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Impact Evaluation of the Job Youth Training Program *Projoven*[*]

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Abstract:

This paper brings new evidence on the impact of The Peruvian Job Youth Training Program (Projoven). Compared with prior evaluations of the program, this one has several advantages. This is the first experimental impact evaluation of Projoven, and also the first to measure impacts over a longer period: almost three years after training. Additionally, the evaluation supplements data from a follow-up survey with administrative data from the country's Electronic Payroll (Planilla Electrónica), allowing for a more accurate measure of formal employment. It also measures whether socio-emotional skills of beneficiaries improved with program participation. The evaluation finds a high long term positive impact of Projoven on formal employment. It also finds certain heterogeneity of program impacts across subpopulations. Impacts on formal employment vary depending on the beneficiaries' gender and age, with different patterns of statistical significance depending on the data source used to measure employment formality. Finally, it does not find significant impacts on socio-emotional skills.

Introduction

Most countries in Latin America and the Caribbean are affected by high levels of youth unemployment and/or labor informality (IDB, 2013). In the case of Peru, informality is the most serious of these problems. Around 9% of youths between 15 and 24 years of age are unemployed, and 80% of youths who are employed have informal jobs.[1]

To improve the labor market conditions of young people, governments in the region are implementing diverse Active Labor Market Policies. These policies aim to achieve short-term labor insertion or reemployment of unemployed workers or vulnerable people whose job placement is difficult (IDB, 2013). Short training programs are the most common active policy in the region. In general, they operate in urban areas and are geared towards the most vulnerable or underprivileged youths (Ibarrarán and Rosas, 2009). In addition, these programs seek to increase the human capital of their beneficiaries as a key mechanism for fast insertion into the formal labor market. Toward this end, they offer short-term technical training courses and formal work experience (through internships). Rigorous impact evaluations available for some of these programs reveal that, in the short term, this type of program does not improve beneficiaries' chances of obtaining a job, but it does increase the probability of getting a formal job, i.e. a higher quality job (González, Ripani, and Rosas, 2012). Given the programs' characteristics and the high levels of informality involved, especially in the case of the young population they target, this result is worth highlighting. On the other hand, evaluations of vocational training programs indicate that the impact of these programs tends to increase with time (Card et al., 2010), hence the importance of measuring their impact beyond the short term (two years).

The Peruvian Job Youth Training Program (Projoven), implemented between 1996 and 2010, was one of these programs. Previous evaluations of Projoven find positive and significant impact of the program in terms of employment and income. Nevertheless, these results must be interpreted with care, since the evaluations reveal methodological limitations that may bias their results.

This study seeks to correct the main limitations of previous evaluations of the program, and to introduce new evidence on the impact of Projoven.[2] The evaluation covers the period from the program's last public call for applications in 2009-2010 to the follow-up survey conducted in 2013. This term corresponds to a period of economic growth and an expansion of registered employment (formal employment) in the country. Rates of average annual economic growth and registered employment growth (formal employment) were 6% and 7%, respectively. However, a more detailed analysis demonstrates that these indicators started progressively slowing down around 2012.

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[1] Labor figures were extracted from the SIMS-LMK database of the Inter-American Development Bank (IDB) which is based on national households surveys. Informality is calculated according to the number of contributors or affiliates to the social security system, over the total number of wage-earners. Since wage-earners are involved, this definition of informality is mainly urban.

[2] The evaluation would not have been possible without the support of Peru's Ministry of Labor and Job Promotion (MTPE).

Compared with prior evaluations of the program, this one has several advantages. First, this is the first evaluation of Projoven that uses an experimental methodology to measure the program's impacts. Second, impact measurements are not short-term, since they were performed almost three years after beneficiaries left the program. Previous evaluations measured the program's impact after 6, 12, or 18 months. Third, this is the first evaluation of the program that combines data from a follow-up survey with administrative data from the Electronic Payroll (*Planilla Electrónica*), a registry with monthly reports of employment from formal employers with more than two workers. The Electronic Payroll registry makes it possible to measure formal employment directly, while employment surveys rely on self-reporting. Last, this is also the first evaluation aimed at measuring whether the program improves beneficiaries' socio-emotional skills, such as motivation and self-esteem. These skills are considered key for professional development and are valued by companies (González, Ripani, and Rosas, 2012).

In line with evidence from impact evaluations of similar programs in the region, this evaluation finds a positive impact of Projoven on formal employment. This impact is very high, reaching 20% according to data from the follow-up survey, and 17% according to data from the Electronic Payroll. When analyzing these figures, the very low levels of formality, for both the control and treatment groups, need to be taken into consideration. For instance, according to the follow-up survey, only 15% of youths from the control group hold formal employment. Another interesting result is that impact is only observed for the year 2013. As mentioned above, that year was characterized by a slowdown in the economic growth rate and registered employment. Thus, the result may indicate that, compared with the control group, the program did not succeed in improving young beneficiaries chances of finding low-qualification formal jobs in times when the economy was generating relatively higher levels of registered employment (which was the case from 2010 to 2012). However, it was able to do so during a period when there were fewer opportunities, in general, of finding a formal job. Another explanation might be that the program's impact tends to increase over time, given that when compared with individuals in the control group, beneficiaries must make up the time they spent in the program (lock-in-effect).

On the other hand, certain heterogeneity is observed in the program's impact, since there are impact variations on formal employment depending on the beneficiaries' gender and age, but the statistical significance of the impact varies depending on the sample used to perform the estimates. When considering the results of the follow-up survey, significant impacts are observed for almost 40% of men, and also for youths under 18. However, although results comparable in sign and magnitude are observed when using data from the Electronic Payroll, which captures the most formal employment in the economy, these results are no longer statistically significant. In contrast, statistically significant impacts of 26% and 35%, respectively, are obtained for women and youths older than 18. These differences in the estimates, depending on the source of information, may be related to limitations in terms of the size of disaggregated samples used for the estimates. They may also be related to other factors that require a more thorough analysis.

This document is organized in five sections. The first section introduces the background on Projoven and its main characteristics. The second section describes the existing evidence on impact evaluations of similar programs, and evidence from previous evaluations of the Projoven program. The third section explains the evaluation design as well as the data and methodology used. The fourth section introduces and analyzes the results of the evaluation. Finally, the fifth section presents the conclusions.

I. Background and Main Features of Projoven[3]

Projoven was created by the Ministry of Labor and Job Promotion (MTPE) in 1996, and was operational until 2010.[4] Its main objective was to facilitate access into the formal labor market for young people with limited resources, providing them with short-term training and labor market experience related to the needs of the productive sector—in other words, training oriented towards demand. Its design was based on a Chilean training program (ChileJoven) that was replicated in several Latin American countries (Ibarrarán and Rosas, 2009). The cost of the program per beneficiary (including operating costs and a stipend) was relatively low, around US$420.

Projoven offered in-classroom technical training for three months, later to be complemented with an internship for three additional months. The program did not directly provided in-classroom training, but hired private or public training agencies (PPTAs) that were responsible for the design and the provision of training for the program's beneficiaries. PPTAs courses were designed in coordination with firms in which beneficiaries would later do their internships. Firms committed to open internship vacancies for beneficiaries after they have completed the 3-month in-classroom training. In order to participate in the program the PPTAs had to submit letters of commitment they had signed with the different firms that would offer internships.

Young people who wished to participate in the program had to come in person to one of Projoven's official registration centers, where they filled out a personal information and socio-economic form in order to determine if they qualified to receive program's benefits and comprise the eligible pool.[5] Those who qualified had to choose their preferred course

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[3] The IDB loan document of Projoven presents more detailed information of the program: http://www.iadb.org/Document.cfm?id=420244. Also, see Gonzalez, Ripani and Rosas (2012) for a comparative analysis of Projoven and other similar training programs in Latin America.

[4] During its first period, between 1996 and 2004, the program was financed with resources from the Public Treasury (Call for Applications 1-12). Afterwards, during the 2005-2010 period, its implementation (Call for Applications 13-16) was financed with resources received through a loan from the Inter-American Development Bank (IDB). In total, during almost 15 years of service, the program trained approximately 73,000 youths. In 2010, after a change of government and the end of financing from the IDB, the MTPE decided to modify the program's design and name, becoming "*Jóvenes a la Obra.*"

[5] In order to determine eligibility, ie. if the candidates fulfilled the poverty/vulnerability conditions to enter the program. Projoven used a proxy means test to this end, based on a score constructed using observable socio-economic characteristics collected in the personal and socio-economic form.

from the list of training courses offered by PPTAs.[6] Once they selected a course, beneficiaries were sent to the corresponding PPTAs to pass a selection process. PPTAs' selection was usually based on vocational and basic skill tests as well as on interviews. This selection process at the PPTAs determined which eligible candidates were suitable for participation, and thus would get placed in a course, becoming Projoven's group of beneficiaries. Each PPTA was able to use its own procedures and criteria to assess the candidates and declare them suitable or unsuitable. This process took place until enough young people were assessed and declared suitable to cover the number of available openings for each course. Young people deemed unsuitable 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 PPTAs had filled their available vacancies.

Selected candidates moved on to the in-classroom technical training stage at a PPTA. The training courses were designed to provide the program's beneficiaries with low-skills required in marketplace jobs. For instance: knitting, sales support, and bakery. Unlike other programs in the region, Projoven did not require PPTAs to include modules on socio-emotional skills, such as motivation or self-esteem, in their training courses.[7] At the end of the course stage, beneficiaries moved on to on-site internships at firms. During the 3-month internship stage, beneficiaries received a stipend lower than the minimum wage as well as health insurance coverage. Both the stipend and the health insurance were afforded by the firms, since employment of interns had to be made through the Peruvian legal framework of vocational training agreements (a special type of contract between employers and interns).

The model used by Projoven and other similar programs in the region was designed to increase the human capital of underprivileged youths as a means to rapidly improve their chances of labor market insertion. By "human capital" we refer to skills (technical, cognitive, and/or socio-emotional) that can be improved through vocational training. This term also encompasses on-the-job training through internships in a formal firm. The logic behind Projoven rested on the observation that underprivileged young people ended their basic education without the required skills and/or lacked the experience or training required to obtain a formal job. Therefore, it was expected that the enhancing of their human capital would translate into easier access to their first formal job experience. This very first formal job experience, despite its short duration, could be equally or even more important than the skills learned during in-classroom training, since it would make it possible for young people to discover the benefits of formality and would motivate them to keep improving their skills in order to stay employed at a formal firm.

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[6] Youths who were not able to find courses they were interested in, or courses that met their expectations, had the possibility to reapply in the next call for applications, with a waiver for the qualification process. According to the program's administrative information, the percentage of youths who returned to Projoven after dropping out from the application process varies between 2% and 3%.

[7] The importance of these skills (perseverance, self-esteem, teamwork, etc.) for work performance is increasingly recognized in the academic literature. For instance, see Heckman et al. (2006) and Bassi et al. (2012). For a discussion on this subject, see also González, Ripani, and Rosas (2012). In general, there is little evidence for the region (Calero et al., 2013).

Another mechanism through which this type of training programs could improve their beneficiaries' labor insertion rates is the information (signals) provided to the productive sector about the beneficiaries. Specifically, by conducting a selection process and offering a first formal work experience, these programs would certify that their beneficiaries were suitable for the formal work environment.

The evaluation was designed to measure Projoven's degree of success in improving labor market insertion for its beneficiaries, both in terms of employment levels and of the quality of this employment. Also, although Projoven did not formally offer courses focused on improving the socio-emotional skills of its beneficiaries, the evaluation study assessed whether the program improved this type of skills. These outcomes are considered because qualitative evidence from González, Ripani, and Rosas (2012) suggested that some PPTAs offered this type of courses, since they were regarded essential for the success of their training process and for reducing drop-outs.

It is worth mentioning that given these programs' characteristics, their success depends on their operation in periods, and/or sectors, and/or geographical areas where formal (low-skilled) employment is generated, and also on the credibility and good standing of the programs in the productive sector.

II.   Evidence on the Impact of Youth Job Training Programs, and previous studies on Projoven

The economics literature on the short-term impact of vocational training programs in developed countries is vast. There is great heterogeneity in the effects observed, and it varies depending on the characteristics of participants and type of training.[8] Regarding vocational training programs for young people, they are generally observed to have a lower impact than programs for adults (Betcherman, Olivas, and Dar, 2004; Card et al., 2010; Kluve, 2010). There is less evidence regarding their long-term impact and conclusive results cannot be obtained (see, for example, Fitzenbergery et al., 2006; Schochet et al., 2008).

Experimental evidence on the impact of youth training programs in Latin America has been increasing during the last 15 years. Most of the existing evaluations measure the program's impact in the short term, i.e., less than two years after beneficiaries finished the program. These evaluations[9] present much more encouraging results than those from developed countries. In general, youth training programs in Latin America do not have an impact in terms of employment, but they do in terms of employment quality, i.e. the possibility of finding a formal job (with employment contract and/or medical insurance

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[8] See, for instance, Heckman et al. (1999); Dar and Tzannatos (1999); and Betcherman, Olivas, and Dar (2004).

[9] See for instance the evaluation of similar programs in Dominican Republic (Card et al., 2011; Ibarrarán et al., 2014); Colombia (Attanasio, Kugler, and Meghir, 2011); Brazil (Calero et al., 2015); and Argentina (Alzúa, Cruces, and López, 2013).

and a retirement pension), and in terms of labor-based income (Ibarrarán and Rosas, 2009). Furthermore, there is great heterogeneity in the results, according to beneficiary type (for example, men vs. women) and where the programs were implemented (for example, capital city vs. other cities). Additionally, some of these evaluations analyze if the programs improve the socio-emotional skills of their beneficiaries, but do not obtain conclusive results (Ibarrarán et al., 2012; Calero et al., 2013). Recently Ibarrarán et al. (2015) presented results for the first long-term impact evaluation of this type of programs. The authors evaluated the impact of a youth training program in Dominican Republic after six years of participation and found impacts on the quality of employment, suggesting that the effect of the program in employment formality is maintained through time.

In the case of Projoven several previous evaluations find statistically significant impacts on employment rates, quality of employment, and monthly income of beneficiaries (Díaz and Jaramillo, 2006). These results can be explained because all of the evaluations are of a non-experimental nature and are not able to adequately correct two potential sources of selection bias. First, the control group includes young neighbors of beneficiaries of the program who did not apply to Projoven, but who had similar (identifiable) socio-economic characteristics. The fact that these individuals were not seeking to participate in Projoven may induce a selection bias in a non-experimental context due to possible self-selection of program participants based on unobservable characteristics to the evaluator. For instance, there are systematic differences regarding the benefits that beneficiaries could expect to obtain from the program, or differences in non-observable opportunity costs that may explain the differences between the beneficiaries and a non-experimental control group.

Second, there may also be a selection bias, given that the final selection of beneficiaries accepted into the training courses relied on the evaluation criteria used by PPTAs. In fact, the selection of beneficiaries was based on criteria that the evaluator did not observe, thus generating a potential source of selection on unobservables. Given that individuals in the group of eligible candidates (qualified applicants) are homogenous throughout various observable dimensions, and that PPTAs were not authorized to use gender, race or general characteristics to conduct selection, the group of beneficiaries could be systematically different in some other characteristics from the group of applicants who were not admitted. Specifically, PPTAs had incentives to choose the best candidates from the pool of eligible youths, due to monetary penalties that apply in case their trainees were not able to complete the in-classroom training stage or when they were not able to reach the on-the-job-training stage. For example, during their processes of selection, PPTAs could use proxies for soft skills or socio-emotional skills like the applicant's

motivation and punctuality.[10] As long as these features were related to the individual's performance, beneficiaries could be systematically different from non-beneficiaries.[11]

Consequently, previous evaluations of Projoven do not guarantee that the true effects of the program can be identified without bias. This would explain the existence of great variability in the results among the different studies. For instance, impacts on labor earnings six months after the end of the program range from 12% to 100%.[12] Additionally, none of these evaluations are focused on Projoven's potential effect on the beneficiaries' socio-emotional skills.

Lastly, these evaluations only measure the impact of the program 6, 12 and/or 18 months after participants left. Thus, there is no measurement over a longer period of time or long-term, a limiting factor since the impact may vary with time (Card et al., 2010).

III.   Evaluation Design, Period of Evaluation, Methodology and Data

1. *Evaluation Design*

To avoid problems that may have affected the results of previous evaluations of the program, this evaluation introduces several innovations, including the adoption of an experimental evaluation design. It was possible to implement this design because Projoven had an excess of demand; there were more young people applying to the program than the number of available vacancies the PPTAs were able to serve.[13] Another characteristic of the evaluation is that the random allocation had to be done based on the group of applicants who had already been declared suitable by PPTAs. In other words, these youths had already gone through the process of qualification, course selection, and evaluation by a PPTA. This should have reduced the risk of potential self-selection biases

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[10]As mentioned above, Projoven did not require that PPTAs include modules for socio-emotional skills in their courses. Nevertheless, some PPTAs provided workshops to improve these skills during the in-classroom training stage, since they were considered crucial for the success of the technical training process, and in order to reduce drop-out levels (González, Ripani, and Rosas, 2012). This may be an indication of the existence of the bias mentioned above, since PPTAs were aware of the market value of these skills.

[11] The importance of these potential sources of selection bias in non-observable characteristics must not be minimized. As shown by Ñopo et al. (2002) and Díaz and Jaramillo (2006), Projoven beneficiaries show the so-called "Ashenfelter's dip" in labor participation and income: both the employment rate and average income levels decrease during the months before their participation in the program, which does not happen among youths from the control group in data from previous evaluations of Projoven. This is likely to occur due to features that make these two groups different, but that the evaluator is not able to observe in the data.

[12] Longitudinal methods, which use information extracted from various time-points, may help to reduce the selection bias under the presumption that systematic discrepancy factors among the beneficiary and comparison groups are invariable in time. In particular, Díaz and Jaramillo (2006) demonstrate that comparisons between longitudinal and transversal methods generate great differences in estimating the impacts of the program.

[13] For the 16th call for applications, the program originally had less than 10,000 vacancies, but 26,770 youths enrolled.

by participants as a result of their options beyond their participation in the program and non-observable characteristics (such as motivation). As it will be seen later on, other innovations include incorporating measurement instruments for socio-emotional skills and long-term outcome measurements.

The design of the evaluation required Projoven to introduce minor modifications in its regular operation processes. First, using a computer assisted program, Projoven had to assign a random number to each individual who applied to the program. For each course,[14] PPTAs had to determine suitable candidates for admission among the total number of applicants who had selected each specific training course. Unlike previous processes, PPTAs were not allowed to determine which candidates would fill a vacancy in a particular course, since the final selection of program´s beneficiaries was performed by random assignment by Projoven in coordination with the evaluation team. In courses with excess of demand (meaning those with more suitable candidates than vacancies), candidates were sorted according to their random number (see Diagram 1). The first on the list would obtain a slot in the course and would make up the treatment group; the rest would make up the control group.[15] If a specific course had less suitable candidates than available vacancies, all applicants would be placed in the course. Only applicants from courses with excess of demand would be included in the final sample of the study.

Second, Projoven had to assign a unique identification code to each applicant, and this number had to be the same in all of the program's processes the youths had to go through. Third, in each course with an excess of demand, PPTAs had to assess and preselect at least 25% additional suitable applicants with respect to the number of available vacancies. This extra 25% of suitable applicants over available vacancies would be used to generate the control group. Fourth, in case of drop-outs during the first week of classes in courses with excess demand, PPTAs were not able to freely select their replacements. Instead replacements had to be selected according to the initial random order. Finally, youths from the control group could not re-register for the program's next call for applications.

The evaluation began in early 2009, when Projoven launched its 16th public call for applications, and interested youths began to apply. A total of 26,770 individuals applied, of whom 23,666 (88%) were declared eligible using the proxy means test. Of these, almost 15,000 selected a course.[16] Nevertheless, around 3,500 young people who selected a course abandoned the program before the evaluation stage at the PPTAs. Among the eligible individuals who selected a course, 11,713 were assessed by PPTAs. Of this total, 7,464 (64%) registered in courses with excess of demand in their first choice of course.

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[14] In fact, when we say "courses" we refer to the course and a specific section, since some courses at some PPTAs were offered in different sections.

[15] Both, the number of youths that select a specific course and the number of available vacancies for the courses, vary between courses and PPTAs. Therefore, the individual probability of being selected as a beneficiary is not the same for all youths, and one must consider not only the random allocation but also a record of the course and PPTA selected.

[16] There is no information that makes it possible to analyze the reasons for which the rest of the youths did not select a course, or what happened with them.

Of these, 7,151 (96%) were declared suitable applicants by PPTAs, and therefore the random sorting was applied to them. Thus, for their first course choice, 5,791 individuals were randomly assigned to fill a vacancy and formed the treatment group, while 1360 individuals were randomly assigned to be part of the control group.

In practice, the evaluation design was not implemented exactly as planned. PPTAs faced problems and delays in reaching the additional 25% of youths with respect to the number of available vacancies per course. This led Projoven to eliminate this condition after a few weeks,[17] and it's why not all PPTAs sent an additional list with 25% more suitable applicants. This also explains why the control group is smaller than originally planned in the evaluation design. Likewise, the program did not use a unique identification code for participants in all its operational processes, which makes the evaluation analysis harder.

Moreover, as it often happens with experimental evaluations of public policies, there was an imperfect compliance of the experimental design. This happened in part because Projoven allowed participants to make a second or third course selection. Thus, applicants deemed unsuitable, or those who did not obtain a vacancy for their first course choice were able to make a second and even a third course selection. As a result, some of the youths from the control group were able to obtain a vacancy in a different course. Consequently, for those applicants whose first chosen course had an excess of demand, the effective allocation to courses does not strictly correspond to the random assignment to treatment and control groups under the established protocol. An analysis of the program's administrative data shows that certain youths from the control group made a second and/or third course selection, obtaining a vacancy in some cases. The option for a second or third course selection for applicants who did not obtain a vacancy in their first selection is the result of self-selection that reflects their motivation, desire or need to participate in the training. Additionally, not all youths who started the training made it to the on-site internship stage. In this way, the group of youths who received complete training differs from the randomly assigned treatment group. This is also the result of self-selection.

Table 1 presents the absolute numbers and percentage structure of the experimental treatment and control groups over both stages of the program. It shows that since the beginning of the training courses, there were discrepancies between the random assignment to treatment and control groups and the effective allocation of youths to training courses. During the first few weeks, 1% of youths assigned to the treatment group abandoned the program and did not begin the training stage at a PPTA. At the same time, 39% of youths assigned to the control group began the course stage at a PPTA because they had the chance to make a second or third course selection. Upon reviewing the situation at the end of the training stage at PPTAs, it was found that 83% of youths assigned to the treatment group and 32% of youths assigned to the control group completed this stage. If the situation is analyzed at the end of the internship stage, then

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[17] In particular, due to operational pressures in the development of enrollment, accreditation and evaluation processes, the additional percentage was reduced to 10%.

only 52% of youths assigned to the treatment group also completed the internship stage and thus completed the program. For the control group this percentage is 22%.

2. Period of Evaluation

The evaluation period goes from early 2009 to mid-2013. This period comprises the longest period of high economic growth achieved by Peru between 2002 and 2013, with the exception of the year 2009, when the economy was affected by the international financial crisis and only grew 0.9%. The average growth rate during the 2002-2013 period was 6.1%. With the economic expansion came an increase in employment rates, particularly of formal employment.

Figure 1 makes it possible to conduct a more detailed analysis of the evolution of GDP and formal employment during the period 2008-2014. Registered employment in the Electronic Payroll was used as an indicator of formal employment. As we will explain later, registered employment is the economy's most formal employment. The first finding is that registered employment is highly sensitive to the business cycle: its growth rate was almost insignificant in 2009, the year in which the economic bust took place, and it increased again when the economy recovered. The second finding is that, beginning in 2012, Peru's GDP growth rate saw a steady decline, followed by a slowdown in registered employment levels and by 2013, registered employment increased only 4% its lowest growth rate besides the one observed in 2009.

3. Evaluation Data

The evaluation sample is comprised of 7,151 youths that were assigned to treatment and control groups in training courses with an excess of demand (on their first course choice).

The final sample used in the evaluation is comprised of 2,924 youths from eight major cities in Peru and who have baseline and follow up data. The data comes from three sources of information, which are described next.

The baseline data

Baseline data were collected in early 2009, during the process of enrollment of applicants. All applicants were required to fill a personal and socio-economic form, used by the program to determine eligibility. This information was complemented with data retrieved from an additional questionnaire attached to the socio-economic form in order to obtain more detailed information on household demographics, recent labor force participation history prior to the enrollment in the program, and a measure of self-esteem based on the Rosenberg scale.

Therefore, compared to previous evaluations of the program, this baseline has two main advantages. First, information is collected on all youths enrolled in the program, not only

on youths from the evaluation sample. Second, the information collected is the same for all enrolled youths and was collected during the same period of time.

The follow-up data

A follow-up survey was targeted to a sample of 4,509 youths selected from the evaluation sample. This sub sample is comprised of youths from all courses with an excess of demand at eight major cities in the country: Lima (the capital city), Ica, Arequipa, Huancayo, Chiclayo, Trujillo, Piura, and Cusco.

The collection of data was conducted between November 2012 and March 2013.[18] Thus the information available corresponds to three years after graduation from Projoven. In total 2,924 youths were interviewed; this represents 65% of the survey sample. Of these youths, 2,378 belonged to the treatment group and 546 to the control group. There are two main reasons for which the rest of the target youths could not be reached: i) they had moved (close to 50%), and ii) the address included in Projoven's records was incorrect (27%).

MTPE's administrative records: the Electronic Payroll

Administrative records from the MTPE are also used for the evaluation and come from the Electronic Payroll. This is an electronic document that formal employers with more than two workers are required to submit through the National Superintendence of Customs and Tax Administration (SUNAT). Formal employers are required to declare their payroll workers, pensioners, service providers (including trainees covered by vocational training agreements), third-party staff, and beneficiaries.[19] Registered employment represents the most formal employment in the Peruvian economy, since employers who report to the Electronic Payroll comply with all labor benefits and entitlements established by Peruvian labor laws.

Access to information from the Electronic Payroll is restricted, and was accessed with express authorization from MTPE, with the goal of identifying the impact of Projoven in formal employment indicators. To be able to use data on registered employment and income, the information from Projoven's records was matched with the information from the Electronic Payroll through the (unique) number of the Peruvian National Identity Document (NID).

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[18] Interviews were performed face-to-face. The questionnaire includes modules on employment and labor earnings designed to retrieve information on outcomes such as employment, access to health insurance and retirement pension, employment contracts, hours worked, and earnings. The questionnaire includes a module on socio-emotional skills that comprise the Rosenberg scale of self-esteem and the Duckworth scales of perseverance and ambition. It also includes a module designed to explore beneficiaries' perceptions about the program.

[19] The Electronic Payroll has been implemented in Peru since 2008.

All the information extracted from the Electronic Payroll was processed by MTPE's Statistics Department, maintaining data confidentiality. From a total of 7,151 youths in the evaluation sample, 6,583 (92%) reported their NID during the enrollment process and were available from Projoven's records.[20] Only these youths were searchable in the Electronic Payroll records that cover the period from January 2009 to June 2013. Of them, 3,590 (55%) appeared at least once in these records.

The two key indicators from the Electronic Payroll that are used for the evaluation are: registered employment and income. The income variable is expressed discretely and refers to earnings above or below the legal minimum wage level of 2010.

Outcomes constructed using information from the follow-up survey based on self-reports on employment and earnings are complemented with outcomes constructed using information on formal employment and labor income from the Electronic Payroll. These data are available for almost all youth who completed the follow up survey (2914 out of 2924).

Attrition

Attrition of the sample is high. About 35% of the follow-up survey sample was not contacted. To determine if such attrition has a systematic relationship with the random allocation of youths to treatment and control groups[21], the following regression is estimated:

$$A_i = \theta_0 + \theta_1 Z_i + \sum_j \theta_{2j} C_{ij} + \varepsilon_i,$$

where i refers to the applicant; j refers to the training course in the first choice of courses; A_i is a dummy variable that takes a value of 1 when the applicant does not have follow-up information and value of 0 otherwise; Z_i is a dummy variable that represents the result of the random assignment and takes a value of 1 for the treatment group and a value of 0 for the control group; C_{ij} represents fixed effects for each of the courses for which there was an excess of demand, and ε_i represents the error term.

Table 2 presents the results of the estimates. There is no evidence that attrition is related to allocation to treatment or control groups. This happens both for the whole sample and when it is separated by gender, age groups, or in areas such as Lima and other urban areas (OUA). In none of these allocations is the coefficient related to the random allocation significant in a statistical sense.

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[20] This may be related to the fact that the minimum age for enrollment in Projoven was 16, and that until recently, Peruvians obtained their NID only at 18 years of age. Currently, the NID is mandatory for all newborns.

[21] Equivalence between attritors in the treatment and control group was also tested. There is no evidence suggesting systematic differences among the observables characteristics of attritors related to the program assignment. Table A4 in the appendix presents these results.

Balance of characteristics in the baseline

Table 3 shows the analysis of statistical equivalence between youths from the treatment and control groups available at baseline, which comes from the information included in the socio-economic questionnaire used during the accreditation process. The comparison is done through the following regression:

$$X_i = \delta_0 + \delta_1 Z_i + \sum_j \delta_{2j} C_{ij} + \varepsilon_i,$$

where X_i is the baseline variable; Z_i is the result of the random assignment and takes a value of 1 for the treatment group and a value of 0 for the control group; C_{ij} represents fixed effects for each of the courses with excess of demand, and ε_i represents an error term. The difference in means for the treatment and control groups is obtained when estimating the coefficient δ_1.

This analysis is performed for two groups. First, it is performed for the complete sample of youths (7,151 youths in the baseline). This allows to verify that the random allocation of treatment and control groups makes it possible to balance the characteristics of youths in both groups. The same analysis is performed for the sample of youths who responded to the follow-up survey (2,924 youths with follow-up data). This makes it possible to verify whether baseline characteristics remained balanced between the treatment and control groups in the sample for which there was outcome information available after graduation from the program.

The characteristics observed in the baseline are balanced between the treatment and control groups, with the exception of having completed a technical course, being a homeowner, and having water services inside the household. None of the three samples show systematic differences in the characteristics of these youths, such as their gender, age, and years of basic schooling. There are also no differences in the head of the household's, gender or education level, nor in the characteristics of the household, such as material on floors, walls, or ceilings, nor in phone lines. Also, the qualification score variable assigned by Projoven does not show any differences between youths in the treatment and control groups.

The baseline also includes a questionnaire containing, among other variables, Rosenberg's self-esteem scale and work history prior to the candidate's participation in the 16th call for applications.[22] Table 3 shows that there are no systematic differences in the self-esteem scale.[23]

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[22] The question for the self-esteem scale presents 15% of cases with incomplete information preventing the calculation of the specific score for youths who did not answer the question. Therefore, a variable was generated that indicates whether the participant does not have self-esteem information and thus has been assigned the average value observed. The variable has been standardized with respect to average values for the control group.

[23] A similar analysis was performed for work histories before participation in Projoven, and no significant differences are observed.

These results suggest that the original random allocation into treatment and control groups makes it possible to balance the characteristics between the two groups. On the other hand, this exercise also makes it possible to observe that there was a similar number of men and women in the program, and that its targeting worked relatively well. On average, beneficiaries of Projoven came from households with scarce economic resources. Also, on average they were only 19 years old and had secondary studies (complete or incomplete).

4. Methodology for Impact Evaluation

To estimate the effects of Projoven, we use the random assignment of youths into treatment and control groups at the time they made their first course selection. The intention-to-treat (ITT) effect is estimated by running the following regression:

$$Y_i = \alpha + \beta Z_i + \sum_j \gamma_j C_{ij} + \sum_k \delta_k X_{ik} + \varepsilon_i,$$

where *Yi* is an outcome variable; Z_i is an indicator of the random assignment result and takes a value of 1 for the treatment group and a value of 0 for the control group; C_{ij} represents fixed effects for each of the courses for which an excess of demand was generated; X_{ik} represents characteristics observed in the baseline, included in order to increase the accuracy of the estimations, and ε_i represents the error term. The ITT effect reflects the difference in the outcome indicator between treatment and control groups, and is obtained by estimating the coefficient β.[24] Standard errors were clustered at the course level.

Outcome indicators

The follow-up survey makes it possible to analyze several outcome indicators aggregated in four groups: (A) labor market insertion and quality of employment (is employed, has health insurance, pension, salaried employment, contract, weekly hours worked); (B) income (per month and per hour, expressed in logarithms); (C) income conditional to remunerated income (per month and per hour, expressed in logarithms); and (D) socio-emotional measures (Rosenberg's scale of self-esteem, and Duckworth's scales of perseverance and ambition).[25]

The Electronic Payroll is used to complement the information obtained in the follow-up survey. The first indicator is registered employment, and refers to a job that complies with all requirements for formal employment, which is registered and declared by all formal employers. This indicator is of a discrete type and is assigned a value of 1 when there is verification that the youth had a registered employment in the Electronic Payroll,

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[24] The Local Average Treatment Effect (LATE) was also calculated. The results are presented in the Appendix.

[25] Socio-emotional indicators were standardized with respect to the mean and standard deviation of the control group.

and is assigned a value of 0 in other cases. This indicator is calculated for every month in the period of analysis, but it is also possible to calculate it in an aggregate manner by year. For instance, it is possible to determine if the participant had a registered job in any month during 2011, 2012, or 2013. In this case, the indicator is assigned a value of 1 when the applicant had a registered job at least once during any month of a given year, and a value of 0 when they did not have a registered job during the whole year. Alternatively, from the binary indicator it is possible to determine the number of months the applicant was at a registered job for each of the years for which there is information available.

The second is an income indicator that for confidentiality reasons was obtained as a discrete variable. To generate the indicator, groups of one twentieth of the distribution of income in the Electronic Payroll for 2010 were used. Twenties from that year were used to define 20 groups based on income for each year for which there was information available in the Electronic Payroll. For 2010, each group accounts for 5% of registered employment in the Electronic Payroll for that year. For the remaining years, each group represents a higher or lower percentage, according to how income levels reported in the Electronic Payroll evolved. To simplify the analysis, a binary-type indicator is defined based on these 20 groups, and is assigned a value of 1 when the income group is higher than S/.550, amount that corresponds to the legal minimum wage until November 2010; otherwise it is set to 0. As for the registered employment indicator, it is possible to determine if the applicant had an income higher than S/.550 during any month for a year when information is available, as well as the number of months during which their income was above this amount for each of those years.

Figure 2 shows the evolution of registered employment and income indicators calculated from the Electronic Payroll based on the initial random allocation (Panel A), and also the formal employment indicators calculated from the follow-up survey (panel B).

Panel A shows very similar trends during the first months of 2009, suggesting that the random allocation was able to balance registered employment and income indicators. Additionally, a similar, increasing trend in registered employment and income is observed between the randomly allocated treatment and control groups during the period from 2010 to 2013, and corresponding to the period after the training. Only towards the end of the period with available information, between January and June 2013, is there an observable advantage in registered employment levels in favor of the treatment group versus the control group, although this is not true for the registered income indicator. On the other hand, Panel B shows a growing evolution in the indicators of formal employment (health insurance and access to a pension) with a clear advantage for the treatment group.

IV.   Analysis of the Impacts of Projoven

Table 4 presents the results of ITT estimates.[26] The second column of the table shows the results for the total sample of youths who were interviewed for the follow-up survey. The following columns respectively show results for only those youths who are women, men, those between 14 and 18 years of age, those between 19 and 26 years of age, youths residing in Lima, and youths residing in other cities.

We do not observe a statistically significant impact of Projoven on employment but rather on the quality of employment (formal employment). Some degree of heterogeneity is also observed, since the impact on quality of employment is only maintained for youths who are men and for youths who are younger. Therefore, the program would have increased the chances for these applicants to obtain access to a formal job inside a context characterized by high youth informality. These results are consistent with short-term impacts found in evaluations of similar programs in the region (González, Ripani, and Rosas, 2012).

Specifically, the probability of having a job with health insurance and the probability of having a pension increase in 3.8 and 3.3 percentage points, respectively, for the treatment group when compared to the control group.[27] The percentage of youths in the control group who reported having a job with health insurance (15.6%) and those who reported having a job with a pension (15.0%) are relatively low. By taking this into account, then the effects are of considerable magnitude, since they represent an increase greater than 20% in the probability of having health insurance or a pension for the treatment group. The magnitude of these effects is almost twice for male youths and those between 14 and 18 years of age.

There are no significant effects on employment or in labor income indicators (panel C) and conditional labor income for having a remunerated job (panel D). None of the estimated coefficients are significant in statistical terms, although all estimated values are positive. These results differ from findings from previous non-experimental evaluations of Projoven, in which there were incremental effects of very large magnitude in terms of the employment and labor income of beneficiaries.

There are no significant impacts neither on the socio-emotional indicators (panel E). Both for the case of Rosenberg's self-esteem scale and for Duckworth's perseverance and ambition scales, values estimated for the ITT effect are positive, but none of them are statistically significant. This result is not surprising, since in strict terms, the program did not offer a socio-emotional skills module.

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[26] The LATE was also estimated. These estimates are included in the Appendix of this document. As expected, the main results are maintained and what varies is their magnitude.

[27] An alternative measure of formality was also tested which consists on aggregating the three formal employment indicators (having health insurance, retirement pension and contract). The overall positive impact on formality remains significant.

Supplementary analysis with outcomes from the Electronic Payroll

Information collected from the Electronic Payroll makes it possible to supplement the indicators obtained from the follow-up survey with indicators of registered employment and income, which reflect employment with a higher degree of formality in the Peruvian economy. Also, since such information is based on administrative data, potential measurement errors that could have affected the follow-up survey are corrected.

As shown in Table 5, the impact of Projoven in formal employment is positive and statistically significant. Although when compared to the estimates based on the follow-up survey, effects of lesser magnitude are observed, since the effect is 17%. This difference may be explained by the fact that the Electronic Payroll captures the more formal employment of the economy.

The impact of the program is only evident in the year 2013. This result is consistent with the results shown in Figure 2. For instance, beneficiaries have 0.22 more months in 2013 in registered jobs than those from the control group, which is equivalent to a 19% effect. This result may be related to a growing trend of the program's effect over time, and/or a decrease of registered employment during that same year, as Figure 1 indicates.

On the other hand, there are different results to those emerging when the effects are estimated with the follow-up survey. First, in this case, a statistically positive effect on the number of months with income higher than S/.550 for the year 2011 and for the whole period between January 2011 and June 2013 is observed,[28] although with a statistical significance of only 10%. The estimated effect suggests that youths from the treatment group had 0.31 additional months of employment with income above S/.550 when compared to the control group during the year 2011. This represents an increase of 18% with regard to the control group.

Second, even though there also was heterogeneity in the results based on the gender and age of beneficiaries, there are statistically significant effects for women and youths over 18. In the case of women and youths older than 18, there is a statistically significant effect on registered employment of 26% and 35%, respectively. A significant impact of 10% may also be observed for beneficiaries residing in Lima, amounting to 20%.

Differences in the results of estimates with different samples may be related to several factors. First, the differences in magnitude of the effect of the program on labor formality may be explained by the different nature of the information captured in the follow-up survey and the Electronic Payroll. Specifically, the first one captures the views of youths on formality, and the second captures the formality declared by firms themselves. It may be that some youths think of themselves as formal workers because they work at formal firms, but these companies do not register them as formal workers and thus do not declare them in their Electronic Payroll.

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[28] For the year 2013, there only is information available from January to June.

Second, these differences may be linked to problems in sample size. The fact that the sign and magnitude of the impact are similar in estimates from the follow-up survey and Electronic Payroll, but not in statistical significance, leads one to suppose that this may be one of the explanations.

Third, for the case of youths under 18, the differences may be explained by the fact that formal firms in Peru prefer not to hire (or register) under-aged youths. For the case of women, the differences could be related to differences in the preferences and opportunities of youths in the labor market according to their gender.[29] An occupation analysis of these youths based on data from the follow-up survey suggests that there are disparities between women and men. For instance, there are more women who work as employees (50%) than men (42%). Furthermore, the occupation second highest in demand by women is sales (19%), while for men it is laborer (30%). Last, the percentage of women without a job is more than twice that of men (44% versus 21%, respectively).

Finally, the differences may also be explained due to attrition problems which would affect the sample in the Electronic Payroll, since estimates were performed without taking into account youths without a NID.

V.   Conclusions

Projoven was implemented in Peru between 1996 and 2010. The program stood out as the main public policy for supporting the insertion of the country's underprivileged urban youths into the formal labor market, and because it was the public program that has had the greatest number of impact evaluations, though they were all made before 2006.

This new evaluation of the program is really innovative for several reasons. It is Projoven's first experimental impact evaluation, and also the first to measure its impact over a longer period: almost three years. Additionally, it is the first evaluation to supplement data from a follow-up survey with information from the country's Electronic Payroll, allowing for a more accurate measure of the program's effect in terms of employment formality. Lastly, the evaluation is also innovative because it introduces measurements of socio-emotional skills.

In general terms, the results of this evaluation differ from those obtained from previous cuasi-experimental evaluations of Projoven, but are aligned with the results of experimental evaluations of similar programs in other countries of the region. Specifically, it may be said that the program increased the opportunities of finding a formal job, in a context of high labor informality.

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[29] In order to verify consistency of the results, we carried out two additional exercises. First, we estimated ITT using registered employment and income for the sample resulting from combining Projoven's baseline with the Electronic Payroll through the NID. Second, we reestimated the ITT for these same outcomes using the sample targeted for the follow-up survey in eight mayor cities (not only the sample who responded to the survey). In both cases, we found similar results to those reported in the document.

The results in formality levels are noteworthy for the magnitude of the impacts as much as the fact that they were observed three years after beneficiaries completed the program. They are especially noteworthy given that the goal was to achieve short-term labor market insertion with a slightly intensive intervention (technical course and internships in firms, for a total maximum duration of six months).

Differentiated results are also obtained according to gender, age and place of residence of youths. Statistically significant impacts are observed on (self-reported) employment formality and income for men and the youngest. Statistically significant impacts are also observed on registered employment and income only for women and youths older than 18 years. These differences may be explained by various factors, for example: limitations in terms of sample size. They may also be related to the specific characteristics of the local labor markets in Peruvian cities, and to preferences and/or opportunities young job seekers have depending on their gender. In any case, it is a matter that needs to be studied in more depth.

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Diagram 1: Selection Process of Eligible Individuals and Beneficiaries of Projoven
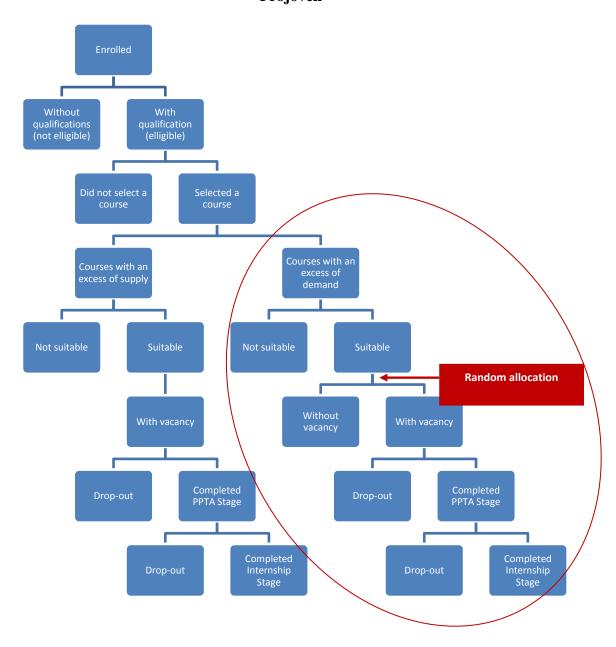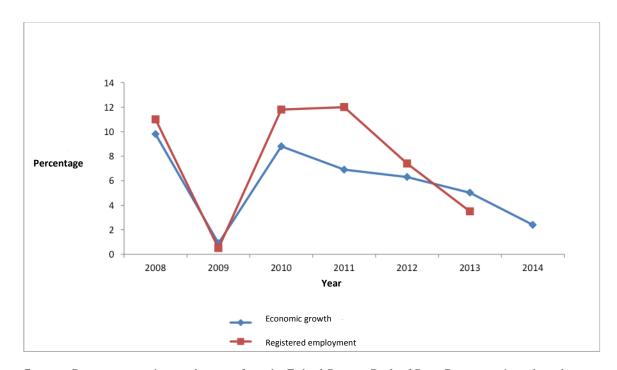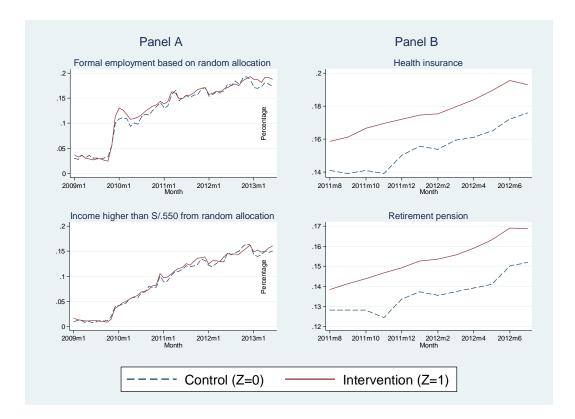
**Figure 1: Evolution of Economic Growth and Registered Employment Rates
between 2008 and 2014**

Sources: Data on economic growth comes from the Federal Reserve Bank of Peru. Data on registered employment comes from the Electronic Payroll records of the Ministry of Labor and Job Promotion (MTPE).

Figure 2: Evolution of Registered Employment Indicators in the Electronic Payroll (Panel A) and Formal Employment Indicators in the follow up survey (Panel B)

Note: The figure was generated with Electronic Payroll data for the sample of youths who possessed a NID (92% of the total sample) and youth in the follow-up survey.

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						
Si	3,028	52	298	22	3,326	47
No	2,763	48	1,062	78	3,825	53

Note: Generated from Projoven's records.

Table 2: Sample attrition and random allocation to treatment and control groups

	Control group level	Treatment/control difference (std.err.)	N	R2
All	0.370	-0.027	4,509	0.096
		(0.020)		
Women	0.365	-0.035	2,583	0.11
		(0.026)		
Men	0.377	-0.026	1,926	0.174
		(0.033)		
14 -18 years old	0.339	-0.014	1,982	0.168
		(0.030)		
19 - 26 years old	0.393	-0.033	2,527	0.121
		(0.027)		
Lima	0.306	-0.033	1,695	0.055
		(0.029)		
Other cities	0.421	-0.023	2,814	0.099
		(0.027)		

Note: The table reports the results of the attrition regression for different samples and groups. The dependent variable is assigned value 1 when the observation does not have follow-up information. The second column reports the estimated coefficient for the variable of random allocation to the treatment group. The regression is controlled with fixed effects from the course-section. Standard errors have been estimated using clusters per course.
Significance: * p<0.1; ** p<0.005; *** p<0.01. The statistical significance is: * p<0.1; ** p<0.05, and *** p<0.01.

Table 3: Equivalence between the treatment and control groups

	Full sample			Sample with follow-up data		
	N	Control group mean	Treatment/ control difference (std. err.)	N	Control group mean	Treatment/control difference (std. err.)
Gender: Male	7,151	0.47	-0.005	2,924	0.40	-0.004
			(0.013)			(0.021)
Age	7,151	19.22	-0.067	2,924	19.13	0.021
			(0.071)			(0.115)
Years of schooling	7,151	10.48	0.016	2,924	10.56	0.023
			(0.038)			(0.058)
Took courses at an institute or university	7,146	0.02	0.008	2,923	0.03	0.011
			(0.005)			(0.009)
Took a technical/trade course	7,146	0.13	0.018*	2,923	0.16	0.000
			(0.011)			(0.018)
Gender of head of the household: Male	7,146	0.64	0.010	2,923	0.63	0.003
			(0.015)			(0.024)
Schooling of head of the household:						
Complete higher education	7,151	0.34	-0.001	2,924	0.34	-0.005
			(0.015)			(0.024)
Incomplete higher education	7,151	0.19	0.003	2,924	0.20	0.022
			(0.012)			(0.020)
Complete secondary education	7,151	0.39	-0.001	2,924	0.37	-0.005
			(0.015)			(0.024)
Incomplete secondary education	7,151	0.04	-0.002	2,924	0.04	-0.004
			(0.006)			(0.010)
Elementary/ illiterate	7,151	0.05	-0.001	2,924	0.05	-0.008
			(0.006)			(0.010)
Overcrowding (people per room)	7,146	3.04	-0.016	2,923	3.26	-0.023
			(0.053)			(0.088)
Housing: Proprietorship	7,146	0.69	-0.046***	2,923	0.78	-0.051**
			(0.014)			(0.022)
Floor material: Floor tile, cement, wood	7,151	0.48	-0.008	2,924	0.49	0.015
			(0.014)			(0.023)
Ceiling material: Concrete, cement, tiles	7,151	0.47	-0.004	2,924	0.59	-0.021
			(0.014)			(0.024)
Wall material: Brick	7,151	0.50	0.014	2,924	0.58	0.032
			(0.014)			(0.023)
Water: Inside the household	7,151	0.81	-0.010	2,924	0.82	-0.037*
			(0.012)			(0.019)
Sanitary facilities: Inside the household	7,151	0.70	0.019	2,924	0.75	0.008
			(0.014)			(0.021)
Phone numbers: None	7,151	0.90	0.006	2,924	0.90	-0.015
			(0.009)			(0.015)
Total accreditation score	7,146	14.34	0.057	2,923	13.73	-0.033
			(0.132)			(0.208)
Self-esteem (Rosenberg)	7,151	0.00	-0.002	2,924	0.05	-0.017
			(0.028)			(0.045)
No data on self-esteem	7,151	0.13	0.012	2,924	0.16	0.007
			(0.011)			(0.018)

Note: The information corresponds to the baseline. The complete sample corresponds to youths enrolled in a course and section with excess of demand for their first course selection. The experimental treatment group corresponds to suitable youths who obtained a random vacancy, 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.

Table 4: Intention to treat effects on outcomes from follow-up survey

	All	**Women**	**Men**	**14-18**	**19-26**	**Lima**	**OUA**
		Women	**Men**	**14-18**	**19-26**	**Lima**	**OUA**
A. Employment indicators							
Employed	0.016	0.015	0.033	0.040	0.013	-0.010	0.030
	(0.025)	(0.034)	(0.038)	(0.042)	(0.036)	(0.033)	(0.037)
	0.641	*0.552*	*0.773*	*0.624*	*0.655*	*0.657*	*0.626*
Wage employment	0.036	0.033	0.036	0.067	0.026	0.007	0.053
	(0.026)	(0.033)	(0.048)	(0.045)	(0.039)	(0.037)	(0.037)
	0.500	*0.433*	*0.600*	*0.472*	*0.524*	*0.534*	*0.468*
Hours per week	1.084	0.886	1.998	2.614	0.880	0.354	1.254
	(1.358)	(1.814)	(2.396)	(2.203)	(2.108)	(1.973)	(1.931)
	30.073	*24.960*	*37.650*	*27.804*	*31.990*	*31.444*	*28.752*
B. Formality indicators							
Health insurance	0.038**	0.030	0.069*	0.068**	0.023	0.046	0.025
	(0.018)	(0.020)	(0.038)	(0.028)	(0.028)	(0.030)	(0.022)
	0.156	*0.141*	*0.177*	*0.140*	*0.169*	*0.213*	*0.101*
Contract	0.028	0.002	0.076*	0.042	0.019	0.024	0.020
	(0.020)	(0.023)	(0.041)	(0.031)	(0.030)	(0.036)	(0.022)
	0.178	*0.163*	*0.200*	*0.156*	*0.196*	*0.265*	*0.094*
Retirement pension	0.033*	0.029	0.054	0.045	0.028	0.046	0.012
	(0.018)	(0.021)	(0.039)	(0.028)	(0.027)	(0.029)	(0.022)
	0.150	*0.129*	*0.182*	*0.140*	*0.159*	*0.205*	*0.097*
C. Income							
Monthly, logarithm	0.134	0.128	0.214	0.285	0.132	-0.130	0.301
	(0.158)	(0.204)	(0.274)	(0.279)	(0.227)	(0.215)	(0.229)
	4,054	*3,372*	*5,064*	*3,935*	*4,154*	*4,268*	*3,847*
Per hour, logarithm	0.031	0,025	0.059	0.044	0.043	-0.061	0.094
	(0.043)	(0.050)	(0.085)	(0.079)	(0.057)	(0.056)	(0.065)
	0,946	*0,769*	*1,209*	*0,945*	*0,947*	*1,027*	*0,868*
D. Income, only earned income							
Monthly, logarithm	0.031	0.079	0.000	-0.010	0.062	-0.025	0.073
	(0.038)	(0.065)	(0.052)	(0.070)	(0.051)	(0.056)	(0.054)
	6,414	*6,244*	*6,591*	*6,385*	*6,436*	*6,572*	*6,252*
Per hour, logarithm	-0.017	0.004	-0.025	-0.044	0.013	-0.102	0.054
	(0.042)	(0.067)	(0.071)	(0.074)	(0.057)	(0.055)	(0.065)
	1,208	*1,101*	*1,320*	*1,250*	*1,174*	*1,339*	*1,076*
E. Socio-emotional indicators							
Self-esteem (Rosenberg)	0.038	0.037	0.028	0.015	-0.002	0.042	0.011
	(0.051)	(0.068)	(0.094)	(0.091)	(0.070)	(0.073)	(0.069)
	-0,002	*-0,002*	*-0,002*	*0,042*	*-0,039*	*-0,074*	*0,068*
Perseverance (Duckworth)	0.032	-0.023	0.042	-0.048	0.071	0.066	-0.034
	(0.053)	(0.067)	(0.101)	(0.085)	(0.075)	(0.089)	(0.063)
	-0,005	*0,052*	*-0,088*	*-0,002*	*-0,007*	*0,039*	*-0,047*
Ambition (Duckworth)	0.009	-0.003	0.043	-0.004	0.002	-0.043	0.056
	(0.049)	(0.068)	(0.090)	(0.078)	(0.074)	(0.074)	(0.069)
	-0,001	*0,025*	*-0,037*	*-0,039*	*0,032*	*0,164*	*-0,160*

Notes: The sample corresponds to individuals who completed the follow-up survey. Outcome variables are extracted from the information obtained in this survey. All estimations include fixed effects per course. Each estimation controls by gender, age, education, household characteristics, employment trends and income. The standard errors were estimated using clusters per course.
Statistical significance: * $p < 0.1$ ** $p < 0.05$, and *** $p < 0.01$.

Table 5: Intention to treat effects on outcomes from the Electronic Payroll

	All	Sub-groups					
		Women	Men	14-18	19-26	Lima	OUA
A. Had a job registered in the Electronic Payroll							
Any month in 2011	0.013	0.009	0.018	0.027	0.013	0.039	-0.012
	(0.025)	(0.030)	(0.049)	(0.039)	(0.033)	(0.043)	(0.030)
	0.337	*0.313*	*0.373*	*0.296*	*0.372*	*0.412*	*0.264*
Any month in 2012	0.024	0.027	0.059	0.028	0.046	0.054	-0.008
	(0.022)	(0.027)	(0.045)	(0.038)	(0.034)	(0.034)	(0.029)
	0.315	*0.279*	*0.368*	*0.316*	*0.314*	*0.360*	*0.272*
Any month in 2013	0.045*	0.061**	0.051	0.026	0.088***	0.065*	0.025
	(0.023)	(0.028)	(0.042)	(0.041)	(0.029)	(0.036)	(0.030)
	0.269	*0.232*	*0.323*	*0.291*	*0.250*	*0.318*	*0.221*
Any month from 2011 to 2013	0.016	0.034	0.015	0.013	0.037	0.014	0.015
	(0.026)	(0.033)	(0.047)	(0.042)	(0.033)	(0.042)	(0.032)
	0.455	*0.409*	*0.523*	*0.453*	*0.456*	*0.551*	*0.362*
Months in 2011	0.308	0.387*	0.348	0.358	0.334	0.526	0.062
	(0.202)	(0.231)	(0.400)	(0.303)	(0.284)	(0.364)	(0.225)
	2.168	*1.969*	*2.459*	*1.806*	*2.470*	*2.704*	*1.649*
Months in 2012	0.258	0.370*	0.316	0.201	0.471	0.373	0.082
	(0.197)	(0.224)	(0.400)	(0.334)	(0.294)	(0.315)	(0.241)
	2.269	*1.978*	*2.695*	*2.263*	*2.274*	*2.730*	*1.822*
Months in 2013	0.221**	0.349***	0.201	0.239	0.355**	0.328*	0.123
	(0.111)	(0.130)	(0.200)	(0.190)	(0.150)	(0.181)	(0.133)
	1.171	*1.015*	*1.400*	*1.202*	*1.145*	*1.431*	*0.920*
Months from 2011 to 2013	0.786*	1.107**	0.865	0.798	1.161*	1.227*	0.267
	(0.435)	(0.466)	(0.879)	(0.696)	(0.615)	(0.707)	(0.533)
	5.608	*4.963*	*6.555*	*5.271*	*5.889*	*6.865*	*4.391*
B. Income higher than S/.550							
Any month in 2011	0.025	0.031	0.026	0.025	0.037	0.063	-0.015
	(0.023)	(0.028)	(0.047)	(0.037)	(0.033)	(0.039)	(0.029)
	0.285	*0.257*	*0.327*	*0.255*	*0.311*	*0.345*	*0.228*
Any month in 2012	0.024	0.035	0.044	0.020	0.052	0.070**	-0.020
	(0.023)	(0.027)	(0.044)	(0.039)	(0.033)	(0.035)	(0.029)
	0.293	*0.251*	*0.355*	*0.300*	*0.287*	*0.326*	*0.261*
Any month in 2013	0.036	0.043	0.049	0.020	0.073**	0.057	0.015
	(0.023)	(0.027)	(0.041)	(0.042)	(0.029)	(0.036)	(0.029)
	0.239	*0.204*	*0.291*	*0.259*	*0.223*	*0.292*	*0.188*
Any month from 2011 to 2013	0.017	0.035	0.013	-0.002	0.053	0.039	-0.008
	(0.024)	(0.032)	(0.045)	(0.041)	(0.033)	(0.037)	(0.033)
	0.413	*0.365*	*0.482*	*0.421*	*0.405*	*0.487*	*0.341*
Months in 2011	0.319*	0.420**	0.260	0.284	0.369	0.592*	0.024
	(0.183)	(0.207)	(0.379)	(0.274)	(0.263)	(0.340)	(0.194)
	1.766	*1.573*	*2.050*	*1.457*	*2.024*	*2.213*	*1.333*
Months in 2012	0.191	0.265	0.224	0.168	0.353	0.352	-0.015
	(0.186)	(0.219)	(0.380)	(0.306)	(0.281)	(0.287)	(0.235)
	2.007	*1.743*	*2.395*	*1.919*	*2.081*	*2.423*	*1.605*
Months in 2013	0.131	0.219*	0.101	0.157	0.243	0.256	0.023
	(0.109)	(0.127)	(0.195)	(0.185)	(0.149)	(0.177)	(0.130)
	1.057	*0.907*	*1.277*	*1.065*	*1.051*	*1.307*	*0.815*
Months from 2011 to 2013	0.641	0.904**	0.585	0.608	0.964	1.200*	0.031
	(0.406)	(0.439)	(0.833)	(0.634)	(0.581)	(0.654)	(0.499)
	4.831	*4.223*	*5.723*	*4.441*	*5.155*	*5.944*	*3.754*

Notes: The sample corresponds to individuals who completed the follow-up survey, and who had a NID. Outcome indicators are generated with the information obtained from the Electronic Payroll.. All estimations include fixed effects per course. Each estimation controls by gender, age, education, household characteristics, employment trends and income. Standard errors have been estimated using clusters per course. The standard error was calculated using *clusters* for each course. The statistical significance is: * p<0.l, ** p<0.05, and *** p<0.01.

APPENDIX

Estimation of the Local Average Treatment Effect (LATE)

The main part of this document estimates the intention-to-treat effect (ITT), adjusting the following regression:

$$Y_i = \alpha + \beta Z_i + \sum_j \gamma_j C_{ij} + \sum_k \delta_j X_{ik} + \varepsilon_i,$$

where Y_i is the outcome indicator; Z_i is the result of the random assignment and takes a value of 1 for the treatment group and a value of 0 for the control group; C_{ij} represents fixed effects for each course with excess demand; X_{ik} represents characteristics observed in the baseline, included to increase the accuracy of the estimations, and represents a term for error. The ITT effect reflects the difference in outcome indicators from the treatment and control groups, and is obtained estimating coefficient

Since there is no perfect compliance of random assignment, assignment to treatment and control groups differs from the effective allocation of youths to training courses, and in particular to completing the training (denoted by $D_i = 1$) or not completing the training $(D_i = 0)$. Under these conditions, it is possible to estimate the effect of the training (the effective allocation) through the Local Average Treatment Effect (LATE) (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. The effect of the local treatment can be written as:

$$LATE = \frac{E[Yi|Zi=1]-E[Yi|Zi=0]}{E[Di|Zi=1]-E[Di|Zi=0]}.$$

The reduced form, the numerator in the expression above, corresponds to the ITT. The first stage, the denominator in the expression above, is a regression in which the dependent variable is the effective allocation D_i and the explanatory variable is the random allocation, the instrument Z_i. The first stage regression also includes the courses fixed effects, which allows us to take into account that the probability of being assigned to the treatment or control groups differs between courses, since the number of applicants varies between different courses.

Next, we will describe the results of the estimations for the first stage, which allow us to transform the ITT effect into the LATE. For the regression of the first stage, the dependent variable is the indicator of effective participation in Projoven, in particular having completed the on-site internship stage. The key explanatory variable, which is used as instrumental variable for LATE estimations, is the outcome indicator for the random allocation to the treatment group ($Z_i = 1$) or to the control group ($Z_i = 0$) at the moment of the first course selection. This random allocation only took place in courses with an excess of demand.

Estimations for the first stage include fixed effects per course as covariables, since the random allocation to treatment and control groups was done at the course level. On the other hand, the

number of participants in each course is different, for which the probability of being assigned to the treatment or control group is specific to each course. They also include two groups of characteristics observed in the baseline as additional covariables. The first considers the sex, schooling, and age of the participant. The second adds household characteristics and the previous work history of the individual. Unlike the random allocation variable, these additional covariables do not constitute instrumental variables.

Table Al presents the results of the estimation for the first stage. This estimation was done for the complete sample and several sub-groups (according to sex, age, and city/region). For the different sub-samples and specifications, we find that the random allocation variable has an approximate estimated coefficient of 0.3, calculated in a very precise manner, with a very low standard error. This indicates that youths assigned to the treatment group have a probability 30 points higher of completing the training than youths assigned randomly to the control group. The value of this coefficient indicates that the results of the ITT represent a third of the LATE (or, in other words, the LATE is three times the ITT effect).

The table also presents the weak instrument test. This test allows us to contrast if the variable of completing the training is sufficiently correlated to the variable of random allocation to treatment and control groups that serves as instrument in the estimation of the local treatment effect using the instrumental variables method. In all cases, we find that statistics for the weak instrument test are higher than critical values calculated by Stock and Yogo (2005). Consequently, it is possible to reject the weak instrument hypothesis.

Next, we present the results from LATE estimations, using indicators from the follow-up survey and the Electronic Payroll. In general, the results follow the same patterns obtained when estimating the ITT, but the magnitudes are higher.

Table Al: First stage regression results and weak instrument tests

	Coefficient (std.err)	N	R2	Weak instrument test
All	0.312*** (0.019)	2,923	0.504	276.6
Women	0.305*** (0.024)	1,718	0.539	157.0
Men	0.327*** (0.032)	1,205	0.561	102.9
14 to 18 years old	0.308*** (0.029)	1,306	0.55	109.5
19 to 26 years old	0.308*** (0.026)	1,617	0.545	138.7
Lima	0.164*** (0.029)	1,224	0.421	32.8
Other cities	0.44*** (0.025)	1,699	0.532	316.9

Note: The table reports the results of the regression of the first stage for different samples and groups. The dependent variable is having completed the training, the instrument is the random allocation, and the regression controls by fixed effects from the course-section. The last column shows the value of the weak instrument test. Standard errors have been estimated using clusters per course. Significance: * $p<0.1$, ** $p<0.05$, and *** $p<0.01$.

Table A2: LATE on outcomes from follow-up survey

	All	Women	Men	14-18	19-26	Lima	OUA
				Sub-groups			
A. Employment indicators							
Employed	0.052	0.049	0.102	0.129	0.041	-0.058	0.067
	(0.075)	(0.103)	(0.106)	(0.125)	(0.106)	(0.189)	(0.078)
	0.641	*0.552*	*0.773*	*0.624*	*0.655*	*0.657*	*0.626*
Wage employment	0.115	0.108	0.109	0.216	0.084	0.042	0.120
	(0.079)	(0.100)	(0.131)	(0.134)	(0.117)	(0.211)	(0.079)
	0.500	*0.433*	*0.600*	*0.472*	*0.524*	*0.534*	*0.468*
Hours per week	3.479	2.901	6.115	8.477	2.860	2.158	2.852
	(4.168)	(5.543)	(6.647)	(6.618)	(6.305)	(11.368)	(4.169)
	30.073	*24.960*	*37.650*	*27.804*	*31.990*	*31.444*	*28.752*
B. Formality indicators							
Health insurance	0.123**	0.099	0.212**	0.220**	0.074	*0.219*	0.057
	(0.057)	(0.062)	(0.107)	(0.088)	(0.084)	(0.180)	(0.048)
	0.156	*0.141*	*0.177*	*0.140*	*0.169*	*0.213*	*0.101*
Contract	0.089	0.006	0.233	0.136	0.062	0.149	0.046
	(0.062)	(0.071)	(0.118)	(0.090)	(0.091)	(0.210)	(0.047)
	0.178	*0.163*	*0.200*	*0.156*	*0.196*	*0.265*	*0.094*
Retirement pension	0.105*	0.096	0.166	0.147*	0.090	0.280	0.028
	(0.056)	(0.063)	(0.108)	(0.085)	(0.081)	(0.171)	(0.047)
	0.150	*0.129*	*0.182*	*0.140*	*0.159*	*0.205*	*0.097*
C. Income							
Monthly, logarithm	0.430	0.419	0.654	0.924	0.429	-0.794	0.686
	(0.481)	(0.620)	(0.758)	(0.830)	(0.676)	(1.232)	(0.491)
	4.054	*3.372*	*5.064*	*3.935*	*4.154*	*4.268*	*3.847*
Per hour, logarithm	0.101	0.081	0.180	0.142	0.138	-0.372	0.214
	(0.132)	(0.152)	(0.234)	(0.234)	(0.167)	(0.317)	(0.138)
	0.946	*0.769*	*1.209*	*0.945*	*0.947*	*1.027*	*0.868*
D. Income, only earned income							
Monthly, logarithm	0.094	0.247	0.000	-0.030	0.192	-0.134	0.161
	(0.108)	(0.182)	(0.136)	(0.181)	(0.139)	(0.276)	(0.110)
	6.414	*6.244*	*6.591*	*6.385*	*6.436*	*6.572*	*6.252*
Per hour, logarithm	-0.051	0.013	-0.074	-0.136	0.041	-0.552**	0.118
	(0.121)	(0.186)	(0.186)	(0.193)	(0.152)	(0.277)	(0.131)
	1.208	*1.101*	*1.320*	*1.250*	*1.174*	*1.339*	*1.076*
E. Socio-emotional indicators							
Self-esteem (Rosenberg)	0.120	0.122	0.087	0.047	-0.005	0.255	0.026
	(0.156)	(0.210)	(0.259)	(0.266)	(0.208)	(0.420)	(0.148)
	-0.002	*-0.002*	*-0.002*	*0.042*	*-0.039*	*-0.074*	*0.068*
Perseverance (Duckworth)	0.102	-0.075	0.130	-0.156	0.231	0.403	-0.077
	(0.166)	(0.206)	(0.283)	(0.251)	(0.224)	(0.539)	(0.138)
	-0.005	*0.052*	*-0.088*	*-0.002*	*-0.007*	*0.039*	*-0.047*
Ambition (Duckworth)	0.029	-0.011	0.134	-0.012	0.008	-0.261	0.129
	(0.151)	(0.209)	(0.248)	(0.228)	(0.223)	(0.435)	(0.154)
	-0.001	*0.025*	*-0.037*	*-0.039*	*0.032*	*0.164*	*-0.160*

Notes: The sample corresponds to individuals who completed the follow-up survey. Outcome variables are extracted from the information obtained in this survey. All estimations include fixed effects per course. Each estimation controls by gender, age, education, household characteristics, employment trends and income. The standard errors were estimated using clusters per course. Statistical significance: * $p<0.1$ ** $p<0.05$, and *** $p<0.01$.

Table A3: LATE on outcomes from Electronic Payroll

		Sub-groups					
	All	Women	Men	14-18	19-26	Lima	OUA
A. Had a job registered in the Electronic Payroll							
Any month in 2011	0.041	0.030	0.056	0.089	0.042	0.236	-0.027
	(0.077)	(0.090)	(0.132)	(0.116)	(0.100)	(0.250)	(0.066)
	0.337	0.313	0.373	0.296	0.372	0.412	0.264
Any month in 2012	0.077	0.090	0.181	0.091	0.150	0.326	-0.017
	(0.069)	(0.085)	(0.123)	(0.112)	(0.102)	(0.199)	(0.063)
	0.315	0.279	0.368	0.316	0.314	0.360	0.272
Any month in 2013	0.143**	0.201**	0.157	0.085	0.286***	0.396*	0.056
	(0.071)	(0.089)	(0.113)	(0.122)	(0.094)	(0.213)	(0.064)
	0.269	0.232	0.323	0.291	0.250	0.318	0.221
Any month from 2011 to 2013	0.050	0.113	0.046	0.044	0.119	0.088	0.034
	(0.078)	(0.100)	(0.129)	(0.124)	(0.100)	(0.237)	(0.070)
	0.455	0.409	0.523	0.453	0.456	0.551	0.362
Months in 2011	0.989	1.271*	1.063	1.176	1.085	3.200	0.141
	(0.619)	(0.712)	(1.076)	(0.897)	(0.848)	(2.168)	(0.484)
	2.168	1.969	2.459	1.806	2.470	2.704	1.649
Months in 2012	0.828	1.216*	0.965	0.661	1.532*	2.266	0.188
	(0.609)	(0.708)	(1.091)	(0.985)	(0.901)	(1.816)	(0.521)
	2.269	1.978	2.695	2.263	2.274	2.730	1.822
Months in 2013	0.712**	1.146***	0.614	0.785	1.154**	1.993*	0.280
	(0.341)	(0.420)	(0.537)	(0.553)	(0.467)	(1.043)	(0.287)
	1.171	1.015	1.400	1.202	1.145	1.431	0.920
Months from 2011 to 2013	2.529*	3.633**	2.642	2.622	3.772**	7.460*	0.609
	(1.338)	(1.485)	(2.372)	(2.040)	(1.880)	(4.169)	(1.148)
	5.608	4.963	6.555	5.271	5.889	6.865	4.391
B. Income higher than S/.550							
Any month in 2011	0.079	0.101	0.079	0.083	0.120	0.380	-0.033
	(0.072)	(0.087)	(0.126)	(0.110)	(0.099)	(0.244)	(0.063)
	0.285	0.257	0.327	0.255	0.311	0.345	0.228
Any month in 2012	0.079	0.114	0.133	0.067	0.168*	0.428**	-0.045
	(0.070)	(0.086)	(0.121)	(0.116)	(0.101)	(0.212)	(0.063)
	0.293	0.251	0.355	0.300	0.287	0.326	0.261
Any month in 2013	0.114*	0.142*	0.150	0.065	0.239***	0.345*	0.033
	(0.069)	(0.085)	(0.110)	(0.124)	(0.090)	(0.208)	(0.062)
	0.239	0.204	0.291	0.259	0.223	0.292	0.188
Any month from 2011 to 2013	0.053	0.114	0.040	-0.008	0.174*	0.239	-0.018
	(0.074)	(0.099)	(0.123)	(0.123)	(0.100)	(0.210)	(0.072)
	0.413	0.365	0.482	0.421	0.405	0.487	0.341
Months in 2011	1.025*	1.377**	0.795	0.933	1.199	3.601*	0.054
	(0.564)	(0.648)	(1.026)	(0.814)	(0.790)	(2.063)	(0.418)
	1.766	1.573	2.050	1.457	2.024	2.213	1.333
Months in 2012	0.615	0.870	0.686	0.551	1.147	2.137	-0.034
	(0.575)	(0.687)	(1.040)	(0.909)	(0.858)	(1.687)	(0.506)
	2.007	1.743	2.395	1.919	2.081	2.423	1.605
Months in 2013	0.421	0.718*	0.308	0.515	0.789*	1.559	0.051
	(0.334)	(0.398)	(0.529)	(0.543)	(0.455)	(1.006)	(0.281)
	1.057	0.907	1.277	1.065	1.051	1.307	0.815
Months from 2011 to 2013	2.060	2.965**	1.789	1.999	3.135*	7.297*	0.071
	(1.255)	(1.397)	(2.264)	(1.872)	(1.778)	(3.924)	(1.077)
	4.831	4.223	5.723	4.441	5.155	5.944	3.754

Notes: The sample corresponds to individuals who completed the follow-up survey, and who had a NID. Outcome indicators are generated with the information obtained from the Electronic Payroll. All estimations include fixed effects per course. Each estimation controls by gender, age, education, household characteristics, employment trends and income. Standard errors have been estimated using clusters per course. The standard error was calculated using *clusters* for each course. The statistical significance is: * p<0.1, ** p<0.05, and *** p<0.01.

Table A4: Equivalence between attritors in the treatment and control groups

Dependent Variable: Being Attritor	Observable Characteristic	Treated	Interaction [Treated/Obs. Characteristic]	N
Gender: Male	0.0063	0.0276	-0.0383	7151
	(0.0236)	(0.0174)	(0.0253)	
Age	-0.0058	-0.06	0.0036	7151
	(0.0049)	(0.1054)	(0.0055)	
Years of schooling	-0.0017	-0.04	0.0047	7151
	(0.0083)	(0.0993)	(0.0094)	
Took courses at an institute or university	-0.0152	0.0094	0.0269	7146
	(0.0758)	(0.0129)	(0.0821)	
Took a technical/trade course	0.0425	0.0172	-0.0563	7146
	(0.0342)	(0.0136)	(0.0376)	
Gender of head of the household: Male	0.0423*	0.0275	-0.0276	7146
	(0.0235)	(0.0210)	(0.0262)	
Schooling of head of the household:				
Complete higher education	0.0439*	0.0148	-0.0148	7151
	(0.0239)	(0.0156)	(0.0265)	
Incomplete higher education	0.0084	0.0088	0.0048	7151
	(0.0285)	(0.0142)	(0.0318)	
Complete secondary education	-0.0405*	0.0093	0.0011	7151
	(0.0232)	(0.0162)	(0.0257)	
Incomplete secondary education	-0.0021	0.0086	0.0342	7151
	(0.0595)	(0.0130)	(0.0663)	
Elementary/ illiterate	-0.0311	0.0086	0.0266	7151
	(0.0534)	(0.0131)	(0.0597)	
Overcrowding (people per room)	0.0147**	0.0216	-0.0037	7146
	(0.0066)	(0.0256)	(0.0073)	
Housing: Proprietorship	0.135***	0.0417*	-0.0393	7146
	(0.0244)	(0.0223)	(0.0269)	
Floor material: Floor tile, cement, wood	-0.058**	-0.0105	0.0423*	7151
	(0.0229)	(0.0175)	(0.0251)	
Ceiling material: Concrete, cement, tiles	0.0524**	0.0179	-0.0172	7151
	(0.0232)	(0.0173)	(0.0252)	
Wall material: Brick	0.0205	0.0034	0.012	7151
	(0.0230)	(0.0179)	(0.0251)	
Water: Inside the household	0.08***	0.0376	-0.0339	7151
	(0.0290)	(0.0286)	(0.0318)	
Sanitary facilities: Inside the household	0.0603**	-0.0033	0.0168	7151
	(0.0249)	(0.0230)	(0.0274)	
Phone numbers: None	0.016	0.0874**	-0.0857**	7151
	(0.0377)	(0.0400)	(0.0420)	
Total accreditation score	0.0035	0.0604	-0.00348339	7146
	(0.0025)	(0.0409)	(0.0027)	
Self-esteem (Rosenberg)	0.01778	0.0097	-0.0082	7151
	(0.0122)	(0.0128)	(0.0136)	
No data on self-esteem	0.02	0.0101	-0.0041	7151
	(0.0333)	(0.0137)	(0.0367)	

Notes: The information corresponds to the baseline for all youths enrolled in a course and section with excess of demand for their first course selection. The experimental treated group corresponds to suitable youths who obtained a random vacancy, the control group corresponds to those who did not obtain a random vacancy. The coefficients presented were obtained from a (separate) regression of a dummy variable for attritors on a dummy for the treatment status, an observable characteristic, and the interaction between the two. Fixed effects per course-section included. The statistical significance is: * p<0.l, ** p<0.05, and *** p<0.01. Standard errors in parenthesis.