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# The long-term effect of a job training programme for youths in Peru

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## ABSTRACT

This paper evaluates the long-term impact of Projovent, a job training programme for vulnerable youths in Peru, on formal labour market outcomes. Covering a 10-year period, this evaluation offers one of the longest evaluations of a training programme in developing countries. Exploiting an experimental design and administrative data, we find that Projovent improved formal employment and earnings in the short-term, but that these effects quickly disappear, and the programme has no effect in the medium- or longer-term. These results reinforce the importance of training comprehensiveness and relevance.

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## KEYWORDS

Long-term effects; youth training; labour market outcomes

## 1. Introduction

High levels of youth unemployment and labour informality are common in most countries across Latin America and the Caribbean (LAC) (Novella et al. 2018). According to official data from the National Institute of Statistics and Informatics (INEI 2020), informal employment affected nearly three quarters of the Peru's national labour force in 2019. In the attempt to improve the labour market conditions of youths, many countries in LAC have implemented short-term training programmes. This paper evaluates the effectiveness of Projovent, a job training programme for youths in Peru, on individuals' labour market outcomes over a 10-year period.

The popularity of training programmes in LAC is partially supported by evidence showing that, in the region, these programmes tend to be effective in the short-term, particularly for young individuals (Escudero et al. 2018). However, evidence on the long-term effects in LAC are mixed. While a few long-term evaluations indicate that the short-term effects do not persist and gradually disappear over time (Alzúa, Cruces, and Lopez 2016; Doerr and Novella 2020; Ibararán et al. 2018), others show positive, persistent effects (Attanasio et al. 2017; Kugler et al. 2022).<sup>1</sup>

Job training programmes are expected to improve labour market outcomes through three primary channels. First, they can equip workers with skills that are relevant and sought after by employers, compensating for some of the failures of the education system (McKenzie 2017). Second, they can help workers signal their productivity and gain experience through the internship component in (scarce) formal jobs (Novella and Valencia 2022; Pallais 2014). Third, training can help workers to expand their network and gain valuable information about the labour market. However, in addition to the technical skills training itself (e.g. socioemotional skills, job placement, vocational

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orientation), the duration of the programme and the number of components have been shown to be important in determining the effectiveness and the duration of training effects (Bandiera et al. 2021; Bertrand et al. 2021; Card, Kluve, and Weber 2018; Chakravarty et al. 2019; Davis and Heller 2020; Escudero et al. 2018; Narayan 2021; Rahman 2018).

Projovent was implemented between 1996 and 2010 as a large-scale public programme offering three-months in-classroom technical training and a three-month internship to vulnerable youths. Our analysis is based on a sample of applicants to the last public call of Projovent who applied to receive training in 2009 – 2010. Baseline data was collected prior to random assignment using an application form and a supplementary survey. To measure treatment effects over a 10-year period, we use administrative data from the Electronic Payroll (*Planilla Electrónica*, PE), a registry with monthly reports (between January 2009 and January 2021) from formal employers in the country, enabling us to measure formal employment and earnings.

This paper is the first evaluation of a training programme in LAC that covers such a long period of time (a 10-year timeframe between 2009–2020).<sup>2</sup> During the period of analysis, Peru's economy experienced considerable changes. From 2010 to 2013, there was a notable expansion in economic growth and formal employment, with average annual growth rates of 6% and 7% respectively. In contrast, between 2013 and 2019, the average economic growth and employment growth rates reduced to just 3%. In 2020, the COVID-19 pandemic severely affected the country's economy, leading to a –11% annual growth rate and a –9% formal employment growth rate.

Compared with the previous non-experimental short-term evaluations of Projovent (Díaz and Jaramillo 2006; Galdo 1998; Ñopo and Saavedra 2003; Ñopo et al. 2002), this paper offers several advantages. Most notably, it is the first impact evaluation of Projovent exploiting an experimental design, combining registration and administrative data, to measure the long-term effectiveness of Projovent.<sup>3</sup>

We estimate intention-to-treat effects and, given non-compliance with the allocated treatment status, we also estimate local average treatment effects. Our results show that Projovent has a positive short-term effect on formal employment and earnings (in the first year after graduation). However, these effects shortly disappear, and we do not find any evidence that Projovent improves workers' outcomes in the medium- (second or third years) or longer-term (four years or more). We find no evidence that these results are driven by a perception of unfair treatment among control individuals who received training.

The rest of the paper is organised in five sections. [Section 2](#) describes the main characteristics of Projovent and the evaluation design. [Section 3](#) presents the data and methodology used. [Section 5](#) presents the results of the evaluation and [Section 6](#) concludes.

## 2. The intervention and the evaluation design

### 2.1. Projovent

Projovent was designed by the Ministry of Labour and Job Promotion of Peru (MTPE, for its acronym in Spanish) in 1996 and operated until 2010.<sup>4</sup> The programme aimed at facilitating access to the formal labour market for youths with limited resources, and offered a three-month in-classroom technical training component followed by an internship for three additional months. Private or public training agencies (PPTA) were contracted by the programme to provide training, with courses collaboratively designed by PPTA in consultation with the companies where beneficiaries would subsequently undertake their internships. Through formal agreements signed with PPTA, these firms committed to providing internship positions to beneficiaries upon successful completion of the three-month in-classroom training phase.

Interested youths applied in-person at designated Projovent registration centres. The initial stage involved completing a personal information and socioeconomic form. The socioeconomic details

gathered from this form were used to generate a score for a proxy means test (PMT), to assess whether an applicant fulfilled the poverty/vulnerability criteria of the programme.

Applicants who passed the PMT were then asked to choose their preferred course from the list of training courses offered by PPTA. Once they selected a course, applicants were sent to the corresponding PPTA to continue with the final step of the selection process. This usually consisted of vocational and basic skill assessments, as well as interviews,<sup>5</sup> to identify suitable candidates for programme participation. In previous cohorts before the one under evaluation, individuals who were deemed suitable by the PPTA were then considered eligible for Projovent and were offered a place in the programme on a first-come-first-served basis. This process took place until the number of eligible applicants matched the number of available slots for each course. Individuals deemed ineligible by a PPTA were given the opportunity to make a second choice or even a third choice of a training course. This process could be extended until applicants had reached their third choice of a training course, or otherwise until all PPTA had filled their available vacancies.

Selected applicants then proceeded to the in-classroom technical training stage at a PPTA. The training courses were designed to provide basic technical skills in low-skilled occupations (e.g. knitting, sales support, bakery). After completing the in-classroom stage, beneficiaries moved on to on-site internships at firms. During the three-month internship, beneficiaries received a stipend lower than the minimum wage and health insurance coverage. Both the stipend and the health insurance were covered by firms. Overall, the cost of the programme per beneficiary (including operating costs and a stipend) was relatively low, at around US\$420.<sup>6</sup>

## **2.2. Evaluation design**

This paper evaluates Projovent using an experimental design for the first time. An excess number of applicants, relative to the number of vacancies, allowed the implementation of the experimental evaluation design.<sup>7</sup>

To facilitate the evaluation, Projovent made slight adjustments to its standard operational procedures. While no changes were made to the application, eligibility, or selection process, some minor modifications were essential to incorporate randomisation and establish a control group. First, using a computer assisted programme, Projovent assigned a random number to each programme applicant. Then, after the PMT, applicants selected their preferred courses, and each PPTA determined candidates that were eligible for each training course. However, unlike previous rounds, the final selection of beneficiaries was not determined on a first-come-first-served basis, but rather through random assignment coordinated by Projovent in collaboration with the evaluation team. To facilitate the experimental design, PPTA were asked to identify at least 25% more suitable applicants than the number of vacancies on the course. Then, in courses with excess demand, candidates who were deemed eligible for the programme by the PPTA were sorted by the random number assigned at the beginning of the application process. Those at the top of the list were allocated a slot in the course and would make up the treatment group, while the remaining applicants would comprise the control group.<sup>8</sup> If a course had fewer suitable candidates than vacancies, all candidates would be placed in the course. Therefore, only applicants from courses with excess demand are part of the evaluation sample.<sup>9</sup>

When comparing the first choice of courses with excess demand to the entire pool of first choices, we observe that 54% of all courses are present in the oversubscribed courses. Nevertheless, the most popular courses are consistent across both lists. 'Promotion and execution of retail sales' emerged as the top choice in both the full list of courses and among oversubscribed courses, and 'Apparel and Retail Control Inspector' was also popular in both lists.

However, craft and trade-related occupations were relatively more frequent in the oversubscribed courses, with courses such as 'Electrical Installation' and 'Wood Carpentry' being more commonly chosen in the list of oversubscribed courses. In this regard, it is important to note that the oversubscribed courses do not fully represent the entire spectrum of courses offered by Projovent.

The evaluation began in early 2009, when Projovent launched its 16th public call for applications, and interested youths began to apply. A total of 26,770 individuals applied and 23,666 (88%) passed the proxy means test. Among these, almost 15,000 selected a course.<sup>10</sup> However, around 3,500 youths who selected a course abandoned the application process before the PPTA evaluation stage. Thus, among the individuals who selected a course 11,713 were assessed by PPTA. Of this group, 7,464 (64%) registered in courses with excess demand for their first choice of course. Out of these, 7,151 (96%) were declared eligible for the programme by PPTA and were sorted based on the random number assigned at the beginning of the application process. From this pool of eligible applicants, 5,791 individuals (81%) were randomly assigned to fill a vacancy and formed the treatment group, while 1,360 individuals (19%) were randomly assigned to be part of the control group.<sup>11</sup>

Figure 1 presents a simplified timeline schema of the programme's implementation stages versus the evaluation timeline. It is important to note that the implementation dates for the different programme stages varied across courses, which is why the different stages overlap slightly. For all courses, training took place between July 2009 and May 2010.

As is common in experimental evaluations of training programmes, there was imperfect compliance of the experimental design. This was partially due to Projovent allowing participants to select a second or third course option. As a result, some of the applicants from the control group were able to obtain a vacancy in a different course after not obtaining their first choice. Consequently, for those applicants whose first chosen course had excess demand, the effective allocation to courses does not strictly correspond to the random assignment to treatment and control groups under the established protocol. Additionally, not all youths who started the training made it to the on-site internship stage. Hence, the group of individuals who fully completed the training differs from the randomly assigned treatment group.

Table 1 presents the distribution of youths in the experimental treatment and control groups across both stages of the programme. Of the total assigned to the treatment group, 1% did not begin the training stage at a PPTA. At the same time, 39% of youths assigned to the control group began courses at a PPTA, at their second or third course choice. 83% of the treatment group and 32% of the control group completed the course stage. At the end of the internship stage, 52% of youths assigned to the treatment group and 22% of those assigned to the control group completed the internship.

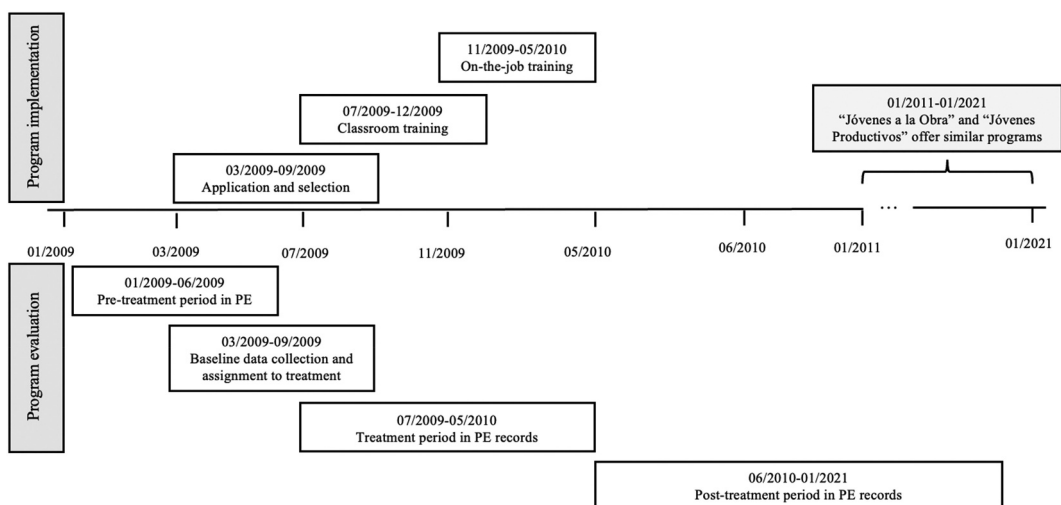


Figure 1. Timeline of projovent program stages and data collection. *Source:* Authors' own illustration. *Notes:* Figure shows the timing of the treatment's stages, as well as the data collection and evaluation timeline.

**Table 1.** Evaluation sample over the training stages.

	Treatment group		Control group		Total	
	N	Percentage	N	Percentage	N	Percentage
Total	5,791	100	1,360	100	7,151	100
Began course stage at a PPTA						
Yes	5,741	99	526	39	6,267	88
No	50	1	834	61	884	12
Completed course stage at a PPTA						
Yes	4,820	83	435	32	5,255	73
No	971	17	925	68	1,896	27
Completed on-site internship stage						
Yes	3,028	52	298	22	3,326	47
No	2,763	48	1,062	78	3,825	53

Source: Projovent's administrative records.

Although there was imperfect compliance, we find that assignment to treatment is significantly correlated with training completion, as individuals assigned to the treatment group have roughly a 29-percentage point higher probability of completing the training than those assigned to the control group (Table A.1 in the Appendix). The table also presents the results of the weak instrument test, which allows us to test whether completing the training is sufficiently correlated to the random allocation of treatment. We find that F-statistic for the weak instrument test is substantially higher than the critical value (Stock and Yogo 2005) – with a value of nearly 200 – and consequently reject the weak instrument hypothesis.

### 3. Data and methodology

#### 3.1. The data

The evaluation combines data from the application form and supplementary questionnaire (baseline) and administrative records. By utilising administrative records, we are able to evaluate the effectiveness of the programme over a 10-year period.

Baseline data was collected in early 2009, during the enrolment process of applicants. All applicants were required to fill a personal and socioeconomic form. This information was complemented with data retrieved from an additional questionnaire attached to the socioeconomic form in order to obtain more detailed information on household demographics, recent labour force participation history prior to the enrolment in the programme, and a measure of self-esteem (i.e. individuals' overall evaluation of their own worth) (Rosenberg 1965).

In addition, the paper uses administrative records of the Electronic Payroll (PE) – a document that formal firms in Peru with more than two workers are required to submit through the National Superintendence of Customs and Tax Administration (SUNAT). The PE includes information on payroll workers, pensioners, service providers (including trainees covered by vocational training agreements), third-party staff, and beneficiaries.<sup>12</sup> Registered employment represents the most formal employment in the Peruvian economy, since firms who report to the PE comply with all labour benefits and entitlements established by national regulations. For our analysis, we use the information on payroll workers to construct monthly variables of formal employment and formal income.

Access to information from the PE is restricted and was accessed with express authorisation from MTPE with the goal of identifying the long-term impact of Projovent on formal employment indicators. To use the data on formal employment and income, Projovent's records were matched with the PE through the (unique) number of the Peruvian National Identity Document (NID). All the information extracted from the PE was processed by MTPE's Statistics Department, maintaining data

confidentiality. The available PE dataset contains, for each participant, monthly panel information between January 2009 and January 2021 (i.e. 145 months).

The evaluation sample consists of 7,151 youths who were assigned to treatment and control groups in training courses with excess demand (on their first course choice). However, the final sample used in the evaluation consists of 6,583 individuals who have baseline data and reported their NID during the application process to Projoven, so that they could be matched to the PE records.<sup>13</sup>

### 3.2. Balance at the baseline

Table 2 shows the baseline characteristics of applicants in the evaluation sample and the analysis of statistical equivalence of characteristics at baseline between youths assigned to the treatment and control groups. It shows that, at baseline, applicants were, on average, 19 years old and had 10 years of education – which is close to completion of secondary education. Furthermore, according to the eligibility criteria, applicants came from households with scarce economic resources, as shown by various household characteristics.

Table 2 also shows the results of testing for balance in the baseline characteristics using the following regression:

$$X_i = \delta_0 + \delta_1 Z_i + \sum_j \delta_{2j} C_{ij} + \varepsilon_i \quad (1)$$

where  $X_i$  is the baseline variable;  $Z_i$  is the indicator of random treatment assignment (takes a value of 1 for the treatment group and 0 for the control group);  $C_{ij}$  represents fixed effects for each of the courses with excess demand, and  $\varepsilon_i$  represents an error term. The difference in means for the treatment and control groups is obtained when estimating the coefficient  $\delta_1$ .

This analysis is performed for two groups. First, it is performed for the complete evaluation sample (7,151 youths in the baseline). This allows us to verify whether the random assignment of treatment successfully balanced the characteristics of individuals in both groups. The same analysis is then performed for the sample of youths used for the analysis (i.e. the 6,583 individuals who reported a NID), enabling us to ascertain whether baseline characteristics remained balanced between the treatment and control groups in the subsample used for the analysis.

For both samples, we find that individuals in the treatment and control groups are similar in all baseline characteristics, with a few exceptions (house ownership and having water services inside the household). The table also presents a joint balance test where the treatment indicator is regressed on all of the baseline covariates and the course fixed effects. Although the p-values of the F-tests are less than 10% for both samples, this appears to be almost entirely driven by differences in house ownership.

### 3.3. Methodology

In this study, we estimate two parameters. First, when some people assigned to treatment fail to receive treatment, or some people assigned to the control group get treatment on their own, it is conventional to estimate the differences in outcomes between individuals allocated to the treatment and control groups as an estimate of the ‘intention to treat’ (ITT) effect (e.g. Angrist, Imbens, and Rubin 1996). In this paper, the ITT identifies the effect of being randomly *offered* a training slot in Projoven. This is a highly relevant parameter from a policy perspective because, as for most active labour market policy programmes, individuals can be offered to participate in Projoven but cannot be forced to participate. Thus, the ITT corresponds to the effect of making the programme available to eligible individuals. We estimate the ITT by running the following regression:

**Table 2.** Balance between the treatment and control groups at baseline.

	Full sample			Sample with administrative data		
	N	Control group mean	Treatment/control difference (SE)	N	Control group mean	Treatment/control difference (SE)
Gender: Male	7,151	0.465	-0.005 (0.013)	6,583	0.462	0.002 (0.013)
Age	7,151	19.216	-0.067 (0.071)	6,583	19.434	-0.118 (0.074)
Years of schooling	7,151	10.479	0.016 (0.038)	6,583	10.502	0.025 (0.039)
Took courses at an institute or university	7,146	0.023	0.008 (0.005)	6,583	0.025	0.008 (0.006)
Took a technical/trade course	7,146	0.125	0.018* (0.011)	6,583	0.134	0.012 (0.011)
Gender of head of the household: Male	7,146	0.637	0.010 (0.015)	6,583	0.629	0.017 (0.016)
Schooling of head of the household:						
Complete higher education	7,151	0.335	-0.001 (0.015)	6,583	0.332	0.007 (0.015)
Incomplete higher education	7,151	0.194	0.003 (0.012)	6,583	0.193	0.002 (0.013)
Complete secondary education	7,151	0.387	-0.001 (0.015)	6,583	0.392	-0.008 (0.016)
Incomplete secondary education	7,151	0.037	-0.002 (0.006)	6,583	0.037	-0.000 (0.006)
Elementary/illiterate	7,151	0.047	-0.001 (0.006)	6,583	0.047	-0.001 (0.007)
Overcrowding (people per room)	7,146	3.036	-0.016 (0.053)	6,578	3.032	-0.012 (0.055)
House ownership	7,146	0.689	-0.046*** (0.014)	6,578	0.691	-0.048*** (0.015)
Floor material: Floor tile, cement, wood	7,151	0.483	-0.008 (0.014)	6,583	0.487	-0.008 (0.015)
Ceiling material: Concrete, cement, tiles	7,151	0.466	-0.004 (0.014)	6,583	0.485	-0.012 (0.015)
Wall material: Brick	7,151	0.502	0.014 (0.014)	6,583	0.520	0.007 (0.015)
Water: Inside the household	7,151	0.810	-0.010 (0.012)	6,583	0.820	-0.022* (0.012)
Sanitary facilities: Inside the household	7,151	0.699	0.019 (0.014)	6,583	0.714	0.008 (0.014)
Phone numbers: None	7,151	0.900	0.006 (0.009)	6,583	0.899	0.007 (0.009)
Total accreditation score	7,146	14.335	0.057 (0.132)	6,578	14.154	0.140 (0.139)
Self-esteem	7,151	-0.000	-0.002 (0.028)	6,583	0.022	-0.019 (0.029)
No data on self-esteem	7,151	0.134	0.012 (0.011)	0.462	0.136	0.013 (0.011)
F-test of joint significance (p-value)			0.059			0.032

Notes: The complete sample corresponds to youths enrolled in a course with excess demand for their first course selection. The treatment group corresponds to individuals who successfully obtained a vacancy, and the control group corresponds to those who did not obtain a random vacancy. The difference between the treatment and control groups is obtained from a regression that includes fixed effects per course-section. SE = standard error. \*  $p < 0.1$ ; \*\*  $p < 0.05$ ; and \*\*\*  $p < 0.01$ .



$$Y_i = \alpha + \beta Z_i + \sum_j \gamma_j C_{ij} + \sum_k \delta_k X_{ik} + \varepsilon_i \quad (2)$$

where  $Y_i$  is an outcome variable, and  $Z_i$  indicates whether the individual was assigned to the treatment (1) or control (0) group.  $C_{ij}$  represents fixed effects for each of the courses with excess demand, which are included as randomisation took place within each first course selection.  $X_{ik}$  represents baseline characteristics (age, gender, education, located in Lima, house ownership, and wealth index<sup>14</sup>), and  $\varepsilon_i$  represents the error term. We control for house ownership due to the observed differences between the treatment groups at baseline, while information on age, sex, education, location, and wealth have been included as prior research suggests that these are important predictors of labour market participation in the country (Favara, Chang, and Sánchez 2018).<sup>15</sup> The ITT effect reflects the difference in the outcome indicator between treatment and control groups and is obtained by estimating the coefficient  $\beta$ .

Second, we estimate the local average treatment effects (LATE). This captures the effect of Projovent for compliers, i.e. for the subset of participants who comply with their original treatment assignment (see Angrist and Imbens 1994; Angrist, Imbens, and Rubin 1996; Duflo, Glennerster, and Kremer 2007). LATE is obtained using instrumental variables regressions, taking advantage of the exogenous variation derived from random allocation for the first course selection as instrumental variable. We estimate the LATE as that there is two-sided imperfect compliance with treatment, and it is possible that the effects of the treatment may vary between individuals who adhere to their treatment status and those who do not. We estimate the LATE in the following specification:

$$Y_i = \alpha + \beta D_i + \sum_j \gamma_j C_{ij} + \sum_k \delta_k X_{ik} + \varepsilon_i \quad (3)$$

where the participation dummy (having completed the on-site internship stage),  $D_i$ , is instrumented with the indicator for random assignment to the Projovent programme,  $Z_i$ , in the first-stage equation

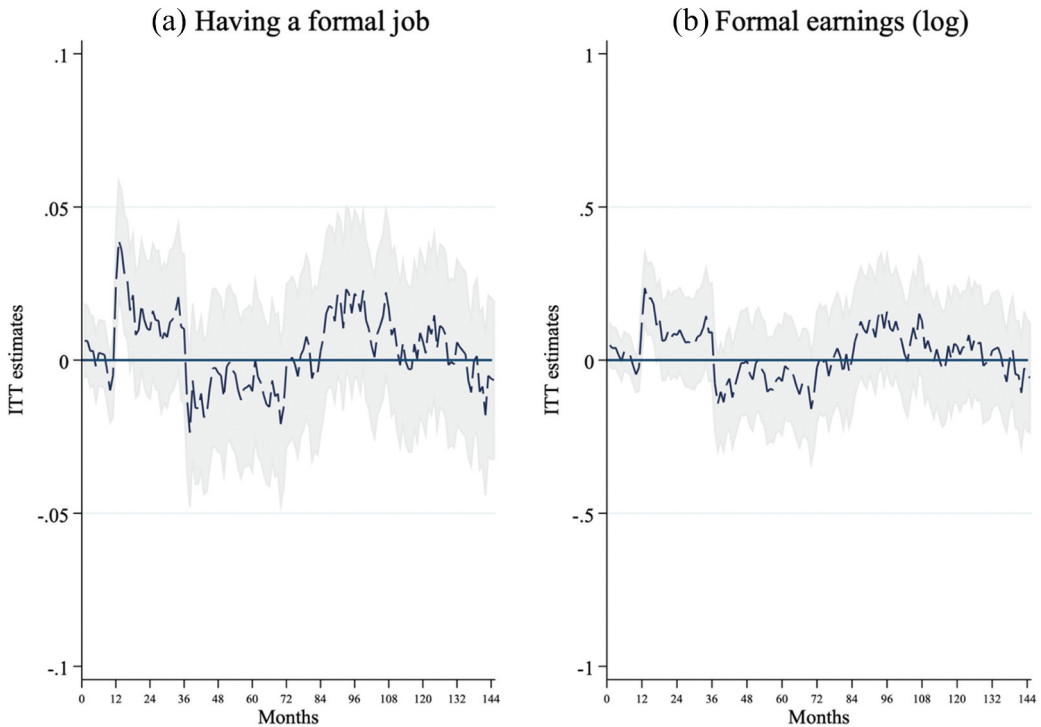
$$D_i = \pi Z_i + \sum_j \tau_j C_{ij} + \sum_k \theta_k X_{ik} + \eta_i \quad (4)$$

Information from the administrative data allow us to measure the effect of Projovent on formal employment and earnings over a 10-year period. The variable for formal employment takes the value of 1 when an individual appears formally registered by the employer and 0, otherwise. Given the granularity of the administrative data, we are able to observe whether individuals are working formally on a monthly basis during the period of analysis. For the earnings variable, we calculate the (log) earning for each month during the period of analysis.

#### 4. Results

Figure 2 presents the monthly treatment effects of Projovent (ITT) on the probability of having a formal job (Panel A) and monthly formal earnings (Panel B), between January 2009 (month 0) and January 2021 (month 144). During the first months of 2009 (during the application and training phases), the outcomes for individuals in the control and treatment groups are similar, which confirms that the random allocation successfully balanced the characteristics of applicants assigned to the treatment and control groups.

Thereafter, we find that Projovent positively impacts the probability of having a formal job, but only in the short-term (up to nearly 4 percentage points in early 2010). Similarly, Projovent positively affects formal earnings, but only in the short-term (up to 24% in 2010). Estimating treatment effects separately by gender, age group (16–18 and 19–24 years), and geographical location, we find that these short-term impacts are driven by females, those aged 16–18, and those located in Lima.<sup>16</sup> However, after this, the programme does not show any significant effects on formal employment or earnings over the rest of the 10-year analysis period.<sup>17</sup>



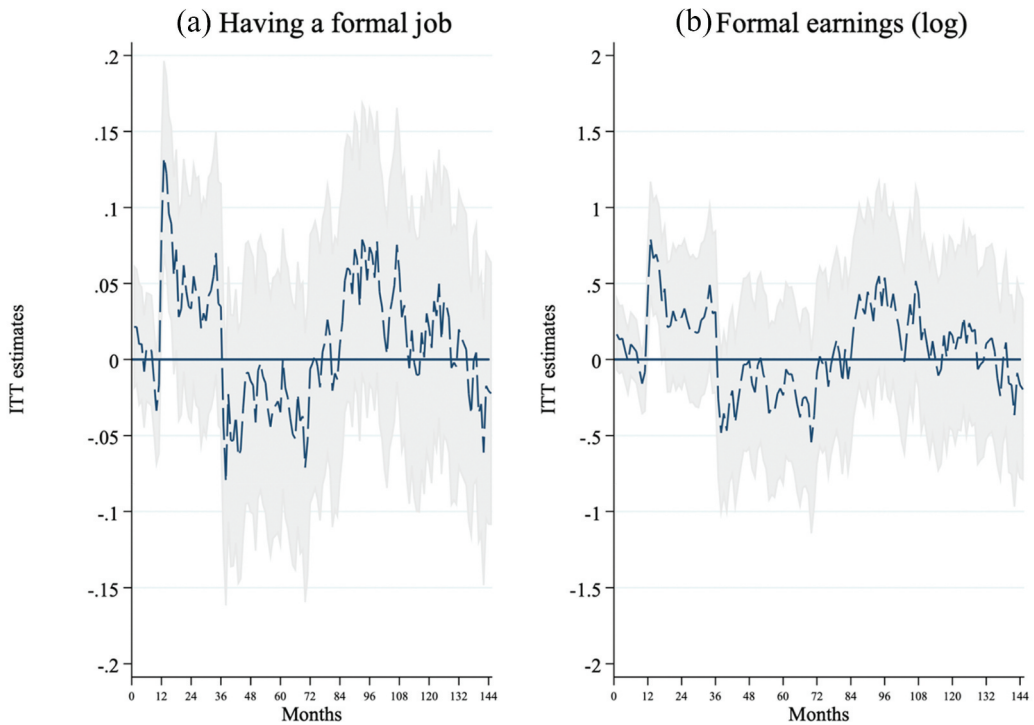
**Figure 2.** Effects (ITT) of projoven on formal employment and earnings. *Notes:* These figures show the ITT effects of being assigned to the Projoven programme on the probability of formal employment and formal income on a monthly basis. The grey shaded area represents the 95% confidence interval. Months range from January 2009 (0) to January 2021 (144). Estimates use data from Planilla Electrónica and are estimated using Equation (2). The sample corresponds to individuals who reported a National Identity Document. Each estimation controls for gender, age, located in Lima, house ownership, wealth index, and course fixed effects.

**Figure 3** presents the monthly LATE of Projoven on formal employment and earnings. We observe exactly the same patterns as the ITTs, but with larger short-term effects (up to 13 percentage points for formal employment and 80% for earnings in 2010). Thus, we find consistent evidence that, apart from some positive effects in the short-term, which dissipate quickly, Projoven has not been successful in improving employment outcomes of its beneficiaries in the longer-term.

## 5. Discussion

In the results presented above, we find that Projoven has a positive impact on the short-term (<1 year) likelihood of formal employment and formal earnings. The observed LATE results are of similar magnitude to previous short-term, non-experimental, evaluations of Projoven, which found an 11–18 percentage point increase in the probability of formal employment (Díaz and Jaramillo 2006). While our ITT results are lower than previous evaluations, this is expected since ITTs estimate the effect of being offered treatment rather than completing it.

As mentioned previously, roughly one in every five control individuals ended up completing training in their second or third choice. Given this, it is theoretically possible that the observed differences in outcomes could be due to these control individuals feeling they were treated unfairly (as they did not get allocated their first choice in the programme). However, we find no evidence of this. In Figure A.6 in the Appendix, we compare the outcomes of treated individuals who completed training with those of control individuals who completed training in their second



**Figure 3.** Effects (LATE) of projovent on formal employment and earnings. *Notes:* These figures show the LATE effects of being assigned to the Projovent programme on the probability of formal employment and formal income on a monthly basis. The grey shaded area represents the 95% confidence interval. Months range from January 2009 (0) to January 2021 (144). Estimates use data from Planilla Electrónica and are estimated using Equations (3) and (4). The sample corresponds to individuals who reported a National Identity Document. Each estimation controls for gender, age, located in Lima, house ownership, wealth index, and course fixed effects.

or third choices. We observe that the positive effects of treatment allocation disappear in this comparison, which yields mostly null results. This suggests that it is unlikely our results are driven by a perception of unfair treatment among control individuals who received training. Instead, in Figure A.7, we observe that the positive effects of the treatment arise when comparing outcomes for those assigned to treatment who completed training versus control individuals who remained true to their assigned status. This possibly helps to explain why, particularly in the short-term, control individuals might have been motivated to pursue alternative training options. However, even within this restricted sample, the treatment effects diminish over time, resulting in largely zero long-term effects.

When considering the factors behind the observed short-term effects, it is likely that the internship programme played an important role. The positive effects of the programme occur largely during the internship period, in the first half of 2010. Indeed, analysing treatment effects separately for those who only completed the classroom training versus the entire programme reveals that the short-term results are driven entirely by those who completed the internship; there are no discernible positive treatment effects among those who only completed the classroom training component. While these findings should be interpreted cautiously due to the non-randomised nature of training completion, they suggest that the internship may have played a crucial role in the observed short-term effects.

As highlighted earlier, the positive effects of Projovent on formal labour market outcomes quickly dissipate after the internship, and the programme has not been successful in improving the long-

term employment prospects of its beneficiaries. Below, we discuss two potential reasons for this lack of sustained impact.

Firstly, Projoven differs from other successful training programmes by not mandating the inclusion of socioemotional skills training, labour market orientation, or job search support in its curriculum. Past evaluations of youth training programmes underscore the importance of a comprehensive approach encompassing various targeted components, including job search assistance, counselling, training, and placement services (Kluve 2014). For instance, the highly successful Jóvenes en Acción programme in Colombia (Attanasio et al. 2017; Attanasio, Kugler, and Meghir 2011) incorporated not only occupational skills training and internships but also focused on developing young people's soft skills (e.g. proactivity, communication, and problems solving skills), as well as their socioemotional capabilities (e.g. positive personal and work perspectives). Projoven lacked these additional training components and demonstrated no (short-term) significant impacts on socioemotional skills, such as self-esteem, perseverance, or ambition, according to previous evaluations (Diaz and Rosas, 2016).

Secondly, the absence of long-term effects may also be attributed to the relevance of the job training provided. Projoven's courses were designed to provide basic technical skills for low-skilled elementary occupations. However, the composition of employment in Peru underwent substantial changes between the programme's initiation and conclusion. Between 2003 and 2012, the share of elementary occupations declined the most in the country, while the share of occupations such as clerical and services and sales workers increased markedly (Cruces et al. 2017). Consequently, the skills taught by Projoven may not have been the most pertinent for individuals in a transforming labour market and may offer one possible explanation for why a substantial portion of the treatment group did not progress to the internship stage. In contrast, the Jóvenes en Acción programme offered a greater number of courses in administrative occupations, aligning more closely with the evolving needs of the labour market (Attanasio et al. 2017).

When interpreting our findings, it is important to keep in mind that the oversubscribed Projoven courses evaluated in this paper do not fully represent the entire spectrum of courses offered by the programme. While this does not compromise the internal validity of our estimates, it might affect the external validity of our results in assessing the broader impact of the Projoven programme. Therefore, additional research might be needed to gain a more comprehensive understanding of the programme's overall impact on young individuals in Peru.

## 6. Conclusions

Projoven was implemented in Peru between 1996 and 2010. The programme stood out as the main public policy instrument for facilitating the integration of the country's underprivileged urban youths into the formal labour market.

This paper offers evidence on the effectiveness of Projoven over a decade-long period, and combines baseline data with administrative records to estimate the effects of Projoven on formal labour market outcomes.

Overall, we find evidence of short-term positive impacts of Projoven on formal employment and earnings. However, similar to other programmes in LAC (Alzúa, Cruces, and Lopez 2016; Doerr and Novella 2020; Ibararán et al. 2018), but in contrast with evidence for Colombia (Attanasio et al. 2017; Kugler et al., 2022), these effects vanish quickly, and the programme does not demonstrate any further impact on formal labour market outcomes in the medium- or long-term.

The fact that the effects of Projoven vanish over time underscores the importance of comprehensive training, encompassing multiple targeted components. It also emphasises the need to tailor job training programmes to align with the demands of local employers, ensuring that the skills imparted meet industry requirements and address emerging skills gaps.

## Notes

1. Considering this literature, in this paper we define short-term effects as those happening during the first year after graduation of the program; medium-term as those between the start of the second and the end of the third year; and long-term effects as those happening on or after the start of the fourth year after graduation.
2. Attanasio et al. (2017) and Kugler et al. (2022) show results for a shorter period of time, specifically six and eight years after the program, respectively.
3. An experimental analysis was previously circulated as a working paper by two of the authors (Díaz and Rosas, 2016). This paper offers two key advantages. First, Díaz and Rosas (2016) only had access to administrative data from 2009–2013, and so we are able to cover a much longer evaluation period. Second, we are able to precisely capture formal earnings, while Díaz and Rosas (2016) were only able to discern whether earnings were above or below the minimum wage.
4. During almost 15 years of operations, the program trained approximately 73,000 youths. In 2010, the MTPE modified the program's design and name, becoming '*Jóvenes a la Obra*'. It is highly unlikely that any individuals in our evaluation sample later took part in *Jóvenes a la Obra*, given that this program is designed for youth, and our evaluation sample would have surpassed the age eligibility. Additionally, though we lack data on participants' involvement in other programs, participation in Projoventes should not influence the likelihood of engaging in any other programs. Consequently, the randomisation process should ensure that this does not impact our treatment estimates.
5. PPTA had discretion to use their own procedures and criteria to assess the candidates and declare them suitable or not.
6. As reference, McKenzie (2017) reports that the cost of vocational training programs offered globally typically ranges from US\$500 to \$1,700 per individual trained.
7. For the cohort being evaluated, the program originally had less than 10,000 vacancies and 26,770 applicants.
8. Both the number of applicants that selected a specific course, and the number of available vacancies for the courses, varied between courses and PPTA. Therefore, the individual probability of being selected as a beneficiary is not the same for all youths. This is considered in the analysis by including not only the random allocation but also identifiers of the course and PPTA selected.
9. If dropouts occurred during the first week of classes, PPTA were not able to freely select replacements but instead had to follow the random number order. Participants in the control group were also restricted from registering in the program's subsequent call for applications.
10. The reasons why the rest of the applicants did not select a course are unknown to the evaluators.
11. The final control group was thus slightly smaller than originally anticipated (23.5% of the treatment group size, rather than 25%).
12. The Electronic Payroll has been implemented in Peru since 2008.
13. Of the 7,151 youths in the evaluation sample, 6,583 (92%) reported their NID during the application process to Projoventes. This may be related to the fact that the minimum age for enrolment in Projoventes was 16, and that until recently, Peruvians obtained their NID only at 18 years of age. Indeed, 98% of those in our sample who are missing NID information are 18 years old or younger.
14. The wealth index is an equally weighted average of having high-quality ceiling materials, brick walls, water inside the household, and sanitary facilities inside the household.
15. Given the randomisation, we do not need to include controls. These covariates have been included to improve the precision of the estimates. However, our main results are unaffected by their inclusion. Figures A.4 and A.5 in the Appendix report the main results without controls for age, sex, education, location, and wealth.
16. Results are shown in Figures A.1–A.3 in the Appendix.
17. Results for males (Figure A.1 in the Appendix) show that, although imprecisely estimated, the effect of Projoventes on formal employment and earnings are positive after the fourth year and that they only become close to zero after the start of the COVID-19 pandemic.

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## Disclosure statement

No potential conflict of interest was reported by the author(s).

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