Direct and Indirect Effects of Subsidized Dual Apprenticeships

Bruno Crépon*
Patrick Premand†

September 20, 2024

Abstract

Public interventions in the apprenticeship market often aim to increase demand or returns. We set up a double-sided experiment with youth and firms to analyze a subsidized dual apprenticeship program. This intervention seeks to relax financial constraints for youth by offering a wage subsidy and to make apprenticeship more attractive by providing vocational training in technical skills to complement on-the-job training. We document a large increase in youth participation in apprenticeship, yet the inflow of apprentices induces little crowding out of traditional apprentices in firms. The intervention leads to an increase in youth demand for apprenticeship, enabling firms to fill open apprenticeship positions. The subsidy compensates apprentices for low wages but does not alleviate financial constraints. Consistent with the dual training component contributing to an increase in youth demand for apprenticeship, youth perform more complex tasks and have higher earnings four years after the start of the experiment.

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

Keywords: Employment, Apprenticeship, Wage Subsidy, Training, Dual Training, Vocational Training, Demand for Training, Direct and Indirect Effects, Equilibrium Effects, Micro- and Small Enterprises, Field Experiment, Africa.

*CREST-ENSAE. e-mail: crepon@ensae.fr
†Development Impact Evaluation Department (DIME), World Bank. e-mail: ppremand@worldbank.org

1 Introduction

Traditional apprenticeships are among the most common types of training in the developing world. They involve on-the-job training in small, informal firms for several years, often based on private arrangements between youth (or their parents) and firm owners. While apprenticeship markets have developed in low-income settings with little public intervention, improving national apprenticeship systems has become an important policy objective.[1] Apprenticeship reforms aim to expand youth demand for apprenticeship, improve the quality of training, and increase returns for youth. For instance, dual apprenticeships—a popular model in Western Europe—combine practical, on-the-job training with vocational training. Other interventions to improve apprenticeship include wage subsidies, enhanced intermediation, training for mastercraftspeople in firms, and certification (Wolter and Ryan, 2011).

Public interventions in the apprenticeship market look to address market failures for youth or firms. Market failures may limit youth demand for apprenticeship, reduce firms' willingness to train apprentices, and hinder matches between potential apprentices and firms. Youth may not invest in training without a guarantee they will learn general skills that can increase their earnings in future occupations. Training provision may remain suboptimal unless firms sign enforceable contracts to offer general training in transferable skills (Dustmann and Schönberg, 2012).[2] This issue may be particularly salient in developing countries, where small firms often have limited technology to teach general skills and formal contracts are rare. Firms may also be unwilling to take apprentices if training is too costly or complex (Alfonsi et al., 2020; Caicedo et al., 2022).[3] Furthermore, if firms are unwilling to cover the costs of training in general transferable skills, apprentices are left to incur these costs (Becker, 1962), and if capital markets are imperfect, youth may be unable to pay for training or to work for low wages in a traditional apprenticeship without financial support (Heckman and Mosso, 2014). Market failures may also hinder matches between potential apprentices and firms due to information asymmetries or imperfect intermediation (Hardy and McCasland, 2023).

In this paper, we analyze the introduction of a subsidized dual apprenticeship program

[1] For instance, see Walther (2008); ILO (2012); UNESCO (2015); Fazio et al. (2016); OECD/ILO (2017).

[2] The role of external certification has also been debated. On the one hand, it may limit the ability of firms to recoup training revenues (Katz and Ziderman, 1990), but on the other hand, it may be needed to provide firms and trainees with the incentives to invest in training (Acemoglu and Pischke, 2000).

[3] Labor market frictions can mitigate some of these challenges by allowing firms to retain trained workers at low wages (Acemoglu and Pischke, 1998, 1999b).

through a randomized experiment in Côte d'Ivoire. This government-led formal apprenticeship program is designed to increase the number of youth (aged 18–24) participating in apprenticeship and improve the quality of training they receive.[4] The intervention aims to tackle financial constraints for youth to invest in training by offering a subsidy of CFAF 30,000 per month (approximately US$54, or half the formal minimum wage), paid directly to apprentices for 12 or 24 months (depending on the occupation). The program also includes a dual training component that complements practical, on-the-job training in firms with technical courses in vocational centers, based on training curricula developed to ensure apprentices learn general skills. In addition, the program puts in place a basic apprenticeship contract and certification mechanism. The dual apprenticeship model can make apprenticeship more attractive for youth and address potential commitment failures in firms by allowing formal apprentices to receive general skills training in vocational centers and obtain certification in these skills. The intervention may also help overcome matching inefficiencies and fill apprenticeship positions by reducing recruitment costs for firms.

This paper makes three main contributions. First, we analyze how subsidized dual apprenticeships increase youth demand for apprenticeship. We document a strong increase in youth participation and show that dual training makes apprenticeship more attractive by improving skills and earnings four years later. These results suggest that demand for apprenticeship is sensitive to the quality of training and its returns. Second, we study both sides of the apprenticeship market. We use a two-sided experiment to identify both youth entry and the crowding-out effect in firms, quantifying the net number of positions filled by the intervention. We find limited crowding out of traditional apprentices in firms, such that the intervention induces a net increase in filled apprenticeship positions. We also explore how constraints on youth and firms combine to yield suboptimal training. Our results are consistent with low youth demand hindering apprenticeship expansion and firms facing high recruitment costs and hiring frictions, possibly due to the scarcity of apprentices. Third, we show how dual apprenticeships impact youth earnings. In the short term, wage subsidies compensate forgone earnings. In the medium term, treated youth perform more complex tasks and have higher earnings through higher productivity in self-employment.

The first part of the paper documents how subsidized dual apprenticeships expand youth

[4]We use the term "formal apprenticeship" throughout as a shorthand for "subsidized dual apprenticeship" and "formal apprentice" as a shorthand for "apprentice in a subsidized dual apprenticeship."

access to apprenticeship. In principle, fostering demand for apprenticeship is insufficient to increase the overall number of filled apprenticeship positions if firms cannot absorb additional apprentices. We thus set up a double-sided experiment that randomizes whether interested youth are assigned to a formal apprenticeship and whether apprenticeship positions opened by firms are filled with formal apprentices. As we show in a conceptual framework, this design is tailored to estimate the direct (windfall) effect on youth and indirect (crowding-out) effect in firms. The framework shows how those estimates combine to provide bounds for the net number of apprenticeship positions filled by the program. It also derives bounds for potential equilibrium effects due to changes in market tightness that might affect the control groups. We find that youth entry into formal apprenticeship increases by 71 percentage points, while entry into traditional apprenticeship declines by 18 percentage points, which means only 0.26 youth (0.18/0.71) who enter a formal apprenticeship would have otherwise entered a traditional apprenticeship. We also find that the inflow of formal apprentices induces little crowding out of traditional apprentices, with just 0.23 traditional apprentices displaced per formal apprentice placed. In summary, the subsidized dual apprenticeship program does not simply induce a reallocation of traditional and formal apprentices within firms but increases the number of filled apprenticeship positions. Using our framework, we conclude that the net number of new positions filled by the program is between 74% (1-0.26) and 77% (1-0.23) of the number of formal apprentices placed.

The second part of the paper documents how subsidized dual apprenticeships affect youth employment and earnings in the short and medium term (two and four years after the program started, respectively). We also assess their impact on skills by building an index of routine task intensity for each worker. In the short term, the subsidy offsets a reduction in labor earnings due to low wages in apprenticeship. In the medium term, however, treated youth have higher earnings by 15%. Youth assigned to formal apprenticeships are also more involved in non-routine analytical tasks, especially if they have completed dual training and certification, in line with the skills-building aim of the dual training component. We do not find that treated youth are more likely to hold wage jobs after the program, but the results are consistent with an increase in productivity in informal self-employment.

The third part of the paper explores how subsidized dual apprenticeships induce greater youth entry into apprenticeship with little crowding out in firms. We study the relative roles

of the subsidy and dual training component as mechanisms for increasing youth demand for apprenticeship. Although the main intent of the subsidy is to address financial constraints, we do not find evidence that it helps a more disadvantaged population enter apprenticeship. Instead, the results are consistent with the dual training component making it more attractive for youth to enter apprenticeship by increasing skills and post-training earnings. Improved matching also plays a role in inducing greater youth entry, but it is insufficient to explain the magnitude of the effect. We also do not find evidence that the program addresses an unwillingness by firms to host apprentices. Apprentices account for more than half of firms' workforce, and few firms do not hire apprentices during the study period. Firms also provide similar training to traditional and formal apprentices, who make comparable overall contributions to firm activities. Instead, the limited crowding-out effect is more consistent with firms facing high costs to hire additional apprentices, which may be related to their relative scarcity and limited youth demand for apprenticeship.

The Online Appendix expands on our contribution to the broader literature, including on the impact of training on employment outcomes (Card et al., 2018; JPAL, 2022). Vocational training in combination with on-the-job training has positively impacted employment outcomes (e.g., Attanasio et al., 2011, 2017; Kugler et al., 2022). Solely on-the-job training, such as traditional apprenticeship, has had more limited results (e.g., Frazer, 2006; Hardy et al., 2019; Alfonsi et al., 2020). Many studies emphasize the role of training in improving access to (formal) wage employment. In contrast, our results show that dual apprenticeships lead to improvements in skills consistent with gains in productivity in informal occupations, including self-employment. The Online Appendix also outlines how the paper complements the literature on labor demand and indirect effects in firms, particularly in the context of apprenticeship (Hardy and McCasland, 2023; Caicedo et al., 2022; Alfonsi et al., 2020).

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 analyzes youth entry into apprenticeship and the crowding-out effect in firms. Section 6 documents the impact on youth earnings in the short and medium term. Section 7 assesses the mechanisms through which the program increases youth demand for apprenticeship, human capital, and skills while inducing a limited crowding-out effect in firms. Section 8 discusses the program's costs, benefits, and scale-up potential. Section

9 concludes. The Annex contains tables, figures, and additional details on the conceptual framework. The Online Appendix contains supplementary material.

2 Intervention and Experimental Design

2.1 Apprenticeship in Developing Economies

Traditional apprenticeship is widespread in the developing world. In Africa, there may be up to four times more traditional apprentices than youth attending vocational school (Filmer et al., 2014). Traditional apprenticeships are one of the few sources of training accessible to the many youth who do not complete primary (elementary) or secondary school. Traditional apprenticeships are private arrangements between young people (or their families) and micro- and small firms, mostly in the informal sector (Walther, 2008). Youth are often placed with mastercraftspeople identified through family connections. A fee (in-kind or cash) is paid for the placement. Arrangements are rarely formalized through a contract. Youth learn the trade through practical, on-the-job training alongside a mastercraftsperson. Over time, apprentices start to be paid. Traditional apprenticeships can last many years and often do not lead to certification, although mastercraftspeople typically grant departure to mark the completion of an apprenticeship. Youth then become wage workers in the host firm, move to another firm, or transition into self-employment—in most cases, still in the informal sector. In our baseline sample, the majority of apprentices aspire to become self-employed.

2.2 The Côte d'Ivoire Formal Apprenticeship Program

Côte d'Ivoire is a lower-middle-income country with a GDP per capita of CFAF 770,000 (approximately US$1,350) in 2015. Following a decade of conflict, stability returned in 2011, and growth has been robust over the last decade, though not enough to induce rapid industrialization, given the low number of formal firms and low share of formal employment. Only 17.4% of working individuals hold wage jobs (less than half formal), with unemployment at 6.7% (Christiaensen and Premand, 2017). Over 90% of formal firms and wage jobs are concentrated in the city of Abidjan. Less than 10% of formal firms export. Most wage employment takes place in small firms serving the domestic market, particularly in the service

sector. In contrast, informal self-employment predominates, with 46.9% of the working population in agriculture and 29.3% in micro-enterprises.

A range of public investments and programs were launched in 2011, including an emergency youth employment and skills development project (PEJEDEC).[5] The PEJEDEC formal apprenticeship program initially aimed to cover 5,000 low-skilled youth between 18 and 24 years old, who were placed in firms for 12 to 24 months (depending on the occupation), where they received on-the-job training under the supervision of a mastercraftsperson. Apprentices signed a contract with the implementation agency (AGEFOP) and were paid a monthly subsidy of CFAF 30,000 (approximately US$54, or half the formal minimum wage) to cover meals and transportation costs. They also received insurance coverage and work equipment. The apprenticeship included a dual training component—practical, on-the-job training was complemented with theoretical training (approximately 180 hours per year) tailored to the needs of apprentices and delivered by local training institutions. AGEFOP defined the key general skills apprentices needed to learn in each trade and developed training curricula. The program also introduced basic regulations through standardized apprenticeship contracts. AGEFOP apprenticeship counselors regularly visited mastercraftspeople and apprentices to track skills acquisition through a booklet detailing learning milestones for each trade. Counselors had the authority to suspend subsidies for absenteeism or performance issues. Formal apprenticeships ended with a skills assessment, with the possibility of certification. Firms were not compensated for taking on apprentices, though they received a small toolkit of materials to facilitate practical learning. Moreover, employers committed 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 program has since gradually been scaled up, as we discuss in Section 8.2.

2.3 Enrollment Process and Experimental Design

The experiment's first objective is to simultaneously measure the subsidized dual apprenticeship program's direct (windfall) effect on youth and indirect (crowding-out) effect on

[5]PEJEDEC is the Projet Emploi Jeune et Développement des Compétences (www.pejedec.org). The PEJEDEC apprenticeship component is led by the office coordinating employment programs, BCP-Emploi (Bureau de Coordinnation des Programmes d'Emploi), with the national training agency, AGEFOP (Agence de la Formation Professionnelle), as implementing agency. See Bertrand et al. (2021) for evidence on the effectiveness of a public works program supported by the same project.

firms. This requires a design that randomly assigns both youth and firms to treatment and control groups. This section presents the key features of the experimental protocol, with more details in the Online Appendix A2. Figure 1 illustrates the design. In Section 3, we show that the experiment identifies parameters from a simple theoretical framework, and we discuss how the effects on youth and firms combine.

The experiment was implemented in seven urban localities in the country's interior. The design was stratified by micromarkets, which are each defined as a trade in a given locality. The implementing agency first identified firms interested in hosting program apprentices and how many positions they had open. This gave the agency a number of positions to be potentially filled in each micromarket. The agency then registered interested, eligible youth in the experiment. In each micromarket, as many youth were registered as there were open positions.

We then randomly assigned firms to treatment and control groups to have an equal number of treatment and control positions in each micromarket. One practical complication was that firms did not offer the same number of positions in each micromarket, and firms could also open positions in several (closely linked) micromarkets.[6] To address this, we paired firms according to the structure of their open positions in the set of micromarkets and then performed randomized assignment within each pair. Once treatment firms were drawn, the number of positions to be filled in each micromarket was obtained. This was usually half the number of positions registered in the first step.

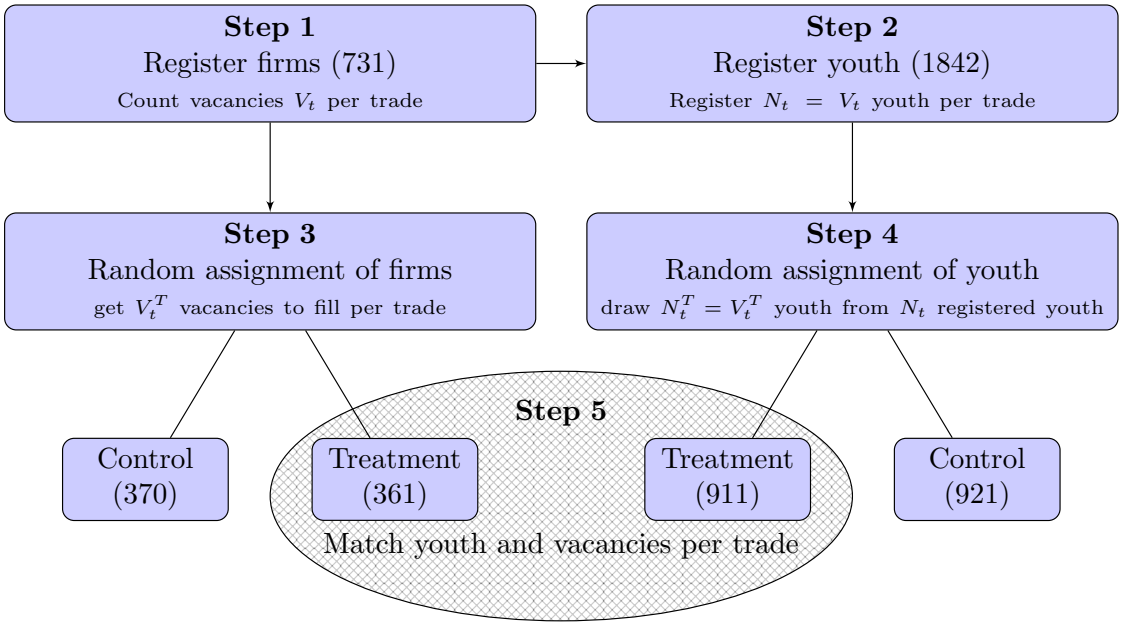
The next step was to randomly assign exactly the same number of youth to treatment in each micromarket as the number of positions to be filled.[7] The rate of treatment assignment for youth is specific to the micromarket, so we use weights in the youth-level analysis (see Section 4). In the final step, counselors from the implementing agency matched the selected youth with selected firms that offered positions in the same trade. The matching took place based on criteria such as the distance between the firm and the youth's place of residence.

[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, and car painter.

[7] Figure A1 presents the distribution of the ratio of treated positions to total number of positions by micromarket. 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. 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 being assigned to treatment or control. In such a case, the youth assignation probability is either 0 or 1. We kept these firms in the data set, but the corresponding youth were not included in the sample for youth regressions. This is the case for 10 youth.

Across the seven localities in the study, 731 firms offered apprenticeship positions, and 361 were randomly selected to host program apprentices. Of 1,842 eligible applicants, 911 youth were assigned to treatment.[8] Most firms offered several positions, and, on average, treatment firms were assigned 2.52 apprentices.

Figure 1: Experimental Design



Note: This figure describes the five steps of the experimental protocol. This design was implemented separately in each of the seven localities in the experiment. The numbers in parentheses provide the number of observations across the seven localities. Ten youth were removed from the sample because of the special case mentioned in footnote 7, so the youth sample has 1,832 observations.

The randomization protocol implies that youth and firms in the treatment and control groups are statistically similar (as discussed further in Section 4). However, one potential concern is whether youth and firms from the control groups are affected by the experiment—for instance, through spill-over effects driven by changes to the tightness of the apprenticeship market (i.e., the chance that firms and potential apprentices match). The conceptual framework in Section 3 discusses sources of adjustment to the tightness of the apprenticeship market and gives reasons to expect the adjustment to be small.

The size of the experiment is one factor that drives the size of the potential adjustment in the tightness of the apprenticeship market. We can assess the experiment's size compared to the market for apprentices in two ways, focusing on either youth or firms (see Table 1).

[8]Of the 1,842 applicants, 921 were assigned to control, and 10 youth were removed from the sample because of the special case mentioned in footnote 7.

First, based on a national employment survey and population census, we estimate the share of youth in the experiment relative to the number of apprentices in the study localities to be approximately 11% (see Table A1 and Online Appendix A2.3.1 for details).[9] Second, based on a listing of firms collected ex post, we estimate that the share of treated firms represents approximately 22% of all firms hosting apprentices in selected sectors and localities (see Table A2 and Online Appendix A2.3.2 for details).

Based on our data, we estimate the shares of youth and firms in the experiment in each locality. This allows us to compare results between localities above or below the median for each measure (reported in bold in Table 1). We discuss further the role of the size of the experiment in Section 3 and document results by saturation levels in Section 5.3.3.

The experiment induces negative shocks on both the demand for and the supply of traditional apprentices. Within each micromarket, the experimental design maintains a similar ratio of apprentices and vacancies in the treatment and control groups. This also ensures that the chances of a match between firms and youth in the control group are unchanged. Importantly, the rankings of the localities in Table 1 according to experiment size on the youth and firm sides are very close.

3 Conceptual Framework

The formal apprenticeship program induces youth to enter apprenticeship and fill formal apprenticeship positions in firms. It creates a shock on both the youth and the firm sides of the traditional apprenticeship market. Some youth who would have participated in traditional apprenticeship may now enter formal apprenticeship and thus exit the market for traditional apprenticeship. Some firms that had positions open for traditional apprentices might fill them with formal apprentices instead and remove these positions from the market. This section discusses how the intervention affects the equilibrium of the traditional apprenticeship market. First, it assesses how many apprenticeship positions are filled for each program apprentice placed, after accounting for the crowding out of traditional apprentices. Second,

[9] As discussed in the Online Appendix, the sectors targeted by the program account for at least 60% of all apprenticeship positions in the study areas, so the share of treated youth among apprentices in these sectors is at most 16%. This is an upper bound for the size of the experiment because we only consider the population of apprentices in the study localities, without including nearby urban areas in the same district. As we show further below, the identification of the total employment effects does not depend strongly on the experiment's small size.

it sheds light on how the experiment may have changed the matching opportunities between youth and firms in the control group. This is informative about the scope for potential interference and the validity of the control group.

We develop a conceptual framework to analyze the equilibrium of the traditional apprenticeship market with and without the intervention (detailed in Annex 12.2). The model analyzes the supply of traditional apprentices $S^{trad}(\theta)$ and the demand for traditional apprentices in firms $D^{trad}(\theta)$. As in the classical labor market literature (Pissarides et al., 2000), the equilibrium is a function of the tightness of the apprenticeship market (θ), which describes the chances that an individual finds an apprenticeship position or that a firm fills an apprenticeship vacancy. In other words, θ is the ratio of available vacancies to the number of youth searching for a position. The model distinguishes between traditional and formal apprentices, and it shows how the introduction of formal (subsidized dual) apprenticeship affects the decisions of youth to enter apprenticeship.

The entry of N_{form} individuals into formal apprenticeship affects the equilibrium of the traditional apprenticeship market: a share ω of these N_{form} individuals would have entered traditional apprenticeship but are now removed from the market, while a share ψ of the N_{form} apprenticeship positions filled by formal apprentices would have otherwise been filled with traditional apprentices but are now also removed from the market. ω and ψ are thus the two key parameters that we estimate based on the experimental design. In this section, we provide some intuition on their interpretation. Annex 12.2 shows how the parameters are derived from the model.

ω measures the share of youth in formal apprenticeship who would have entered traditional apprenticeship absent the program, which we call the "windfall effect." The model in Annex 12.2.3 (Equation 14) shows that $\omega = \frac{\lambda(\theta_1)}{\lambda_f} P(trad|reg)$, where $P(trad|reg)$ denotes the proportion of registered youth who would search for a traditional apprenticeship position absent the program. $\lambda(\theta_1)$ is the matching rate for traditional apprentices given the market tightness conditions after the program is introduced (θ_1).[10] λ_f is the share of youth who register in the program and enter a formal apprenticeship position.[11]

[10] As will be discussed below, the introduction of the program creates new market conditions, including a new equilibrium tightness, which is noted θ_1.

[11] Youth first register in the program, and then are made an offer. *reg* denotes the initial number of "pre-offer" youth—i.e., the number of youth signing up for the program before offers are made. Not all of these youth will receive an offer and participate in the program. λ_f represents the share of these "post-offer"

The magnitude of ω is thus a function of two main factors. First, related to $P(trad|reg)$, the model points to two reasons why some youth apply to the formal apprenticeship program but would not search for a traditional apprenticeship position. On the one hand, formal apprenticeship may attract new individuals by providing dual training that improves the quality of training and later increases post-training earnings. The commitment to dual training and the possibility of certification can increase youth expectations about post-training earnings (e.g., Alfonsi et al., 2020; Bandiera et al., forthcoming). On the other hand, formal apprenticeship may also attract new individuals into apprenticeship by providing a subsidy. Participation in apprenticeship has a high opportunity cost because apprentices are paid low wages. The subsidy may be needed for those who do not have access to financial resources (support from family or credit) to meet their minimum needs during the training period. The second determinant of ω relates to $\frac{\lambda(\theta_1)}{\lambda_f}$ and captures the probability that youth searching for a traditional apprenticeship position actually enter one, relative to the probability of youth assigned to treatment actually entering formal apprenticeship. In other words, the second determinant captures the relative matching (entry) rates in the two types of apprenticeship among those who search.

ψ measures the crowding out of traditional apprentices by formal apprentices in firms. If $\psi = 1$, there is full crowding out: each entry of a formal apprentice is associated with one fewer traditional apprentice in the firm, such that the total number of apprentices remains unchanged. On the other hand, if $\psi = 0$, there is no crowding out: the entry of one formal apprentice has no effect on the number of traditional apprentices in the firm. The parameter ψ (derived in Annex 12.2.2) writes $\psi = -af''/(c'' - f'')$, with a as the productivity differential between formal and traditional apprentices, f the firm's production function, and c the cost of recruiting apprentices. The magnitude of ψ thus depends on various factors, including the firm's production technology, the productivity differential between traditional and formal apprentices, the firm's capacity to host apprentices, and the firm's ability to recruit additional apprentices.[12] ψ will be high if formal apprentices are more productive than traditional apprentices and if firms face binding constraints on the number of apprentices

individuals among the "pre-offer" individuals. *trad* denotes youth searching for a traditional apprenticeship position absent the program.

[12]The capacity constraints on the number of apprentices the firm can host is formally captured by the second derivative of the production function f'', while the constraint on the number of apprentices the firm can recruit is formally captured by the second derivative of the recruitment cost c''.

they can employ. Conversely, even if formal apprentices are more productive than traditional apprentices, ψ may be low if firms face binding constraints on the recruitment side—i.e., if the cost of recruiting additional traditional apprentices is high.

Annex 12.3.2 shows how the parameters ω and ψ can be estimated based on an instrumental variable strategy. ω is estimated by regressing entry into apprenticeship on entry into formal apprenticeship, using youth assignment to treatment as an instrument. ψ is estimated by regressing the total number of apprentices entering firms on the number of formal apprentices entering firms, using firm assignment to treatment as an instrument.

Absent the program, the equilibrium number of traditional apprentices is N_0, and the market tightness is θ_0. We can take linear approximations around this equilibrium in the absence of intervention:[13]

$$S^{trad}(\theta) = N_0 + A_s(\theta - \theta_0) - \omega N_{form}\,, \tag{1}$$

$$D^{trad}(\theta) = N_0 - A_d(\theta - \theta_0) - \psi N_{form}\,. \tag{2}$$

With the program, there is a new equilibrium in the number of traditional apprentices entering firms (N_1^{trad}) and apprenticeship market tightness (θ_1). We can express the change in the number of traditional apprentices ($N_1^{trad} - N_0$) and in the tightness ($\theta_1 - \theta_0$) as a function of the two shift parameters, ψ and ω. This can be done by expanding the supply and demand functions around the final equilibrium value θ_1:

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

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

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

As Equation 3 shows, the overall reduction in the number of traditional apprentices on the market is an average of the windfall effect (ω) on the supply side, and the crowding-out effect (ψ) on the demand side. This has two important implications. First, in our double-sided experiment, the net number of apprenticeship positions filled can be bounded: $(N_{form} + N_1^{trad} - N_0)/N_{form} \in [1 - max(\omega, \psi), 1 - min(\omega, \psi)]$. The validity of these bounds

[13]Note that ω and ψ depend on the tightness, but they are defined at the new equilibrium θ_1.

does not depend on the assumption that the size of the experiment is small, or that the labor market tightness adjustment is small.

Second, one-sided experiments, limited to either the demand or supply side of the labor market, would not provide sufficient information to estimate the net number of positions filled by the program. For example, a one-sided experiment focusing on youth would only estimate ω, while ψ would be unobserved. However, in a weak labor market or any situation in which $A_s \ll A_d$, the final adjustment would depend on the unobserved parameter $(N_{form} + N_1^{trad} - N_0)/N_{form} \approx 1 - \psi$.[14]

Equation 4 provides information about the magnitude of the labor market tightness adjustments. First, the windfall (ω) and crowding-out (ψ) parameters are expected to be positive, so they would tend to cancel each other in the adjustment of θ. In the empirical analysis, we will estimate the two parameters so that we can also 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, such that their order of magnitude is of the size of the market (say M). The order of magnitude of N_{form} is of the size of the experiment (say E), thus, the order of magnitude of the second term in Equation 3 is the share σ of the experiment relative to the market (E/M).[15] Going back to the adjustment in the tightness, we get the following approximation:

$$(\theta_1 - \theta_0) \approx (\omega - \psi)\sigma\,. \tag{5}$$

Equation 5 sheds light on the extent of potential interference between the treated and control units. It shows that the tightness adjustment is small if σ is small or if ω and ψ are of similar magnitude. As discussed in Section 2.3, the relative size of the experiment is small. This is one reason to expect the adjustment in market tightness to be small. In addition, as mentioned above, ω and ψ are expected to be positive, so they would tend to cancel each other in the adjustment of θ. In the empirical analysis, we will estimate the two parameters and on that basis confirm that the adjustment of θ is small.

[14]The magnitude of A_s/A_d depends on θ_0. In the different context of job placement services, Crépon et al. (2013) show that the magnitude of displacement effects also depends on the relative slopes of the supply and demand curves and can be particularly strong in weak labor markets, characterized by a low A_s/A_d (Michaillat, 2012).

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

Nevertheless, even if the adjustment is small, it is not zero. As such, the counterfactual we estimate in the control group is not exactly the one that would have prevailed in absence of the intervention. While our estimated (ITT) parameters can in theory be biased, we show in Annex 12.3.1 that we can bound the potential bias by $\sigma(\omega - \psi)\lambda_f$ for youth and by $\sigma(\omega - \psi)n_f$ for firms (with n_f denoting the average number of formal apprentices hired per firm). We can compute these bounds by measuring σ and estimating $\omega - \psi$, λ_f and n_f. On this basis, we show in the empirical analysis that the estimated bias is very small in our setting.

Although small on average, the size of the experiment varies between localities (Table 1). This variation is not random but can be used to assess whether parameters ω and ψ and entry rates into apprenticeship in the control groups vary by saturation level.

4 Data and Estimation Strategy

4.1 Data

The formal apprenticeship program was rolled out by locality. Baseline data were collected during the enrollment process for each firm that offered apprenticeship positions validated by program staff. Separately, baseline data were collected among youth deemed eligible after they successfully passed a motivation interview. Baseline data collection took place between July and October 2014. The randomization of firms and youth took place in each locality shortly after, and placements were mostly completed by January 2015.

The midline survey took place between March and June 2016. A youth survey was collected by phone, and a firm survey was collected in person. The surveys were collected 20 months after the start of the program, on average. Since most apprenticeships last 24 months, results based on the midline survey should be interpreted as providing information about short-term impacts, while apprentices were still in the program.[16]

The endline survey took place between May and October 2018, approximately four years after the start of the experiment, or 18–24 months after the end of the program. The survey focused on youth to estimate impacts on employment and earnings after the completion of

[16] Of the 914 treated youth, 754 (or 82%) were in trades where the apprenticeship lasted 24 months.

formal apprenticeships. It was collected by phone.[17]

Substantial efforts were made to minimize attrition. As a result, 1,661 youth (response rate: 90.7%) and 674 firms (response rate: 92.2%) were surveyed at midline, and 1,686 youth (response rate: 92%) at endline. The response rates are balanced across the treatment and control groups.[18]

Last, a listing of all firms with apprentices in localities and trades covered by the experiment was collected in 2021. Although obtained ex post, this provides additional information to estimate the size of the experiment on the side of firms, as we discuss further below (and in Online Appendix A2.3.2).[19]

4.2 Youth and Firm Baseline Characteristics

Tables A3 and A4 present baseline characteristics and balance checks for youth and firms, respectively. Table A3 shows that youth interested in formal apprenticeship are, on average, 20.7 years old and mostly men (87%). They have limited education: 63% have completed primary school and 17% lower secondary school. Of applicants, 54% aspire to become self-employed. Youth treatment and control groups are well-balanced at baseline.[20]

Table A4 shows that most sample firms are micro- and small, informal enterprises: 84% have no formal legal status, and 68% do not keep books. Firms have, on average, 6.4 permanent employees (counting the owner), of which 3.4 are apprentices. Traditional apprentices therefore constitute over half the workforce in the sample firms. Traditional apprentices are mostly hired through private channels, 82% based on a request from their parents. Approximately half of the apprentices in firms at baseline pay fees to the mastercraftspeople. In the

[17]Youth who could not be contacted by phone were interviewed in person.

[18] The last row of Table A3 presents the balance check for the response rate among youth in the midline and endline surveys ($p = 0.92$ for midline and $p = 0.44$ for endline). The last row of Table A4 contains the balance check for the response rate among firms at midline ($p = 0.48$). Note that part of this attrition is due to firm closure. We designed a specific module for employers whose firm had closed by the time of the midline. Twelve cases were identified. "True" attrition is limited to 6.2%. An unfortunate issue with the online server used for electronic data collection led to the loss of baseline data for 26% of youth and 5% of firms. The problem was concentrated in two localities. The loss of some baseline data limited the availability of contact information to track youth (and firms). This contributed to a lower response rate among youth in localities where these issues occurred.

[19]The firm data (baseline and midline) (Crépon and Premand, 2024a), the youth data (baseline, midline and endline) (Crépon and Premand, 2024b), and the firm listing data (Crépon et al., 2024) are posted in the Wold Bank microdata library.

[20]The share of available baseline data displayed in the last row (see footnote 18) is not perfectly balanced, but the response rates at midline and endline are well-balanced, which is what matters most because it is the sample used for empirical estimation.

two years before the baseline survey, 1.2 apprentices per firm finished their training. Importantly, among firms who had at least one apprentice finishing, 0.57 apprentices were hired by the firm, 0.63 were hired by another firm, and 1.68 became self-employed. This shows that apprenticeship is a pathway towards self-employment, and not solely wage jobs. Table A4 documents that the experiment led to good baseline balance between the treatment and control firms.

Table A6 shows how youth in the sample compare to traditional apprentices who enter control firms shortly after the randomization, as well as youth (between 18 and 24) and traditional apprentices across urban areas of Côte d'Ivoire (based on a national employment survey). Sample youth have higher educational attainment compared to traditional apprentices in control firms, as well as youth and traditional apprentices in urban areas. They are more likely to be male than the population of youth and apprentices, though not compared to traditional apprentices in control firms, likely due to the selection of sectors for the program. Section 7.1 discusses in greater detail how the program affected the demand for apprenticeship and the profile of apprentices entering firms.

The population of firms can also be compared to small firms in urban areas in the interior of the country. Existing firm census and enterprise surveys in Côte d'Ivoire only cover formal firms. Figure A2 plots the distribution of employees in small formal firms outside Abidjan based on a national enterprise survey.[21]. The mean number of employees in sample firms (6.5) is close to the mean number of employees in these small, formal firms (7.5), though our sample includes more micro-firms (defined as having fewer than 5 employees) than small firms (defined as having between 5 and 20 employees). While there is no representative survey of informal firms that we can compare our sample with, our study takes place in an understudied segment of micro- and small firms that accounts for a large share of employment in low-income and lower-middle-income countries, such as Côte d'Ivoire.

4.3 Youth Midline and Endline Surveys

We collected data on youth employment, earnings, and hours worked for primary and secondary activities at midline and endline. In the employment survey module, we distinguish

[21]The Enterprise Survey for Côte d'Ivoire was collected in 2016 (Global Indicators Department, Enterprise Analysis Unit, 2017)

between occupation as a wage worker, self-employed, or an apprentice. This provides a measure of apprenticeship participation, our first main outcome for youth. At midline, we also measured entry into apprenticeship since the start of the experiment. This comes from a module on human capital investments that captures participation in apprenticeship, vocational training, and schooling. We can then distinguish between participation in formal and traditional apprenticeship based on a question about enrollment in formal, government-supported apprenticeship programs.[22] Self-reported earnings is our second main outcome for youth, in line with most studies on employment and training programs (Card et al., 2018; JPAL, 2022). Self-reported earnings may be subject to recall errors, especially if they are irregular or affected by seasonality, but administrative records are only available for formal jobs, which would not capture the vast majority of occupations in our sample.[23]

At endline, we collected detailed information on youth human capital investments since the start of the experiment, including after the end of the program. We also collected data on the types of tasks youth undertake in their primary occupation. This procedure is based on the "task approach" to estimate the skill content of occupations developed by Autor et al. (2003) and Acemoglu and Autor (2011).[24] We use this module to test whether the

[22]The list of possible formal apprenticeship programs includes the subsidized dual apprenticeship program offered by PEJEDEC as well as a smaller program offered by AGEFOP. The two programs can sometimes be hard for youth to distinguish because PEJEDEC was also implemented by AGEFOP. We thus consider a formal apprenticeship variable capturing youth who reported being enrolled in formal apprenticeship supported by either PEJEDEC or AGEFOP. Traditional apprenticeship is the only modality almost all firms can offer in the absence of the PEJEDEC formal apprenticeship program.

[23]We use modules from the Cote d'Ivoire national labor survey (ENSETE 2013) developed by the National Statistical Institute with technical assistance from the ILO and World Bank (Christiaensen and Premand, 2017). The employment module captures information for primary and secondary activities. For each activity, it collects (i) a description of the occupation, (ii) time worked over the last seven days and last month, (iii) the type of activity (wage, self-employed, apprentice, others including agriculture/livestock), and then, separately, (iv) cash and in-kind earnings over the last month. Additional information is collected on any other labor earnings and non-labor earnings (stipends, transfers, etc.). The next module then verifies whether the respondent had any self-employed activity and, if so, captures revenues and profits for that activity for the last month. (The literature has shown that direct reports of revenues and profits do not provide noisier measures than detailed accounting (De Mel et al., 2009).) We aggregate earnings in cash and in kind across these various activities. We also document results separately for wage earnings, self-employment earnings, and apprenticeship earnings. The youth endline survey further includes a retrospective employment calendar that captures all occupations since midline. Note that the intervention specifically targeted individuals and operated in urban areas where household agriculture and livestock activities are less common. We were not able to collect a consumption module in our context, but consumption measures would be helpful to complement earnings in future studies, including to bridge the literature on household-based poverty reduction programs and the urban labor literature that focuses more on individual-level interventions and measures of earnings.

[24]Specifically, Autor and Handel (2013) measure the content of tasks at the worker level. This approach was adapted to large-scale surveys in developing countries by Dicarlo et al. (2016).

intervention affected youth skills. For each worker, we build an index of routine task intensity. The index aggregates routine and non-routine (analytical or interpersonal) tasks. We then estimate whether participation in formal apprenticeship impacted skills by increasing the frequency of non-routine tasks and decreasing routine task intensity.

4.4 Firm Midline Survey and Employer-Employee Data

The firm midline survey collected data on firms' characteristics, workforce, sales, and profits. It listed 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. These employer-employee data enable us to accurately measure the flows of apprentices into and out of firms, as well as their contributions to firm activity. We can also identify each apprentice enrolled in a formal apprenticeship program. This provides us with our main outcome, the number of (traditional and formal) apprentices who entered the firm since the start of the experiment and who were present in firms at the time of the midline survey.[25] For each apprentice, the survey asked about the number of days worked in the last seven days and the number of hours worked in the last business day. To measure apprentices' contribution to firm activity, we asked employers about the amount they 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 (see Online Appendix A3). We also asked employers about the compensation paid to each apprentice. 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 apprentices' work 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.

[25] This allows us to 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.

4.5 Estimation Strategy

Given the double-sided randomization design, we estimate intent-to-treat (ITT) program impacts on firms 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 midline:

$$y_i = a + bT_i + \sum_v \gamma_v 1_v + \sum_s \delta_s 1_s + u_i \,. \tag{6}$$

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.[26]

In parallel, we estimate ITT program impacts on youth 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 groups with probabilities specific to each trade in each locality, producing a set of corresponding strata S_t. We compute the empirical assignment rate in each stratum $\widehat{\pi_m}$ and estimate inversely propensity-weighted regressions. The weights are defined as $T_i/\widehat{\pi}_m + (1 - T_i)/(1 - \widehat{\pi}_m)$. To obtain accuracy gains from stratification, we run an inversely propensity-weighted regression with strata dummies on the 1,661 youth observations:

$$y_i = a + bT_i + \sum_{St} \mu_{St} 1_{St} + u_i \,. \tag{7}$$

We compute White-Huber robust standard errors. As a robustness check, we implement permutation tests for the main ITT estimates for youth and firms (Imbens and Rubin, 2015). We also report p-values adjusted for the false discovery rate (Benjamini and Hochberg, 1995) and family-wise error rate (Romano and Wolf, 2016) for multiple hypothesis testing.

Last, we analyze differences in variables measured at the apprentice level in the firm survey (described in Section 4.4), including baseline characteristics, as well as performance,

[26] Sectors are broader than trades. "Sectors" refers to firm activities, and "trades" refers to youth occupations. The two concepts are often the same, but, in some cases, firms in a given sector are active in several trades. See Online Appendix A2 for more details.

compensation, and payments at midline:

$$y = a + b_f f + b_{tT} t \times T + \sum_s \gamma_s 1_s + \sum_t \delta_t 1_t + u\,. \tag{8}$$

The coefficients b_f and b_{tT} measure the difference between formal apprentices in treatment firms and, respectively, traditional apprentices in treatment firms and traditional apprentices in control firms, which is the excluded category. Observed differences are due either to the treatment or to selection into formal apprenticeships. While they cannot be given a causal interpretation, they help analyze mechanisms. Note that the structure of the apprentice population is different between the treatment and control firms. To take this into account when estimating Equation 8, we weigh observations of the control group by $P/(1-P)$, where P is the proportion of observations from treated firms for a given trade and a given location.

5 Short-Term Windfall and Crowding-Out Effects

5.1 Youth Entry into Apprenticeship and Windfall Effect

In this section, we document how the formal apprenticeship program affected youth entry into apprenticeship and the extent to which it induced a windfall effect as youth switched between traditional and formal apprenticeship. The left panel of Table 2 ("Youth") presents ITT estimates for youth participation in apprenticeship, decomposed between traditional apprenticeship and formal apprenticeship.

The top panel of Table 2 documents the impacts on participation in apprenticeship at midline. We find a 36.5 percentage point increase in the share of youth who participate in apprenticeship 20 months after the start of the experiment (column (3)). While only 17.9% of youth in the control group are in apprenticeship, the proportion in the treatment group reaches 54.4%. Column (1) documents a large increase in participation in formal apprenticeship, which is 49 percentage points larger in the treatment group than in the control group. Participation in formal apprenticeship is in part associated with substitution out of traditional apprenticeship (column (2)): 16.1% of youth in the control group participate in traditional apprenticeship, but this proportion is reduced by 12.5 percentage points in the treatment group. The results are robust to multiple hypothesis corrections, and there is

strong concordance between asymptotic results and results from permutation tests.[27]

Our surveys also enable us to measure inflows into apprenticeship positions over the duration of the experiment, including youth who entered an apprenticeship position but dropped out. The bottom panel of Table 2 presents ITT estimates for impacts on entry into apprenticeship. These estimates confirm our previous findings of increased participation as well as a significant windfall effect. The share of youth entering formal apprenticeship over the course of the experiment increases by 71.2 percentage points.[28] The share of youth entering any type of apprenticeship increases by 52.8 percentage points. The difference between the two estimates shows that 18.5% of treated youth would have entered traditional apprenticeship without the intervention, a significant but small windfall effect.[29]

5.2 Apprentice Intake and Crowding-Out Effect in Firms

We now analyze how the formal apprenticeship program affected firms' demand for apprentices in the short term. This includes the key question of whether the program induced an indirect crowding-out effect in firms.

Table 2 (upper right panel, "Firm") documents the program's impact on the number of apprentices in firms at midline. The program led to an increase in the total number of apprentices by 0.613 apprentices per firm (column (6)). This is a key result: offering youth the opportunity to participate in subsidized dual apprenticeship increases firms' total number of apprentices. We can decompose this effect between changes in the number of formal and traditional apprentices. The total number of formal apprentices in firms increases by 0.787 (column (4)). The difference between the estimates in columns (6) and (4) is the impact on the entry of traditional apprentices, which is directly related to the crowding-out effect: 0.174 traditional apprentices are displaced per formal apprentice placed (column (5)). This

[27] Table A10 shows the results from permutation tests (top panel, first three columns).

[28] There is also small non-compliance when analyzing the inflow of apprentices. The share of control youth in formal apprenticeship is 3.8%. This is due to our definition of formal apprenticeship, which includes both the AGEFOP and PEJEDEC programs, as mentioned in Section 4.1 and footnote 22.

[29] Differences between results on the share of youth in apprenticeship at midline (top panel) and over the course of the experiment (bottom panel) are related to drop-out. Drop-out is a common issue in many employment or training programs, including in apprenticeship (e.g., Cho et al., 2013). We observe drop-out from formal apprenticeship as well as from traditional apprenticeship. Of the 75% of treated youth who enter a formal apprenticeship position, 50.8% are in a formal apprenticeship position at midline. Similarly, of the 22.5% of control youth who enter a traditional apprenticeship position, 16.1% are still in an apprenticeship position at the moment of the survey. The drop-out rate is similar in formal and traditional apprenticeship (32.4% and 28.4%, respectively).

effect is small and not statistically significant. The results are robust to multiple hypothesis corrections. Asymptotic results and results from permutation tests are also similar.[30]

As explained in Section 4.1, our firm survey asked employers to list all apprentices who have worked in the firm over the course of the experiment, including those who left the firm since randomization. We can thus measure flows of apprentices into and out of firms between the start of the experiment and the midline. Table 2 (lower panel) documents net program impacts on the number of apprentices who entered since randomization. Results show that there are 1.080 additional apprentices per firm who entered since the date of randomization and 1.398 additional formal apprentices per firm.[31] Over the course of the experiment, the estimated crowding-out effect shows that there are 0.318 fewer traditional apprentices in firms. This is a crowding-out effect of small magnitude and is statistically significant at the 10% level.[32]

In this section, we have documented an increase in the entry of apprentices over the course of the experiment and in the number of apprentices that remained in firms at midline. We obtain these results based on rich data listing all apprentices. The midline firm survey also provides information about the total workforce in firms, including apprentices and other types of employees. These data simply count employees, ignoring their date of entry. Table A11 presents the estimated impact on those variables aggregated at the firm level. 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 midline. The estimated impact is 0.464, with a standard error of 0.362. The impact on flows is not large enough to affect stocks significantly, which may, in part, be due to large standard errors in the stock variables.[33]

[30]Table A10 presents the results from permutation tests (bottom panel, first three columns).

[31]Figure A3 provides additional information about the inflow of apprentices into firms. The figure shows monthly inflows of formal and traditional apprentices into treated and control firms by date (with zero being the randomization date). The figure clearly shows a spike in the entry of formal apprentices into treated firms shortly after randomization.

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

[33]For completeness, the impact of 0.464 on the stock of apprentices combines the previous impact of 0.613 on the total number of apprentices who entered over the course of the experiment and are still in firms at midline 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). While there is a negative effect on the retention of pre-program apprentices, it is not significant.

5.3 Net Impact on Apprenticeship Positions Filled

So far, we have discussed ITT estimates of offering youth the opportunity to enter formal apprenticeship and of assigning formal apprentices to firms with open positions. The results show that there is a statistically significant, but small, windfall effect for youth and a limited crowding-out effect in firms. The size of these effects implies that the net number of positions filled by the program is smaller than the number of formal apprentices placed. Consistent with the conceptual framework in Section 3, we now discuss the overall impact of the intervention on the net number of apprenticeship positions filled, and we show that the program expanded the number of filled apprenticeship positions. As mentioned in Section 3 and detailed in Annex Section 12.3.2, we estimate the two parameters ω and ψ using instrumental variables. We then show how the windfall and crowding-out effects combine.

5.3.1 Instrumental Variable Estimation of ω and ψ

We first consider participation in traditional or formal apprenticeship since the start of the experiment. Columns (a) and (b) of Table 3 contain the reduced form ITT estimates presented above. Column (c) presents instrumental variable (IV) estimates for $(1-\omega)$ and $(1-\psi)$, which are the ratios of the first two columns. The last column provides the estimated crowding-out and windfall parameters, ψ and ω. The estimated windfall parameter for youth is 0.259, with a standard error of 0.022. On the firm side, there are 0.773 youth entering firms per formal apprentice placed, thus leading to an estimated crowding-out parameter of 0.227, with a standard error of 0.128.

Table 3 estimates the windfall and crowding-out parameters using the inflow of apprentices since the start of the experiment, including those who dropped out, with the first-stage estimates corresponding to the lower panel of Table 2. We could instead have used the upper panel, which focuses on youth who are still apprentices at midline. Importantly, the estimated ω and ψ would have been very close, with $\omega = 0.125/0.490 = 0.255$ and $\psi = 0.174/0.787 = 0.221$. This highlights the robustness of the estimated windfall and crowding-out effects.

These findings have important implications. 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. We cannot determine the weights precisely because they are a function of demand

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

$$\frac{A_s\psi + A_d\omega}{A_s + A_d} \in [min(\psi, \omega), max(\psi, \omega)] = [0.227, 0.259]\,. \tag{9}$$

Since the two parameters, ψ and ω, are very close, the interval is narrow. We thus estimate the net number of apprenticeship positions filled by the program to be between 74.1% and 77.3% of the number of formal apprentices placed.[34] While these results confirm that there are crowding-out and windfall effects, the effects are small. Overall, the subsidized dual apprenticeship program does expand access to apprenticeship by increasing the net number of filled apprenticeship positions.

Furthermore, we can obtain confidence intervals for partially identified parameters (Imbens and Manski, 2004). The 95% confidence interval is $[-0.0098, 0.2997]$,[35] meaning that the net number of positions filled by the program is between 70.3% and 101% of the number of formal apprentices placed.

5.3.2 Market Tightness Adjustment and Potential Interference

The scope for interference between treated and control units depends on potential changes in market conditions that may affect control youth or firms. We can assess the magnitude of market tightness adjustments based on the results. As shown in Equation 4, the order of magnitude of the change in market tightness is $\sigma(\omega - \psi)$, where σ is the size of the experiment. The adjustment thus depends on the difference between the two estimated parameters ψ and ω. In our case, the difference is 0.032. From Table 1, the size of the experiment lies between 0.11 (youth side) and 0.22 (firm side). Using the more conservative 0.22, the order of magnitude of the change in θ is 0.007. Moreover, as emphasized in Section 3, we can compute upper bounds for the bias in ITT parameters. The bounds are 0.006 for youth entry into apprenticeship and 0.011 for firms' hiring of new apprentices.[36] Compared

[34] As was the case for the calculation of the windfall and crowding-out effects, rather than estimating the net number of apprenticeship positions filled by the program over the course of the experiment, we could have estimated the net number of apprenticeship positions filled at midline. In this case, with an estimated crowding-out effect of 0.221 and an estimated windfall effect of 0.255, we estimate the net number of positions filled by the program to be between 74.5% and 77.9% of the number of formal apprentices placed.

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

[36] These are $\sigma|\omega - \psi|\lambda_f$ and $\sigma|\omega - \psi|n_f$, which we can compute using estimates of λ_f (0.263+0.528=0.791) and n_f (0.188+1.398=1.586) from the bottom panel of Table 2 (bottom panel, columns (3) and (4)).

to the ITT estimates in Table 2 (0.528 and 1.080, respectively), the bias is very small. This means control firms and youth are unlikely to be affected by spillovers due to changes in market conditions.

5.3.3 Results by Saturation Level

Treated youth represent, on average, 11% of entrants into apprenticeship in program localities, and treated firms represent 22% of firms with apprenticeship positions. Table 1 shows that there is variation in the size of the experiment between localities. We can explore whether the estimated parameters ω and ψ vary between areas with high or low saturation. We do so by identifying localities that are above the median saturation level based on the size of the experiment for either youth or firms.[37] We caution that this analysis is not causal as the variation in σ is not random and is based on a small number of localities.

Table A7 shows estimates of ω and ψ separately for localities with high and low saturation based on the share of treated youth (column (1) of Table 1), and Table A8 shows estimates based on the share of treated firms (column (2) of Table 1). The estimates of ω are very similar for localities with low or high shares of treated youth or firms. This suggests that the windfall effect varies little with changes in saturation levels. Importantly, however, the estimates of ψ are very close to zero for localities with a low share of treated youth or firms. Hence, the limited crowding-out effect observed in firms stems from localities with a higher share of treated youth or firms. This suggests that, as σ increases, the scope of the crowding-out effect in firms may also increase.

Using a similar approach as in Section 5.3.2, we can assess the tightness adjustment in localities with high and low saturation. Considering an experiment size of 0.1 in low-saturation localities and 0.3 in high-saturation localities, we get tightness adjustments of $+0.03$ and -0.05, respectively. The upper bounds for the bias in the ITT parameters are 0.025 for the estimate of youth entry into apprenticeship and 0.045 for firms' hiring of new apprentices in low-saturation localities and 0.04 and 0.075, respectively, in high-saturation localities. Although slightly larger than in Section 5.3.2, the bias remains small relative to the ITT estimates, which are in the order of 0.5 for youth entry into apprenticeship and 0.8 for firms' hiring of apprentices.

[37] For youth, those localities are Adzope, Mankono and Daoukro. For firms, they are Man, Mankono and Daoukro.

Last, another way to assess whether equilibrium effects impact the control group is to analyze whether control youth's entry into apprenticeship and control firms' hiring of traditional apprentices vary between high- and low-saturation localities.[38] Since the tightness adjustment is positive in low-saturation localities and negative in high-saturation localities, in the presence of interference we would expect lower youth entry rates and higher firm hiring rates in high-saturation localities than in low-saturation localities. Table A9 shows that most differences are not significant when controlling for covariates, and even when significant, they have the opposite sign to the effects that could be explained by tightness adjustments.[39] We conclude that these effects are the result of pre-existing differences between localities with high and low saturation. Consistent with the analysis above, we do not find evidence of significant equilibrium effects that impact the control group.

6 Short- and Medium-Term Impacts on Youth Earnings

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

6.1 Short-Term Impacts on Youth Employment and Earnings

We first analyze the program's short-term impacts on youth employment, activities, and earnings—that is, impacts while apprentices were still in the program. Results show sub-

[38] This is in the same spirit as Crépon et al. (2013), although in our case we only have seven localities and the saturation intensity is not randomized.

[39] Specifically, when we define saturation on the youth side, we observe no difference for control youth and a negative effect on control firms' hiring of apprentices, which becomes insignificant when we control for covariates. When we define saturation on the firm side, we observe higher control youth entry into apprenticeship but no difference in control firms' hiring of apprentices.

stantial opportunity costs for youth to participate in apprenticeship. Table 4 documents ITT estimates (Equation 7) for employment, hours worked, and earnings by type of occupation.

The upper panel presents results on youth employment. Control youth are mostly active, as 91% are engaged in some economic activity, and the average number of activities is larger than one, indicating that some youth engage in several activities. The program only induces small increases in participation (by 3.4 percentage points) and in the average number of activities (by 0.05). However, youth reorganize their portfolio of activities and forgo some employment opportunities to enter apprenticeship. Specifically, individuals in the treatment group are less likely to hold wage jobs (by 13.5 percentage points) or to be self-employed (by 7.2 percentage points), and they are more likely to be apprentices (by 36.5 percentage points).[40] The middle panel shows similar effects for hours worked per week. Total hours marginally increase (+3.7). The increase in hours worked as apprentices (+18.2) is offset by a decrease in hours worked in wage employment (−6.5) and in self-employment (−4.4).

The bottom panel presents estimates of the program's impact on total (monthly) earnings, earnings by type of occupation, and non-labor earnings. Overall, the program has no short-term impact on average earnings for youth. Labor earnings decrease by CFAF 10,494 (25%), while non-labor earnings increase by CFAF 10,213 (135%). The decrease in labor earnings is driven by earnings in wage employment (−CFAF 6,414) and self-employment (−CFAF 4,157), which is only partly offset by an increase in apprenticeship earnings paid by employers (+CFAF 3,238). The program subsidy, paid by the implementing agency (and not the firm) directly to youth, is included in non-labor income. The increase in non-labor earnings in the treatment group (+CFAF 10,213) is driven by the subsidy.[41] It is thus only after accounting for the program subsidy that forgone labor earnings are fully compensated. Overall, although the total number of hours worked increases slightly, employment earnings decrease, and total earnings remain stable.[42]

We can compute the difference in monthly labor earnings between apprentices in the con-

[40]Bertrand et al. (2021) also find that the employment impacts of a public works program in Côte d'Ivoire take the form of a reorganization of economic activities.

[41]Non-labor income is built from three sources: "Stipends," "Remittances," and "Other sources of income." While the estimated effect on stipends is large and significant (CFAF 10,672), no effect is detected on the two other variables. They, however, account for most of the mean in the control group, with the mean stipend being only CFAF 458.

[42]The top panel of Table A10 presents the results from permutation tests for the ITT estimates on total hours worked per week and total earnings (last two columns). The asymptotic results and the results from permutation tests are very close.

trol group (4,746 / 0.18 = CFAF 26,367) and treatment group ((4,746+3,238)/(0.18+0.365) = CFAF 14,650). Interestingly, the average labor earnings of apprentices in the treatment group is lower. This illustrates how the subsidy changes the structure of payments made by employers to apprentices: the subsidy leads employers to pay formal apprentices less. This is because the stipend paid by the program to formal apprentices is labeled for food and transportation, which are usually covered by the employer and represent a substantial share of traditional apprentices' income. The difference in average earnings represents 39% of the stipend ($(26,367 - 14,650)$ / 30,000).[43] The associated amount is small, representing less than 3% of firm revenues.

Overall, the opportunity costs of participating in apprenticeship are quite high in the short term. Individuals forgo earnings in wage jobs and self-employment, although the program subsidy contributes to balancing the costs of participating in apprenticeship. Section 7.1 further analyzes whether the subsidy helps youth overcome financial constraints to participation.

During the program, the estimated average treatment effect on earnings of offering participation in formal apprenticeship is close to zero. However, consistent with our framework, we expect some heterogeneity in impacts on earnings due to variations in the employment situation of participants absent the program. For some youth with limited outside opportunities, participation in formal apprenticeship might lead to an increase in earnings—for example, because of the subsidy. However, for other youth with better opportunities, the impact on earnings might be smaller, possibly even negative, if gains expected in the future are large. The left panel of Figure 2 presents estimates of the cumulative distribution of hours and earnings during the program in the treatment and control groups. The cumulative distribution of earnings in the treatment group is first below, and then above, the cumulative distribution in the control group, meaning that there is no stochastic dominance of one distribution over the other. This is important because it implies that at least some youth who enter formal apprenticeships see a reduction in earnings compared to what they would have earned absent the program. This, in turn, implies that these youth expect an increase in future earnings due to the formal apprenticeship.

To formally test the null hypothesis of the same distribution of potential outcomes, we

[43]Note that this estimate represents an upper bound for the share of the subsidy absorbed by firms as it does not account for the fact that formal apprentices pay lower fees to firms.

implement Mann-Whitney ranksum tests of the same average rank of observations in the treatment and control groups. This test has the advantage of being robust to outliers. We present exact p-values obtained using permutation tests with 10,000 permutations. Results are shown below in Figure 2. The null hypothesis is clearly rejected.

6.2 Medium-Term Impacts on Youth Employment and Earnings

We now turn to analyzing the program's impacts on youth employment and earnings four years after the start of the experiment (Table 5). As in the midline, almost all individuals in the control group (98%) work at endline. The intervention thus has only a small impact on the share of individuals employed (+1 percentage point) and on the number of activities youth are engaged in (+0.08). The share of youth engaged in wage employment does not change, but a small increase in the share of self-employed individuals (+4.4 percentage points) and the share of youth employed as apprentices (+9.4 percentage points) is observed. While youth in the treatment group have exited formal apprenticeship positions as the program ended and their contracts expired, approximately 26.6% remain employed as (traditional) apprentices in firms. These findings also show that a substantial share of youth remain in their occupation nearly two years after the end of the intervention.[44]

The results for time worked (Table 5, middle panel) show similar patterns as the results for participation. No effect is found on the total time worked. A small increase in the number of hours worked as an apprentice (+4.4) offsets a small decrease in the number of hours worked in wage employment (−2.6). Time worked in self-employment remains unchanged, however.

Crucially, Table 5 (bottom panel) documents positive medium-term impacts on youth earnings. Total labor earnings increased by CFAF 8,987 per month (17%), and total earnings by CFAF 9,394 per month (15%). This is noteworthy since youth no longer receive any subsidy. Consistent with completion of the program, no impact is detected on non-labor earnings. Youth earnings in apprenticeship increased by CFAF 3,593, on average (62%). Earnings in self-employment increased by CFAF 4,512 on average, or 23%. Earnings in wage employment remained stable. These results show that, even as treated youth are slightly

[44]Note that the share of control youth in apprenticeship is stable between midline (17.9%) and endline (17.2%). In contrast, the share of treated youth in apprenticeship positions decreases from 54.4% to 26.6%. This indicates that the intervention did accelerate exits from apprenticeship positions, though not all treated youth fully exited.

more likely to remain apprentices, impacts on earnings are positive on average.

The right panel of Figure 2 presents the estimated cumulative distributions of medium-term hours and earnings. The proportion of youth who earn less than a given amount is uniformly and significantly lower in the treatment group than in the control group. This suggests robust positive program impacts on earnings across the distribution. The cumulative distribution function shows that the ITT estimate for earnings is not driven by outliers. Results also confirm the absence of an effect on hours.[45]

7 Mechanisms

The strength of our two-sided experiment is that it identifies both the windfall effect (ω) for youth and the crowding-out effect (ψ) in firms. Based on these two parameters, we can estimate the net increase in the number of apprentices. However, the experimental design does not directly identify the main constraints on youth and firms that the subsidized dual apprenticeship program relaxes in order to expand youth entry into apprenticeship. In this section, we explore the mechanisms through which the program led to small values for ω and ψ. We do so by analyzing the determinants of ω and ψ based on the conceptual framework.

We first study explanations for the low windfall effect (ω). The conceptual framework (Section 3 and Annex 12) highlight that $\omega = P(trad|reg)\lambda(\theta_1)/\lambda_f$. On the one hand, $P(trad|reg)$ captures the increase in youth demand for apprenticeship. The subsidized dual apprenticeship program was designed to address two main market failures potentially limiting youth entry into apprenticeship and returns to apprenticeship. First, the program provides a subsidy to address financial constraints. Second, the dual training component facilitates the acquisition of general skills and certification. These two features aim, respectively, to increase earnings during and after the training, which may contribute to an increase in youth demand for apprenticeship. On the other hand, $\lambda(\theta_1)/\lambda_f$ captures improved matching. The intervention may also increase youth participation in apprenticeship by making the matching process more efficient, and it may increase the likelihood of youth matching by inducing firms

[45] We implement Mann-Whithey rank sum tests and compute p-values using permutation tests with 10,000 replications. Out of 10,000 replications, the computed statistics are above the statistic obtained with the true assignment in only 2 cases for earnings. The results also confirm the absence of an effect on hours: out of 10,000 replications, the computed statistics are above the true statistic in 2344 cases. We also implemented permutation tests for the estimated ITT on total hours and earnings. Results are presented in Table A10. Asymptotic results and results from permutation tests are very close.

to create new positions solely for formal apprentices.

We start by assessing the role of increased demand in inducing a low ω. We find evidence consistent with the dual training component increasing post-intervention earnings and thus the attractiveness of apprenticeship (Sections 6.2 and 7.1), but not that the subsidy addresses access constraints among particularly vulnerable youth (Section 7.1). We also show that the intervention helps participants invest in their human capital and acquire skills, which is in line with the objectives of the dual training component (Sections 7.2 and 7.3). We further discuss that improvements in the matching process alone are unlikely to be the main mechanism behind the results as they are only consistent with a small increase in the proportion of youth in apprenticeship (Section 7.4).

We then explore mechanisms in firms to shed light on potential explanations for a low ψ. Firms do not appear to create new apprenticeship positions solely for formal apprentices, as they are willing to host apprentices without the intervention (Section 7.5). Firms provide comparable efforts to train traditional and formal apprentices, who are substitutes making similar total contributions to firm activities. We also show that the amount of the subsidy absorbed by firms is small. Another explanation for a low crowding-out of traditional apprentices (ψ) is that firms face a high cost to recruit additional apprentices, possibly due to their scarcity.

We conclude that the dual training component contributes to enhance the attractiveness of apprenticeship and increase youth demand (low ω), which is also compatible with firms facing high recruitment costs due to the scarcity of apprentices (low ψ).

7.1 What Drives Increased Youth Demand for Apprenticeship?

In this section, we explore explanations for the increase in youth demand for apprenticeship (related to $P(trad|reg)$). We assess the relative roles of the subsidy and the dual training component in making apprenticeship more attractive and explaining the low windfall effect (ω). To analyze the decision to enter apprenticeship, we distinguish between *always-takers*, *compliers*, and *never-takers* in relation to participation in apprenticeship. Always-takers enter an apprenticeship position even in the absence of the program, compliers only enter an apprenticeship position if one is offered by the program, and never-takers do not enter an

apprenticeship position.[46]

The subsidy component of the formal apprenticeship program aims to help youth who are more financially constrained, or from more vulnerable or lower-income families, enter apprenticeship. This is particularly relevant in a context where there are substantial forgone earnings, as documented in Section 6.1. We use youth baseline data to build an index of household assets as a proxy for wealth and an index of financial constraints.[47] We compare the distribution of these indices for two groups of youth entering apprenticeship across treatment and control, including compliers and always-takers. We perform a Kolmogorov-Smirnov test of the equality of the distributions. Figure 3 shows that there is no difference between the two distributions of each index. The two distributions are close to each other and within the confidence interval. This shows that the program did not facilitate the entry of youth facing greater financial constraints or from less-wealthy households. Table A15 tests the difference in average characteristics between compliers and always-takers. There is again no indication that the program facilitated access to youth from more disadvantaged socioeconomic backgrounds, as proxied by the asset and financial constraint indices.[48] Overall, we do not find evidence that the subsidy alleviates financial constraints among youth. Beyond proxies of financial constraints, we find that compliers are slightly more likely to be women, to aspire to wage jobs rather than self-employment, and to be more educated.[49]

Next, we assess the role of the dual training component in increasing the perceived quality of apprenticeship, which might trigger the decision of youth to participate in apprenticeship by making it more attractive, as shown in the conceptual framework. We compare the distribution of potential outcomes among non-apprentices for compliers (those who would have entered apprenticeship if offered) and never-takers for labor income at baseline, midline, and endline (Imbens and Rubin, 1997). We show that youth who self-select to participate in formal apprenticeship are those for whom future earnings would have been smaller absent the

[46]Never-takers may register for the program, but they do not enter a formal apprenticeship position once they learn about its true value. See Annex 12 for more details.

[47]We consider the number of self-reported constraints for basic expenditures, the lack of savings, the need to use savings to meet basic expenditures, the existence of debt, self-reported problems in paying back debt, and the lack of access to credit.

[48]There is also no significant difference in an index of exposure to crisis, an index of distance to infrastructure, or a ratio of employed household members.

[49] We find similar results when comparing the background characteristics of formal apprentices to traditional apprentices in treatment or control firms based on the firm midline survey and Equation 8. Table A17 shows that formal apprentices do not have significantly different socioeconomic backgrounds, but they are more educated and have higher aspirations for wage jobs.

program. Figure 4 shows no difference in the distributions of baseline and midline earnings between compliers and never-takers (top and bottom left panel, respectively). Specifically, the mean differences are not significant and are close to zero (CFAF $-6,288$ and CFAF $-1,998$, respectively).[50] On the other hand, the endline earnings distribution for never-takers dominates the distribution for compliers and is at the margin of the 95% confidence interval. The mean difference is larger (CFAF $-13,974$) than at midline, with a confidence interval from CFAF $-31,969$ to $4,021$.[51]

These results suggest that the decision to participate in apprenticeship depends on longer-term income prospects in the absence of the program. This is consistent with the dual training component making apprenticeship more attractive. We could have ruled out the role of the dual training component if only short-term earnings were correlated with the decision to participate, but this is not the case. Note that, while the subsidy does not relax financial constraints, it may still increase the value of apprenticeship and thus influence entry decisions by offsetting forgone earnings (see Annex 12).[52]

7.2 Do Youth Acquire Skills through Dual Apprenticeships?

The previous section showed that the formal apprenticeship program increased youth demand for apprenticeship by making it more attractive, but not because the subsidy alleviated financial constraints. We now analyze whether the intervention led youth to acquire skills, which is a key channel for dual apprenticeships to improve earnings and productivity.

Table 6 documents how the program affects skills through an index of routine task intensity. As mentioned in Section 4.1, we collected data on the complexity of tasks in the primary occupation at endline. For each individual, we build an overall index of routine task intensity (Table 6, column (1)) that aggregates subindices of routine tasks, non-routine analytical tasks, and non-routine interpersonal tasks (columns (2) to (4)).[53] Results show large

[50] The confidence interval is broader at baseline, which may be due to the reduced sample size for which baseline data are available.

[51] Table A16 compares compliers and never-takers. Only a few differences are borderline significant. They suggest that never-takers are closer than compliers to infrastructure, which could be associated with better access to economic opportunities, and lack assets, which could be needed to obtain returns to the training. Overall, however, potential differences in the earnings progression between the two groups is likely mostly due to unobserved factors.

[52] The subsidy may also help youth save to increase future earnings—see the increase in savings in Table A13.

[53] The overall index adds up the routine subindex and subtracts the non-routine (analytical and interper-

changes in the tasks performed by treated youth in their primary occupation. Specifically, youth assigned to formal apprenticeships are substantially more likely to undertake non-routine analytical tasks (+0.24 sd) and non-routine interpersonal tasks (+0.08 sd). They are also slightly more likely to perform routine tasks (+0.11 sd). The aggregate index of routine task intensity decreases by 0.21 sd. Treated youth are involved in a wider range of tasks, particularly more complex, non-routine tasks. This points to substantial improvements in skills consistent with the observed increase in earnings and productivity in the medium term.

Table A14 disaggregates impacts for all tasks of each subindex. Treated youth are significantly more likely to perform non-routine analytical tasks that involve reading, writing, taking measurements, performing math operations, or thinking. This explains the large impact on the subindex of non-routine analytical tasks. On the other hand, evidence of impacts on non-routine interpersonal tasks is weaker. The estimated coefficient for the subindex is small and only significant at the 10% level. Moreover, of the four tasks in the list, we detect a small, statistically significant impact on only one variable (advising co-workers). The impact on the routine task subindex is also small and only significant at the 10% level. We observe an increase in the use of heavy equipment and tasks related to repairing and maintaining electronic equipment but a reduction in repetitive tasks.

Our measures of skills based on the task approach are not occupation-specific. This allows us to compare the complexity of tasks performed by youth in the treatment and control groups even if they work in different sectors. We can also decompose how much of the changes in the complexity of tasks is driven by treated youth working in different occupations than control youth. We consider the average value of a given index I over youth in the control group in each occupation s: $\overline{I}_s^C$. The within-between decomposition writes $I_i = [I_i - \overline{I}_{s(i)}^C] + \overline{I}_{s(i)}^C = W_i + B_i$. Assessing impacts on W_i and B_i indicates whether observed changes in I are related to changes in occupations—for example, switching to occupations involving more abstract tasks (changes in B)—or whether they are related to changes in tasks performed within sectors (W). Table 6 (middle panel) shows that for each subindex, the observed changes in the upper panel are partly driven by changes in occupation. Thanks to the program, youth enter occupations involving more diverse tasks, including more

sonal) indices. Each subindex is built based on a principal component analysis of several questions. Each subindex is standardized based on the mean and standard deviations in the control group. Table A14 lists the items for each subindex.

routine, non-routine analytical, and non-routine interpersonal tasks. No impact is detected on the within component of routine tasks and non-routine interpersonal tasks. Importantly, however, there is also a significant increase in the within component of non-routine analytical tasks, showing that treated youth perform more of these tasks even within occupations. This explains the decrease in the aggregate routine task intensity index.

Overall, we find that youth assigned to subsidized dual apprenticeship positions are engaged in more complex tasks, even after accounting for occupational changes. Together with an increase in earnings, this is consistent with youth acquiring skills and becoming more productive. In the next section, we delve further into the various elements of the dual apprenticeship program that can contribute to improving skills.

7.3 Human Capital Investments, Dual Training, and Certification

The dual apprenticeship program includes several elements to facilitate skills acquisition—it combines on-the-job, practical training in firms with theoretical training in vocational centers. In this section, we document further how the program increases human capital investments, and we explore the role of dual training and certification.

Control youth do not participate in much training, and the program is a unique opportunity for many youth to invest in their human capital. The program clearly increases youth human capital investments through training. The midline shows a boost in entry into apprenticeship (Section 5.1). At endline, we collected retrospective data on all training activities begun since the start of the experiment (2014–2018).[54] Table 7 (top panel) shows that the program strongly increases participation in training. The share of youth participating in any type of training activity increases by 47.7 percentage points, from 24.9% to 72.6% (column (1)).[55] On average, treated youth participate in 10.8 additional months of training, a large increase since control youth only spend 3.2 months in training on average (column (3)).[56] In line with the program's intent to facilitate access, higher participation does not come at a higher cost (column (4)).

[54]For each training activity, the data include the start date, the type of training, and the end date.

[55]The number of training attended increases by 0.498 (column (2)), from 0.287 in the control group. Few youth have more than one training spell.

[56]The training activities treated youth participate in are also longer by approximately 6.5 months. In the control group, conditional on participation, youth spend $3.195/0.249 = 12.8$ months in training, while in the treatment group, the length of training is on average $(10.8 + 3.195)/(0.477 + 0.249) = 19.3$ months.

Training participation can be decomposed between solely firm-based training, dual training (firm-based training combined with training in vocational centers), and training in vocational centers only. Table 7 (middle panel) shows that, as expected, the program has no impact on participation in training based in vocational centers only. On the other hand, the number of solely firm-based training activities increases by 0.205, and the number of dual training activities by 0.268. These results also show that not all treated youth who received training in firms participated in the theoretical training, partly because it was provided toward the end of the program and some youth had dropped out before then.[57]

Control youth do not catch up after the program. Table 7 (middle panel, column (4)) shows that the share of youth who started a training in 2017–2018 is very low, and the treatment effect is zero. The program thus does not solely shift training participation over time; it truly increases youth human capital investments.

Last, the program establishes procedures to certify skills. The intervention leads to more youth obtaining certification (+14.7 percentage points, from 11.1% in the control group). Almost all of this increase is driven by increases in formal government certification (+16.4 points), which is rare in the control group (3.2%). This is noteworthy because formal certification can help youth signal general skills and thus facilitate their mobility in the labor market, in particular to access better wage jobs. Still, the increase in the share of youth with certification is small in absolute terms. Less than half of treatment apprentices who completed dual apprenticeships obtained a formal certificate.[58] In part, this may be due to implementation delays in the setup of the certification mechanisms. It is also consistent with some treated youth remaining in firms as apprentices after the end of the program.

To assess the role of dual training and certification, we examine whether participating in dual training and certification correlates with income and routine task intensity for treated youth. The results are presented in Figure 6.[59] In the treatment group, compliers who were

[57]Based on the data, we see that treated youth report participating in 0.085 + 0.205 = 0.29 solely firm-based training activities and 0.083 + 0.268 = 0.351 dual training activities. This suggests that about half of treated youth who started formal apprenticeships received theoretical training.

[58]Specifically, 0.147 + 0.111 = 25.8% of treated youth received certification, less than half of the 72.6% of treated youth who took any training.

[59]In the figure, the width of the bars represents the size of the related populations. To learn more about these populations, Table A18 documents baseline characteristics among treated youth, based on whether they completed dual training and certification, using the same variables as in Table A17. There are very few differences that are statistically significant between groups. The implementation of the dual training (and subsequent certification) was delayed by the preparation of the curriculum and procurement of vocational training centers. As such, most of the training took place in blocks during the program's second year.

dually trained and certified have higher mean earnings and lower routine task intensity than other apprentices and never-takers. This suggests that the main impacts on earnings and technical skills are driven by apprentices who were dually trained and certified.

Our results are consistent with the increase in earnings reflecting an increase in skills and productivity in current youth activities, rather than reflecting higher labor market mobility and easier access to new wage jobs for trained youth (as in Alfonsi et al., 2020). Table 5 shows that the program does not increase entry into wage employment, for which certification may be most needed. Figure 5 shows the number of employment spells (counting the number of activities started after the program) in various occupations for treated and control youth based on a retrospective employment calendar. The number of post-program employment spells is very similar in treatment and control. If anything, treated youth have fewer employment spells in wage jobs and self-employment. This suggests that the intervention did not facilitate access to new wage jobs. Instead, recall that we also observe increases in skills and productivity within the occupations in which youth are employed. Table 5 shows that impacts on endline earnings are partly driven by increases in self-employment earnings, despite time worked in self-employment remaining the same. Overall, this suggests that dual training contributes to increasing youth productivity in self-employment. This mechanism differs from those discussed in previous literature, which focuses on facilitating access to wage jobs (e.g., Alfonsi et al., 2020; Attanasio et al., 2017). This may, in part, reflect the largely informal setting in which our experiment takes place.

7.4 Contribution of Increased Demand and Improved Matching

So far, we have highlighted the role of higher demand from youth in increasing youth entry into apprenticeship. We have focused on apprenticeships becoming more attractive, a channel consistent with the documented increases in earnings and skills in the medium term. The framework shows that $\omega = P(trad|reg)\lambda(\theta_1)/\lambda_f$. A lower ratio $P(trad|reg)$ indicates a larger effect of increased demand. In other words, $1/P(trad|reg)$ captures the magnitude of the demand effect. We now consider another potential explanation for ω being low: the intervention may improve matching by connecting youth interested in apprenticeship with

Whether or not an apprentice participated in dual training activities may have been less a decision on their part than a function of implementation speed across localities and trades.

firms. In other words, rather than $P(trad|reg)$ being small, an alternative reason for ω being low would be that $\lambda_f/\lambda(\theta_1)$ is large.

The demand for apprenticeship in the control group $P(trad|reg)$ at midline includes the share of control youth in an apprenticeship position and searching for an apprenticeship position. Participation in apprenticeship is 26% in the control group (Table 2). To estimate the share of control youth searching for an apprenticeship position, we note that 21% search for a job (including an apprenticeship) at midline. We consider various scenarios by assuming either that none of them, all of them, or 50% of them search for an apprenticeship position.[60] Based on these estimates (column (2)), Table 8 (column (3)) displays the estimated share of control youth either in apprenticeship or searching for an apprenticeship ($P(trad|reg)$). The program increases the demand for apprenticeship by up to a factor of four (1/0.26), or 2.7 (1/0.37) in the intermediate scenario. Even in the most conservative scenario, the program increases the share of youth searching for apprenticeship by a factor of two (1/0.48). The increase in youth demand for apprenticeship is thus an important driver of the increase in apprenticeship entry.

Column (4) shows the estimated ratio $\lambda_f/\lambda(\theta_1)$, which ranges from 0.78 to 1.44.[61] We can contrast these results with Hardy et al. (2019), who study an intervention in Ghana aimed at improving the matching between potential apprentices and host firms, with no change in the features of apprenticeships (hence, where we can assume that $P(trad|reg) = 1$). Hardy et al. (2019) find an increase in apprenticeship participation of 13 percentage points—much smaller than the effect we find on youth entry. In their study, the ratio of participation in the treated group (0.75) relative to the control group (0.62) is 1.2, which provides a benchmark estimate for $\lambda_f/\lambda(\theta_1)$. Our intermediate case indicates a ratio of 1.11, which points to improvements in the matching process of similar magnitude to Hardy et al. (2019),

[60] Based on the retrospective employment calendar from the endline survey, the share of control youth starting an apprenticeship after the program is 0.11. We can use this as a proxy by assuming that all youth who search for an apprenticeship position would find one between midline and endline. This is the same estimate we obtain when assuming that 50% of control youth who search for a job search for an apprenticeship position.

[61] To perform this analysis, we consider ω to be the ratio of participation in apprenticeship in the treatment and control groups (from column (3) of the bottom panel of Table 2, this is $0.263/(0.263 + 0.528) = 0.333$). In Table 3, the parameter ω is presented as 1 minus the IV estimate of participation in apprenticeship on participation in formal apprenticeship. These two quantities are the same under perfect compliance, but the small non-compliance documented in Section 5.2 makes them slightly different. We use this approach to be consistent with the computation of $P(trad|reg)$, which uses the share of apprentices in the control group (0.263).

but which is alone insufficient to explain the much larger effect on youth entry that we find in our context. Importantly, even in the most conservative scenario, the ratios in column (4) are not much larger than those in Hardy et al. (2019). These results clearly show that increased demand contributes much more strongly than improved matching to explaining the low value of ω and the large increase in the share of youth in apprenticeship positions.[62]

7.5 Firms' Willingness to Open Apprenticeship Positions

There is another possible mechanism by which improved matching may drive the results: if there is no impact on youth demand for apprenticeship ($P(trad|reg) = 1$), the ratio $\lambda_f/\lambda(\theta_1) = 1/\omega$ would need to be large for ω to be low. This could occur if the subsidized dual apprenticeship program led firms to open new positions specifically for formal apprentices (large λ_f), allowing more treated youth to match and enter apprenticeship positions. We now discuss whether the intervention changes firms' willingness to open apprenticeship positions.

To study this, we analyze mechanisms in firms and the potential firm-side constraints that the intervention may address to induce ω and ψ to be low. We examine different reasons why firms might be reluctant to open apprenticeship positions or train traditional apprentices. We show that firms do not appear to create new apprenticeship positions solely for formal apprentices, given their willingness to host apprentices in the absence of the intervention. We also highlight that firms make a comparable effort to train traditional and formal apprentices and that formal and traditional apprentices are substitutes making similar total contributions to firm activities. We further show that the amount of the subsidy absorbed by firms is small. This rules out firm-side constraints related to the cost of training apprentices (or other financial constraints), though the results remain compatible with firms facing high recruitment costs for additional apprentices.

First, recall that apprenticeship is very common in small, informal firms in Côte d'Ivoire. Of our sample firms, 80% host apprentices at midline, and the intervention has only a small impact in reducing the share of firms without apprentices (by 5.4 percentage points; see Table A11, last column). Even in a favorable context, with the program, 15% of firms had

[62]The last column shows the implied value of $\lambda(\theta_1)$. In the intermediate case, we find $0.26/0.37 = 0.70$, which is slightly higher than the matching rate of 0.62 found in the control group in Hardy et al. (2019). The estimated values of $\lambda(\theta_1)$ are reassuring as they rule out the possibility that the ratio $\lambda_f/\lambda(\theta_1)$ is high because of a decrease in the matching rate in the control group due to an equilibrium effect reducing the number of available positions.

open apprenticeship positions that they could not fill. Apprentices also make important contributions to firm activities. There are 3.38 apprentices per firm at baseline, representing over half of the total workers (6.36) (Table A4). The estimated production function in control firms at midline (Table A5) highlights that apprentices have a production elasticity of 0.343, similar to regular workers (0.372) and higher than casual workers (0.197).[63] Apprentices are thus an important source of labor, and it is very common for firms to open apprenticeship positions.

Second, firms may be reluctant to take on traditional apprentices because of high training costs or constraints in the number of apprentices they can train. For example, in Colombia, Caicedo et al. (2022) show that firms in high-skill sectors are particularly unwilling to employ apprentices, while the majority of firms in low-skill sectors recruit as many apprentices as possible.[64] However, dual apprenticeships may allow firms to open new positions specifically for formal apprentices by outsourcing more of the training to vocational training centers. In our data, however, we do not see that firms exert less effort to train either formal or traditional apprentices. We compare the training of the two types of apprentice by using our midline employer-employee data to estimate Equation 8. Table 9 (panel 1) shows that formal and traditional apprentices spend the same amount of time working under the mastercraftsperson's supervision (2.6 hours per day).[65] The time spent by apprentices working under the supervision of the mastercraftsperson can be costly for the firm, but the mastercraftsperson's level of effort is similar for formal and traditional apprentices. We thus do not find evidence that the program releases constraints on the capacity of firms to train apprentices.

Third, firms may absorb part of the subsidy, which could provide them with the cash needed to finance apprentices, especially when they are new and not very productive. Table 9 (panel 3) shows that formal apprentices are paid less than traditional apprentices. Firms make lower payments to formal apprentices for transportation, room, and food, which the program subsidy contributes to. Formal apprentices still receive similar "bonus" payments

[63]To estimate this production function, we follow the approach of Olley and Pakes (1996) to deal with fixed effects and simultaneity by also introducing control variables such as the baseline number of each type of employee.

[64]This is largely consistent with the central role of apprentices in the production process of small, informal firms in Côte d'Ivoire, as mentioned above.

[65]If anything, formal apprentices spend more time working autonomously and less time watching the mastercraftsperson than traditional apprentices.

to traditional apprentices in treatment firms. This shows that firms complement the subsidy offered by the program to motivate apprentices directly. The total monthly payments (wage bill) made by firms per formal apprentice tends to be lower than the payments made per traditional apprentice (by 18%, though not statistically significant with a p-value of 0.20) despite differences in daily productivity. However, formal apprentices pay significantly lower fees to firms than traditional apprentices in both treatment and control firms. This is consistent with the program's intent to subsidize access. Over the 24-month duration of an apprenticeship, the reduction in monthly payments to apprentices outweighs the reduction in fees, but firms only absorb approximately 5% of the subsidy.[66] The fact that the amount of the subsidy absorbed by the firm is small suggests that financial constraints were not the main reason they did not host additional apprentices in the absence of the intervention.

Fourth, formal apprentices provide a similar total contribution to firm activities as traditional apprentices. This suggests that formal apprentices and traditional apprentices are substitutes, and formal apprentices do not fill positions that are opened solely for them. Table 9 (panel 3) shows how mastercraftspeople rate their apprentices' technical skills.[67] They rate formal apprentices as having higher technical skills than traditional apprentices, which is consistent with dual training facilitating skills acquisition and increasing the complexity of tasks undertaken by apprentices (as in Section 7.2). Importantly, however, employers rate formal apprentices as having lower behavioral skills,[68] including higher absenteeism and tardiness.[69] At midline, we asked firm owners to recall the work performed by each ap-

[66]While training fees are CFAF 21,024 for traditional apprentices up to the survey, they are CFAF 20,536 lower, and almost zero, for formal apprentices. The reduction of monthly payments to apprentices (2,413) minus the monthly fee reduction over the course of the apprenticeship (20,536/24) equals 1,557. This provides another estimate of the share of the subsidy absorbed by the firm: $1,557/30,000 = 5.2\%$. The amount represents less than 0.5% of firm revenues. Note that this difference is based on a comparison of the population of traditional and formal apprentices in firms (reported by the employer) and is smaller than the difference based on the comparison of apprentices in the treatment and control groups (Section 5.3.2). Both comparisons may involve selection effects of different sorts. However, in both cases the amount of the subsidy captured by firms is small.

[67]Technical skills are assessed through a set of questions directed 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 two to seven 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.

[68]The behavioral skills index averages several questions directed to the employer about each apprentice regarding their attitude at work, including absenteeism, punctuality, respect for clients and boss, seriousness, and motivation.

[69]In fact, formal apprentices work seven fewer days per month (35% less) than traditional apprentices (panel 3), which cannot be explained by participation in vocational training only.

prentice during their last working day and to estimate how much they would have had to pay an occasional worker to accomplish the same tasks.[70] Table 9 (panel 3) shows that the program leads to a strongly positive and significant increase in this measure of the (daily) value of the work performed by formal apprentices compared to traditional apprentices. This is consistent with increases in technical skills and productivity. However, since formal apprentices have higher absenteeism and work fewer days, there is no difference in the value of work per month (i.e., their total contribution to the firm activities) compared to traditional apprentices.

Overall, formal apprentices appear similar to traditional apprentices in terms of the training they receive from firms and their total contribution to firm activities. On this basis, it does not appear likely that the formal apprenticeship program leads to a low ω by inducing firms to open new apprenticeship positions to be filled solely by formal apprentices (which would imply a large λ_f).

These results on firm-side mechanisms can also help explain why ψ is low. The results are consistent with a situation in which firms are limited in their ability to fill apprenticeship positions due to difficulty in recruiting additional apprentices, rather than the profitability of these apprentices (excluding recruitment costs). Difficulty in recruiting traditional apprentices is compatible with apprentices being relatively scarce. Recall that the framework and Annex 12.3 show that $\psi = -af''/(c'' - f'')$, with f the production function, c the recruitment cost, and a the relative contribution to the production of formal versus traditional apprentices. Based on the analysis above, traditional and formal apprentices make similar total contributions to firm activities: thus, the parameter a is close to 1. A low ψ in this framework is possible if the second derivative of the cost function is large compared to the second derivative of the production function. This means that the number of apprentices employed in firms is more sensitive to constraints on the number of apprentices that the firm can recruit than constraints on the number of apprentices that the firm can train or employ in profitable tasks.

[70] See Appendix A3. Although this measure has obvious limitations, it is close in spirit to the one used in the studies that have performed cost-benefit analyses of apprenticeship for firms in Europe. These studies are based on surveys in which employers are asked how much time each apprentice spends on productive tasks. This time is then valued using the wage of an unskilled worker (see Acemoglu and Pischke, 1999a). Importantly, this variable is used to make comparisons between apprentices, hence we only need it to be a good correlate of a true productivity measure.

Overall, this section has shown that the main channel through which the intervention increases the number of apprentices is through an increase in youth demand. An improvement in matching between apprentices and firms also contributes to the observed increase, as in Hardy et al. (2019), but it induces a much smaller effect. It is also unlikely that the program led firms to open apprenticeship positions solely for formal apprentices. These results are consistent with firms facing capacity constraints related to the number of apprentices they can recruit.

8 Cost-Effectiveness and Scale-Up Potential

8.1 Program Costs and Benefits

Table 10 provides cost-benefit calculations by comparing program costs to impacts on youth earnings. The estimated cost of a 24-month apprenticeship is CFAF 1,135,030 (approximately US$2,045). This includes CFAF 720,000 (US$1,297) for subsidies, CFAF 330,000 (US$595) for other direct costs (theoretical training, tools, equipment, etc.), and CFAF 85,030 (US$153) for indirect costs (selection, monitoring, etc.). Taking into account the time when youth drop out, the average cost per sample youth is CFAF 654,817 (panel A). The subsidy accounts for 70% of total costs. The training in vocational centers represents less than 10% of the costs.

Panel B presents impacts per individual based on the ITT estimates in Section 6.2. We consider alternative hypotheses about the dissipation of impacts on earnings after the endline survey, including no dissipation and 5%–15% yearly dissipation. Panel D displays internal rates of return. While the subsidized dual apprenticeship program would not be cost-effective if impacts fully dissipated immediately after the endline survey, the rates of return become positive if impacts are partly sustained thereafter. The internal rate of return reaches 3.4% with an annual dissipation of 15%, and 6.6% and 10% for an annual dissipation of 10% and 5%, respectively. The program would become cost-effective after eight years under the 5% yearly dissipation scenario.[71]

[71]Section 7.5 documents the differences in monthly payments, training fees, and the net value of work between traditional and formal apprentices in firms. When aggregating these effects at the firm level (Table A19), there is a substantial increase in the value of the work provided by apprentices net of their compensation and fees paid during the program. This is largely driven by the net inflow of apprentices in firms. Still,

8.2 Potential for Scale-Up

Our results show that the subsidized dual apprenticeship program increased the number of apprenticeship positions by raising youth demand for apprenticeship without creating a large windfall effect among youth or crowding-out effect in firms. Impacts on youth earnings are positive after four years, making the program cost-effective if effects partly sustain.

There are many paths to gradually scale up effective policies based on evidence (Banerjee et al., 2017). In our setting, the program could scale up in two dimensions under relatively weak assumptions. We consider the total number of additional apprenticeship positions in each scenario. First, at the average saturation level in the experiment, the program could expand to other urban localities in Côte d'Ivoire, as well as to other sectors, both within existing localities and in other localities. This would reach an additional 7,500 youth,[72] relying on the assumption that results have external validity in other urban localities and apprenticeship sectors. Second, program coverage could increase within localities, implying higher saturation. Section 5.3.3 shows that the windfall and crowding-out effects at the higher saturation levels in our sample remain moderate. These results support scaling up the intervention to reach an additional 15,000 youth by increasing saturation from the mean in our low-saturation localities to the mean in our high-saturation localities. In total, a partial scale-up could thus cover an additional 22,500 formal apprentices by expanding to urban areas nationwide and staying within the range of the saturation levels we observe in the sample. This scale would represent one-sixth of the total stock of traditional apprentices in the country. The net number of additional apprenticeship positions per program youth corresponds to $min(1-\omega, 1-\psi)$, for which a conservative estimate is 60%,[73] hence this would represent a net increase of $0.6 * 22,500 = 13,500$ additional apprenticeship positions,

we do not find an increase in firm profits in the short term (Table A20). Therefore, the increase in the value of work by apprentices may not be sufficient to increase profits. Youth indirectly compensate firms for providing training since their remuneration is lower than their productivity. Yet the provision of training also has an opportunity cost for firms, including the opportunity cost of supervisors' time. Finding zero impact on profits is consistent with these effects canceling each other out. Given these null results on firm profits, we focus the cost-benefit analysis on youth.

[72] We estimate that 6,136 youth enter traditional apprenticeship each year in study localities in any sector (column (5), Table A1), and 4,175 youth in the targeted sectors. The program could enroll approximately 450 additional youth by expanding to other sectors without changing saturation levels in each micromarket. More importantly, the program could expand to other cities. There are 1.75 million youth aged 15 to 24 in urban areas in Côte d'Ivoire, including 245,000 in study localities, and 1.505 million in other localities. The program could reach approximately 7,050 youth in these other localities by keeping a similar saturation level as in the experiment.

[73] 0.582 is the lowest estimate across Tables A7 and A8.

an increase by 10% of the total stock of apprentices. Note that this increase in the flow of new apprentices would compound over time and that some trained apprentices who become self-employed may in turn start training future apprentices. The analysis of scale-up trajectories could be the subject of future research.

Importantly, the intervention we study was implemented by a government agency in real conditions, such that an expansion could occur without concerns about worsening implementation quality. However, there are additional margins for scale-up for which our results may or may not generalize. First, coverage could increase above the levels of saturation in the sample (i.e., above 0.4). Recall that the crowding-out effect we observe in firms appears more sensitive to increases in saturation levels than the windfall effect among youth. It is thus possible that firm-side constraints may bind and accentuate crowding out if saturation reaches very high levels. Second, the apprenticeship program set a maximum of three apprentices per trainer. If this quota was relaxed, it is possible that some firms would reach their capacity constraints, also potentially inducing a larger crowding-out effect.

Our study results have been directly relevant to policy: they have informed the national apprenticeship strategy in Côte d'Ivoire, in particular a decision to gradually scale up the subsidized dual apprenticeship model to an additional 32,000 apprentices over several years.[74] Our results are also directly relevant for policy makers in other low-income and lower-middle-income countries who are rolling out apprenticeship programs of similar scale.[75]

9 Conclusion

Market failures can limit youth demand for apprenticeship, the quality of training apprenticeships provide, and their returns. We find that offering subsidized dual apprenticeships strongly increases youth demand and entry into apprenticeship positions. Using a double-sided experimental design, we show that the program fills 0.74–0.77 new positions per subsidized apprentice placed. This indicates that there is room to increase apprenticeship provision without displacing traditional apprentices.

Our results also point to the value of dual apprenticeships, which combine on-the-job and

[74] Given budget and operational constraints, many policies are scaled up progressively, such as in this case, with a first wave of 4,000, a second wave of 10,000, and a third wave of 18,000 apprentices.

[75] Examples include Benin, Nigeria, and Senegal, among others.

vocational training: treated youth have higher earnings by 15% two years after the end of the intervention. These results complement findings by Attanasio et al. (2011, 2017) on a program combining classroom and on-the-job training, as well as by Alfonsi et al. (2020) on vocational training, where training improves earnings and facilitates access to wage jobs. In our context, characterized by high informality, we do not find a higher likelihood of accessing wage jobs. Rather, dual apprenticeships improve skills, and youth perform more complex tasks, contributing to higher productivity and earnings in self-employment.

Last, we show that the dual training component enhances the attractiveness of apprenticeship, which is consistent with higher skills and earnings. The program also addresses matching inefficiencies (in line with Hardy and McCasland (2023)), but this induces a much smaller effect on entry than the increase in youth demand. We do not find evidence that the subsidy relaxes financial constraints, nor do we find evidence that firms are unwilling to host apprentices, which is consistent with the significant role of apprentices in micro- and small firms in Côte d'Ivoire and with firms' willingness to recruit apprentices in low-skill sectors in Caicedo et al. (2022). The low crowding-out effect in firms can be explained by high recruitment costs for additional apprentices, which is compatible with their relative scarcity and constraints on youth demand for apprenticeship.

Several questions about the optimal design of national apprenticeship systems remain for future research. The relative effectiveness of improving on-the-job training, center-based training, skills monitoring, and certification need to be isolated, especially as they are core elements of apprenticeship policy reforms. The long-term impacts of apprenticeship programs also remain to be assessed, which is critical for overall cost-effectiveness.

Our results have broad relevance in developing economies, where many jobs are concentrated in small, informal firms that have limited technology and may be unable to provide general skills training. In such settings, this study has important policy implications. It shows that dual apprenticeships can raise youth demand for training by increasing the attractiveness of apprenticeship, as well as contribute to small firm expansion and benefit youth over the medium term.

Data Availability Statement The code needed to reproduce the results in this article is available on Zenodo at https://zenodo.org/records/13153322 and the World Bank Reproducible Research Repository at https://doi.org/10.60572/hn8v-8x69. The data underlying this article are available in the World Bank Reproducible Research Repository, at https://doi.org/10.48529/766e-b621, https://doi.org/10.48529/w2sz-d257, and https://doi.org/10.48529/kfcz-6t55. DIME analytics verified the computational reproducibility of

the results.

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

References

Abadie, A. (2003): "Semiparametric Instrumental Variable Estimation of Treatment Response Models," Journal of Econometrics, 113, 231–263.

Abebe, G., A. S. Caria, M. Fafchamps, P. Falco, S. Franklin, and S. Quinn (2020): "Anonymity or Distance? Job Search and Labour Market Exclusion in a Growing African City," The Review of Economic Studies, 88, 1279–1310.

Abel, M., R. Burger, and P. Piraino (2020): "The Value of Reference Letters: Experimental Evidence from South Africa," American Economic Journal: Applied Economics, 12, 40–71.

Acemoglu, D. and D. Autor (2011): "Skills, Tasks and Technologies: Implications for Employment and Earnings," in Handbook of Labor Economics, ed. by O. Ashenfelter and D. Card, Elsevier, vol. 4b, 1043–1171.

Acemoglu, D. and J.-S. Pischke (1998): "Why Do Firms Train? Theory and Evidence," The Quarterly Journal of Economics, 113, 79–119.

——— (1999a): "Beyond Becker: Training in Imperfect Labour Markets," The Economic Journal, 109, 112–142.

——— (1999b): "The Structure of Wages and Investment in General Training," Journal of Political Economy, 107, 539–572.

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

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

Alfonsi, L., O. Bandiera, V. Bassi, R. Burgess, I. Rasul, M. Sulaiman, and A. Vitali (2020): "Tackling Youth Unemployment: Evidence from a Labor Market Experiment in Uganda," Econometrica, 88, 2369–2414.

Almeida, R., L. Orr, and D. Robalino (2014): "Wage Subsidies in Developing Countries as a Tool to Build Human Capital: Design and Implementation Issues," IZA Journal of Labor Policy, 3, 12.

Angelucci, M. and G. De Giorgi (2009): "Indirect Effects of an Aid Program: How Do Cash Transfers Affect Ineligibles' Consumption?" American Economic Review, 99, 486–508.

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

Attanasio, O., A. Kugler, and C. Meghir (2011): "Subsidizing Vocational Training for Disadvantaged Youth in Colombia: Evidence from a Randomized Trial," American Economic Journal: Applied Economics, 3, 188–220.

Autor, D. H. and M. J. Handel (2013): "Putting Tasks to the Test: Human Capital, Job Tasks, and Wages," Journal of Labor Economics, 31, S59–S96.

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

Babcock, L., W. J. Congdon, L. F. Katz, and S. Mullainathan (2012): "Notes on Behavioral Economics and Labor Market Policy," IZA Journal of Labor Policy, 1, 2.

Bandiera, O., V. Bassi, R. Burgess, I. Rasul, M. Sulaiman, and A. Vitali (forthcoming): "The Search for Good Jobs: Evidence from a Six-year Field Experiment in Uganda," Journal of Labor Economics.

Bandiera, O., R. Burgess, N. Das, S. Gulesci, I. Rasul, and M. Sulaiman (2017): "Labor Markets and Poverty in Village Economies," The Quarterly Journal of Economics, 132, 811–870.

Banerjee, A., R. Banerji, J. Berry, E. Duflo, H. Kannan, S. Mukerji, M. Shotland, and M. Walton (2017): "From Proof of Concept to Scalable Policies: Challenges and Solutions, with an Application," Journal of Economic Perspectives, 31, 73–102.

Barnow, B. S. and J. Smith (2016): "Employment and Training Programs," in Economics of Means-Tested Transfer Programs in the United States, Volume II, University of Chicago Press.

Bassi, V. and A. Nansamba (2022): "Screening and Signalling Non-cognitive Skills: Experimental Evidence from Uganda," The Economic Journal, 132, 471–511.

Becker, G. (1962): "Investment in Human Capital: A Theoretical Analysis," Journal of Political Economy, 70, 9–49.

Benjamini, Y. and Y. Hochberg (1995): "Controlling the False Discovery Rate: A Practical and Powerful Approach to Multiple Testing," Journal of the Royal Statistical Society: Series B (Methodological), 57, 289–300.

Bertrand, M., B. Crépon, A. Marguerie, and P. Premand (2021): "Do Workfare Programs Live up to Their Promises? Experimental Evidence from Côte D'Ivoire," Working Paper 28664, National Bureau of Economic Research.

Black, D. A., J. A. Smith, M. C. Berger, and B. J. Noel (2003): "Is the Threat of Reemployment Services More Effective than the Services Themselves? Evidence from Random Assignment in the UI System," American Economic Review, 93, 1313–1327.

Blattman, C. and S. Dercon (2018): "The Impacts of Industrial and Entrepreneurial Work on Income and Health: Experimental Evidence from Ethiopia," American Economic Journal: Applied Economics, 10, 1–38.

Breza, E., S. Kaur, and Y. Shamdasani (2021): "Labor Rationing," American Economic Review, 111, 3184–3224.

Caicedo, S., M. Espinosa, and A. Seibold (2022): "Unwilling to Train? Firm Responses to the Colombian Apprenticeship Regulation," Econometrica, 90, 507–550.

Card, D., J. Kluve, and A. Weber (2018): "What Works? A Meta Analysis of Recent Active Labor Market Program Evaluations," Journal of the European Economic Association, 16, 894–931.

Carranza, E., R. Garlick, K. Orkin, and N. Rankin (2022): "Job Search and Hiring with Limited Information about Workseekers' Skills," American Economic Review, 112, 3547–83.

Chakravorty, B., W. Arulampalam, A. Y. Bhatiya, C. Imbert, and R. Rathelot (2024): "Can information about jobs improve the effectiveness of vocational training? Experimental evidence from India," Journal of Development Economics, 169, 103273.

Cheema, A., A. I. Khwaja, M. F. Naseer, and J. N. Shapiro (2022): "Glass Walls: Experimental Evidence on Access Constraints Faced by Women," G^{2}LM|LIC Working Paper 65.

Cho, Y., D. Kalomba, A. M. Mobarak, and V. Orozco (2013): "Gender Differences in the Effects of Vocational Training: Constraints on Women and Drop-Out Behavior," World Bank Policy Research Working Paper 6545.

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

Crépon, B., E. Duflo, M. Gurgand, R. Rathelot, and P. Zamora (2013): "Do Labor Market Policies Have Displacement Effects? Evidence from a Clustered Randomized Experiment," The Quarterly Journal of Economics, 128, 531–580.

Crépon, B., E. Lestant, and P. Premand (2024): Youth Employment and Skills Development Project - Apprenticeship Firm Listing 2021 [Data set], World Bank, Development Data Group.
https://doi.org/10.48529/KFCZ-6T55.

Crépon, B. and P. Premand (2024a): Youth Employment and Skills Development Project - Apprenticeship Firms Surveys 2014-2016 [Data set], World Bank, Development Data Group.
https://doi.org/10.48529/W2SZ-D257.

——— (2024b): Youth Employment and Skills Development Project - Apprenticeship Youth Surveys 2014-2018 [Data set], World Bank, Development Data Group.
https://doi.org/10.48529/766E-B621.

Cunha, J. M., G. De Giorgi, and S. Jayachandran (2018): "The Price Effects of Cash versus In-Kind Transfers," The Review of Economic Studies, 86, 240–281.

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

De Mel, S., D. J. McKenzie, and C. Woodruff (2009): "Measuring Microenterprise Profits: Must We Ask How the Sausage Is Made?" Journal of Development Economics, 88, 19–31.

Dicarlo, E., S. Lo Bello, S. Monroy-Taborda, A. M. Oviedo, M. L. Sanchez Puerta, and I. V. Santos (2016): "The Skill Content of Occupations across Low and Middle Income Countries: Evidence from Harmonized Data," IZA Discussion Paper.

Dustmann, C. and U. Schönberg (2012): "What Makes Firm-Based Vocational Training Schemes Successful? The Role of Commitment," American Economic Journal: Applied Economics, 4, 36–61.

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

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

Frazer, G. (2006): "Learning the Master's Trade: Apprenticeship and Human Capital in Ghana," Journal of Development Economics, 81, 259–298.

Frölich, M. and B. Melly (2013): "Unconditional Quantile Treatment Effects under Endogeneity," Journal of Business & Economic Statistics, 31, 346–357.

Global Indicators Department, Enterprise Analysis Unit (2017): Enterprise Survey 2016 [Data set], World Bank, Development Data Group. https://doi.org/10.48529/JA3S-0T37.

Hardy, M., I. M. Mbiti, J. L. Mccasland, and I. Salcher (2019): "The Apprenticeship-to-Work Transition: Experimental Evidence from Ghana," World Bank Policy Research Working Paper 8851.

Hardy, M. and J. McCasland (2023): "Are Small Firms Labor Constrained? Experimental Evidence from Ghana," American Economic Journal: Applied Economics, 15, 253–84.

Heckman, J. and J. Smith (2004): "The Determinants of Participation in a Social Program: Evidence from a Prototypical Job Training Program," Journal of Labor Economics, 22, 243–298.

Heckman, J. J. and S. Mosso (2014): "The Economics of Human Development and Social Mobility," Annual Review of Economics, 6, 689–733.

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

Imbens, G. W. and C. F. Manski (2004): "Confidence Intervals for Partially Identified Parameters," Econometrica, 72, 1845–1857.

Imbens, G. W. and D. B. Rubin (1997): "Estimating Outcome Distributions for Compliers in Instrumental Variables Models," The Review of Economic Studies, 64, 555–574.

——— (2015): Causal Inference in Statistics, Social, and Biomedical Sciences, Cambridge University Press.

Jacobson, L. and J. Davis (2017): "The Relative Returns to Workforce Investment Act–Supported Training in Florida by Field, Gender, and Education and Ways to Improve Trainees' Choices," Journal of Labor Economics, 35, S337–S375.

Jayachandran, S. (2021): "Microentrepreneurship in Developing Countries," in Handbook of Labor, Human Resources and Population Economics, ed. by K. F. Zimmermann, Springer, 1–31.

Jensen, R. (2010): "The (Perceived) Returns to Education and the Demand for Schooling," The Quarterly Journal of Economics, 125, 515–548.

JPAL (2022): "Vocational and Skills Training Programs to Improve Labor Market Outcomes," Policy Insight Note.

Kaas, L. and P. Kircher (2015): "Efficient Firm Dynamics in a Frictional Labor Market," American Economic Review, 105, 3030–60.

Katz, E. and A. Ziderman (1990): "Investment in General Training: The Role of Information and Labour Mobility," The Economic Journal, 100, 1147–1158.

Kugler, A., M. Kugler, J. E. Saavedra, and L. O. Herrera-Prada (2022): "Long-Term Educational Consequences of Vocational Training in Colombia Impacts on Young Trainees and Their Relatives," Journal of Human Resources, 57, 178–216.

Laajaj, R. and K. Macours (2019): "Measuring Skills in Developing Countries," Journal of Human Resources, 56, 1254–1295.

McKenzie, D. (2017): "How Effective Are Active Labor Market Policies in Developing Countries? A Critical Review of Recent Evidence," The World Bank Research Observer, 32, 127–154.

——— (2021): "Small business training to improve management practices in developing countries: re-assessing the evidence for 'training doesn't work'," Oxford Review of Economic Policy, 37, 276–301.

Michaillat, P. (2012): "Do Matching Frictions Explain Unemployment? Not in Bad Times," American Economic Review, 102, 1721–50.

Moffitt, R. A. (2001): "Policy Interventions, Low-Level Equilibria, and Social Interactions," Social Dynamics, 4, 6–17.

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

Olley, G. and A. Pakes (1996): "The Dynamics of Productivity in the Telecommunications Equipment Industry," Econometrica, 64, 1263–1297.

Pissarides, C. A. et al. (2000): Equilibrium Unemployment Theory, The MIT Press.

Quinn, S. and C. Woodruff (2019): "Experiments and Entrepreneurship in Developing Countries," Annual Review of Economics, 11, 225–248.

Romano, J. P. and M. Wolf (2016): "Efficient Computation of Adjusted p-Values for Resampling-Based Stepdown Multiple Testing," Statistics & Probability Letters, 113, 38–40.

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

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

Wolter, S. and P. Ryan (2011): "Apprenticeship," in Handbook of the Economics of Education, ed. by E. A. Hanushek, S. Machin, and L. Woessmann, Elsevier, vol. 3, 521–576.

10 Tables

Table 1: Experiment Size Ratio in Study Localities

Locality	Saturation on Youth Side (1)	Saturation on Firm Side (2)
Bouake	0.04	0.09
Gagnoa	0.08	0.21
Divo	0.11	0.25
Man	0.15	**0.29**
Adzope	**0.23**	0.23
Mankono	**0.37**	**0.41**
Daoukro	**0.37**	**0.41**
Total	0.11	0.22

Column (1) gives the size of the experiment defined as the number of youth who started a formal apprenticeship divided by an estimate of the number of youth starting an apprenticeship yearly in the locality (see Online Appendix A2.3.1 and Table A1 for details.)

Column (2) gives the size of the experiment defined as the number of firms assigned to treatment in the experiment divided by the number of firms hosting apprentices in the locality (see Online Appendix A2.3.2 and Table A2 for details.)

Estimates in bold are above the median.

Table 2: Entry into Apprenticeship

	Youth			Firm		
	Formal	Traditional	Total	Formal	Traditional	Total
	(1)	(2)	(3)	(4)	(5)	(6)
In apprenticeship at midline						
Treated	0.490***	−0.125***	0.365***	0.787***	−0.174	0.613***
	(0.018)	(0.014)	(0.022)	(0.065)	(0.149)	(0.172)
Mean	0.018	0.161	0.179	0.058	1.512	1.570
FDR	0.000	0.000	0.000	0.000	0.243	0.000
FWER	0.000	0.000	0.000	0.000	0.218	0.001
Started since randomization						
Treated	0.712***	−0.185***	0.528***	1.398***	−0.318*	1.080***
	(0.016)	(0.016)	(0.021)	(0.096)	(0.178)	(0.208)
Mean	0.038	0.225	0.263	0.188	1.942	2.130
FDR	0.000	0.000	0.000	0.000	0.100	0.000
FWER	0.000	0.000	0.000	0.000	0.095	0.000

Source: Youth midline survey (1,661 observations) and firm midline survey (674 observations).
Note: Left panel: Estimation of Equation 7 for youth (White-Huber robust standard errors are in parentheses).
Right panel: Estimation of Equation 6 for firms (White-Huber robust standard errors are in parentheses).
p-values adjusted for false discovery rate (FDR) (Benjamini and Hochberg, 1995) and family-wise error rate (FWER) (Romano and Wolf, 2016) are presented at the bottom of each panel. The six outcomes in each panel (for youth in the left panel and firms in the right panel) were jointly tested.

Table 3: Overall Impact on Number of Apprentices

Youth Side

Reduced Form			Instrumental Variable			
	Total (a)	Formal (b)		Total (c=a/b)		Windfall (d=1-c)
Treated	0.528***	0.712***	$1-\omega$	0.741***	ω	0.259***
	(0.021)	(0.016)		(0.022)		(0.022)

Firm side

Reduced Form			Instrumental Variable			
	Total (a)	Formal (b)		Total (c=a/b)		Crowding Out (d=1-c)
Treated	1.080***	1.398***	$1-\psi$	0.773***	ψ	0.227*
	(0.208)	(0.096)		(0.128)		(0.128)

Source: Youth midline survey (1,661 observations) and firm midline survey (674 observations).

Note: Columns (a) and (b) present ITT estimates of Equations 7 (upper panel) and 6 (lower panel). Column (c) presents IV estimates of Equations 21 (upper panel) and 22, which is the ratio of column (a) to (b). Column (d) presents estimates for parameters ω and ψ, as obtained from column (c). 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 are in parentheses.

Table 4: Youth Employment, Hours, and Earnings at Midline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Employment							
	Apprentice	Wage empl.	Self-empl.	Other activities	Total # activities	At least one	
Treated	0.365***	−0.135***	−0.0724***	−0.0891***	0.0526*	0.0336***	
	(0.0216)	(0.0222)	(0.0209)	(0.0202)	(0.0310)	(0.0128)	
Mean	0.18	0.36	0.29	0.27	1.19	0.91	
FDR	0.000	0.000	0.001	0.000			
FWER	0.000	0.000	0.004	0.000			
Hours							
	As an apprentice	As wage empl.	As self-empl.	In other activities	Total		
Treated	18.20***	−6.462***	−4.384***	−2.793***	3.687**		
	(1.170)	(1.235)	(1.009)	(0.801)	(1.492)		
Mean	7.56	15.10	10.51	7.42	41.93		
FDR	0.000	0.000	0.000	0.001			
FWER	0.000	0.000	0.000	0.004			
Earnings							
	Apprentice	Wage empl.	Self-empl.	In other activities	Total Labor	Non-labor[a]	Total
Treated	3,238***	−6,414***	−4,157**	−2,103***	−10,494***	10,213***	−1,408
	(749)	(1,407)	(1,959)	(624)	(2,654)	(870)	(3,295)
Mean	4,746	15,398	14,279	4,752	41,776	7,540	51,484
FDR	0.000	0.000	0.034	0.001			
FWER	0.000	0.000	0.033	0.004			

Source: Youth midline survey (1,661 observations).

Note: The three panels present ITT estimates of Equation 7 for outcome variables related to employment, hours worked, and earnings. Hours worked and earnings variables are winsorized at the 99th percentile. White-Huber robust standard errors are in parentheses.

p-values adjusted for false discovery rate (FDR) (Benjamini and Hochberg, 1995) and family-wise error rate (FWER) (Romano and Wolf, 2016) are presented at the bottom of each panel. The 12 outcomes were jointly tested.

a. Non-labor earnings include the program stipend.

Table 5: Youth Employment, Hours, and Earnings at Endline

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Employment							
	Apprentice	Wage empl.	Self-empl.	Other activities	Total # activities	At least one	
Treated	0.0937***	−0.0363	0.0443*	−0.0218	0.0823***	0.0125**	
	(0.0200)	(0.0241)	(0.0234)	(0.0203)	(0.0304)	(0.00562)	
Mean	0.17	0.38	0.32	0.23	1.23	0.98	
FDR	0.000	0.228	0.141	0.377			
FWER	0.000	0.487	0.324	0.714			
Hours							
	As an apprentice	As wage empl.	As self-empl.	In other activities	Total		
Treated	4.436***	−2.600*	1.058	−1.149	1.457		
	(0.986)	(1.358)	(1.306)	(0.911)	(1.346)		
Mean	6.68	16.61	12.66	8.01	41.19		
FDR	0.000	0.141	0.502	0.312			
FWER	0.000	0.324	0.798	0.625			
Earnings							
	Apprentice	Wage empl.	Self-empl.	In other activities	Total Labor	Non-labor	Total
Treated	3593***	544	4512*	398	8987**	551	9394**
	(1004)	(2133)	(2711)	(731)	(3548)	(1078)	(3928)
Mean	5770	20650	19350	4393	53029	8925	62747
FDR	0.001	0.799	0.192	0.639			
FWER	0.002	0.823	0.437	0.823			

Source: Youth endline survey (1,670 observations).

Note: The three panels present ITT estimates of Equation 7 for outcome variables related to employment, hours worked, and earnings. Hours worked and earnings variables are winsorized at the 99th percentile. White-Huber robust standard errors are in parentheses.

p-values adjusted for false discovery rate (FDR) (Benjamini and Hochberg, 1995) and family-wise error rate (FWER) (Romano and Wolf, 2016) are presented at the bottom of each panel. The 12 outcomes were jointly tested.

Table 6: Impact on Skills at Endline

	(1)	(2)	(3)	(4)
	Routine Task Intensity Index (RTI)	Routine (R)	Non-routine Analytical (NRA)	Non-routine Interpersonal (NRI)
Treated	−0.213***	0.110**	0.243***	0.0793*
	(0.0802)	(0.0472)	(0.0501)	(0.0472)
Between sectors of occupation				
Treated	−0.0374	0.151***	0.107***	0.0814***
	(0.0304)	(0.0244)	(0.0229)	(0.0166)
Within sector of occupation				
Treated	−0.175**	−0.0410	0.139***	−0.00497
	(0.0779)	(0.0448)	(0.0465)	(0.0451)

Source: Youth endline survey (1,670 observations).
Note: Task complexity measures are based on tasks undertaken by youth in their primary occupations, adapted from Autor et al. (2003), Autor and Handel (2013), and Dicarlo et al. (2016). See Table A14 for impacts on specific tasks within each subindex.
RTI index = R − NRA − NRI.
Estimation of Equation 7 with robust standard errors.

Table 7: Impact on Training Participation and Certification

	(1)	(2)	(3)	(4)
Any form of training				
	Participated	Number	Total Months	Cost
Treated	0.477***	0.498***	10.800***	−6,049.768
	(0.022)	(0.027)	(0.527)	(4,176.374)
Mean	0.249	0.287	3.195	18,025.330
Number of training, per type				
	Firm-Based	Dual	Center-Based	Post-Program
Treated	0.205***	0.268***	0.025	−0.010
	(0.021)	(0.019)	(0.019)	(0.011)
Mean	0.085	0.083	0.119	0.052
Certification				
	Any	From Government	From Center	From Firm
Treated	0.147***	0.164***	−0.020**	0.000
	(0.020)	(0.015)	(0.010)	(0.006)
Mean	0.111	0.032	0.048	0.015

Source: Youth endline survey (1,670 observations).

Note: The training considered includes on-the-job training in firms, technical and vocational training (TVET) in training centers, and dual training, combining on-the-job training in firms and center-based training. Any training activity begun since the start of the experiment is included. Post-program training only includes training started after the program.

Government certification includes certification provided by the national apprenticeship agency (AGEFOP). Certifications other than those provided by the government, training centers and firms are included in "any certification," not as a separate category.

Estimation of Equation 7 with robust standard errors.

Table 8: Role of Increased Demand for Apprenticeship or Improved Matching

Share of youth searching for apprenticeship at midline		Implied		
Assumption	Estimate	$P(Trad\|Reg)$	$\lambda_f/\lambda(\theta_1)$	$\lambda(\theta_1)$
(1)	(2)	(3)	(4)	(5)
0% of youth searching for a job	0	0.26	0.78	1
50% of youth searching for a job	0.11	0.37	1.11	0.70
100% of youth searching for a job	0.21	0.48	1.44	0.54

Source: Youth midline survey.

Note: Based on the assumption in column (1), column (2) provides an estimate of the share of youth in the control group who search for an apprenticeship position at midline.

Column (3) shows the implied $P(Trad|Reg)$: the overall share of youth in apprenticeship positions or searching for apprenticeship (col(2)+share of apprentices in the control group).

Column (4) shows the implied relative improvement in matching rates of those participating in the program compared to those in the control group (i.e., searching for traditional apprenticeship) (col(3)/ω).

To build this table, we abstract from the small non-compliance in the control group. We use $\omega = 0.26/0.79 = 0.33$ (i.e., the ratio of participation in apprenticeship in the treatment and control groups in Table 2 (bottom panel, column (3)) instead of the slightly different IV estimate from Table 3.

Table 9: Apprentices' Training, Supervision, and Performance in Firms

	(1)	(2)	(3)	(4)	(5)
	Reference	Formal	p-value	Traditional	p-value
(1) Hours of work and supervision					
Autonomous work	2.378	0.495	0.091	−0.191	0.471
Work under supervision	2.603	0.001	0.996	0.014	0.958
Watching mastercraftsperson	1.793	−0.420	0.042	0.257	0.264
(2) Performance and payments					
Payment for transport and food	9,394	−2,660	0.049	−999	0.382
Bonus payment (for motivation)	3,978	247	0.771	482	0.512
Total payment (wage bill)	13,371	−2,413	0.204	−517	0.742
Fee paid at entry	5,251	−5,148	0.000	−160	0.906
Fee paid monthly	686	−671	0.097	308	0.577
Total fees (until midline survey)	21,024	−20,536	0.000	−469	0.948
Value of work last day	1,479	876	0.006	196	0.365
Nb of days worked per month	20	−7	0.000	0	0.786
Net value of work	17,407	8,367	0.243	2,458	0.613
(3) Skills as rated by mastercraftsperson					
Behavioral skills index	0.062	−0.247	0.020	−0.026	0.773
Technical skills index	0.366	0.461	0.000	−0.000	0.995

Source: Firm midline survey, apprentice module, for panel (1) (948 observations);
Firm midline survey, employer module, for panel (2) and (3) (1,260 observations).
Note: Estimation of Equation 8, with probability weights.
The reference category is traditional apprentices who entered control firms within six months of the randomization date.
Column "Formal" presents the difference in means between formal apprentices in treated firms and the reference category (traditional apprentices in control firms).
Column "Traditional" presents the difference in means between traditional apprentices in treatment firms and the reference category (traditional apprentices in control firms).
Skills are measured using a set of questions asked to the employer about each apprentice. The technical skills measure 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 two to seven 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 behavioral skills index averages several questions about attitude at work, including absenteeism, punctuality, respect for clients and supervisor, seriousness, and motivation.
The value of work is the amount that the firm owner would have had to pay an occasional worker to accomplish the same tasks as the apprentice during their last working day. (See Online Appendix A3 for details). The net value of work subtracts apprentices' compensation from the monthly value of their work (value of work during the last work day multiplied by number of days worked during the month) and fees.
Standard errors are clustered at the firm level.

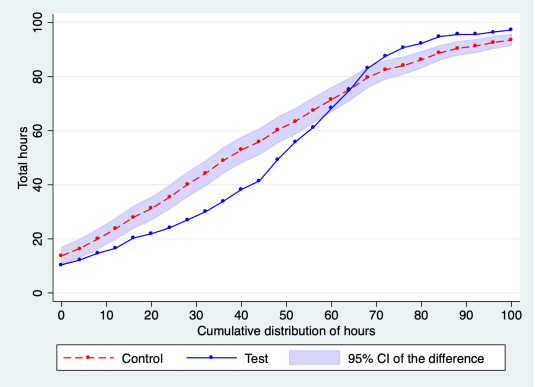
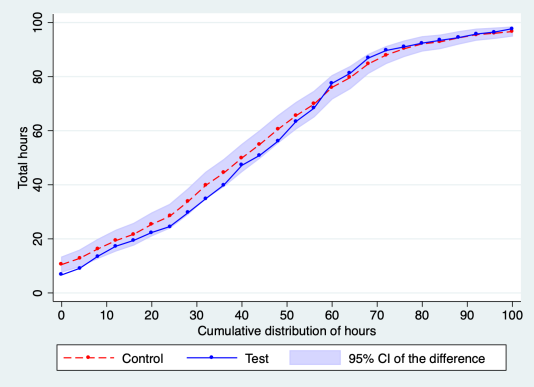
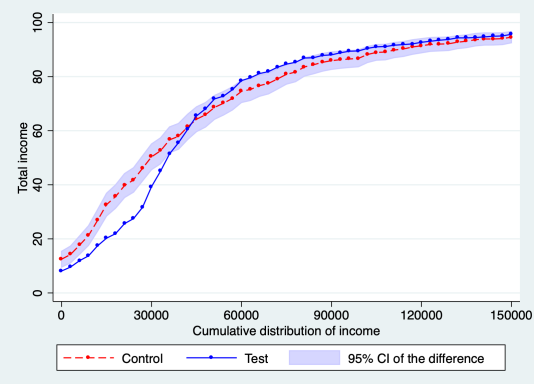
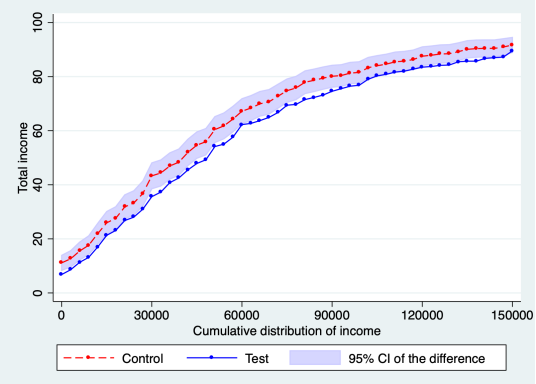
Table 10: Program Costs and Benefits for Youth

	Amount in CFAF per youth
A: Program costs per youth	
Medical check-up and insurance	4,399
Stipends	457,319
Work, personal and learning equipment	97,729
General training in vocational centers and certification	58,352
Indirect cost (enrollment, monitoring, ...)	37,017
Total nominal cost at year 0	654,817
NPV total cost at year 4 (5% discount rate)	795,934
B: Benefits per youth	
Year 3: Total income gains (year 4 ITT impact inflated at 5%)	118,366
Year 4: Total income gains (endline ITT impact)	112,729
Year 5 onward: Total income gains, 15% annual dissipation	478,943
Year 5 onward: Total income gains, 10% annual dissipation	674,442
Year 5 onward: Total income gains, 5% annual dissipation	1,047,045
Year 5 onward: Total income gains, assuming year 4 gains persist	1,901,502
A: Total income gains for years 1-4, 5% discount rate	231,095
B1: Total income gains, 5% discount rate, 15% annual dissipation	710,037
B2: Total income gains, 5% discount rate, 10% annual dissipation	905,536
B3: Total income gains, 5% discount rate, 5% annual dissipation	1,278,140
C: Total income gains, 5% discount rate, assuming year 4 gains persist	2,132,597
C: Benefit / cost ratio	
A: Income gains years 1-4, 5% discount rate	0.29
B1: Total income gains, 5% discount rate, 15% annual dissipation	0.89
B2: Total income gains, 5% discount rate, 10% annual dissipation	1.14
B3: Total income gains, 5% discount rate, 5% annual dissipation	1.61
C: Total income gains, 5% discount rate, assuming year 4 gains persist	2.68
D: Internal rate of returns (%)	
A: Income gains years 1-4, 5% discount rate	−0.26
B1: Total income gains, 5% discount rate, 15% annual dissipation	0.03
B2: Total income gains, 5% discount rate, 10% annual dissipation	0.07
B3: Total income gains, 5% discount rate, 5% annual dissipation	0.10
C: Total income gains, 5% discount rate, assuming year 4 gains persist	0.13

Note: Benefits for year four are based on the observed impacts on youth earnings at endline (ITT estimates). Impacts on earnings during the program are zero (in line with midline results). After year four, we assume annual dissipation of 15% (scenario B1), 10% (scenario B2), 5% (scenario B3), or no dissipation (scenario C). Impacts are assumed constant within year. All monetary amounts are in nominal CFAF.

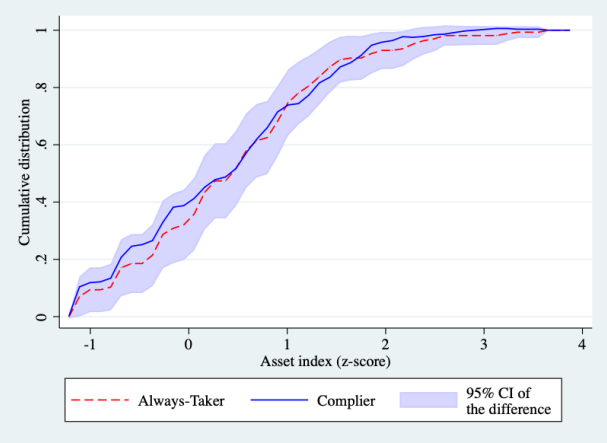
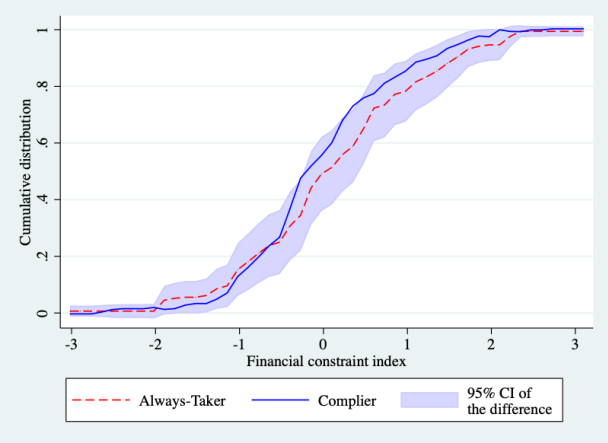
11 Figures

Figure 2: Distribution of Potential Outcomes for Hours Worked and Income

Midline: Hours	Endline: Hours
	
Mann Whitney test: p-values from 10,000 permutations (within strata, see note for details).	
$p = 6/10000$	$p = 2344/10000$
Midline: Income	Endline: Income
	
Mann Whitney test: p-values from 10,000 permutations (within strata, see note for details).	
$p = 62/10000$	$p = 2/10000$

Source: Youth midline (1,661 observations) and endline surveys (1,670 observations).
Note: The figures show the results of the estimation of the cumulative distribution of potential outcomes in the two assigned groups. Hours and income are winsorized at the 99th percentile. They are based on the estimation of Equation 7, with variables defined as $1(y < t)$ for t varying over the support of y. The red curve shows the average of those variables in the control group. The blue curve adds to this average the estimated treatment coefficient. The grey area around the red curve represents a band of ± 1.96 times the standard error of the estimated treatment effect.
The intermediate panels present the result of the Mann-Whitney rank test implemented using 10,000 permutations within randomization strata. The p-value is the ratio of the number of times the statistic from a permuted assignment variable was found larger than the statistic obtained from the true assignment variable to the total number of permutations.

Figure 3: Distribution of Baseline Indices of Assets and Financial Constraints for Compliers and Always-Takers

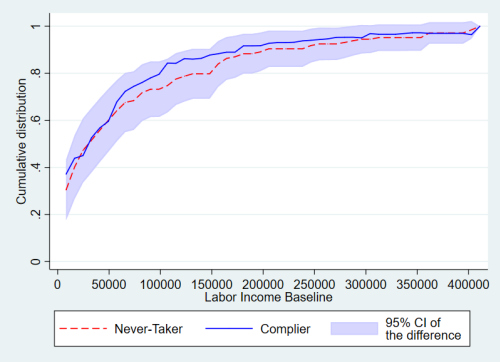
Asset Index	Financial Constraints
	

Kolmogorov Smirnov p-values	
$p = 0.951$	$p = 0.354$

Source: Youth baseline and midline surveys (1,313 observations).
Note: The figures show the results of the estimation of the cumulative distribution of two indices computed from the baseline survey for always-takers (dashed red line) and compliers (solid blue line). They are obtained following Abadie (2003). The grey area around the dashed red curve represents the 95% confidence interval of the difference. Always-takers participate in traditional apprenticeship even in the absence of the formal apprenticeship program. Compliers participate in formal apprenticeship only if offered the program and would not otherwise enter apprenticeship.

Figure 4: Distribution of Potential Outcomes $y(0)$ for Labor Income for Compliers and Never-Takers at Baseline, Midline, and Endline

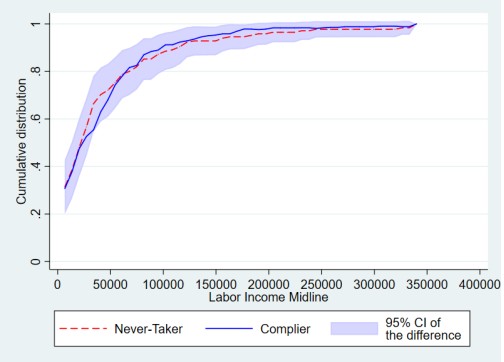
Baseline



Mean Difference Between Compliers and Never-Takers

$-6,848$
$[\ -36,994\ ,\ 23{,}299]$

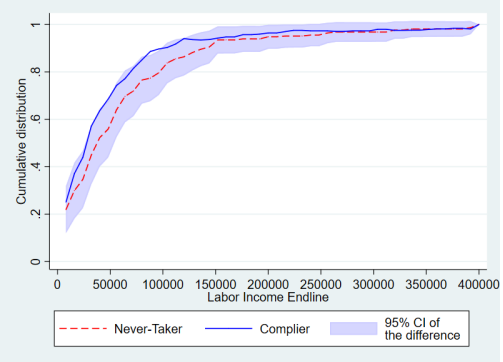
Midline



Mean Difference Between Compliers and Never-Takers

$-1,998$
$[\ -17,099\ ,\ 13{,}104]$

Endline

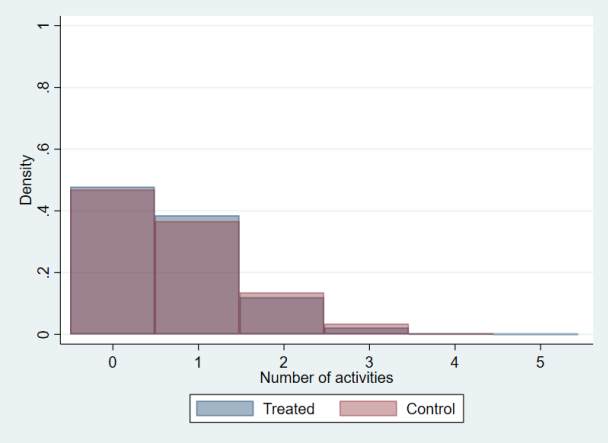
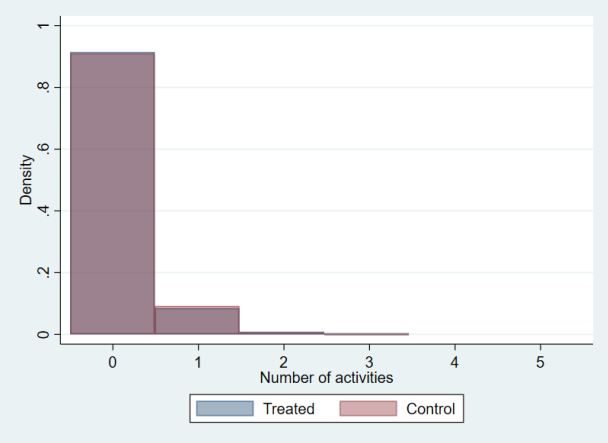
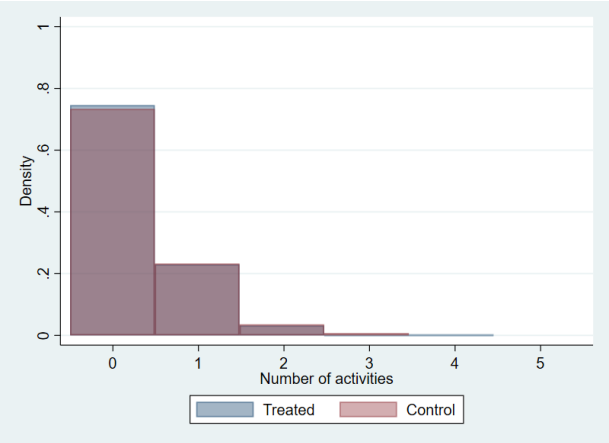
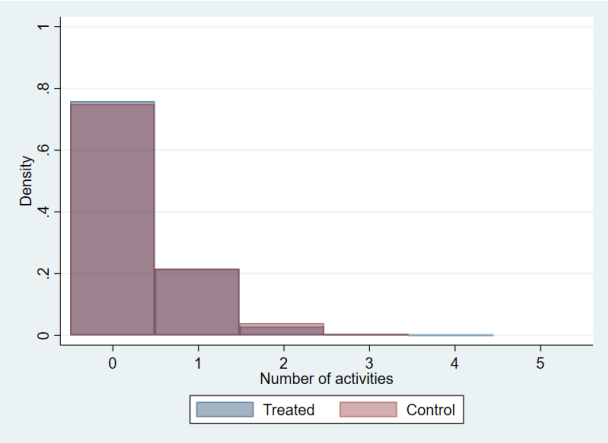


Mean Difference Between Compliers and Never-Takers

$-13,974$
$[\ -31,969\ ,\ 4{,}021]$

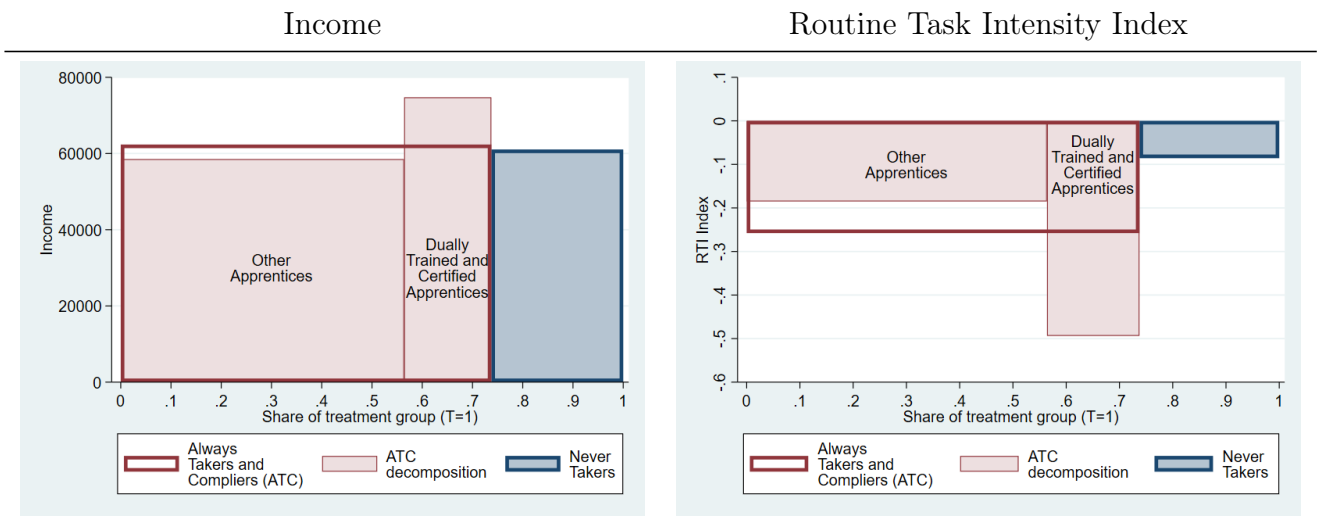
Source: Youth baseline (1,372 observations), midline (1,661 observations), and endline surveys (1,670 observations).
Note: Income winsorized at the 99th percentile. The figures show the results of the estimation of the cumulative distribution of potential outcomes $y(0)$ for compliers (solid blue line) and never-takers (dashed red line). They are based on averages of variables defined as $1(y < t)$ for t varying over the support of y on subsamples of non-apprentice youth based on their assignment status following Imbens and Rubin (1997) (see Annex 12.1 for precise formulas). The grey area around the red curve represents the 95% confidence interval of the difference. Compliers participate in formal apprenticeships only if offered the program, and would not otherwise enter apprenticeship. Never-takers do not enter apprenticeship even if offered.

Figure 5: Distribution of Activities Started after the End of the Program

Number of All Activities	Number of Apprenticeship Activities
	
Kolmogorov Smirnov p-values	
$p = 0.863$	$p = 1.000$
Mean Difference between Treatment and Control	
-0.085	-0.009
$[-0.165\ ,\ -0.005]$	$[-0.044\ ,\ 0.025]$
Number of Self-Employed Activities	Number of Wage Activities
	
Kolmogorov Smirnov p-values	
$p = 1.000$	$p = 1.000$
Mean Difference between Treatment and Control	
-0.034	-0.035
$[-0.090\ ,\ 0.022]$	$[-0.091\ ,\ 0.022]$

Source: Youth endline survey (1,670 observations).
Note: The figures show the distribution of the number of activities started between January 2017 and April 2018, for treated youth (blue bars) and control youth (red bars).

Figure 6: Decomposition of Treatment Effects for Compliers, Always-Takers, and Never-Takers at Endline, by Participation in Dual Training and Certification



Source: Youth endline survey (1670 observations).

Note: Income winsorized at the 99th percentile. A dually trained apprentice has undertaken either dual apprenticeship or center-based training. Always-takers participate in traditional apprenticeship even in absence of the formal apprenticeship program. Compliers participate in formal apprenticeships only if offered the program and would not otherwise enter apprenticeship. Never-takers do not enter apprenticeship even if offered.

The decomposition procedure involves taking the means among treated apprentices conditional on having completed both dual training and gotten a certification since randomization or not.

12 Additional Details on the Conceptual Framework

12.1 Supply of Apprentices

We model youth decision to enter traditional apprenticeship as a sequential process. Youth decide first whether to enter the market for apprenticeship and start searching, and second they enter apprenticeship with a probability $\lambda(\theta)$.

We model youth decision to enter formal apprenticeship in the same way. Youth first register in the program, and second a share λ_f of them enter a formal apprenticeship position. We denote $V(0)$ the present value if youth do not participate in apprenticeship, $V(trad)$ the value if youth enter traditional apprenticeship, and $V(f)$ the value if they register in the program. Before registering in the program, youth have an expectation $\mathcal{E}(V(f))$ of the value of participating in the program. They register if this expectation is larger than the outside option $V(0)$. Once they register, they learn about the true value of participating in the program (which can be above or below their expectation) and decide to start a formal apprenticeship if $V(f) > V(0)$.

The values $V(0)$, $V(t)$, and $V(f)$ can be decomposed in earnings between two periods, one during apprenticeship and the other after the end of the apprenticeship: $V(0) = w_1(0) + w_2(0)$, $V(t) = (w_1(t) - Fee + B + R) + (w_2(t) - B)$ and $V(f) = (w_1(f) + S + B + R) + (w_2(f) - B)$, where Fee corresponds to the fee youth have to pay to enter traditional apprenticeships, S the stipend received during formal apprenticeships, directly paid to them by the program, B the amount youth can borrow to fund their apprenticeships, and R the financial support they can receive (e.g., from their parents). We also assume that youth earnings have to be above a minimum threshold during the first period: $w_1(0) > w_{min}$, $w_1(t) - Fee + B + R > w_{min}$ and $w_1(f) + S + B + R > w_{min}$. We assume S is large enough to have $w_1(f) + S + B + R > w_{min}$, which means that there are no remaining financial constraints for participants in formal apprenticeship.

Youth decide to search for a traditional apprenticeship or to register in the formal apprenticeship program if the two following conditions are respectively met:[76]

(10)

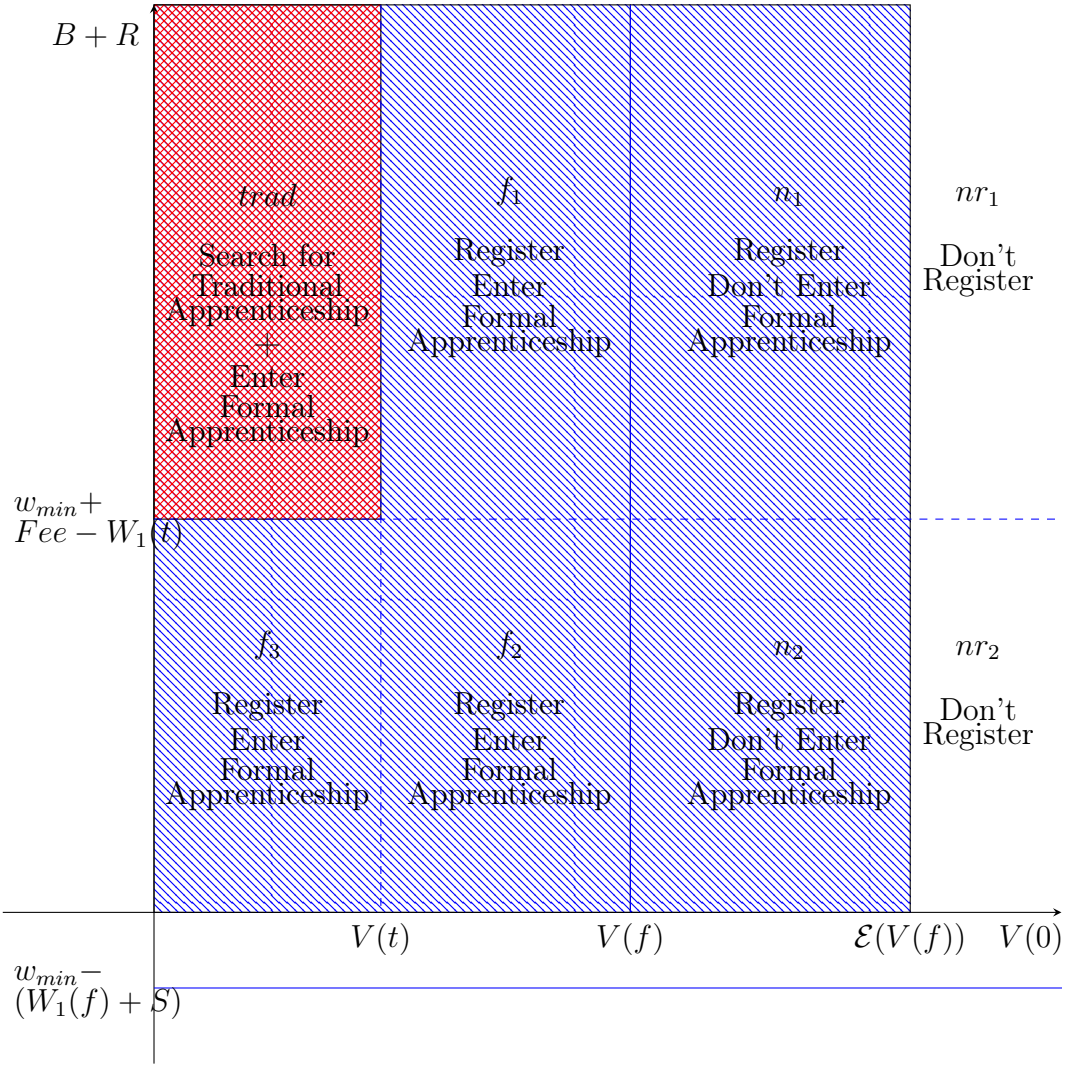
trad: start searching for traditional apprenticeship if	**reg**: register in formal apprenticeship program if
$\begin{cases} V(t) > V(0) \\ w_1(t) - Fee + B + R > w_{min} \end{cases}$	$\begin{cases} \mathcal{E}(V(f)) > V(0) \\ w_1(f) + S + B + R > w_{min} \, . \end{cases}$

Figure 7 describes the various possible cases.

- There are youth who would search for a traditional apprenticeship in absence of a formal apprenticeship program (area *trad* in the figure). These youth have limited outside opportunities, such that even if the training quality is low, apprenticeships are their best option. In addition, these youth can borrow or receive sufficient support from their family to meet their minimum needs.

[76] We assume all youth would prefer a formal apprenticeship over a traditional apprenticeship $V(f) > V(t)$ and $w_1(f) + S + B + R > w_1(t) - Fee + B + R$.

Figure 7: Youth Decision to Participate in Apprenticeship



Note: The figure shows the decision to search for traditional apprenticeship positions, register for the formal apprenticeship program, and enter a formal apprenticeship as a function of the value of the different options and financial constraints.

$trad$ youth search for an apprenticeship position in the absence of the program.

Youth who register in the program are those in area $trad \cup f_1 \cup f_2 \cup f_3 \cup n_1 \cup n_2$ that we label as reg in the text.

f_1, f_2 and f_3, youth register in the program and enter formal apprenticeship when offered but would not otherwise enter traditional apprenticeship.

n_1 and n_2, youth register in the program but don't enter formal apprenticeship.

nr_1 and nr_2 youth do not register in the program.

- There are youth who register in the program (area $reg = trad \cup f_1 \cup f_2 \cup f_3 \cup n_1 \cup n_2$). The introduction of the program attracts two new types of youth. First, the improvements in the quality of the training (and the provision of the subsidy) lead to a perceived value of participation in the program $\mathcal{E}(V(f))$. Some youth who are not interested in traditional apprenticeship because they have better outside options become interested in registering in the formal apprenticeship program (area f_1, f_2, n_1 and n_2 in the figure). Second, thanks to the subsidy, some youth who do not have access to financial resources (financial support from family or a loan) to meet their minimum needs can enter formal apprenticeships (area f_3, f_2, and n_2 in the figure). All these youth, as well as those who would search for traditional apprenticeship, register in the program.

- Once registered, youth learn the true value of participation in formal apprenticeship $V(f)$. Based on this information, part of the registered youth for whom this value is too low decide to quit and do not enter apprenticeship (areas n_1 and n_2 in the figure).

To learn more about the mechanisms and in particular the role of the subsidy and the vocational training, we examine baseline differences between compliers and always-takers (for example, by considering variables positively correlated with $B + R$). We also compare potential outcome $y(0)$ for compliers and never-takers (in particular, earnings). These notions are closely related to the different types of youth described above. Always-takers participate in apprenticeship even in the absence of a formal program. They include the share $\lambda(\theta)$ of youth in area $trad$ who found a traditional apprenticeship position. Compliers enter apprenticeship only because of the program. They include individuals for whom financial constraints are alleviated and individuals for whom formal apprenticeship is more attractive than their outside option (areas f_1, f_2 and f_3). There are also youth in area $trad$ who would participate in apprenticeship even in the absence of the program but do not find a position (the remaining share $1 - \lambda(\theta)$). Last, never-takers apply to the program but do not enter apprenticeship after realizing that the value of participation is less than their outside option (areas n_1 and n_2 in the figure).

It is important to note that the comparison of the means of characteristics related to $B + R$ and $V(0)$ for compliers and always-takers is ambiguous. Compliers in areas f_2 and f_3 should unambiguously have lower values of $B + R$ than youth in area $trad$, but this is not clear for youth in area f_1. Similarly, compliers in areas f_1 and f_2 should unambiguously have larger values of $V(0)$ than youth in area $trad$, but this is not clear for youth in area f_3. On the other hand, the distributions of $V(0)$ are unambiguously different between compliers and never-takers.

We follow the literature on the local average treatment effect (LATE) and use the following equation (we note Z the assignment variable and App the apprenticeship status):[77]

[77] Abadie (2003) proposes a direct method to compute average functions of potential outcomes on compliers (denoted C). We do not follow this approach as we also want to compute averages for always-takers (A) and never-takers (N).

$$P(C|C \cup A) = \frac{P(App = 1|Z = 1) - P(App|Z = 0)}{P(App|Z = 1)}$$
$$E(g(x)|A) = E(g(x)|Z = 0, App = 1)$$
$$E(g(x))|C) = \frac{E(g(x)|Z = 1, App = 1) - E(g(x)|Z = 0, App = 1)(1 - P(C|C \cup A))}{P(C|C \cup A)}$$
$$E(g(x))|C) - E(g(x)|A) = \frac{E(g(x)|Z = 1, App = 1) - E(g(x)|Z = 0, App = 1)}{P(C|C \cup A)}$$
$$P(C|C \cup N) = \frac{P(App = 1|Z = 1) - P(App|Z = 0)}{P(App|Z = 0)}$$
$$E(g(y(0))|N) = E(g(y)|Z = 1, App = 0)$$
$$E(g(y(0))|C) = \frac{E(g(y)|Z = 0, App = 0) - E(g(y)|Z = 1, App = 0)(1 - P(C|C \cup N))}{P(C|C \cup N)}$$
$$E(g(y(0))|C) - E(g(y(0))|N) = \frac{E(g(y)|Z = 0, App = 0) - E(g(y)|Z = 1, App = 0)}{P(C|C \cup N)}.$$

12.2 Equilibrium of the Market for Traditional Apprentices with and without the Intervention

12.2.1 Aggregate Supply of Apprentices

Following the previous set of decisions, there are proportions $P(trad)$ and $P(reg) = P(trad \cup f_1 \cup f_2 \cup f_3 \cup n_1 \cup n_2)$ of youth who would be ready to enter traditional apprenticeship or register in the formal apprenticeship program when it is available.

Assume that, absent the program, there are N_y youth. $P(trad)N_y$ youth thus search for a traditional apprenticeship position. Also assume that, when searching for a traditional apprenticeship position, youth find a position with a probability $\lambda(\theta)$ that depends positively on the tightness of the apprenticeship market—i.e., the ratio of apprenticeship vacancies to the number of youth searching for an apprenticeship position.[78] The supply side relation $S^{trad}(\theta)$ between the number of apprentices and the tightness writes

$$S^{trad}(\theta) = \lambda(\theta)P(trad)N_y\,. \tag{11}$$

Assume that a share σ_a of the N_y youth is offered the opportunity to participate in formal apprenticeship. There are $N_{reg} = \sigma_a P(reg)N_y$ youth who register in the program. We assume the take-up rate for them is λ_f. The number of youth entering formal apprenticeship is thus

$$N_{form} = \lambda_f \sigma_a P(reg)N_y = \lambda_f N_{reg}\,. \tag{12}$$

When the formal apprenticeship program is introduced, a share σ_a of youth is removed

[78] $\lambda(\theta)$ is derived from a homogeneous matching function: $\lambda(\theta) = M(Y_a, V)/Y_a$, where Y_a is the number of youth searching for a traditional apprenticeship position.

from the traditional apprenticeship market: the new supply of traditional apprentices $S^{trad}(\theta, 1)$ writes:

$$S^{trad}(\theta, 1) = (1 - \sigma_a) S^{trad}(\theta)\,. \tag{13}$$

12.2.2 Demand for Apprentices

Firms are assumed to have a production technology in which traditional apprentices (n_a) and formal apprentices (n_f) are inputs that are perfect substitutes.[79] We consider the partial demand for traditional apprentices once n_f formal apprentices have been hired as a function of θ. We approximate this partial demand as $d(\theta) - \psi n_f$. To shed light into what ψ represents, we consider a production function $f(N_0 - s + n_a + a.n_f)$ and a cost of filling positions $c(n, \theta)$.[80] The demand for apprenticeship at the firm level is thus given by $f'(N_0 - s + n_a + a.n_f) - w - c'(n, \theta) = 0$. The parameter ψ can be interpreted as $-dn_a/dn_f = a(-f'')/(c'' - f'')$. ψ thus captures a mix of the crowding out effect, the constraints on the production function, and how strongly the marginal cost of recruitment increases. ψ is the first key parameter of the experiment. It captures the intuitive idea that formal apprentices can crowd out traditional apprentices. $d(\theta)$ is decreasing in θ as the cost of matching increases when the number of vacancies to fill per youth searching increases.

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

12.2.3 Equilibrium

Absent any intervention (i.e., for $\sigma_a = \sigma_f = 0$), the supply and demand functions for traditional apprentices determine an equilibrium in which the tightness is θ_0 and the total number of apprentices is $N_0 = S_a^{trad}(\theta_0) = D^{trad}(\theta_0)$. This is represented by point E_0 in Figure 8. The intervention causes a downward shift in the supply of traditional apprentices (by $\sigma_a S_a^{trad}(\theta)$) and another downward shift in the demand for traditional apprentices (by ψN_{form}). At the new equilibrium, N_1^{trad} youth are hired as traditional apprentices and the tightness becomes θ_1. The new equilibrium is represented by point E_1 (θ_1 and N_1^{trad}) in Figure 8.

We consider the windfall parameter (ω) defined by

$$\omega = \sigma_a S_a^{trad}(\theta_1)/N_{form} = \lambda(\theta_1) P(trad)/(\lambda_f P(reg)) = \frac{\lambda(\theta_1)}{\lambda_f} P(trad|reg)\,. \tag{14}$$

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

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

[80] The cost of a vacancy is usually modeled as $c/q(\theta)$, where $1/q(\theta)$ is the expected duration to fill a vacancy. The recent literature has refined this model to take into account the effort made by the firm to fill vacancies (Kaas and Kircher, 2015).

at the new equilibrium market tightness θ_1.

ω and ψ are the two key parameters that our experiment will estimate to describe the change in equilibrium induced by the program.

We can expand the supply and demand functions around the new equilibrium θ_1 (see Figure 8). For example, on the supply side, we get $N_1^{trad} = (1-\sigma_a)S_a^{trad}(\theta_1) = N_0 + A_s(\theta_1 - \theta_0) - \sigma_a S_a^{trad}(\theta_1) = N_0 + A_s(\theta_1 - \theta_0) - \omega N_{form}$. We obtain the following two equations:

$$\text{Supply: } N_1 = N_0 + A_s(\theta_1 - \theta_0) - \omega N_{form}\,, \tag{15}$$

$$\text{Demand: } N_1 = N_0 - A_d(\theta_1 - \theta_0) - \psi N_{form}\,. \tag{16}$$

A_s is the slope parameter of the supply of traditional apprentices, and A_d is the opposite of the slope parameter of the demand for traditional apprentices. Both parameters are positive. Using Equations 15 and 16, we can easily express the change in the number of traditional apprentices $N_1^{trad} - N_0$ and in the tightness $\theta_1 - \theta_0$ as a function of the two shift parameters ψ and ω:

$$\frac{N_1^{trad} - N_0}{N_{form}} = -\frac{A_s\omega + A_d\psi}{A_s + A_d}\,, \tag{17}$$

$$\theta_1 - \theta_0 = (\omega - \psi)\frac{N_{form}}{A_s + A_d}\,. \tag{18}$$

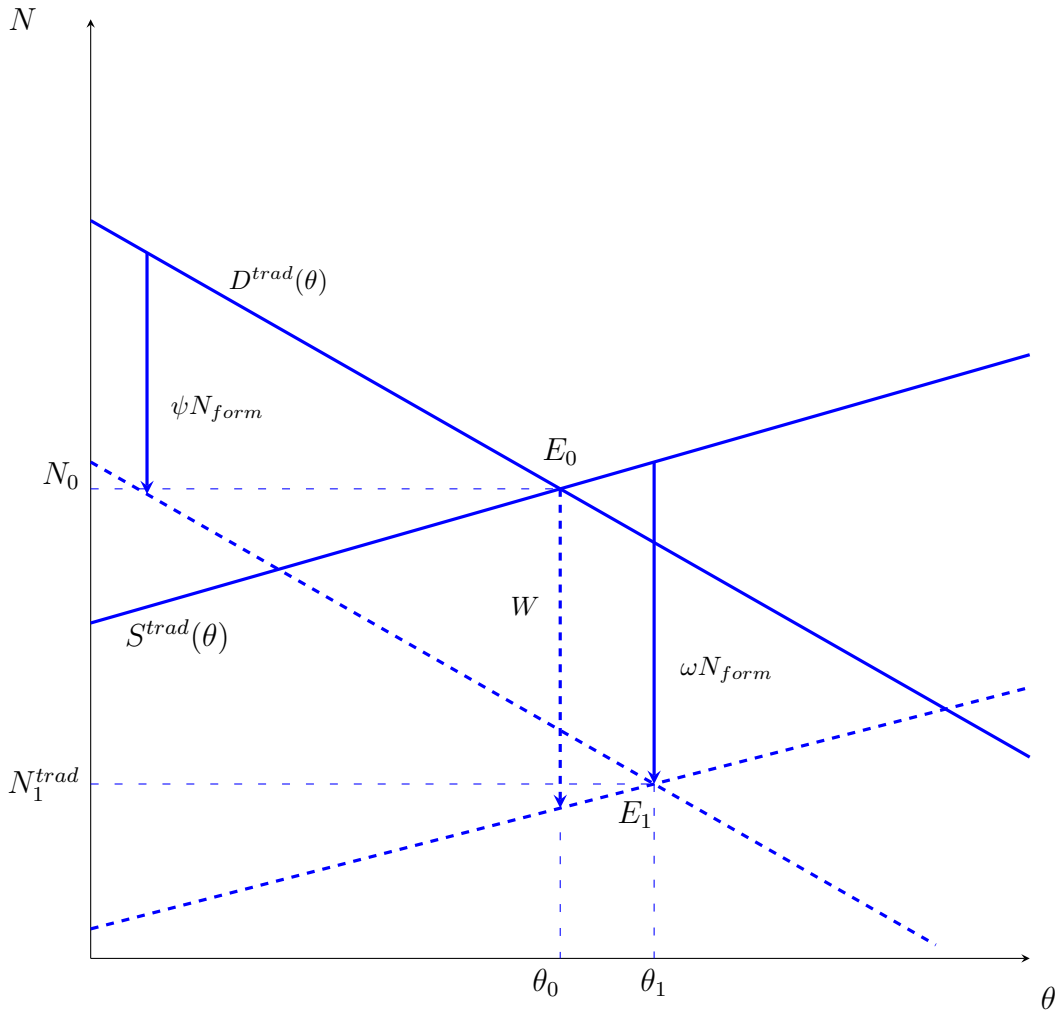
12.3 Implications for Estimation

12.3.1 Intention-to-Treat Parameters

The framework derives the theoretical parameters to be estimated in the empirical analysis and provides insights on how to interpret the empirical results. We consider intention-to-treat (ITT) parameters that are obtained by comparing means between the treatment and control groups. There are N_{reg} youth who register in the program and are assigned to treatment (and similarly N_{reg} youth who register in the program and are assigned to control). Consider, for example, participation in either traditional or formal apprenticeship. The entry rate into (any) apprenticeship position in the treatment group is $\tau_1 = N_{form}/N_{reg}$. The ITT parameter we measure compares this entry rate with the entry rate into apprenticeship in the control group ($\tau_0 = \omega N_{form}/N_{reg} = \sigma_a S^{trad}(\theta_1)/N_{reg}$). Thus our estimated ITT parameter is $ITT_{youth} = N_{form}/N_{reg} - \omega N_{form}/N_{reg}$. The entry rate in the control group is measured at prevailing market conditions θ_1, but the "true" ITT is based on the entry rate into apprenticeship absent the program at market condition θ_0. Figure 8 shows that the "true" ITT would involve $W = \sigma_a S^{trad}(\theta_0)$ as counterfactual, while the estimated counterfactual is $\omega N_{form} = \sigma_a S^{trad}(\theta_1)$. However, the difference between the two quantities ($W/N_{reg} - \omega N_{form}/N_{reg} = \sigma_a(S^{trad}(\theta_1) - S^{trad}(\theta_0))/N_{reg}$) is of the order of magnitude of the adjustment in the tightness. As already discussed and shown in Equation 18, $(\theta_1 - \theta_0) \approx (\omega - \psi)\sigma$. The adjustment in the tightness is small under two conditions: when the shifts in supply and demand are of the same order of magnitude or when the size of the experiment is small.

Note that $\sigma(\omega - \psi)$ is also the order of magnitude of the bias in the ITT parameter,

Figure 8: Equilibrium Employment of Traditional Apprentices and Tightness



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

say B_y. Indeed, $B_y = W/N_{reg} - \omega N_{form}/N_{reg} = \sigma_a(S^{trad}(\theta_1) - S^{trad}(\theta_0))/N_{reg}$. From the linear approximation, $S^{trad}(\theta_1) - S^{trad}(\theta_0) \approx A_s(\theta_1 - \theta_0)$. Thus, from Equation 18, $S^{trad}(\theta_1) - S^{trad}(\theta_0) \approx A_s/(A_s + A_d)(\omega - \psi)N_{form}$. As a result, the bias in the ITT parameter is $B_y \approx \sigma_a A_s/(A_s + A_d)(\omega - \psi)N_{form}/N_{reg} = \sigma_a(\omega - \psi)\lambda_f A_s/(A_s + A_d)$. Thus we can bound the ITT parameter on youth entry in apprenticeship by

$$|B_y| \leq \sigma_a(\omega - \psi)\lambda_f \,. \tag{19}$$

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

As for the youth side, $\sigma(\omega - \psi)$ is also the order of magnitude of the bias on the ITT parameter on the firm side, say B_f. Indeed, the bias is $B_f = d(\theta_1) - d(\theta_0) \approx A_d/N_f(\theta_1 - \theta_0) \approx n_f A_d/(A_s + A_d)(\omega - \psi)\sigma_f$. Thus we can bound the ITT parameter on firm hires on apprentices by

$$|B_f| \leq n_f(\omega - \psi)\sigma_f \,. \tag{20}$$

This analysis extends to other parameters obtained by comparing youth or firms in the treatment and control groups.

12.3.2 Instrumental Variable Estimation of ω and ψ

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

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

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

$$a_i = \alpha_y + \beta_y f_i + \sum_{St} \mu_{St} 1_{St} + u_i \,, \tag{21}$$

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

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

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

$$n_{tot,i} = \alpha_f + \beta_f n_{f,i} + \sum_v \gamma_v 1_v + \sum_s \delta_s 1_s + u_i \,, \tag{22}$$

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. This instrumental variable estimate identifies $1-\psi$. ψ is the crowding-out parameter: for each formal apprentice entering firms, there are ψ fewer traditional apprentices.

It is important to note that these estimations of ω and ψ do not rely, as for the ITT, on the assumption that the adjustment in the tightness is small, because they are defined at the new equilibrium market condition θ_1.