

From Training to Earning

The 7-Year Impact of Dual Apprenticeships on Youth Employment

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Abstract

This paper studies the long-term impacts of dual apprenticeships on youth employment in a high-informality labor market. A Randomized Controlled Trial in Côte d'Ivoire with four follow-up surveys collected over seven years shows that dual apprenticeships have sustained impacts: youth earnings increase by 14 to 20 percent two to five years after program completion. Gains are observed across the earnings distribution, and the share of youth in working poverty—with earnings below the minimum wage—decreases by 11

percent. Importantly, results highlight a distinct pathway whereby training raises earnings through self-employment, with no impact on access to wage employment. Youth perform more complex, non-routine tasks, consistent with improved technical skills and productivity. In a setting where formal wage jobs are rare, these findings show that dual apprenticeships constitute an effective and inclusive skilling model that raises earnings in self-employment and reduces working poverty.

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1 Introduction

In many low- or middle-income economies, formal wage jobs are rare, but a large share of the population works in low-paying casual jobs or subsistence self-employment (Filmer et al., 2014; Breza and Kaur, 2025). In this context, many individuals have low earnings and working poverty is widespread. Skills training is one of the most common instruments considered as part of policies to address these employment challenges, and the literature on its effectiveness is vast.¹ Yet the pathways through which training raises earnings in highly informal labor markets or lifts individuals out of working poverty remain imperfectly understood. On the one hand, the existing literature on training most often considers how programs facilitate the transition into wage jobs. On the other hand, studies explicitly addressing self-employment tend to focus on business training or other instruments such as capital support, with impacts at times concentrated among a subset of high-potential entrepreneurs.² This leaves open questions on whether training can promote more skilled self-employment and reduce working poverty by raising the earnings of those at the bottom of the income distribution.

This paper takes up these questions by analyzing dual apprenticeships that combine extended on-the-job training with complementary technical training in vocational centers. While apprenticeships are increasingly considered by policy makers, credible evidence on their long-term impacts and underlying mechanisms remains limited. We contribute to the literature by examining whether dual apprenticeships generate sustained gains in earnings over 7 years through wage jobs or self-employment pathways. We also consider impacts across the earnings distribution, including on working poverty.

Specifically, we analyze a dual apprenticeship program in Côte d’Ivoire that combines on-the-job training in small or medium enterprises, similar to the traditional apprenticeships prevalent in West Africa, with center-based technical training and a temporary subsidy paid to apprentices for up to 24 months. In an earlier article, we analyzed the short-term effects of introducing this dual apprenticeship model on both youth and firms (Crépon and Premand, 2024). Using a double-sided randomized design, we showed that the program substantially increased youth entry into apprenticeship without crowding out firms’ hiring of traditional apprentices, consistent with unmet demand for apprentices in small and medium enterprises. In this paper, we extend earlier work by analyzing a rich panel of follow-up surveys conducted during the apprenticeship, two, three, and five years after program completion (i.e., up to 7 years since the start of the experiment). This allows us to study youth employment trajectories and earnings dynamics well beyond the training phase.

We find that dual apprenticeships generate sizable and persistent impacts: earnings increase

¹For recent reviews, see Card et al. (2018); JPAL (2022); Carranza and McKenzie (2024).

²For recent reviews on entrepreneurship and capital support, see Cai et al. (2023); Jayachandran (2021).

by 14 to 20 percent two to five years after program completion. These gains are observed across the earnings distribution, including at the bottom. In particular, dual apprenticeships reduce the share of youth in extreme working poverty—earning less than half the formal minimum wage—by 7.3 percentage points (20 percent relative to a baseline of 37 percent), and the share of working poor earning below the formal minimum wage by 6.9 percentage points (11 percent relative to a baseline of 62 percent).

Importantly, these earnings gains do not stem from increased access to wage employment. Instead, they are driven by higher earnings in self-employment, associated with higher hourly earnings and youth performing more complex, non-routine tasks, consistent with improved technical skills and productivity.

The main contribution of the paper is to provide the first experimental estimates of the impacts of dual apprenticeships on youth employment and sustained exit out of working poverty in an informal economy over the medium- to long-term. Specifically, we complement the literature on training and apprenticeship in several ways.

First, recent syntheses show that the average effects of job training programs are typically modest, particularly for public interventions, and that long-run evidence on earnings trajectories remains scarce (Carranza and McKenzie, 2024). We add to the limited number of studies that analyze medium- to long-term impacts of Active Labor Market Policies in developing countries.³ We do so by documenting not only the persistence of impacts, but also their significance at the bottom of the distribution. Indeed, impacts on the most vulnerable youth emerge after apprenticeship completion and persist 5 years post-program. This demonstrates that dual apprenticeships reduce (extreme) working poverty and do not merely provide temporary benefits.

Second, we specifically study “dual apprenticeships” that combine on-the-job and center-based training. There is still limited experimental evidence on dual training, with the exception of Attanasio et al. (2011), who study a classroom training combined with 6-month internships. We also complement the broader literature analyzing on-the-job training models. For instance, Brown et al. (2024) show that improving training quality through incentives to the trainers of on-the-job apprentices increases self-employment earnings in Ghana. We show that dual apprenticeships have sustained impacts on earnings 5 years post-program.

Third, we analyze the drivers of sustained gains in earnings. We disentangle wage employment and self-employment, and highlight a mechanism whereby skill upgrading increases earnings in self-employment and productivity. (Informal) self-employment is prevalent in low-income or lower-middle-income countries, especially for low-skill youth, yet much of the existing literature and theoretical modeling conceptualize training as a pathway to wage employment,

³There is a growing body of evidence showing impacts after 5 years or more, but it remains small relative to the number of short-run studies. Examples with long-run follow-ups include Colombia (Attanasio et al., 2017), the Dominican Republic (Ibarraran et al., 2019), Peru (Novella et al., 2025), or Chile (Doerr and Novella, 2024).

often in a formal labor market or without clearly distinguishing wage from self-employment. As such, we complement results from [Alfonsi et al. \(2020\)](#), who find that impacts of a vocational training program in Uganda arise through signaling and higher mobility in wage jobs. We also complement results from [Attanasio et al. \(2017\)](#) in Colombia, who use rich administrative data to document impacts of dual training on formal wage employment over the medium- to long-term. Informality is highly prevalent in our setting, mirroring the situation that prevails in many low-income countries. In this context, we find that dual apprenticeships build technical skills that enhance self-employment outcomes and productivity, rather than facilitating mobility into wage jobs. This highlights that training can improve labor market outcomes even when access to wage jobs remains constrained.

Lastly, our results also add to the broader literature on self-employment and entrepreneurship support interventions targeting youth or low-income individuals.⁴ A prominent class of programs provides financing or capital injections to facilitate entry into self-employment. This literature shows that returns to capital can be substantial, but highly heterogeneous and often concentrated among a subset of entrepreneurs ([de Mel et al., 2008](#); [Fafchamps et al., 2014](#); [Banerjee et al., 2019](#)). Evidence on long-term impacts is mixed, with several studies documenting attenuation of income gains over time and partial convergence toward control outcomes ([de Mel et al., 2012](#); [Blattman et al., 2020](#); [Karlan et al., 2015](#)). Relatedly, the literature on business training for microentrepreneurs documents improvements in business practices but generally finds small and heterogeneous effects on profits, with limited evidence of durable income gains or poverty reduction.⁵ In contrast, we show that skill training through dual apprenticeships leads to gains in earnings across the distribution, including at the bottom, and hence a sustained reduction in working poverty.

2 Experimental Design and Data

2.1 Intervention and Study Design

Dual apprenticeships were introduced in Côte d’Ivoire to target youth between 18 and 24 years old with low skills but an ability to read and write.⁶ The study is a randomized controlled trial implemented during the initial roll-out of the dual apprenticeship model: the program was advertised in seven urban localities (outside the capital city) and eligible youth interested

⁴For a review of entrepreneurship support programs, see [Jayachandran \(2021\)](#).

⁵For a review of business skills training programs, see [McKenzie et al. \(2025\)](#); [JPAL \(2023\)](#).

⁶The dual apprenticeship program was financed by a youth employment and skills development project (PEJEDEC; *Projet Emploi Jeune et Développement des Compétences*, www.pejedec.org). The PEJEDEC apprenticeship component was led by the office coordinating employment programs, BCP-Emploi (Bureau de Coordination des Programmes d’Emploi), with the national training agency, AGEFOP (Agence de la Formation Professionnelle), as the lead implementing agency.

in participating were randomly assigned to firms with open apprenticeship positions. Among eligible youth in the participating localities, 911 youth were assigned to treatment, and 921 to control.⁷ The online supplementary appendix (Section A.3) provides additional information on the randomized design, with more detail in Crépon and Premand (2024).

After the randomization, youth were placed in (mostly informal) small or medium enterprises for 12 to 24 months (depending on the occupation), where they received on-the-job training under the supervision of a master craftsman. Apprentices signed a contract and were paid a monthly subsidy of CFAF 30,000 (approximately US\$54, or half the formal minimum wage) from the national apprenticeship agency (AGEFOP) to cover meals and transportation. They also received insurance coverage and work equipment. The apprenticeship included a dual training component—practical, on-the-job training was complemented with center-based training in technical skills delivered by local training institutions (approximately 180 hours per year). AGEFOP defined the general skills apprentices needed to learn in each trade and oversaw the development of training curricula. AGEFOP apprenticeship counselors regularly visited master craftspeople and apprentices to track skills acquisition through a booklet detailing learning milestones for each trade. Dual apprenticeships ended with a skills assessment, with the possibility of certification. Crépon and Premand (2024) document how the introduction of the dual model increased entry into apprenticeship, with 75% of treated youth participating over the course of the experiment and limited crowding-out effects.

2.2 Timeline and Data

We collected baseline data between July and October 2014 among youth who expressed interest in the program and were confirmed as eligible after an interview organized by AGEFOP. Randomization was performed shortly after, and placements were mostly completed by January 2015.

The first youth follow-up survey was collected between March and June 2016, approximately 20 months after the start of the program, when most treated youth were still participating.

⁷The implementing agency first identified firms interested in hosting program apprentices and how many open positions they had. The agency then registered interested, eligible youth in the experiment. The design was stratified by micromarkets, which were each defined as a trade in a given locality. In each micromarket, as many youth were registered as there were open positions. Firms were then randomly assigned to treatment and control groups to have an equal number of treatment and control positions in each micromarket. Once treatment firms were drawn, the number of positions to be filled in each micromarket was obtained. 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. 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. Across the seven localities in the study, 731 firms offered apprenticeship positions, and 361 were randomly selected to host program apprentices. Effects on firms were measured in the short-term and are discussed in Crépon and Premand (2024). In this paper, we focus on medium- to long-term impacts on youth.

The second follow-up survey was collected between May and October 2018, approximately 4 years after the start of the experiment, and 2 years after the end of the program.⁸ Our third follow-up survey took place in 2019 and was finalized in January 2020, approximately 3 years post-program and just before COVID-19 reached West Africa. Our fourth survey took place between July and September 2021, approximately 5 years post-program.

The COVID-19 pandemic had minimal lasting effects on employment and income in Côte d’Ivoire. Mobility restrictions were implemented from March to May 2020, but economic activity had recovered to pre-pandemic levels by December 2020 (Dupas et al. (2023)). Since our sample completed their dual apprenticeships in 2016 - well before the pandemic - and we did not collect data during the year affected by mobility restrictions, our results do not provide information on the dynamics during that period. The long-term impact estimates are unlikely to be affected by confounding effects since both our treatment and control groups faced similar circumstances.

Table A1 documents baseline balance for the effective sample at each follow-up. Youth are on average 20.8 years old at baseline, and 64% have only completed primary education. Of the individuals in the sample, 87% are male, which is driven by the sectors prioritized by the implementing agency for the wave of the program being studied.⁹ Furthermore, 87% of youth report engaging in some income-generating activity at baseline, reflecting the employment situation among low-income individuals across the region, who are often active in casual jobs or self-employment but may not be able to afford remaining unemployed.¹⁰ Of youth in the baseline sample, 44% aspire to a wage job, and 56% to self-employment.

All youth follow-up surveys were collected by phone, but substantial efforts were made to minimize attrition, including some in-person tracking. As a result, 1,666 youth were interviewed in the first follow-up survey (response rate in 2016: 90.9%), 1,676 in the second (response rate in 2018: 91.5%), 1,623 in the third (response rate in 2019: 88.6%), and 1,581 in the fourth (response rate in 2021: 86.3%) (see Table A2). On average, youth were interviewed 4.3 times over the course of the experiment, and the number of times they were interviewed is balanced between treatment and control.¹¹ We note that there is a small imbalance in attrition between the treatment and control groups in 2019 that is significant at the 5% level, but the difference in response rates remains relatively small (less than 3 percentage points). Importantly, we show

⁸The survey was collected 18–24 months after the end of the program, but we describe it as 2 years post-program for simplicity.

⁹This issue was later addressed, with higher female participation in subsequent phases of the program that are outside the scope of the RCT.

¹⁰See for instance Filmer et al. (2014) or Christiaensen and Premand (2017).

¹¹The first row of Table A2 presents the balance check for the share of youth with available baseline data. We show this because an unfortunate issue with the online server used for electronic data collection led to the loss of baseline data for approximately 25% of the youth. The problem was concentrated in two localities. The loss of some baseline data limited the availability of contact information to track the youth. This contributed to a lower response rate among youth in localities where these issues occurred.

results separately for each survey wave, and find largely consistent patterns in 2018, 2019, and 2021, which indicates that the small differential attrition in 2019 is unlikely to affect the overall results.

2.3 Estimation

We estimate ITT impacts on youth by comparing outcomes between those assigned to dual apprenticeships and control. We run an inversely propensity-weighted regression with strata dummies on the youth observations:¹²

$$y_{i,t} = \alpha + \beta T_i + \sum_s \mu_s \mathbf{1}\{i \in s\} + \delta_t + u_{i,t}. \quad (1)$$

We compute Huber–White robust standard errors. We report results on the main outcomes related to earnings, employment, labor productivity, working poverty, and task complexity. We further disentangle the impacts on employment and earnings between wage and self-employment. The Supplementary Appendix (Section A.4) provides additional information on the main outcome measures.

Equation (1) is estimated either separately for each survey wave—during the program (2016) and post-program (2018, 2019, and 2021)—or on pooled post-program data, in which case β captures the average post-program effect and standard errors are clustered at the individual level. Pooling across post-program waves reduces the number of hypotheses tested when drawing inference about medium to long-term effects.

We consider a set of eight primary outcomes, which together capture the main empirical results. We apply multiple-hypothesis testing adjustments to their pooled post-program estimates using the false discovery rate (FDR) based on Simes-adjusted q -values. The eight primary outcomes are: total earnings, earnings from wage employment, earnings from self-employment, participation in wage employment, participation in self-employment, labor productivity (hourly earnings), a non-routine task intensity index, and an indicator for the prevalence of extreme working poverty defined as having earnings below half the minimum wage (30,000 FCFA). Table A3 consolidates the estimates for these main outcomes, their associated p -values and adjusted q -values.

¹²As explained in the online supplementary appendix (Section A.3) and in more detail in Crépon and Premand (2024), youth were assigned to treatment and control groups with probabilities specific to each trade in each locality, producing a set of corresponding strata s . We compute the empirical assignment rate in each stratum $\widehat{\pi}_m$ and estimate inversely propensity-weighted regressions. The weights are defined as $T_i/\widehat{\pi}_m + (1 - T_i)/(1 - \widehat{\pi}_m)$.

3 Results

3.1 Higher Earnings and Lower Working Poverty

Figure 1 illustrates how dual apprenticeships impact youth earnings during the program (in 2016) and in the medium to long-term up to 5 years post-program (until 2021). Treated youth have similar total earnings to the control group during the program. However, dual apprenticeships have positive and sustained effects on post-program earnings. Two years after the end of the program, treated youths earn 9,394 FCFA more per month, a 15% increase relative to the control group. These gains in total earnings are sustained 3 years (+11,505 FCFA, or 19.6%) and 5 years (+12,127 FCFA, or 13.5%) after the end of the program. Pooling the post-program survey waves, earnings are higher by 10,973 FCFA, or 15.7%.¹³

Importantly, gains in earnings are also observed at the bottom of the distribution: the incidence of (extreme) working poverty decreases substantially. Indeed, dual apprenticeships decrease the share of youth earning less than half the formal minimum wage by 7.3 percentage points post-program (or 20 percent from a base extreme working poverty rate of 37%), and the share of those earnings less than the formal minimum wage by 6.9 percentage points (or 11 percent from a base working poverty rate of 62%) (Table 1, columns 2 and 4, bottom panel). Reductions are also found in the depth of extreme working poverty (by 20.8 percent) and working poverty (by 15.9 percent).¹⁴ Since 98% of youth in the control group work, and the intervention only has a very small marginal effect on the share of youth in employment (see discussion in Section 3.3), these results mean that treated youth are less likely to be working in low-paying jobs post-program, either those paying below the formal minimum wage or below half the formal minimum wage.¹⁵

Figure A1 also shows that dual apprenticeships reduce the share of youth spending multiple spells in extreme working poverty. The share of youth who are never in extreme working poverty decreases by 9 percentage points. Looking at the total number of spells in extreme working poverty during the study period (using the 30,000 FCFA cutoff), the mean number of spells is 0.98 (SD = 0.96) in the control group, and the treatment effect is -0.24 (SE = 0.05), pointing to a 24 percent reduction. The corresponding decrease in the number of spells in working poverty is 13 percent.¹⁶

Figure A2 shows the distribution of total earnings for each post-program survey wave, as well

¹³For the estimates, see Table 3, column 1.

¹⁴The depth of working poverty is calculated as the average of the difference between earnings and the formal minimum wage across sample individuals in poverty, as in Foster et al. (1984).

¹⁵This is partly driven by a reduction in the share of youth who work without earning any income, which can be the case for occupations such as family helpers (Table 1, column 1).

¹⁶Using the 30,000 FCFA cutoff, the control mean is 1.60 (SD = 1.10) and the treatment effect is -0.21 (SE = 0.05), a 13 percent reduction.

as in the pooled post-program sample. This clearly reveals an overall shift in the distribution, which drives the reduction of working poverty discussed above. Indeed, based on a rank-sum test, we can clearly reject the equality of the distributions for each round ($p < 0.01$). The results also illustrate that the effects are not driven by the top of the distribution, where impacts are not stronger than at the bottom. For instance, impacts on the share of individuals with high earnings (more than 3 times the formal minimum wage) are not significant at any point in time, and only increase by 1.4 percentage points in the pooled post-program sample (see Table 1, column 6).

3.2 Higher Task Complexity and Productivity

How do youth get higher earnings? Table 2 (columns 1-4) documents impacts on the complexity of tasks undertaken by youth in their primary occupation. Indices are built based on the “task approach” to estimate the skill content of occupations at the worker level developed by Autor et al. (2003), and later adapted to large-scale surveys in developing countries by Dicarolo et al. (2016). The survey module covers items related to non-routine analytical (NRA), non-routine interpersonal (NRI), and routine tasks (R). Within each family, we (i) standardize every item to a z-score so higher values reflect greater task intensity, (ii) sum standardized items to form a sub-index, and (iii) normalize the sub-index to have unit variance and control mean centered at zero to facilitate interpretation. This yields three indices of routine (R), non-routine analytical (NRA), and non-routine interpersonal (NRI) tasks, along with a summary Routine Task Intensity index defined as $RTI=R-NRA-NRI$.¹⁷ Negative treatment effects on the Routine Task Intensity (RTI) index therefore measure the extent to which the intervention shifts participants away from performing routine tasks and towards performing more non-routine tasks.

Across the three pooled post-program survey waves, there is no change in the intensity of routine tasks (R, column 1) between treatment and control. In contrast, treated youth are more likely to undertake non-routine abstract tasks (NRA, column 2) by 0.17 standard deviations and non-routine interpersonal tasks (NRI, column 3) by 0.1 standard deviations. Therefore, the overall index of routine task intensity (RTI, column 4) decreases by 0.27 standard deviations. This means that youth have a higher ability to perform complex tasks in their primary occupation, which also indicates higher technical skills.¹⁸

The last column of Table 2 further documents impacts on hourly earnings, a direct proxy for productivity. Across the post-program survey waves, dual apprenticeships increase hourly earnings by 60.8 FCFA from a base of 414 FCFA. This indicates gains in productivity in

¹⁷See the Supplementary Appendix (Section A.4.2) for additional information on the construction of these indices.

¹⁸Table A4 provides disaggregated results for items within each index.

the order of 15 percent and is consistent with youth performing more complex tasks in their occupations, as well as higher overall earnings.

3.3 Higher Earnings in Self-Employment but Not Wage Employment

Table 3 disentangles the impacts on total earnings discussed above (column 1) between apprenticeship (column 2), wage employment (column 3), self-employment (column 4), and non-labor (column 5) earnings. Table 4 then displays impacts on the share of youth employed (column 1), the total number of activities in which youth work (column 2), and the share of youth employed in apprenticeship (column 3), wage employment (column 4) and self-employment (column 5). Together, these two tables shed light on the sources of the earnings gains.

During the program, treated youth have higher non-labor earnings, which include transfers from the wage subsidies paid by the national apprenticeship agency, and slightly higher earnings from apprenticeships, which include direct payments made by employers to motivate apprentices. These sources of income related to apprenticeship offset a substantial decrease in earnings from wage employment and self-employment. Taken all together, this results in an overall null effect on total earnings during the program (Table 3, column 1, top panel).

Some impacts on earnings from apprenticeship continue to be observed 2 years after the end of the program (+3,593 FCFA), and to a lesser extent 3 years post-program (+1,530 FCFA), before converging to a null effect 5 years post-program (Table 3, column 2). These results reflect a gradual transition out of apprenticeship. As the program ends, some youth remain in firms as (informal) apprentices before slowly moving on to other occupations. To illustrate, the share of treated youth in apprenticeship remains higher than control two years after the program (Table 4, column 3), before fading out as youth exit their apprenticeship.

Importantly, the post-program impacts on total earnings are driven by higher earnings from self-employment, which increase by 6,138 FCFA per month, or 26.7 percent on average from a mean in control of 22,950 FCFA (Table 3, column 4). In fact, significant impacts on earnings from self-employment are observed in the three post-program survey waves, pointing to sustained effects. These impacts on earnings in part stem from the gains in productivity outlined above. They also reflect a slightly higher share of youth in self-employment post-program, by 4.7 percentage points across the three survey waves, as shown in the last column of Table 4 (bottom panel).

In contrast, we do not observe significant effects on earnings from wage employment at any point in time after the program. The point estimate is very close to zero two years post-program, and small but far from statistically significant 3 and 5 years post-program (Table 3, column 3). These results highlight that the gains in total earnings are not explained by wage

employment, but rather are mostly driven by self-employment. Similarly, there is no change in the share of youth active in wage employment (Table 4, column 4).¹⁹ This makes clear that gains in earnings and productivity are derived from self-employment.²⁰

Overall, dual apprenticeships only have a marginal effect on the share of youth employed in our high-informality, low-income setting. Of control youth, 98% are employed post-program, although mostly in informal occupations. While statistically significant, the treatment effect on overall employment is just 1.1 percentage point. There is only a slightly larger increase in the number of income-generating activities, by 0.079 from a control mean of 1.13, consistent with additional self-employment activities (Table 4, columns 1 and 2).

Table A5 presents the results for hours worked to complement those for employment. Control youth work close to 40 hours per week, in line with the high overall employment rate in the population. Dual apprenticeships slightly increase hours worked by 2.7 hours per week, or 7 percent. This partly reflects more time worked in apprenticeship immediately after the end of the program, which compensates for a small reduction of hours in wage employment. In contrast, hours worked in self-employment remain largely stable.²¹

3.4 Robustness to Multiple Hypothesis Testing

Our main analysis focuses on post-program impacts on total earnings, the incidence of (extreme) working poverty at the bottom of the earnings distribution, employment in wage or self-employment and associated earnings, as well as labor productivity (hourly earnings) and task complexity. These dimensions correspond to the eight core outcomes reported across the main tables. Together, these outcomes underpin our main empirical claims regarding the existence of sustained post-program effects on earnings, including for youth at the lower end of the

¹⁹Treated youth also have higher reservation wages, suggesting that they would only enter other wage jobs if they are offered higher salaries. Self-declared reservation wages are high to start with, but dual apprenticeships further increase them by 7.5 percent (Table A7, column 1). This may in part explain why the share of youth in wage jobs does not increase, since they now have higher salary requirements, possibly due to the observed earnings gains in other occupations.

²⁰While dual apprenticeships lead to a sustained improvement in earnings and a reduction in the prevalence of (extreme) working poverty, Table A7 only documents marginal changes in dimensions of job satisfaction other than earnings at the 7-year follow-up survey. A slightly higher share of treated youth report an aspiration to remain in the exact same job in the future (+3.5pp, or 23 percent from a control mean of 15%). Even if youth are more prone to like their current job, we do not observe broader changes in an index of job satisfaction that also captures pride or job reputation. Youth aspirations between wage and self-employment also remain similar between treatment and control youth over time. Participation in dual apprenticeships does not affect their aspiration to change occupation. A slight preference for self-employment relative to wage employment is observed at baseline. While it decreases over time as youth get older, participation in dual apprenticeship does not have a significant effect on these aspirations at any point in time (Table A8).

²¹Table A6 presents results on hourly earnings by type of occupation. The pooled results show an increase in hourly earnings in self-employment, consistent with higher productivity in that sector, given higher earnings (Table 3) but constant hours (Table A5). In contrast, there is no significant effect on hourly earnings in wage employment.

earnings distribution, and how these effects operate through self-employment rather than wage employment, with related productivity gains.

For these eight outcomes, we estimate treatment effects using pooled data from the three post-program survey waves and compute standard errors clustered at the individual level. We first test the null hypothesis of no effect using conventional two-sided t -tests. Given the multiple hypotheses being tested, we also account for multiple inference by adjusting p -values using the Simes procedure to control the false discovery rate (FDR).

Table A3 contains the results. The first column reports the estimated coefficients, with corresponding standard errors in the second column. The third column presents conventional (unadjusted) p -values (as in the main results tables), while the last column reports FDR-adjusted q -values. Results are ordered by increasing p -value. The table shows that, using conventional tests that ignore multiple hypothesis testing, all null hypotheses are rejected except those related to wage employment—both the probability of holding a wage job and earnings from wage employment. Importantly, adjusting for multiple testing does not alter the main results. We reject the null hypothesis of no post-program effect on total earnings, on the incidence of extreme working poverty, on labor productivity and task complexity, and on self-employment and self-employment earnings. By contrast, we fail to reject the null hypothesis of no effect on wage employment and its associated earnings.

3.5 Cost-effectiveness

The results show gains in earnings in the range of 14 to 20 percent. This is above a benchmark return to a year of education (8%), relative to which McKenzie (2021) compares the magnitude of impacts of (business) training programs. Using cost data collected during the program, we can analyze cost-effectiveness more precisely. The dual apprenticeship program cost CFAF 654,817 (approximately US\$1,180) per sample youth.²² Figure A3 presents benefit-cost ratios that account for impacts on total earnings up to the follow-up survey collected 5-years post-program, with various dissipation scenarios thereafter. The benefit-cost ratio is 81% assuming a 5% discount rate and considering only impacts up to the 2021 endline (see Figure A3, 100% dissipation scenario after the endline survey). This means that the intervention had not yet passed the break-even point by the time of the 5-year post-program survey. However, the program becomes cost-effective if impacts dissipate by 40% (or less) after that. Given the sustained effects found 2-5 years post-program, expecting such partial additional persistence

²²The program cost CFAF 1,135,030 (approximately US\$2,045) for the entire 24 months. 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.). Since some youth dropped out before the end of the program, the average cost per sample youth is CFAF 654,817. See Crépon and Premand (2024) for more detail.

does not appear unrealistic. These results highlight a broader point: even for a relatively effective training program, the time path of impacts over a relatively long period of time needs to be considered to reach cost-effectiveness.

4 Conclusion

This paper contributes to the literature on skills training by providing rare experimental evidence of sustained impacts on earnings in a highly informal labor market. While much of the existing findings on training programs document mixed or modest average effects, we show that a dual model combining extensive on-the-job apprenticeships with vocational training generates earnings gains that persist 5 years after program completion. These results add to recent work emphasizing the importance of training quality and demonstrate that well-designed apprenticeship models can deliver durable returns.

Our findings also contribute to shedding light on the mechanisms through which training affects employment outcomes in low-income economies. Rather than operating through wage employment, the gains we document arise primarily through higher earnings in self-employment. Dual apprenticeships enable youth to perform more complex and non-routine tasks, consistent with sustained improvements in technical skills and productivity. This highlights that self-employment not only involves subsistence activities but also offers skilled occupations and productive employment opportunities. In this context, dual apprenticeships provide a meaningful pathway for skill upgrading and growth in self-employment earnings. As such, the results are noteworthy as they highlight a mechanism for youth to secure better-paying jobs that can arise even in the absence of job creation in the formal private sector.

Finally, this paper speaks to the broader challenge of reducing working poverty in developing economies. We show that earnings gains from dual apprenticeships are broad-based and translate into a sustained reduction in both working poverty and extreme working poverty. Dual apprenticeships thus constitute a promising element of strategies to improve employment opportunities in low-income settings where formal wage jobs remain scarce.

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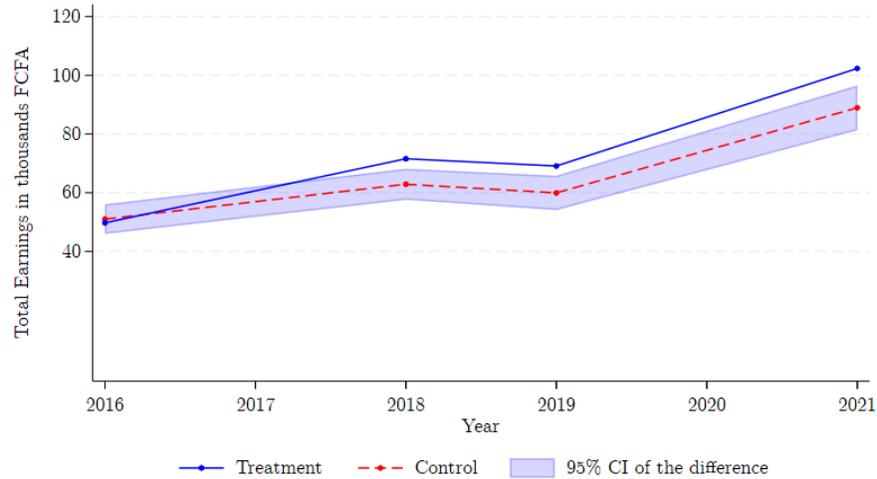
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5 Main Exhibits

Figure 1: Evolution of Total Earnings by Treatment Status



Source: Youth follow-up surveys from 2016 to 2021.

Notes: The figure reports average monthly earnings (in FCFA) for the control group and implied average earnings for the treatment group, constructed as the sum of the control group mean and the corresponding ITT estimates from equation (1). Estimation includes strata fixed effects and weights observations by the inverse of the probabilities of assignment by strata (see footnote 12). The shaded area around the control mean represents the 95% confidence interval of the estimated treatment effect based on Huber–White robust standard errors. Results are shown for earnings during the program (2016) and post-program (2018, 2019, and 2021), with earnings winsorized at the 99th percentile. See estimates in column 1 of Table 3.

Table 1: Impacts at the Bottom of the Earnings Distribution and on Working Poverty

	(1)	(2)	(3)	(4)	(5)	(6)
	Youth with earnings = 0	Youth with earnings < 30k	Depth of extreme working poverty (< 30k)	Youth with earnings < 60k	Depth of working poverty (< 60k)	Youth with earnings > 180k
+0 years (2016)						
Treatment	-0.044*** (0.015)	-0.131*** (0.024)	-0.103*** (0.018)	0.044** (0.021)	-0.055*** (0.017)	-0.005 (0.009)
Control mean	0.12	0.47	0.30	0.73	0.46	0.04
Observations	1,661	1,661	1,661	1,661	1,661	1,661
+2 years (2018)						
Treatment	-0.043*** (0.014)	-0.066*** (0.024)	-0.048*** (0.017)	-0.069*** (0.024)	-0.058*** (0.018)	0.011 (0.013)
Control mean	0.11	0.38	0.25	0.65	0.39	0.07
Observations	1,670	1,670	1,670	1,670	1,670	1,670
+3 years (2019)						
Treatment	-0.058*** (0.018)	-0.099*** (0.024)	-0.075*** (0.019)	-0.073*** (0.024)	-0.083*** (0.019)	0.017 (0.012)
Control mean	0.18	0.44	0.31	0.68	0.45	0.06
Observations	1,612	1,612	1,612	1,612	1,612	1,612
+5 years (2021)						
Treatment	-0.018 (0.014)	-0.053** (0.023)	-0.032* (0.017)	-0.063** (0.025)	-0.046** (0.018)	0.014 (0.017)
Control mean	0.09	0.30	0.20	0.52	0.31	0.13
Observations	1,570	1,570	1,570	1,570	1,570	1,570
Post-Program (Pooled, 2018-2021)						
Treatment	-0.040*** (0.010)	-0.073*** (0.016)	-0.052*** (0.012)	-0.069*** (0.017)	-0.062*** (0.013)	0.014 (0.009)
Control mean	0.13	0.37	0.25	0.62	0.39	0.08
Time FE	{Yes}	{Yes}	{Yes}	{Yes}	{Yes}	{Yes}
Observations	4,852	4,852	4,852	4,852	4,852	4,852

Source: Youth surveys from 2016 (during the program), 2018, 2019 and 2021 (post-program).

Notes Estimation results from equation (1). We include strata fixed effects and weight observations by the inverse of the probabilities of assignment by strata (see footnote 12). The first three panels present estimation results separately for each year. The last panel presents pooled estimates based on the three post-program surveys. We report Huber–White robust standard errors (clustered at the youth level for the bottom panel).

The threshold of 60,000 FCFA (Column (4) and (5)) corresponds to the legal minimum wage; as such 30,000 FCFA (Column (2) and (3)) is half of the minimum wage, and 180,000 FCFA is 3 times the minimum wage (Column (6)).

Depth of poverty measures use Foster–Greer–Thorbecke (Foster et al., 1984) gaps relative to the stated threshold.

Table 2: Task Complexity Indices and Productivity

	(1)	(2)	(3)	(4)	(5)
	Routine (R)	Non-Routine Abstract (NRA)	Non-routine Interpersonal (NRI)	Routine Task Index(RTI)	Hourly Earnings (Total Labor)
+2 years (2018)					
Treatment	0.110** (0.047)	0.251*** (0.050)	0.079* (0.047)	-0.303*** (0.097)	33.948 (57.227)
Control mean	0.00	0.00	0.00	0.00	519.05
Observations	1,670	1,670	1,670	1,670	1,670
+3 years (2019)					
Treatment	0.014 (0.049)	0.146*** (0.050)	0.080* (0.048)	-0.252** (0.098)	110.996*** (31.429)
Control mean	0.00	0.00	0.00	0.00	323.25
Observations	1,584	1,584	1,584	1,584	1,612
+5 years (2021)					
Treatment	0.028 (0.051)	0.112** (0.050)	0.153*** (0.049)	-0.263*** (0.099)	49.981 (62.531)
Control mean	0.00	0.00	0.00	0.00	627.39
Observations	1,570	1,570	1,570	1,570	1,569
Post-Program (Pooled, 2018-2021)					
Treatment	0.051 (0.033)	0.170*** (0.036)	0.102*** (0.032)	-0.271*** (0.065)	60.825*** (20.744)
Control mean	-0.00	-0.00	0.00	-0.00	414.20
Time FE	{Yes}	{Yes}	{Yes}	{Yes}	{Yes}
Observations	4,852	4,852	4,852	4,852	4,851

Source: Youth surveys from 2018, 2019 and 2021 (post-program).

Notes: Task complexity measures are based on tasks undertaken by youth in their primary occupation, using the approach adapted from [Autor et al. \(2003\)](#), [Autor and Handel \(2013\)](#), [Dicarlo et al. \(2016\)](#). Indices are standardized (mean 0, SD 1) in the pooled post-program sample. More details are given in [Appendix A.4.2](#). Hourly earnings are computed as (monthly labor earnings)/(4.33×weekly hours).

Estimation results from equation (1). We include strata fixed effects and weight observations by the inverse of the probabilities of assignment by strata (see footnote 12). The first three panels present estimation results separately for each year. The last panel presents pooled estimates based on the three post-program surveys. We report Huber–White robust standard errors (clustered at the youth level for the bottom panel).

Table 3: Earnings

	(1)	(2)	(3)	(4)	(5)
	Total	Apprenticeship	Wage empl.	Self-empl	Non-Labor
+0 years (2016)					
Treatment	-1,408 (3,295)	3,238*** (749)	-6,414*** (1,407)	-4,157** (1,959)	10,213*** (870)
Control mean	51,484	4,746	15,398	14,279	7,540
Observations	1,661	1,660	1,661	1,661	1,661
+2 years (2018)					
Treatment	9,394** (3,928)	3,593*** (1,004)	544 (2,133)	4,512* (2,711)	551 (1,078)
Control mean	62,747	5,770	20,650	19,350	8,925
Observations	1,670	1,670	1,670	1,668	1,670
+3 years (2019)					
Treatment	11,505*** (4,252)	1,530* (835)	1,730 (2,310)	5,576** (2,404)	1,167 (1,147)
Control mean	58,720	4,340	23,093	15,111	8,812
Observations	1,612	1,611	1,606	1,608	1,611
+5 years (2021)					
Treatment	12,127** (6,070)	113 (671)	2,345 (3,089)	8,729* (4,831)	164 (1,382)
Control mean	89,704	3,363	32,558	34,961	11,604
Observations	1,570	1,569	1,570	1,565	1,570
Post-Program (Pooled, 2018-2021)					
Treatment	10,973*** (3,401)	1,787*** (579)	1,474 (1,734)	6,138*** (2,350)	630 (780)
Control mean	70,100	4,520	25,263	22,950	9,751
Time FE	{Yes}	{Yes}	{Yes}	{Yes}	{Yes}
Observations	4,852	4,852	4,852	4,852	4,852

Source: Youth surveys from 2016 (during the program), 2018, 2019 and 2021 (post-program).

Notes Estimation results from equation (1). We include strata fixed effects and weight observations by the inverse of the probabilities of assignment by strata (see footnote 12). The first four panels present estimation results separately for each year. The last panel presents pooled estimates based on the three post-program surveys. We report Huber–White robust standard errors (clustered at the youth level for the bottom panel).

Earnings are winsorized at the 99th percentile.

Table 4: Employment

	(1)	(2)	(3)	(4)	(5)
	At least one Act.	Total N Act.	Apprenticeship	Wage empl.	Self-empl
+0 years (2016)					
Treatment	0.034*** (0.013)	0.053* (0.031)	0.365*** (0.022)	-0.135*** (0.022)	-0.072*** (0.021)
Control mean	0.91	1.19	0.18	0.36	0.29
Observations	1,661	1,661	1,661	1,661	1,661
+2 years (2018)					
Treatment	0.013** (0.006)	0.082*** (0.030)	0.094*** (0.020)	-0.036 (0.024)	0.044* (0.023)
Control mean	0.98	1.23	0.17	0.38	0.32
Observations	1,670	1,670	1,670	1,670	1,670
+3 years (2019)					
Treatment	0.014** (0.006)	0.095*** (0.029)	0.014 (0.018)	0.008 (0.024)	0.050** (0.023)
Control mean	0.98	1.02	0.14	0.36	0.28
Observations	1,612	1,612	1,612	1,612	1,612
+5 years (2021)					
Treatment	0.006 (0.008)	0.058** (0.028)	0.000 (0.015)	0.010 (0.025)	0.048* (0.025)
Control mean	0.97	1.12	0.09	0.40	0.38
Observations	1,570	1,570	1,570	1,570	1,570
Post-Program (Pooled, 2018-2021)					
Treatment	0.011** (0.004)	0.079*** (0.020)	0.037*** (0.013)	-0.007 (0.017)	0.047*** (0.017)
Control mean	0.98	1.13	0.14	0.38	0.33
Time FE	{Yes}	{Yes}	{Yes}	{Yes}	{Yes}
Observations	4,852	4,852	4,852	4,852	4,852

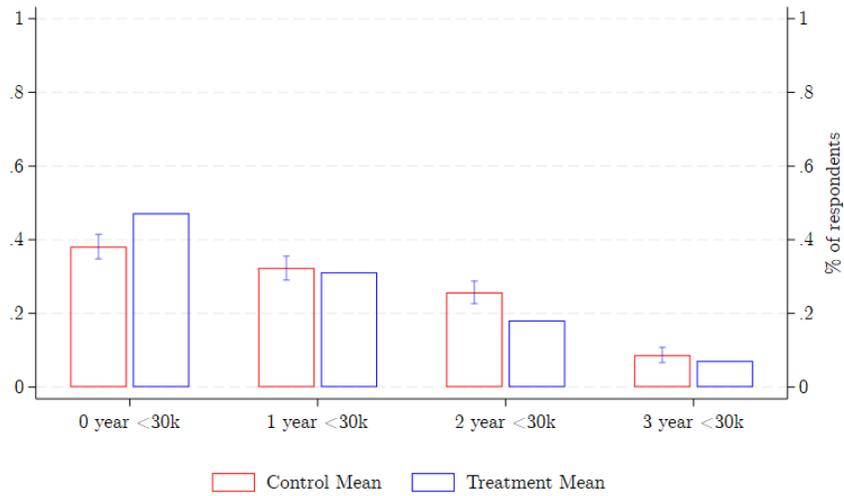
Source: Youth surveys from 2016 (during the program), 2018, 2019 and 2021 (post-program).

Notes Estimation results from equation (1). We include strata fixed effects and weight observations by the inverse of the probabilities of assignment by strata (see footnote 12). The first four panels present estimation results separately for each year. The last panel presents pooled estimates based on the three post-program surveys. We report Huber–White robust standard errors (clustered at the youth level for the bottom panel).

A Supplementary Appendix

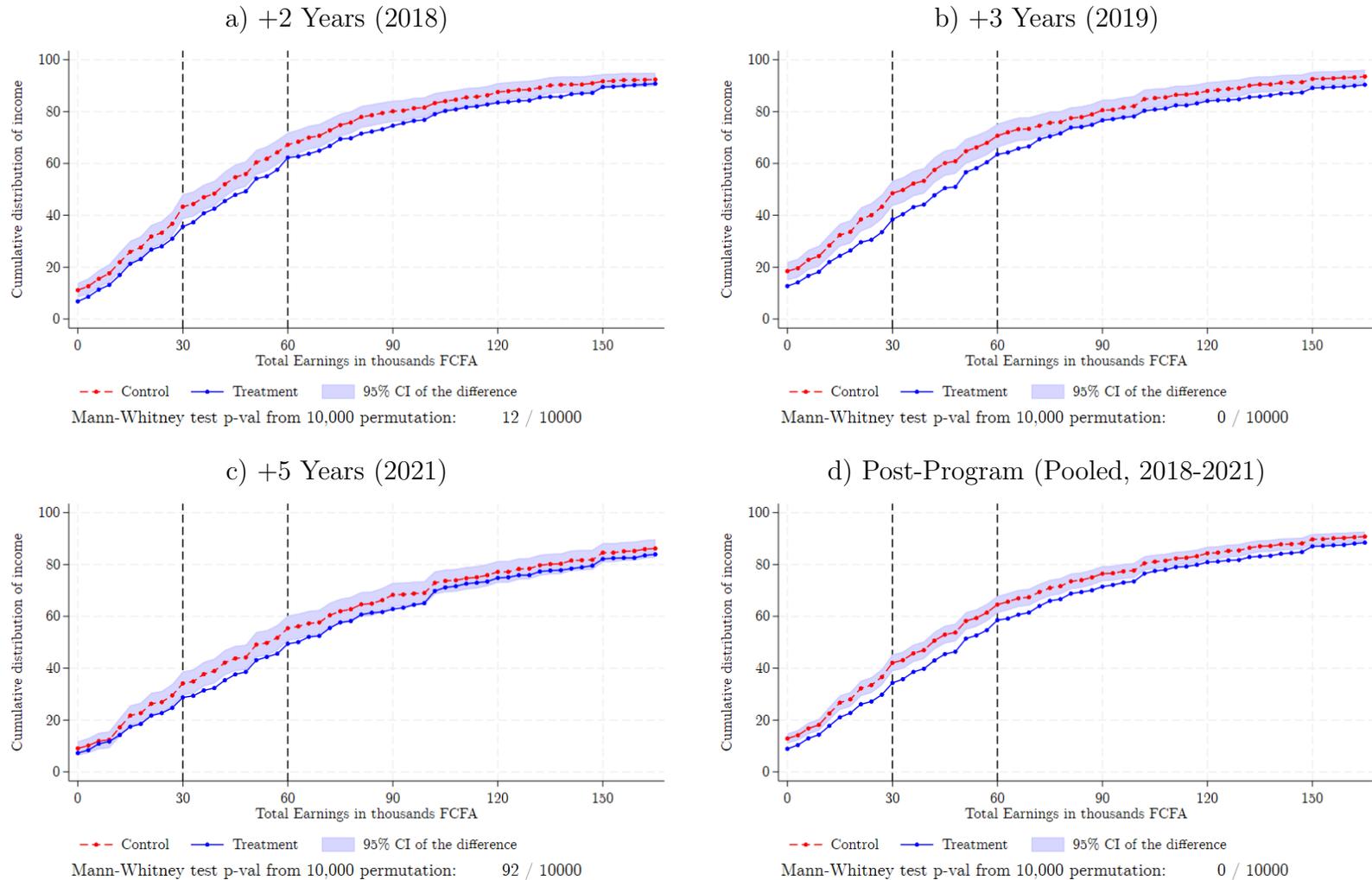
A.1 Supplementary Appendix Figures

Figure A1: Number of Spells in Extreme Working Poverty



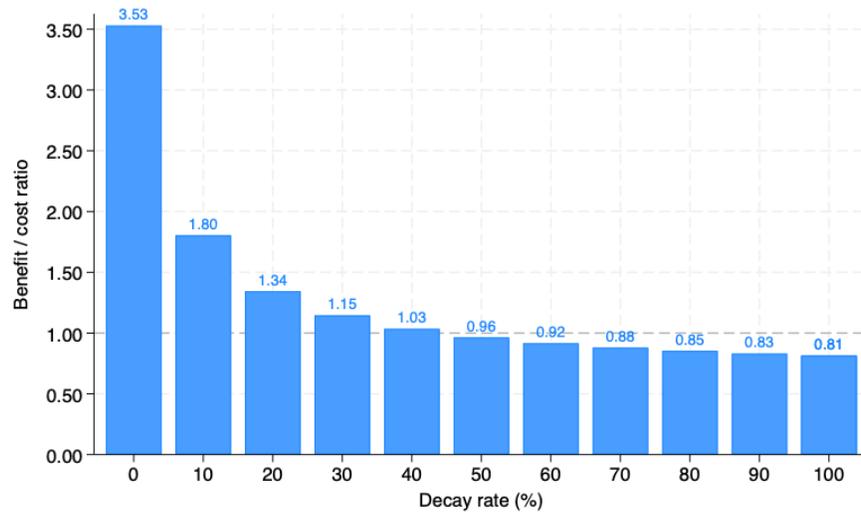
Notes: Count of spells (survey waves) with monthly earnings below half the formal minimum wage (30,000 FCFA). Spells computed across post-program rounds; distribution shown by treatment status, with 95% Confidence Interval whiskers.

Figure A2: Distribution of Total Earnings



Note: The vertical lines mark the minimum wage at 60,000 FCFA and half of the minimum wage at 30,000 FCFA. The figures show the results of the estimation of the cumulative distribution of potential outcomes in the treatment and control groups. Income is winsorized at the 99th percentile. The dotted curve shows the average of those variables in the control group. The solid curve adds to this average the estimated treatment coefficient. The grey area around the dotted curve represents a band of ± 1.96 times the standard error of the estimated treatment effect.

Figure A3: Benefit-cost ratio, assuming different decay rates after 2021



Notes: The yearly benefits at two, three, and five years after treatment come from the monthly estimates in Table 3, multiplied by twelve. To estimate the effect in the first year after treatment, we assume it is equal to the effect in the second year. The effect in the fourth year is a simple average between effects in the third and fifth years. We assume that the effect in the fifth year holds for 35 more years, under different yearly decay rates, from 100% to 0%. The discount rate is 5%.

A.2 Supplementary Appendix Tables

Table A1: Baseline Balance (for effective sample at each follow-up)

Baseline characteristics	2016		2018		2019		2021		Joint test
	Cont	Diff	Cont	Diff	Cont	Diff	Cont	Diff	p-val
Demographics									
Male	0.87	-0.02	0.86	-0.01	0.87	-0.02	0.87	-0.02	0.02
Age	20.75	0.08	20.77	0.05	20.73	0.10	20.71	0.13	0.53
Married	0.02	0.00	0.02	0.00	0.01	0.01	0.01	0.00	0.75
No diploma	0.20	-0.00	0.20	0.01	0.20	0.00	0.19	0.01	0.84
Primary education	0.64	0.00	0.64	-0.01	0.63	0.00	0.65	-0.01	0.32
Lower sec education or above	0.16	0.00	0.16	-0.00	0.17	-0.01	0.16	-0.00	0.02
Has received training	0.22	0.04	0.22	0.04	0.21	0.04	0.21	0.04	0.26
Skills									
Skill Index (All)	1.72	0.01	1.72	0.00	1.71	0.01	1.71	0.02	0.39
Cognitive Skill Index	0.79	-0.00	0.79	-0.01	0.79	0.00	0.79	0.01	0.44
Non Cognitive Skill Index	0.93	0.01	0.93	0.01	0.93	0.01	0.92	0.01	0.80
Economic Activity									
Has activity	0.87	0.01	0.87	0.00	0.87	-0.00	0.87	0.00	0.05
N of agricultural act	0.21	-0.03	0.22	-0.04	0.21	-0.03	0.21	-0.02	0.00
Total nb of activities	1.38	-0.02	1.40	-0.03	1.39	-0.02	1.39	-0.00	0.10
Nb of non agricultural act	1.17	0.01	1.19	0.01	1.18	0.00	1.18	0.02	0.32
Total income (KCFA)	70.86	5.21	69.46	4.34	70.44	4.68	72.50	1.81	0.15
Total income (hyperb sin)	3.27	-0.07	3.24	-0.05	3.25	-0.07	3.25	-0.09	0.53
Employment aspirations									
Searching for a job	0.44	0.01	0.44	0.02	0.43	0.02	0.44	0.02	0.77

... continued

	Cont	Diff	Cont	Diff	Cont	Diff	Cont	Diff	p-val
Aspires to wage job	0.46	0.01	0.46	0.00	0.46	0.01	0.45	0.01	0.77
Aspires to self-employment	0.54	-0.01	0.53	-0.00	0.53	-0.01	0.54	-0.01	0.69
N of hh members in wage jobs	0.70	-0.04	0.68	-0.02	0.67	-0.01	0.67	-0.02	0.63
Has relatives in wage jobs	0.51	0.02	0.51	0.01	0.51	0.01	0.51	0.01	0.46
Has friends in wage jobs	0.53	0.03	0.52	0.04	0.52	0.03	0.52	0.02	0.83
N of hh members with IGA	1.78	-0.02	1.77	-0.02	1.80	-0.05	1.78	-0.02	0.33
Has relatives with IGA	0.71	-0.01	0.71	-0.00	0.71	-0.01	0.71	-0.01	0.80
Has friends with IGA	0.78	-0.07	0.78	-0.08	0.78	-0.08	0.78	-0.07	0.04
Financial constraints									
N financial constraints	2.83	-0.05	2.75	0.02	2.73	0.04	2.78	-0.03	0.98
Saved during last 3 months	0.49	-0.01	0.48	0.01	0.49	-0.00	0.48	-0.01	0.48
Has saving account	0.05	0.01	0.05	0.01	0.05	0.00	0.05	-0.01	0.10
Has debt	0.31	0.01	0.32	0.01	0.32	0.01	0.32	0.02	0.27
Has problem paying back debt	0.16	0.00	0.17	-0.00	0.17	-0.01	0.17	0.00	0.30
Is credit constrained	0.52	-0.03	0.51	-0.02	0.51	-0.03	0.51	-0.02	0.01
Observations	1,299		1,270		1,234		1,195		

Source: Baseline survey and follow-up surveys from 2016 to 2021. Some baseline data were lost due to server issues in two localities, see footnote 11.

Notes Baseline characteristics by treatment assignment for respondent to the different follow-up survey; “Cont” is the mean in the control groupe, “Diff” is treatment–control mean difference corresponding to the estimation of equation (1). “Joint test” column reports the p-value from seemingly unrelated regressions across years.

Table A2: Response Rates for each Survey

	N Sample	N Observed	Control mean	Diff	p-val
Observed at baseline (2014)	1,832	1,367	0.76	0.04	0.03
Observed at follow-up (2016)	1,832	1,666	0.91	-0.00	0.92
Observed at follow-up (2018)	1,832	1,676	0.90	-0.01	0.44
Observed at follow-up (2019)	1,832	1,623	0.87	-0.03	0.03
Observed at follow-up (2021)	1,832	1,581	0.85	-0.02	0.19
N times observed (2014/2021)	1,832	1,806	4.29	0.05	0.28

Source: Experiment registry, baseline, and follow-up surveys from 2016 to 2021. Some baseline data were lost due to server issues in two localities, see footnote 11.

Notes: The table reports the number of observations in the experiment registry (sample), the number of observations observed in each survey wave, the average response rate in the control group, and differential attrition. Differential attrition is estimated by regressing an indicator for survey response on treatment assignment using equation (1); the associated p-values are reported.

Table A3: Robustness to Multiple Hypothesis Testing: q Values Adjusted for False Discovery Rate (FDR) for Main Outcomes

	Coef.	SE	p-value	q-value
Extreme working poverty (earnings < 30k)	-0.073	0.016	0.0000	0.0000
Routine Task Intensity Index (RTI)	-0.271	0.065	0.0000	0.0001
Hourly Earnings (Total Labor, in 1,000 FCFA)	60.825	20.744	0.0034	0.0068
Total Earnings (in 1,000 FCFA)	10.973	3.401	0.0013	0.0034
Employment: Self-employed	0.047	0.017	0.0043	0.0069
Earnings: Self-employed (in 1,000 FCFA)	6.138	2.350	0.0091	0.0121
Employment: Wage-employed	-0.007	0.017	0.6752	0.6752
Earnings: Wage-employed (in 1,000 FCFA)	1.474	1.734	0.3954	0.4519

Source: Youth (pooled) post-program surveys.

Notes: columns (1) and (2) report estimated coefficients and their standard error for the 8 primary outcomes. Column (3) reports unadjusted p-values and column (4) adjusted q-values using Simes method controlling for the False Discovery Rate.

Table A4: Impact on Disaggregated Tasks

	Control Mean	Coefficient	p-value	q-value
Non Routine Abstract Tasks (NRA)				
Read forms	0.223	0.035	0.011	0.022
Read bills or financial statements	0.232	0.046	0.001	0.002
Read instructions or operating manuals	0.209	0.050	0.000	0.001
Read reports	0.171	0.021	0.096	0.123
Read plans	0.159	0.066	0.000	0.000
Read other documents	0.063	-0.000	0.975	0.975
Longest document read (n pages)	118.627	-92.468	0.329	0.384
Has to write	0.477	0.068	0.000	0.000
Longest document written (n pages)	5.865	0.064	0.944	0.975
Take measures or estimate sizes, weights, distances	0.495	0.053	0.001	0.003
Calculate prices or costs	0.633	0.037	0.018	0.032
Perform simple math operations	0.574	0.058	0.000	0.001
Other math computations	0.209	0.029	0.022	0.033
Undertake tasks requiring 30 min of thinking (1-5)	2.804	0.088	0.054	0.076
NRA Sub-Index	-0.000	0.170	0.000	
Non Routine Interpersonal Tasks (NRI)				
Has interactions with clients	3.102	0.096	0.008	0.010
Advice co-workers	0.845	0.045	0.000	0.000
Teach co-workers	0.786	0.026	0.051	0.051
Monitor co-workers	0.645	0.046	0.003	0.005
NRI Sub-Index	0.000	0.102	0.002	
Routine Tasks (R)				
Carry out short, repetitive tasks (1-4)	2.849	-0.009	0.759	0.759
No freedom to decide how to organize work (1-10)	5.190	-0.105	0.371	0.619
Use heavy equipments	0.221	0.045	0.001	0.007
Perform physical work	6.150	-0.048	0.610	0.759
Repair or maintain electronic equipments	0.256	0.041	0.003	0.007
R Sub-Index	-0.000	0.051	0.121	

Source: Youth 2021 endline survey (1,581 observations).

Note: The table presents the mean of each variable in the control group, the estimated coefficient, and the corresponding p-value. Column (4) accounts for multiple testing, controlling for the False Discovery Rate ([Benjamini and Hochberg, 1995](#)). The first, second and third panels correspond to indices related to Non Routine Analytical, Non Routine Interpersonal, respectively Routine tasks. The last row of each panel includes a subindex defined as the sum of the standardized variables in each panel, which is then normalized to have a unit variance.

Table A5: Hours

	(1)	(2)	(3)	(4)
	Total Labor	Apprenticeship	Wage empl.	Self-empl
+0 years (2016)				
Treatment	3.687** (1.492)	18.200*** (1.170)	-6.462*** (1.235)	-4.384*** (1.009)
Control mean	41.93	7.56	15.10	10.51
Observations	1,660	1,661	1,661	1,660
+2 years (2018)				
Treatment	1.457 (1.346)	4.436*** (0.986)	-2.600* (1.358)	1.058 (1.306)
Control mean	41.19	6.68	16.61	12.66
Observations	1,670	1,670	1,670	1,670
+3 years (2019)				
Treatment	3.320** (1.436)	0.764 (0.852)	1.692 (1.406)	0.741 (1.355)
Control mean	38.01	5.78	16.44	12.62
Observations	1,611	1,612	1,612	1,612
+5 years (2021)				
Treatment	3.351** (1.320)	0.025 (0.667)	3.056** (1.447)	1.375 (1.567)
Control mean	39.71	3.74	17.78	17.05
Observations	1,570	1,570	1,570	1,570
Post-Program (Pooled, 2018-2021)				
Treatment	2.683*** (0.942)	1.800*** (0.616)	0.642 (0.960)	1.055 (1.001)
Control mean	39.66	5.44	16.93	14.06
Time FE	{Yes}	{Yes}	{Yes}	{Yes}
Observations	4,851	4,852	4,852	4,852

Source: Youth surveys from 2016 (during the program), 2018, 2019 and 2021 (post-program).

Notes ITT estimates for hours of work. Estimation results from equation (1). We include strata fixed effects and weight observations by the inverse of the probabilities of assignment by strata (see footnote 12). The first four panels present estimation results separately for each year. The last panel presents pooled estimates based on the three post program surveys. We report Huber–White robust standard errors (clustered at the youth level for the bottom panel).

Table A6: Hourly earnings

	(1)	(2)	(3)	(4)
	Total Labor	Apprenticeship	Wage empl.	Self-empl
+0 years (2016)				
Treatment	-104.6*** (21.6)	14.6*** (4.9)	-41.0*** (11.0)	-27.2 (23.3)
Control mean	300.0	31.9	110.8	147.4
Observations	1,661	1,660	1,661	1,661
+2 years (2018)				
Treatment	33.9 (57.2)	23.6*** (7.6)	8.8 (28.8)	60.7 (37.7)
Control mean	519.0	42.4	193.6	219.8
Observations	1,670	1,670	1,670	1,668
+3 years (2019)				
Treatment	111.0*** (31.4)	8.9 (5.7)	3.6 (17.4)	61.3*** (22.6)
Control mean	323.2	31.1	156.1	125.2
Observations	1,612	1,611	1,606	1,608
+5 years (2021)				
Treatment	50.0 (62.5)	-0.9 (4.6)	-12.1 (25.4)	113.6** (54.2)
Control mean	627.4	23.7	236.5	298.2
Observations	1,569	1,569	1,570	1,565
Post-Program (Pooled, 2018-2021)				
Treatment	60.8*** (20.7)	8.4*** (3.2)	1.5 (10.2)	56.4*** (15.5)
Control mean	414.2	28.1	161.1	166.2
Time FE	{Yes}	{Yes}	{Yes}	{Yes}
Observations	4,851	4,850	4,846	4,841

Source: Youth surveys from 2016 (during the program), 2018, 2019 and 2021 (post-program).

Notes: The five panels present ITT estimates for hourly earnings by activity type. We add 1 hour to weekly hours to avoid division by zero, then compute hourly rate, and winsorize the top 1% within each wave. Estimation results from equation (1). We include strata fixed effects and weight observations by the inverse of the probabilities of assignment by strata (see footnote 12). The first four panels present estimation results separately for each year. The last panel presents pooled estimates based on the three post program surveys. We report Huber–White robust standard errors (clustered at the youth level for the bottom panel).

Table A7: Job satisfaction

	Reservation wage	Aspires to have the same job	Aspires to have the same type of job	Job satisfaction (z-score)
+5 years (2021)				
Treatment	13,395* (7,337)	0.035* (0.019)	-0.014 (0.023)	0.075 (0.049)
Control mean	178,003	0.15	0.31	-0.00
Observations	1,566	1,570	1,570	1,570

Source: Youth survey from 2021 (post-program).

Notes: ITT estimates for the “job satisfaction“ index is the standardized average of the following items: “satisfied with current job”; “satisfied with earnings”; “likes job”; “proud of job”.

Estimation results from equation (1). We include strata fixed effects and weight observations by the inverse of the probabilities of assignment by strata (see footnote 12). We report Huber–White robust standard errors.

Table A8: Job Aspirations

	(1)	(2)
	Wage employment	Self-employment
+0 years (2016)		
Treatment	0.019 (0.025)	-0.005 (0.025)
Control mean	0.44	0.52
Observations	1,661	1,661
+2 years (2018)		
Treatment	0.039 (0.025)	-0.035 (0.025)
Control mean	0.46	0.50
Observations	1,670	1,670
+3 years (2019)		
Treatment	-0.004 (0.025)	0.018 (0.024)
Control mean	0.50	0.38
Observations	1,612	1,612
+5 years (2021)		
Treatment	-0.017 (0.025)	0.039 (0.025)
Control mean	0.52	0.40
Observations	1,570	1,570
Post-Program (Pooled, 2018-2021)		
Treatment	0.007 (0.017)	0.002 (0.016)
Control mean	0.49	0.43
Time FE	{Yes}	{Yes}
Observations	4,852	4,852

Source: Youth surveys from 2016 (during the program), 2018, 2019 and 2021 (post-program).

Notes: ITT estimates for indicators of “aspiring to wage employment” and “aspiring to self-employment”.

Estimation results from equation (1). We include strata fixed effects and weight observations by the inverse of the probabilities of assignment by strata (see footnote 12). The first four panels present estimation results separately for each year. The last panel presents pooled estimates based on the three post-program surveys. We report Huber–White robust standard errors (clustered at the youth level for the bottom panel).

A.3 Experimental Design and Program Implementation

In each locality, a double-sided randomization protocol was implemented. The procedure was the following: firms were paired according to the number of open positions they had per trade, and within each pair a firm was assigned to treatment and another to control. The reason for implementing this pairing procedure, instead of a simple stratification by trade, is that some firms opened positions in different trades (see [Crépon and Premand \(2024\)](#) for additional details on the firm side of the experiment). Once the firm randomization was implemented, the number of open positions in treatment firms was counted by trade. This gave the exact number of youth to select. The next step of the randomization was then implemented, randomly assigning youth to treatment by trade. We assigned the exact same number of youth to treatment as the number of open positions to fill. As a result, the probability of youth assignment to treatment is trade-specific. On the side of youth, the experiment is thus stratified by locality (as the randomization procedure was implemented separately in each locality) and by trade. Since assignment probabilities are strata-specific, we include strata-specific weights in the specification used to estimate impacts on youth. There are 10 cases in which the assignment ratio is either 0 or 1. Indeed, as we draw firms, in some cases all positions in a small trade can be assigned to control or treatment, which is due to the fact that firms offer a portfolio of positions in different trades.

Once apprenticeship positions to be filled were drawn, and youth were selected in each locality, AGEFOP counsellors matched selected youth to selected firms with open positions in the same trade. The matching took place based on criteria such as distance between the firm and a youth’s residence. Once assigned to firms, youth passed a medical visit and were invited to sign a contract and start their apprenticeship. The patterns of entry and participation in apprenticeship are analyzed in detail in [Crépon and Premand \(2024\)](#). In this paper, we focus on impacts on medium- to long-term youth employment outcomes.

Youth 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 master craftsperson. 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. 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 master craftspeople and apprentices to track skills

acquisition through a booklet containing learning milestones for each trade. Counselors had the authority to suspend subsidies in case of absenteeism or performance issues. Dual 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.

The study design was peer-reviewed on April 21, 2014, and later filed as an attachment in AEARCTR-0002726. We mention here the main deviations from the original design. First, the number of firms and apprentices recruited varied from the original plan. Some localities (such as Abidjan) were dropped, which was mostly due to challenges in recruiting youth. Second, the key outcomes of interest (related to employment and earnings) remain largely similar to the original design, but there were some adjustments in secondary outcomes. The range of skills measures was fine-tuned over time, in particular as the literature expanded the use of task intensity measures in developing countries, which we prioritized over measures of soft skills that at times showed some lack of reliability in low-income settings (e.g. [Laajaj and Macours \(2019\)](#)) and became less prominent in Côte d’Ivoire as the country transitioned away from conflict. Cost data was simplified due to the nature of the data collected by the government agency implementing the program. Last, the focus on working poverty is introduced in this paper to enrich the analysis of the medium-term impacts on youth in the context of a high-informality labor market.

A.4 Description of Main Outcomes

A.4.1 Earnings, Employment, Hours Worked and Working Poverty

We collected data on youth employment, earnings, and hours worked for primary and secondary activities in all follow-up surveys. In the employment survey module, we distinguish between occupations as wage worker, self-employed, or apprentice. Self-reported earnings is a main outcome for youth, in line with most studies on employment and training programs ([Card et al., 2018](#); [JPAL, 2022](#)). We use modules from the Côte 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) time worked over the last seven days and last month, (ii) the type of activity (wage employed, self-employed, apprentice, others), and then, separately, (iii) 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.). 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. To analyze working poverty, we use the Foster–Greer–Thorbecke (FGT) measures to

obtain a working poverty headcount (FGT0), which provides the share of individuals below the poverty line, as well as a poverty gap (FGT1), which indicates the average depth of poverty for the poor by calculating the average income shortfall relative to the poverty line (Foster et al., 1984). We compute these measures for a poverty threshold set at the minimum wage (60,000 FCFA), and an extreme poverty threshold set at half the minimum wage (30,000 FCFA).

A.4.2 Construction of Task Complexity Indices

The youth follow-up surveys from 2018, 2019 and 2021 record the tasks that respondents undertake in their main occupation using a module adapted from Autor et al. (2003), Autor and Handel (2013) and Dicarolo et al. (2016). The questionnaire captures the types of tasks realized at work along three dimensions: non-routine analytical, non-routine interpersonal, and routine. The module includes both frequency/intensity questions using Likert scales, binary indicators, and continuous measures such as the number of pages written or read. To aggregate these items into interpretable indices, we follow a two-step standardization-and-summing approach within each task family and then normalize the resulting sums to have unit variance.

The Routine (R) index aggregates items that reflect repetitive and rules-based tasks, which involve little freedom to decide how to organize work, as well as routine manual tasks, such as performing physical work, using heavy equipment, or repairing or maintaining equipment. The resulting composite is normalized to have a standard deviation of one and centered at a control mean of zero, such that higher values indicate more routine-intensive occupations.

For the Non-Routine Analytical (NRA) index, we combine items that relate to information processing and problem solving at work. These include whether the job requires reading forms, bills/financial statements, instructions/operating manuals, reports, plans, or other documents; the longest document read; writing and the longest document written; taking measures or estimating sizes, weights, or distances; calculating prices or costs; performing simple math operations or other computations; and undertaking tasks that require at least thirty minutes of focused thinking. Each variable is first transformed into a standardized z-score, so that higher values uniformly indicate greater analytical content, and then summed across the NRA items for each worker. We then rescale this sum to have a unit variance and center it so the control group mean is zero, which makes the treatment coefficient directly interpretable as a standard-deviation change relative to the control.

The Non-Routine Interpersonal (NRI) index is built similarly, pooling items that capture task content emphasizing person-to-person interactions and coordination. Specifically, we standardize and sum responses on the frequency/intensity of interactions with clients, advising co-workers, teaching co-workers, and monitoring co-workers. As with the NRA index, the composite is normalized to have a unit variance and a control mean of zero, so positive values

represent occupations with more intensive interpersonal tasks relative to the control group.

To summarize the overall task structure in a single measure, we report a Routine Task Intensity (RTI) index defined as $RTI=R-NRA-NRI$. By construction, higher RTI values indicate occupations that are more routine and less non-routine in both analytical and interpersonal dimensions. Because all subindices are standardized, the RTI index can be interpreted as a standardized difference in routine versus non-routine content. Negative treatment effects on RTI therefore measure the extent to which the intervention shifts participants away from performing routine tasks and towards performing more non-routine tasks. Centering on the control group implies that the reported coefficients represent changes relative to the tasks performed by control youth. The reported treatment coefficients are in standard-deviation units of the corresponding index.