Creating New Positions? Direct and Indirect Effects of a Subsidized Apprenticeship Program*

Bruno Crépon (Crest)
Patrick Premand (World Bank and ETH Zürich)

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Abstract

Evaluations of employment programs usually focus on direct impacts on participants, but potential indirect effects are rarely quantified. This paper analyzes how the introduction of a subsidized apprenticeship program in Côte d'Ivoire impacts youths' decision to enter apprenticeship and firms' demand for apprentices in the short-term. The experiment simultaneously randomized whether apprenticeship positions opened by firms were filled by the program, and whether interested youths were assigned to a formal apprenticeship. This design allows for estimating whether individuals forgo other apprenticeship opportunities (windfall effects), and whether firms replace other apprentices with program participants (substitution effects). We find both effects to be moderate. A framework shows how they combine. Overall, 0.74 to 0.77 apprenticeship position is created per subsidized apprentice. This shows that the intervention expands access to apprenticeships and increases the net number of positions in firms. The subsidy offsets foregone labor earnings while youth are in formal apprenticeships. At the same time, the net value of work provided by apprentices increases, pointing to large indirect effects in firms.

JEL classification: D22, J23, J24, O12, C93.

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1 Introduction

Governments around the world are implementing a range of employment or skills development initiatives. They often make strong claims about the number of people trained or the number of jobs created by these programs. Employment programs have been widely evaluated (see Card et al. (2018) for a meta analysis), but evaluations usually focus on impacts on participants. Yet these programs can induce a range of indirect effects (Calmfors, 1994; Abbring and Heckman, 2007). In particular, they might affect firms' hiring decisions and performance. While the total number of jobs created and the overall cost-effectiveness of employment programs largely depend on the magnitude of indirect effects, they are rarely quantified.

So-called "windfall" effects arise when some program beneficiaries would have taken a similar position absent any intervention. Most studies evaluating short-term impacts of employment programs have focused on assessing such windfall effects. However, when programs seek to place individuals in firms, "substitution" effects may arise if firms displace other workers with subsidized individuals. These substitution effects at the firm level can rarely be estimated precisely. However, the total number of positions created depends on both windfall and substitution effects.

In this paper, we analyze how the introduction of a subsidized apprenticeship program in Côte d'Ivoire impacts youths' decision to enter apprenticeship and firms' demand for apprentices in the short-term. Apprenticeships are one of the most common types of training in developing countries. Yet barriers for youths to participate are considered widespread, and many governments seek to expand access to apprenticeships. In this context, wage subsidies are often discussed. It remains a subject of active debate whether they could create a large number of additional positions, or crowd out unsubsidized workers in firms.

The innovation of this paper is to simultaneously measure windfall effects among individuals and substitution effects in firms. We set-up a double-sided experiment that randomized whether apprenticeship positions opened by firms were filled by the program, and whether interested youths were assigned to a formal apprenticeship. A simple theoretical framework shows that our simultaneous randomization design on both sides of the market is tailored to estimate direct impacts and *windfall* effects for youths, as well as indirect impacts and *substitution* effects in firms hosting apprentices. Specifically, a sub-set of firms was ran-

domly chosen to have pre-identified apprenticeship positions offered to a random sub-set of youths interested in apprenticeship. This allows measuring how the inflow of new workers is transmitted at the firm level. We can identify substitution effects on firms' hiring of traditional apprentices in the private market. At the same time, we can measure impacts on participation in apprenticeships for youth, including windfall effects driven by youth exiting traditional apprenticeships to enter formal apprenticeships. We can also measure forgone earnings and employment for youths.

A theoretical framework shows how effects on both sides of the labor market combine. We show that we can estimate bounds for the impact on the net number of positions created by the program. We conduct this analysis in the short-term, while youth are still in the program. By taking into account direct effects on youths and indirect effects in firms, the experiment provides a more comprehensive assessment of program performance than evaluations focusing on individuals only.

The program we study offered a subsidy of 30,000 FCFA per month (approximately USD 54, or half the formal minimum wage), paid directly to apprentices for 12 or 24 months (depending on occupations). It included dual training, with practical on-the-job learning complemented by mentoring and theoretical courses. Results show that the program induces moderate windfall and substitution effects. The share of youths in formal apprenticeship increases by 71.2 percentage points, but 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, but substitution effects are observed: for each formal apprentice placed, 0.23 traditional apprentices are 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 (1-0.26, the windfall effect) and 0.77 (1-0.23, the substitution effect) percent of the number of formal apprentices placed.

The short-term cost-effectiveness of the apprenticeship scheme likewise depends on its impact on earnings for both youths and firms. On the side of youth, we find no average short-term impact on earnings, but we observe youths forgo employment opportunities. The share of individuals engaged in wage employment or self-employment decreases by 13.5 and 12.9 percentage points, respectively. Labor earnings decrease by 25.1 percent, which highlights a large opportunity cost from participation in apprenticeship. The program subsidy

contributes to offset these losses in labor earnings.

At the same time, results show substantial indirect program impacts on firms. Firms benefit from a strong increase in the value of the work provided by apprentices net of their compensation. For each formal apprentice hired, the net value of work increases by an amount close to the wage subsidy. Our framework shows that the large implied difference between marginal productivity and payments to apprentices includes a compensation for firms to provide training.

Our paper contributes to several active strands of the literature, starting with the identification of indirect and equilibrium effects in program evaluation. In the case of employment programs, the question of indirect effects was first raised by Calmfors (1994) and is further discussed by Abbring and Heckman (2007). Various strategies have been used to identify indirect and equilibrium effects. Some recent papers rely on large experiments or shocks to measure general equilibrium effects (for instance, the micro-finance crisis in India (Breza and Kinnan, 2018), or the modernization of the large-scale NREGA program (Muralidharan et al., 2017)). Other 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., forthcoming). Others use a double randomization design proposed by Moffitt (2001) to identify impacts on the eligible population that is not participating (Crépon et al., 2013; Akram et al., 2017). In all these cases, the identification of indirect effects requires powerful experiments and adapted designs (Baird et al., forthcoming). While we do not measure general equilibrium effects, our paper adds to this literature by using a new design to identify specific types of indirect effects. Our randomization procedure on the two sides of the market is well suited and powered to measure direct impacts on participants as well as a range of indirect effects on firms, including substitution effects. In another recent paper, Alfonsi et al. (2017) analyze several active labor market programs in which both youth and firms are randomly assigned to treatment and control.

Our paper also contributes to the literature on wage subsidies. Most empirical studies focus on the impacts of wage subsidies on employment of individual workers. Positive impacts have been documented in the short run, although results are mixed in the long run when subsidies are phased out (for reviews, see Almeida et al. (2014); Card et al. (2018)). Berniell and de la Mata (2017) is a recent example where a 12 months wage subsidy was found to

have large short and long run impacts on employment in Argentina. However, wage subsidies can affect the labor market through a range of indirect effects, including substitution effects in firms and equilibrium wage effects (Calmfors, 1994; Katz, 1998). Lise et al. (2004) use a general equilibrium model to study the impacts of a subsidized program, and show that accounting for these effects significantly alter the earlier findings of Michalopoulos et al. (2005). Blundell et al. (2004) compare the population of eligible and non-eligible individuals across areas where the program has or has not been implemented and find no equilibrium effect. Some other papers have attempted to identify impacts on labor demand that could arise from substitution effects, but most results to date come from non-experimental studies in high-income countries, with mixed results (see for example Rotger and Arendt (2011) and references therein). De Mel et al. (2016) set-up a wage subsidy experiment to analyze firm labor demand in Sri Lanka. They find that informal micro-enterprises increase employment while they receive the subsidies, but the effect does not last. Our paper adds to this literature by using a well-powered double randomization design to measure simultaneously direct impacts on youths and indirect impacts on firms. We discuss how windfall and substitution effects combine to assess the net number of positions created by the program. We find large impacts on the number of apprentices in firms, moderate eviction effects, and a large increase in the net value of work provided by apprentices in firms.

Our paper further contributes to the literature on training and apprenticeship programs. Evidence on the effectiveness of training programs has been mixed overall (for reviews, see Bertrand et al. (2013); Blattman and Ralston (2015); McKenzie (2017)), though there have been recent studies with more promising findings (Attanasio et al., 2017; Alfonsi et al., 2017). Evidence on apprenticeship programs also remains very thin despite apprenticeship being one of the most common sources of training in developing countries (Teal, 2016).[1] On the side of youth, our paper focuses on the impact of offering participation in the program on entry into apprenticeship. As such, it contributes to the literature on the demand for training.[2] Black et al. (2003) highlight that low demand is a critical consideration in the analysis of training and employment programs. Here, we consider the role of wage subsidies in the decision to enter apprenticeship in a developing country context. Importantly, it has also been argued

[1] A few studies estimate the returns to traditional apprenticeships (e.g. Frazer (2006)), or short public apprenticeship schemes (e.g. Cho et al. (2013)).

[2] The objective of the paper is not to document long term impacts of apprenticeship on youths, which we will study in future work based on additional data to be collected.

that employers benefit from apprenticeships (e.g. Lerman (2014)). Recent findings in the literature tend to confirm this conjecture, although empirical identification of benefits to firms has been challenging. Alfonsi et al. (2017) analyze vocational and on-the-job training programs and show that firms absorb a significant part of the surplus. Hardy and McCasland (2015) analyze whether a placement intervention improves the matching between potential apprentices and firms. By focusing on firms, they show that addressing inefficiencies in the screening of apprentices leads to increases in employment and profits in firms. Our analysis adds to these recent findings by showing that the indirect effects of a formal apprenticeship program on firms are substantial.

Finally, our study relates to the theoretical framework developed by Acemoglu and Pischke (1999), who show that a form of monopsony power is a condition for an apprenticeship system to be viable. Firms provide training only if they can capture part of the surplus generated by that training. Our paper presents several elements of evidence related to this framework. First, it illustrates the form the monopsony power can take. In traditional apprenticeships such as those prevalent in Côte d'Ivoire, apprentices receive low remuneration during several years before they are able to exit. Our results show that firms obtain relatively large compensation from providing training. Second, it documents large opportunity costs of training for potential apprentices. Lastly, and importantly, our results show that a subsidized program reducing the cost of training and providing a commitment to a minimum level of training increases participation in apprenticeship.

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 documents windfall and substitution effects on youths and firms. Section 6 studies impacts on youth earnings and profits in firms. Section 7 discusses additional mechanisms. Section 8 concludes. The appendix contains supplementary material.

2 Intervention and Experimental Design

2.1 Apprenticeships in Developing Economies

Traditional apprenticeships are one of the most prevalent types of training in the developing world. In some African countries, recent data suggest that around 20 percent of youths have been an apprentice, while less than 5 percent have attended technical and vocational training (Filmer et al., 2014). The vast majority of apprentices are in traditional apprenticeships, which 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. Traditional apprenticeships are also one of the main sources of skill acquisition for informal operators. Despite its prevalence, traditional apprenticeship remains 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, youth transition either as an employee in the host firm, as a wage worker in another firm, or in self-employment. Most youth remain in the informal sector, in part due to the scarcity of formal wage jobs suitable for workers with limited education.

While traditional apprenticeships have developed over time through a private, market-based system with little public intervention, the optimality of the model has been questioned. The improvement of apprenticeship systems has become an important objective in many countries around the world (OECD/ILO, 2017), particularly in West Africa (Walther, 2008; UNESCO, 2015). One of the common rationale for reforms is that policies can facilitate access to apprenticeships, while at the same time improving training quality and returns

for youths. The set-up of dual apprenticeship schemes combining theoretical and practical training is often considered, modeled after institutions from high-income countries such as Germany or Switzerland.

The effectiveness of public interventions in the market for apprenticeships, and ultimately the rationale for reforms, depends on successfully addressing market failures faced by youths or firms. The creation of a formal apprenticeship system may induce youths to forgo other human capital investments, such as traditional apprenticeships, or employment opportunities. Beyond effects on youths, the cost-effectiveness of public apprenticeship schemes in part also depends on indirect effects, such as whether they benefit firms. For instance, it hinges on the absence of negative substitution effects on private traditional apprentices, meaning that there is absorptive capacity for additional apprentices in firms. However, impacts on firms, such as substitution effects on the hiring of other apprentices, are rarely quantified. The literature on the benefits and costs of apprenticeships is extremely thin, and mostly concentrated in a few high-income countries such as Germany or Switzerland (for a review, see Lerman (2017)). The lack of evidence in developing countries is particularly striking given the prevalence of apprenticeship as one of the most common types of training, its mostly private nature, and the importance it is given in skills development or youth employment strategies. Most existing studies focus on access and returns for youths, with little evidence on indirect effects on firms, and little distinction between private (traditional) and publicly subsidized (formal) apprenticeships.

2.2 The Côte d'Ivoire Formal Apprenticeship Program

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 strong 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 young men and women 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, either the enterprise owner or an experienced employee. 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. Apprentices are also mentored by AGEFOP apprenticeship counselors, who regularly visit master craftsmen and apprentices, and 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, leading to certification. Firms are not compensated for taking on apprentices, though they do 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).

[3]PEJEDEC: Projet Emploi Jeune et Développement des Compétences (www.pejedec.org). See Bertrand et al. (2017) for evidence on the cost-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.

2.3 Enrollment Process and Experimental Design

One of the main objectives of the experiment embedded in the Côte d'Ivoire formal apprenticeship program is to measure simultaneously windfall effects among youth and indirect effects in firms. 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 youth to treatment in each micro market as the number of positions to fill. Figure A2 presents the distribution of the ratio of assignment of youth by micro market.[7] The rate

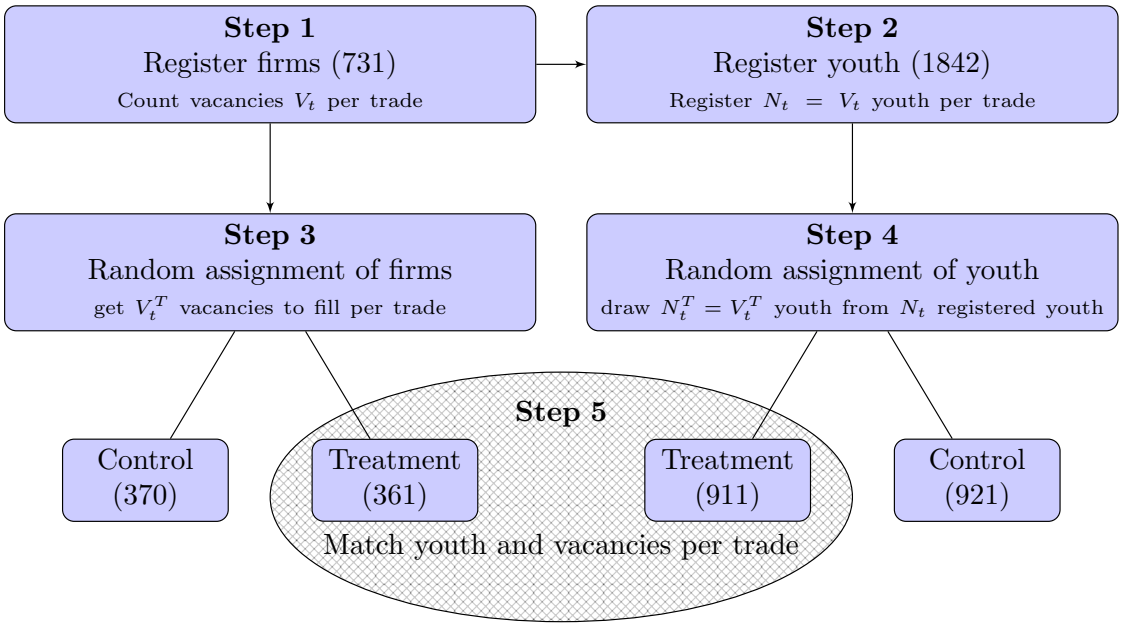
[6] In some cases, firms in a given sector are active in several trades. For example, the garage sector includes apprenticeship positions in several trades: coach builder, car mechanic, car electrician, or car painter.

[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

of assignment of youth 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.

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.

The randomization protocol implies that youth and firms in the treatment and control groups are statistically similar (as we discuss further in Section 4). However, one potential concern is whether youth and firms from the control groups have been affected by the experiment, for instance through changes in the tightness of the apprenticeship market (the

0 or 1. We kept the firms in the data set, but the corresponding youth were not included in the sample for youth regressions. The case arises for 10 youth.

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

chance of firms and potential apprentices to match). The conceptual framework in Section 3 shows that we can estimate bounds for the impact on the number of position created by the intervention. This does not rely on the size of the experiment being small enough to limit such indirect effects.

It remains 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 youth in sub-Saharan Africa. Based on the Cote 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 (see Appendix A1 for details). The order of magnitude is less than 10%.

3 Conceptual Framework

We develop a simple framework to describe how the introduction of a subsidized apprenticeship program affects the aggregate supply of apprentices, the aggregate demand from firms, and the equilibrium in the market for traditional apprentices. The framework derives the theoretical parameters to be estimated in the empirical analysis and provides insights on how to interpret the empirical results. The framework is summarized in this section, and presented in details in Appendix A2. The framework highlights several important features of the experiment.

First, it shows how windfall effects for youths and substitution effects for firms combine to reduce the total number of traditional apprenticeship positions created by the program. In fact, the total number of traditional apprenticeship positions created is a weighted average of the windfall and substitution effects. It can thus be bounded by their minimum and maximum. This also makes clear that net employment effects can only be bounded if both parameters are estimated, which our double-sided experiment makes possible.

Second, the framework considers the tightness of the apprenticeship market (θ), i.e. the ratio of available vacancies to the number of youth searching for a position. Specifically, market adjustments are a function of the tightness and not the wage. This is primarily because the main purpose of the model is to describe how the intervention affects the chances

of youth and firms to match, which is directly related to changes in the tightness.[9] In addition, 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".

Third, and most importantly, the framework shows that the net employment effects can be bounded by parameters that we can estimate. This does not rely on an assumption that the size of the experiment is small.

Fourth, the framework also indicates that equilibrium adjustments in labor-market tightness are expected to be small anyway, and that their order of magnitude can be assessed ex post.

3.1 Framework Summary

Individual youths decide to enter 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. Aggregating across youths, the aggregate supply of (traditional) apprentices can be written $S_a^{trad}(\theta)$ and is increasing in the tightness of the apprenticeship market.

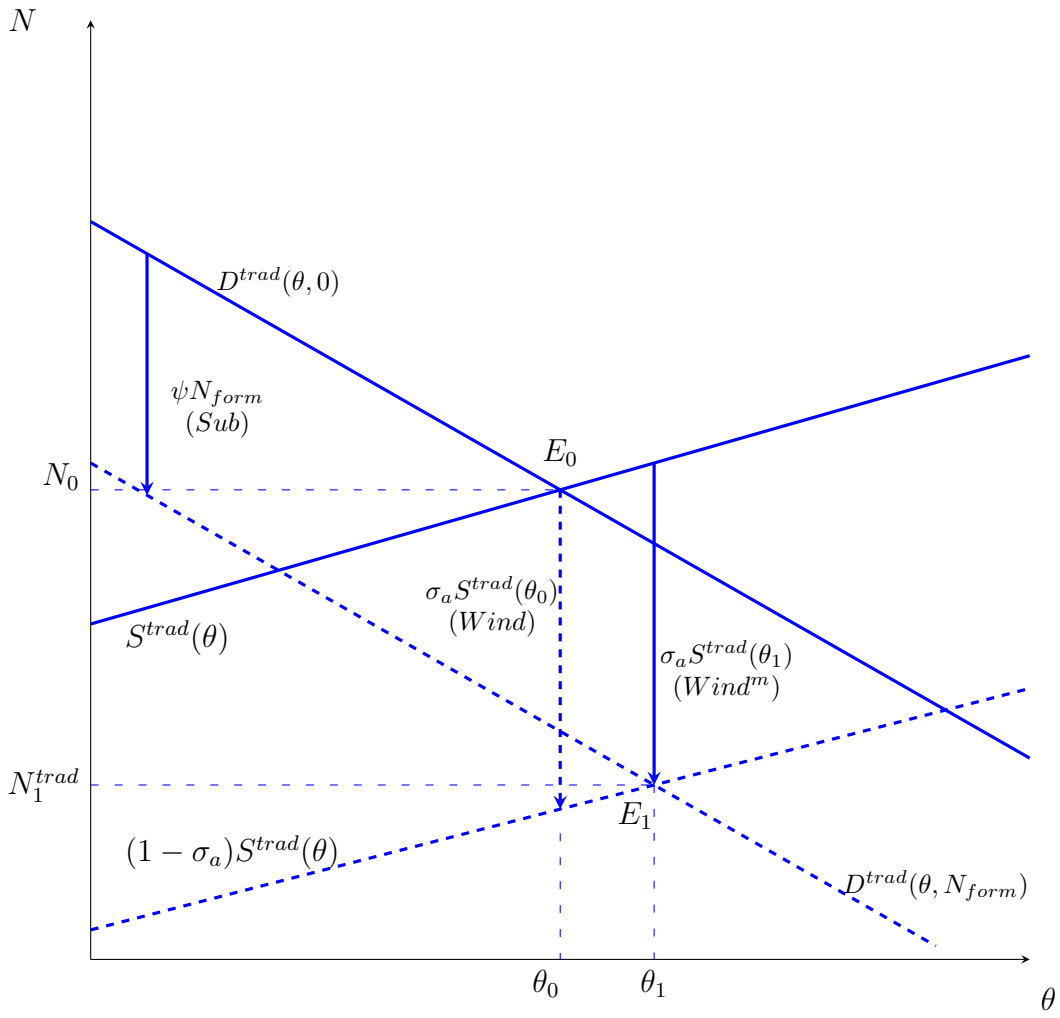
Firms are assumed to have a production technology with decreasing returns $f(n_a, n_1)$, where traditional apprentices (n_a) and formal apprentices (n_1) are imperfect substitutes. We consider the partial demand for traditional apprentices at the firm level $d(\theta, n_1)$ once n_1 formal apprentices have been hired. This is a decreasing function in θ and we approximate it by $d(\theta, n_1) = d(\theta, 0) - \psi n_1$.[10] 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. Individual demands can be aggregated across firms with formal apprentices and firms without formal apprentices to obtain the aggregate partial demand for traditional apprentices: $D^{trad}(\theta, N_{form}) \approx D^{trad}(\theta, 0) - \psi N_{form}$, where N_{form} is the total number of formal apprentices in the market.

[9] Chances that young applicants find apprenticeship positions, or that firms find apprentices are directly linked to the market tightness and underlying matching function.

[10] The partial demand 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 ($c(\theta)$).

Absent any intervention, the supply and demand functions determine an equilibrium in which the tightness is θ_0, and the total number of apprentices is N_0, which is represented as point E_0 in Figure 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 how the intervention shifts the firms' demand function downward by ψN_{form} (which captures substitution effects). The figure also shows a downward shift in the supply function on the youth side by $Wind = \sigma_a S^{trad}(\theta_0)$ (which is the "true" Windfall effect). The vertical arrow shows the estimated windfall effect, which is measured at the market conditions θ_1 that prevail after the formal apprenticeship program is introduced, so that $Wind^m = \sigma_a S^{trad}(\theta_1)$.

We now consider the effect of an intervention offering formal apprenticeships to a share σ_a of youth who will be placed in a set of firms (in a share σ_f of local firms). The intervention provides a subsidy to youths and alters the perceived long-term gains from apprenticeships, as such leading some youths who would not have entered traditional apprenticeships to

enter formal apprenticeships. Conversely, some youth entering formal apprenticeships would have participated in the market for traditional apprenticeships. Their transition to formal apprenticeship induces a downward shift in the aggregate supply of traditional apprentices, which becomes $(1-\sigma_a)S_a^{trad}(\theta)$.

Consider the number of youths assigned to the treatment group (A), of which some (N_{form}) enter formal apprenticeships. The entry rate is $\tau_1 = N_{form}/A$. The windfall effect ($Wind$) is the number of those youths who would have entered traditional apprenticeship absent the intervention: $\sigma_a S_a^{trad}(\theta_0)$. It can be expressed as a share of youths in the treatment group: $\tau_0 = Wind/A$. The impact of the program on the share of youths in apprenticeship is thus $\tau_1 - \tau_0$. The share of youth in formal apprenticeship who would have entered traditional apprenticeship absent the intervention is the second key parameter of the experiment: $\omega = \tau_0/\tau_1 = Wind/N_{form}$.

The intervention leads to the entry of formal apprentices in firms which, as described before, induces a downward shift in the demand for traditional apprentices. The demand for traditional apprentices becomes $D^{trad}(\theta, 0) - Sub$, where $Sub = \psi N_{form}$ captures the substitution effect.

The introduction of the intervention leads to a new equilibrium represented by point E_1 (θ_1 and N_1^{trad}) in Figure 2. If $-A_d$ and A_s denote the slope of the demand, respectively the supply functions, we can express the change in the equilibrium as a function of ω and ψ:[11]

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

$$(2) \qquad \theta_1 - \theta_0 = (\omega - \psi)\frac{N_{form}}{(1-\sigma_a)A_s + A_d}$$

We cannot measure the true windfall effect at market tightness θ_0, but are able to measure the share of youth who would have taken an apprenticeship position at the new market tightness θ_1. As such, we can define $Wind^m = \sigma_a S^{trad}(\theta_1)$, as well as $\tau_0^m = Wind^m/A$ and $\omega^m = \tau_0^m/\tau_1$. This is sufficient to bound the effect of the intervention on the net number of apprenticeship positions. Indeed, the impact on the net number of apprentices and the market tightness adjustment can also be easily expressed as a function of the parameter

[11] Alternatively, as a function of $Wind$ and Sub, we get $N_1^{trad} - N_0 = ((1-\sigma_a)A_s Sub + A_d Wind)/((1-\sigma_a)A_s + A_d)$ and $\theta_1 - \theta_0 = (Wind - Sub)/((1-\sigma_a)A_s + A_d)$.

we can estimate (ω^m) instead of ω (see Appendix A2 for details). We get the following expressions:

$$\frac{N_1^{trad} - N_0}{N_{form}} = -\frac{A_s\omega^m + A_d\psi}{A_s + A_d} \tag{3}$$

$$\theta_1 - \theta_0 = (\omega^m - \psi)\frac{N_{form}}{A_s + A_d} \tag{4}$$

The parameters ω, ω^m and ψ are expected to be positive, so that the decrease in the number of traditional apprentices driven by the shifts in the supply and demand functions combine positively. Importantly, as Equations 1 and 3 show, the overall reduction in the number of traditional apprentices is an average of the windfall effect on the supply side and the substitution effect on the demand side.

As already mentioned, Equations 1 and 3 also make clear that the net employment effects can only be bounded if both windfall and substitution parameters (in our case, ω^m and ψ) are estimated. This is not typically possible in one-sided experiments focusing on either the demand or supply side of the labor market. However, our double-sided design allows measuring both parameters, and as such estimating bounds for the net number of positions created by the formal apprenticeship program. 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.

Equation 4 can nevertheless 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^m 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). It follows that the order of magnitude of $\theta_1 - \theta_0$ is of the size of the experiment relative to the market (σ).[12] As discussed in Section 2.3, the relative

[12]The size of the intervention can be characterized by the share of youth enrolled in the experiment σ_a

size of the experiment is modest. This is another reason to expect the adjustment in market tightness to be small.[13]

3.2 Parameters to be Estimated

The framework shows which parameters can be estimated based on the experiment, and how equilibrium effects factor in.[14]

First, we would like to estimate the "true" impact on program applicants' decision to enter apprenticeship. The true program impact among applicants is

$$True = \tau_1 - \tau_0 = \tau_1 - Wind/A = \tau_1 - \frac{\sigma_a S^{trad}(\theta_0)}{A} \tag{5}$$

Instead, we compare the share of applicants entering apprenticeship in the treatment and control groups observed under the new equilibrium tightness θ_1 (not under θ_0) so that:

$$Measured = \tau_1 - \tau_0^m = \tau_1 - Wind^m/A = \tau_1 - \frac{\sigma_a S^{trad}(\theta_1)}{A} \tag{6}$$

However, as Figure 2 illustrates, the difference is small, and it can be shown that it is of the same order of magnitude as $\theta_1 - \theta_0$.[15] As already discussed, there are solid reasons to assume that this difference is negligible. Comparing the share of applicants in the treatment and control group entering traditional apprenticeship measures $-\tau_0^m$.

Second, we run an instrumental variable regression of being an apprentice on being a

and the share of firms hosting formal apprentices σ_f.

[13]Note that the experimental design also maintains a similar ratio of apprentices and available vacancies in the treatment and control groups. Half the youths and half the firms registered in the experiment were assigned to treatment and the other half to control. This ensures that the chances of a match between firms and youth in the control group (which are just part of the possible matches) are unchanged.

[14]To simplify, the framework assumes that a share σ of firms or youth is randomly drawn form large populations, and then randomly assigned to treatment and control groups. This would imply that the groups are representative of the populations. This is not exactly how the experiment was implemented. Instead, information about the program was provided, and interested youths and firms registered to participate. Randomized assignment then took place among self-selected applicants. In this case, the resulting treatment and control groups are not representative of the overall population. This may affect the generalizability of the findings, but not their internal validity.

[15]The indirect effect $Indirect = True - Measured$ is given by $Indirect = \sigma_a(S^{trad}(\theta_0) - S^{trad}(\theta_1))/A \approx \sigma M/A(\theta_1 - \theta_0)$. Thus it is of the order of magnitude of $\theta_1 - \theta_0$.

formal apprentice, using the assignment variable an instrument, to estimate:

$$1 - \omega^m = \frac{N_{form}/A - Wind^m/A}{N_{form}/A} = 1 - \frac{Wind^m}{N_{form}} \tag{7}$$

By running an instrumental variable regression of being a traditional apprentice on being a formal apprentice, using the assignment variable as instrument, we estimate a parameter $b_y^1 = -\omega^m$.

Third, we estimate the impact on the number of apprentices in firms by comparing treatment and control firms. The total number of apprentices is $n_{tot} = n_f + d(\theta_1, n_f) = d(\theta_1, 0) + (1 - \psi)n_f$ in treatment firms, and $n_{tot} = d(\theta_1, 0)$ in control firms. Since all firms share the same market conditions, the comparison between the two provides exactly the substitution effect:[16]

$$E(n_f + d(\theta_1, n_f)) - E(d(\theta_1, 0)) = (1 - \psi)E(n_f) \tag{8}$$

Similarly, comparing the number of traditional apprentices entering the firm between treatment and control firm estimates $E(d(\theta_1, n_f)) - E(d(\theta_1, 0)) = -\psi E(n_f)$.

Fourth, we run an instrumental variable regression of the number of apprentices in firms on the the number of formal apprentices instrumented by the assignment variable. This estimates:

$$1 - \psi = \frac{E(n_f + d(\theta_1, n_f)) - E(d(\theta_1, 0))}{E(n_f)} = 1 - \frac{Sub}{N_{form}} \tag{9}$$

Moreover, by running an instrumental variable regression of the number of traditional apprentices in firms on the the number of formal apprentices instrumented by the assignment variable, we estimate:

$$b_f^1 = \frac{E(d(\theta_1, n_f)) - E(d(\theta_1, 0))}{E(n_f)} = -\psi = -\frac{Sub}{N_{form}}$$

In the detailed presentation of the framework in Appendix A2, we also consider additional questions such as the impact of the intervention on firm profits. In particular, we show that

[16]Note that the true impact on the number of apprentices in firms would be $E(n_f + d(\theta_1, n_f)) - E(d(\theta_0, 0)$ which is $d(\theta_1, 0) - d(\theta_0, 0) + (1 - \psi)E(n_f)$. Again, when the adjustment in market tightness is negligible, we estimate the true impact on firms.

the impact on profits captures a compensation for firms to provide training to apprentices.

Our design randomly assigns youth and firms within a same micro market. Some alternative designs are worth mentioning. (Appendix A2 provides more details about alternative designs, including the parameters they identify and their advantages and disadvantages.) First, micro markets could have been randomly assigned to treatment and control. This would have allowed to directly measure the impact on the number of apprenticeship positions. However, the estimates would have been difficult to interpret outside the experiment. For example, the parameter ψ would not be identified. Moreover such a design would have been difficult to implement in the context of the study given the small number of micro markets (111) and the heterogeneity across micro markets. Second, a two-step design could have been put in place, first randomly assigning micro markets to treatment or control, and then assigning youth and firms within treatment micro markets to treatment or control. Such a design would have allowed to measure a broader set of parameters, including the impact on the net number of apprenticeship positions as well as the windfall and substitution parameters. However, this design was not feasible in the context of the study due to the number of micro markets and observations within micro-markets. Still, as shown above, our design is adequate to estimate the key windfall and substitution parameters, as well as provide tight bounds for the net number of apprenticeship positions created by the program.

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 youth 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 youth took place shortly after, and placements were mostly completed by January 2015.

The follow-up surveys 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.[17] Since most apprenticeships last 24 months, results based on the follow-up survey should be interpreted as providing short-term impacts while apprentices are still in the program.[18]

Substantial efforts were made to minimize attrition during the follow-up survey.[19] As a result, 1,661 youth were surveyed, implying a response rate of 90.7% (or an attrition rate of 9.3%, with 171 youth not surveyed, 84 in Treatment and 87 in Control). Similarly 674 firms were surveyed, leading to a response rate of 92.2% (or an attrition rate of 7.8%, with 57 firms not surveyed, 26 in Treatment and 31 in Control).[20] The response rates in the follow-up survey are balanced across the treatment and control groups.[21]

Tables A2 and A3 present baseline characteristics and balance checks for youth, 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.

Table A2 shows that youth 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. 45 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 youth, who are largely comparable and well-balanced at baseline. As can be seen from the table, the share of available baseline data (see footnote 19) is not perfectly balanced, but the response rate at

[17] Figure A3 documents precisely the timing of surveys as a function of the randomization date.

[18] 754 of the 914 youths in the treatment group (or 82%) were in trades where the apprenticeship contract lasted 24 months.

[19] 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 youth, 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 youth (and firms) at follow-up. This contributed to a lower response rate among youths in localities where IT issues occurred.

[20] 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).

[21] The last row of Table A2 presents the balance check for the response rate among youths (see last column, p-val=0.92), and the last row of Table A3 contains the balance check for the response rate among firms (see last column, p-val=0.48).

follow-up is 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 68 percent do not keep books. Firms have 6.3 permanent employees on average (counting the owner), of which 3.3 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, 82 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 follow-up youth survey, the most important variables for the analysis are 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 apprentice (formal or traditional), wage worker and self-employed. Other important variables relate to youth human capital investments, including participation in apprenticeship (formal or traditional), vocational training and schooling.

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. For each apprentice, the survey also asks 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.[22] 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 six 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:

$$y_i = a + bT_i + \sum_v \gamma_v 1_v + \sum_s \delta_s 1_s + u_i \tag{10}$$

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. Sectors are broader than trade.[23]

In parallel, intent-to-treat program impacts on youth can be estimated by comparing outcomes between youth assigned to treatment (i.e. offered a formal apprenticeship position in a treatment firm), and control. We account for the fact that youth 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

[22] 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 youth 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.

[23] 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.

each stratum $\widehat{\pi_m}$ and estimate inversely propensity weighted regressions. The weights are simply defined as $T_i/\widehat{\pi}_m + (1-T_i)/(1-\widehat{\pi}_m)$.[24] To obtain accuracy gains from stratification, we run an inversely propensity weighted regression with strata dummies on the 1,661 youth observations:

$$y_i = a + bT_i + \sum_{St} \mu_{St} 1_{St} + u_i \tag{11}$$

We compute White-Huber robust standard errors.[25]

As a robustness check, we also implement permutation tests for the main ITT estimates for youth 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.

[24]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 youth observations 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.

[25]In our framework $\sigma_a S^{Trad}\theta_1/A$ is a key parameter. It estimates the share of youth who do not enter the traditional apprenticeship market following the introduction of the intervention. We measure it as the entry rate of control youth in traditional apprenticeship. Or, considering the presence of always takers in the the treatment group, it is also minus the ITT estimate for the decision to enter traditional apprenticeship. In our design, youth are randomly assigned to treatment with probabilities π_m, which depend on the micro market. By using the weights mentioned previously $(T_i/\widehat{\pi}_m + (1-T_i)/(1-\widehat{\pi}_m))$ we obtain estimates at the level of micro markets, weighted by the size of the experiment in each micro market. An alternative weighting scheme would be to weight observations by $T_i + (1-T_i)\widehat{\pi}_m/(1-\widehat{\pi}_m)$, so that estimates at the level of the micro market would be weighted by the size of the treatment group in each micro market. In practice the difference is negligible.

5 Windfall and Substitution Effects

5.1 Youth Entry into Apprenticeship and Windfall Effects

Table 2 presents ITT estimates of human capital investments for youth, covering the experiment period between randomization and the follow-up survey. We consider human capital investments in the form of schooling and training. As part of training, we distinguish between technical and vocational training (TVET) and apprenticeship. Participation in apprenticeship is further decomposed between traditional apprenticeships, i.e. private arrangements that exist independently of the program, and formal apprenticeships of the type promoted by the program.

The left part of the upper panel contains some of the key parameters from the conceptual framework in Section 3. It documents the impact on entry into apprenticeship, including formal (column e_1), traditional (column e_2) or either formal or traditional apprenticeship (column e_3). It clearly shows that the program leads to a large increase in participation in formal apprenticeship: the share of youth in formal apprenticeship is 71.2 percentage points larger in the treatment group than in the control group.[26] The estimated increase in the participation in formal apprenticeship corresponds to the parameter τ_1 of the conceptual framework: $\tau_1 = 0.712$.[27]

Column e_2 of the table documents a significant windfall effect. The participation in formal apprenticeship is in part associated with substitution out of traditional apprenticeships. 22.5 percent of youth in the control group participate in traditional apprenticeships but this proportion is reduced by 18.5 percentage points in the treatment group. This is the estimate of the parameter τ_0 in the conceptual framework, which relates to the windfall effect: $\tau_0 = 0.185$. The impact of offering participation in the program on entry into apprenticeship is given in column e_3: $\tau_1 - \tau_0 = 0.528$.

The right part of the upper panel of Table 2 documents the impact of offering participation in the program on other forms of human capital investments. The program induced small

[26] A very low, but non-zero participation in formal apprenticeship is observed in the control group. Overall, 75 percent of youths in the treatment group participate in formal apprenticeship, which is highly consistent with program take-up measures based on process evaluation or administrative data. Appendix A3 and Table A4 provide additional information on take-up measures from other data sources.

[27] The top panel of Table A6 in appendix presents the results from permutation tests for the main ITT estimates on participation in apprenticeship among youth (first 3 columns). There is a remarkable concordance between asymptotic results and results from permutation tests.

substitutions out of other forms of human capital investments such as schooling or technical and vocational training (TVET).[28] Still, a large proportion of youth makes no human capital investment of any form in the control group (51.4 percent). Offering participation in the program sharply reduces this proportion by 36.3 percentage points, approximately half the program take-up.

So far, we have focused on impacts on entry into apprenticeship between the baseline and follow-up surveys, which captures inflow into apprenticeships. In Table 2, the right part of the lower panel (columns labeled c) presents ITT estimates of impacts on youth participation in apprenticeship at the time of the follow-up survey (i.e. approximately 20 months after the start of the program). The left part of the lower panel of Table 2 presents ITT estimates for the share of youth who started apprenticeship but dropped-out before the follow-up survey.[29]

Drop-out is a common issue in many employment or training programs. It has been shown to be important in apprenticeship as well (e.g. Cho et al. (2013)). We can compute drop-out rates from Table 2. Drop-out is estimated as $0.222/0.712$=31.2% in formal apprenticeships and $0.060/0.185$=32.5% in traditional apprenticeships. These results show that drop-out is not an issue specific to formal apprenticeships as the drop-out rate in traditional apprenticeship is close. These results nevertheless show that the program did not reduce the prevalence of drop-out compared to traditional apprenticeships.

Results on participation in apprenticeship at the time of the follow-up survey confirm previous findings on increased participation but also significant windfall effects. The share of youth in formal apprenticeship increases by 49 percentage points. However, the share of youth in any type of apprenticeship increases by only 36.5 percentage points. The difference between the two estimates, 12.5 percentage points, provides another estimate of the windfall effect, namely the share of youth who substituted out of traditional apprenticeships to be in formal apprenticeships at the time of the follow-up survey.

[28]To build the human capital investment variables, we use a question in the follow-up survey about participation in apprenticeship and TVET, as well as a separate question about youth participation in public apprenticeship programs. We classify as participation in formal apprenticeship those who are either in apprenticeship or TVET and say they were registered in a public apprenticeship program. As we detail in Appendix A3, although not perfect, and subject to some recall bias, this is the classification that is most consistent with information from the process evaluation survey for the subsample of treated youth that can be matched in that survey.

[29]Those estimates (labeled as d in the table) are simply obtained as the difference between estimates of entry in apprenticeship since the start of the experiment (labeled e) and estimates of youth still in apprenticeship at the time of the follow-up survey (labeled c).

5.2 Intake of Apprentices and Substitution Effects in Firms

We now turn to the other core research question of whether the program induced indirect employment effects in firms.[30] Table 3 (upper panel) documents the impact of the program on the flow of apprentices into firms since the date of the randomization.[31] The program led to an increase in the total number of apprentices that entered by 1.080 apprentice per firm over the course of the experiment. Yet the total number of youth who entered formal apprenticeship in these firms increased by 1.398. These estimates relate to the conceptual framework, with 1.080 corresponding to the left hand side of equation (8) ($E(n_f+d(\theta_1,n_f))-E(d(\theta_1,0))$, and 1.398 to the right hand side ($E(n_f)$).[32] The difference between the two points to a substantial substitution effect, with a reduction of 0.318 traditional apprentices entering firms.

The net increase in formal apprentices in treatment firms can be contrasted with the number of apprenticeship positions offered by these firms. On average, firms offered 2.51 positions.[33] A substantial share of these positions was not filled, and the placement ratio is relatively weak: only $1.398/2.51 = 55.5\%$ of positions offered by firms were effectively filled. This pattern is consistent with substantial drop-out among selected youths. It also suggests that firms face challenges in attracting youths, even despite the program subsidy.

Table 3 (intermediate panel) documents the impact of the program on exit of apprentices from firms. Exits of apprenticeship are substantial. Out of 1.08 apprentices who entered firms due to the intervention, 0.467 apprentices had exited by the time of the follow-up survey. This amounts to a 43.2% drop-out rate. As discussed in the previous section, drop-out is a pervasive aspect of apprenticeship. While it is large, it is not higher in the treatment

[30] As explained in Section 4, 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 the randomization. We can thus measure flows of apprentices in and out of firms between the start of the experiment and the follow-up survey.

[31] The bottom panel of Table A6 in appendix 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.

[32] Figure A4 provides additional information about the impact of the program on the inflow of apprentices 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.

[33] There are 911 youth assigned to 361 firms in the full treated sample (see Section 2.3), resulting in an average of 2.52 per firm. In the regression sample, 864 youths were assigned to 334 firms resulting in an average of 2.51 per firm.

group than in the control group.[34]

Table 3 (lower panel) documents net program impacts on the number of apprentices who entered since randomization and are still in firms at the time of the follow-up survey. Results can simply be deduced as the difference between the top two panels. They show that, 20 months after the launch of the program, there are 0.613 total apprentices per firm that entered since the date of randomization, and 0.787 additional formal apprentices per firm. The difference, 0.174, which in this case is not statistically significant, provides the estimated substitution effect at the time of the follow-up survey.[35]

The follow-up firm survey also provides information about the total workforce in firms (including apprentices and other types of employees). Results are presented in appendix Table A7. While there is a significant 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.[36] Consistent with the lack of significant impact on the total number of apprentices in firms, the program does not have an impact on the overall workforce in firms at follow-up. No significant impact is found on the number of interns or occasional workers either.

In terms of measurement, the study shows the importance of carefully defining outcome measures. If we had implemented a simple survey only asking about the number of employees and apprentices in firms, we would have concluded that there was no significant impact on employment. However, this stock measure would have missed important impacts of the program. Indeed, the impact on stock is the combination of impact on inflows and outflows (see footnote 36). In addition, the flows between the start of the experiment and follow-up combine both entry and dropouts. With an employer-employee survey, data on the dates of

[34]If we consider youth who entered firms within 6 months after randomization, we find drop-out rates of 43.3% in the treatment group and 47.1% in the control group.

[35]As can be seen in the top two panels of Table 3, there is also a bit of non-compliance with the experimental protocol. The average number of formal apprentices who entered since the start of the experiment per control group firm is 0.188. This imperfect compliance did not last long: most (0.13) formal apprentices in control firms had left by the time of the follow-up survey. The non-compliance was highly concentrated in two out of the seven localities in which the program was implemented.

[36]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 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).

entry and exit for each apprentice enable us to build a more precise set of measures of both entry and exit, providing a richer understanding of indirect impacts in firms.

5.3 Net Impact on Apprenticeship Positions Created

So far, we have discussed ITT estimates of offering youth to enter formal apprenticeships, and on assigning formal apprentices to firms with open positions. Results show that there are significant windfall effects for youth 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^m and ψ, and consider how windfall and substitution effects combine.

We first obtain LATE estimates that represent impacts per youth entering formal apprenticeships. On the youth side, we estimate:

$$a_i = \alpha + (1 - \omega^m) f_i + \sum_{St} \mu_{St} 1_{St} + u_i \tag{12}$$

where a_i stands for having started an apprenticeship since the beginning of the experiment and f_i for having started a formal apprenticeship. We estimate this equation using the treatment assignment variable as an instrument. In the conceptual framework, ω^m relates to the windfall effect and captures the proportion of formal apprentices who would have started an apprenticeship anyway absent the program.

We also analyze the impact of entries into formal apprenticeships on the total number of entries of apprentices in firms. We estimate:

$$n_{tot,i} = a + (1 - \psi) n_{f,i} + \sum_{v} \gamma_v 1_v + \sum_{s} \delta_s 1_s + u_i \tag{13}$$

where $n_{tot,i}$ is the total number of youth entering apprenticeship in firms and $n_{f,i}$ is the total number of formal apprentices entering firms. As highlighted in the conceptual framework, for each formal apprentice entering firms, there is ψ less youth entering as a traditional apprentice. This equation is also estimated using the treatment assignment variable as an

instrument.

Table 4 presents the results. The fist two columns contain the reduced form, which are the ITT estimates presented above. The third column presents IV estimates for (1-ω^m) 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^m. As can be seen from the table, the estimated value of the windfall parameter for youths is 0.259, with a standard error of 0.022.[37] On the firm side, there are 0.773 youths entering firms per formal apprentice placed, thus leading to an estimated substitution parameter of 0.227, with a standard error of 0.128.[38]

These findings have important implications. First, as shown in Equation 3, the reduction in the total number of traditional apprentices per formal apprentice placed is a weighted average of ψ and ω^m. 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:

$$\frac{A_s\psi + A_d\omega^m}{A_s + A_d} \in [min(\psi, \omega^m), max(\psi, \omega^m)] = [0.227, 0.259] \tag{14}$$

Since the two parameters ψ and ω^m 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.[39]

We can obtain Imbens-Manski confidence intervals for partially identified parameters (Imbens and Manski, 2004). The 95% confidence interval we obtain is $[-0.0098, 0.2997]$.[40]

[37] Instead of using the upper panel of Table 2, we could have estimated the windfall parameter using the lower panel of Table 2 which focuses on youths who started apprenticeship since randomization and are still apprentices at the moment of the follow-up survey. The estimated ω^m would have been very close: 0.125/0.490=0.255.

[38] Here again, instead of using the upper panel of Table 3, we could have estimated the substitution parameter using the lower part of Table 3, which focuses on apprentices who entered since randomization and are still present at the moment of the follow-up survey. The estimated ψ would have been very close again: 0.174/0.787=0.221.

[39] 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 (see footnote 37) and an estimated windfall effect of 0.255 (see footnote 38), 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.

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

In other words, the 95% confidence interval for the net number of positions created by the program is between 70.3% and 99% of the number of formal apprentices placed.

We can also assess the magnitude of market tightness adjustments. Note that estimates of the two parameters ω^m and ψ are of the same order of magnitude. As shown in Equation 4, the order of magnitude of the change in the market tightness is $\sigma(\psi - \omega^m)$, 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 4 clearly shows, the adjustment also depends on the difference between the two estimated parameters ψ and ω^m. The findings confirm ex post that the tightness adjustment is in practice very small.

6 Earnings for Youths and Firms

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 youth and firms. We now turn to analyzing the short-term impacts of the apprenticeship program on earnings for both youth and firms. This provides additional information on opportunity costs from participation in apprenticeship among youth, as well as potential indirect benefits to firms.

6.1 Youth Employment and Earnings

We first analyze short-term impacts of the program on youth employment, activities and earnings. Results show that there are substantial opportunity costs for youth to participate in apprenticeships. Table 5 documents ITT estimates (equation 11) for employment, hours worked and earnings by type of employment.

The upper panel of the table presents results on youth activities. It shows that youth 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 in economic activity (by 3.4 percentage points) and in the average number of

activities (by 0.05). However, the program induces youth 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 become apprentices (by 36.5 percentage points).

The intermediate panel of the table presents results about hours worked and shows similar effects. 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).[41]

The third panel of the table presents estimates of program impact on total earnings, earnings by source of employment, and non-labor earnings. Overall, the program has no short-term impacts on average earnings for youth. 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 employment 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. 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 increases, employment earnings decrease, and total earnings remain stable.[42]

The bottom panel of the table presents 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. The table shows that, in the control group, youths involved in apprenticeship earn

[41] 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 impacts of a public works program on employment mostly take the form of a reorganization of economic activities, as opposed to an increase in overall employment rates.

[42] The top panel of Table A6 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 again very close.

on average FCFA 628 per hour from their employers, far lower than hourly earnings in wage employment or self-employment (respectively FCFA 1030 and FCFA 1082). 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 (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 apprentices less.[43]

Overall, results show that the opportunity costs of participating in apprenticeship are quite large. Individuals are foregoing 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 is 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 youth with limited outside opportunities, participation in formal apprenticeship might lead to an increase in earnings, for example because of the subsidy. For other youth with better opportunities, the impact on earnings might be smaller, and even possibly negative. Figure 3 illustrates impact heterogeneity. We first consider the strong assumption of homogeneous zero treatment effect. The intermediate panel provides the results of the corresponding Mann-Whitney test. The test is implemented using a large number of permutations, which enables to obtain an exact p-value (Imbens and Rubin (2015)). The test has the advantage of being robust to outliers, and clearly rejects homogeneity. The upper panel of the figure displays estimates of the cumulative distributions of potential outcomes in the groups assigned to treatment (blue doted line) and to control (red doted line), as well as the confidence interval for the difference between the two. As can be seen from the figure, the cumulative distribution 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. However, only 71% of youth assigned to the treatment

[43]While assignment to treatment has no impact on youth earnings in the short-term, some small positive effects are found on the share of youth able to save. This may be due to the fact that the subsidy is paid regularly into bank accounts. Positive effects are also found on youth self-esteem in the short-term.

group entered formal apprenticeship. Even if the impact on participation in formal apprenticeship is constant, imperfect take-up can lead to the observed patterns in the distribution of potential outcomes. The lower panel of Figure 3 presents quantile treatment effects on the population of youth complying with assignment to formal apprenticeship.[44] The figure clearly shows a positive and increasing effect for low quantiles, but then a declining pattern and a change in sign. Quantile treatment effects at large quantiles are negative and significant. Although these quantile treatment effects cannot be estimated as effects at quantile (unless assuming rank preservation), the observed patterns are consistent with heterogeneity in treatment effects (see Heckman et al. (1997); Djebbari and Smith (2008)).

6.2 Value of Apprentices' Work and Profits in Firms

We now analyze how the increase in inflow of apprentices indirectly affects firms, including through changes in labor input (time worked), the value of work provided by apprentices, as well as firm profits. During the follow-up survey, firm owners were asked questions about each 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 aggregate apprentice-level measures in each firm across all apprentices who joined since randomization.

Table 6 documents impact on total labor input from apprentices who entered firms since randomization. The total time worked by apprentices at the time of the follow-up survey increases slightly. Firms see a small increase in labor input of apprentices by 6.9 days or 55.9 hours per month. This effect represents a 23 percent increase in days worked by apprentices. These results are statistically significant, but given the size of the increase in the number of apprentices at follow-up (0.613, as can be seen from Table 3), they are actually relatively small. For example, the number of days worked per new apprentice entering firms is $6.9/0.613 \approx 11$. One simple explanation for this limited impact is that there are more youth working, but they work less hours. In Section 7, we use disaggregated apprentice-level data to show that absenteeism (a pervasive phenomenon in apprenticeship) is in fact higher among formal apprentices.

[44] We estimate unconditional instrumental variable treatment effects as developed by Frölich and Melly (2013), in which entry into formal apprenticeship is instrumented by the assignment variable.

One important question pertains to the overall value of work provided by apprentices in firms. In the follow-up survey, we 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 can then estimate the value of work performed by each apprentice by multiplying this estimated value of work by the number of days worked in the last month. This apprentice-level measure is then aggregated at the firm level across all apprentices who started since randomization. The third column of Table 6 shows that the program led to a strongly positive and significant increase of the value of work by apprentices in treatment firms. The estimated value of work by apprentices increases by 25 543 FCFA per month, a significant 62 percent increase.

Separately, we can estimate the payments made by firms to apprentices (wage bill). The survey describes precisely the types of compensation received by apprentice for meals, transportation, clothing and "motivation", including both in cash and in-kind payments. We sum all these components at the apprentice level and aggregate again at the firm level. We also compute a net value of work at the firm level by taking the difference between the value of work and wage bill for apprentices. Columns 4 and 5 of Table 6 show that although the number of apprentices substantially and significantly increased, the total wage bill for apprentices did not increase significantly in treatment firms. A small increase in the wage bill is observed as employers provide some payments to program apprentices, but the increase is not significant. 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 by apprentices in control firms.[45]

As detailed in the conceptual framework in Appendix A2, we can compare the production of apprentices $f(n_a, n_f)$ net of the total wage bill $(w_a n_a + \widetilde{w}_a n_f)$ between treated and control firms. Denoting this net value π, we obtain:

$$(15) \qquad E(\pi(1) - \pi(0)) = [(c(\theta_1) + \Delta)(1 - \psi) + (f'_{n_f} - \widetilde{w}_a - c_1 - \Delta) + c_1 - c(\theta_1)]E(n_f)$$

The difference in the net value of apprentices' work has three components. The first

[45]The bottom panel of Table A6 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 again very close.

component is linked to the search cost and to the part of the value of apprentices' work that the employer keep as a compensation for the training provided: $(c(\theta_1) + \Delta)(1 - \psi)$. The second component is the difference between the marginal productivity of formal apprentices and their total cost: $f'_{n_f} - \widetilde{w}_a - c_1 - \Delta$. As firms were rationed in the number of apprentices they get, this component is positive. The last component is the difference between search costs for formal and traditional apprentices: $c_1 - c(\theta_1)$. It is most likely negative. It is difficult to determine precisely the source of the large difference we observe. Because of the considerations above, however, it is likely that the substantial observed difference is largely due to the first component. Whether it stems from the search cost or the compensation for training provision (Δ) cannot be ascertained. A large compensation for training provision would be consistent with Acemoglu and Pischke (1999), however.[46]

Since we observe an increase in the net value of work by apprentices, a natural question is whether we can also detect an impact on firm profits. Measures of firm sales and profits are notoriously noisy. In the context of the study, we collected up to six measures of sales and six measures of profit, $y_{im}, \quad m \in \{1, \ldots, 6\}$.[47] We estimate the following regression pooling all the observations together, and including dummies for each type of measure:

$$y_{i,m} = a + bT_i + \delta_m + v_{i,m} \tag{16}$$

Results are presented in Table A8. We do not detect any significant effect on average sales or profits, either using the variables themselves or an inverse hyperbolic sine transformation. These results suggest that, while the net value of work from new apprentices increases, no significant impacts are found on average firm profits. This could be due in part to more limited statistical power given the dispersion of the profit variables.[48]

[46]Notice that the number of hours that apprentices spend working under the direct supervision of master trainers is rather limited, suggesting youth learn largely by working alongside master craftsmen, seemingly inducing limited investments in time spent by master craftsmen solely teaching apprentices (see lower panel of Table A10, which we discuss in Section 7).

[47]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 to obtain a second measure. Based on this listing, we then asked them to report again sales and profits, which provides a third (and our preferred) measure. Moreover, experienced supervisors conducted near-systematic back-check of firm surveys to obtain repeat measures (for 598 out of 677 firms).

[48]Figure A5 provides additional information on impacts on the distribution of revenues and profits. It shows that the point estimates for program impacts on firm profits and revenues are positive for most of the distribution. However, while these effects are significantly positive in a few parts of the distribution, they are not significant overall. They also tend to be negative at the top of the distribution, driven by a few firms.

6.3 Summing up Impacts on Earnings

As in Section 5.3, we now obtain LATE estimates for impacts on earnings per formal apprentice placed by the program. We first estimate impacts on youth who were in formal apprenticeships at some point since randomization. We simply estimate parameter b_I of the following equation:

$$Income_i = a_I + b_I \times f_i + \sum_{St} \mu_{St} 1_{St} + u_i \tag{17}$$

f_i captures participation in formal apprenticeship (since the start of the experiment), which we again instrument by the youth treatment assignment variable.

On the side of firms, we measure the impact of the entry of one formal apprentice on the net value of work performed by apprentices in firms as parameter b_S of the following equation:

$$Net\ value\ of\ work_i = a_S + b_S \times e_i^f + \sum_{v} \gamma_v 1_v + \sum_{s} \delta_s 1_s + u_i \tag{18}$$

e_i^f stands for the number of formal apprentices entering firms. Again, we use firm treatment assignment as an instrument.

Other indirect effects also need to be considered when accounting for program impacts on youth and firms. As seen in Section 5.3, the entry of formal apprentices in treatment firms crowds out some traditional apprentices. The loss of these traditional apprentices should also be taken into account in a comprehensive cost-benefit analysis. They are, however, complicated to value.

Table 7 presents the results. The upper panel presents the reduced-form estimates discussed earlier. The lower panel presents instrumental variable estimates, which are simply the ratio of the first two rows in the upper panel. The first column of the table presents results for youth and the second column results for firms. Results show that, unsurprisingly, there is no significant impact on youth, with a non-significant reduction of FCFA -1 977 in total earnings per youth entering formal apprenticeship. On the other hand, at the firm level, the increase in the net value of work is positive, large and significant, with a value of FCFA 27,165 per formal apprentice. The sum of the two effects is FCFA 25 188. Note

that this is not significantly different from the subsidy of 30,000 FCFA paid per program apprentice. Thus, even if program impacts on average firm profits are not significant, the estimated net value of work provided by apprentices is close to the subsidy paid by the program. This suggests that indirect effects on firm may be nearly sufficient to make the program cost-effective in the short-term.

7 Additional Mechanisms

We now turn to analyzing a range of additional mechanisms that shed further light on the results. First, as we have shown, the intervention leads youths to enter apprenticeships, which some of them would not have done absent the program. There is a new population of apprentices entering firms, which partly substitutes for a population of traditional apprentices. These populations can be compared to document patterns of selection into (formal) apprenticeships. Second, we analyze the performance of formal apprentices in firms, and document their higher productivity and absenteeism. Third, we show how the program (and the wage subsidy) affect payments between apprentices and firms.

7.1 Youths' Selection into Apprenticeship

We first explore the characteristics of youths who entered any form of apprenticeship ($a = 1$) thanks to the intervention. Are these youths similar to those who would have entered apprenticeship anyway? We categorize the status of youth with respect to apprenticeship as "Always-takers", "Compliers" and "Never-takers".[49] We are particularly interested in the comparison of baseline characteristics between Compliers and Always-takers.[50] Results are presented in Table A9. 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-takers, and the second column the estimated mean for Compliers. For

[49]Note this is different from applying those concepts to the decision to enter formal apprenticeships promoted by the program

[50]Following Abadie (2003), the average characteristic x of "Compliers" is obtained through the regression of ax on a using T as an instrumental variable. The "Always-taker" population is directly observable as those for whom $a = 1$ and $T = 0$. Testing the equality of means between Compliers and Always-takers is simply obtained as the test of $\alpha_{aT} = 0$ in the regression $E(x|a,T) = \alpha_{aT}aT + \alpha_{T(1-a)}T(1-a) + \alpha_{(1-T)(1-a)}(1-T)(1-a)$, in which the excluded category is $a(1-T) = 1$, the "Always-taker" population (in $T = 0$) and $aT = 1$ identifies the population of Always-takers and Compliers (in $T = 1$).

completeness, the third column presents the mean for Never-takers. The last column contains the p-value for the test of equality in means between Compliers and Always-takers. The variables are organized by domains: demographics, skills, employment and earnings, aspirations and jobs search, socio-economic background and financial constraints.

In the conceptual framework detailed in Appendix A2, we discuss how the subsidy can affect youth decision to enter the apprenticeship market and highlight two classical margins. The first margin predicts that compliers should have better current labor market opportunities than Always-takers. The table does not provide strong support for this. The only noticeable difference is that the share of Compliers aspiring to become self-employed is lower. It is not clear whether this indicates more limited economic opportunities. The second margin predicts that compliers should be more financially constrained. Results do not provide supportive evidence either. These results are only based on proxies of financial constraints at baseline. One of the key findings from the previous section is that entering apprenticeship entails large opportunity costs in terms of forgone employment opportunities. In this context, the program can help address constraints to access apprenticeship either because there are few financial instruments to smooth consumption over the long duration of an apprenticeship, or because opportunity costs are so high they make apprenticeship not profitable.

We can also document changes in the population of youths entering firms as apprentices. The 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 heterogeneity in the apprenticeship population:

$$x = a + b_f f + b_{tT} tT + \sum_s \gamma_s 1_s + \sum_t \delta_t 1_t + u \tag{19}$$

Traditional apprentices entering 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 the selection effect. The second parameter compares the population of traditional apprentices between treatment and control firms. This allows to document the

characteristics of youth crowded out of apprenticeships by the entry of formal apprentices.

The top panel of Table A10 presents the results. It shows that formal apprentices are older (by 2.10 years), and more likely to be women (by 11 percentage points) than apprentices in control firms. Formal apprentices also have a higher education level: 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. The program therefore places youths that are more educated than the workforce traditionally hired by firms. This is consistent with traditional and formal apprentices being imperfect substitutes. There is no indication that the program helped insert youths with more limited networks, or more disadvantaged socio-economic backgrounds.[51]

7.2 Apprentices' Performance

Section 6.2 showed that, despite an increase in the total number of apprentices entering firms, there is only a weak increase in the total number of days worked, but a large increase in the value of apprentices' work in firms. In this section, we further explain these results by looking at disaggregated data at the apprentice level. The upper panel of Table A10 showed results estimating equation 19 with dependent variables for apprentice attributes. The purpose was to document selection into apprenticeship. The middle panel of Table A10 presents the results of the estimation of equation 19 on a set variable obtained from the follow-up firm survey. This allows documenting differences by type of apprentices at follow-up. These differences cannot be given a causal interpretation. They can be explained by selection effects, by some of the effects of program participation, but also by the difference in experience across apprentices.[52] Still, these results are useful to shed further light on the aggregate impacts observed in firms.

We first analyze apprentices' participation in firm activities and their productivity. We find two main results. First, absenteeism is more important among formal apprentices than other apprentices: traditional apprentices in control firms worked on average 20.14 days, but

[51]The table also shows that there are very few differences in characteristics between traditional apprentices in treatment and control firms (see row labeled "Traditional" in top panel). This suggests that the crowding-out taking place was not associated with stronger selection of a particular profile of apprentice.

[52]The inflow of formal apprentices took place within six month after the start of the experiment, while other apprentices entered over the whole 20 month period (see figure A4).

the average for formal apprentices is smaller by 7.09 days. Second, formal apprentices are more productive than traditional apprentices in control firms. The value of tasks performed the last day of work is on average FCFA 1,296 for traditional apprentices in control firms. It is higher by FCFA 839 for formal apprentices.

To analyze differences in performance, we build several indices of skills: a technical skill index, a behavioral skill index and a learning skill index.[53] Results show that formal apprentices in treatment firms have higher technical skills than traditional apprentices, which can contribute to explain their higher productivity.

The results can also explain the higher absenteeism observed among formal apprentices. One striking result is that employers rate formal apprentices as having lower behavioral skills than traditional apprentices. Looking into the components of the index, the items that drive the results are related to absenteeism and punctuality. There are various possible explanations for a higher absenteeism of formal apprentices. First the program might attract youths who are less interested in apprenticeships in the first place. A second explanation is that the program might not meet youth expectations. The bottom panel of the table shows levels of satisfaction reported by apprentices in treatment and control firms. Formal apprentices are more dissatisfied in general than traditional apprentices, and this is largely driven by a dissatisfaction with the level of earnings received from firms. Interestingly, they are less satisfied with their labor earnings from apprenticeship than with their income in general. These results are consistent with the impacts on earnings documented above.

7.3 Payments between Employers and Apprentices

We now document how the program modified payments between apprentices and firms. The middle panel of Table A10 details the financial arrangements between employers and youths or their families. Traditional apprenticeship arrangements involve the apprentices

[53]Skills are measured using a set of questions asked to the employer about each apprentice. The technical skills index includes two general questions about how well apprentices master techniques, tools and safety procedures. It also includes questions specific to each trade: for each trade, we worked with the national training agency (AGEFOP) to identify a list of 2 to 7 technical tasks and asked the employer how well each apprentice performed these tasks (on a scale from 0 to 10). The apprentice-level technical skill index is the average of the scores obtained across the trade-specific questions and the two general questions. The learning skills and behavioral skills indices each average several general questions. For the learning skills index, these include: ability to learn, quantity of work, quality of work, speed at work. For the behavioral skills index, these include attitude at work such as: absenteeism, punctuality, respect of clients and boss, seriousness and motivation.

(or their family) paying a fee to the master craftsmen. Over time, firms start compensating apprentices, with payments divided between a regular payment (said to be for "soap") to cover transport costs, room and board, and a "bonus" payment to motivate apprentices. The payment for transport, room and board is often made weekly, and can be partly in kind (e.g. meals). The "bonus" payment is typically paid monthly.

Results show that formal apprentices pay significantly lower fees to firms compared to traditional apprentices in treatment and control firms. This is consistent with the program 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 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. The decrease in overall payment by firms is consistent with dissatisfaction with labor earnings among apprentices, as reported above.

The table also provides some information on the indirect costs incurred by firms to train apprentices. These costs are key to understand firms' training decision (Acemoglu and Pischke (1999)). As shown in the conceptual framework in Appendix A2 and equation 15, the difference in the net value of work can be partly explained by the cost of training provided by firms. 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. The lower panel of Table A10 presents the results. In the control group, youth 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. These figures suggest that youth learn mostly by working alongside master craftsmen, seemingly inducing limited investments in time spent solely teaching apprentices from master craftsmen. There are also few differences between the various types of apprentices.

8 Conclusion

Evaluations of employment programs usually focus on direct impacts on participants, but these programs can have a range of indirect effects that are rarely taken into account. We designed a double-sided randomized control trial of a subsidized apprenticeship program with the specific objective to measure windfall effects among youths and substitution effects in firms. Consistent with a simple conceptual framework, we can then estimate the net number of new positions created by the intervention.

Results show that the apprenticeship program leads to an increase in youth participating in formal apprenticeship by 71.2 percentage points. This increase includes a significant windfall effect: 26% of youth placed in formal apprenticeships actually substituted out of traditional apprenticeships. On the side of firms, the program leads to an increase in the entry of formal apprentices. Substitution effects are also observed, however, as firms hire 0.23 less traditional apprentice per formal apprentice placed.

A simple conceptual framework shows how windfall and substitution effects combine. It also shows how the net number of apprenticeship positions created by the program can be bounded. Overall, the net number of apprenticeship positions created by the program is between 74 and 77 percent of the number of individuals placed. The framework also shows how the magnitude of adjustments in labor market tightness can be assessed ex post, and results confirm they are very small.

We interpret substitution effects as being moderate in magnitude, and clearly far from full substitution. This shows that the intervention expands access to apprenticeships and increases the net number of positions in firms. As such, the results do not support concerns that supply-side employment programs are purely redistributive, leading to no overall effects on the number of jobs in the economy. The results also contrast with findings from De Mel et al. (2016), who conclude based on a wage subsidy experiment in Sri Lanka that micro-enterprises do not face large labor market frictions.

The results point to failures of the traditional apprenticeship system that the subsidized apprenticeship program helps address. While many youths enter traditional apprenticeships on their own, there is still an underinvestment in training and scope to expand access. Participating in apprenticeship has large opportunity costs in terms of forgone earnings in wage or self-employment. Youth reorganize their activities to enter formal apprenticeships,

and the net average impact on youth earnings is zero in the short-term. By providing wage subsidies, the program increases the flow of young people who are able to afford these opportunity costs. At the same time, this leaves vacancies unfilled at the firm level, meaning there is room to increase participation in apprenticeship without inducing large substitution effects.

A natural question is why firms do not increase the compensation they offer to apprentices. This topic would deserve additional research, especially given the unusual nature of the compensation provided to apprentices, such as its various components and the way it varies over time,... However, findings are consistent with the mechanisms highlighted by Hardy and McCasland (2015), who show that entry fees can serve as a self-selection device. A low initial remuneration might also help select more motivated youths. Offering the subsidy eliminates this selection mechanism, and potentially implies that the program leads to an inflow of formal apprentices who are less motivated than traditional apprentices. We find consistent evidence that formal apprentices are less assiduous and less satisfied than traditional apprentices.

The increase in the number of apprentices entering firms is associated with a strong increase in the net value of work provided by apprentices. Consistent with the framework by Acemoglu and Pischke (1999), this suggests that firms receive compensation for providing training. As such, an important contribution of the paper is to document how formal apprenticeships have indirect benefits of substantial economic magnitude for firms that host apprentices. In fact, this indirect effect may be sufficient to make the program cost-effective in the short-term, even as direct impacts on youth earnings are not significant.

Ultimately, the finding that a "supply-side" apprenticeship program has impacts on firms on the "demand-side" of the labor market is important. It shows that indirect effects need to be quantified to provide a robust assessment of program performance. It also shows that human capital interventions targeting youths can have broader benefits for firms in the economy.

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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 youth starting apprenticeship in study localities	6670
(4) Registered youth assigned to treatment	910
(5) Registered youth assigned to treatment effectively starting apprenticeship	662
(6) Experiment size ratio	9.9%

See Table Appendix A1 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: Human Capital Investments

	Apprentice			TVET	Any	School	
	Formal	Traditional	Total		Training		None
	e_1	e_2	e_3				
	Started since randomization						
Treated	0.712***	-0.185***	0.528***	-0.057***	0.471***	-0.057***	-0.363***
	(0.017)	(0.016)	(0.021)	(0.010)	(0.021)	(0.019)	(0.021)
Mean	0.038	0.225	0.263	0.072	0.335	0.205	0.514

	Started apprenticeship and dropped out				Apprentice at follow-up		
	Formal	Traditional	Total		Formal	Traditional	Total
	d_1	d_2	d_3		c_1	c_2	c_3
Treated	0.222***	-0.060***	0.163***	Treated	0.490***	-0.125***	0.365***
	(0.016)	(0.016)	(0.023)		(0.018)	(0.014)	(0.022)
Mean	0.020	0.064	0.084	Mean	0.018	0.161	0.179

Source: Youth follow-up survey (1661 observations)

Notes: Estimation of equation 11 (White-Huber robust standard errors in parenthesis).

Upper panel (e) uses information from the human capital module of the follow-up survey, covering the duration of the experiment. (See footnote 28 and Appendix A3 for definition of variables). Column "None" means neither school nor any training.

The right part of the lower panel (c) uses information from the employment module of the follow-up survey. It measures occupation in apprenticeship at that time.

The left part of the lower panel measures impacts on dropouts (d). (For each category of youth : (e)=(d)+(c)).

Table 3: Inflow of apprentices into firms

	Formal	Traditional	Total
Inflow since randomization (e)			
Treated	1.398***	-0.318*	1.080***
	(0.096)	(0.178)	(0.208)
Mean	0.188	1.942	2.130
Exits since randomization (x)			
Treated	0.611***	-0.144**	0.467***
	(0.071)	(0.068)	(0.093)
Mean	0.130	0.430	0.561
In the firm at follow-up (s)			
Treated	0.787***	-0.174	0.613***
	(0.065)	(0.149)	(0.172)
Mean	0.058	1.512	1.570

Source: Firm follow-up survey (674 observations).

Notes: Estimation of equation 10 (White-Huber robust standard errors in parenthesis). The upper panel gives the total number of new apprentices since randomization (e). The intermediate panel gives the number of apprentices who left the firm since randomization (x), and the lower panel the number of apprentices still in the firm at the time of the follow-up survey (s=e-x).

Table 4: Overall impact on number of apprentices

	Formal	Total	Per formal apprentice		
On youth side					
Treated youth	0.712***	0.528***	0.741***	ω^m	0.259***
	(0.016)	(0.021)	(0.022)		(0.022)
Mean	0.038	0.263			
On firm side					
Treated firm	1.398***	1.080***	0.773***	ψ	0.227
	(0.096)	(0.208)	(0.128)		(0.128)
Mean	0.188	2.130			

Sources: Firm and youth follow-up survey (respectively 674 and 1661 observations).

Notes: The first two columns present ITT estimates of equations 11 (upper panel) and 10 (lower panel). The third column presents IV estimates of equations 12 (upper panel) and 13. The last column presents estimates for parameters ω^m 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 5: Youth Activities, Hours and Earnings

Activities

	Apprentice	Wage empl.	Self-empl.	Other activities	Total # activities	At least one
Treated	0.365***	-0.135***	-0.129***	-0.014	0.053*	0.034***
	(0.022)	(0.022)	(0.024)	(0.011)	(0.031)	(0.013)
Mean	0.179	0.356	0.471	0.056	1.191	0.910

Hours

	As an apprentice	As wage empl.	As self-empl.	In other activities	Total
Treated	18.200***	-6.462***	-7.692***	-0.418	3.687**
	(1.170)	(1.235)	(1.302)	(0.401)	(1.492)
Mean	7.558	14.954	17.637	1.748	41.880

Earnings

	Apprentice	Wage empl.	Self-empl.	In other activities	Total Labor	Non-labor[a]	Total
Treated	3,238***	-6,414***	-6,381***	-167.6	-10,494***	10,213***	-1,408
	(749.3)	(1,407)	(2,157)	(221.7)	(2,654)	(870.3)	(3,295)
Mean	4746	15398	19089	799.9	41776	7540	51484

Average Hourly Earnings

						Adjusted[b]	
	Apprentice	Wage empl.	Self-empl.	In other activities	Total Labor	Apprentices	Total
Control	628	1030	1082	458	998		
Treated	310	1058	1278	475	687	706	1099

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

Notes: The first three panels present ITT estimates of equation 11 for outcome variables related to occupation, hours worked and earnings. (See footnote 28 and Appendix A3 for definitions of variables - White-Huber robust standard errors in parenthesis).

The lower panel presents estimates of hourly earnings across different occupations, obtained as the ratio of average earnings to the average number of hours worked.

a - Includes the program stipend

b - Adding non-labor earnings (including program stipend) to apprenticeship earnings and total labor earnings, respectively.

Table 6: Apprentices' participation in firms' activities

	# days of work	# hours of work	Value of work	Wage Bill	Net value of work
Treated	6.945**	55.91**	25,543***	4,162	21,380***
	(3.253)	(26.71)	(7,023)	(3,094)	(5,932)
Mean	30.38	252.6	41080	21854	19226

Source: Follow-up firm survey (674 observations).
ITT estimates from equation 10 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.

Table 7: Impacts on youth earnings and net value of apprentices' work in firms

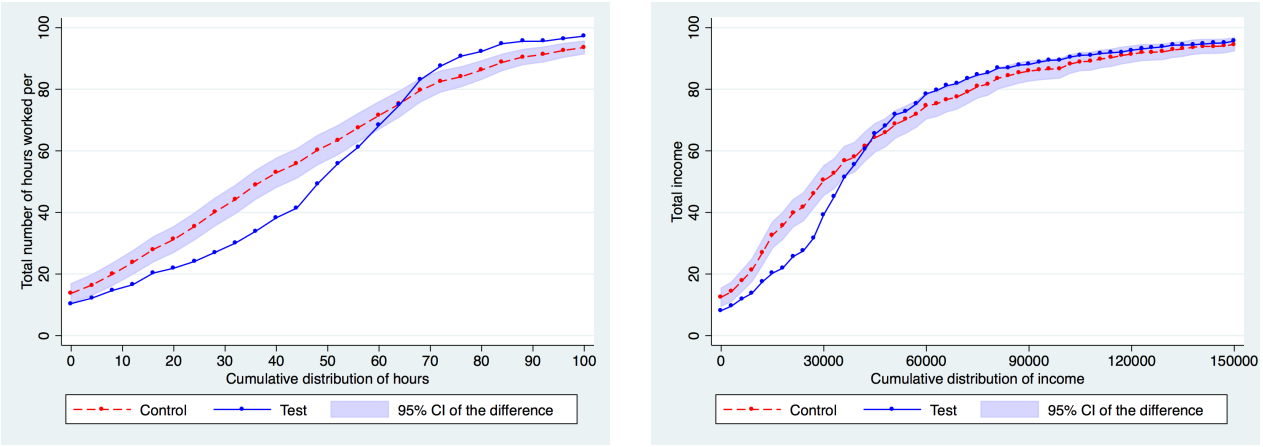
Reduced form			
	Youth Earnings		Net Value of work
Treated youth	-1,408	Treated firm	21,380***
	(3,295)		(5,932)
	In Formal Apprenticeship		# of Formal Apprentices
Treated youth	0.712***	Treated firm	0.787***
	(0.016)		(0.065)
Control Mean	0.038		0.058
Second stage			
b_I	-1,977	b_S	27,165***
	(4,473)		(7,315)
Control Mean	51,484		19,226

Source: Firm and youth follow-up survey (respectively 674 and 1661 observations).
Notes: The first column presents ITT estimates of equation 11 on youth total earnings (upper panel) and youth participation in formal apprenticeship (intermediate panel), and then IV estimates of equation 12 using assignment to treatment as an instrument (lower panel).
The second column presents ITT estimates of equation 10 for net value of apprentices' work in firms (upper panel) and the number of formal apprentices in the firm at the moment of the follow-up survey (intermediate panel). IV estimates of equation 13 using treatment assignment as an instrument are presented in the lower panel.
White-Huber robust standard errors in parenthesis.

Figure 3: Distributions of potential outcomes and unconditional quantile treatment effects on compliers for hours worked and income

Weekly hours	Monthly income

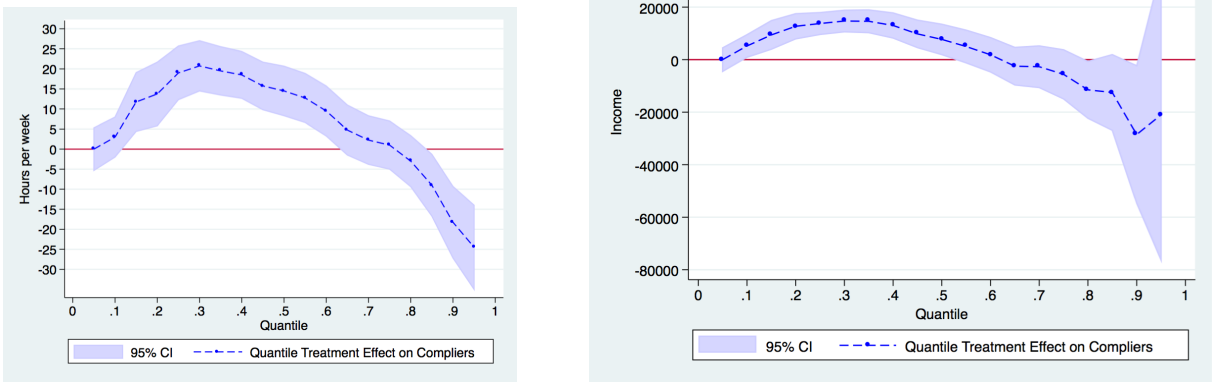
Distribution of potential outcomes



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

$p = 0/10000$	$p = 53/10000$

Unconditional instrumental variable quantile treatment effects



Source: Youth follow-up survey (1661 observations)

Notes: The figures in the upper panel 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 11, with variables defined as $1(y < t)$ for t varying over the support of y). The intermediate panel presents the result of the Mann-Whitney rank test implemented using 10,000 permutations within randomization strata. The figures in the lower panel presents the results of the estimation of unconditional instrumental variables quantile treamtent effect Frölich and Melly (2013). The doted blue line provides, for a given q, the estimated parameter and the shaded area its confidence interval.

a - The p-value is computed as the ratio of the number of times the statistics from a permuted assignment variable was found larger than the statistic obtained with the true assignment variable to the total number of permutations.

A1 Experimental design and implementation

The program was implemented in 7 urban areas in the interior of the country.[54] 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 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.[55] 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.[56] 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 youth motivation for doing an apprenticeship as well as their choice of trade. A baseline survey was implemented among all youth who successfully passed the interview.[57] This registration process led to the same number of youths eligible and motivated than open positions, in each locality and in each

[54]These included Man (35% of youth 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 youth to include in the program was set for each locality based on the estimated number of available apprenticeship positions and other considerations.

[55]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. A good example is the garage sector, which includes apprenticeship positions in several trades: coach builder, car mechanic, car electrician, or car painter.

[56]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%).

[57]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.

trade.

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 55). 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 youth to treatment by trade. We assigned the exact same number of youth 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 A2 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.[58]

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 youth residence. Once assigned to firms, youth passed a medical visit and were invited to sign a contract and start their apprenticeship.

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

Across the 7 localities covered by the study, 731 firms offered apprenticeship positions and 1,832 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. Most firms offered several positions, and on average treatment firms were assigned 2.52 apprentices.

Even though it is not required to estimate the parameters of interest, 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 that 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 youth only represent 4.1% of the population of youths entering apprenticeships. In contrast, in small localities like Mankono or Daoukro, the share is 31%, respectively 35.2%. On average, treated youth represent less than 10% of the population of youths entering apprenticeship in the study localities.

A2 Detailed Conceptual framework

Supply of apprentices. We model youth decisions to enter apprenticeship as a sequential process. Youth 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).[59] We assume that there is no cost or effort to search for an apprenticeship position. Youth 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 youth, and denote F the cumulative distribution function of w_0. The share of youth searching for an apprenticeship position is thus $F(\gamma_a)$ and the number of youth searching for an apprenticeship position is $N_y F(\gamma_a)$. When searching, youth 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 youth searching for an apprenticeship position and V the number of apprenticeship vacancies.[60] The supply side relation between the number of apprentices and the tightness writes:

$$S^{trad}(\theta) = \lambda(\theta) F(\gamma_a) N_y \tag{A2}$$

Demand for apprentices. Next, we model firms' demand for apprentices. We assume there are N_{firm} firms. The production function is a technology with decreasing returns: $f(n_a, n_1)$, where n_a represents traditional apprentices and n_1 formal apprentices. We consider that the two types of apprentices are not perfect substitutes. The total cost of an apprentice has several components. First, firms pay traditional apprentices a wage w_a. Second, we account for frictions in the search for apprentices, which induce an additional cost: $c(\theta)$.[61] Following Acemoglu and Pischke (1999), we also account for the fact that there is an amount

[59] Π also includes fees to be paid at the end of the apprenticeship, with a high discount rate in case there are financial constraints (see Acemoglu and Pischke (1999)).

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

[61] $c(\theta) = c/q(\theta)$, where c is the cost of a vacancy (which also captures the probability of dropout) and $q(\theta)$ is derived from the matching function $q(\theta) = M(Y_a, V)/V = \lambda(\theta)/\theta$. We assume that youth who drop out do not re-enter the market for apprentices.

Δ that firms keep as compensation for the training provided to apprentices. In this setting, following Michaillat (2012), the demand for traditional apprentices for a given number of formal apprentices can be derived from the first-order condition: $f'_{n_a}(n_a, n_1) - \Delta = w_a + c(\theta)$. The relationship can be rewritten as $n_a^d = d(\theta, n_1)$, which we approximate as $n_a^d = d(\theta, 0) - \psi n_1$. ψ is the first key parameter of the experiment. It depends on the production technology and is expected to be positive if returns to scale are low and the two types of apprentices are largely substitute.[62] For a total number of formal apprentices N_{form} and a given tightness of the market for traditional apprentices, the aggregate demand for traditional apprentices $D^{trad}(\theta, N_{form})$ becomes:

$$D^{trad}(\theta, N_{form}) = N_{firm} d(\theta, 0) - \psi N_{form} \tag{A3}$$

which is decreasing in θ as $c(\theta)$ is a component of the total cost which is increasing in θ.

Absent any intervention, the demand function is $D(\theta, 0)$. Together with equation A2, it determines an equilibrium in which the tightness is θ_0, and the total number of traditional apprentices is $N_0 = S^{trad}(\theta_0) = D(\theta_0, 0)$ (see Figure 2).[63] The adjustment variable is the tightness and not the wage. This is common in equilibrium search models in which wages are determined under a variety of mechanisms. In our case, wages are set in an informal way. They are paid in multiple components and there is no contractual commitment on any of these components. Moreover, one important objective of the model is to help conceptualize potential displacement effects, i.e. the possibility that program youth crowd-out traditional apprentices. The tightness of the apprenticeship market is thus the most relevant parameter.

Modelling the intervention for youth. The intervention consists in offering a share σ_a of youth a subsidy S, providing them a formal training and matching them with a firm. We assume that the wage paid by firms $\widetilde{w}_a$ and the apprenticeship entry fee requested by firms $\widetilde{\Phi}$ can be different.[64] The perceived long-term gains from formal apprenticeships are also expected to be different: $\widetilde{\Pi} \geq \Pi$. In this context, youth decide to enter apprenticeship

[62] Assuming $f(x_1, x_2) = (x_1^r + x_2^r)^{\alpha/r}$, and the cost of factor 1 is c_1, the demand for x_1 conditional on x_2 satisfies $dlog(x_1) = \mu dlog(c_1) + \eta dlog(x_2)$ and it can be shown $\eta \propto \alpha - (\sigma - 1)/\sigma$ with σ the elasticity of substitution associated to r. Thus, if σ is large and α is small, η is negative

[63] The number of traditional apprentices per firm is $n_0^{trad} = d(\theta_0, 0)$ and thus $N_0 = N_{firm} n_0^{trad}$.

[64] In practice, $\widetilde{w}_a$ is likely to be smaller than w_a as the subsidy is supposed to cover transportation cost, meal, and clothing, which are important components of the compensation received by traditional apprentices. Similarly, $\widetilde{\Phi} \leq \Phi$ as firms were requested not to charge apprenticeship entry fees.

if:

$$w_0 \leq \widetilde{\gamma}_a + S = \widetilde{w}_a + S - \widetilde{\Phi} + \widetilde{\Pi} \tag{A4}$$

As a result, there are A youth applying to the formal apprenticeship program:

$$A = N_y \sigma_a F(\widetilde{\gamma}_a + S) \tag{A5}$$

and the number of youth who enter a formal apprenticeship is

$$N_{form} = \tau_1 A \tag{A6}$$

where τ_1 is the matching rate of formal apprentices.

The windfall effect corresponds to the fact that $\sigma_a N_0$ of these youth would have found a traditional apprenticeship position absent the program:

$$Wind = \sigma_a N_0 = \sigma_a S^{trad}(\theta_0) \tag{A7}$$

The direct consequence of offering a share σ_a of youth to participate in the program is that the supply of traditional apprentices is reduced by a factor $1 - \sigma_a$. Thus the supply of traditional apprentices left once the program has been implemented is:

$$S^{trad}_{left}(\theta) = (1 - \sigma_a) S^{trad}(\theta) \tag{A8}$$

We can expand the demand equations around θ_0 to get

$$S^{trad}(\theta) \approx N_0 + A_s(\theta - \theta_0) \tag{A9}$$

with $A_s = dS^{trad\prime}(\theta_0) = N_y F(\gamma_a)\lambda'(\theta_0)$. We can then approximate

$$S^{trad}_{left}(\theta) \approx N_0 + (1 - \sigma_a) A_s(\theta - \theta_0) - \sigma_a N_0 = N_0 + (1 - \sigma_a) A_s(\theta - \theta_0) - Wind \tag{A10}$$

For the new equilibrium tightness ($\theta = \theta_1$), the expression can be rewritten as:

$$S^{trad}_{left}(\theta_1) = (1-\sigma_a)S^{trad}(\theta_1) \approx S^{trad}(\theta_0) + A_s(\theta_1 - \theta_0) - \sigma_a S^{trad}(\theta_1) \quad \text{(A11)}$$

$$= N_0 + A_s(\theta_1 - \theta_0) - Wind^m \quad \text{(A12)}$$

$Wind^m$ is the number of program youth who would have taken an apprenticeship position absent the program, at post-program market tightness θ_1. The experiment is able to measure $Wind^m$, which differs from the true windfall effect $Wind = \sigma_a S^{trad}(\theta_0)$. As we show below, this is sufficient to bound the net effect of the program on the number of positions created.

Modelling the intervention for firms. A share σ_f of firms are offered formal apprentices. The number of formal positions they can fill ($n_1 = n_f$) is rationed. We assume formal and traditional apprentices are imperfect substitutes. Formal apprentices are paid $\widetilde{w}_a$ and can be found without searching, for a matching cost c_1. Thus the cost of a formal apprentice is $\widetilde{w}_a + \Delta + c_1$. We assume that this cost and the productivity of formal apprentices are such that the optimal strategy for firms is to open positions and hire as many formal apprentices as possible and then hire additional traditional apprentices according to the partial demand previously described: $d(\theta, n_f)$. This leads to an aggregate demand $D^{trad}(\theta, N_{form})$. If we expand the demand functions around the apprenticeship market tightness θ_0, we simply get $d(\theta, n_f) \approx n_{a,0} - \alpha_d(\theta - \theta_0) - \psi n_f$, with $\alpha_d > 0$, from which we get

$$D^{trad}(\theta, N_{form}) \approx N_0 - A_d(\theta - \theta_0) - Sub \quad \text{(A13)}$$

with $A_d = N_{firm}\alpha_d$ and $Sub = \psi N_{form}$.

New equlibrium. Equations A8 and A13 define a new equilibrium and new level of traditional apprentices (see Figure 2):

$$\theta_1 - \theta_0 = \frac{Wind - Sub}{(1-\sigma_a)A_s + A_d} \quad \text{(A14)}$$

$$N_1^{trad} - N_0 = -\frac{(1-\sigma_a)A_s Sub + A_d Wind}{(1-\sigma_a)A_s + A_d} \quad \text{(A15)}$$

Importantly, these equations give the impact of the program on the number of traditional apprentices and the market tightness as a function of the windfall effect $Wind$. However, as

already mentioned, we cannot identify the true windfall effect due to the changing market conditions. However, it is straightforward to rewrite the adjustments $N_1 - N_0$ and $\theta_1 - \theta_0$ as a function of the measured Windfall effect $Wind^m$:

$$\theta_1 - \theta_0 = \frac{Wind^m - Sub}{A_s + A_d} \tag{A16}$$

$$N_1^{trad} - N_0 = -\frac{A_s Sub + A_d Wind^m}{A_s + A_d} \tag{A17}$$

These equations directly provide bounds for the net number of apprenticeship positions created by the program:

$$N_1 - N_0 = N_{form} - \frac{(1-\sigma_a)A_s Sub + A_d Wind}{(1-\sigma_a)A_s + A_d} = N_{form} - \frac{A_s Sub + A_d Wind^m}{A_s + A_d} \tag{A18}$$

Since the two parameters $Wind$ and Sub are positive, the reductions from the supply side and from the demand side combine positively. They induce an overall reduction in the number of traditional apprentices, which is an average of the windfall effect on the supply side and the substitution effect on the demand side.

Overall, the sign of the change in tightness is unknown. The reduction in the supply of apprentices should increase the tightness. At the same time, the reduction in the demand for traditional apprentices due to the substitution effect mitigates this increase.

Order of magnitude. The important parameters in our model are the following: $Sub = \psi N_{form} = \sigma_f \psi N_{firm} n_f$, $Wind = \sigma_a N_0$, $A_s = N_y F(\gamma_a)\lambda'(\theta_0)$ and $A_d = N_{firm}\alpha_d$. We define M to be the size of the market. Quantities like N_y and N_{firm} are of the order of magnitude of the size of market, which we denote as $O(M)$. We also define σM to be the size of the experiment and denote quantities of the same order of magnitude as $O(\sigma M)$. Last, we define the relative size of the experiment σ and denote its order of magnitude as $O(\sigma)$. Quantities such as σ_a and σ_f are $O(\sigma)$. We clearly see that Sub, N_{form} are $O(\sigma M)$ and that A_s and A_d are $O(M)$. As a result, we also see that $N_1 - N_0$ is $O(\sigma M)$ and $\theta_1 - \theta_0$ is $O(\sigma)$.

As discussed above, there is a slight difference between the $True$ and $Measured$ impacts on entry into apprenticeship. We measure the share of youth entering traditional apprenticeship in the control group, but this corresponds to a state of the market with a tightness θ_1. The true impact would be based on the share of youth entering traditional

apprenticeship absent the program, at initial tightness θ_0. We define the *Indirect* effect as: $Measured = True - Indirect$ and thus $Indirect = \sigma_a(S^{trad}(\theta_1) - S^{trad}(\theta_0))/A$. As a result from the previous considerations about order of magnitudes of adjustments, we have $Indirect = O(\sigma)$.

Investigating the difference in the value of work. We can also consider the difference in profits between treatment and control firms. We define profits narrowly as the value of the work of apprentices net of the compensation given to them. Formally, we can write this as

$$\pi(T) = f(n_a(T), n_f(T)) - w_a n_a(T) - \widetilde{w}_a n_f(T) \tag{A19}$$

where T is the treatment assignment variable. Thus we have:

$$\begin{aligned} \pi(1) - \pi(0) &= f(n_a(1), n_f) - f(n_a(0), 0) - w_a(n_a(1) - n_a(0)) - \widetilde{w}_a n_f \\ &= (f'_{n_a} - w_a)(n_a(1) - n_a(0)) + (f'_{n_f} - \widetilde{w}_a) n_f \end{aligned}$$

Given the first order condition $f'_{n_a} - w_a - c(\theta_1) - \Delta$ and $E(n_a(1) - n_a(0)) = -\psi E(n_f)$, we get

$$\begin{aligned} E(\pi(1) - \pi(0)) &= -\psi(c(\theta_1) + \Delta) + (f'_{n_f} - \widetilde{w}_a) E(n_f) \\ &= [(c(\theta_1) + \Delta)(1 - \psi) + (f'_{n_1} - \widetilde{w}_a - c_1 - \Delta) + c_1 - c(\theta_1)] E(n_f) \end{aligned}$$

Additional analysis of the decision to enter apprenticeship. The decision to enter apprenticeship when offered or not to participate in the program is described in equation A1 and A4. We can adjust this slightly to account explicitly for financial constraints. If we assume that there is a minimum amount of earnings $\overline{w}$ needed and that it is above $w_a - \Phi$ and $\widetilde{w}_a + S - \widetilde{\Phi}$ youth have to borrow to afford a minimum standard of leaving when trained as an apprentice. The equations that will determine whether youth enter apprenticeship become

$$w_0 + r(\overline{w} - (w_a - \Phi)) \leq w_a - \Phi + \Pi \tag{A20}$$

and

$$w_0 + r(\overline{w} - (\widetilde{w}_a - \widetilde{\Phi} + S)) \leq \widetilde{w}_a - \widetilde{\Phi} + S + \widetilde{\Pi} \tag{A21}$$

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

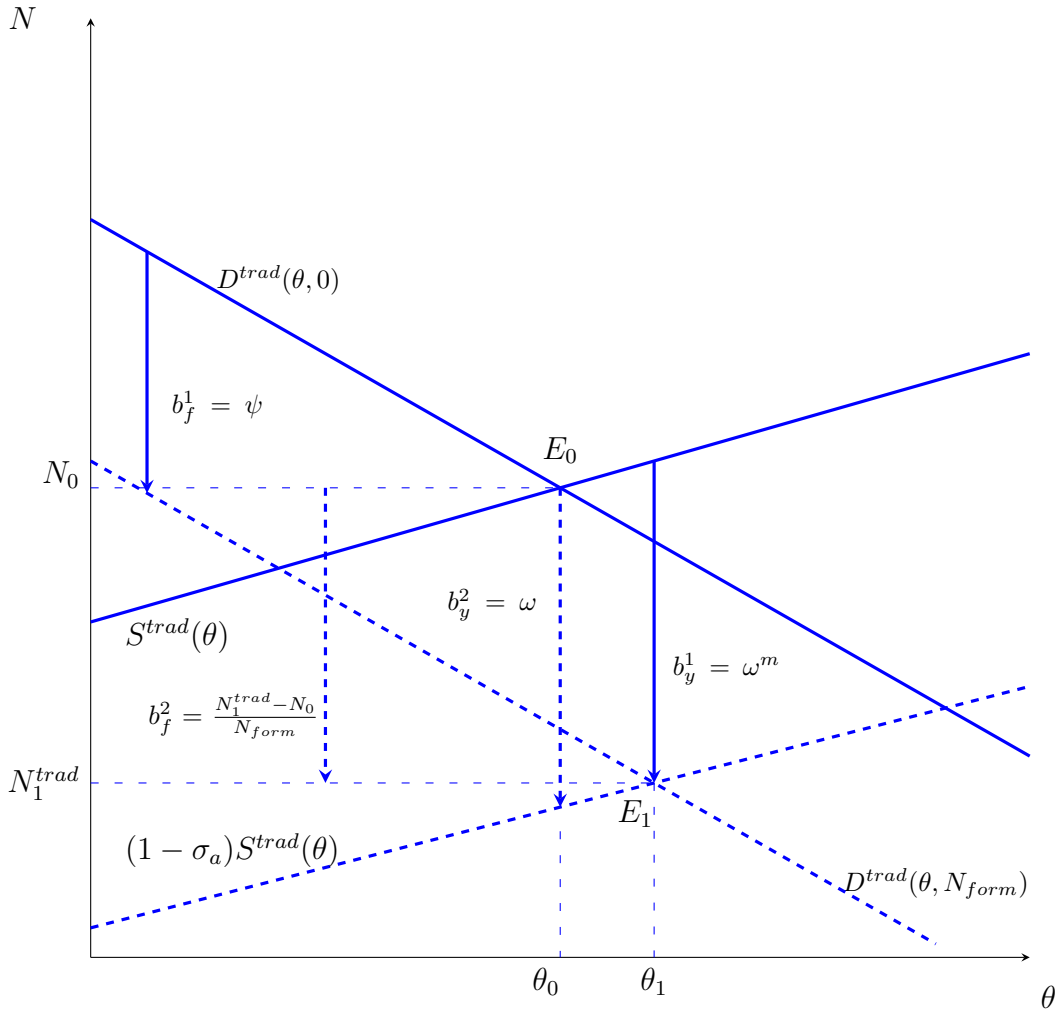
Discussion of alternative designs. Our design randomly assigns youth and firms to control and treatment groups within a same micro market. By using instrumental variable regressions on traditional apprentices outcome variables, we can identify $b_f^1 = -\psi$ and $b_y^1 = -\omega^m$, from which we can express $N_1^{trad} - N_0 = (A_s b_f^1 + A_d b_y^1)/(A_d + A_s)$ and $\theta_1 - \theta_0 = (b_f^1 - b_y^1)N_{form}/(A_s + A_d)$. It is worth mentioning alternative designs that could have been used. The same instrumental variable regression would have identified a different set of parameters. Figure A1 summarizes the two sets of parameters.

a. The first alternative design would have randomly assigned micro markets to treatment or control. Instrumental variable regressions based on this design would have measured directly $b_f^2 = (N_1^{trad} - N_0)/N_{form}$ based on firm-level regressions and $b_y^2 = -\omega$ based on youth-level regression. From these estimates, the substitution parameter can be written $\psi = -b_f^2 + (b_y^2 - b_f^2)(1 - \sigma_a)A_s/A_d$ and $\theta_1 - \theta_0 = (b_f^2 - b_y^2)N_{form}/A_d$. This design would have directly measured the impact on the net number of apprenticeship positions. However, it would have produced estimates difficult to interpret outside the experiment. In particular, the parameter ψ would not have been identified. Moreover, potential imperfect compliance among youth would complicate interpretation as non-compliers are in a market that is affected by the intervention. The implementation of such a design was not possible for practical reasons related to program implementation. Moreover, there are 111 micro-markets in the context of the study, which might not provide enough randomization units. There is also some heterogeneity between markets, for instance related to the size of the localities.

b. A second alternative design would have combined the implemented design and the first alternative design. It would have randomly assigned micro markets to treatment and control groups, and then within the treatment micro-markets it would have assigned firms to treatment and control groups. This design would have allowed to measure the

full set of parameters b_y^1, b_f^1, b_y^2 and b_f^2. This design would thus allow to measure both ψ and ω^m, but also how they combine to lead to the observed reduction in traditional apprenticeship positions $N_1^{trad} - N_0$ and the relative slopes A_s/A_d. This design would have been complicated to implement in the context of the study, and the number of micro-market (111) would not have been sufficient.

Figure A1: Equilibrium employment of traditional apprentices and tightness



The figure shows the parameters identified by instrumental variable regressions for traditional apprentices outcome variables at the youth level (b_y^1) and firm level (b_f^1) with the design that we implemented (plain arrows). It also shows the parameters that would be identified by instrumental variable regressions for traditional apprentices outcome variables at the youth level (b_y^2) and firm level (b_f^2) with an alternative design that would randomly assign micro markets to treatment and control (dashed arrows).

A3 Definition of apprenticeship and training variables

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.

Not all youth assigned to treatment started an apprenticeship. 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 follow-up survey, and on average 12 months into the program. The process evaluation survey asked youth several questions to understand take-up and the timing of potential drop-outs. Youth 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.[65] 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 large, indicating that around 40 percent of youths from the treatment and the control groups dropped out in the first year of the program (see Table 2). These figures remain consistent with most drop-out taking place early in the program.

The administrative dataset provides information consistent with the process evaluation 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 youth 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 apprentice-

[65] 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 youth report having signed a contract but did not start the apprenticeship.

ships) dropped-out before the end of their contract.

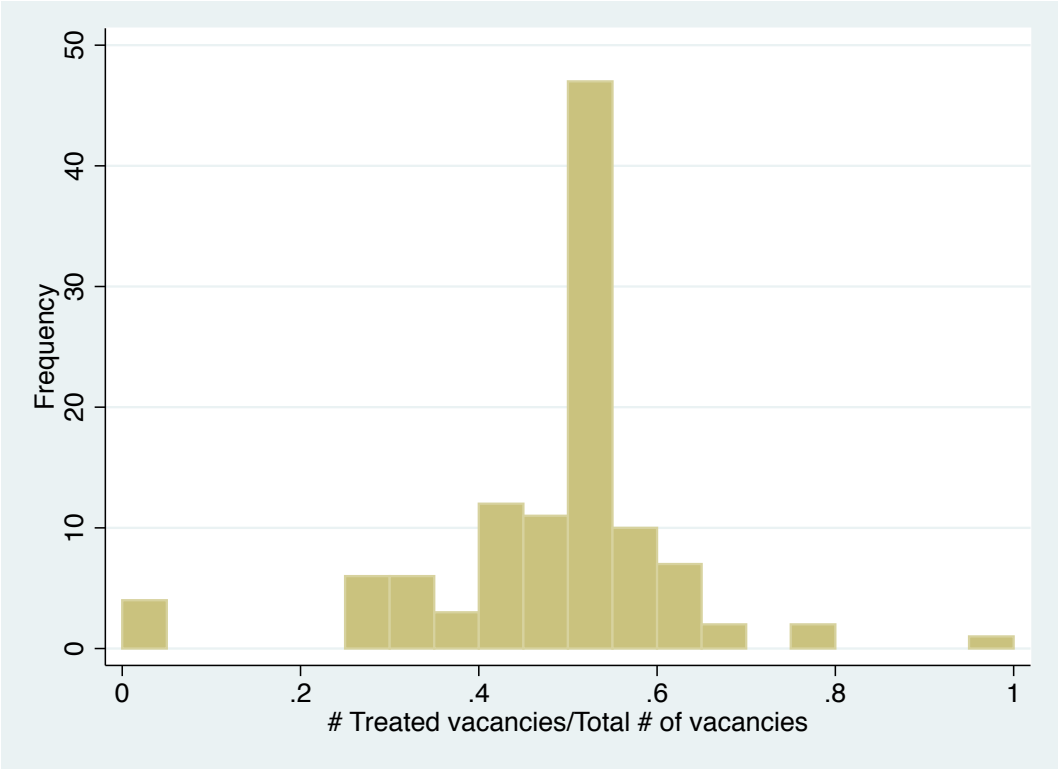
We now turn to the measurement of participation in apprenticeship and other human capital investments. The follow-up survey asks youth whether they were involved in PEJEDEC or AGEFOP apprenticeship programs. Youth sometimes confused the two. The survey also asked youth whether they had been involved in apprenticeship or TVET. The two answers are mutually exclusive. Youth involved in apprenticeship programs with dual practical and theoretical training also at times confused whether it was an apprenticeship or TVET program. We define a "formal apprentice" as a youth who reported being involved in the PEJEDEC or AGEFOP program, and reported being in either apprenticeship or TVET.

Table A5 presents some results supporting the choice of the definition of a formal apprentice. In the first column (Take-up), we consider the answer to the question about participation in a public program such as PEJEDEC (in the first panel) or 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 some youths confused the AGEFOP and PEJEDEC programs: when we define take-up as participation in the AGEFOP program, the treatment effect on the take-up variable is large. We thus consider both programs together (third panel). The second result is that many youths considered dual apprenticeships 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 youth answering they are enrolled in any of the governmental programs (AGEFOP or PEJEDEC), and reporting they participated in either TVET or apprenticeship training.

A4 Additional Tables and Figures

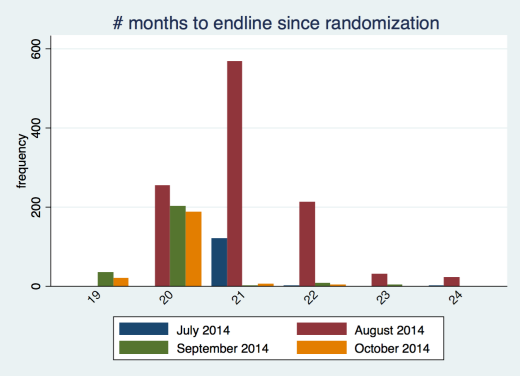
Figure A2: Ratio of treated positions to total number of positions, by micro market



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 youth to the total number of youth in a micro-market.

Figure A3: Timing of firm and youth follow-up surveys

Youth survey



Firm survey

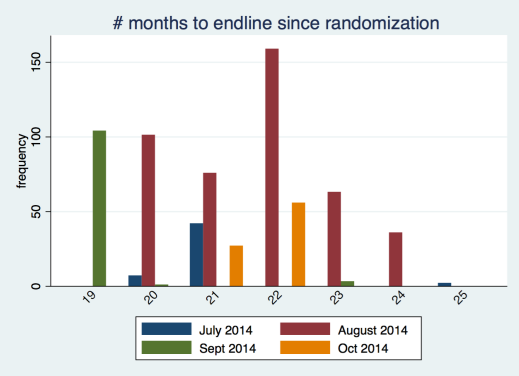
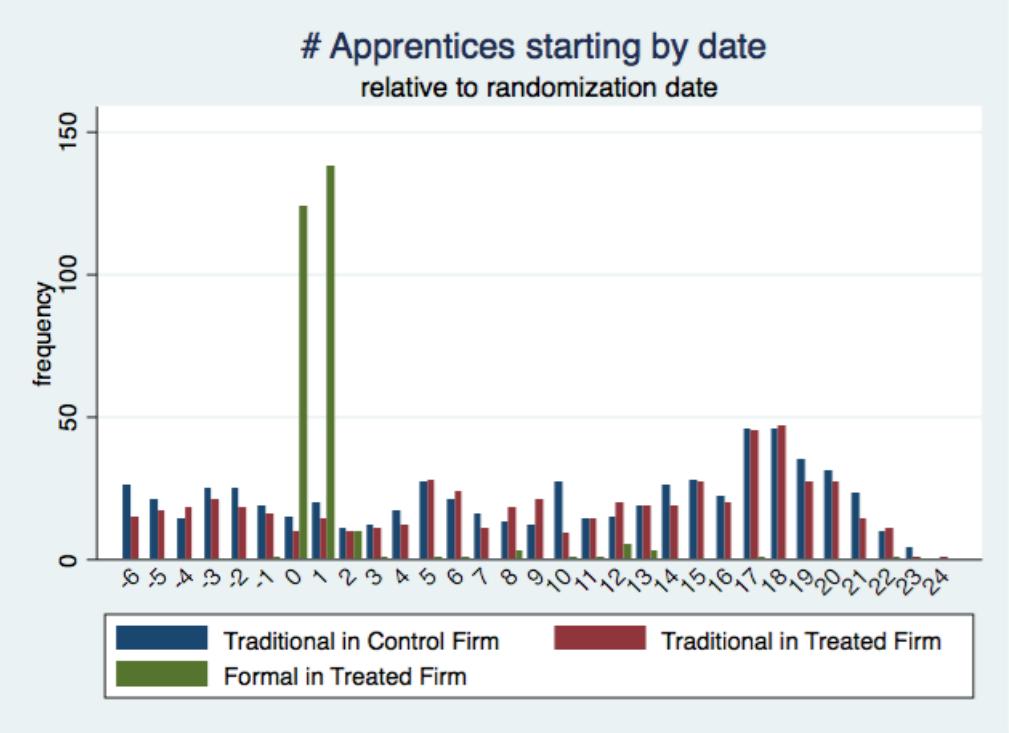


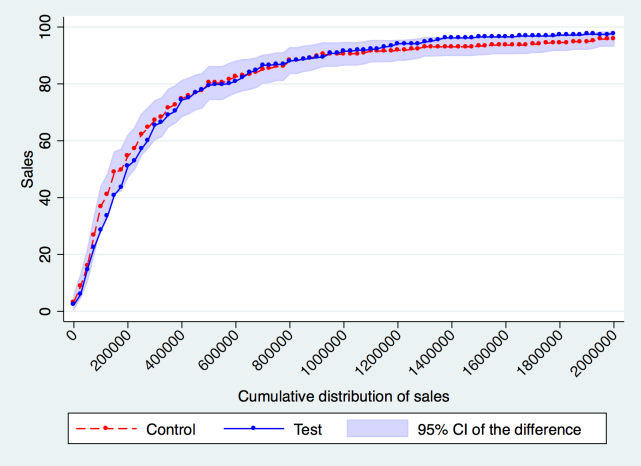
Figure A4: Entry of traditional and formal apprentices in treatment and control firms, by month



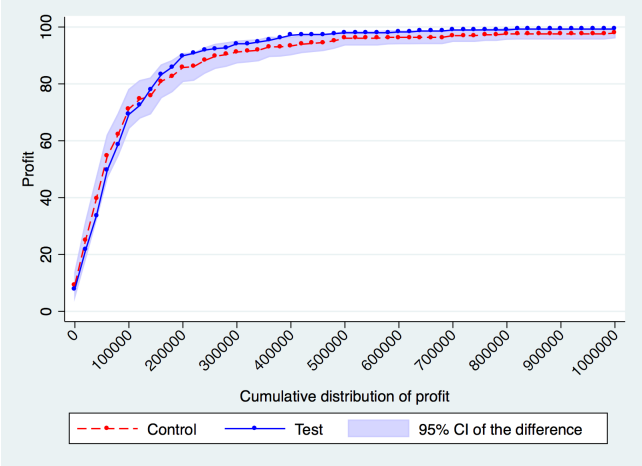
Source: Firm follow-up survey (674 observations)
Notes: Number of youth entering treated and control firms as formal or traditional apprentices, by month. (0 is the randomization date.)

Figure A5: Impact on Revenues and Profit

Revenues



Profit



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

p=2158/10000 p=4484/10000

Source: Firm follow-up survey (674 observations)

Notes: Estimation of equation 10, 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 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 youth aged 15-24 into apprenticeship. We first apply the national share of youth aged 15-24 in the population (0.2295) to total population (in column (4)). We then multiply the result by the share of youth 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 youth assigned to treatment in each locality. Column (7) gives the number of youth in the treatment group who actually started apprenticeship.

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

Table A2: Balance for Youth

	Baseline			Follow-up		
Variables	Cont	Coef	p-val	Cont	Coef	p-val
Demographics						
Male	0.87	-0.02	0.35	0.87	-0.02	0.26
Age	20.74	0.09	0.43	20.75	0.08	0.51
Married	0.02	0.01	0.47	0.02	0.00	0.60
No diploma	0.20	-0.00	0.81	0.20	-0.00	0.84
Primary education	0.63	0.00	0.88	0.64	0.00	0.98
Lower secondary education or above	0.17	0.00	0.96	0.16	0.00	0.86
Has received training	0.22	0.03	0.13	0.22	0.04	0.08
Skills						
Skill Index (All)	1.72	0.01	0.65	1.72	0.01	0.75
Learning Skill Index	0.79	0.00	0.90	0.79	-0.00	0.92
Behavioral Skill Index	0.93	0.01	0.43	0.93	0.01	0.36
Economic Activity						
Has activity	0.87	0.00	0.81	0.87	0.01	0.56
Nb of agricultural activities	0.20	-0.02	0.32	0.21	-0.03	0.22
Total nb of activities	1.36	-0.01	0.86	1.38	-0.02	0.75
Nb of non agricultural activities	1.17	0.01	0.69	1.17	0.01	0.72
Total income (KCFA)	70.53	4.57	0.51	70.86	5.21	0.47
Total income (KCFA) (hyperbolic sin)	3.25	-0.07	0.57	3.27	-0.07	0.61
Employment aspirations						
Searching for a job	0.44	0.01	0.66	0.44	0.01	0.75
Aspires to wage job	0.46	0.01	0.72	0.46	0.01	0.84
Aspires to self-employment	0.54	-0.01	0.69	0.54	-0.01	0.80
Nb of hhd members in wage jobs	0.70	-0.04	0.38	0.70	-0.04	0.44
Has relatives in wage jobs	0.50	0.02	0.57	0.51	0.02	0.52
Has friends in wage jobs	0.52	0.03	0.22	0.53	0.03	0.34
Nb of hhd members with IGA	1.78	-0.04	0.64	1.78	-0.02	0.82
Has relatives with IGA	0.71	-0.00	0.88	0.71	-0.01	0.84
Has friends with IGA	0.78	-0.07	0.00	0.78	-0.07	0.00
Exposure to crisis						
Parents were present when 15	0.76	-0.01	0.72	0.76	-0.01	0.74
Household subject to crisis	0.12	-0.01	0.62	0.13	-0.01	0.77
Family subject to crisis	0.19	-0.04	0.07	0.19	-0.04	0.05
Lost employment during crisis	0.03	0.00	0.88	0.02	0.00	0.77
Financial constraints						
Nb financial constraints	2.78	-0.02	0.89	2.83	-0.05	0.73
Saved during last 3 months	0.49	-0.01	0.77	0.49	-0.01	0.84
Has saving account	0.05	0.01	0.59	0.05	0.01	0.49
Forced to use savings to faced emergencies	0.85	-0.02	0.38	0.85	-0.02	0.36

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... table A2 continued

Variables	Cont	Coef	p-val	Cont	Coef	p-val
Has debt	0.31	0.01	0.76	0.31	0.01	0.68
Has problem paying back debt	0.16	0.00	0.87	0.16	0.00	0.82
Is credit constrained	0.52	-0.03	0.24	0.52	-0.03	0.37
				Respondent to survey		
1832 youth registered	0.76	-0.04	0.03	0.91	0.00	0.92

Sources: Youth baseline and follow-up surveys
Notes: Each row in the table considers a specific baseline characteristic and presents the result of the estimation of equation (11) on the whole sample for which the baseline is available (left panel - 1357 youths), or the sample with both baseline and follow-up survey respondent (right panel - 1299 youths). In each panel, the first column gives the number of observations used in the regression. 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 19.)

Table A3: Balance for Firms

Variables	Baseline			Follow-up		
	Cont	Coef	p-val	Cont	Coef	p-val
Nb of open apprenticeship positions	2.51	0.02	0.87	2.45	0.09	0.47
Firm Status						
No legal status	0.84	-0.00	0.95	0.86	-0.02	0.55
No accounting	0.68	-0.04	0.26	0.69	-0.04	0.26
No salary slip	0.97	0.01	0.66	0.98	0.00	0.89
Workforce						
Permanent workers	6.32	0.39	0.39	6.19	0.16	0.73
Autonomous workers	3.33	0.12	0.63	3.23	-0.03	0.88
Supervisors	2.37	0.12	0.38	2.32	0.14	0.33
Apprentices	3.38	0.13	0.67	3.38	0.04	0.89
Channels to recruit apprentices						
Spontaneous application	0.10	-0.03	0.20	0.10	-0.03	0.10
Parents asked	0.82	0.03	0.25	0.82	0.03	0.23
Referral	0.04	-0.02	0.18	0.03	-0.01	0.34
National agency	0.02	0.01	0.58	0.02	0.00	0.80
Other recruitment channel	0.03	0.01	0.55	0.02	0.01	0.31
Reasons to hire apprentices						

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... table A3 continued

Variables	Cont	Coef	p-val	Cont	Coef	p-val
To get workers	0.09	-0.01	0.71	0.08	0.00	0.87
To transmit knowledge	0.45	0.02	0.60	0.48	-0.01	0.89
To help youth	0.41	-0.01	0.70	0.40	-0.00	0.92
Because it pays	0.01	-0.00	0.67	0.01	0.00	0.69
Other reasons	0.04	0.01	0.71	0.04	0.00	0.86
First criterion to select apprentices						
Skills	0.03	0.01	0.55	0.03	0.01	0.57
Motivation	0.29	-0.02	0.56	0.29	-0.03	0.44
Respect	0.60	0.02	0.51	0.60	0.03	0.44
Tuition requested						
At start	0.51	0.01	0.75	0.53	0.01	0.75
Amount	37944	-920	0.75	38059	-570	0.85
During training	0.26	0.02	0.47	0.26	0.02	0.48
Apprenticeship dropouts between 2012-2014 and reasons for dropping out						
Total	2.00	0.35	0.09	1.96	0.37	0.09
Unable	0.07	0.05	0.09	0.06	0.05	0.09
Not interested	0.21	0.02	0.66	0.20	0.02	0.63
Financial reasons	0.13	-0.07	0.01	0.14	-0.08	0.01
No work perspec	0.00	0.00	0.61	0.00	-0.00	0.93
Found a job	0.08	0.00	0.99	0.09	-0.00	0.93
Disciplinary reason	0.17	-0.03	0.41	0.17	-0.03	0.44
Apprenticeship finishers between 2012-2014						
Number	1.21	0.02	0.92	1.20	-0.05	0.77
Hired in firm	0.24	0.03	0.69	0.20	0.02	0.72
Hired outside	0.25	-0.04	0.49	0.25	-0.05	0.39
Starting business	0.67	0.07	0.58	0.69	0.03	0.82
			Respondent to survey			
731 registered firms	0.95	0.00	0.86	0.91	0.01	0.48

Notes: Each row in the table considers a specific baseline characteristic and presents the result of the estimation of equation 10 on the whole sample for which the baseline is available (left panel - 694 firms), or the sample with both baseline and follow-up survey respondents (right panel - 643 firms). In each panel, the first column gives the number of observations used in the regression. Some variables (for example in the dropout section) are only defined conditionally on another variable in the table (e.g. among those who had at least one dropout). The second column gives the estimated coefficient and the third column the p-value. The first row contains the number of apprenticeship positions offered 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 19.)

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

	Take-up	Apprentice	TVET	Any training
PEJEDEC program				
Treated youth	0.643***	0.472***	0.135***	0.607***
	(0.017)	(0.017)	(0.012)	(0.017)
Control Mean	0.028	0.015	0.004	0.019
AGEFOP program				
Treated youth	0.708***	0.504***	0.168***	0.672***
	(0.017)	(0.018)	(0.013)	(0.017)
Control Mean	0.063	0.024	0.012	0.036
Any of the 2 governmental programs				
Treated youth	0.747***	0.528***	0.184***	0.712***
	(0.016)	(0.018)	(0.014)	(0.016)
Control Mean	0.069	0.025	0.013	0.038

Source: Youth follow-up survey (1661 observations)
Notes: The table documents options to build human capital variables (see discussion in Appendix A3).

Table A6: Permutation test for main ITT estimates

Youth	Formal	Traditional	Total	Total hours per week	Total earnings
Asymptotic p	<0.001	<0.001	<0.001	0.0136	.6691
Permutation p	<0.001	<0.001	<0.001	0.0138	.6705
Firms	**Formal**	**Traditional**	**Total**	**Hours of work**	**Net value of work**
Asymptotic p	<0.001	0.0747	<0.001	0.0367	<0.001
Permutation p	<0.001	0.0746	<0.001	0.0382	<0.001

Upper panel: Youth follow-up survey (1661 observations). Estimation of equation 11.
Lower panel: Firm survey (674 observations). Estimation of equation 10.
For each variable, the table presents first the asymptotic p-value and then the p-value from permutation tests after 10.000 permutation within randomization strata.

Table A7: 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	7.128	3.379	3.719	0.030	0.970	0.203

Source: Firm follow-up survey (674 observations)
Notes: Estimation of equation 10 on various workforce variables obtained from the employer module of the firm follow-up survey.

Table A8: Sales and Profit of firms

	Sales		Profit	
	Level	Inv Hyper sine	Level	Inv Hyper sine
Treated	-63,474	0.122	-22,682	0.0284
	(46,934)	(0.173)	(15,857)	(0.211)
Control Mean	469338	12.47	169330	11.11

Source: Firm follow-up survey, 3810 observations (3 measures for all 674 firms, and 3 measures for 596 back-checked firms).
Estimation of equation 16 for sales (left panel) and profit (right panel). In each panel, the first column considers the variable as measured. The second column considers an inverse hyperbolic sine transformation: $arsinh(x) = log(x + \sqrt{x^2 + 1})$.

Table A9: Youth selection into apprenticeship

Variable Names	Always-takers	Compliers	Never-takers	p-val
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 secondary education or above	0.133	0.166	0.220	0.481
Skill Index (All)	1.766	1.713	1.696	0.337
Learning Skill Index	0.831	0.776	0.765	0.208
Behavioral Skill Index	0.935	0.937	0.932	0.954
Has activity	0.855	0.863	0.917	0.862
Total nb of activities	1.474	1.291	1.376	0.182
Total income (KCFA)	64.893	75.960	87.298	0.450
Total income (KCFA) (hyperbolic sin)	3.088	3.079	3.610	0.979
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

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... table A9 continued

Variable Names	Always Taker	Complier	Never Taker	p-val
Has friends with IGA	0.756	0.675	0.706	0.163
Parent 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
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
Forced to use savings to face emergencies	0.846	0.813	0.841	0.500
Has debt	0.319	0.309	0.342	0.876
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: The first column presents average baseline characteristics for "Always-Takers", i.e. youth assigned to the control group who entered traditional apprenticeship.

The second column presents average baseline characteristics of "Compliers", as in Abadie (2003).

The third column presents average baseline characteristics of "Never-Takers", i.e. youth assigned to the treatment group who did not start an apprenticeship.

The last column gives the p-value for the test of equality of means between "Always-takers" and "Compliers" (see footnote 50).

Table A10: Apprentice characteristics, performance and satisfaction

Apprentice characteristics (Follow-up firm survey, apprentice module)

			Education					Mastercraftman			
	Age	Male	No	Prim.	Second	Affected by crisis	Knows one	from family	from. neighb.	family acqu.	Wealth index
Formal	2.10***	-0.11***	-0.48***	0.35***	0.13***	0.04	-0.03	-0.05	0.03	-0.06	0.12
	(0.29)	(0.02)	(0.04)	(0.04)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)	(0.04)	(0.08)
Tradi	-1.01***	0.02***	0.08**	-0.08**	-0.00	-0.02	-0.06	-0.02	-0.08**	-0.03	0.05
tional	(0.37)	(0.01)	(0.04)	(0.03)	(0.02)	(0.02)	(0.04)	(0.03)	(0.04)	(0.04)	(0.07)
Ref.	20.67	0.95	0.68	0.25	0.07	0.11	0.57	0.21	0.26	0.38	-0.04

Apprentice performance (Follow-up firm survey, employer module)

	Work										
	Days	Value	Skills Index			Fees			Compensation		
	Last month	Last day	Tech.	Learning	Behav.	Entry	Last month	Exit	Meals and Transport	Bonus or Motivation	Total
Formal	-7.09***	839***	1.01***	0.27	-0.49**	-5,303***	-703	-9,416***	-3,697***	-356	-4,053**
	(0.87)	(272)	(0.21)	(0.20)	(0.21)	(879)	(451)	(2,301)	(1,293)	(716)	(1,744)
Tradi	0.04	338**	0.08	0.16	-0.01	320	274	-3,210	-1,189	646	-543
tional	(0.61)	(163)	(0.18)	(0.17)	(0.19)	(1,023)	(568)	(2,232)	(1,080)	(647)	(1,448)
Ref.	20.14	1296	4.152	5.187	6.795	4481	992.2	7991	9507	4287	13794

Apprentice satisfaction (Follow-up firm survey, apprentice module)

	Satisfaction with					Hours of work				Aspiration	
	Tasks	Hours	Earnings	Income	Work	Total	Auton--omous	Superv. by Master	Observing Master	Salaried	Self Employment
Formal	-0.10*	-0.06	-0.31***	0.06	-0.12**	-0.09	0.55**	-0.02	-0.28	0.23***	-0.23***
	(0.06)	(0.06)	(0.09)	(0.09)	(0.06)	(0.21)	(0.25)	(0.27)	(0.19)	(0.04)	(0.04)
Traditi	0.00	0.01	-0.05	0.05	0.06	0.27	-0.09	-0.01	0.25	0.00	0.00
tional	(0.06)	(0.05)	(0.08)	(0.08)	(0.06)	(0.19)	(0.22)	(0.25)	(0.19)	(0.03)	(0.03)
Ref.	3.645	3.587	3.036	2.969	3.645	8.170	2.506	2.628	1.726	0.182	0.791

Follow-up firm survey, apprentice module, for upper and lower panels (948 observations);
Follow-up survey, employer module, for intermediate panel (1260 observations).
Estimation of equation 19. Rows "Formal" present the difference in means between formal apprentices in treated firms and traditional apprentices in control firms who entered within 6 month of the randomization date. Rows "Traditional" compares average characteristics traditional apprentices who entered within 6 months of randomization in treatment and control firms. Other coefficients of equation 19 are not reported.
Traditional apprentices who entered control firms within 6 months of randomization are the reference category.