have been expected to be even larger given the magnitude of the increase in training participation. This highlights that not all treated youths obtained certification. In part, this may be due to implementation delays in the set-up of certification mechanisms. It is also consistent with some treated youths remaining in firms as apprentice after the end of the program.

Lastly, we do not see that the increase in participation comes with an increase in the direct cost of training. This is in line with the program's intent to facilitate access.

7.2 Skill Acquisition

Table 7 documents how the program affected youths' skills through changes in an index of routine task intensity. As mentioned in Section 4.1, we collected data on the complexity of tasks undertaken by youths in their main occupation at the time of the second follow-up survey. We used the "task approach" (Autor et al., 2003) applied to measure the content of tasks realized by individual workers (Autor and Handel, 2013). Our survey module was based on a questionnaire implemented in several developing countries (Dicarlo et al., 2016). For each worker, we build an overall index of routine task intensity. The index aggregates sub-indices of routine tasks, non-routine analytical tasks, and non-routine interpersonal tasks. Table 7 presents results for the overall routine task intensity index (last column), and its various sub-indices (column 1 to 3).[32]

Results show large changes in the content of tasks realized at work by treated youths. Specifically, youths assigned to formal apprenticeships are substantially more likely to undertake non-routine analytical tasks (by 0.24 standard deviations) and non-routine interpersonal tasks (by 0.08 standard deviations). They are also slightly more likely to perform routine tasks (by 0.11 standard deviations). The overall index of routine task intensity, which aggregates across the types of tasks performed by individuals, decreases by 0.21 standard deviations. This shows that treated youths are involved in a wider range of tasks. They are particularly more likely to be involved in complex, non-routine tasks. This points to substantial improvements in skills and is consistent with the observed increase in earnings and productivity in the medium-term.

Table A10 documents disaggregated impacts for all specific tasks included in each sub-index (non-routine analytical, non-routine interpersonal and routine).[33] The table shows

[32]The overall index adds up the routine sub-index, and subtracts the non-routine (analytical and interpersonal) indices. Each sub-index is built based on a principal component analysis of several questions. Each sub-index is standardized based on the mean and standard deviations in the control group. The routine sub-index includes questions related to routine cognitive tasks, such as whether the individual undertakes tasks without autonomy (exclusively following instructions), or short, repetitive tasks. It also includes questions related to routine manual tasks, such as whether the individual undertakes tasks that require physical efforts or operating heavy equipment. The non-routine analytical sub-index captures tasks that require reading various documents (manuals, plans,...), writing documents (notes, instructions, bills,...), taking measurements or estimates, calculating prices or costs, or other tasks that require at least 30 minutes of thinking. It also includes making repairs or performing maintenance on electronic equipment. The non-routine interpersonal sub-index captures tasks such as interacting with customers, advising colleagues, teaching or supervising other workers. Table A10 lists all the questions in the survey module, organized by sub-index.

[33]The table presents the mean of each variable in the control group, the estimated coefficient as well as the corresponding p-value. We also account for multiple testing, controlling for the False Discovery Rate

that treated youths are significantly more likely to perform a range of non-routine analytical tasks, including tasks that involve reading, writing, taking measurements, performing math operations, or tasks that require thinking. This explains the large impact on the normalized sub-index of non routine analytical tasks. On the other hand, evidence of impacts on non-routine interpersonal tasks is weaker. The estimated coefficient for the sub-index is small and only significant at the 10 percent level. Moreover, among the four tasks in the list, we detect a small significant impact on only one variable (advising co-workers). The impact on the routine task sub-index is also small and only significant at the 10 percent level. We observe an increase in the use of heavy equipment and tasks related to repair and maintenance of electronic equipment, but a reduction in repetitive tasks.

Our measures of skills based on the "task approach" are not occupation-specific. This allows comparing the complexity of tasks performed by youths in the treatment and control groups even if they are engaged in different sectors. We can also decompose how much of the changes in the complexity of tasks are driven by treated youths working in different occupations than control youths. We consider the average value of a given index I over youths in the control group in each occupation "s": $\overline{I}_s^C$. We can write the Within/Between decomposition: $I_i = [I_i - \overline{I}_{s(i)}^C] + \overline{I}_{s(i)}^C = W_i + B_i$. Assessing impacts on W_i and B_i helps to disentangle whether observed changes in I are related to changes in youths' occupations, for example switching to occupations involving more abstract tasks (changes in B), or whether they are related to changes in tasks performed within sectors (W). The middle panel of Table 7 makes clear that for each sub-index the observed changes in the upper panel are partly driven by changes in occupations. Thanks to the program, youths enter occupations involving more diverse tasks, including more routine tasks, more non-routine analytical tasks and more non-routine interpersonal tasks. No impact is detected on the within component of routine tasks and non-routine interpersonal tasks. Importantly, however, there is also a significant increase in the within component of non-routine analytical tasks, showing that treated youths perform more of these tasks even within occupations. This contributes to the decrease in the overall routine task intensity index.

Overall, we find that subsidized dual apprenticeships upgrade youths' skills. Youths are engaged in more complex tasks and are also more likely to obtain certification. This suggests

using the method proposed in Benjamini-Hochberg which appears in column (4).

they they have gained general skills. The result from Section 6.1 showed that participation in apprenticeship had large opportunity costs, which the subsidy compensated. In addition, Section 7.1 did not find an increase in the direct cost for youths to participate in training. Taken together, these results thus show that the program made possible for youths to improve their general skills and become more productive, without incurring training costs.

7.3 Self-selection and Changes in the Profile of Apprentices

As we have shown, the intervention leads youths to enter apprenticeships, which some would not have done absent the program. This means that a new population of apprentices enters firms. This population can be compared to the population of traditional apprentices to document how the program affects selection into apprenticeship. This can be done by comparing baseline characteristics of treated and control youths entering any form of apprenticeship. We consider three categories of youths. First, we call Never-Apprentice (NA) youths who do not enter apprenticeship even when they are offered participation in the program. Second, the Always-Apprentice (AA) enter apprenticeship even absent the program. Lastly, the Formal-Apprentice-Only (FO) is the population of youths who enter apprenticeship only when they are offered a formal apprenticeship.[34]

Results are presented in Table A11. We use the same baseline characteristics as when checking balance between the treatment and control groups. The first column shows the mean of characteristics for Always-Apprentice (AA), and the second column the estimated mean for Formal-Apprentice-Only (FO). For completeness, the third column presents the mean for Never-Apprentice (NA). The last column contains the p-value for the test of equality in means between FO and AA groups. The variables are organized by domains: demographics, skills, employment and earnings, aspirations and jobs search, socio-economic background and financial constraints.

The conceptual framework (see Appendix A3) shows how the program affects youths' decision to enter apprenticeship in presence of financial constraints. Both the subsidy and changes in expected future earnings can induce youths with better labor market opportunities or worse borrowing conditions to enter apprenticeship. While results do not provide direct

[34]We use this terminology instead of the usual notions of "Compliers", "Always-takers" and "Never-takers" to avoid confusion with our instrumental variable estimation in Section 5.3.1.

evidence on differences in borrowing conditions or credit constraints, Table A11 provides some evidence consistent with differences in labor market opportunities. The share of FO aspiring to become self-employed is lower, and the share of FO aspiring to become wage-employed is higher. FO are also less likely not to have a diploma.

Among the 33 variable we considered, only two show differences that are significant at the 5 percent level and four at the 10 percent level. We are cautious not to over-interpret the observed differences. Also note that the experiment has limited power to detect differences in average characteristics for the population of apprentices in the control group (223 youths), and the population of apprentices in the treatment group (657 youths). Only large differences can be detected.

Importantly, however, we obtain consistent results on youth education and aspiration when comparing the profile of traditional and formal apprentices in firms at the time of the first follow-up survey (Table A12).[35] There is no indication that the program helped insert youths with more limited networks, or from more disadvantaged socio-economic backgrounds. In contrast, formal apprentices are older (by 2.10 years), and more likely to be women (by 11 percentage points) than apprentices in control firms. In addition, one of the main differences is that formal apprentices have substantially higher education level.[36] This is consistent with traditional and formal apprentices being imperfect substitutes. The program expands access by inducing more educated youths with better outside labor market opportunities to enter apprenticeships.

Panel (5) of Table A12 documents differences in apprentices' aspirations based on the follow-up apprentice survey. Results need to be interpreted with caution as they might be

[35] The first follow-up survey asks each apprentice in firms a set of questions about their background characteristics. Three main populations of youths can be compared: formal apprentices f, traditional apprentices in treatment firm tT and traditional apprentices in control firms. We use the following regression to describe differences between the various types of apprentices:

$$x = a + b_f f + b_{tT} tT + \sum_s \gamma_s 1_s + \sum_t \delta_t 1_t + u \tag{9}$$

Traditional apprentices in control firms are the excluded category. The two important coefficients in this regression are b_f and b_{tT}. The first compares the population of formal apprentices with the population of traditional apprentices in control firms, which helps document selection effects. The second parameter compares the population of traditional apprentices between treatment and control firms. This documents the characteristics of youths who were crowded out of apprenticeships by the entry of formal apprentices.

[36] Compared to traditional apprentices in control firms, formal apprentices are 48 percentage points less likely to have no education, 35 percentage points more likely to have completed primary school, and 13 percentage points more likely to have completed lower secondary school.

affected by apprentices' experience in firms and in the program. However, there is clearly a large difference in aspirations between formal apprentices and traditional apprentices. While 18.2 percent of traditional apprentices aspire to wage jobs, this proportion is higher by 23 percentage points among formal apprentices. This is consistent with differences in baseline aspirations among youths documented in Table A11, and suggests that earlier findings were not just statistical noise.

8 Value of Apprentices' Work in Firms

In the previous sections, we have shown that the program leads to an inflow of apprentices in firms. We now examine the impact of this entry on firms' activity and performance during the program. One important conjecture in the literature is that apprentices pay for training by accepting low pay. If this is the case, apprentices' contribution to firm activities would be higher than their remuneration. On the other hand, it is possible that apprentices have low pay simply because they are still unproductive.

Our experiment induces a shock on the entry of apprentices into firms. We consider the value of the work performed by apprentices as $V = f(n_a, n_f)$, where n_a represents the number of traditional apprentices and n_f the number of formal apprentices. We write the total compensation received by apprentices $W = w_a n_a + w_f n_f$. We define the net value of the work performed by apprentices as $NV = V - W = f(n_a, n_f) - w_a n_a - w_f n_f$. The impact on this net value of work writes:

$$NV(1) - NV(0) = (f'_{n_a} - w_a)(n_a(1) - n_a(0)) + (f'_{n_f} - w_f)n_f \tag{10}$$

where $n = n_a + n_f$.[37] The equation makes clear that by causing an exogenous inflow of apprentices into firms, the experiment provides information about the difference between apprentices' marginal productivity and their compensation. If apprentices are paid at their marginal productivity, we should not observe any impact on the net value of work. On the other hand, observing a positive impact is highly suggestive that apprentices are paid below their productivity.

During the short-term follow-up survey, firm owners were asked questions about each

[37]Recall that from our simple model $E(n_a(1) - n_a(0)) = -\psi n_f$, and our estimate of ψ is around 0.2.

apprentice who entered the firm since randomization. We measure how these apprentices contribute to firm activities, their hours worked, and whether they are involved in productive tasks. We also ask enterprise owners to recall the work performed by each apprentice during their last working day, and to estimate how much they would have had to pay an occasional worker to accomplish the same tasks. We then build an estimate of the value of work performed monthly by each apprentice by multiplying the value of work during the last day by the number of days worked in the last month. The survey also describes precisely the types of compensation received by each apprentice for meals, transportation, clothing and "motivation", including both in cash and in-kind payments. We sum all these components at the apprentice level to get measures of the value of work performed by apprentices V, the total compensation received by each apprentice as well as the "net value of work" NV, taking the difference between the value of work and the total compensation received.[38]

We aggregate all these apprentice-level measures in each firm across all apprentices who joined since randomization and look at impacts on those firm-level performance variables by estimating equation 4. Results are displayed in Table 8. Despite the increase in the number of apprentices entering firms (0.613, in column (1)), the increase in the number of days and hours worked during the last month are rather small (respectively 6.118, column (2) and 46.58, column (3)). The increase in the number of hours worked per additional day worked is 7.61 (46.58/6.118). This is consistent with the average in the control group (263.1/31.44=8.37). However, the additional number of days worked per additional apprentice in treatment firms is 6.118/0.613=9.98. This is below the average number of days worked by apprentices in control firms (31.44/1.570=20.02). Based on analysis at the youth level below, we will show that this is due to a lower number of days worked by formal apprentices. While participation in theoretical training in vocational centers may in part explain this, absenteeism is likely higher among formal apprentices. Yet column (4) shows that the program leads to a strongly positive and significant increase of the value of work performed by apprentices in treatment firms. The estimated value of work by apprentices increases by 32 115 FCFA per month, a significant 78 percent increase. Although the number of apprentices significantly increases, column (5) shows that the total wage bill for apprentices does not increase significantly in treatment firms. A small increase is observed, but it is not signifi-

[38]We do not have similar information at the time of the second follow-up survey, since it was designed to focus on impacts on employment and earnings among youth.

cant. As such, the impact on the net value of work (value of work minus wage bill) remains large. This increase amounts to 21 380 FCFA per month, more than doubling the net value of work performed by apprentices in control firms.[39]

Results presented in Table 8 are based on the aggregation of data across apprentices in firms. We can also look directly at differences in performance measures at the apprentice level, as well as other variables related to apprentice compensation and payments. The estimated equation is similar to Equation 9 in footnote 35:

$$y = a + b_f f + b_{tT} tT + \sum_s \gamma_s 1_s + \sum_t \delta_t 1_t + u \tag{11}$$

Coefficients b_f and b_{tT} measure the difference between formal apprentices in treatment firms, respectively traditional apprentices in treatment firms and traditional apprentices in control firms, which is the excluded category. It is worth to note that the observed differences can be due either to the treatment or to selection into formal apprenticeships. They are mainly descriptive and cannot be given a causal interpretation.

Results are displayed in Table A13. Apprenticeship arrangements involve the apprentices (or their family) paying a fee to the master craftsmen. Over time, firms start compensating apprentices, with payments divided between a regular payment to cover transport costs, room and board, and a "bonus" payment to motivate apprentices. The table shows that formal apprentices pay significantly lower fees to firms compared to traditional apprentices in treatment and control firms. This is consistent with the program's intent in subsidizing access. Employers were requested not to charge fees. In parallel, firms make lower payments to formal apprentices, and these lower payments are mostly driven by a decrease in payments for transport, room and board. The firms internalize that the program subsidy covers such costs, so they strongly reduce their contribution. This behavioral response from firms implies in turn that part of the program subsidy is in fact transmitted to firms. Formal apprentices still receive similar "bonus" payments than traditional apprentices in treatment firms. This shows that firms complement the subsidy offered by the program and attempt to directly motivate apprentices. Yet the total payments made by firms per formal apprentice remain

[39]The bottom panel of Table A7 in appendix presents the results from permutation tests for the ITT estimates on hours worked by apprentices and their net value of work in firms (last 2 columns). Asymptotic results and results from permutation tests are very close.

lower than payments made per traditional apprentice.

Lastly, a natural question is whether there are any impacts on firm profits. On the one hand, the increase in the net value of work by apprentice shown in Table 8 may contribute to higher profits. On the other hand, the firm has to provide training and this might be costly for firm activities and profits. For instance, the training of apprentices may divert master craftsmen from spending time on production, with the possibility of a negative impact on profits. For example, panel (2) of Table A13 shows that apprentices work 2.628 hours per day under the supervision of their master, out of the 8.170 hours they work daily.[40]

Measures of firm sales and profits are notoriously noisy and difficult to obtain. In the context of the study, we collected up to four measures of sales and profits, $y_{im}, \quad m \in \{1, \ldots, 4\}$.[41] We estimate impacts on revenue and profits for each measure using equation 4. We also pool all four measures and estimate the following regression, including dummies for each type of measure:

$$y_{i,m} = a + bT_i + \delta_m + v_{i,m} \tag{12}$$

Results are presented in Table A9. We do not detect any significant effect on average sales or profits. Estimated coefficients are negative, but they also have a large standard error and only one coefficient (the first measure for profit for the initial call) is significant at the 10 percent level. This lack of effect could be due in part to more limited statistical power given the dispersion of the profit variables. The increase in the value of work by apprentices may not be sufficient to increase profits given the indirect costs involved for firms to provide training. Youths indirectly compensate firms for providing training since their compensation is lower than their productivity. But the provision of training also has an opportunity cost for firms, including the opportunity cost of time of mastercraftmen. Finding zero effects on profits is consistent with these effects cancelling each others.

[40]The follow-up survey asked apprentices about the number of hours they spent working independently, under direct supervision of master trainers or watching their master. In control firms, apprentices spent on average 2.5, 2.6 and 1.7 hours during their last day of work, working independently, under the supervision of their master, respectively watching him/her (see lower panel of Table A13).

[41]As described above, we asked firm owners to directly report sales and profits (following De Mel et al. (2009)). We also asked firm owners to list all their sales from the previous months. After this exercise, we asked them to report again sales and profits, which provides a second measure. Moreover, experienced supervisors conducted near-systematic back-check of firm surveys to obtain repeat measures (for 596 out of 674 firms).

9 Conclusion

This paper analyzes the impact of subsidized dual apprenticeships based on the randomized control trial of a government program. As in many developing economies, traditional (informal) apprenticeships are common in Côte d'Ivoire. A subsidized dual apprenticeship program was put in place to expand access to apprenticeship and improve training quality. It combined wage subsidies and a dual training approach. On-the-job training in firms was complemented by theoretical training in vocational training centers. Formal apprenticeship contracts were monitored by counsellors from the national training agency. A certification scheme was also set up.

The program was tailored to address two potential market failures highlighted in the theoretical literature: financial constraints and firms' inability to commit to provide general skills training. Becker (1962) showed that firms should be expected to incur the costs of firm-specific skill training, and trainees to incur the cost of training in general skills that are transferable to other firms. For instance, trainees may accept lower earnings during the training period. While this should lead to optimal provision of training, capital market imperfections or financial constraints may prevent individuals to invest in training unless they are ready to accept a reduction of their utility during the training. The literature has also highlighted that there might be an under-provision of training in firms. In absence of comprehensive contracts, firms are not able to credibly commit to provide a certain level of training. If such commitment failures exist, individuals may not be willing to pay for training or accept lower earnings during the training period (Acemoglu and Pischke, 1999). Commitment failures can thus induce a sub-optimal level of training (Dustmann and Schönberg, 2012). Subsidized dual apprenticeships can in principle address these two market failures. On the one hand, wage subsidies can tackle financial constraints. On the other hand, dual apprenticeships can resolve commitment failures by ensuring that formal apprentices receive general skills training in vocational centers. The set-up of a certification scheme or a basic regulatory framework can also encourage youths to invest in their training (Acemoglu and Pischke, 2000; Dustmann and Schönberg, 2012).

The main results in this paper are consistent with financial constraints or commitment failures being prevalent, and with subsidized dual apprenticeships effectively addressing these market failures. We show that subsidized dual apprenticeships successfully expand access

to training, while increasing skills and earnings four years after the start of the experiment. One of our key results is that treated youths have significantly higher earnings approximately 2 years after the end of the intervention. This is important as it is rare for training programs to have such impacts on earnings. The average increase in earnings is consistent with gains in productivity. We find that youths are engaged in more complex tasks, including non-routine abstract tasks, a sign they were able to improve their human capital and technical skills. We also find that more youths obtain certification, which suggests that part of the skills acquired are general and transferable.

These positive results contrast with the rather disappointing overall evidence on training programs reported by McKenzie (2017). Our results also contrast with those obtained from a program that mostly addresses intermediation failures in access to traditional apprenticeship. In a setting close to Côte d'Ivoire, Hardy et al. (2019) show that a placement intervention only has a small effect on access to traditional apprenticeship and leads to negative effects on earnings. We show that subsidized dual apprenticeships induce large effects on entry and positive effects on earnings. The contrast between the two sets of results suggests that the most binding constraints may not be those related to intermediation inefficiencies or information asymmetries, but rather those related to capital market imperfections or firms' failures to commit to provide general skill training. Our results also complement findings in Alfonsi et al. (2019), who show that the impacts of traditional apprenticeship are concentrated in the year after the start of the intervention. We show that subsidized dual apprenticeships combining theoretical training and a certification scheme with on-the-job training have impacts on earnings four years after the start of the intervention. This suggests that the dual approach can help overcome some limitations of traditional apprenticeships.

Our double-sided randomized control trial was designed to identify indirect effects in firms, including substitution effects. The results confirm that the program leads to a net entry of apprentices in firms. Substitution effects are observed, but they are small in magnitude. This shows that the intervention expands overall access to apprenticeship and increases the net number of positions in firms. There were vacancies unfilled at the firm level, with room to increase the provision of apprenticeship training without displacing traditional apprentices. As such, the results do not support concerns that the program is purely redistributive and only replaces informal apprentices with formal apprentices.

Lastly, the entry of new apprentices in treatment firms is associated with an increase in the net value of work provided by apprentices in these firms during the program. The gap between the productivity of apprentices and how much they are paid is informative. It shows that part of the subsidy that offsets youths' forgone earnings compensate firms for the training provided.

Generalization to other settings requires careful consideration. Results are likely to have broad relevance in developing country contexts where a large number of jobs are concentrated in small informal firms with limited technology and where traditional apprenticeships are common. These are contexts where capital market imperfections are widespread and formal contracts rare so that commitment failures can arise. In such contexts, the study has important policy implications in showing that subsidized dual apprenticeships can expand access to training and improve skills. Apprenticeship systems may be upgraded to increase incentives for youths to participate, in a way that is beneficial for individuals over the medium-term.

The findings open several areas for future research. A first question relates to the time path of impacts over the long-term. Our medium-term results are measured four years after the start of the experiment, a time frame similar to Hardy et al. (2019) or Alfonsi et al. (2019). Longer-term follow-up of these study cohorts will be informative to assess overall cost-effectiveness. There are also additional questions that would be worth addressing. In this paper, we cannot formally disentangle the relative effect of the wage subsidies and the dual training approach. In future research, it would be interesting to directly compare dual apprenticeships without subsidies, or wage subsidies without the dual approach. It is not clear whether an increase in wages alone would be sufficient to attract youths without a commitment to ensure the provision of general and transferable skills. Vice versa, the subsidy can attract youths that would not enter apprenticeship without the subsidy even in the absence of credit market imperfections. Lastly, the identification of characteristics of youths who self-select into apprenticeship when the features of the apprenticeship contract vary is an important topic for future research. The introduction of mechanisms to label trainers' quality or adjustments in regulatory frameworks could also be considered for additional testing.

References

Abadie, A. (2003): "Semiparametric instrumental variable estimation of treatment response models," Journal of Econometrics, 113, 231–263.

Abebe, G. T., S. Caria, M. Fafchamps, P. Falco, S. Franklin, and S. Quinn (2018): "Anonymity or Distance? Job Search and Labour Market Exclusion in a Growing African City," CEPR Discussion Paper No. DP13136.

Abel, M., R. Burger, and P. Piraino (forthcoming): "The Value of Reference Letters: Experimental Evidence from South Africa," American Economic Journal: Applied Economics.

Acemoglu, D. and D. Autor (2011): "Chapter 12 - Skills, Tasks and Technologies: Implications for Employment and Earnings," Handbook of Labor Economics.

Acemoglu, D. and J.-S. Pischke (1998): "Why do firms train? Theory and evidence," The Quarterly Journal of Economics, 113, 79–119.

——— (1999): "The structure of wages and investment in general training," Journal of Political Economy, 107, 539–572.

——— (2000): "Certification of training and training outcomes," European Economic Review, 44, 917–927.

Akram, A. A., S. Chowdhury, and A. M. Mobarak (2017): "Effects of Emigration on Rural Labor Markets," National Bureau of Economic Research Working Paper 23929.

Alfonsi, L., O. Bandiera, V. Bassi, R. Burgess, I. Rasul, M. Sulaiman, and A. Vitali (2019): "Tackling Youth Unemployment: Evidence from a Labor Market Experiment in Uganda," Working Paper eopp64, Suntory and Toyota International Centres for Economics and Related Disciplines, LSE.

Almeida, R., L. Orr, and D. Robalino (2014): "Wage subsidies in developing countries as a tool to build human capital: Design and implementation issues," IZA Journal of Labor Policy, 3, 12.

Angelucci, M. and G. De Giorgi (2009): "Indirect effects of an aid program: how do cash transfers affect ineligibles' consumption?" American Economic Review, 99, 486–508.

Attanasio, O., A. Guarín, C. Medina, and C. Meghir (2017): "Vocational Training for Disadvantaged Youth in Colombia: A Long-Term Follow-Up," American Economic Journal: Applied Economics, 9, 131–43.

Attanasio, O., A. Kugler, and C. Meghir (2011): "Subsidizing vocational training for disadvantaged youth in Colombia: Evidence from a randomized trial," American Economic Journal: Applied Economics, 3, 188–220.

Autor, D. H. and M. J. Handel (2013): "Putting Tasks to the Test: Human Capital, Job Tasks, and Wages," Journal of Labor Economics, 31.

Autor, D. H., F. Levy, and R. J. Murnane (2003): "The Skill Content of Recent Technological Change: An Empirical Exploration," The Quarterly Journal of Economics, 118, 1279–1333.

Baird, S., J. A. Bohren, C. McIntosh, and B. Özler (2018): "Optimal Design of Experiments in the Presence of Interference," The Review of Economics and Statistics, 100, 844–860.

Bandiera, O., R. Burgess, N. Das, S. Gulesci, I. Rasul, and M. Sulaiman (2017): "Labor markets and poverty in village economies," The Quarterly Journal of Economics, 132, 811–870.

Bassi, V. and A. Nansamba (2018): "Screening and Signaling Non-Cognitive Skills: Experimental Evidence from Uganda," USC-INET Research Paper 18-19.

Becker, G. (1962): "Investment in human capital: a theoretical analysis," Journal of Political Economy, 70, 9–49.

Bertrand, M., B. Crépon, A. Chuan, R. Haget, M. Mahoney, D. Murphy, and K. Takavarasha (2013): "J-PAL Youth Initiative review paper," Abdul latif jameel poverty action lab.

Bertrand, M., B. Crépon, A. Marguerie, and P. Premand (2017): "Contemporaneous and Post-Program Impacts of a Public Works Program," World bank working paper.

Blattman, C. and L. Ralston (2015): "Generating employment in poor and fragile states: Evidence from labor market and entrepreneurship programs," Ssrn working paper no. 2622220.

Card, D., J. Kluve, and A. Weber (2018): "What works? A meta analysis of recent active labor market program evaluations," Journal of the European Economic Association, 16, 894–931.

Carranza, E., R. Garlick, K. Orkin, and N. Rankin (2019): "Job search and hiring with two-sided limited information about workseekers' skills," Duke university working paper.

Cho, Y., D. Kalomba, A. M. Mobarak, and V. Orozco (2013): "Gender differences in the effects of vocational training: Constraints on women and drop-out behavior," World Bank Policy Research Working Paper 6545.

Christiaensen, L. and P. Premand (2017): Cote d'Ivoire Jobs Diagnostic, World Bank, Washington DC.

Crépon, B., E. Duflo, M. Gurgand, R. Rathelot, and P. Zamora (2013): "Do labor market policies have displacement effects? Evidence from a clustered randomized experiment," The Quarterly Journal of Economics, 128, 531–580.

Cunha, J. M., G. De Giorgi, and S. Jayachandran (2018): "The price effects of cash versus in-kind transfers," The Review of Economic Studies, 86, 240–281.

de Mel, S., D. McKenzie, and C. Woodruff (2019): "Labor Drops: Experimental Evidence on the Return to Additional Labor in Microenterprises," American Economic Journal: Applied Economics, 11, 202–35.

De Mel, S., D. J. McKenzie, and C. Woodruff (2009): "Measuring microenterprise profits: Must we ask how the sausage is made?" Journal of Development Economics, 88, 19–31.

Dicarlo, E., S. Lo Bello, S. Monroy-Taborda, A. M. Oviedo, M. L. Sanchez Puerta, and I. V. Santos (2016): "The skill content of occupations across low and middle income countries: evidence from harmonized data," Iza discussion paper.

Dustmann, C. and U. Schönberg (2012): "What makes firm-based vocational training schemes successful? The role of commitment," American Economic Journal: Applied Economics, 4, 36–61.

Fazio, M. V., R. Fernández-Coto, and L. Ripani (2016): Apprenticeships for the XXI century: a model for Latin America and the Caribbean?, Inter-American Development Bank, Washington DC.

Filmer, D., L. Fox, K. Brooks, A. Goya, T. Mengistae, P. Premand, D. Ringold, S. Sharma, and S. Zorya (2014): Youth employment in sub-Saharan Africa, World Bank, Africa Development Series.

Frazer, G. (2006): "Learning the master's trade: apprenticeship and human capital in Ghana," Journal of Development Economics, 81, 259–298.

Frölich, M. and B. Melly (2013): "Unconditional quantile treatment effects under endogeneity," Journal of Business & Economic Statistics, 31, 346–357.

Hardy, M. and J. McCasland (2015): "Are small firms labor constrained? experimental evidence from ghana," Working paper.

Hardy, M. L., I. M. Mbiti, J. L. Mccasland, and I. Salcher (2019): "The Apprenticeship-to-Work Transition: Experimental Evidence from Ghana," World bank policy research working paper no. 8851.

ILO (2012): Upgrading Informal Apprenticeship, ILO, Geneva.

Imbens, G. W. and C. F. Manski (2004): "Confidence intervals for partially identified parameters," Econometrica, 72, 1845–1857.

Imbens, G. W. and D. B. Rubin (2015): Causal inference in statistics, social, and biomedical sciences, Cambridge University Press.

McKenzie, D. (2017): "How effective are active labor market policies in developing countries? a critical review of recent evidence," The World Bank Research Observer, 32, 127–154.

Moffitt, R. A. (2001): "Policy interventions, low-level equilibria, and social interactions," Social Dynamics, 4, 6–17.

OECD/ILO (2017): Engaging Employers in Apprenticeship Opportunities, OECD Publishing.

UNESCO (2015): Delivering TVET through Quality Apprenticeships, UNESCO Publication.

Walther, R. (2008): Towards a renewal of apprenticeship in West Africa, Agence Française de Développement (AFD), Paris.

Wolter, S. and P. Ryan (2011): "Chapter 11 - Apprenticeship," Handbook of the Economics of Education.

Table 1: Order of Magnitude for the Size of the Experiment

(1) Share of apprentices among urban youths in study localities	8.15%
(2) Total population in study localities	1069804
(3) Yearly inflow of youths starting apprenticeship in study localities	6670
(4) Registered youths assigned to treatment	910
(5) Registered youths assigned to treatment effectively starting apprenticeship	662
(6) Experiment size ratio	9.9%

See Table A1 in Appendix for details.
Row (1) from 2013 national employment survey. Row (2) from 2014 national census. Row (3): (1)*(2)*0.2295/3 (where 0.2295 is the share of youth aged 15-24 in the population and 3 is the median duration of an apprenticeship). Row (6): (5)/(3).

Table 2: Entry into apprenticeship

	Youth			Firm		
	Formal	Traditional	Total	Formal	Traditional	Total
In apprenticeship at midline survey						
Treated	0.490***	-0.125***	0.365***	0.787***	-0.174	0.613***
	(0.018)	(0.014)	(0.022)	(0.065)	(0.149)	(0.172)
Mean	0.018	0.161	0.179	0.058	1.512	1.570
Started since randomization						
Treated	0.712***	-0.185***	0.528***	1.398***	-0.318*	1.080***
	(0.016)	(0.016)	(0.021)	(0.096)	(0.178)	(0.208)
Mean	0.038	0.225	0.263	0.188	1.942	2.130

Youth first follow-up survey (1661 observations) and firm follow-up survey (674 observations)

Left panel: Estimation of equation 5 for youth (White-Huber robust standard errors in parenthesis). See Appendix A2 for definition of youth apprenticeship variables.

Right panel: Estimation of equation 4 for firms (White-Huber robust standard errors in parenthesis).

Table 3: Overall impact on number of apprentices

	Youth side					
	Reduced form		Instrumental Variable			
	Total (a)	Formal (b)		Total (c=a/b)		Windfall (d=1-c)
Treated	0.528^{***}	0.712^{***}	$1-\omega$	0.741^{***}	ω	0.259^{***}
	(0.021)	(0.016)		(0.022)		(0.022)
	Firm side					
	Reduced form		Instrumental Variable			
	Total (a)	Formal (b)		Total (c=a/b)		Substitution (d=1-c)
Treated	1.080^{***}	1.398^{***}	$1-\psi$	0.773^{***}	ψ	0.227^{*}
	(0.208)	(0.096)		(0.128)		(0.128)

Sources: Youth first follow-up survey (1661 observations) and firm follow-up survey (674 observations)

Column (a) and (b) present ITT estimates of equations 5 (upper panel) and 4 (lower panel). Column (c) presents IV estimates of equations 6 (upper panel) and 7, which is simply the ratio of column (a) to (b). Column (d) presents estimates for parameters ω and ψ, as obtained from the third column. The outcome variables are: entry into formal apprenticeship and entry into any apprenticeship since randomization (upper panel), and total number of formal apprentices and total number of apprentices of any type who entered firms since randomization (lower panel).

White-Huber robust standard errors in parenthesis.

Table 4: Youths' Activities, Hours and Earnings at Midline

Activities							
	Apprentice	Wage empl.	Self-empl.	Other activities	Total # activities	At least one	
Treated	0.365***	-0.135***	-0.129***	-0.0138	0.0526*	0.0336***	
	(0.0216)	(0.0222)	(0.0236)	(0.0107)	(0.0310)	(0.0128)	
Mean	0.179	0.356	0.471	0.0564	1.191	0.910	
Hours							
	As an apprentice	As wage empl.	As self-empl.	In other activities	Total		
Treated	18.20***	-6.462***	-7.692***	-0.418	3.687**		
	(1.170)	(1.235)	(1.302)	(0.401)	(1.492)		
Mean	7.558	15.10	17.62	1.716	41.93		
Earnings							
	Apprentice	Wage empl.	Self-empl.	In other activities	Total Labor	Non-labor[a]	Total
Treated	3238***	-6414***	-6381***	-168	-10494***	10213***	-1408
	(749)	(1407)	(2157)	(222)	(2654)	(870)	(3295)
Mean	4746	15398	19089	800	41776	7540	51484

Source: Youth first follow-up (midline) survey (1661 observations).

Notes: The three panels present ITT estimates of equation 5 for outcome variables related to occupation, hours worked and earnings. See Appendix A2 for definitions of apprenticeship variables. White-Huber robust standard errors in parenthesis.

a - Includes the program stipend

Table 5: Youths' Activities, Hours and Earnings at Endline

Activities							
	Apprentice	Wage empl.	Self-empl.	Other activities	Total # activities	At least one	
Treated	0.0937***	-0.0363	0.0148	0.00751	0.0823***	0.0125**	
	(0.0200)	(0.0241)	(0.0248)	(0.0129)	(0.0304)	(0.00562)	
Mean	0.172	0.378	0.461	0.0732	1.234	0.982	
Hours							
	As an apprentice	As wage empl.	As self-empl.	In other activities	Total		
Treated	4.436***	-2.600*	1.058	0.178	1.457		
	(0.986)	(1.358)	(1.306)	(0.524)	(1.346)		
Mean	6.681	16.61	12.66	2.524	41.19		
Earnings							
	Apprentice	Wage empl.	Self-empl.	In other activities	Total Labor	Non-labor	Total
Treated	3593***	544	4512*	570	8987**	551	9394**
	(1004)	(2133)	(2711)	(455)	(3548)	(1078)	(3928)
Mean	5770	20650	19350	1529	53029	8925	62747

Source: Youth second follow-up (endline) survey (1670 observations).

Notes: The three panels present ITT estimates of equation 5 for outcome variables related to occupation, hours worked and earnings. See Appendix A2 for definitions of apprenticeship variables. White-Huber robust standard errors in parenthesis.

Table 6: Impact on Training Participation

	Participation in Training			Certification	Cost
	At least one	Number	Months	Number	(1000s FCFA)
	(1)	(2)	(3)	(4)	(5)
Before the end of program					
Treated	0.484***	0.509***	10.829***	0.151***	-5.671
	(0.022)	(0.025)	(0.528)	(0.019)	(4.071)
Mean	0.216	0.234	2.934	0.095	16.229
Since the end of program					
Treated	-0.004	-0.011	-0.033	-0.003	-0.378
	(0.010)	(0.012)	(0.071)	(0.006)	(1.003)
Mean	0.046	0.053	0.266	0.015	1.796

Source: Youth second follow-up (endline) survey (1670 observations).
The table distinguishes between training that started before the end of the program, and after the end of the program. The start time of training was defined based on data from a calendar of training collected during the second follow-up survey. The training considered includes on-the-job training in firms, technical and vocational training (TVET) in training centers, and training combining both.
Estimation of equation 5 with robust standard errors.

Table 7: Impact on skills at endline

	Routine (R)	Non-Routine Analytical (NRA)	Non-routine Interpersonal (NRI)	Routine Task Intensity Index (RTI)
Treated	0.110**	0.243***	0.0793*	-0.213***
	(0.0472)	(0.0501)	(0.0472)	(0.0802)
Between sector of occupation				
Treated	0.151***	0.107***	0.0814***	-0.0374
	(0.0244)	(0.0229)	(0.0166)	(0.0304)
Within sector of occupation				
Treated	-0.0410	0.139***	-0.00497	-0.175**
	(0.0448)	(0.0465)	(0.0451)	(0.0779)

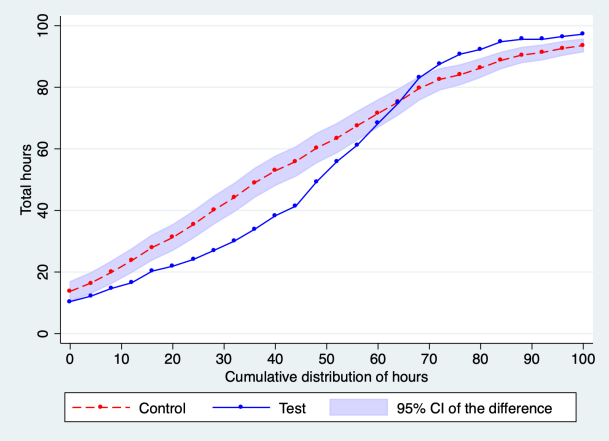
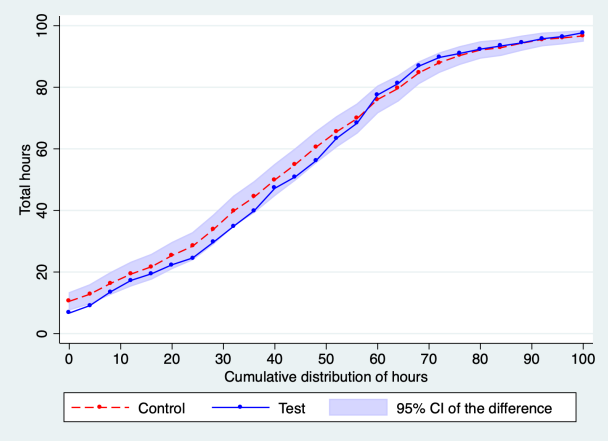
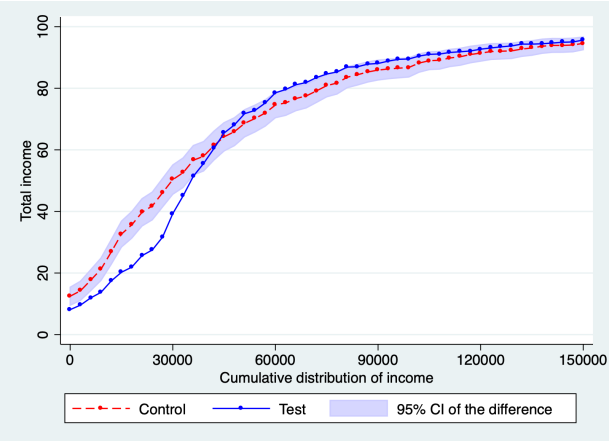
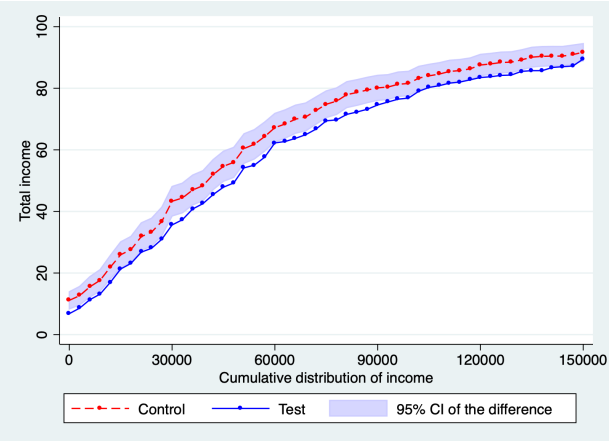
Source: Youth second follow-up (endline) survey (1670 observations).
Measures based on tasks realized by youths in their primary occupations, based the approach in Autor et al. (2003), Autor and Handel (2013) and Dicarlo et al. (2016). See Table A10 for impacts on specific tasks within each sub-index.
RTI index = R - NRA - NRI.
Estimation of equation 5 with robust standard errors.

Table 8: Apprentices' participation in firms' activities

	# apprentices (1)	# days of work (2)	# hours of work (3)	Value of work (4)	Wage Bill (5)	Net value of work (6)
Treated	0.613***	6.118*	46.58	32.12***	4.391	21.38***
	(0.172)	(3.515)	(29.10)	(9.511)	(3.246)	(5.932)
Mean	1.570	31.44	263.1	41.26	22.09	19.23

Source: Follow-up firm survey (674 observations).
ITT estimates from equation 4 for outcome variables related to apprentice participation in firm activity (White-Huber robust standard errors in parenthesis). The variables are first defined at the apprentice level and then aggregated at the firm level across all apprentices who started after the randomization date.
column (4) and (5) in 1000s FCFA.

Figure 3: Distribution of potential outcomes for hours worked and income

Midline: Hours	Endline: Hours
	
Mann Whitney test: p values from 10,000 permutations (within strata, see notes for details).	
$p = 0/10000$	$p = 2166/10000$
Midline: Income	Endline: Income
	
Mann Whitney test: p values from 10,000 permutations (within strata, see notes for details).	
$p = 58/10000$	$p = 5/10000$

Source: Youth midline (1661 observations) and endline surveys (1670 observations).
Notes: The figures show the results of the estimation of the cumulative distribution of potential outcomes in the two assigned groups. They are based on the estimation of equation 5, with variables defined as $1(y < t)$ for t varying over the support of y. The red curve shows the average of those variables in the control group. The blue curve adds to this average the estimated treatment coefficient. The grey area around the red curve represents a band of +/- 1.96 times the standard error of the estimated treatment effect.
The intermediate panel presents the result of the Mann-Whitney rank test implemented using 10,000 permutations within randomization strata. The p-value is the ratio of the number of times the statistic from a permuted assignment variable was found larger than the statistic obtained from the true assignment variable to the total number of permutations.

A1 Experimental Design and Implementation

A1.1 Youth and Firm Registration in the Experiment

The program was implemented in 7 urban areas in the interior of the country.[42] In each locality, AGEFOP worked with private sector organizations (such as chambers of commerce or trade associations) to identify firms interested in hosting formal apprentices. For each firm, the number of available apprenticeship positions was collected. AGEFOP staff then systematically visited all firms to explain the program, to check each firm's ability to train apprentices and to confirm the number of apprenticeship positions they could offer. A baseline firm survey was implemented right after the collection of apprenticeship positions. Once all positions were identified in a given locality, they were grouped and advertised by trade.[43] Youths between 18 and 24 years old were then invited to visit a central location in each locality to apply for apprenticeship positions in available trades.[44] They filled an application form and indicated the trade they were interested in. The program targeted low-skilled youths, but did require an ability to read and write to ensure youths could participate in the theoretical training, de facto implying that youths had at least a few years of schooling. Youths who met basic eligibility criteria were invited to an interview with AGEFOP apprenticeship counselors. The interview sought to confirm youths' motivation for doing an apprenticeship as well as their choice of trade. A baseline survey was implemented among all youths who successfully passed the interview.[45] This registration process led to

[42]These included Man (35% of youths in the sample), Daoukro (15%), Gagnoa (14%), Divo (12%), Bouaké (12%), Adzopé (7%) and Mankono (5%). It was planned that Abidjan would also participate in the experiment, but demand for apprenticeship positions among youth was limited and there was not enough oversubscription, so that Abidjan had to be dropped from the sample. The program was launched between July 2014 (Adzopé), August 2014 (Daoukro, Gagnoa, Man and Mankono), September 2014 (Divo) and October 2014 (Bouaké). A target number of youths to include in the program was set for each locality based on the estimated number of available apprenticeship positions and other considerations.

[43]Throughout the paper, we make a distinction between "sectors" and "trades". Sectors refer to the activity of the firm and "trades" refer to jobs taught to youth. The two concepts are often the same, but in some cases firms in a given sector are active in several trades. An example is the garage sector, which includes apprenticeship positions in several trades: coach builder, car mechanic, car electrician, or car painter.

[44]The most popular trades included car or motor mechanic (21% of positions), metalworker, boilermaker, welder (14%), bricklayer, painter, plumber (11%), carpenter (9%), car electrician (9%), electrician (8%), coach-builder (8%), repairman for fridges and freezers (7%).

[45]Despite the efforts made to advertise the program, it was not possible to find enough interested youths in some trades in some localities. In such cases, a rationing occurred at the firm level: the number of positions to be filled was reduced proportionally, while ensuring as much as possible that firms would keep at least one open position. In a few cases, this was not possible and some firms had to be randomly excluded, even though they had been initially registered and surveyed.

the same number of eligible youths than open positions, in each locality and in each trade.

A1.2 Randomization

In each locality, following the interviews, a double-sided randomization protocol was implemented. The procedure was the following: firms were paired according to the number of positions they opened per trade, and within each pair a firm was assigned to treatment and another to control. The reason for implementing this pairing procedure, instead of a theoretically more appealing stratification by trade, is that some firms opened positions in different trades (see footnote 43). Once the firm randomization was implemented, the number of open positions in treatment firms was counted by trade. This gave the exact number of youths to select. The next step of the randomization was then implemented, randomly assigning youths to treatment by trade. We assigned the exact same number of youths to treatment as the number of open positions to fill. As a result, the probability of youth assignment to treatment is trade-specific. On the side of youth, the experiment is thus stratified by locality (since the randomization procedure was implemented separately in each locality) and by trade. Since assignment probabilities are strata-specific, we include strata-specific weights in the specification used to estimate impacts on youth, as discussed further below. Figure A1 displays the distribution of this ratio by stratum, showing as expected a strong concentration around 0.5. As can be seen from the figure, there are a few cases in which the assignment ratio is either 0 or 1. This case arises 10 times and is related to the fact that firms offer a portfolio of positions in different trades. As we draw firms, in some cases this can lead to all the position in a small trade being assigned to control or all to treatment. In these rare cases, the corresponding micro market is not included in the youth analysis.[46]

Once apprenticeship positions to be filled were selected, and youths were selected in each locality, AGEFOP counsellors matched selected youths to selected firms with open positions in the same trade. The matching took place based on criteria such as distance between the firm and youths' residence. Once assigned to firms, youths passed a medical visit and were invited to sign a contract and start their apprenticeship.[47]

[46] When a small number of positions is offered in some trades, and when those positions are offered together with positions in other trades, the firm randomization process can lead to all the positions in a given trade assigned to treatment or to control. We kept the firms in the data set, but the corresponding youths were not included in the sample for youth regressions.

[47] This is a demanding experimental protocol requiring a lot of specific actions and close coordination

A1.3 Size of the Experiment

Across the 7 localities covered by the study, 731 firms offered apprenticeship positions and 1,842 young applicants were eligible and passed the motivation interview. Approximately half the firms (361), were randomly selected to host program apprentices. 911 eligible and motivated youths were assigned to the program and 921 to the control group.[48] Most firms offered several positions, and on average treatment firms were assigned 2.52 apprentices.

Our experiment has the characteristic that the share of treated youths in the apprenticeship market is small. Using information from a recent population census and a national employment survey, Table A1 shows the estimated share of youths starting apprenticeships in the treatment group relative to the number of youths entering apprenticeships in the study localities. The order of magnitude is less than 10%.

To build Table A1 we start by using data from the 2013 national employment survey, collected in February 2014. The data are representative at the district level for urban and rural areas (12 districts with urban and rural areas, plus Abidjan, for a total of 25 strata). The 7 study localities (column 2 in Table A1) are located in 6 districts (column 1). We estimate the share of youths aged 15-24 who are apprentices in urban areas of these districts. Column 3 provides the share of all youths who are apprentices. We then use data from the 2014 national census to obtain the total population of the locality. We estimate the total population of youths aged 15-24. 45.9% of the national population is aged between 15 and 34. We estimate that youths aged 15-24 constitute half this share (23%, a lower bound). We then estimate the total number of youths aged 15-24 entering apprenticeship per locality (column 5). We do so by dividing the share of youths in apprenticeship by the median duration of apprenticeship in the survey (3 years). Column (6) provides the total number of treated youths in the experiment, column (7) the total number of treated youths who effectively started apprenticeship, and column (8) the ratio of treated youths starting apprenticeship over the total number of youths in apprenticeships per locality. This proportion varies, but is relatively small on average. In Bouake, a large city, treated youths only represent 4.1% of the

with the implementation agency in a short period of time. We had a team of three highly skilled research associates based in the field, as well as a full data collection team implementing baseline surveys. Once the experimental protocol was implemented in a given locality, a detailed summary report was written to list all the specific implementation aspects. For example, the report registered the initial number of positions offered in each trade, the number of firms involved, as well as any rationing that occurred and the number and identity of any firm randomly excluded from the experimental protocol.

[48] 10 youths were dropped due to the special case mention in footnote 46.

population of youths entering apprenticeships. In contrast, in small localities like Mankono or Daoukro, the share is 31%, respectively 35.2%. On average, treated youths represent less than 10% of the population of youths entering apprenticeship in the study localities.

A2 Definition of Apprenticeship and Training Variables

A2.1 Administrative Data and Process Evaluation Survey Data

The formal apprenticeship program we study is part of the PEJEDEC project, but was implemented by AGEFOP, the national training agency. AGEFOP also runs a smaller, similar but independent program in some localities.

Table A4 provides information about the take-up of formal apprenticeship for youths assigned to the treatment group. The results are based on a short process evaluation survey collected to assess quality of program implementation among treated youths and firms (column 1) and from the program administrative data (column 2).

The process evaluation took place in September 2015, between the baseline and first follow-up surveys, and on average 12 months into the program. The process evaluation survey asked youths several questions to understand take-up and the timing of potential drop-outs. Youths dropped out at various points. 83.4 percent of the overall sample of treated youths signed a contract, and 74.7 percent report that they started an apprenticeship.[49] Table A4 also documents dropouts within 12 months of the start of the apprenticeships, by the time of the process evaluation survey, showing that drop-out was substantial. 61 percent of youths in the treatment group were still in formal apprenticeships. This implies a dropout rate of 18.3 percent among youths who started apprenticeships.

As discussed in Section 5.1, the dropout rate measured from the follow-up survey is substantial. Around 30 percent of youths from the treatment and the control groups dropped out in the first year of the program. These figures are consistent with most drop-out taking place early in the program.

The administrative dataset provides information consistent with the process evaluation

[49]There are several reasons for imperfect take-up. 11.5 percent of selected youths could not be re-contacted by the implementing agency. An additional 5.1 percent of youths were contacted but did not sign the contract. This can be considered as early dropout and might be due to imperfections in the process of matching youths to firms. Finally, 8.7 percent of youths report having signed a contract but did not start the apprenticeship.

survey. It shows that 72 percent of youths in the treatment group signed a contract and started their apprenticeship. The administrative data also contains additional information on youths having completed the program. It shows that 53.2 percent of youths in the treatment group (or 73.5 percent of those who started apprenticeships) completed the full program, while 19.1 percent of the treatment group (or 26.5 percent of those who started apprenticeships) dropped-out before the end of their contract.

A2.2 Measures of Apprenticeship and Program Participation

We now detail how we measure participation in apprenticeship and in the program at the moment of the follow-up survey and over the course of the experiment. We then discuss how we measure the number of formal apprentices in firms.

A2.2.1 Youth participation at the time of the first follow-up survey

Our follow-up surveys have a detailed module about youth primary and secondary activities. As part of this module, youths are asked about their employment status, which includes apprenticeship as an option. They are then asked about their hours of work and earnings in each activity. Our analysis of youths' occupation, hours and earnings is based on this module.

To define participation in formal apprenticeship, we combine the measure of youths' occupation as an apprentice with information on participation in formal apprenticeship programs delivered by government institutions. This is based on a question about participation in training and government-supported programs. The list of formal apprenticeship programs include the PEJEDEC subsidized dual apprenticeship program, as well as a smaller program offered by AGEFOP. The two programs can at times be hard for youths to distinguish since the PEJEDEC program was also implemented by AGEFOP. We build a formal apprenticeship variable for youths who report being enrolled in formal apprenticeship supported by either PEJEDEC or AGEFOP.

Importantly, our measure of participation in formal apprenticeship is consistent with other available administrative data. Overall, from Table 2, 50.8 percent of youths in the treatment group participate in formal apprenticeship. Appendix Table A4 shows that 53.2% of youths assigned to treatment finished the program.

Table A5 (top panel) presents additional results supporting the definition of the formal apprenticeship variable. It analyzes how to combine answers about enrollment in government-supported formal apprenticeship programs. Column (1) documents treatment effects on participation in any type of formal apprenticeship provided by PEJEDEC or AGEFOP. Columns (2) and (3) disaggregate results between youths who report participating in the PEJEDEC program and youths who report participating in the AGEFOP program only. Column (4) and (5) show the symmetrical results for youths who report participating in the AGEFOP program and youths who only report participating in the PEJEDEC program. The table clearly shows that youths assigned to the program did not make a clear-cut connection with PEJEDEC. Column (3) shows that some youths considered that it was only an AGEFOP program. Column (5) shows that few youths considered that it was only a PEJEDEC program. This is understandable since the PEJEDEC program was delivered by AGEFOP, and it justifies the choice of our broad definition of participation in formal apprenticeship.

The subsidized apprenticeship program lasted 24 months for most youths. However, formal apprenticeships lasted only 12 months in some trades, for example in “construction” or "hotel and restaurant". 18% of youths in the experimental sample registered in such trades. The survey was collected approximately 21 months after the program started, so that these youths had already left the program. This does not affect answers about participation in government-supported programs. However, it affects current employment status for treated youths who left their occupation as an apprentice after the program ended. The lower part of Table A5 reports current participation in formal apprenticeship for youths who applied in trades for which formal apprenticeships last 24 months. While the control mean is largely unchanged between the top and bottom panel, treatment effects on participation are slightly larger in the bottom-panel (by no more than 5 percentage points). This is consistent with the finding in Section 6.2 showing that some youths remained in apprenticeship after the end of the program.

A2.2.2 Youth participation over the full period of the experiment

We now consider youth participation in apprenticeship over the full period between the start of the experiment and the date of the first follow-up (midline) survey. This measure

includes some youths who started an apprenticeship (traditional or formal) but dropped-out before the date of the first youth follow-up (midline) survey. Our measure builds on the same variable about youth enrollment in government-supported formal apprenticeship programs discussed in the previous section. Our measure also builds on another question about youth participation in apprenticeship or training (such as TVET) since the start of the experiment. Answers about TVET and apprenticeship programs are mutually exclusive. Youths involved in apprenticeships with dual practical and theoretical training also at times confused whether it was an apprenticeship or TVET. We define as "formal apprentice" any youth who reported being involved in the PEJEDEC or AGEFOP program, and reported being in either apprenticeship or TVET.

Table A6 presents results supporting the choice of the definition of participation in formal apprenticeship. In the first column (Governmental Program), we consider the answer to the question about participation in a government program such as PEJEDEC (in the first panel), AGEFOP (in the second panel), and any of the two (in the third panel). The second column considers a boolean variable for youth answering they have been involved in apprenticeship and each government program. The third column does the same using the TVET variable instead of the apprenticeship variable. Last, the third column shows results when considering the apprenticeship and TVET variables together.

Results show that youths at times mis-classified their participation in the AGEFOP and PEJEDEC programs: when we define GP as participation in the AGEFOP program, the estimated treatment effect is large. We thus consider both programs together (third panel). Many youths also considered dual apprenticeship as TVET. The treatment effect for the second and third columns are of a similar order of magnitude. Based on this, we thus define participation in formal apprenticeship as youths answering they enrolled in any governmental programs (AGEFOP or PEJEDEC) and participated in either TVET or apprenticeship training.

Importantly, the entry rate in formal apprenticeship measured from the survey is consistent with administrative data. Overall, the measure we consider is reported in the lower panel of Table A6, which shows that 75% of youths in the treatment group are found to participate in formal apprenticeship (broadly defined). Administrative data provides a share of 72.32% (see Table A4).

A2.2.3 Measurement of the number of apprentices in firms

One key aspect of our firm survey is that employers were asked to list all apprentices working in the firm at the moment of the survey. This is module D of the survey. Employers were also asked to list all apprentices who have worked in the firm but left the firm between the start of the experiment and the date of the survey. This is module F of the survey. For each apprentice listed in module D and module F, employers are asked a series of question including the date when each apprentice started of an apprenticeship and the channel through which they were recruited. This enables us to measure the inflow of apprentices into firms since the start of the experiment. We can count apprentices working in each firm at the moment of the survey (based on module D). We can also build a measure capturing the inflow of apprentices into firms since the start of the experiment (based on module F). When reporting how firms recruited apprentices, employers can select "PEJEDEC". We use this variable to identify formal apprentices, either for apprentices in firms at follow-up (based on module D) or for the inflow of apprentices into firms over the course of the experiment (based on module F).

A2.2.4 Consistency between measures from youth and firm surveys

In a perfect setting, we would expect to find a similar number of formal apprentices in the youth and firm surveys. However, direct comparison between the two data sources is complicated for various reasons. First, there is attrition among youths and firms. For now, let us assume that attrition is random. Second, on the side of youths, there is (small) non-compliance and the measure of participation in formal apprenticeships include both AGEFOP and PEJEDEC programs. As discussed above, this is because the two programs are not easy to distinguish for youths. The underlying assumption is that apparent non-compliance on the youth side is related to our broad measure, not to some control youths participating in the PEJEDEC program. Based on this and Table 2, we would expect a rate of youth participation in formal apprenticeship of 0.49 at the time of the follow-up survey, or 0.712 over the course of the experiment.

Third, the analysis also reported non-compliance on the side of firms. Let us assume this is "true" non-compliance, whereby all youths reported as formal apprentices in treatment and control firms are truly formal apprentices. Under this assumption, the rate of participation

in formal apprenticeship on the side of firms can be computed as the average number of formal apprentices in treatment and control firms:

$$(\Delta + M) * 361 + M * 370)/911$$

Δ is the impact on the total number of formal apprentices, M is the control mean, 361 and 370 are the number of firms in treatment, respectively the control group, and 911 is the number of youths assigned to treatment.

Based on these assumptions and the results in Table 2, the estimated ratios are 0.358, respectively 0.705. We obtain the right order of magnitude for the flow of apprentices over the course of the experiment (0.705 compared to 0.712). However, participation in formal apprenticeship estimated on the side of firms at the moment of the follow-up survey (0.358) is below the participate rate on the side of youths (0.49). There are various potential explanations for this difference. First, the youth and firm surveys were not collected exactly at the same time. Firm surveys were collected roughly one month after the youth survey. Second, the surveys were implemented toward the end of the program, which is when theoretical courses were taking place. Some formal apprentices attending theoretical training may not have been listed as present in firms at the time of the follow-up firm survey.

The assumptions we make about non-compliance for youths and firms in these calculations appear reasonable. As explained above, it is understandable that youths were not able to clearly distinguish AGEFOP and PEJEDEC programs, and that non-compliance among youths was due to the broad measure we use. On the side of firms, although the experiment was closely monitored, we cannot fully rule out that some treated youths were assigned to control firms. The calculations above can be interpreted as showing that results from the youth and firm survey are compatible under these assumptions.

A3 Additional Details on the Conceptual Framework

A3.1 Framework Summary

Supply of apprentices. We model youths' decisions to enter apprenticeship as a sequential process. Youths decide first whether or not to enter the market for apprenticeship, and second

they search. The decision to enter apprenticeship is based on a comparison of the expected wage (w_a), a fee paid to enter apprenticeship (Φ), the value of future earnings related to the increase in human capital (Π) and the cumulated value of alternative current and future earnings (w_0). We assume that there is no cost or effort to search for an apprenticeship position. Youths decide to enter apprenticeship if the following condition is fulfilled:

$$w_0 \leq \gamma_a = w_a - \Phi + \Pi \tag{A1}$$

We consider there are N_y youths, and denote F the cumulative distribution function of w_0. The share of youths searching for an apprenticeship position is thus $F(\gamma_a)$ and the number of youths searching for an apprenticeship position is $Y_a = N_y F(\gamma_a)$. When searching, youths will find a position with a probability $\lambda(\theta)$ which depends positively on the tightness of the apprenticeship market $\theta = V/Y_a$, with Y_a the number of youths searching for an apprenticeship position and V the number of apprenticeship vacancies.[50] The supply side relation between the number of apprentices and the tightness writes:

$$S^{trad}(\theta) = \lambda(\theta) Y_a \tag{A2}$$

Modelling the intervention for youths. The intervention consists in offering a share σ_a of the N_y youths a subsidy S, providing them a formal training and matching them with a firm. We assume that the wage paid by firms w_f and the apprenticeship entry fee requested by firms Φ_f can be different. The perceived long-term gains from formal apprenticeships are also expected to be different: $\Pi_f \geq \Pi$. In this context, youths decide to enter apprenticeship if:

$$w_0 \leq \gamma_f + S = w_f + S - \Phi_f + \Pi_f \tag{A3}$$

Individual youths decide to enter formal apprenticeship if current and future earnings in apprenticeship (net of any fee paid to enter apprenticeship) are larger than the cumulated value of current and future earnings outside apprenticeship. Assuming $\gamma_f + S > \gamma_a$, all youths ready to enter traditional apprenticeship are ready to enter formal apprenticeship.[51]

[50] $\lambda(\theta)$ is derived as usual from a homogeneous matching function: $\lambda(\theta) = M(Y_a, V)/Y_a$

[51] In practice, w_f is likely to be smaller than w_a as the subsidy is supposed to cover transportation cost,

The introduction of the program removes a share σ_a of potential apprentices from the traditional apprenticeship market. At any given tightness θ, it induces $\sigma_a S^{trad}(\theta)$ youths to exit the pool of traditional apprentices, leaving $(1-\sigma_a)S^{trad}(\theta)$ youths left for potential matches.

Accounting for financial constraints. The decision to enter apprenticeship when offered or not to participate in the program is described in equation A1 and A3. We can adjust this to account explicitly for financial constraints. Assume that a minimum amount of earnings $\overline{w}$ is needed. If it is above $w_a - \Phi$ and $w_f + S - \Phi_f$, apprentices have to borrow to afford a minimum standard of leaving during their training. The equations that will determine whether youths enter apprenticeship become

$$w_0 + r(\overline{w} - (w_a - \Phi)) \leq w_a - \Phi + \Pi \tag{A4}$$

and

$$w_0 + r(\overline{w} - (w_f - \Phi_f + S)) \leq w_f - \Phi_f + S + \Pi_f \tag{A5}$$

When S increases, there are youths with better employment opportunities (larger w_0) or worse borrowing conditions (larger r) who will find it profitable to enter apprenticeship.

Demand for apprentices. Firms are assumed to have a production technology in which traditional apprentices (n_a) and formal apprentices (n_f) are inputs that are imperfect substitutes.[52] We consider the partial demand for traditional apprentices once n_f formal apprentices have been hired. It is defined such that the marginal productivity of traditional apprentices is equal to the cost associated with hosting apprentices, including wages (w_a), compensation for the firm to provide training (Δ) and the cost of filling a vacancy ($c(\theta)$). We approximate this partial demand as $d(\theta) - \psi n_f$. ψ captures the substitution effect and is the first key parameter of the experiment. It is expected to be positive, as long as returns to scale are low and the two types of apprentices are largely substitute. This captures the intuitive idea that formal apprentices can crowd out traditional apprentices. $d(\theta)$ is decreasing in θ

meal, and clothing, which are important components of the compensation received by traditional apprentices. Similarly, $\Phi_f \leq \Phi$ as firms were requested not to charge apprenticeship entry fees. On the other hand, the delivery of theoretical training in vocational centers is expected to upgrade skills and push future earnings up: $\Pi_f > \Pi_a$.

[52] We consider all other inputs, including regular labor, as given and omit them.

as the cost of matching increases when the number of vacancies to fill per youth searching increases.

Summing across treated and control firms leads to an aggregate partial demand for traditional apprentices: $N_f[\sigma_f(d(\theta) - \psi n_f) + (1 - \sigma_f)d(\theta)] = D^{trad}(\theta) - \psi N_{form}$, where N_{form} is the total number of formal apprentices hired.

Equilibrium. Absent any intervention (i.e. for $\sigma_a = \sigma_f = 0$), the supply and demand functions for traditional apprentices determine an equilibrium in which the tightness is θ_0, and the total number of apprentices is $N_0 = S_a^{trad}(\theta_0) = D^{trad}(\theta_0)$. This is represented by point E_0 in Figure 2. The intervention causes a downward shift in the supply of traditional apprentices (by $\sigma_a S_a^{trad}(\theta)$), and similarly a downward shift in the demand for traditional apprentices (by ψN_{form}). At the new equilibrium, N_1^{trad} youths are hired as traditional apprentices and the tightness becomes θ_1. The new equilibrium is represented by point E_1 (θ_1 and N_1^{trad}) in Figure 2.

We consider the windfall parameter ω defined by:

$$\omega = \sigma_a S_a^{trad}(\theta_1)/N_{form} \tag{A6}$$

$\sigma_a S_a^{trad}(\theta_1)$ is the number of youths in the treatment group who would have taken a traditional apprenticeship position absent the program. ω thus represents the share of formal apprentices who would have entered traditional apprenticeship at the new equilibrium market tightness θ_1.

ω and ψ are the two key parameters that our experiment will estimate to describe the change in equilibrium induced by the program. We can expand the supply and demand functions around the new equilibrium θ_1. For example, on the supply side we get: $N_1 = (1 - \sigma_a)S_a^{trad}(\theta_1) = N_0 + A_s(\theta_1 - \theta_0) - \sigma_a S_a^{trad}(\theta_1) = N_0 + A_s(\theta_1 - \theta_0) - \omega N_{form}$. We obtain the following two equations:

$$\text{Supply: } N_1 = N_0 + A_s(\theta_1 - \theta_0) - \omega N_{form} \tag{A7}$$

$$\text{Demand: } N_1 = N_0 - A_d(\theta_1 - \theta_0) - \psi N_{form} \tag{A8}$$

Where A_s is the slope parameter of the supply of traditional apprentices and A_d the opposite of the slope parameter of the demand for traditional apprentices. Both parameters are

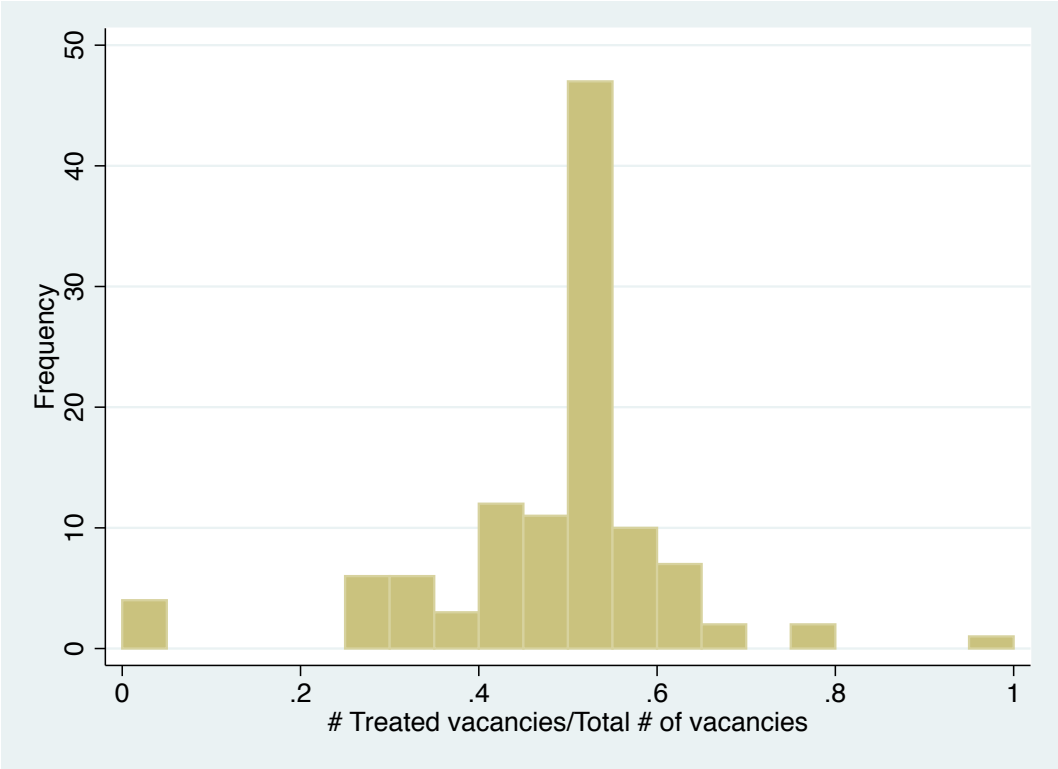
positive. Using equations A7 and A8, we can easily express the change in the number of traditional apprentices $N_1 - N_0$ and in the tightness $\theta_1 - \theta_0$ as function of the two shift parameters ψ and ω:

$$\frac{N_1^{trad} - N_0}{N_{form}} = -\frac{A_s\omega + A_d\psi}{A_s + A_d} \tag{A9}$$

$$\theta_1 - \theta_0 = (\omega - \psi)\frac{N_{form}}{A_s + A_d} \tag{A10}$$

A4 Additional Tables and Figures

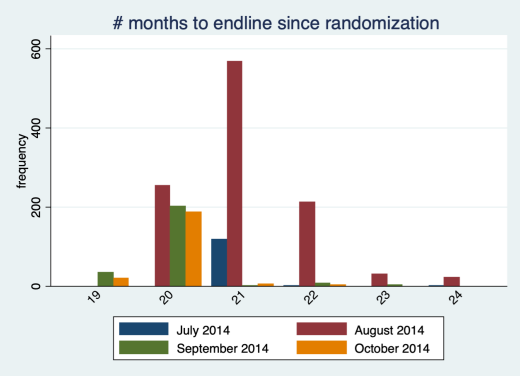
Figure A1: Ratio of treated positions to total number of positions, by micro market



Source: Administrative dataset used for randomization.
Notes: 111 micro markets, defined as locality × trade.
Total number of positions in treated firms in a micro market, divided by total number of positions in registered firms in the micro market. By construction, this ratio is the same as the ratio of the number of treated youths to the total number of youths in a micro market.

Figure A2: Timing of firm and youth first follow-up surveys

Youth survey



Firm survey

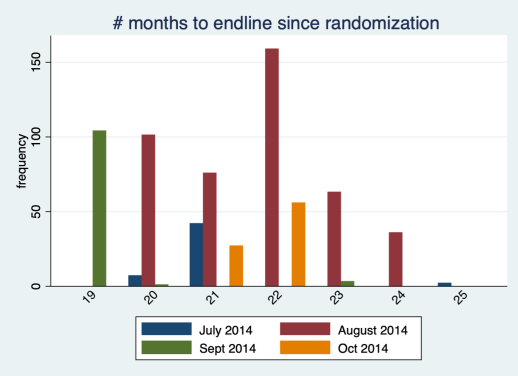
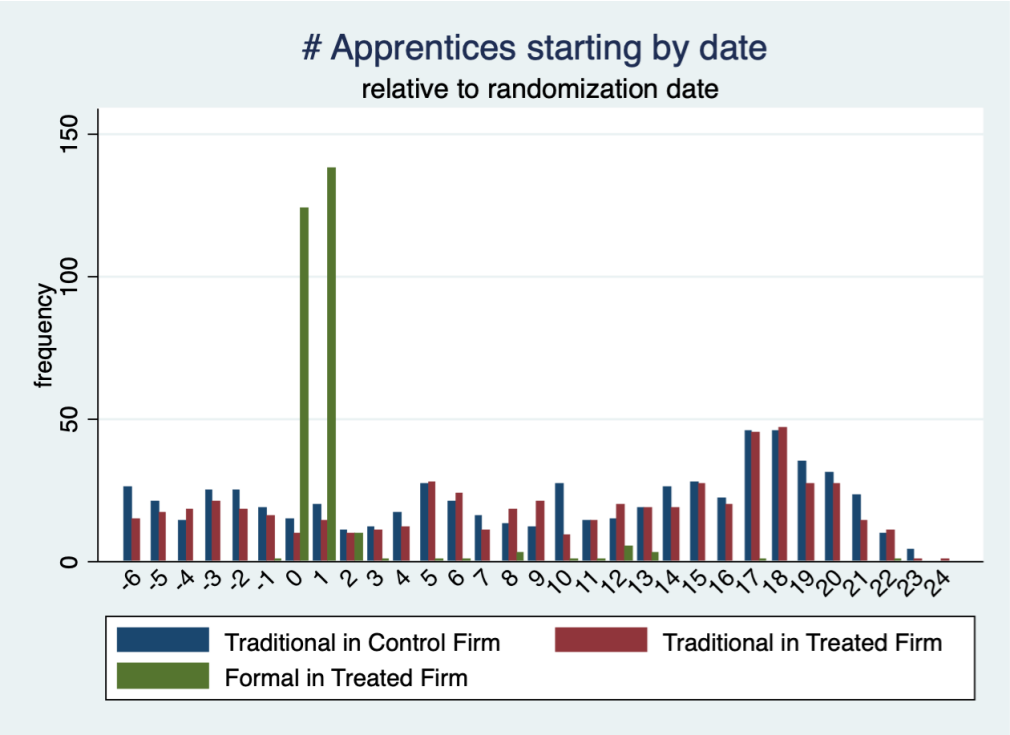


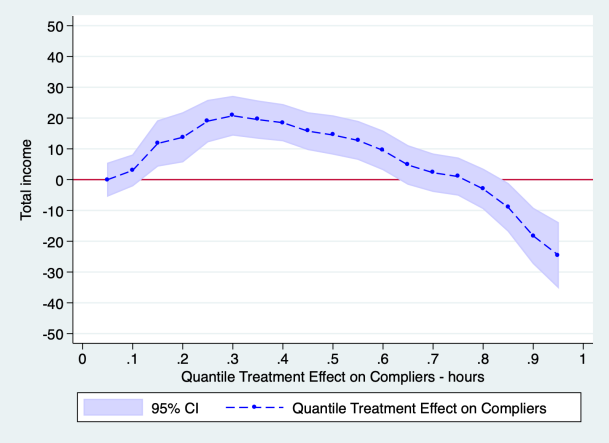
Figure A3: Entry of traditional and formal apprentices in treatment and control firms, by month



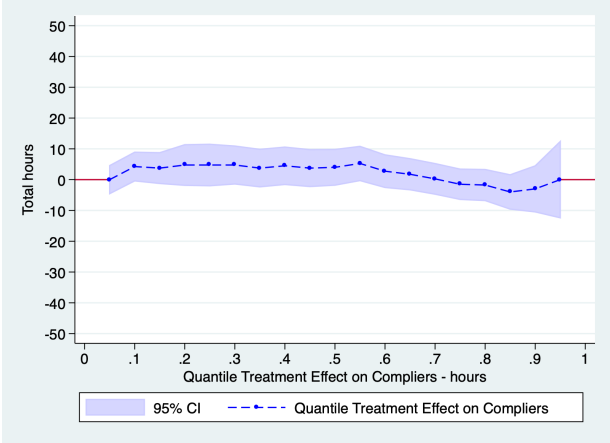
Source: Firm follow-up survey (674 observations)
Notes: Number of youths entering treated and control firms as formal or traditional apprentices, by month. (0 is the randomization date.)

Figure A4: Unconditional quantile treatment effects on compliers for hours worked and income

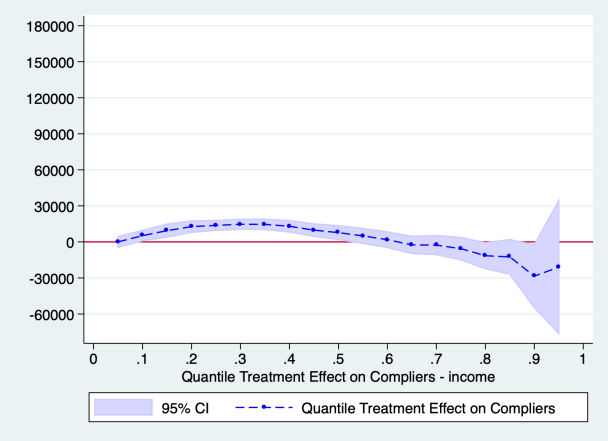
Midline: Hours



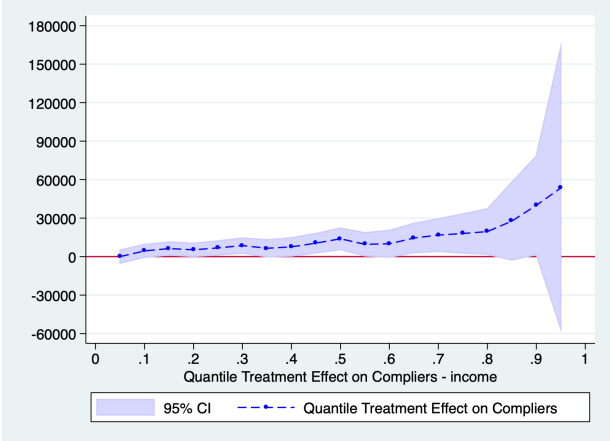
Endline: Hours



Midline: Income



Endline: Income

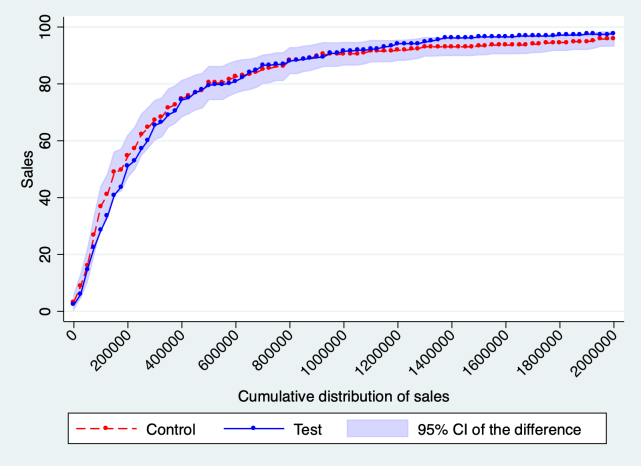


Source: Youth first follow-up (midline) (1661 observations) and second follow-up (endline) surveys (1670 observations)

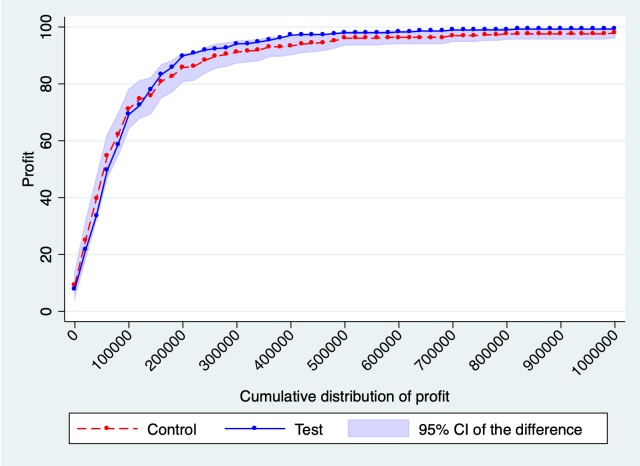
Notes: The figures present the results of the estimation of unconditional instrumental variables quantile treatment effect (Frölich and Melly, 2013). The doted blue line provides the estimated parameter for a given q. The shaded area displays its confidence interval.

Figure A5: Impact on Revenues and Profit

Revenues



Profit



Mann Whitney test, with p values obtained from 10,000 permutations within strata[a]

p=2244/10000 p=4538/10000

Source: Firm follow-up survey (674 observations)

Notes: Estimation of equation 4, with variables defined as $1(y < t)$ for t varying over the support of y. The doted red line provides, for a given t, the average in the control group. The solid blue line provides the sum of the average in the control group and the estimated coefficient. The shaded area represents the confidence interval of the estimated coefficient. The Mann Whitney test is implemented using 10,000 permutations within randomization strata.

a - see notes in Figure 3.

Table A1: Experiment Size Ratio in Study Localities

District	Locality included (among others)	Share of apprentices among urban youths	Population in the locality		Treated youths		Experiment size ratio
		(15-24)	Total	Inflow of Apprentice	Registered	Started	
	(2)	(3)	(4)	(5)=0.2295*(3)*(4)/3	(6)	(7)	(8)=(7)/(5)
Lagune	Adzope	4.82%	58722	217	61	47	21.7%
Bandama	Bouake	5.17%	536719	2123	113	87	4.1%
Lacs	Daoukro	8.05%	44342	273	135	96	35.2%
Goh-Djiboua	Divo	9.50%	105397	766	114	87	11.4%
	Gagnoa	9.50%	160465	1166	125	95	8.1%
Montagnes	Man	13.13%	149041	1497	318	221	14.8%
Woroba	Mankono	8.20%	15118	94	44	29	31.0%
Total		8.15%	1069804	6670	910	662	9.9%

Sources: Column (3) is based on the 2013 national employment survey (collected in February 2014), which is representative at both urban and rural levels in each district. It provides the share of apprentices in the population of youths aged 15-24.

Column (4) comes from the 2014 national census and gives the total population in the locality.

Column (5) gives an estimate of the yearly inflow of youths aged 15-24 into apprenticeship. We first apply the national share of youths aged 15-24 in the population (0.2295) to total population (in column (4)). We then multiply the result by the share of youths in apprenticeship (in column(3)). Lastly, we divide by the median duration of apprenticeship (3 years in the national employment survey).

Column (6) provides the number of youths assigned to treatment in each locality. Column (7) gives the number of youths in the treatment group who actually started apprenticeship.

The last column (8) provides the experiment size ratio defined as the number of youths starting apprenticeship in the program divided by the estimated number of youths starting apprenticeship in each locality.

Table A2: Balancing - Youth side

Variable Names	Baseline			Midline			Endline		
	Cont	Coef	p v	Cont	Coef	p v	Cont	Coef	p v
Demographics									
Male	0.87	-0.02	0.35	0.87	-0.02	0.26	0.86	-0.01	0.50
Age	20.74	0.09	0.43	20.75	0.08	0.51	20.77	0.05	0.66
Married	0.02	0.01	0.47	0.02	0.00	0.60	0.02	0.00	0.51
No diploma	0.20	-0.00	0.81	0.20	-0.00	0.84	0.20	0.01	0.63
Primary education	0.63	0.00	0.88	0.64	0.00	0.98	0.64	-0.01	0.77
Lower sec education or above	0.17	0.00	0.96	0.16	0.00	0.86	0.16	-0.00	0.90
Has received training	0.22	0.03	0.13	0.22	0.04	0.08	0.22	0.04	0.12
Skills									
Skill Index (All)	1.72	0.01	0.65	1.72	0.01	0.75	1.72	0.00	0.89
Learning Skill Index	0.79	0.00	0.90	0.79	-0.00	0.92	0.79	-0.01	0.76
Non Cog Skill Index	0.93	0.01	0.43	0.93	0.01	0.36	0.93	0.01	0.37
Economic Activity									
Has activity	0.87	0.00	0.81	0.87	0.01	0.56	0.87	0.00	0.80
Nb of agricultural act.	0.20	-0.02	0.32	0.21	-0.03	0.22	0.22	-0.04	0.12
Total nb of activities	1.36	-0.01	0.86	1.38	-0.02	0.75	1.40	-0.03	0.61
Nb of non agricultural act.	1.17	0.01	0.69	1.17	0.01	0.72	1.19	0.01	0.76
Total income (KFCFA)	70.53	4.57	0.51	70.86	5.21	0.47	69.46	4.34	0.54
Total income (hyperb sin)	3.25	-0.07	0.57	3.27	-0.07	0.61	3.24	-0.05	0.70
Employment aspirations									
Searching for a job	0.44	0.01	0.66	0.44	0.01	0.75	0.44	0.02	0.40
Aspires to wage job	0.46	0.01	0.72	0.46	0.01	0.84	0.46	0.00	0.99
Aspires to self-employment	0.54	-0.01	0.69	0.54	-0.01	0.80	0.53	-0.00	0.96
Nb of hhd members in wage jobs	0.70	-0.04	0.38	0.70	-0.04	0.44	0.68	-0.02	0.68
Has relatives in wage jobs	0.50	0.02	0.57	0.51	0.02	0.52	0.51	0.01	0.82
Has friends in wage jobs	0.52	0.03	0.22	0.53	0.03	0.34	0.52	0.04	0.20
Nb of hhd members with IGA	1.78	-0.04	0.64	1.78	-0.02	0.82	1.77	-0.02	0.79
Has relatives with IGA	0.71	-0.00	0.88	0.71	-0.01	0.84	0.71	-0.00	0.87
Has friends with IGA	0.78	-0.07	0.00	0.78	-0.07	0.00	0.78	-0.08	0.00
Exposure to crisis									
Parents were present when 15	0.76	-0.01	0.72	0.76	-0.01	0.74	0.77	-0.02	0.44
Household subject to crisis	0.12	-0.01	0.62	0.13	-0.01	0.77	0.11	0.01	0.60
Family subject to crisis	0.19	-0.04	0.07	0.19	-0.04	0.05	0.20	-0.05	0.02
Lost employment during crisis	0.03	0.00	0.88	0.02	0.00	0.77	0.02	0.01	0.42
Financial constraints									
Nb financial constraints	2.78	-0.02	0.89	2.83	-0.05	0.73	2.75	0.02	0.91

Continued on next page...

... table A2 continued

Variable Names	Cont	Coef	p v	Cont	Coef	p v	Cont	Coef	p v
Saved during last 3 months	0.49	-0.01	0.77	0.49	-0.01	0.84	0.48	0.01	0.84
Has saving account	0.05	0.01	0.59	0.05	0.01	0.49	0.05	0.01	0.53
Dissave to face emergency	0.85	-0.02	0.38	0.85	-0.02	0.36	0.85	-0.02	0.37
Has debt	0.31	0.01	0.76	0.31	0.01	0.68	0.32	0.01	0.80
Has problem paying back debt	0.16	0.00	0.87	0.16	0.00	0.82	0.17	-0.00	0.87
Is credit constrained	0.52	-0.03	0.24	0.52	-0.03	0.37	0.51	-0.02	0.53
Survey response rate									
Response Rate	0.76	-0.04	0.03	0.91	0.00	0.92	0.90	0.01	0.44

Sources: Youth baseline, first follow-up (midline) and second follow-up (endline) surveys.
Notes: Each row in the table considers a specific baseline characteristic and presents the result of the estimation of equation (5) on the whole sample for which the baseline is available (left panel - 1357 youths), the sample with both baseline and first follow-up survey respondent (middle panel - 1299 youths), or the sample with both baseline and second follow-up survey respondent (right panel). In each panel, the first column gives the mean in the control group, the second column gives the estimated coefficient and the third column the p-value. The last row provides the survey response rate. (For the baseline survey, the response rate captures the share of available data following an IT issue with the online server, see footnote 14.)

Table A3: Balancing - Firm side

	Baseline				Follow-up			
Variable Names	N	Cont	Coef	p v	N	Cont	Coef	p v
Nb of open positions	731	2.51	0.02	0.87	674	2.45	0.09	0.47
Firm status								
No legal status	695	0.84	-0.00	0.94	644	0.86	-0.02	0.47
No accounting	695	0.67	-0.03	0.37	644	0.69	-0.03	0.33
No salary slip	695	0.97	0.01	0.34	644	0.97	0.00	0.65
Workforce								
Nb Permanent workers	695	6.37	0.31	0.50	644	6.27	0.06	0.89
Nb Autonomous workers	695	3.36	0.05	0.82	644	3.27	-0.08	0.71
Nb Supervisors	695	2.39	0.08	0.55	644	2.34	0.10	0.46
Nb Apprentices	695	3.37	0.16	0.62	644	3.38	0.04	0.90
Channels to recruit apprentices								
Spontaneous application	695	0.10	-0.03	0.20	644	0.10	-0.03	0.11
Parents asked	695	0.81	0.03	0.22	644	0.82	0.03	0.24
Referral	695	0.04	-0.02	0.18	644	0.03	-0.01	0.34
National agency	695	0.02	0.00	0.78	644	0.02	-0.00	0.98
Other recruitment channel	695	0.03	0.01	0.56	644	0.02	0.02	0.23
Reasons to hire apprentices								
To get workers	695	0.09	-0.01	0.72	644	0.08	0.00	0.87
To transmit knowledge	695	0.45	0.02	0.63	644	0.48	-0.01	0.73
To help youth	695	0.41	-0.01	0.72	644	0.39	0.00	0.92
Because it pays	695	0.01	-0.00	0.68	644	0.01	0.00	0.68
Other reasons	695	0.04	0.01	0.71	644	0.04	0.00	0.85
Main criterion to select apprentices								
Skills	695	0.03	0.01	0.54	644	0.03	0.01	0.57
Motivation	695	0.30	-0.03	0.36	644	0.29	-0.04	0.31
Respect	695	0.59	0.04	0.34	644	0.60	0.04	0.32
Training Fees								
Entry Fee	695	0.52	0.00	0.90	644	0.53	0.00	0.97
Amount	362	38136	-1098	0.70	342	38260	-998	0.74
Fee during training	695	0.25	0.03	0.40	644	0.26	0.02	0.52
Number of dropouts and reasons to drop-out between 2012-2014								
Total	695	2.01	0.32	0.12	644	1.95	0.35	0.10
Unable	465	0.07	0.05	0.08	432	0.06	0.05	0.09
Not interested	465	0.21	0.02	0.64	432	0.20	0.02	0.61
Financial reasons	465	0.13	-0.06	0.04	432	0.13	-0.07	0.01

Continued on next page...

... table A3 continued

Variables	N	Cont	Coef	p-val	N	Cont	Coef	p-val
No work perspectives	465	0.00	0.00	0.59	432	0.00	-0.00	0.94
Found a job	465	0.08	0.00	0.98	432	0.09	-0.00	0.93
Disciplinary reason	465	0.18	-0.04	0.23	432	0.18	-0.04	0.27
Finishers between 2012-2014								
Number	695	1.23	-0.02	0.89	644	1.22	-0.11	0.54
Hired in firm	695	0.24	0.03	0.68	644	0.20	0.02	0.70
Hired outside	695	0.26	-0.05	0.40	644	0.25	-0.06	0.30
Started a business	695	0.69	0.04	0.76	644	0.71	-0.01	0.92
Survey response rate								
Response rate	731	0.95	-0.00	1.00	731	0.91	0.01	0.48

Sources: Firm baseline and first follow-up (midline) surveys.
Notes: Each row in the table considers a specific baseline characteristic and presents the result of the estimation of equation 4 on the whole sample for which the baseline is available (left panel - 694 firms), or the sample with both baseline and follow-up survey (right panel - 643 firms). In each panel, the first column gives the number of observations used in the regression. Some variables (for example on dropouts) are defined conditionally on another variable in the table (e.g. among firms who had at least one dropout). The second column gives the mean in the control group, the third column the estimated coefficient and the fourth column the p-value. The first row contains the number of apprenticeship positions offered by firms before randomization (from administrative data sources). The last row provides the survey response rate. (For the baseline survey, the response rate captures the share of available data following an IT issue with the online server, see footnote 14.)

Table A4: Program Take-up and Dropout (process evaluation and administrative data)

Process Evaluation data			Administrative data		
	Count	As %		Count	As%
Enrolled	914	100.00	Enrolled	914	100.00
Contacted by implementer	809	88.51	Did not sign	253	27.68
Signed a contract	762	83.37	Signed a contract	661	72.32
Started apprenticeship	683	74.73	Dropped out	175	19.14
Still in apprenticeship	558	61.05	Finished	486	53.17

Sources: Process evaluation survey (left panel) and administrative dataset (right panel).

Table A5: Definitions of participation in formal apprenticeship at the moment of the survey

	PEJEDEC and AGEFOP	PEJEDEC	AGEFOP only	AGEFOP	PEJEDEC only
	(1)	(2)	(3)	(4)	(5)
All trades					
Treated	0.490***	0.429***	0.061***	0.466***	0.024***
	(0.018)	(0.017)	(0.009)	(0.018)	(0.006)
Mean	0.018	0.010	0.007	0.016	0.002
Only trades for which formal apprenticeships lasts 24 months					
Treated	0.538***	0.480***	0.058***	0.511***	0.027***
	(0.020)	(0.019)	(0.009)	(0.020)	(0.007)
Mean	0.017	0.011	0.006	0.015	0.003

Impact of treatment assignment on participation in apprenticeship, based on combinations of answers to questions about enrollment in government-supported apprenticeship programs.
Upper panel (1661 youths): results for all youths in first follow-up survey (midline).
Lower panel (1365 youths): results for youths applying to the program for trades for which formal apprenticeship lasts 24 months.

Table A6: Definitions of participation in formal apprenticeship over the course of the experiment

	GP All (1)	GP and Apprentice (2)	GP and TVET (3)	GP and Any training (4)
Governmental Program: PEJEDEC program				
Treated	0.643***	0.472***	0.135***	0.607***
	(0.017)	(0.017)	(0.012)	(0.017)
Mean	0.028	0.015	0.004	0.019
Governmental Program: AGEFOP program				
Treated	0.708***	0.504***	0.168***	0.672***
	(0.017)	(0.018)	(0.013)	(0.017)
Mean	0.063	0.024	0.012	0.036
Any of the 2 Governmental Program				
Treated	0.747***	0.528***	0.184***	0.712***
	(0.016)	(0.018)	(0.014)	(0.016)
Mean	0.069	0.025	0.013	0.038

Source: Youth first follow-up (midline) survey (1661 observations)

Notes: The table documents alternative definitions to build human capital variables. GP stands for Governmental Program

Column (1): share of youths reporting they have been enrolled in a government program (GP) considered in the panel.

Column (2) to (4): share of youths defined in the same way as in Column (1) and who report that they have been enrolled in apprenticeship (column (2)) , TVET (column (3)), or either TVET or apprenticeship (column (4)).

Table A7: Permutation test for main ITT estimates

Youth at midline					
	Formal	Traditional	Total	Total hours per week	Total earnings
Asymptotic p-value	<0.0001	<0.0001	<0.0001	0.0136	0.6691
Permutation p-value	0	0	0	0.0144	0.6697
Firm at midline					
	Formal	Traditional	Total	Total Hours (month)	Net Value of work
Asymptotic p-value	<0.0001	0.0747	<0.0001	0.0367	0.0003
Permutation p-value	0	0.0697	0	0.0356	0.0002
Youth at endline					
	Apprentice	Training month	Skill index	Total hours per week	Total earnings
Asymptotic p-value	<0.0001	<0.0001	<0.0001	0.2795	0.0169
Permutation p-value	0	0	0	0.2845	0.0194

Upper panel: Youth first (midline) follow-up survey (1661 observations). Estimation of equation 5.
Intermediate panel: Firm survey (674 observations). Estimation of equation 4.
Lower panel: Youth second (endline) follow-up survey (1670 observations). Estimation of equation 5.
For each variable, the table presents first the asymptotic p-value and then the p-value from permutation tests after 10000 permutations within randomization strata.

Table A8: Stock of apprentices and other employees in firms

	Total # of employees	Full-time workers	Apprentices	Interns	Occasional workers	No apprentices in the firm
Treated	0.495	0.030	0.464	0.001	0.165	-0.054**
	(0.554)	(0.247)	(0.362)	(0.016)	(0.133)	(0.027)
Mean	6.939	3.205	3.693	0.042	0.913	0.200

Source: Firm follow-up survey (674 observations).
Notes: Estimation of equation 4 on various workforce variables obtained from the employer module of the firm follow-up survey.

Table A9: Sales and Profit of firms

	First Measure		Second Measure		Pooled
	First survey	Backcheck	First survey	Backcheck	
Revenue					
Treated	-55015	-49304	-56903	-48090	-52922
	(50011)	(47305)	(52145)	(55356)	(46045)
Mean	424476	438653	458329	487748	451691
Profit					
Treated	-21947*	-11616	-19500	-10423	-15820
	(12171)	(11222)	(12972)	(12656)	(10680)
Mean	117381	115863	123321	124305	120225
Nobs	674	596	674	596	2540

Source: Firm follow-up survey.
Estimation of equation 12 for sales (upper panel) and profit (lower panel). The firm survey first asks about profits and sales (First measure, column (1)). It then asks firms to recall their past activities, after which it asks firms again about their profit and sales (Second measure, column (3)). Those questions were back-checked for most firms (columns (2) and (4)). The last column pools all four measures.

Table A10: Impact on disaggregated tasks

	Control Mean	Coefficient	p values Regular	Adjusted
Non Routine Analytical				
Read forms	0.229	0.042	0.050	0.078
Read bills or financial statements	0.234	0.053	0.013	0.026
Read instructions or operating manuals	0.230	0.073	0.001	0.004
Read reports	0.185	0.024	0.236	0.276
Read plans	0.161	0.105	<0.001	<0.001
Read other documents	0.089	-0.002	0.883	0.883
Longest document read (n pages)	21.124	16.505	0.046	0.007
Has to write	0.494	0.073	0.003	0.007
Longest document written (n pages)	6.139	0.789	0.611	0.658
Take measures or estimate sizes, weights, distances	0.443	0.088	<0.001	0.003
Calculate prices or costs	0.595	0.037	0.134	0.171
Perform simple math operations	0.576	0.081	0.001	0.003
Other Math Computations	0.268	0.040	0.072	0.101
Undertake tasks requiring 30 min of thinking (1-5)	2.906	0.214	0.002	0.006
Sub-index of Non Routine Analytical Tasks	0.000	0.254	<0.001	
Non Routine Interpersonal				
Has interactions with clients (1-4)	3.125	0.036	0.499	0.629
Advise co-workers	0.877	0.034	0.022	0.088
Teach co-workers	0.814	0.009	0.629	0.629
Monitor co-workers	0.670	0.033	0.148	0.295
Sub-index of Non Routine Interpersonal Tasks	0.000	0.084	0.0766	
Routine				
Carry out short, repetitive tasks (1-4)	2.722	-0.113	0.020	0.034
No freedom to decide how to organize work (1-10)	5.555	-0.085	0.627	0.784
Use heavy equipments	0.193	0.074	<0.001	0.001
Perform physical work (1-10)	6.120	0.064	0.638	0.638
Repair or maintain electronic equipments	0.315	0.067	0.004	0.010
Sub-index of Routine Tasks	0.000	0.081	0.084	

Source: Youth second (endline) follow-up survey (1670 observations).

Estimation of equation 5 for tasks realized by youths in their primary occupations, based the approach in Autor et al. (2003), Autor and Handel (2013) and Dicarlo et al. (2016). Our questionnaire covers 14 tasks for the multi-country survey in Dicarlo et al. (2016).

The first, second and third panel correspond to questions related to Non Routine Analytical, Non Routine Interpersonal, respectively Routine tasks. The last row of each panel includes a sub-index defined as the sum of the standardized variables in each panel, which is then normalized to have a unit variance.

The table presents the mean of the variable in the control group, the estimated treatment coefficient, its p-value and the p-value adjusted for false discovery rates using the Benjamini-Hochberg method.

Table A11: Youths' selection into apprenticeship

Variable Names	Always Apprentice (1)	Formal Apprentice Only (2)	Never Apprentice (3)	p-val (4)
Demographics				
Male	0.900	0.812	0.860	0.038
Age	20.743	20.907	20.650	0.536
Married	0.025	0.015	0.015	0.622
No diploma	0.261	0.156	0.205	0.062
Primary education	0.605	0.679	0.575	0.256
Lower sec education or above	0.133	0.166	0.220	0.481
Has received training	0.299	0.233	0.248	0.269
Economic Activity				
Has activity	0.855	0.863	0.917	0.862
Nb of agricultural act.	0.253	0.139	0.170	0.077
Total nb of activities	1.474	1.291	1.376	0.182
Nb of non agricultural act.	1.221	1.152	1.206	0.489
Total income (KCFA)	64.893	75.960	87.298	0.450
Total income (hyperb sin)	3.088	3.079	3.610	0.979
Employment aspirations				
Searching for a job	0.415	0.489	0.401	0.262
Aspires to wage job	0.366	0.509	0.467	0.026
Aspires to self-employment	0.634	0.479	0.527	0.016
Nb of hhd members in wage jobs	0.671	0.674	0.587	0.980
Has relatives in wage jobs	0.466	0.548	0.558	0.213
Has friends in wage jobs	0.540	0.572	0.529	0.626
Nb of hhd members with IGA	1.756	1.754	1.728	0.989
Has relatives with IGA	0.745	0.682	0.722	0.277
Has friends with IGA	0.756	0.675	0.706	0.163
Exposure to crisis				
Parents were present when 15	0.779	0.717	0.789	0.271
Household subject to crisis	0.112	0.112	0.158	0.985
Family subject to crisis	0.189	0.147	0.135	0.405
Lost employment during crisis	0.022	0.038	0.009	0.435
Financial constraints				
Nb financial constraints	2.843	2.823	2.700	0.950
Saved during last 3 months	0.463	0.495	0.473	0.623
Has saving account	0.040	0.057	0.089	0.523
Dissave to face emergency	0.846	0.813	0.841	0.500
Has debt	0.319	0.309	0.342	0.876

Continued on next page...

... table A11 continued

Variable Names	Always Apprentice	Only Formal Apprentice	Never Apprentice	p-val
Has problem paying back debt	0.190	0.149	0.176	0.418
Is credit constrained	0.503	0.470	0.503	0.611

Source: Youth baseline survey

Notes: A any form of apprenticeship, Z assignment to treatment.

Column (1): average baseline characteristics for "Always-Apprentice" (AA). Average of X for youths assigned to the control group ($Z = 0$) who entered (traditional) apprenticeship ($A = 1$).

Column (2): average baseline characteristics for "Formal Apprentice Only' (FO). This is obtained using the regression of $A \times X$ on A using the assignment variable Z as an instrumental variable (Abadie, 2003).

Column (3): average baseline characteristics of "Never Apprentice" (NA) ($A = 0$ and $Z = 1$).

Column (4): p-value for the test of equality of column (1) and column (2). Test of $\alpha_{AZ} = 0$ in the regression $E(X|A, Z) = \alpha_0 + \alpha_{AZ}AZ + \alpha_{Z(1-A)}Z(1 - A) + \alpha_{(1-Z)(1-A)}(1 - Z)(1 - A)$, in which the excluded category is $A(1 - Z) = 1$, the "AA" population (in $Z = 0$). $AZ = 1$ identifies the mix of the population of "FO" and "AA" (in $Z = 1$).

Table A12: Apprentices' characteristics

	Reference	Formal	p-value	Traditional	p-value
(1) Demographics					
Age	20.670	2.105	0.000	-1.012	0.007
Male	0.947	-0.108	0.000	0.023	0.009
No Education	0.682	-0.475	0.000	0.079	0.045
Primary education	0.246	0.348	0.000	-0.078	0.025
Secondary education or above	0.073	0.128	0.000	-0.000	0.983
Affected by crisis	0.112	0.041	0.134	-0.019	0.393
Wealth index	-0.045	0.124	0.122	0.046	0.507
(2) Connection with Mastercraftman					
Know the Mastercraftman	0.573	-0.033	0.455	-0.064	0.117
Mastercraftman is from the family	0.209	-0.047	0.155	-0.020	0.554
Mastercraftman is from area	0.257	0.034	0.390	-0.076	0.032
Mastercraftman is a family connection	0.377	-0.062	0.143	-0.030	0.481

Follow-up firm survey, apprentice module (948 observations).
Estimation of equation 9.
Traditional apprentices who entered control firms are the reference category.
Column "Formal" presents the difference in means between formal apprentices in treated firms and the reference category.
Column "Traditional" presents the difference in means between traditional apprentices in treated firms and the reference category.
Standard errors are clustered at the firm level.

Table A13: Apprentices' performance and supervision

	Reference	Formal	p-value	Traditional	p-value
(1) Performance and payments					
Nb of days worked per month	20	-7	0.000	0	0.952
Value of work last day	1296	840	0.002	338	0.039
Value of work last month	26250	6858	0.302	4901	0.179
Fee paid at entry	4481	-5303	0.000	320	0.754
Fee paid monthly	992	-703	0.120	274	0.630
Fee to be paid at exit	7991	-9417	0.000	-3210	0.151
Payment for transport and food	9507	-3698	0.004	-1190	0.272
"Bonus" payment (for motivation)	4287	-356	0.619	647	0.318
Total wage bill	13794	-4054	0.021	-543	0.708
Net value of work	12404	10968	0.077	5494	0.129
(2) Hours of work and supervision					
Total	8.170	-0.085	0.679	0.267	0.161
Autonomous	2.506	0.553	0.030	-0.086	0.695
Under master supervision	2.628	-0.022	0.934	-0.008	0.974
Watching master	1.726	-0.282	0.132	0.246	0.198
(3) Aspirations					
Aspire to wage employment	0.182	0.230	0.000	0.000	0.995
Aspire to be self employed	0.791	-0.233	0.000	0.003	0.930

Follow-up firm survey, apprentice module, for panels (2) and (3) (948 observations);
Follow-up survey, employer module, for panel (1) (1260 observations).
Estimation of equation 11.
Traditional apprentices who entered control firms are the reference category.
Column "Formal" presents the difference in means between formal apprentices in treated firms and the reference category.
Column "Traditional" presents the difference in means between traditional apprentices wand the reference category.
Standard errors are clustered at the firm level.

Table A14: Permutation test on ranks tests on youths' income and hours and firm performance

	Income midline	Income endline	Hours midline	Hours endline	Firm Revenue	Firm Profit
# of draws above computed stat	58	5	0	2166	2244	4538
Replications	10000	10000	10000	10000	10000	10000
Permutation p-value	0.0058	0.0005	0.0000	0.2166	0.2244	0.4538

This table presents the results of permutation tests for ranksum tests reported in cdf Figure 3 and Figure A5