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AN EVALUATION OF THE PERUVIAN "YOUTH LABOR TRAINING PROGRAM" - PROJOVEN

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An Evaluation of the Peruvian "Youth Labor Training Program" - PROJOVEN

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* This study was prepared by Juan José Díaz and Miguel Jaramillo from Grupo de Análisis para el Desarrollo (GRADE).

This working paper is one of several background reports to OVE's Labour Training Thematic Evaluation carried out during the 2005-2006 Ex-Post Evaluation Cycle.

The team of this Thematic Study was comprised of Pablo Ibarraran, David Rosas-Shady, and coordinated by Inder J. Ruprah. The authors thank the staff of PROJoven for providing and helping us handling the data for this study and for answering many inquiries about the program. Kristian Lopez and Martin Sotelo provided valuable assistance in the elaboration of a previous evaluation report. The findings and interpretations of the authors do not necessarily represent the views of the Inter-American Development Bank and/or GRADE. The usual disclaimer applies. Correspondence to: Pablo Ibarraran, e-mail: pibarraran@iadb.org, Office of Evaluation and Oversight, Inter-American Development Bank, Stop B-750, 1300 New York Avenue, NW, Washington, D.C. 20577.

TABLE OF CONTENTS

ANNEXES

APPENDIX

INTRODUCTION

This paper summarizes the findings of an impact evaluation of the Mexican training programs PROBECAT_SICAT for the period 1999-2004. It is a study commissioned by the Office of Evaluation and Oversight of the Inter-American Development Bank in accordance to the Bank's policy of ex-post evaluation of operations.

The "Youth Labor Training Program" (Programa de Capacitacion Laboral Juvenil - PROJoven) is an ongoing job-training program created in 1996 by the Ministry of Labor (Ministerio de Trabajo y Promocion del Empleo) in response to the precarious conditions of youth in the Peruvian labor market. The goals of the program are to improve employment opportunities of youth in poverty and to promote competition and higher quality of services in the vocational training system. By design, PROJoven finances vocational and training courses for its beneficiaries, but the services are provided by private and public training institutions (Entidades de Capacitación - ECAP), which compete in public calls to get funding for their course offerings. The type and content of courses (technical phase) provided by these ECAPs are driven by demand, since a requirement of the program is the existence of a written commitment by private firms to provide paid internships, so beneficiaries can acquire on-the-job experience (practical phase) for a period not shorter than three months. Part of the payment received by ECAP is contingent upon documenting that the trainee is performing job training at a private firm. This is the program's strongest instrument to ensure the pertinence of course offerings.

In this paper, we present the main results of an evaluation of PROJoven impacts on program beneficiaries. We provide an institutional analysis of the program, documenting the origins and rationale of the intervention, we explore how and why PROJoven was designed, and the political and economic environment at the time of its inception and afterwards. The main results of this analysis indicate that PROJoven is a well designed program, using current available knowledge and improving upon it. It also has been remarkably stable in its operation and technically managed. This is associated with features that made it unattractive to political capture, such as its small size, location in the poorest ministry, little visibility, and the difficulties of selling vocational training politically as compared with, for instance, infrastructure investment or plain temporary employment programs. Recent instability, particularly during 2005, is associated to political capture. The proximity of elections makes it difficult to predict whether the Program will go back to a more technical management.

An important feature of PROJoven is that since its inception the program includes a quasi-experimental evaluation component designed to measure impacts on its beneficiaries. The program gathers data on program beneficiaries (treatment group) and eligible non-participants youths (comparison group) from four surveys: a baseline conducted at the beginning of the job training, and three follow-ups surveys carried out six, twelve, and eighteen months after. These evaluation data have been used in several studies to evaluate PROJoven impacts on a broad set of outcomes, such as employment and unemployment status, labor status transitions, weekly working hours, labor earnings and so forth, by applying different econometric methods to estimate the treatment effects of PROJoven. However, the results of these evaluation studies are not strictly comparable across the different rounds.

We provide a comprehensive re-examination of PROJoven impacts on beneficiaries in terms of employment status (employed, paid employment and formal jobs), earnings (monthly and hourly) and weekly hours of work. Our results suggest that there are positive and statistically significant effects for all the public calls we analyze in terms of paid jobs and formal employment probabilities after participation in PROJoven, as well as in terms of monthly earnings. Patterns of program impacts for hourly earnings and hours of work are less clear, but in general we find positive effects, especially for female youths and 16-20 year olds beneficiaries. Additionally, we also find that the program impacts on real monthly earnings decreased from the first to fourth public calls (1996-1998) and then rebound and grew from the sixth to eighth (1999-2000) calls, thus presenting a U-shape. Based on these results, we believe the program has been relevant for the target population, in particular by providing quality training to individuals that otherwise would have not acquired labor training or have acquired lower quality training.

Another important set of results of our analysis relates to the state of the PROJoven's evaluation data. We find that the Program's evaluation data has not been well kept and at this point access to raw data is difficult, if not impossible, to achieve. Further, important portions of information are missing. In particular, in the first and second public calls employment histories were collected during baseline data field work, but never put in magnetic format. A similar problem occurs in the third follow-up survey of the eighth public call. These data were gathered in the field work, but delays on the processing of the questionnaires into magnetic format led program officers to store them for later use but were ultimately lost during office relocation. Finally, a shortcoming in PROJoven's evaluation data is that they do not contain information on the private companies or firms where trainees get on-the-job training after the classroom phase at the ECAP. It would be advisable to begin collecting this kind of data.

I. INSTITUTIONAL ANALYSIS OF PROJOVEN

A. The Economic Context

The economic context in which the Program was conceived was one of a vigorous economic recovery after the implementation of an aggressive stabilization – structural reform agenda. Indeed, Peru in the early nineties was one of the countries that moved faster in the direction of opening up the economy, eliminating price controls (literally, overnight), and restricting the role of the State in the economy. At the same time, fiscal and monetary policy reforms were implemented in order to restore basic macroeconomic equilibrium and reduce inflation.[1] After a period of adjustment-induced recession, in 1993 the economy was growing and in the following two years it was among the fastest growing economies in the region. The results of the 1995 election were supposed to secure the continuation of reform, though history did not quite turn out this way. In any case, thanks to the brisk recovery and an effective tax reform by 1995 the country's fiscal position had improved dramatically and increasing resources were being allocated to the social sector. [2]

Employment growth followed growth in output, though not equitably for different social or demographic groups. Specifically, both unemployment and underemployment rates for youth more than doubled those for adult workers. Thus, this was one group that seemed to be in need of extra help in order to take advantage of the new economic environment. In addition, individuals between 15 and 24 years old were a sizable part of total population, slightly above 30 percent. Its participation in the labor force was also large, accounting for more than one-fourth of it.[3] Figure 1 displays the evolution of real GDP, employment population-ratio, the percentage of formal employment, the GDP growth rate and the unemployment rate. Figure 2 displays unemployment rates and real earnings by gender and age groups.

The evolution of real earnings during the nineties responded mainly to market forces, while institutional wage setting mechanisms lost importance. The minimum wage was fixed in nominal terms between 1991 and 1995 and its real

[1] A detailed account of policies during this period can be found in Jaramillo and Saavedra (2005).

[2] Note that it is also the case that Chile Joven was implemented in a period where the economy was growing at high rates. Indeed, as expressed by one of the professionals involved in the design of the Program, this was a pre-condition for the Program to work, because if there is no demand for labor training may only lead to frustration among trainees. This was also one lesson from the Chilean experience (Marín 2003).

[3] For detailed descriptions of the situation of youth in the labor and education markets see Saavedra and Chacaltana, 2001; Arróspide and Egger, 2000.

value declined markedly, collective wage bargaining was eliminated at the sector level, unions lost power while the percentage of unionized workers fell dramatically, and wage indexation mechanisms for private contracts were abolished. This represented a huge change in the performance of the Peruvian labor market given that minimum wages and collective wage bargaining had had an important role on wage determination during the period prior to the hyperinflation of the late eighties. Real earnings were very flexible during the adjustment period after the implementation of the structural reform and stabilization programs of the nineties. It seems that the observed wage flexibility has been one of the most important mechanisms of adjustment in the Peruvian labor market.

Real monthly earnings fell sharply between 1987 and 1989, because of the recession and hyperinflation of the late eighties. This drop was exacerbated in 1990 by the stabilization program implemented at the beginning of Fujimori's government aimed at stopping the hyperinflation process. The fall in real earnings coupled with the declining trend of LFP between 1986 and 1992 appear to explain why the unemployment rate did not explode as a consequence of the dramatic drop on the GDP and the labor demand. The declining trend of real earnings had a turnaround after 1990. Between 1992 and 1997 real monthly earnings grew at a 3.3 percent per year, which accompanied the rise in GDP and labor demand, the "winners" during this period were women (+6.1 percent) and high skill workers (+3.9 percent). Notice that despite the increase of labor supply, real earnings grew during the nineties, so this higher labor supply might have been out weighted by the expansion of labor demand.

B. The Training Market

In the decade before the implementation of PROJoven, the Peruvian training market had expanded significantly, particularly through the growth of the private sector, though the public sector still has more than a third of training institutions. The sector was, and still is, essentially unregulated and efforts to introduce quality standards through certification have moved extremely slowly. In effect, IDB's program with the Ministry of Education for the reform of technical education started in mid-nineties and contemplated a component focused on certification. However, so far no certification system is at work. The result is that the training supply is quite heterogeneous as far as quality and generally lacks a connection with the productive sector. In addition, it is unevenly distributed geographically.

Using data from mid-nineties Saavedra and Chacaltana (2001) have documented that although youth from poor households have access to training, they use it with less frequency than youth from non-poor households. In addition, they tend to attend institutions of lower quality and to concentrate on public sector entities. They also found that public entities tend to provide training services of lower quality than private ones. Finally, private entities oriented to the poorer segments of the population tend to have less adequate infrastructure.

A study of the Ministry of Labor, based on a nationwide survey applied to 1,112 graduates from 123 urban Technical Institutes (Institutos Superiores Tecnologicos – IST) in 1996, found that there exists a high degree of heterogeneity among these institutions. The study provides evidence that substantial differences in terms of quality characterize the Peruvian post-secondary educational system. Moreover, the study finds that this heterogeneity has large effects in terms of earnings for IST graduates, and that these effects vary between ISTs from Lima (the capital city) and those from other Peruvian cities. In particular, graduates from a high quality IST earns on average 46 percent more than graduates from low quality ISTs. In other cities this difference in earnings is about 17 percent. On the other hand, studies conducted by Valdivia (1994 and 1997), find a positive correlation between socioeconomic status of IST students and the quality of the institutions. That is, poorer youngsters tend to acquire post-secondary education and training in lower quality institutions than youngsters with higher socioeconomic status. In addition, several studies show that there is a mismatch between the education and training an individual gets and real requirements in terms of labor demand (Arregui 1993, Verdera 1995, Rodriguez 1996, Díaz 1996, Burga and Moreno 1999, Saavedra and Chacaltana 2001, Chacaltana and Sulmont 2004, Herrera 2005). Although a systemic reform was, and still is, in order, PROJoven intended to contribute to introduce more dynamic in the training market by promoting a closer connection between training entities and the productive sector.

C. The "Youth Labor Training Program" - PROJoven

The Peruvian "Youth Labor Training Program" PROJoven is an ongoing training program that targets youth in poverty, created in 1996 by the Ministry of Labor as a response to the precarious conditions of these individuals in the labor market. The goals of the program are to improve employment opportunities of young individuals in poverty and to promote competition and higher quality of services in the vocational training system.

PROJoven provides funding for basic or semi-skilled training in particular occupations. The vocational training has two main components or phases. The first is a learning phase where training courses are directly provided by training centers (ECAPs), beneficiaries attend their training courses for three months and the costs of courses is covered by PROJoven. The second is an internship phase at private firms where trainees acquire on-the-job experience; the internship has a length of three months during which the trainee receives a market wage paid by the internship firm. After these three months the firm may or may not hire the trainee on a more permanent basis.

PROJoven beneficiaries are 16-24 year olds, have low levels of formal education, and none or minimum labor market experience, and are currently underemployed, unemployed or out-of-the labor force. These youngsters primarily come from poor families, targeting errors have been documented to be small (Arrospide 2000). The selection process of PROJoven's beneficiaries takes place at the program's headquarters in Lima and at its decentralized regional offices. The program is voluntary and operates on a first-come first-served basis. Between 1996 and 2003, PROJoven has provided vocational training to approximately 42,000 youngsters in ten major cities across the country (Lima, Callao, Arequipa, Trujillo, Chiclayo, Cusco, Piura, Huancayo, Chimbote and Iquitos).

ECAPs are pre-selected by PROJoven on the basis of past training experience, administrative capacity and the adequacy of the courses provided. These ECAPs should also provide their PROJoven trainees with paid internships at private firms. ECAPs that comply with all PROJoven requirements are included in the Registry of Training Centers (RECAP), and only centers in the RECAP are allowed to participate in PROJoven's public calls. Since 1996, a total of 542 ECAPs have participated at least once in the program, providing more than 2,160 vocational courses (see Table 1).

1. Origins and rationale

The idea of a training program focused on socially disadvantaged youth was first introduced in Peru at the beginning of ex-president Fujimori's second term, in the second half of 1995. As part of its cooperation program with the country, then director of ILO's Multidisciplinary Technical Team in the Regional Office for the Andean countries, Norberto García, came with the idea to the Labor Minister, Sandro Fuentes, and vicepresident, Ricardo Marquez. By then, Chile Joven had been in operation for a few years and Argentina had also launched a similar initiative. The idea was well received and, through ILO and UNPD's financial support, consultants were hired to work on the diagnostic studies associated to the design of the Program. The Inter American Development Bank (IDB), that had financed the Chilean program, soon joined the effort and through a PPF funded a significant part of the pre-investment effort. Consultants that had worked in the design as well as in the execution stage of the Chilean and

Argentinean programs participated alongside with local consultants in the design of PROJoven. The basic studies and design stage took about a year and in the second semester of 1996 a pilot program started to be implemented.

2. Program Design

The original design contemplated a much larger program than it has actually so far been achieved. In effect, initially it was planned that it should benefit directly 160,000 youngsters between 16 and 25 years old in five years of operation. Two reasons are associated to this re-scaling. First, after the pilot experiences the Program was supposed to be financed through an IADB loan that was ready for approval in 1997, when the Peruvian government decided not to proceed with the operation. Second, the first calls suggested that there were supply side constraints for such a fast growth in the Program.

Though the design of the Program took from the experiences of Chile and Argentina, the idea was also to learn from the mistakes of those experiences. As in these countries, the idea was not to provide participants with full occupational qualifications, but instead basic training (semi-qualification) followed by a short period (3 months) of practical training in the firm.[4] However, greater emphasis has been placed on the demand-driven feature of the Program as well as on the pertinence of the training offered. Indeed the program is not about providing training, but about providing employability. The goal is insertion in the labor market. Several mechanisms are in place in order to make this goal feasible.

One such mechanism is that the program does not finance the practical experience in the firm. This is an aspect in which the Peruvian PROJoven innovated *vis-á-vis* its predecessors. Instead, in order to ensure that practical training in the firm occurs, the Program requires training entities to get letters of intent from private sector firms to provide internships/practical training to beneficiaries of the Program. Although it has happened that some firms did not honor their letters of intent, generally a firm will not commit to taking in a trainee to whom it will have to pay no less than the minimum wage were he not trained in an occupation that the firm demands. In addition, a significant part of the payments to the training entity is contingent upon students completing their practical training phase.

[4] It should be noted that Chile Joven incorporates four different sub-programs. The training and labor experience in firms is the largest one, accounting for about 80% of beneficiaries in the first eight calls. This is the one most comparable to ProJoven. In addition, it has a sub-program focused on independent workers, another based on the German dual system of training and work in the firm, and a third one focused on youth at risk.

Another feature in which PROJoven has departed from prior experiences is the targeting strategy. In effect, while Chile Joven relies solely on self-targeting, Peru's PROJoven uses a mix of self-targeting with individual assessments through objective indicators to evaluate whether the prospective beneficiary fulfills the basic condition of coming from a poor household. For this purpose a standard socio-economic fiche is collected for each prospective participant. Through an algorithm the Program assigns a score to each individual, which is higher the poorer the individual. Generally, only individuals above a threshold score qualify to participate. For those close to this threshold value, additional criteria are used to decide whether they are accepted in the Program.[5] Thus, though any youth can apply to participate in the Program, only those that meet the minimum criteria are accepted.

Results indicate that the targeting strategy has been quite effective. Further, it has tended to improve as experience was gained and mechanisms adjusted. Thus, while in the first call 14 percent of participants were non poor, by the fifth call the figure had dropped to 9.8 percent. These numbers compare favorably *vis-á-vis* those for Chile Joven (Marín, 2003).

Program design has undergone a few adjustments over time, but the basic design has been kept. The targeting instrument (socio economic fiche) was revised and changes implemented so as to reduce leakages. Also, the procedures to evaluate course offerings have undergone changes and the requisites for training entities to enter PROJoven's RECAP have been adjusted over time. The basic structure as well the essential mechanisms of the Program have remained in place over time, however.

3. PROJoven in operation

PROJoven is run by a Coordinating Unit (CU), within the Labor Ministry, enjoying financial and administrative autonomy. Consultants with different and complementary qualifications work in this Unit.[6] Its main functions are planning activities, managing their execution, and monitoring and evaluating the performance of the Program. Work is divided in areas: Register of ECAPs, Planning and Technical Evaluation, Supervision, Targeting, Communications, Legal Advice, Administration, and Statistics and Informatics. The CU is small in size and rather flat hierarchically. The type of work dynamic allowed for the emergence of an organizational culture with a strong commitment to the objectives of the Program. Budgeting, procurement, and contracting procedures are well-established for a timely execution of the Program. The main processes

[5] More on this in the sub section on selection of beneficiaries.
[6] This section is based largely on Arróspide and Egger (2000).

of the Program: registration of ECAPs, allocation of courses, and selection of beneficiaries are intensive in micromanagement, so the project cycle is quite demanding. These processes are described below.

PROJoven is now in its tenth year of continuous operation. This is rare for a training program in Peru. It is also the Peruvian public sector program that has been more often rigorously (and positively) evaluated. Further, one even rarer feature of the program is that until recently it has had a quite stable and technical management team. Until 2004, only four individuals had occupied the position of program coordinator; which compares with the five coordinators that the program had in 2005. Further, all of them, except, for obvious reasons, the first one, had previously worked in different positions within the Program. Furthermore, turnover among the technical team has been remarkably low for Peruvian public administration standards. Most of the professionals that started in the program as trainees have either continued working with the Program or gone on to other technical positions in the public administration. Others did go on to pursue graduate studies and some of them came back to work in the Program.[7] PROJoven has thus contributed to the formation of human capital for public management. It is thus of interest to explore what conditions made this possible.

This question is associated with that of what makes public programs subject to political capture and which conditions allow for a technical, independent management. Several factors may account for making the program unattractive for political capture. One such factor is its location in the Labor Ministry. This is the poorest ministry in the Peruvian public administration, accounting for less than 1 percent of the central government budget.[8] This allowed for little interference from the political powers. Thus, most labor ministers and vice ministers in the relevant period had a technical background. This has been the case until Minister Fernando Villarán, the first of Toledo's Labor ministers. After him the tendency has been towards more politically guided appointments, with the exception of Dr. Javier Neves. Political appointments have made program management more volatile, as illustrated by the fact that since governing party member Juan Sheput's appointments in February 2005 there have been five different program coordinators, none of them with any experience with either training programs or any other type of labor market or social policy intervention. By the end of the year, not one of the members of PROJoven's Coordinating Unit that started the year worked in the Program anymore.

[7] A case in point is that of Milagros Alvarado, who first entered the program as a *practicante* in 1996, worked in the Program for the next five years, then went on to get a Masters degree in England and came back to be coordinator of the Program.
[8] In the second half of the nineties, during PROJoven's growth, the Labor Ministry represented 0.6% of the central government budget.

A second factor is the modest size of the program. At its peak less than 6000 youth have been beneficiaries from the Program yearly. This is hardly a large political market to capture. In addition, though over time the investment made in PROJoven is considerable, the Program has never enjoyed long-term funding. Instead, it has depended on diverse sources of funding, including IDB, a swap of external debt for social investment with the German government, funds from winning the Fondoempleo yearly contest, as well the Minstry's own sources of revenue. The recent loan contract with IDB (US18 million dollars) is so far the largest amount allocated to the Program. Finally, as different from infrastructure investment, training output is not very visible and thus difficult to sell politically. This may also have shielded the program from political capture.

Clearly, recent instability is associated with political appointments. Pressure for public jobs from ruling party members were a constant throughout Toledo's administration. From this perspective, it does not seem surprising that with political appointments long-standing technical people involved in the Program have been replaced. However, in this case the cost was large because of PROJoven's demanding project cycle. Not long after the report on which this paper is based was written, the IDB stopped disbursements for the program, likely on the grounds of lacking operating capability at the program.

4. PROJoven's selection of Training Centers (ECAPs)

PROJoven selects the ECAPs that will provide training for their beneficiaries on a competitive basis. ECAPs that succeed in the accreditation process are included in Registry of Training Centers (*Registro de Entidades de Capacitación* – RECAP) and are allowed to bid for courses during the public call; no other training institution is allowed to participate in the public call. The selection process of ECAPS and training courses is depicted in Chart 1; this process precedes the selection of the eligible youngsters and program beneficiaries.

ECAPs must present a dossier following specific instructions provided by RECAP regulations. The information provided by ECAPs is evaluated according to a procedure that grants points to infrastructure, experience in training, the quality of their faculty, the degree of formality and compliance with regulations from the Ministry of Education. ECAPs are also required to provide evidence of a commitment with private companies in order to guarantee that internships for on-the-job training will be provided to PROJoven trainees. Although each public call has its own procedure and regulations, the basic goal is to select training institutions on the base of their competence to provide quality training services coupled with internships.

After the appraisal of dossiers, ECAPs are classified into three categories, accredited (*apta*), non-accredited (*no apta*), and under observation (*observada*) which represent border-line cases that require additional verification of the information provided. ECAPs that are accredited enter the RECAP. They can participate in the public call to bid training courses that will be sponsored and paid by PROJoven. During the biding process accredited ECAPs compete with each other and offer courses which are ranked according to the procedures established in the RECAP regulation for course selection, courses with scores above a predefined threshold are selected. It is possible that even after being accredited and registered in RECAP some ECAPs do not participate in the biding process (non bidders).[9] In the end, ECAPs can be classified into five groups: a) accredited ECAPs with awarded courses and providing training (*adjudicadas*), b) accredited ECAPs with awarded courses and later cancellation (*adjudicadas anuladas*), c) accredited ECAPs with not awarded courses (*no adjudicadas*), d) accredited non-biding ECAPs, and finally, e) non-accredited ECAPs.[10]

Once the courses have been awarded the selection process of beneficiaries begins. This process is made as soon as social workers of PROJoven conclude the selection of the eligible group, which we describe in the following section. It is important to mention that by procedures of the program the ECAPs must submit a copy of the entry test they will use to evaluate admission of eligible youngsters with the documentation of the courses they bid for. PROJoven evaluates whether these tests contain discriminatory filters. Nevertheless, there are no procedures that verify that the test sent is the one finally used by the ECAP at the moment eligible youngsters apply to the ECAP. In addition, the results of these tests are not sent to the program. What the ECAPs send to the program are reports of attendance, desertion, and completion rates.

Private firms that offer on-the-job training do not have a direct relationship with PROJoven. All the information that arrives at the program about them is by means of the ECAPs. ECAPs must demonstrate that on-the-job training took place and at which company in order to be fully paid by PROJoven. What the program does is to conduct routine visits to a sample of companies to verify the information provided by the ECAPs is accurate. During these visits program supervisors collect information on the number of interns in the firm, the hours and duration of the training, the type of contract youngsters are in, and some questions regarding satisfaction with the performance of the trainee.

[9] A longitudinal analysis of RECAP data confirms that this is happening more often in recent calls.

[10] The reasons for cancellation could be low or null enrollment or sanctions to the ECAP due to serious offenses.

5. PROJoven's selection of eligible and beneficiary groups

PROJoven fieldwork begins by carrying out several activities in order to provide information about the program to its target population to promote the enrollment of potential beneficiary youngsters. To this end, the program puts in place dissemination and information campaigns directed to community leaders, local authorities, and uses advertisement directed to potential beneficiaries by broadcasting PROJoven activities and goals in TV and radio, and by using printed ads in the press, and also distributing pamphlets and other printed materials in localities where poverty rates are higher.[11] Thus, there is a first stage where self-selection takes place, given that more motivated or more disadvantaged youngsters may decide to act upon this information and participate in the selection process of eligible and beneficiaries.

The beneficiary group of PROJoven emerges from a selection process of several steps; this process is depicted in Chart 2. All the youngsters that show-up to the PROJoven headquarter or decentralized offices receive a ticket to go through an accreditation interview, aimed at verifying that they are actually poor. In general, the interview takes place about a month after the ticket is received, and the process of interviewing youngsters continues until the number of eligible individuals is twice that of course vacancies. Some of the youngsters that show up and receive a ticket never return to the accreditation interview and no registry on those people is kept. Those who return to the interview respond to several socioeconomic and demographic questions in the Accreditation Form (*ficha de acreditación*) and are required to document that the information they are giving is accurate. For instance, they are required to present their identification or military card to verify identity, and utility bills from recent months to confirm address and place of residence.

Beginning at the second public call, the information collected during the accreditation interview is used to compute a poverty score.[12] The accreditation form contains the scores for each characteristic, by filling this form and adding scores PROJoven's social workers easily obtain the total poverty score. Based on the results from the algorithm, program's social workers are able identify the youngsters that belong to the target population of the program and select them as accredited (*acreditados*) or program eligible. In order to verify the results of the

[11] The localities where promotion and dissemination concentrate are selected based on poverty rates computed using information from available household surveys. However, by broadcasting on TV and radio, and by using mass press, the program reaches a large pool of youngsters in other localities as well.

[12] In the first public call, youngsters were classified in five socioeconomic strata based on the district of residence and their dwelling's characteristics.

procedure and that the information collected during the interviews is reliable, a sample of eligible youngsters is selected and their information is re-verified by means of a visit to their homes. Border-line cases, where social workers have doubts because of potential differences between the results from the algorithm and some other characteristics of the individual, are also visited to confirm whether the youngsters qualify as eligible. This way, and acknowledging the multidimensional nature of poverty, PROJoven tries to minimize potential targeting errors.

Once eligible youngsters are selected, they are asked to choose a course from the list of training courses that are going to be supplied by ECAPs. This step takes place about two weeks after the eligible group is chosen and potential inconsistencies of information are resolved. Eligible individuals receive an orientation talk, where social workers respond their questions and provide advice and counseling regarding vocational training and on the importance of choosing their courses appropriately. When youngsters feel unable to choose, either because they don't like the menu of courses or because they don't wish to continue the process at the current time, they are offered the possibility to show-up again during the next public call and are exonerated from the accreditation stage.

After the orientation talks, eligible youngsters are sent to the corresponding ECAPs according to the courses they have chosen. At this stage the selection of beneficiaries takes place. Given the size of the eligible group, ECAPs usually have to choose half of the applicants for each course. ECAPs are free to employ its own procedures, such as entry tests, personal interviews, or any combination of these. However, they are not allowed to discriminate in terms of age, gender, or place of residence for instance. In some cases, ECAPs choose on the basis of whether the youngster arrives on time to their interview or test stage. Courses begin shortly after all vacancies are filled, which only takes a few days. In the event of drop outs during the first week of courses, deserters are replaced by other beneficiaries. However, if desertion occurs after one week there is no replacement and PROJoven does not pay the ECAP the cost of that vacancy. Individuals in the eligible group who are not admitted by the ECAPs of their choice return to additional orientation talks and are directed to another ECAP. This re-orientation process can take place up to three times or until all course vacancies are filled.

The information generated and acquired by PROJoven during this process is collected in the Registry of Eligible and Beneficiaries. This Registry comprises information on the accreditation process as well as on the selection of beneficiaries. This way there is a link between each beneficiary in the Program's database and the ECAP where she received training. ECAPs send information to PROJoven on the youngsters who took their examination, those who were admitted, those who being admitted deserted and those who finish the training stage.

II. ANALYSIS OF PROJOVEN IMPACTS ON BENEFICIARIES: METHODS AND DATA

A. Methods

The parameter of interest in the impact evaluation of PROJoven is the effect of **treatment on the treated**, which answers the question "how does the treatment change the outcomes of participants relative to what they would have experienced had they not received treatment?" Using the notation of Heckman, LaLonde and Smith (1999), we denote outcomes by Y and program participation by D, and let $D=1$ for those who receive the treatment and $D=0$ for those who do not. Then, the average treatment on the treated parameter can be expressed as:[13]

$$\begin{aligned}\Delta^{TT} &= E(Y_1 - Y_0 \mid D=1) \\ &= E(Y_1 \mid D=1) - E(Y_0 \mid D=1).\end{aligned}$$

The last term in this expression is the counterfactual of interest: what the outcome for treated units would have been had they not received the treatment. The problem is that this counterfactual is not directly observable, it has to be estimated. A randomized experiment would provide a suitable estimate of this counterfactual without selection bias. In the context of PROJoven the quasi-experimental design of the evaluation data allows to construct the counterfactual of interest under the assumption of selection on observables or the conditional independence assumption. However, as for any other non-experimental method, the possibility of selection bias cannot be ruled out a priori.

1. Potential Sources of Selection Bias in PROJoven

In the context of PROJoven, there are at least two potential sources of selection bias. First, even when the comparison group is composed by eligible non-participant individuals, the very fact that these individuals did not seek treatment might induce selection bias in a non-experimental setting because of self-selection on unobserved (to the evaluator) characteristics. Applicants must attend at least twice to the Registration Centers to be recognized as eligible and to the

[13] Note that potential outcomes are not directly observed, what the researcher observes instead is the realization of the outcome Y which depends on the particular state. This can be expressed as $Y = DY_1 + (1-D)Y_0$, so we observe $Y = Y_1$ only when $D=1$ and $Y = Y_0$ only when $D=0$.

ECAPs to be selected as beneficiaries. Thus, applicants (and beneficiaries in the treatment sample) may be systematically different from their comparison group counterparts on those unobserved characteristics that make applicants more prone to seek treatment.

Second, the selection of beneficiaries depends on the ECAPs criteria which are likely based on unobserved (to the evaluator) characteristics. Given that individuals in the eligible group are homogeneous along several observable dimensions and that ECAPs are not allowed to use gender, race or background characteristics to select their trainees, it is likely that the beneficiary group is systematically different from the rejected eligible group in some other characteristics such as motivation or punctuality. In particular, ECAPs have incentives to choose the best applicants from the pool of eligible individuals because of the monetary penalties they incur when their trainees are not accepted for on-the-job training; thus, it is likely that ECAPs perform some sort of “cream skimming”.

2. Dealing with Selection Bias in the context of PROJoven

We base our re-examination of PROJoven on the longitudinal version of propensity score matching to deal with the issue of selection bias. The standard cross-sectional version of matching removes any systematic difference between treatment and comparison units when program participation depends only on observable characteristics. That is, when the identification assumption is that outcomes in the untreated state (Y_0) are independent of program participation (D) conditional on a particular set of observable characteristics.[14] Denoting by X the relevant set of observable characteristics, the identification assumption can be expressed as $Y_0 \perp D \mid X$ where the symbol $\perp$ denotes independence, and Y_0 is the observed outcome for comparison units.[15] Rosenbaum and Rubin (1983) proved that if the conditional independence assumption holds by conditioning on X, then it also holds by conditioning on the conditional probability of participation (the propensity score: $P(X) = \Pr(D = 1 \mid X)$); that is:

$$(Y_0 \perp D) \mid X \Rightarrow (Y_0 \perp D) \mid P(X).$$

[14] This is the conditional independence assumption, the ignorable treatment assignment assumption (Rosenbaum and Rubin 1983), or the selection on observables assumption (Heckman and Robb 1985).

[15] Note that this is the same assumption imposed by standard cross-sectional regression methods.

The idea behind this result is that for a given $P(X)$, treatment and comparison units will appear in the same proportion in X. Actually what we require is a weaker condition to identify the treatment parameter, that of conditional mean independence:

$$E(Y_0 \mid D=1, P(X)) = E(Y_0 \mid D=0, P(X)).$$

However, as we have discussed, it is likely that program participation in PROJoven depends on both observable and unobservable characteristics which leads to self-selection. A potential solution to the self-selection problem is to assume that the systematic and unobserved differences between treatment and comparison units in the evaluation data from PROJoven are time invariant. Conditional on the propensity score, a difference-in-difference matching procedure will remove the time invariant factor. The method was proposed in Heckman, Ichimura and Todd (1998) and Heckman, Ichimura, Smith and Todd (1998); the identifying assumption can be written as:

$$E(Y_{0t'} - Y_{0t} \mid D=0, P(X)) = E(Y_{0t'} - Y_{0t} \mid D=1, P(X)),$$

where t and t' represent respectively the time periods before and after the treatment. In words, the identification assumption is that the evolution of outcomes in the untreated state is independent of program participation conditional on observable pre-treatment characteristics.

In order to clarify ideas, suppose we estimate program impacts using the cross-sectional version of matching. Using post-treatment data we would obtain a combined estimate of the treatment on the treated parameter and the bias: $\Delta_{M,t'} = \Delta^{TT} + BIAS$. Using pre-treatment data, we should get an estimate equal to zero since no treatment was administered. However, given that systematic unobserved differences between treatment and comparison units could exist, we will obtain an estimated measure of the bias: $\Delta_{M,t} = BIAS$. As long as this $BIAS$ term is time-invariant, we can remove it using the longitudinal version of matching, thus:

$$\Delta_{DIDM} = \Delta_{M,t'} - \Delta_{M,t} = \Delta^{TT}.$$

Given the availability of panel data, the difference-in-difference matching estimator (on the propensity score) is given by:

$$\Delta_{DIDM} = \frac{1}{n_1} \sum_{i \in I_1 \cap S}^{n_1} \left[(Y_{1t'i} - Y_{1ti}) - \sum_{j \in I_0 \cap S} W(i, j)(Y_{0t'i} - Y_{0ti}) \right],$$

where Δ_{DIDM} denotes the DID matching estimator on the propensity score, n_1 denotes the number of observations in the treatment sample, Y_{1si} represent the outcome for treatment units at time s $(= t, t')$, and Y_{0si} represent the outcome for comparison units at time s. The terms I_1 and I_0 denote the set of treatment and comparison units respectively, and S represents the region of common support where the densities of the propensity score for treatment and comparison units overlap. Finally, the term $W(i, j)$ represents a weighting function that depends on the specific matching estimator.

We compute our estimates using a kernel version of propensity score matching. The kernel estimator matches treatment units to a kernel weighted average of comparison units. This procedure can be thought as a non-parametric regression of a particular outcome on a constant term. The weights are given by:

$$W(i, j) = \frac{G\left(\frac{P_j - P_i}{h_n}\right)}{\sum_{k \in I_0} G\left(\frac{P_k - P_i}{h_n}\right)},$$

where $G(\cdot)$ is a kernel function, P is the propensity score, and h_n is a bandwidth parameter.

3. Outcomes of Interest

In our re-examination of PROJoven impacts we analyze several outcomes of interest such as employment probabilities, paid employment probabilities, formal employment[16] probabilities, monthly and hourly earnings, and weekly hours of

[16] This outcome indicates whether the youth is employed under a formal contract, whether she has access to a health or an accident insurance, or whether she has access to a social security fund (Seguro Social). All earnings are computed in real terms using the Consumer Price Index of each city considered in the evaluation data, all prices were set relative to that of Lima and fixed at December 2001.

work. Employment characteristics such as formality, earnings, and hours of work are drawn using information for the main job.[17]

B. PROJoven Evaluation Data

PROJoven collects evaluation data using survey instruments specially designed with the purpose of providing relevant information for the evaluation the program's impacts. There are four evaluation surveys: a baseline survey which records pre-treatment information, and three follow-up surveys carried out 6, 12 and 18 months after program participation. These evaluation surveys gather information on a treated group sample and a comparison group sample. The treated sample is comprised by program beneficiaries participating in training courses sponsored by PROJoven. This sample is drawn from the universe of beneficiaries using a stratified random sample procedure, the stratification depending on gender, age, employment status, and district of residence. Youngsters that did not participate in the selection of program beneficiaries, but would qualify as program eligible comprise the comparison sample. In particular, once the treatment sample is selected, a sample of comparison youngsters is selected by a survey fielded in the same neighborhoods where individuals from the treatment sample reside. The idea is to obtain a one-to-one matched sample of community neighbors in the same block of residence based on characteristics that would make them eligible to participate in the program. These individuals would otherwise be selected at the first stage of selection at PROJoven Registration Centers, because the same protocols to select eligible individuals are used during the fieldwork to choose comparison individuals.

The baseline evaluation data provide rich information in terms of individual demographics and background characteristics. They also provide information on earnings and employment histories, although the retrospective period covers only the past six months prior to program enrollment. The three follow-up surveys provide information on the current labor market status (employed, unemployed, weekly working hours, and labor conditions such as working on a permanent or temporary contract, being unionized, etc.), and labor earnings. Thus, the evaluation data consist on four longitudinal observations for each individual from the treated and comparison samples.

In addition to its core questionnaire, each of these four evaluation surveys also gather retrospective information on employment histories spanning the previous six months before the time of the baseline, and the second and third follow-up

[17] All earnings are computed in real terms using the Consumer Price Index of each city considered in the evaluation data, all prices were set relative the price index in Lima and fixed at December 2001.

surveys, and the previous twelve months before the time of the first follow-up survey. These employment histories contain month-by-month information on labor status, for those employed at any given month, information on occupational category, firm size and monthly earnings is collected. These data are complete and available for the fourth, sixth and eighth public calls. For the first and second public calls, unfortunately, data on employment histories are not available on magnetic format for the period before baseline surveys, only data collected during the follow-up surveys are available in these two cases. As far as we have been informed by program personnel, even when the survey instruments applied during field-work included the retrospective section, the information was not converted to data format. The reasons explaining or justifying these omissions are unclear.[18]

In the course of this study we acquire these evaluation data directly from PROJoven. We have found that in general these data has not been kept in a systematic way, and that at this point access to original raw data is difficult, if not impossible, to achieve. For instance, during the eighth public call data from the third follow-up survey are missing. These data were gathered during the field-work, but delays on the processing of the survey responses into magnetic format led program officers to store them for later use but were ultimately lost during office relocation. As we just mentioned, data on employment histories are also missing from the baseline surveys in the first and second public calls, even when the data were collected in the field-work, they were never put into magnetic format. Finally, a shortcoming in PROJoven's evaluation data is that they do not contain information on the private companies where trainees get on-the-job training after the technical phase at the ECAP. It would be advisable to begin collecting this kind of data.

1. Advantages of PROJoven evaluation data

Given that the evaluation data is designed to serve the purposes of impact evaluation, information on treated and comparison units are recorded by the same source providing several advantages. First, comparison youngsters are selected using the same protocol applied to identify eligible youngsters, that is, using the same questions applied to program beneficiaries during the accreditation interview. Second, both treated and comparison group individuals come from the same neighborhoods. In this regard it is likely that they both face the same idiosyncratic costs as long as the distances and transportation costs to PROJoven headquarters or recruitment centers and to potential place of work are the same.

[18] Thus, it is not possible to control for employment history in the evaluation data from the first and second public calls. However, we do control for past employment when working with evaluation data from the fourth, sixth, and eighth public calls.

These costs may induce youngsters from other neighborhoods not to participate in the program, even when their other observable characteristics would make them qualify as eligible. Thus, neighborhood effects are controlled for.

Third, the same survey instruments and definitions are applied to both treated and comparison units. Thus, outcomes are measured in a consistent way, in the same units and using the same question wording. This guarantees the comparability of information across groups and over time. Finally, the survey instruments are applied to treated and comparison units at the same calendar time, minimizing potential discrepancies on the data between these groups and of timing biases, such as seasonal differences in earnings or employment.

2. Disadvantages of PROJoven evaluation data

It has to be recognized that the evaluation data also entail some disadvantages. We have already mentioned two problems that may induce selection bias in the evaluation of PROJoven. The first is that even when comparison youngsters are eligible non-participant individuals, these youngsters did not look for treatment, this difference might induce self-selection bias. The second is that, in the end, the selection of program beneficiaries depends on ECAPs which have incentives to cream skim and choose the best applicants from the pool of eligible individuals in order to avoid penalties and get the full amount of payment for their services. A third disadvantage is that the procedures to select a comparison youngster when there is none in the same neighborhood block of her treated counterpart were not always applied by fieldworkers. Given that the program has continued to expand and the sample size of each evaluation data has increased over time, it is becoming more difficult and costly to find good quality comparison units.

As we report later, even when the selection of comparison youngsters actually balance treated and comparison units in terms of some observable characteristics, there are statistically significant differences on baseline or pre-treatment monthly earnings, especially in the data from the first, sixth and eighth public calls. In particular, the data show that at the time of the baseline survey, real monthly earnings for the treated group were consistently lower than for their comparison counterparts. To some extent, these differences in earnings in the baseline surveys could be the result of the typical Ashenfelter's dip, which is the self-selection of treated units into the program because they have, precisely, lower earnings.

We believe, however, that a potential explanation is related to the time when baseline data are collected. In particular, the baseline survey is usually applied to treated youngsters during the initial weeks of training courses at the ECAPs. At the same calendar time the field-work to find comparison youngsters takes place on the neighborhoods where the trainees reside. This timing of the baseline survey may induce systematic but mechanical differences on earnings between treated and comparison units. The baseline survey collects information about the youngster characteristics' and their labor market outcomes during the month before treatment. However, given that in most cases program beneficiaries already know that they have been selected as such one month before they begin their training courses, their employment status and earnings may be observationally different just because they begin to leave or quit low quality jobs once they realize they have been admitted by the ECAPs. Thus, it is possible that differences in terms of earnings or paid employment are in part a mechanical result of the timing of the baseline survey fieldwork.

III. ANALYSIS OF PROJOVEN IMPACTS ON BENEFICIARIES: EVIDENCE

A. The Evaluation Data

Table 2a reports summary statistics for variables used in the estimation of the propensity scores. Table 2b and Figures 4 to 6 present time trends of the outcome variables of interest for the baseline and the three follow-up surveys.[19] In general, we observe that outcomes for treated and comparison units begin with similar levels in the baseline survey. After the treatment, raw data from follow-up surveys show that treated youths perform better than their comparison counterparts in many of the outcomes considered, these are however unconditional differences.

In order to interpret results reported later, it is important to keep in mind that during the first public call there were many more unpaid family workers in the treated group than in the comparison group. For this public call, the data from the baseline survey show that 35 percent of youngsters in the treatment group had a paid job, this figure was nearly 50 percent among youngsters in the comparison group. However, six months after PROJoven treatment, these figures turned to 59 and 55 percent respectively, and the reverse in the ordering stay twelve and eighteen months after treatment. This initial difference on paid jobs rates is not present in the other public calls.

It is also important to keep in mind that both treated and comparison youngsters have similar formal employment rates in the baseline survey across all the public calls. We use a legal-view definition of formal employment; in particular we consider that a job is a formal one when there is compliance with any of the following conditions: whether the youngster signed an employment contract, whether she is covered by accident or health insurance, or whether she has entitlement to a retirement fund pension. The follow-up data reveal that after participating in PROJoven, program beneficiaries experienced a dramatic increase in the likelihood of being formally employed relative to their comparison counterparts.

B. Assessing the Sources of Selection Bias

Before presenting estimated program effects, we spend a few words on two potential sources of bias mentioned before. We first address the issue of

[19] Data to produce these figures are averages for each outcome variable for treatment and comparison units drawn from the evaluation data. These averages are reported in Appendix 1, which also includes the figures for the male and female samples independently.

systematic differences among program beneficiaries and other treatment seekers, that is, between youngsters that were admitted at ECAPs and those who enrolled in PROJoven but did not enter to the beneficiary group. For this analysis, we use data from PROJoven registry of eligible seekers and beneficiaries. The other issue is the Ashenfelter´s dip, here we analyze PROJoven evaluation data and compare program beneficiaries from the treatment sample to their quasi-experimental comparison counterparts.

1. Eligible seekers vs. Beneficiaries: analysis of the registry of eligible and beneficiaries

In this sub section we address the issue of observable differences between eligible non-beneficiaries and beneficiaries. As we have mentioned before, the selection of eligible youngsters is conducted following a pre-specified accreditation process aimed at reaching the poorest among all applicants to the program. However, once the accreditation at PROJoven is completed, the selection processes of beneficiary individuals take place at the ECAPs. These ECAPs apply their own selection criteria, which is not under the direct control of PROJoven.

We analyze the final stage in the selection of program beneficiaries in terms of the ex-post outcomes of these processes. To this end, we exploit data from PROJoven's Registry of Eligible and Beneficiaries. This registry contains information on all the individuals who applied to PROJoven, and identifies those who where accredited as eligible and those who ultimately were selected as beneficiaries. After completing the Accreditation Form (*Ficha de Acreditación*), PROJoven social workers are able to determine whether an applicant qualifies as eligible. When a young applicant qualifies as eligible and accreditation is granted, then a Socioeconomic Status Form (SSF) for the individual has to be completed. This SSF gathers specific information on the individual, their household and some variables to track her living standard. In this regard, the variables used to construct the poverty score which serves as the targeting instrument to select eligible youngsters are also recorded on this form. Additionally, the registry also identifies youngsters that receive admission to an ECAP and the F3 file identifies whether the youngster completed the training.[20]

Despite the fact that all accredited youngsters in the registry are eligible to receive training, only a fraction of about 60 to 70 percent is finally selected as beneficiaries at the ECAPs selection process. Using the information comprised in the Registry of Eligible and Beneficiaries we are able to test for differences in

[20] Non accredited youngsters are also included in the registry, but only limited information on them is available because they did not qualify and so didn't complete the Socioeconomic Status Form.

observable characteristics between youngsters accredited and admitted at an ECAP (PROJoven beneficiaries) and youngsters accredited but not admitted (eligible non-beneficiaries).

Table 3 reports averages for eligible non-beneficiaries and for beneficiaries and corresponding p-values for tests of differences for several characteristics drawn from the registry across several public calls. Overall, we find that those in the beneficiary group are less likely to be high school dropouts and more likely to have acquired a high school diploma. Program beneficiaries have slightly more years of schooling than eligible non-beneficiaries do, but this small difference is statistically significant. Additionally, those who have received vocational training before PROJoven are less likely to enter the beneficiary pool, although the difference with respect to non-beneficiaries is significant only at the 10 percent level. We find no significant differences between beneficiaries and eligible non-beneficiaries on other observable dimensions, such as age, percentage of female youngsters, current school enrollment or whether they worked during the last week prior to the recollection of information. Finally, as expected, the accreditation score computed using the formulae provided in the Accreditation Forms turned out not statistically significant between eligible non-beneficiaries and beneficiaries.

2. The Ashenfelter's dip

We also explore the possibility of systematic pre-treatment differences on employment trends between treated and comparison youths that may lead to selection bias when data are available. Figure 3 displays monthly employment rates for treated and comparison individuals based on retrospective information reported by these youngsters in the baseline and follow-up surveys during the fourth, sixth and eighth public calls.[21] For the baseline survey, these retrospective data cover the time span corresponding to the previous six months before training. For the follow-up surveys, the data covers the time span between the previous survey and the date of the current follow-up survey. In each panel, we use vertical lines to indicate the reference period for the information asked over in the baseline survey and the span of PROJoven training. The first vertical line corresponds to the reference period of baseline information. However, it is important to bear in mind that the baseline surveys were actually conducted during the first month of training, not at the calendar time of the reference period. The second and third vertical lines mark the beginning and end of the six-month period of training. These lines are only referential because not all courses began at the same calendar month, in practice there could be a gap of one to one and a

[21] The evaluation data we received did not include retrospective information for baseline surveys in the first and second public calls.

half month between early and late beginnings, this happens because differences on starting dates on ECAPs within a city or because of differences between cities.

A visual inspection of the evolution of employment rates reveal that employment rates fell in the months before the beginning of training for treated youths but not for their comparison counterparts. This evidence may suggest the presence of an Ashenfelters's dip in these data. This dip on employment rates may be the result of self-selection, but as we claimed before, it may also be the result of the timing of the baseline survey. In this context, we believe that PROJoven can improve its evaluation system by fielding the baseline survey before training begins. Given the differences on employment rates trends between treated and comparison youths before training, we use the employment history when data are available to construct dummy variables indicating whether each individual in the samples is working or during the months before the baseline survey and include them in our estimation of the propensity score.

It is important to mention that the employment status variable obtained from the core baseline questionnaire differs from the employment status obtained from the retrospective questionnaire. The data obtained from the retrospective section do not match exactly that obtained from the core baseline questionnaire. We believe that the differences might be related to the fact that the core questionnaire contains more stringent questions than the retrospective section to construct employment indicators. In particular, the core questionnaire contains a sequence of several questions designed to disentangle whether the youngster is actually working on a paid job, whether she is actively looking for a job, or not while the retrospective questionnaire only asks for a self-report on employment status. Despite these differences, it is interesting that the evolution of employment rates after treatment concludes is quite stable.

C. Estimates of PROJOven Impacts

We exploit the panel dimension of the evaluation data to implement both difference-in-difference (DID) and cross-section (CS) versions of propensity score matching.[22] In our estimations of PROjoven impacts we used the Epanechnikov kernel to compute weighting functions to estimate the counterfactuals. In order to impose the common support condition, we follow the procedure proposed by Heckman, Ichimura and Todd (1997, 1998) and

[22] In a lengthier evaluation report, we also provide CS and DID regressions instead of matching. The rationale for these comparisons is that each of these non-experimental methods imposes different identification assumptions. Given the non-experimental design of the evaluation data, it is a good practice to explore the implications of these different assumptions on the estimated program impacts. However, the qualitative results do not differ from those reported in this paper.

Heckman, Ichimura, Smith and Todd (1998), using a trimming rule of 5 percent. We compute standard errors using the bootstrap method based on 200 replications. Figure 7 displays estimated propensity score densities for treated and comparison units.[23]

We analyze two sets of outcome variables. The first set comprises discrete outcomes: whether the youngster is employed (working), whether she is working in a paid job, and whether she is working in a formal job. For these outcomes, we compute the percentage gain (in terms of change in probability) after program participation. The other set of outcomes are continuous variables: monthly and hourly earnings, and weekly working hours. For these outcomes, we estimate point program effects and provide the percent effect computed as the percentage gain with respect to the average outcome in the comparison group. We also estimate program effects on the censored version of these outcomes because in the data we have youngsters working as unpaid family workers, unemployed youngsters or youngsters out of the labor force at the time of the evaluation surveys. For these individuals, there are no data on earnings (for unpaid workers and those not working) or hours of work (for those not working). In these cases, we compute censored outcomes by replacing missing data by zeroes, that is, we set the outcome equal to zero when the youth was not working and compute the DID and CS kernel estimators over the whole set of observations.

1. Employment

We first explore the effects of program participation on employment rates. Estimates for the overall sample do not present any clear pattern of program effects over time within public calls or across public calls on employment probabilities, see Panel A from Table 4. Actually, few of our estimates are statistically significant in the overall sample, in particular CS estimates. For instance, during the first public call we find a positive and statistically significant CS effect equivalent to an increase of 12 percentage points on employment rates 18 months after PROJoven participation. During the eighth public call, we find positive and statistically significant CS effects 6 moths after and 12 months after training. These estimates suggest that employment rates increased by 8.6 percentage points 6 months after training and by 7.3 percentage points 12 months after training.

For males, the only statistically significant estimates are actually negative. Both DID and CS estimates suggest that employment rates are lower for treated youngsters by between 17 and 14 percentage points during the second public call

[23] Probit models used to estimate the propensity scores are reported in the Appendix.

6 months after program participation, and by between 7 and 5 percentage points during the sixth public call 18 months after program participation. The only pattern that emerges among male youths is that most of the estimates are not statistically significant. However, for female youngsters we do find several positive and statistically significant program effects on employment rates in all the public calls. Both DID and CS estimates, when statistically significant, are of similar magnitudes. For instance, during the second public call employment rates increased by 20 percentage points for treated female youngsters with respect to the comparison group according to the DID estimate, and by 19 percentage points according to the CS estimate.

Splitting the sample by age groups reveals no clear patter and we do not find statistically significant employment effects for 21-25 year olds. We find only few positive and statistically significant employment effects for 16-20 year olds, in particular during the eighth public call. For this call both DID and CS estimates are positive and statistically significant; even more, these estimates are of similar magnitude, suggesting that employment rates increased by between 6 and 7 percentage points 6 and 12 months after program participation.

2. Paid employment

Another outcome variable of interest is the probability of having a paid job. As in many other developing countries, in Peru there is a large fraction of people employed as unpaid family workers, particularly among youths. In Table 5 we explore PROJoven impacts on the likelihood of having a paid job. Overall we find positive and statistically significant program impacts in all the calls except the fourth call when we use the DID estimator; when we use the CS estimator estimates from the first public call also turn to statistically insignificant. For instance, during the first public call the DID estimator suggests that PROJoven beneficiaries increased their likelihood of being a paid worker by 17 percentage points six months after program participation, by 19 percentage points twelve months after program participation, and by 20 percentage points eighteen months after program participation; however, none of the CS estimates are statistically significant. During the second public call, the likelihood of having a paid job increased by 9 percentage points sixth months after program according to the DID estimator and by 8 percentage points according to the CS estimator. During the sixth and eighth public calls, the likelihood of having a paid job increased by 4 to 8 percentage points.

When we split the sample by gender, we do not find statistically significant program effects on the likelihood of having paid job for male youths. However, for females we find that the likelihood of being a paid worker increased after participating in PROJoven with positive and large effects. These results are consistent with the fact that among women there is a larger fraction of unpaid family workers than among men. Splitting the sample by age, we also find that PROJoven has positive impacts on the likelihood of being a paid worker for 16-20 year olds (first, second, sixth and eight calls) but not for 21-25 year olds, both using DID and CS estimators. These results are also expected since younger individuals usually have lower levels of labor market experience, so the program might have bigger impacts among them.

3. Formal jobs

We also explore program effects on job/employment quality. To measure employment quality we use a definition of formal employment typically used in the Peruvian labor literature: we consider formal jobs as those with a signed job contract, with access to social security, with access to accident insurance, or access to health insurance. Table 6 reports DID and CS estimates on the likelihood of having a formal job. For all the public calls under analysis, we find positive and statistically significant program effects on the probabilities of being a formal worker. Estimated program effects using both the DID and CS estimators indicate that formal employment increased after program participation by 7 to 18 percentage points. The estimated program effects suggest that the impact on formal employment remain positive and statistically significant over the time span covered by the three follow-up surveys in each public call, but the effects are higher six months after program participation than after eighteen months after participation. In particular, six months after participation the likelihood of having a formal job increased by 11 to 18 percentage points, while eighteen months after participation the estimated program effect is 8 to 13 percentage points higher for program beneficiaries than for they comparison counterparts. The estimates also reveal a decreasing trend of PROJoven impacts on likelihood of having a formal job across calls.

An important factor that may help explain these results is that during the on-the-job training stage, program beneficiaries must be hired at the firms providing the internships under the legal terms of the so called youth labor training agreements (*convenios de formación laboral juvenil*). These agreements are in place to promote youth labor training by allowing firms to hire youths 16 to 25 years old under special conditions regarding the labor contract. These agreements replace the ordinary labor contract, do not grant social security, but provide insurance coverage events of illness and accidents. Another important feature of these agreements is that youth employees under a labor training agreement must be paid at least the mandated minimum wage. Additionally, the very fact that the internship firms agreed to hire interns under the terms of these agreements is an indication that these firms are likely formal instead of informal enterprises. Many of PROJoven beneficiaries keep their jobs at the internship firms at the time of the first follow-up survey carried out six months after having completed the training provided by PROJoven, this might explain in part the results.

However, in every public call under study, these positive program effects on formal employment remain positive and statistically significant, even after twelve and eighteen months after completing the training. Despite we find a declining program effects in every call between the six-months after and the eighteen-months after follow-up surveys, these impacts did not completely fade out. We argue that its is possible that even if the program does not contribute to increase beneficiaries' productivity, it is likely that having worked for a formal firm contributes to the likelihood of getting better jobs later. This also has important implications in terms of earnings, as we will comment on later.

Splitting the sample by gender, we find positive and statistically significant program effects on the likelihood of having a formal job both for male and females beneficiaries with respect to their comparison counterparts. However, our estimates suggest that the effects are higher for women than for men. These results are consistent with the fact that young women face worse labor conditions and therefore are likely to benefit the most out of program participation. For instance, during the first public call DID estimates vary between 7 to 13 percentage points and CS estimates between 5 to 11 percentage points for males; however, for females these estimates vary between 17 to 21 and 15 to 20 percentage points respectively (see panels B and C from Table 6, columns 1 and 6). When we explore treatment effect heterogeneity by age, we find that PROJoven had slightly bigger program effects on the likelihood of having a formal job among 16-20 year olds than among 21-25 year olds during the first, second and eighth public calls. On the contrary, we find that during the fourth and sixth public calls PROJoven program effects on the likelihood of having a formal job were bigger among 21-25 year olds.

4. Real monthly and hourly earnings

In general we find that PROJoven had high positive and statistically significant program effects in terms of real monthly earnings across all the public calls we study, both using the DID and CS estimators. An important result, however, is that CS estimates are much lower than DID estimates in particular during the first, sixth and eighth public calls. This happens because average real earnings for PROJoven beneficiaries were lower than for their comparison counterparts at the recall time of the baseline surveys in those calls (cf. Figure 5).

In Tables 7 and 8 we report our DID and CS estimates of PROJoven program effects on monthly earnings. We find the highest program effects on monthly earnings during the first public call. Our DID estimates suggest that PROJoven beneficiaries experienced increases of 60 percent, 44 percent, and 39 percent with respect to their comparison counterparts six, twelve, and eighteen months after program participation. The corresponding CS estimates are 30 percent, 12 percent, and 7 percent, respectively. For the other public calls, we also find high positive and statistically program effects, with the exception of the sixth call in which the CS were not statistically significant. We also find that PROJoven program effects on monthly earnings do not disappear over time, for all calls except the sixth we find that PROJoven effects on earnings remain positive and statistically significant twelve and eighteen months after program participation. Additionally, we find that PROJoven program effects on earnings, measured as percent gains, are even higher when we compute the DID and CS estimates using censored earnings. The reason for this is that PROJoven had also positive impacts on employment rates and job quality, so not only beneficiaries experienced an increase on their earnings, but also more youngsters are working after program participation. Comparing program effects across calls we find that the short-term PROJoven program effects on earnings computed as the percent gain, that is, six months after program participation, decrease between the first and sixth public calls, and then rebound during the eighth public call. However, time patterns across calls are less clear when comparing percent gains twelve and eighteen months after program participation. In general, comparing our results to evidence reported in the international literature on job training programs we find that PROJoven program effects on earnings are large; we comment more on this later.

Exploring program effects heterogeneity by gender, we find that PROJoven impacts are higher for female youths than for their male counterparts. For male youngsters, we find positive and statistically significant effects six months after program participation in the first, second and eighth public calls with estimated percent gains between 18 and 53 percent; there are no statistically significant short-term effects during the second and fourth public calls. Eighteen months after program participation, we only find statistically significant effects using the DID estimator during the first, fourth and sixth public calls; instead, none of the CS estimates were statistically significant. On the other hand, for female youngsters, we find positive and statistically significant PROJoven program effects in all the public calls under study. The estimated effects are as high as three times the baseline in the first public call. Even more, in almost all the cases female beneficiary youths duplicate their (censored) monthly earnings after training with respect to the untreated counterparts. We also find slightly higher point estimated among 16-20 year olds than among 21-25 year olds.

Additionally, when looking at the censored monthly earnings we find the same patterns just described: positive and statistically significant program impacts on monthly earnings, higher impacts among women, and higher impacts among 16-20 year olds. More over, we find even higher percent effects when using censored monthly earnings. The combined effects on employment and employment quality might explain these results for women and 6-20 year olds.

In most evaluation studies of this sort of programs, the estimated effects are low and in most cases, no positive program effects are found. Is it possible to reconcile these results from the international experience with those from PROJoven? Could six month of training be so effective? We argue that several factors may help explain these results.

A first important feature of PROJoven is that program beneficiaries specially are targeted to be among the poorest youngsters from Peruvian urban areas. Even if no productivity effects materialize, having participated in the program may induce some sort of credentialism effect of program beneficiaries among the pool of poor youngsters. On the other hand, the training courses are designed to provide PROJoven beneficiaries the skills actually required by firms in the marketplace. The match between the contents of courses and actual demand for skills at private firms is reinforced by the internship stage of the training. It is important to remember that participating firms must pay the trainees, so they are not free labor. By minimizing a mismatch between the skills provided by ECAPs and actual human resources requirements on quality by firma might be generating some real improvements in productivity for program beneficiaries so these youngsters are likely to meet labor demand requirements at real firms.

On the other hand, at least initially, beneficiaries must be hired under the aforementioned agreements of youth labor training, which imply better quality jobs than those accessible to average poor youngsters. These agreements not only improve on employment conditions (health and accident insurance coverage, etc.), but also imply that PROJoven trainees receive at least the mandated minimum wage. However, it is possible that because of participating in the internships, the beneficiaries improve their labor prospects because now they have “a line” in their CVs, so PROJoven may entail a credential effect instead of or in addition to potential productivity enhancements. In this sense, the program improves the likelihood to engage in formal jobs, instead of on unpaid family jobs or sporadic informal jobs. This is of particular importance for females and youngsters that have recently completed basic education and have no labor experience in a formal private firm.

There is also the possibility that pre-treatment earnings data provided by the baseline survey might have problems as described earlier, in the sense that beneficiaries may decide to leave jobs once they receive notice of being admitted by ECAPs. This combined with the fact that some beneficiaries would still be working in their internship firm under labor agreements during the first follow-up surveys, would affect our DID estimates of program effects, in particular it would overestimate the impact of PROJoven. However, when we estimate program effects on earnings using only the cross-sections of the follow-up surveys we also find positive, although lower, and statistically significant estimates. Thus, it seems that the program effectively contributes to increase real earnings.

In terms of hourly earnings we also find positive and statistically significant effects of PROJoven, both in terms of the DID and CS estimates, as reported in Tables 9 and 10. In general, however, we find fewer statistically significant effects on hourly earnings compared to monthly earnings. When looking at different subgroups, we find that program effects on uncensored hourly earnings are higher for women and for 16-20 year olds. On the other hand, when looking at the censored hourly earnings we find that most of the estimates are statistically significant and positive for the overall sample. Again, we find that CS estimates are lower than the DID counterparts, but still we find that some of our CS estimates are positive and statistically significant. When splitting the sample by gender we find that among men beneficiaries only the DID estimates during the first, sixth, and eight calls are positive and statistically significant; no effect is found for the second and fourth public calls, nor using the CS estimator. By the contrary, for women we do find high positive and statistically significant program effects on hourly earnings, both using DID and CS estimates (with the only exception of the first and second follow-up CS estimates during the sixth public call). In particular, the percent effects on censored hourly earnings are huge, ranging between 30-120 percent. We also find that the program is most effective among 16-20 year olds beneficiaries.

5. Hours of work

The final set of outcomes are uncensored and censored weekly hours of work. Tables 11 and 12 report the DID and CS estimates. In terms of uncensored weekly working hours for the overall sample we find positive and statistically significant PROJoven impacts using the DID estimator in the first and eight calls and for all the baseline/follow-up comparisons in those calls. There are also positive DID estimates for the third follow-up of the fourth public call and the second follow-up of the sixth public call. The only positive and statistically significant CS estimates we find correspond to the first follow-up of the eighth public call. Splitting the sample by gender, we find no program effects on hours of work for men (except during the first public call using, DID estimate) and that most of the point estimates for men are actually negative, although not statistically significant. For women we find positive impacts on weekly working hours during the first and eighth public calls, in particular both the DID and CS estimates are positive in the first follow-up but the CS turns insignificant statistically in second and third follow-ups. Although most of the estimates for females are not statistically significant, the point estimates are positive. When looking at differences by age groups, we find that 16-20 year olds benefit more from the program in terms of hours of work. However, we find positive and statistically significant estimates only in the first and eighth calls for the 16-20-cohort and for the fourth and sixth calls for the 21-25-cohort.

Finally, in terms of censored weekly working hours we find positive estimates for the overall sample but not in every follow-up survey. We only find positive and statistically significant program effects estimates, both DID and CS, on censored hours during the first public call using data from the third follow-up, during the second and fourth public calls using data from the second follow-up, and during the eighth public call using data from the first and second follow-ups. We also find treatment effect heterogeneity by gender: among men, we find negative effects on hours, while for women the effect is positive. When splitting the sample by age groups we find that most of the estimates are not statistically significant, and the only clear difference by age in favor of 16-20 year olds is found during the eighth call when no effects are found for the 21-25 year olds.

D. Additional Discussion

Even when the quasi-experimental design of PROJoven evaluation data allows to get a comparison group statistically equivalent to treated youngsters on some relevant observable characteristics (such as gender, age, education, place of residence and socioeconomic status), we believe that beneficiaries self-selection and selection induced by the admission processes at ECAPs based on other unobservable characteristics to us might be important.

We have used DID methods in an attempt to remove any time invariant secular trend between treated and comparison youngsters that may confound the true program effects. However, we also find that pre-treatment differences on earnings between the treated and comparison groups may be the result of the timing of the baseline survey and their reference periods used to collect pre-treatment data. If we assume that the difference in pre-treatment earnings between beneficiaries and non treated youths are only born by this timing, the DID estimates overestimate program impacts. If those are only mechanical differences and not the result of selection on unobserved factors, CS estimates are cleaner. Using these CS estimators we still find positive but smaller program effects on earnings after participation in PROJoven's training.

Still, it is possible that the quasi-experimental comparison group is not equivalent to the treated group because of self-selection. We suggest some ways to overcome the problems we have found with PROJoven evaluation data. One possible alternative is to implement an experimental evaluation. This is the preferred evaluation design because avoids the problems of self-selection, both in terms of observable and unobservable characteristics, by separating eligible youngsters into beneficiaries (treated) and non-beneficiaries (controls) randomly. However, this design has its own problems and limitations in the present context that have to be addressed correctly. Two important issues are how and when to implement the randomization of eligible youngsters into the treatment and comparison groups. This is important PROJoven only selects eligible youngsters but the final decision of whether an eligible youngster becomes a program beneficiary is a decision made by ECAPs, which behave strategically to select the best candidates among the pool of eligible youngsters. Thus, implementing an experimental evaluation will likely imply modifications in the program's rules to gain control over the way beneficiaries are selected, or to persuade ECAPs to accept randomized-in beneficiaries instead of selecting them.

A second alternative is to remain under the current quasi-experimental design, but improving the way comparison youngsters are selected. This could be attained under the current design by increasing the number of matching characteristics and the size of the comparison sample to allow a second stage matching upon the data. The baseline data should also contain retrospective information on employment and earnings histories, recorded in the same way as in the core interview questionnaire. It will be crucial to design the baseline survey in order to avoid divergent trends between treated and comparison units and to carry-out the field work and setting the reference periods in such a way as to avoid that knowledge about program acceptance cause mechanical differences between treated and comparison groups. In this context, it is extremely important to improve the quality of the retrospective section of the baseline survey along the lines already mentioned.

A third alternative is to implement a different quasi-experimental design. Based on the selection process of eligible youngsters and beneficiaries, one possibility that should not imply additional costs to the program is to collect information on eligible youngsters that did not fill a course vacant. In contrast to youngsters in the quasi-experimental comparison group under the current design, these eligible youngsters are equivalent in their motivation to participate in PROJoven and are also equivalent in the observables that accredited them as potential beneficiaries.

However, given that the ECAPs select beneficiaries in a self-interest way, these youngsters may be systematically different from their beneficiary counterparts along other dimensions we do not observe. But it is possible for PROJoven to design additional survey instruments to collect information of the sorts of characteristics typically used by ECAPs officials in deciding who to admit. This could be done by conducting in-depth interviews with ECAPs officials for instance to find out what those characteristics are. These characteristics would allow implementing an instrumental variable approach or a regression discontinuity design. Since all the eligible individuals (beneficiaries and non-beneficiaries) participate in the selection process at the same calendar time, applying a baseline survey to all of them would provide richer data. An additional advantage of having eligible non-beneficiaries as the comparison group is that PROJoven could apply the baseline survey two or three months before training begins and before eligible youngsters realize whether they have been granted a course vacant. These data could be gathered at the same time when socioeconomic information used to grant eligibility is collected.

It is also necessary to improve the survey questionnaires, putting efforts on retrospective information and selecting an appropriate timing for the fieldwork of the baseline survey, specifically conducting the baseline data gathering well before program participation begins.

Additionally, we believe that it is necessary to design and maintain a data bank system that allows record all the information (evaluation data, RECAP, monitoring reports, etc.) in a systematic way. We have found several difficulties to obtain the evaluation data we use to perform this study. For instance, it is no clear that there exist a unique official version of these data. We have received up to four different versions of the data involving different numbers of observations or with a different set of variables depending on the public call. After going back and forth with PROJoven personnel, we were assured that the data we finally obtained and have access to are the definite version. In this process, we discovered that PROJoven does not have a systematic protocol to store and maintain its information. The sorts of information or database that are actually stored depend on decisions made in a call-by-call fashion. We believe this could easily be improved by the program.

IV. COST-BENEFIT ANALYSIS AND ESTIMATION OF THE INTERNAL RATE OF RETURN

In this section we report the results of a cost-benefit analysis and our estimates of the internal rate of return (IRR) of PROJoven under different scenarios. It is important to bear in mind that given the difficulty to obtain benefit figures and the true IRR, several assumptions shall be made and only proxy figures will be obtained. Our approach will be to provide upper and lower bounds for the IRR.

A. Data

1. Benefits Data

We estimate two types of benefits. First, we compute the benefits received during the treatment: a) stipends-subsidies and insurance received during the training stage (which are also costs to the program); and b) stipends received during the on-the-job training stage. These benefits will be computed using administrative data from PROJoven. Second, we use the estimated benefits because of PROJoven participation, which are the gains in terms of employment opportunities and earnings for beneficiaries with respect to the comparison group. These benefits are computed using the estimation of program impacts from the previous section for the censored real monthly earning variable. The estimated effect for this variable gives the combined gain in earning, employment and hours of work for beneficiaries.

2. Costs Data

We use information provided by PROJoven to quantify the total cost of the program. This includes both the direct costs of the program (i.e. the cost of training courses, stipends and other subsidies given to beneficiaries, and administrative costs) and opportunity costs for beneficiaries (lost earnings during the training).

We got access to costs data for the First, Sixth and Eighth Public Calls. Costs data for the Second Public Call were not provided, while data provided for the Fourth Public Call seem unreasonably high. For these calls, we used the unit cost from the First call. To estimate the (per capita) opportunity costs for each call, we use data from the baseline evaluation data, these costs are equal to the real monthly earning of the treatment group. We assume that beneficiaries incur these costs for four months, three months of course duration plus one extra month before the courses.

3. Scenarios to compute IRR

In order to compute IRR for PROJoven we use a 45-year span. The costs are incurred during period. Benefits for beneficiaries (gains in terms of earnings, employment opportunities and hours of work) during the first year after having received treatment are equal to the DID estimates for the first follow-up multiplied by 12 months. For the second year, we assume that the total benefits are equal to the DID estimates of the obtained from the second and third follow-ups each multiplied by six moths.

We assume three scenarios regarding the evolution of benefits for beneficiaries after receiving treatment. Under a pessimistic scenario, we assume that benefits decrease at a 50 percent rate per year; under a neutral scenario, we assume that benefits decrease at a 25 percent rate per year; while under an optimistic scenario, we assume that benefits decrease at a 10 percent rate per year.

B. Results

Table 3.7 presents the results of our calculations. Even in the pessimistic scenario the simulated IRR are above 4 percent. We use both the DID and CS estimates to simulate benefits streams, thus we also simulate two sets of IRR. In cases where the net present value (NPV) of program costs is greater than the NPV of the stream of benefits, meaning that only a negative interest rate can net out the NPV of benefits and costs streams, we report an IRR equal to zero. Additionally, for the sixth call none of the CS estimates were statistically significant, so we set the IRR to zero.

As expected from our analysis of DID and CS program impact estimates, we find higher IRR for the DID estimates than for the CS estimates. Assuming that the CS estimates provide a lower bound for program impacts, we find relatively low IRR for PROJoven, even when looking at the neutral scenario for benefits streams. To have an idea about the magnitude of these simulated IRR, the real active interest rate (TAME) and the real interest rate on savings accounts corresponding to the calendar year of each public call were 16.8 and -0.9 percent (1996), 22.5 and 3.3 percent (1997), 29.3 and 4.3 percent (1998), 22.0 and 3.7 percent (2000), and 23.2 and 3.1 percent (2001).

V. CONCLUSIONS

Our analysis arrives at four sets of conclusions. The first one concerns the institutional analysis. The second focuses on the evaluation data and more generally data management in the Program. The third has to do with impacts on beneficiaries, while the fourth focuses on impacts on the vocational training market.

The institutional analysis indicates that PROJoven has been a remarkably stable and well managed program. This probably has to do with features of the Program that made it unattractive to political capture, such as its size, its location in the poorest ministry of the central government's public administration, small visibility, and the difficulties of selling vocational training politically as compared, for instance, with infrastructure investment or plain temporary employment programs. The Program was well designed, incorporating the experiences of similar programs in the region and an important effort was made to improve upon them, particularly focusing on the demand driven mechanism and on the pertinence of training. In addition, impact evaluation was considered from the outset. Recent instability is associated to political capture. However, it should be noted that in order to make political capture sustainable some degree of efficiency is needed. So it is likely that the current turmoil situation the Program is experiencing will eventually settle down in order to make the Program viable. However, given the proximity of elections it is difficult to predict whether the Program will go back to a more technical management.

We find poor data management practices in PROJoven. In most cases the data exist, but are difficult to use just because are not stored in formats that can make them more easily available and user friendly. Part of the information produced during the several processes of accreditation and selection of beneficiaries and ECAPs is never processed and stored in magnetic format. Valuable information is lost after it is used during a public call. For instance, there is a huge amount of information that PROJoven collects when dealing with the selection of ECAPs and the process of courses biding. Some of these data are converted into magnetic format and enter the RECAP dataset. However, large fractions of the whole data never reach the RECAP dataset or are processed in a non systematic way (some information that enters the RECAP is not updated when an ECAP apply again two calls later). This limits the ability of PROJoven to study its impacts on the vocational training market, just because its administrative data collection process is not exploited better. Additionally, evaluation data has not been well kept and at this point access to raw data in their original format is difficult. In particular, data on employment histories for the baselines from the first and second public calls evaluation data are lost (as long as we have been informed) because they were never processed into magnetic format. A similar problem occurs to the third follow-up survey of the evaluation data from the eighth public call. A recommendation is that an effort should be made to organize the data and generate a system for maintaining it in the future. Processes should be incorporated in the project cycle to ensure that data collected is properly stored and kept. This should also involve the RECAP data as well that on the performance of training firms.

For our analysis of PROJoven impacts on beneficiaries, we have conducted longitudinal version of propensity score matching to tackle the issue of selection bias that arises because of the way beneficiaries are selected into the program and how the evaluation data is constructed. In particular, there are two potential sources of selection bias in these data. First, the very fact that youngsters in the comparison group did not seek employment training might reflect systematic (and unobserved) differences with respect to beneficiaries even when these controls are drawn from the same local labor markets and neighborhoods where beneficiaries in the evaluation sample reside. Second, it is not completely clear how ECAPs select beneficiaries, it is likely that they apply some sort of entry test but they may well be using different selection strategies. Our analysis of systematic differences on observable characteristics using data drawn from the Registry of Eligible and Beneficiaries suggest that youngsters that finally get admission into ECAPs to receive training are more educated than eligible non-beneficiaries are. However, we also find pre-treatment differences in monthly earnings in the evaluation data that may be the result of the timing of the fieldwork in the baseline surveys. For this reason, we also report cross-section propensity score matching estimates that compare outcomes of treated and controls in the post-treatment period. It is important to mention that we cannot rule out systematic differences between treatment and comparison units on time invariant unobserved characteristics. Additionally, we believe that an alternative quasi-experimental control group drawn from the pool of eligible non-beneficiaries youngsters might serve as a better counterfactual. Alternatively, we suggest contemplating the possibility to move instead to an experimental evaluation design.

Our overall DID and CS estimates suggest that there are positive and statistically significant effects in terms of paid jobs and formal employment probabilities, and in terms of monthly earnings for all the public calls, we study. When studying treatment effects, heterogeneity we also find that female youngsters and 16-20 year olds seem to benefit more from the program. In general, they experienced higher PROJoven impacts on paid job probabilities, formal jobs probabilities and monthly earnings than their male and 21-25 year olds counterparts. We also find that, overall, the positive effect of PROJoven on real monthly earnings was extremely high during the first public call, that the impacts decreased from the first to fourth (1996-1998) public calls and then rebound and grew from the sixth to eighth (1999-2000) calls presenting a U-shape.

Despite international evidence on this sort of training programs, we find that PROJoven has high positive impacts in terms of earnings. Our DID estimates suggest that program impacts on monthly earnings and on censored monthly earnings (considering those not working with earnings equal to zero) seem unreasonably large when compared to international evidence. Using CS estimator we find much lower program effects but still are well above 12 percent, and as high as 30 percent.

We argue that, at least in part, this is the result of the match between courses design and real labor demand requirements in the labor market, and that beneficiaries must be hired for their internships under Youth Labor Training Agreements, which provide better job conditions and pay. This is important to understand the large DID estimates using the first follow-up data. On the other hand, PROJoven might be also providing some additional credentials for its beneficiaries as long as the firms participating in PROJoven are firms from the formal private sector, thus after completing the course and internship training phases, these youngsters have acquire signals for other potential employers. We believe that some productivity enhancement and some credentialism must be operating in order to explain the positive program effects on monthly earnings even 18 month after training, particularly for females and 16-20 year olds. However, we cannot rule out problems with the timing of baseline earnings data, or other sources of selection on time variant unobserved characteristics.

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TABLES

Table 1. Evolution of PROJoven: Participating Institutions, Courses, and Beneficiaries

Public Call	Year	ECAPs	Courses	Beneficiaries		
				All	Graduates	Job-training
1	1,996	14	75	1,505	1,450	1,201
2	1,997	19	96	1,807	1,729	1,443
3	1,998	22	122	2,243	2,146	1,762
4	1,998	39	140	2,671	2,527	2,056
5	1,999	43	171	3,075	2,945	2,267
6	2,000	43	203	3,651	3,481	2,768
7	2,001	59	220	4,178	4,052	3,106
8	2,001	61	266	5,157	5,010	3,880
9	2,002	78	292	5,942	5,788	4,668
10	2,002	27	76	1,795	1,736	1,590
11	2,003	29	125	2,312	2,226	-
12	2,004	43	128	2,680	-	-
13	2,005	65	246	5,213	-	-
All		542	2,160	42,229	33,090	24,741

Source: PROJoven

Table 2a. Summary statistics for selected variables

	First Call		Second Call		Fourth Call		Sixth Call		Eighth Call	
	Treat.	Comp.	Treat.	Comp.	Treat.	Comp.	Treat.	Comp.	Treat.	Comp.
Sex	0.434	0.434	0.436	0.423	0.441	0.436	0.481	0.499	0.463	0.454
	(0.496)	(0.496)	(0.497)	(0.495)	(0.497)	(0.496)	(0.500)	(0.500)	(0.499)	(0.498)
Age	19.469	20.206	20.341	20.223	20.261	19.957	19.609	19.754	19.014	19.026
	(2.508)	(2.357)	(2.371)	(2.322)	(2.392)	(2.394)	(2.444)	(2.376)	(2.139)	(2.050)
Secondary education	0.855	0.836	0.865	0.861	0.825	0.803	0.798	0.797	0.495	0.456
	(0.352)	(0.371)	(0.342)	(0.346)	(0.381)	(0.398)	(0.402)	(0.402)	(0.500)	(0.498)
Single	0.913	0.711	0.905	0.763	0.898	0.779	0.895	0.779	0.933	0.839
	(0.282)	(0.454)	(0.293)	(0.426)	(0.303)	(0.415)	(0.306)	(0.415)	(0.250)	(0.368)
Has children	0.148	0.305	0.159	0.299	0.172	0.243	0.142	0.242	0.110	0.181
	(0.356)	(0.461)	(0.366)	(0.459)	(0.378)	(0.429)	(0.350)	(0.428)	(0.312)	(0.385)
Number of children	0.203	0.408	0.189	0.401	0.207	0.332	---	---	---	---
	(0.607)	(0.675)	(0.485)	(0.674)	(0.491)	(0.667)	---	---	---	---
Mother's	0.402	0.312	0.473	0.456	0.366	0.377	0.392	0.377	0.351	0.206
secondary education	(0.491)	(0.464)	(0.500)	(0.499)	(0.482)	(0.485)	(0.489)	(0.485)	(0.478)	(0.404)
Father's	0.588	0.476	0.618	0.609	0.533	0.497	0.526	0.499	0.891	0.927
secondary education	(0.493)	(0.500)	(0.487)	(0.489)	(0.499)	(0.500)	(0.500)	(0.500)	(0.311)	(0.260)
Out-of-labor force	0.241	0.190	0.209	0.266	0.238	0.213	0.245	0.255	0.247	0.194
	(0.428)	(0.393)	(0.408)	(0.443)	(0.426)	(0.410)	(0.430)	(0.436)	(0.431)	(0.396)
Unemployed	0.219	0.293	0.199	0.153	0.196	0.226	0.126	0.136	0.205	0.258
	(0.414)	(0.456)	(0.400)	(0.361)	(0.397)	(0.419)	(0.333)	(0.343)	(0.404)	(0.438)
Poverty score					16.993	17.395	16.703	16.863	16.403	16.517
					(3.559)	(3.376)	(4.137)	(3.492)	(3.891)	(3.297)
Arequipa					0.243	0.241	0.203	0.218	0.199	0.203
					(0.429)	(0.428)	(0.403)	(0.413)	(0.400)	(0.403)
Lima					0.573	0.577	0.362	0.272	0.325	0.319
					(0.495)	(0.494)	(0.481)	(0.445)	(0.469)	(0.466)
Trujillo					0.184	0.182	0.198	0.221	0.209	0.203
					(0.388)	(0.386)	(0.399)	(0.415)	(0.407)	(0.403)
Chiclayo					---	---	0.122	0.145	0.147	0.149
					---	---	(0.327)	(0.352)	(0.354)	(0.356)
Cusco					---	---	0.116	0.144	---	---
					---	---	(0.320)	(0.351)	---	---
Huancayo					---	---	---	---	0.120	0.126
					---	---	---	---	(0.325)	(0.332)
Employed t-1					0.428	0.532	0.396	0.498	0.157	0.509
					(0.495)	(0.499)	(0.489)	(0.500)	(0.364)	(0.500)
Employed t-2					0.457	0.501	0.521	0.581	0.365	0.521
					(0.499)	(0.500)	(0.500)	(0.494)	(0.482)	(0.500)
Employed t-3					0.597	0.570	0.671	0.636	0.416	0.545
					(0.491)	(0.496)	(0.470)	(0.481)	(0.493)	(0.498)
Employed t-4					0.534	0.558	0.527	0.553	0.571	0.555
					(0.499)	(0.497)	(0.500)	(0.497)	(0.495)	(0.497)
Employed t-5					0.515	0.544	0.486	0.506	0.451	0.499
					(0.500)	(0.499)	(0.500)	(0.500)	(0.498)	(0.500)
Employed t-6					0.532	0.551	0.476	0.496	0.412	0.471
					(0.499)	(0.498)	(0.500)	(0.500)	(0.492)	(0.499)
Log earnings					4.399	4.520	4.436	4.852	3.999	4.733
last 3 months					(2.891)	(3.064)	(2.766)	(3.011)	(2.817)	(3.029)

Source: PROJoven Evaluation Data, baseline surveys.

Table 2b. Outcome variables

	First Call		Second Call		Fourth Call		Sixth Call		Eighth Call	
	Treat.	Comp.	Treat.	Comp.	Treat.	Comp.	Treat.	Comp.	Treat.	Comp.
A. Employment (%)										
Baseline	0.540	0.518	0.591	0.580	0.567	0.560	0.628	0.609	0.547	0.548
6-months	0.630	0.601	0.639	0.588	0.626	0.603	0.667	0.631	0.642	0.596
12-months	0.617	0.547	0.655	0.562	0.659	0.618	0.664	0.676	0.618	0.572
18-months	0.585	0.460	0.510	0.482	0.688	0.660	0.680	0.656	---	---
B. Paid Jobs (%)										
Baseline	0.354	0.498	0.449	0.460	0.416	0.429	0.518	0.553	0.532	0.538
6-months	0.592	0.547	0.605	0.518	0.589	0.558	0.661	0.625	0.640	0.591
12-months	0.592	0.521	0.625	0.500	0.634	0.594	0.655	0.666	0.612	0.566
18-months	0.505	0.431	0.493	0.482	0.666	0.649	0.671	0.643	---	---
C. Formal Jobs (%)										
Baseline	0.032	0.055	0.014	0.036	0.028	0.028	0.115	0.152	0.129	0.145
6-months	0.228	0.071	0.220	0.058	0.258	0.135	0.307	0.197	0.213	0.144
12-months	0.260	0.132	0.233	0.058	0.256	0.173	0.319	0.243	0.248	0.155
18-months	0.177	0.064	0.203	0.099	0.284	0.171	0.317	0.243	---	---
D. Real Monthly Earnings										
Baseline	209.7	300.5	222.5	242.3	179.4	211.0	238.3	315.9	166.4	239.0
6-months	384.9	297.4	375.5	316.6	341.1	309.7	406.9	416.2	285.9	266.2
12-months	394.4	354.8	427.8	328.8	357.8	337.2	308.2	289.2	339.8	303.9
18-months	389.8	366.5	432.9	363.5	365.8	299.0	319.2	296.1	---	---
E. Real Hourly Earnings										
Baseline	1.453	1.744	1.218	1.457	1.022	1.086	1.365	1.713	1.027	1.277
6-months	1.805	1.430	1.742	1.595	1.507	1.338	1.920	1.961	1.297	1.246
12-months	1.845	1.594	1.908	1.437	1.643	1.524	1.364	1.334	1.460	1.331
18-months	1.707	1.582	1.986	1.696	1.639	1.379	1.451	1.338	---	---
F. Weekly Working Hours										
Baseline	34.1	43.9	42.4	41.5	44.7	48.3	43.1	46.4	42.5	46.5
6-months	49.8	47.7	53.0	52.1	54.5	56.0	52.1	53.5	53.3	52.5
12-months	51.8	54.2	56.1	53.9	54.3	55.0	54.3	54.7	55.6	55.8
18-months	53.3	53.0	53.3	52.4	54.7	53.8	53.3	54.1	---	---
G. Real Monthly Earnings (censored)										
Baseline	63.4	131.4	89.5	103.5	90.8	110.0	124.1	180.4	79.4	122.2
6-months	200.5	148.2	225.8	158.3	205.1	176.4	256.0	250.4	173.7	147.3
12-months	230.8	176.9	254.4	152.4	226.9	197.1	192.1	187.7	202.9	164.9
18-months	191.8	149.7	210.6	164.5	238.4	189.7	204.7	182.2		
H. Real Hourly Earnings (censored)										
Baseline	0.439	0.763	0.490	0.622	0.517	0.566	0.711	0.979	0.490	0.653
6-months	0.940	0.713	1.047	0.797	0.906	0.762	1.208	1.180	0.788	0.690
12-months	1.080	0.794	1.134	0.666	1.042	0.891	0.850	0.866	0.872	0.722
18-months	0.840	0.646	0.966	0.768	1.068	0.875	0.930	0.823	---	---
I. Weekly Working Hours (censored)										
Baseline	16.6	20.2	23.1	22.7	24.2	26.2	24.7	27.5	21.6	24.9
6-months	27.4	26.4	33.9	29.8	33.7	32.7	33.5	33.0	33.4	30.5
12-months	31.7	28.4	35.7	28.5	35.3	33.2	35.1	36.5	34.1	31.7
18-months	30.8	23.3	26.8	24.3	36.9	35.2	35.5	34.5	---	---

Source: PROJoven Evaluation Data, baseline and follow-up surveys.

Table 3. Differences between PROJoven eligible no-beneficiaries and beneficiaries

	Eligible non-beneficiary	Beneficiary	Diff. p-value
First Call			
Age (in years)	20.17	20.12	0.56
% Female	0.53	0.56	0.11
Years of schooling	11.62	11.76	**0.00**
% Complete primary education	0.01	0.01	**0.06**
% Incomplete high school	0.18	0.13	**0.00**
% Complete high school	0.80	0.86	**0.00**
% Vocational training	0.13	0.11	**0.08**
% Enrolled in school	0.01	0.01	0.72
% Worked last week	0.19	0.19	0.87
Fourth Call			
Age (in years)	20.28	20.28	0.97
% Female	0.54	0.56	0.21
Years of schooling	8.51	8.37	**0.00**
% Complete primary education	0.01	0.00	**0.00**
% Incomplete high school	0.79	0.86	**0.00**
% Complete high school	0.17	0.12	**0.00**
% Vocational training	0.01	0.01	1.00
% Enrolled in school	0.15	0.15	0.78
% Worked last week	0.17	0.15	**0.03**
Accreditation score	14.23	14.07	0.09
Sixth Call			
Age (in years)	19.65	19.43	**0.00**
% Female	0.56	0.54	0.12
Years of schooling	10.00	10.37	**0.00**
% Complete primary education	0.04	0.02	**0.00**
% Incomplete high school	0.23	0.16	**0.00**
% Complete high school	0.72	0.81	**0.00**
Accreditation score	16.05	15.93	0.23

Source: PROJoven Registry of Eligible and Beneficiaries.

Table 4. Estimated treatment effects of PROJoven on employment probabilities

	Difference-in-Difference					Cross-Section				
	First	Second	Fourth	Sixth	Eighth	First	Second	Fourth	Sixth	Eighth
A. Overall Sample										
6 months after	-0.02	0.04	0.03	0.03	0.05	0.02	0.05	0.02	0.04	0.05
	(0.05)	(0.06)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)	(0.02)	(0.02)
12 moths after	0.01	0.08	0.04	-0.02	0.06	0.05	0.09	0.03	-0.01	0.06
	(0.06)	(0.05)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)	(0.02)	(0.02)
18 months after	0.08	0.01	0.03	0.02		0.12	0.02	0.02	0.03	
	(0.06)	(0.06)	(0.04)	(0.03)		(0.04)	(0.05)	(0.03)	(0.02)	
B. Males										
6 months after	-0.08	-0.17	-0.05	-0.02	-0.03	-0.04	-0.14	-0.07	0.00	-0.02
	(0.08)	(0.08)	(0.05)	(0.04)	(0.04)	(0.05)	(0.06)	(0.04)	(0.03)	(0.03)
12 moths after	-0.05	-0.02	-0.02	-0.07	0.00	-0.02	0.01	-0.03	-0.05	0.00
	(0.08)	(0.09)	(0.05)	(0.04)	(0.04)	(0.06)	(0.06)	(0.04)	(0.03)	(0.03)
18 months after	-0.03	-0.07	-0.01	-0.07		0.01	-0.04	-0.03	-0.05	
	(0.08)	(0.09)	(0.05)	(0.03)		(0.06)	(0.06)	(0.04)	(0.02)	
C. Females										
6 months after	0.04	0.20	0.10	0.08	0.13	0.07	0.19	0.09	0.08	0.12
	(0.07)	(0.08)	(0.05)	(0.04)	(0.04)	(0.05)	(0.06)	(0.04)	(0.03)	(0.03)
12 moths after	0.10	0.15	0.09	0.03	0.11	0.13	0.14	0.09	0.03	0.09
	(0.07)	(0.08)	(0.05)	(0.04)	(0.04)	(0.05)	(0.06)	(0.04)	(0.03)	(0.03)
18 months after	0.17	0.08	0.06	0.10		0.20	0.07	0.06	0.10	
	(0.07)	(0.08)	(0.05)	(0.04)		(0.05)	(0.05)	(0.04)	(0.03)	
D. 16-20 year olds										
6 months after	-0.01	0.12	0.03	0.04	0.07	0.04	0.09	0.02	0.07	0.07
	(0.07)	(0.08)	(0.05)	(0.03)	(0.03)	(0.05)	(0.05)	(0.04)	(0.02)	(0.03)
12 moths after	0.01	0.12	0.04	-0.03	0.06	0.06	0.09	0.04	0.00	0.06
	(0.07)	(0.08)	(0.05)	(0.04)	(0.03)	(0.05)	(0.06)	(0.04)	(0.03)	(0.02)
18 months after	0.07	0.02	0.05	0.01		0.12	-0.01	0.04	0.04	
	(0.07)	(0.08)	(0.05)	(0.04)		(0.05)	(0.06)	(0.04)	(0.03)	
E. 21-25 year olds										
6 months after	-0.09	0.00	0.03	0.00	0.00	-0.02	0.02	0.01	-0.01	0.00
	(0.09)	(0.08)	(0.06)	(0.04)	(0.05)	(0.07)	(0.06)	(0.05)	(0.03)	(0.04)
12 moths after	-0.02	0.05	0.05	-0.01	0.05	0.05	0.07	0.03	-0.02	0.05
	(0.09)	(0.08)	(0.06)	(0.04)	(0.05)	(0.06)	(0.06)	(0.04)	(0.03)	(0.04)
18 months after	0.05	0.03	0.02	0.03		0.13	0.05	0.00	0.02	
	(0.09)	(0.09)	(0.06)	(0.04)		(0.07)	(0.07)	(0.04)	(0.03)	

Source: PROJoven Evaluation Data.

The table reports difference-in-difference (DID) and cross-section (CS) versions of propensity score matching. The working sample corresponds to panel observations from the evaluation data. We used the Epanechnikov kernel to compute weighting functions to estimate the counterfactuals. The common support condition was imposed using the procedure proposed by Heckman, Ichimura and Todd (1997, 1998) and Heckman, Ichimura, Smith and Todd (1998), with a trimming rule of 5 percent. Standard errors computed using the bootstrap method based on 200 replications.

Table 5. Estimated treatment effects of PROJoven on paid jobs probabilities

	Difference-in-Difference					Cross-Section				
	First	Second	Fourth	Sixth	Eighth	First	Second	Fourth	Sixth	Eighth
A. Overall Sample										
6 months after	0.17	0.09	0.05	0.08	0.06	0.04	0.08	0.03	0.04	0.05
	(0.05)	(0.06)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)	(0.02)	(0.02)
12 moths after	0.19	0.14	0.06	0.03	0.06	0.06	0.13	0.04	-0.01	0.05
	(0.06)	(0.06)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)	(0.02)	(0.02)
18 months after	0.20	0.01	0.03	0.08		0.07	0.00	0.01	0.04	
	(0.06)	(0.06)	(0.04)	(0.03)		(0.04)	(0.05)	(0.03)	(0.02)	
B. Males										
6 months after	0.12	-0.12	-0.05	0.03	-0.03	-0.01	-0.08	-0.09	0.00	-0.02
	(0.09)	(0.09)	(0.06)	(0.04)	(0.04)	(0.06)	(0.07)	(0.04)	(0.03)	(0.03)
12 moths after	0.12	0.01	-0.01	-0.02	0.00	-0.01	0.05	-0.05	-0.06	0.01
	(0.08)	(0.09)	(0.06)	(0.04)	(0.04)	(0.06)	(0.06)	(0.04)	(0.03)	(0.03)
18 months after	0.13	-0.08	0.00	-0.01		-0.01	-0.04	-0.04	-0.04	
	(0.09)	(0.09)	(0.05)	(0.04)		(0.06)	(0.06)	(0.04)	(0.02)	
C. Females										
6 months after	0.22	0.25	0.14	0.12	0.14	0.08	0.21	0.12	0.08	0.12
	(0.07)	(0.08)	(0.05)	(0.04)	(0.04)	(0.05)	(0.06)	(0.04)	(0.03)	(0.03)
12 moths after	0.27	0.22	0.11	0.08	0.11	0.13	0.18	0.10	0.03	0.09
	(0.07)	(0.08)	(0.06)	(0.04)	(0.04)	(0.05)	(0.06)	(0.04)	(0.03)	(0.03)
18 months after	0.28	0.09	0.06	0.15		0.13	0.04	0.04	0.11	
	(0.07)	(0.07)	(0.05)	(0.04)		(0.05)	(0.05)	(0.04)	(0.03)	
D. 16-20 year olds										
6 months after	0.23	0.15	0.05	0.08	0.08	0.08	0.13	0.03	0.06	0.07
	(0.07)	(0.08)	(0.05)	(0.03)	(0.03)	(0.05)	(0.06)	(0.04)	(0.02)	(0.03)
12 moths after	0.22	0.16	0.05	0.02	0.06	0.06	0.13	0.03	0.01	0.06
	(0.07)	(0.08)	(0.05)	(0.04)	(0.03)	(0.05)	(0.06)	(0.04)	(0.03)	(0.02)
18 months after	0.22	0.00	0.04	0.07		0.07	-0.03	0.02	0.05	
	(0.07)	(0.09)	(0.05)	(0.04)		(0.05)	(0.06)	(0.04)	(0.03)	
E. 21-25 year olds										
6 months after	0.02	0.07	0.08	0.07	0.01	-0.02	0.05	0.02	-0.01	0.00
	(0.09)	(0.08)	(0.07)	(0.04)	(0.05)	(0.07)	(0.06)	(0.05)	(0.03)	(0.04)
12 moths after	0.11	0.11	0.10	0.04	0.06	0.07	0.09	0.03	-0.03	0.06
	(0.09)	(0.09)	(0.06)	(0.04)	(0.05)	(0.06)	(0.06)	(0.05)	(0.03)	(0.04)
18 months after	0.14	0.06	0.06	0.08		0.10	0.05	0.00	0.01	
	(0.09)	(0.09)	(0.06)	(0.04)		(0.07)	(0.07)	(0.04)	(0.03)	

Source: PROJoven Evaluation Data.
See notes to Table 4 for further details.

Table 6. Estimated treatment effects of PROJoven on formal employment probabilities

	Difference-in-Difference					Cross-Section				
	First	Second	Fourth	Sixth	Eighth	First	Second	Fourth	Sixth	Eighth
A. Overall Sample										
6 months after	0.18	0.18	0.12	0.14	0.09	0.16	0.16	0.12	0.11	0.07
	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
12 moths after	0.15	0.19	0.08	0.11	0.11	0.12	0.17	0.08	0.07	0.09
	(0.04)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)
18 months after	0.13	0.12	0.11	0.11		0.11	0.10	0.11	0.08	
	(0.03)	(0.03)	(0.03)	(0.02)		(0.03)	(0.03)	(0.03)	(0.02)	
B. Males										
6 months after	0.13	0.16	0.13	0.09	0.05	0.11	0.12	0.12	0.08	0.05
	(0.06)	(0.05)	(0.04)	(0.03)	(0.03)	(0.05)	(0.05)	(0.04)	(0.03)	(0.02)
12 moths after	0.07	0.18	0.09	0.06	0.08	0.05	0.13	0.08	0.05	0.08
	(0.06)	(0.05)	(0.04)	(0.04)	(0.03)	(0.05)	(0.05)	(0.04)	(0.03)	(0.03)
18 months after	0.09	0.11	0.13	0.06		0.07	0.07	0.12	0.05	
	(0.06)	(0.06)	(0.04)	(0.04)		(0.05)	(0.05)	(0.04)	(0.03)	
C. Females										
6 months after	0.21	0.20	0.11	0.19	0.12	0.20	0.19	0.12	0.14	0.09
	(0.04)	(0.04)	(0.03)	(0.03)	(0.02)	(0.03)	(0.04)	(0.03)	(0.02)	(0.02)
12 moths after	0.20	0.20	0.08	0.15	0.13	0.19	0.19	0.09	0.10	0.10
	(0.04)	(0.04)	(0.03)	(0.03)	(0.02)	(0.04)	(0.03)	(0.03)	(0.02)	(0.02)
18 months after	0.17	0.13	0.09	0.16		0.15	0.13	0.10	0.11	
	(0.03)	(0.04)	(0.03)	(0.03)		(0.03)	(0.04)	(0.03)	(0.02)	
D. 16-20 year olds										
6 months after	0.20	0.24	0.11	0.12	0.10	0.18	0.22	0.11	0.09	0.08
	(0.04)	(0.04)	(0.03)	(0.03)	(0.02)	(0.03)	(0.04)	(0.03)	(0.02)	(0.02)
12 moths after	0.16	0.19	0.09	0.10	0.11	0.13	0.18	0.09	0.07	0.09
	(0.05)	(0.04)	(0.04)	(0.03)	(0.02)	(0.04)	(0.04)	(0.04)	(0.02)	(0.02)
18 months after	0.12	0.14	0.10	0.09		0.09	0.13	0.10	0.06	
	(0.04)	(0.05)	(0.03)	(0.03)		(0.03)	(0.05)	(0.03)	(0.03)	
E. 21-25 year olds										
6 months after	0.16	0.12	0.14	0.17	0.08	0.15	0.08	0.13	0.14	0.07
	(0.06)	(0.05)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.04)	(0.03)	(0.03)
12 moths after	0.13	0.20	0.05	0.13	0.09	0.11	0.17	0.05	0.10	0.08
	(0.06)	(0.06)	(0.04)	(0.04)	(0.05)	(0.05)	(0.05)	(0.04)	(0.03)	(0.04)
18 months after	0.16	0.12	0.14	0.15		0.14	0.08	0.13	0.11	
	(0.05)	(0.05)	(0.04)	(0.04)		(0.05)	(0.04)	(0.04)	(0.03)	

Source: PROJoven Evaluation Data.
See notes to Table 4 for further details.

Table 7. Estimated treatment effects of PROJoven on real monthly earnings

	Difference-in-Difference					Cross-Section				
	First	Second	Fourth	Sixth	Eighth	First	Second	Fourth	Sixth	Eighth
A. Overall Sample										
6 moths after	181.2	87.0	59.1	66.4	93.5	90.1	63.7	31.5	-10.9	27.4
	(31.1)	(26.9)	(17.2)	(23.0)	(11.9)	(19.7)	(21.0)	(14.1)	(19.8)	(8.9)
% effect	*60.3*	*35.9*	*28.0*	*21.0*	*39.2*	*30.3*	*20.1*	*10.2*	*-2.6*	*10.3*
12 months after	132.8	119.3	46.7	95.4	104.4	41.8	96.1	19.1	18.0	38.3
	(28.6)	(31.3)	(18.9)	(17.1)	(12.5)	(16.7)	(27.2)	(15.8)	(11.7)	(10.0)
% effect	*44.2*	*49.2*	*22.1*	*30.2*	*43.8*	*11.8*	*29.2*	*5.7*	*6.2*	*12.6*
18 months after	116.1	85.6	96.4	99.2		25.0	62.4	68.8	21.8	
	(35.2)	(36.4)	(19.9)	(16.0)		(27.7)	(33.3)	(17.3)	(10.6)	
% effect	*38.6*	*35.3*	*45.7*	*31.4*		*6.8*	*17.2*	*23.0*	*7.4*	
B. Males										
6 moths after	182.1	90.5	28.6	50.1	79.2	60.7	62.8	-6.4	-34.4	8.6
	(35.5)	(43.1)	(24.5)	(28.0)	(17.7)	(25.9)	(36.4)	(20.4)	(23.6)	(12.1)
% effect	*52.9*	*32.8*	*11.8*	*14.0*	*29.0*	*18.0*	*17.8*	*-1.8*	*-7.4*	*2.8*
12 months after	122.8	105.1	23.7	95.9	106.4	1.4	77.4	-11.3	11.4	35.8
	(35.7)	(47.6)	(26.2)	(21.7)	(18.3)	(26.7)	(42.6)	(22.7)	(15.2)	(14.3)
% effect	*35.7*	*38.0*	*9.8*	*26.8*	*39.0*	*0.3*	*19.3*	*-2.9*	*3.5*	*10.3*
18 months after	124.7	61.4	65.4	100.0		3.3	33.7	30.4	15.5	
	(37.3)	(57.8)	(23.5)	(20.4)		(28.8)	(53.6)	(19.6)	(14.1)	
% effect	*36.2*	*22.2*	*27.1*	*28.0*		*0.8*	*7.7*	*9.0*	*4.7*	
C. Females										
6 moths after	176.8	113.2	93.6	90.9	119.0	126.1	72.0	74.1	28.2	51.9
	(51.8)	(31.7)	(24.7)	(34.8)	(14.7)	(29.6)	(24.6)	(20.0)	(30.1)	(10.9)
% effect	*73.8*	*53.9*	*52.9*	*35.5*	*60.0*	*51.4*	*26.8*	*29.8*	*8.3*	*24.2*
12 months after	146.7	169.0	83.8	97.1	121.0	96.0	127.8	64.3	34.4	53.9
	(50.1)	(35.1)	(24.8)	(23.4)	(18.2)	(23.9)	(29.2)	(21.1)	(16.6)	(15.2)
% effect	*61.2*	*80.4*	*47.4*	*38.0*	*61.0*	*34.4*	*53.4*	*23.9*	*14.6*	*22.2*
18 months after	127.8	140.5	136.4	110.0		77.1	99.3	116.9	47.3	
	(57.8)	(42.3)	(35.2)	(25.3)		(36.8)	(37.7)	(31.8)	(18.7)	
% effect	*53.3*	*66.9*	*77.1*	*43.0*		*27.3*	*35.4*	*46.6*	*20.0*	
D. 16-20 year olds										
6 moths after	194.1	82.3	50.6	87.7	105.8	121.8	70.0	37.8	22.4	40.9
	(36.2)	(31.4)	(23.7)	(27.0)	(14.3)	(25.0)	(24.1)	(19.7)	(23.3)	(9.8)
% effect	*71.6*	*34.0*	*27.7*	*31.9*	*46.3*	*45.1*	*23.9*	*12.6*	*5.9*	*16.2*
12 months after	117.9	96.8	38.3	100.0	118.9	45.6	84.5	25.5	34.7	54.0
	(30.6)	(38.8)	(24.4)	(19.4)	(16.2)	(18.0)	(33.1)	(20.3)	(14.0)	(12.3)
% effect	*43.5*	*40.0*	*20.9*	*36.3*	*52.0*	*14.0*	*25.0*	*7.8*	*13.1*	*18.8*
18 months after	104.0	20.5	86.9	79.2		31.7	8.2	74.1	13.9	
	(36.4)	(48.6)	(31.0)	(19.2)		(25.7)	(42.2)	(28.3)	(13.3)	
% effect	*38.3*	*8.5*	*47.5*	*28.8*		*9.9*	*2.1*	*24.7*	*4.9*	
E. 21-25 year olds										
6 moths after	162.4	87.0	69.4	28.6	65.9	40.5	47.1	26.9	-57.6	-3.8
	(52.0)	(43.9)	(28.2)	(34.0)	(22.4)	(37.0)	(36.9)	(24.7)	(28.4)	(16.4)
% effect	*48.6*	*35.9*	*28.3*	*7.8*	*25.3*	*12.2*	*13.7*	*8.2*	*-12.2*	*-1.3*
12 months after	168.2	133.7	55.2	84.4	70.6	46.3	93.7	12.7	-1.9	0.9
	(55.2)	(48.6)	(28.1)	(26.7)	(28.3)	(37.9)	(42.4)	(23.9)	(17.6)	(24.5)
% effect	*50.3*	*55.1*	*22.5*	*23.0*	*27.1*	*11.8*	*29.4*	*3.6*	*-0.6*	*0.3*
18 months after	151.4	150.8	111.6	125.7		29.5	110.8	69.0	39.4	
	(62.1)	(57.8)	(25.5)	(25.2)		(48.5)	(52.7)	(20.8)	(17.3)	
% effect	*45.3*	*62.1*	*45.6*	*34.2*		*7.0*	*33.4*	*23.2*	*12.5*	

Source: PROJoven Evaluation Data.
Real earning in *Nuevos Soles* of 2001. See notes to Table 4 for further details.

Table 8. Estimated treatment effects of PROJoven on censored real monthly earnings

	Difference-in-Difference					Cross-Section				
	First	Second	Fourth	Sixth	Eighth	First	Second	Fourth	Sixth	Eighth
A. Overall Sample										
6 moths after	115.9	77.5	49.0	59.5	69.7	49.2	63.2	28.4	3.1	30.0
	(21.0)	(22.1)	(14.9)	(14.8)	(9.4)	(17.3)	(18.6)	(13.0)	(12.9)	(7.7)
% effect	*88.2*	*74.9*	*44.5*	*33.0*	*57.3*	*33.2*	*39.9*	*16.1*	*1.2*	*20.4*
12 months after	115.6	115.7	49.6	59.5	79.7	49.0	101.4	29.0	3.1	40.0
	(22.5)	(25.2)	(14.9)	(12.9)	(10.6)	(18.3)	(22.6)	(12.6)	(9.5)	(9.1)
% effect	*88.0*	*111.8*	*45.1*	*33.0*	*65.5*	*27.7*	*66.5*	*14.7*	*1.6*	*24.2*
18 months after	106.5	55.1	70.4	80.1		39.9	40.8	49.8	23.7	
	(23.3)	(26.3)	(16.0)	(13.1)		(19.4)	(23.5)	(14.2)	(10.1)	
% effect	*81.0*	*53.2*	*64.0*	*44.4*		*26.6*	*24.8*	*26.3*	*13.0*	
B. Males										
6 moths after	135.2	8.8	8.8	30.7	48.0	33.1	10.4	-23.0	-33.0	1.5
	(34.4)	(38.4)	(24.0)	(25.6)	(14.8)	(27.4)	(32.1)	(20.7)	(21.6)	(11.6)
% effect	*67.1*	*6.5*	*5.8*	*12.7*	*29.0*	*15.1*	*4.4*	*-8.8*	*-9.8*	*0.7*
12 months after	107.1	66.5	14.3	50.6	70.1	5.1	68.1	-17.5	-13.1	23.7
	(35.7)	(46.3)	(24.7)	(20.0)	(16.7)	(29.4)	(41.5)	(21.0)	(14.9)	(13.9)
% effect	*53.2*	*49.0*	*9.4*	*20.9*	*42.4*	*1.9*	*28.1*	*-6.1*	*-5.2*	*9.8*
18 months after	103.9	-1.7	40.4	57.5		1.8	-0.1	8.5	-6.2	
	(35.7)	(44.0)	(24.6)	(18.5)		(30.0)	(39.3)	(21.0)	(11.9)	
% effect	*51.6*	*-1.3*	*26.6*	*23.7*		*0.8*	*0.0*	*3.2*	*-2.4*	
C. Females										
6 moths after	106.9	131.8	81.1	83.8	87.9	67.9	104.4	67.3	39.2	53.4
	(25.4)	(23.8)	(17.2)	(20.9)	(10.4)	(21.1)	(20.8)	(14.9)	(18.5)	(8.9)
% effect	*137.7*	*165.2*	*104.8*	*70.5*	*103.1*	*72.8*	*104.2*	*61.0*	*23.9*	*58.9*
12 months after	130.2	149.3	81.1	66.3	88.3	91.2	121.8	67.3	21.7	53.8
	(25.2)	(25.8)	(20.8)	(15.4)	(11.0)	(20.7)	(22.4)	(18.6)	(11.7)	(9.3)
% effect	*167.7*	*187.1*	*104.8*	*55.8*	*103.6*	*84.6*	*141.1*	*53.6*	*17.3*	*53.5*
18 months after	113.0	99.2	94.2	101.3		74.1	71.8	80.4	56.7	
	(25.1)	(25.1)	(25.0)	(17.0)		(20.9)	(22.5)	(23.5)	(13.8)	
% effect	*145.6*	*124.4*	*121.8*	*85.3*		*94.2*	*69.7*	*62.5*	*51.7*	
D. 16-20 year olds										
6 moths after	140.8	102.8	34.9	71.6	77.8	82.3	85.1	23.3	29.9	42.7
	(25.5)	(28.3)	(19.7)	(19.7)	(10.9)	(20.9)	(22.9)	(17.6)	(17.4)	(9.0)
% effect	*130.1*	*104.5*	*41.0*	*52.3*	*73.1*	*64.8*	*61.5*	*13.8*	*14.1*	*33.3*
12 months after	105.7	105.1	39.4	57.1	85.1	47.2	87.3	27.8	15.4	50.0
	(26.1)	(32.8)	(19.8)	(13.7)	(11.7)	(22.2)	(29.4)	(18.0)	(10.3)	(9.9)
% effect	*97.7*	*106.8*	*46.2*	*41.7*	*79.9*	*30.1*	*58.9*	*15.1*	*9.7*	*33.8*
18 months after	99.4	30.5	62.2	61.8		40.9	12.8	50.6	20.1	
	(23.4)	(36.4)	(23.0)	(14.7)		(18.3)	(32.6)	(21.1)	(11.9)	
% effect	*91.9*	*31.0*	*73.0*	*45.1*		*34.3*	*6.9*	*27.9*	*12.3*	
E. 21-25 year olds										
6 moths after	70.9	70.9	69.7	42.0	49.1	3.4	51.6	31.6	-33.0	-3.1
	(37.1)	(35.9)	(24.4)	(28.3)	(19.5)	(30.0)	(31.5)	(20.9)	(23.5)	(15.5)
% effect	*43.1*	*64.4*	*46.9*	*16.3*	*29.1*	*1.9*	*28.0*	*16.7*	*-10.3*	*-1.5*
12 months after	133.2	118.3	63.6	64.1	69.4	65.7	99.0	25.5	-10.9	17.2
	(38.2)	(39.1)	(25.6)	(23.4)	(21.9)	(31.1)	(34.4)	(22.2)	(17.2)	(18.7)
% effect	*80.9*	*107.4*	*42.8*	*24.8*	*41.2*	*31.9*	*62.7*	*11.7*	*-4.5*	*7.9*
18 months after	122.5	97.0	88.8	106.4		55.0	77.7	50.7	31.4	
	(43.3)	(39.1)	(24.3)	(22.3)		(36.5)	(35.3)	(20.5)	(16.5)	
% effect	*74.4*	*88.1*	*59.8*	*41.2*		*28.5*	*57.0*	*24.9*	*14.6*	

Source: PROJoven Evaluation Data.
Real earning in *Nuevos Soles* of 2001. See notes to Table 4 for further details.

Table 9. Estimated treatment effects of PROJoven on real hourly earnings

	Difference-in-Difference					Cross-Section				
	First	Second	Fourth	Sixth	Eighth	First	Second	Fourth	Sixth	Eighth
A. Overall Sample										
6 moths after	0.7	0.4	0.2	0.3	0.3	0.4	0.2	0.2	0.0	0.1
	(0.2)	(0.2)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.0)
% effect	*37.4*	*28.3*	*19.2*	*17.7*	*22.3*	*26.8*	*10.5*	*12.2*	*-2.1*	*4.6*
12 months after	0.5	0.7	0.2	0.4	0.4	0.3	0.5	0.1	0.0	0.1
	(0.2)	(0.2)	(0.1)	(0.1)	(0.1)	(0.1)	(0.2)	(0.1)	(0.1)	(0.0)
% effect	*30.1*	*48.5*	*14.8*	*21.7*	*27.9*	*16.1*	*32.2*	*7.5*	*2.1*	*9.6*
18 months after	0.4	0.5	0.3	0.5		0.1	0.3	0.3	0.1	
	(0.2)	(0.2)	(0.1)	(0.1)		(0.1)	(0.2)	(0.1)	(0.1)	
% effect	*22.9*	*34.0*	*27.3*	*26.8*		*8.1*	*14.8*	*18.2*	*8.5*	
B. Males										
6 moths after	0.9	0.5	0.2	0.3	0.3	0.4	0.2	0.1	-0.1	0.0
	(0.2)	(0.3)	(0.1)	(0.1)	(0.1)	(0.1)	(0.2)	(0.1)	(0.1)	(0.1)
% effect	*46.7*	*34.9*	*16.1*	*14.5*	*21.7*	*25.1*	*10.9*	*9.0*	*-5.8*	*2.1*
12 months after	0.6	0.9	0.0	0.5	0.4	0.1	0.5	0.0	0.1	0.1
	(0.2)	(0.3)	(0.1)	(0.1)	(0.1)	(0.1)	(0.3)	(0.1)	(0.1)	(0.1)
% effect	*31.4*	*54.9*	*2.1*	*25.4*	*28.0*	*5.2*	*31.5*	*-2.0*	*5.8*	*7.9*
18 months after	0.6	0.5	0.1	0.6		0.1	0.1	0.1	0.2	
	(0.2)	(0.3)	(0.1)	(0.1)		(0.1)	(0.3)	(0.1)	(0.1)	
% effect	*30.0*	*29.1*	*10.5*	*31.6*		*3.8*	*5.1*	*4.3*	*14.2*	
C. Females										
6 moths after	0.3	0.4	0.3	0.3	0.3	0.4	0.2	0.2	0.1	0.1
	(0.4)	(0.2)	(0.2)	(0.2)	(0.1)	(0.1)	(0.1)	(0.1)	(0.2)	(0.1)
% effect	*23.0*	*30.0*	*27.5*	*21.8*	*26.3*	*31.3*	*13.6*	*18.0*	*4.7*	*9.1*
12 months after	0.4	0.7	0.4	0.2	0.4	0.4	0.5	0.3	0.0	0.2
	(0.5)	(0.2)	(0.2)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)
% effect	*26.7*	*51.7*	*35.9*	*16.1*	*33.4*	*32.6*	*38.8*	*22.7*	*-0.2*	*15.9*
18 months after	0.2	0.7	0.5	0.3		0.3	0.5	0.4	0.0	
	(0.5)	(0.2)	(0.2)	(0.1)		(0.2)	(0.2)	(0.2)	(0.1)	
% effect	*14.3*	*51.1*	*50.1*	*19.7*		*19.0*	*35.8*	*36.4*	*4.1*	
D. 16-20 year olds										
6 moths after	0.7	0.6	0.3	0.4	0.4	0.4	0.3	0.3	0.1	0.1
	(0.2)	(0.2)	(0.1)	(0.2)	(0.1)	(0.1)	(0.2)	(0.1)	(0.1)	(0.0)
% effect	*43.6*	*38.2*	*28.7*	*28.2*	*29.3*	*31.0*	*20.3*	*21.2*	*5.9*	*11.3*
12 months after	0.4	0.6	0.3	0.5	0.4	0.1	0.3	0.2	0.1	0.2
	(0.2)	(0.2)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)
% effect	*25.4*	*40.8*	*26.6*	*28.8*	*32.8*	*9.3*	*23.1*	*17.1*	*9.2*	*13.5*
18 months after	0.4	0.3	0.3	0.4		0.1	0.0	0.3	0.1	
	(0.2)	(0.3)	(0.2)	(0.1)		(0.1)	(0.2)	(0.1)	(0.1)	
% effect	*24.1*	*20.2*	*30.4*	*26.2*		*8.4*	*1.1*	*20.2*	*5.6*	
E. 21-25 year olds										
6 moths after	0.7	0.3	0.1	0.1	0.1	0.4	0.0	0.0	-0.2	-0.1
	(0.4)	(0.2)	(0.1)	(0.2)	(0.1)	(0.2)	(0.2)	(0.1)	(0.1)	(0.1)
% effect	*36.5*	*17.8*	*9.6*	*4.4*	*6.6*	*24.0*	*2.2*	*3.2*	*-10.8*	*-8.6*
12 months after	0.8	0.8	0.0	0.2	0.2	0.5	0.6	0.0	-0.1	0.0
	(0.5)	(0.3)	(0.1)	(0.1)	(0.2)	(0.2)	(0.3)	(0.1)	(0.1)	(0.1)
% effect	*41.6*	*56.3*	*3.0*	*13.3*	*16.9*	*27.5*	*39.7*	*-1.9*	*-5.3*	*1.2*
18 months after	0.6	0.6	0.3	0.5		0.2	0.4	0.3	0.2	
	(0.4)	(0.3)	(0.1)	(0.1)		(0.2)	(0.3)	(0.1)	(0.1)	
% effect	*28.4*	*40.4*	*27.7*	*28.2*		*11.5*	*21.1*	*19.3*	*14.3*	

Source: PROJoven Evaluation Data.
Real earning in *Nuevos Soles* of 2001. See notes to Table 4 for further details.

Table 10. Estimated treatment effects of PROJoven on censored real hourly earnings

	Difference-in-Difference					Cross-Section				
	First	Second	Fourth	Sixth	Eighth	First	Second	Fourth	Sixth	Eighth
A. Overall Sample										
6 moths after	0.5	0.4	0.2	0.3	0.3	0.2	0.2	0.1	0.0	0.1
	(0.1)	(0.1)	(0.1)	(0.1)	(0.0)	(0.1)	(0.1)	(0.1)	(0.1)	(0.0)
% effect	*69.1*	*58.9*	*33.7*	*29.8*	*39.4*	*30.1*	*28.9*	*18.4*	*1.9*	*15.2*
12 months after	0.6	0.6	0.2	0.3	0.3	0.3	0.5	0.2	0.0	0.2
	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.0)	(0.0)
% effect	*74.6*	*97.1*	*35.9*	*25.6*	*47.5*	*32.3*	*70.2*	*17.1*	*-2.1*	*21.7*
18 months after	0.5	0.3	0.2	0.4		0.2	0.2	0.2	0.1	
	(0.1)	(0.1)	(0.1)	(0.1)		(0.1)	(0.1)	(0.1)	(0.0)	
% effect	*65.0*	*50.3*	*42.4*	*39.1*		*28.4*	*23.1*	*21.7*	*13.9*	
B. Males										
6 moths after	0.8	0.1	0.1	0.2	0.2	0.2	0.0	0.0	-0.1	0.0
	(0.2)	(0.2)	(0.1)	(0.1)	(0.1)	(0.1)	(0.2)	(0.1)	(0.1)	(0.1)
% effect	*65.5*	*8.4*	*13.5*	*13.5*	*22.6*	*22.0*	*-2.1*	*1.6*	*-8.6*	*1.0*
12 months after	0.6	0.5	0.0	0.3	0.3	0.1	0.4	-0.1	0.0	0.1
	(0.2)	(0.2)	(0.1)	(0.1)	(0.1)	(0.1)	(0.2)	(0.1)	(0.1)	(0.1)
% effect	*54.0*	*62.0*	*1.8*	*21.2*	*31.3*	*8.2*	*39.6*	*-5.6*	*-3.0*	*8.2*
18 months after	0.6	0.1	0.1	0.4		0.1	0.0	0.0	0.1	
	(0.2)	(0.2)	(0.1)	(0.1)		(0.1)	(0.2)	(0.1)	(0.1)	
% effect	*50.5*	*10.1*	*9.0*	*29.1*		*5.5*	*-1.1*	*-1.3*	*6.4*	
C. Females										
6 moths after	0.4	0.6	0.3	0.4	0.3	0.2	0.4	0.2	0.2	0.2
	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.0)
% effect	*85.4*	*115.3*	*64.5*	*55.6*	*64.8*	*49.1*	*87.8*	*43.0*	*20.1*	*40.2*
12 months after	0.6	0.7	0.4	0.2	0.3	0.4	0.5	0.3	0.0	0.2
	(0.2)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.0)
% effect	*127.6*	*128.0*	*85.3*	*32.1*	*70.6*	*81.9*	*113.2*	*51.8*	*1.6*	*46.3*
18 months after	0.5	0.5	0.4	0.4		0.3	0.3	0.3	0.2	
	(0.1)	(0.1)	(0.1)	(0.1)		(0.1)	(0.1)	(0.1)	(0.1)	
% effect	*102.0*	*93.3*	*89.2*	*56.7*		*80.6*	*67.2*	*54.2*	*31.1*	
D. 16-20 year olds										
6 moths after	0.6	0.5	0.2	0.4	0.3	0.3	0.4	0.2	0.1	0.2
	(0.1)	(0.2)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.0)
% effect	*98.1*	*89.6*	*42.6*	*46.2*	*54.2*	*48.1*	*54.2*	*23.0*	*13.8*	*28.2*
12 months after	0.5	0.5	0.2	0.3	0.3	0.2	0.3	0.2	0.0	0.2
	(0.1)	(0.2)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.0)	(0.0)
% effect	*78.0*	*87.2*	*50.8*	*34.7*	*58.1*	*25.2*	*56.1*	*25.0*	*6.3*	*28.8*
18 months after	0.5	0.3	0.2	0.3		0.2	0.1	0.2	0.1	
	(0.1)	(0.2)	(0.1)	(0.1)		(0.1)	(0.2)	(0.1)	(0.1)	
% effect	*75.9*	*43.2*	*53.8*	*40.9*		*31.9*	*9.8*	*25.4*	*12.8*	
E. 21-25 year olds										
6 moths after	0.3	0.2	0.2	0.2	0.1	0.1	0.1	0.1	-0.1	-0.1
	(0.2)	(0.2)	(0.1)	(0.1)	(0.1)	(0.1)	(0.2)	(0.1)	(0.1)	(0.1)
% effect	*35.0*	*39.0*	*27.5*	*12.4*	*11.0*	*12.0*	*14.7*	*10.7*	*-9.5*	*-8.2*
12 months after	0.7	0.6	0.2	0.2	0.3	0.4	0.5	0.1	-0.1	0.1
	(0.2)	(0.2)	(0.1)	(0.1)	(0.1)	(0.2)	(0.2)	(0.1)	(0.1)	(0.1)
% effect	*69.8*	*100.8*	*22.3*	*16.0*	*28.9*	*49.5*	*74.0*	*5.2*	*-8.9*	*8.6*
18 months after	0.5	0.4	0.3	0.5		0.3	0.3	0.2	0.2	
	(0.2)	(0.2)	(0.1)	(0.1)		(0.1)	(0.2)	(0.1)	(0.1)	
% effect	*51.9*	*63.2*	*41.5*	*35.5*		*32.7*	*42.7*	*20.7*	*16.5*	

Source: PROJoven Evaluation Data.
Real earning in *Nuevos Soles* of 2001. See notes to Table 4 for further details.

Table 11. Estimated treatment effects of PROJoven on weekly hours of work

	Difference-in-Difference					Cross-Section				
	First	Second	Fourth	Sixth	Eighth	First	Second	Fourth	Sixth	Eighth
A. Overall Sample										
6 moths after	11.7	0.1	2.3	2.1	5.3	2.2	1.1	-1.4	-1.3	2.0
	(3.3)	(3.3)	(2.2)	(1.7)	(1.7)	(2.1)	(2.1)	(1.5)	(1.2)	(1.1)
% effect	*26.6*	*0.2*	*4.8*	*4.4*	*11.5*	*4.6*	*2.1*	*-2.4*	*-2.4*	*3.8*
12 months after	7.1	1.2	3.3	3.0	3.4	-2.4	2.2	-0.4	-0.4	0.0
	(2.9)	(3.3)	(2.2)	(1.5)	(1.7)	(1.9)	(2.3)	(1.5)	(1.0)	(1.1)
% effect	*16.2*	*2.9*	*6.8*	*6.5*	*7.3*	*-4.4*	*4.1*	*-0.7*	*-0.7*	*0.1*
18 months after	9.6	-0.3	4.8	2.6		0.2	0.7	1.2	-0.7	
	(2.8)	(3.3)	(2.3)	(1.6)		(1.9)	(2.3)	(1.6)	(1.1)	
% effect	*22.0*	*-0.7*	*10.0*	*5.6*		*0.3*	*1.4*	*2.2*	*-1.4*	
B. Males										
6 moths after	10.0	-2.2	-1.2	1.3	2.1	-1.9	-0.9	-5.6	-2.0	-1.2
	(3.8)	(4.4)	(2.8)	(2.0)	(2.1)	(2.4)	(3.1)	(1.9)	(1.4)	(1.4)
% effect	*21.6*	*-5.1*	*-2.3*	*2.7*	*4.5*	*-3.8*	*-1.6*	*-9.5*	*-3.6*	*-2.2*
12 months after	8.8	-1.6	3.2	0.8	3.3	-3.1	-0.2	-1.2	-2.4	0.0
	(3.7)	(4.2)	(2.7)	(1.9)	(2.2)	(2.3)	(2.8)	(1.6)	(1.3)	(1.4)
% effect	*19.0*	*-3.6*	*6.3*	*1.7*	*7.0*	*-5.5*	*-0.4*	*-2.0*	*-4.3*	*0.0*
18 months after	9.2	-1.2	4.4	0.0		-2.7	0.2	0.0	-3.3	
	(4.2)	(4.2)	(2.8)	(2.0)		(2.5)	(2.6)	(2.0)	(1.2)	
% effect	*19.8*	*-2.7*	*8.6*	*-0.1*		*-4.7*	*0.3*	*0.1*	*-5.8*	
C. Females										
6 moths after	13.0	2.8	5.2	3.0	9.5	7.1	2.7	3.1	0.0	5.2
	(4.7)	(4.6)	(3.1)	(2.7)	(2.7)	(2.7)	(2.9)	(2.1)	(1.8)	(1.7)
% effect	*32.3*	*7.1*	*11.6*	*6.6*	*21.0*	*15.6*	*5.3*	*6.0*	*0.1*	*10.5*
12 months after	4.5	5.1	3.5	4.7	5.0	-1.4	5.0	1.4	1.8	0.7
	(4.7)	(5.0)	(3.4)	(2.7)	(2.6)	(2.9)	(3.3)	(2.4)	(1.7)	(1.7)
% effect	*11.1*	*12.9*	*7.7*	*10.4*	*11.1*	*-2.8*	*10.4*	*2.6*	*3.4*	*1.3*
18 months after	11.1	0.2	4.3	6.2		5.2	0.1	2.2	3.2	
	(4.7)	(4.6)	(3.5)	(2.7)		(3.4)	(3.0)	(2.2)	(1.7)	
% effect	*27.7*	*0.4*	*9.6*	*13.6*		*11.2*	*0.2*	*4.3*	*6.4*	
D. 16-20 year olds										
6 moths after	15.3	-3.0	-3.0	0.8	5.4	6.3	0.9	-2.9	-1.3	1.8
	(4.0)	(4.4)	(2.9)	(2.1)	(1.9)	(2.3)	(2.9)	(1.9)	(1.3)	(1.3)
% effect	*35.5*	*-7.5*	*-6.6*	*1.8*	*11.8*	*14.1*	*1.7*	*-5.1*	*-2.4*	*3.3*
12 months after	9.6	-0.4	-1.3	0.6	3.9	0.6	3.4	-1.1	-1.5	0.3
	(3.8)	(4.4)	(3.3)	(2.1)	(1.9)	(2.3)	(3.1)	(2.0)	(1.3)	(1.3)
% effect	*22.3*	*-1.1*	*-2.8*	*1.3*	*8.5*	*1.1*	*6.2*	*-2.1*	*-2.7*	*0.5*
18 months after	11.5	-3.4	0.1	0.1		2.4	0.5	0.2	-1.9	
	(3.8)	(4.3)	(2.9)	(2.2)		(2.3)	(2.9)	(1.9)	(1.3)	
% effect	*26.6*	*-8.4*	*0.2*	*0.3*		*4.9*	*1.0*	*0.4*	*-3.5*	
E. 21-25 year olds										
6 moths after	7.1	4.7	8.6	3.8	5.9	-4.5	1.2	1.1	-1.1	2.3
	(4.8)	(4.7)	(3.4)	(2.6)	(3.3)	(3.1)	(3.3)	(2.2)	(1.9)	(2.3)
% effect	*16.0*	*10.8*	*16.9*	*7.9*	*12.7*	*-8.7*	*2.3*	*2.0*	*-2.1*	*4.5*
12 months after	5.1	3.8	7.7	6.3	2.6	-6.6	0.3	0.2	1.3	-1.1
	(4.8)	(4.7)	(3.5)	(2.4)	(3.1)	(3.2)	(3.0)	(2.4)	(1.7)	(2.1)
% effect	*11.4*	*8.8*	*15.2*	*12.8*	*5.5*	*-11.4*	*0.6*	*0.4*	*2.4*	*-1.9*
18 months after	10.7	5.9	9.1	6.2		-0.9	2.4	1.6	1.3	
	(4.7)	(4.8)	(3.2)	(2.3)		(3.1)	(3.4)	(2.0)	(1.4)	
% effect	*24.0*	*13.5*	*18.0*	*12.7*		*-1.6*	*4.8*	*3.1*	*2.3*	

Source: PROJoven Evaluation Data.
See notes to Table 4 for further details.

Table 12. Estimated treatment effects of PROJoven on censored weekly hours of work

	Difference-in-Difference					Cross-Section				
	First	Second	Fourth	Sixth	Eighth	First	Second	Fourth	Sixth	Eighth
A. Overall Sample										
6 moths after	3.5	3.5	3.6	3.6	6.7	0.4	3.8	1.0	0.5	3.7
	(2.9)	(3.3)	(2.5)	(1.5)	(1.6)	(2.3)	(2.4)	(1.9)	(1.2)	(1.3)
% effect	*17.2*	*15.2*	*13.8*	*12.9*	*26.9*	*1.7*	*12.6*	*2.9*	*1.5*	*12.2*
12 months after	5.6	6.8	4.5	1.6	6.1	2.6	7.1	1.9	-1.4	3.1
	(3.2)	(3.3)	(2.3)	(1.7)	(1.6)	(2.5)	(2.5)	(1.7)	(1.2)	(1.3)
% effect	*27.7*	*29.7*	*17.2*	*5.9*	*24.6*	*9.0*	*24.7*	*5.6*	*-3.9*	*9.9*
18 months after	10.1	1.5	4.4	4.4		7.0	1.8	1.7	1.3	
	(3.1)	(3.4)	(2.3)	(1.6)		(2.5)	(2.7)	(1.8)	(1.2)	
% effect	*49.9*	*6.6*	*16.7*	*15.9*		*30.1*	*7.4*	*4.9*	*3.8*	
B. Males										
6 moths after	2.3	-8.3	-3.3	-0.2	1.0	-4.5	-7.1	-6.9	-2.3	-2.1
	(4.4)	(5.0)	(3.4)	(2.3)	(2.2)	(3.4)	(3.5)	(2.8)	(1.7)	(1.7)
% effect	*8.1*	*-29.7*	*-9.9*	*-0.6*	*3.4*	*-12.6*	*-17.7*	*-15.8*	*-5.7*	*-5.2*
12 months after	4.2	0.4	1.3	-2.9	2.7	-2.6	1.6	-2.3	-5.0	-0.4
	(4.5)	(5.5)	(3.6)	(2.4)	(2.3)	(3.2)	(4.3)	(2.7)	(1.8)	(1.7)
% effect	*14.8*	*1.5*	*4.1*	*-9.1*	*9.1*	*-6.8*	*4.1*	*-5.3*	*-11.4*	*-1.0*
18 months after	6.2	-3.3	1.4	-3.2		-0.6	-2.1	-2.2	-5.2	
	(4.6)	(5.0)	(3.3)	(2.2)		(3.5)	(3.8)	(2.4)	(1.5)	
% effect	*21.5*	*-11.9*	*4.4*	*-9.8*		*-1.8*	*-7.0*	*-4.9*	*-11.8*	
C. Females										
6 moths after	4.9	13.5	9.2	7.3	11.5	4.6	12.4	7.2	3.3	8.3
	(3.7)	(4.2)	(3.0)	(2.4)	(2.1)	(2.9)	(3.3)	(2.3)	(1.8)	(1.6)
% effect	*35.9*	*71.4*	*43.6*	*32.0*	*55.1*	*23.7*	*55.4*	*29.7*	*12.6*	*36.7*
12 months after	7.5	12.0	7.4	6.1	9.0	7.2	10.8	5.4	2.2	5.8
	(3.9)	(4.4)	(3.3)	(2.4)	(2.1)	(3.2)	(3.3)	(2.5)	(1.8)	(1.6)
% effect	*54.9*	*63.0*	*35.2*	*26.9*	*43.0*	*34.2*	*50.6*	*21.1*	*7.4*	*23.9*
18 months after	13.2	6.2	6.1	11.7		12.8	5.0	4.1	7.8	
	(3.7)	(4.1)	(3.3)	(2.4)		(2.9)	(3.1)	(2.6)	(1.7)	
% effect	*96.1*	*32.8*	*29.2*	*51.7*		*87.5*	*25.7*	*14.8*	*31.3*	
D. 16-20 year olds										
6 moths after	5.9	6.4	0.1	3.3	7.9	3.6	5.5	-0.7	1.5	5.0
	(3.6)	(4.5)	(3.2)	(2.1)	(2.0)	(2.7)	(3.4)	(2.5)	(1.6)	(1.6)
% effect	*31.8*	*28.8*	*0.5*	*13.9*	*34.7*	*14.9*	*19.3*	*-2.2*	*4.9*	*17.4*
12 months after	6.0	8.0	2.2	0.2	7.0	3.7	7.1	1.4	-1.5	4.0
	(3.9)	(4.4)	(3.2)	(2.1)	(2.0)	(3.2)	(3.7)	(2.5)	(1.7)	(1.5)
% effect	*32.3*	*35.8*	*9.7*	*1.1*	*30.5*	*13.9*	*24.6*	*4.2*	*-4.4*	*13.4*
18 months after	10.2	1.3	2.5	2.4		8.0	0.3	1.7	0.7	
	(3.7)	(4.6)	(3.1)	(2.2)		(2.7)	(3.6)	(2.3)	(1.7)	
% effect	*55.5*	*5.6*	*11.1*	*10.4*		*38.9*	*1.2*	*5.0*	*2.1*	
E. 21-25 year olds										
6 moths after	-1.6	3.5	8.6	3.9	4.1	-4.537(	3.3	3.2	-0.8	0.8
	(4.6)	(4.8)	(3.8)	(2.7)	(3.1)	(3.6)	(3.6)	(3.0)	(2.0)	(2.4)
% effect	*-7.1*	*15.2*	*27.5*	*11.3*	*13.5*	*-15.411*	*10.3*	*10.1*	*-2.1*	*2.3*
12 months after	3.8	5.6	8.1	4.2	4.7	0.8	5.3	2.6	-0.5	1.4
	(4.9)	(5.0)	(3.7)	(2.6)	(3.3)	(3.7)	(3.6)	(2.9)	(1.9)	(2.5)
% effect	*16.7*	*24.1*	*25.7*	*12.0*	*15.3*	*2.7*	*18.9*	*7.7*	*-1.3*	*3.7*
18 months after	9.6	5.0	7.0	6.9		6.7	4.8	1.6	2.1	
	(4.9)	(4.9)	(3.6)	(2.5)		(3.9)	(3.8)	(2.8)	(2.0)	
% effect	*42.4*	*21.6*	*22.4*	*19.7*		*24.3*	*23.0*	*4.3*	*5.7*	

Source: PROJoven Evaluation Data.
See notes to Table 4 for further details.

Table 13. Estimation of Internal Rates of Return

	Public Call				
	First	Second	Fourth	Sixth	Eighth
A. Beneficiaries	1,505	1,807	2,671	3,651	5,157
B. Benefits[1]					
Stipends received by beneficiaries	467	439	385	419	243
Post-1 DID	116	78	49	60	70
Post-2 DID	116	116	50	60	80
Post-3 DID	107	55	70	80	80
Post-1 CS	49	63	28	0	30
Post-2 CS	49	101	29	0	40
Post-3 CS	40	41	50	0	40
C. Costs[2]					
Operative costs	2682	2670	2202	1427	1085
Stipends given to beneficiaries	467	439	385	419	243
Opportunity costs	63	89	91	124	79
D. Internal Rate of Return using DID estimates of benefits					
Pessimistic[3]	27.3	4.8	0.0	21.0	50.6
Neutral[4]	40.5	19.5	9.8	34.0	61.6
Optimistic[5]	46.4	26.1	16.8	39.8	66.6
E. Internal Rate of Return using CS estimates of benefits					
Pessimistic[3]	0	0.0	0.0	0.0	1.7
Neutral[4]	4	11.9	0.0	0.0	16.0
Optimistic[5]	11.6	18.9	4.0	0.0	22.6

All figures in real values of December 2001.

1. Benefits are those estimated in the impact evaluation. Given that there is no third follow-up survey in the evaluation data for the Eighth Public Call, we assume that the benefits estimated for the second follow-up would remain for another six months.

2. Costs are provided by PROJoven for the First, Sixth and Eighth Public Calls. Costs data for the Second Public Call were not provided, while data provided for the Fourth Public Call seem unreasonably high. For these calls, we used the unit cost from the First call.

   Operative costs. These costs include the cost of courses (payments to ECAPs) and administrative costs of the program.

   Opportunity costs. To estimate the (per capita) opportunity costs, we use data from the baseline evaluation data, these costs are equal to the real monthly earning of the treatment group. We assume that beneficiaries incur these costs for four months, three months of course duration plus one extra month prior to the courses.

3. The *pessimistic* scenario assumes that benefits decrease at a 50 percent rate per year.

4. The *neutral* scenario assumes that benefits decrease at a 25 percent rate per year.

5. The *optimistic* scenario assumes that benefits decrease at a 10 percent rate per year.

CHARTS

Chart 1. Selection of ECAPs

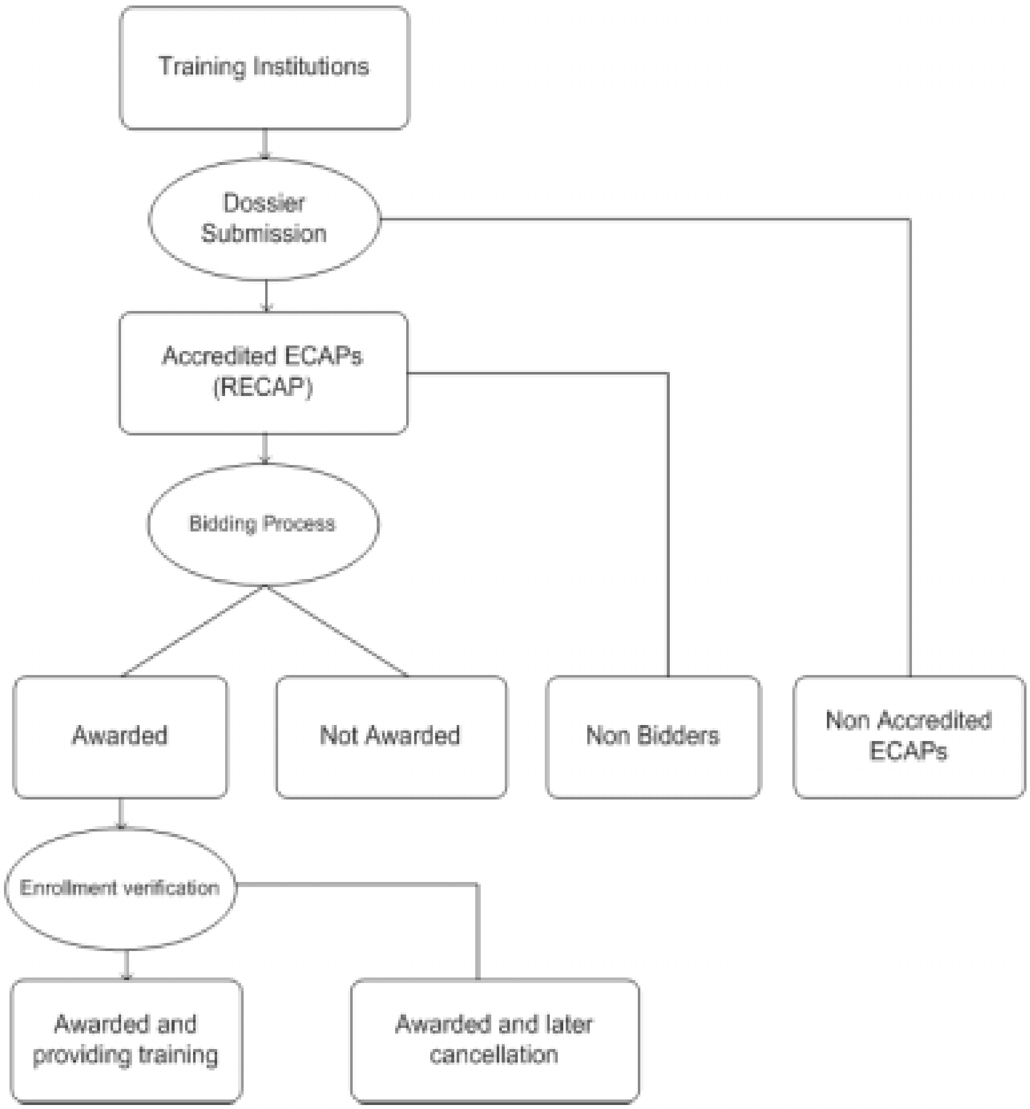
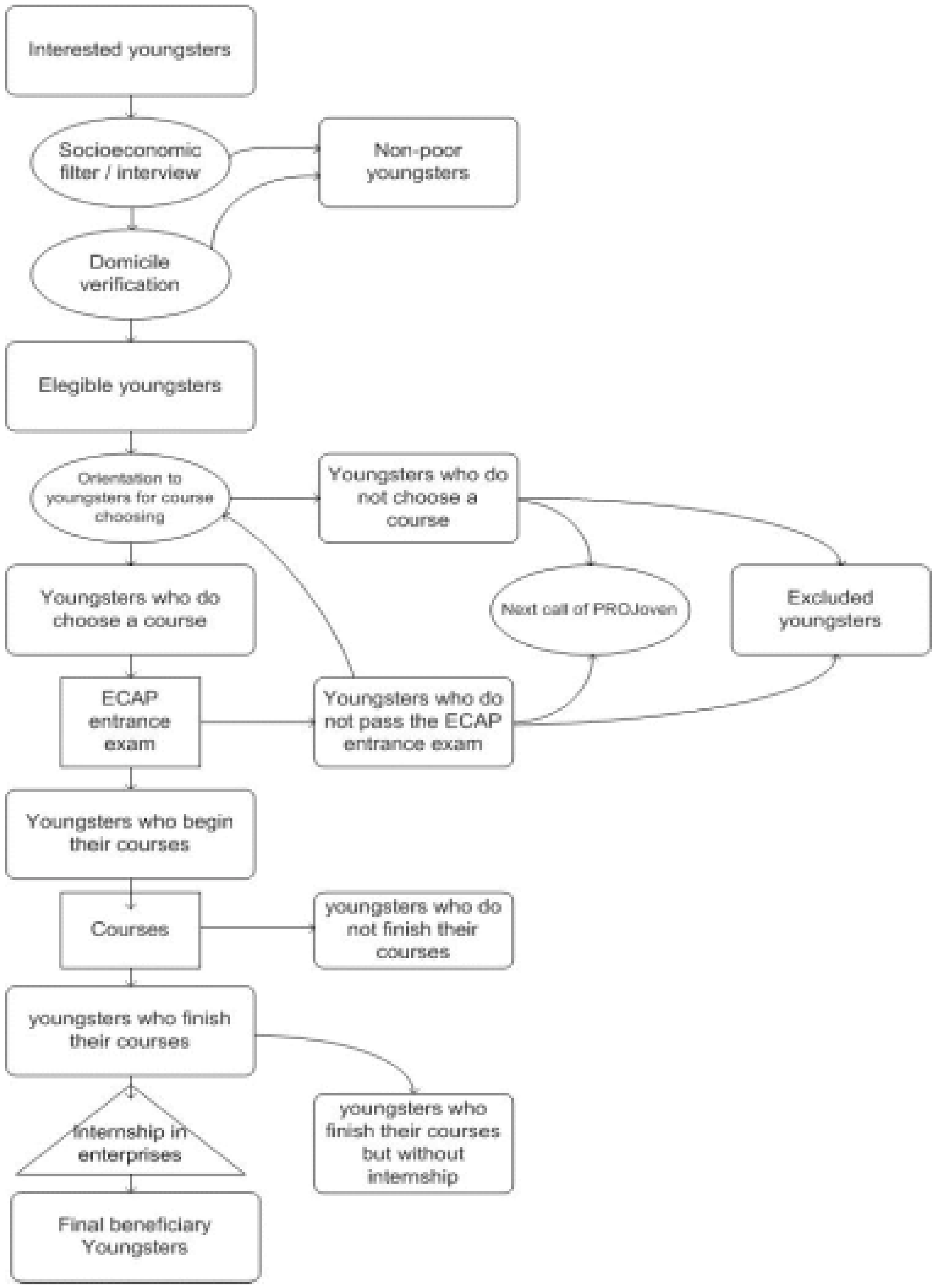


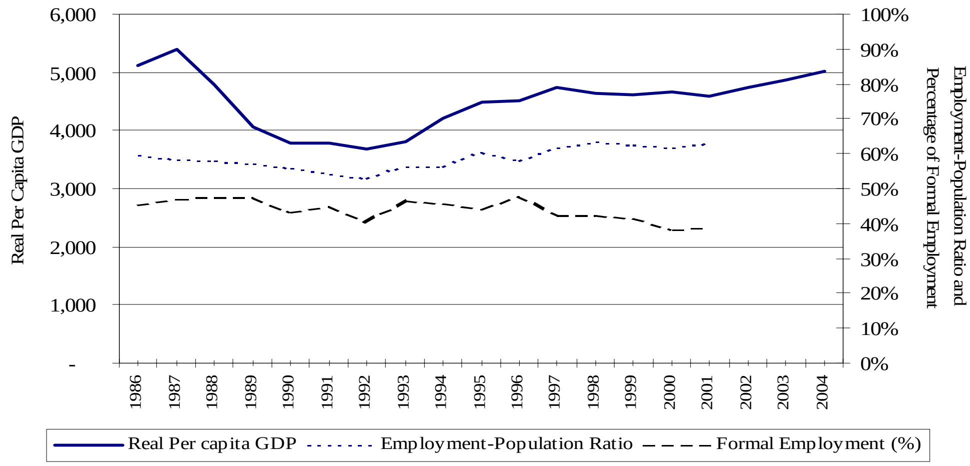
Chart 2. Selection of Eligible and Beneficiaries youngsters



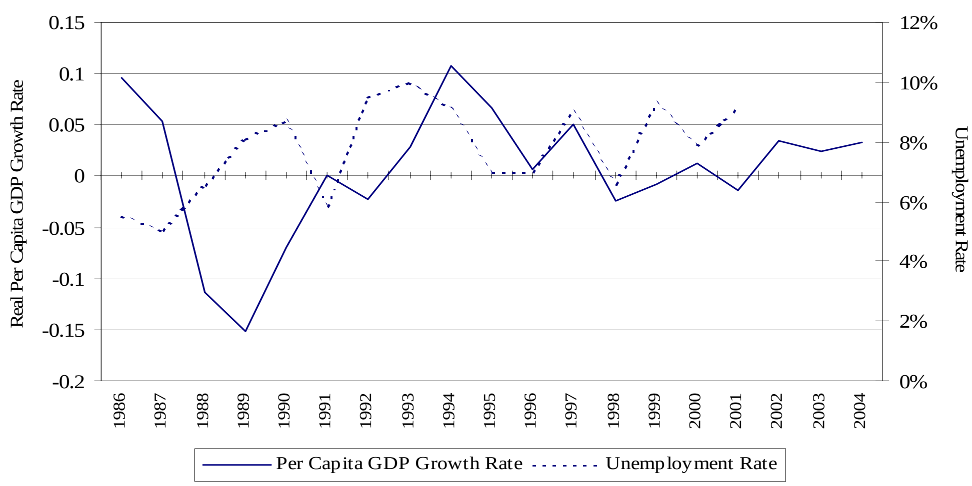
FIGURES

Figure 1. Peru: GDP, Employment and Unemployment

Per Capita GDP, Employment-Population Ratio and Formal Employment



Per Capita GDP Growth Rates and Unemployment Rates



Sources:
Central Bank (Memoria del BCR).
Household Survey, Ministry of Labor
National Household Survey, INEI
Note: Employment and unemployment figures correspond to Metropolitan Lima.

Figure 2. Metropolitan Lima: unemployment rates and real earnings 1996-2001

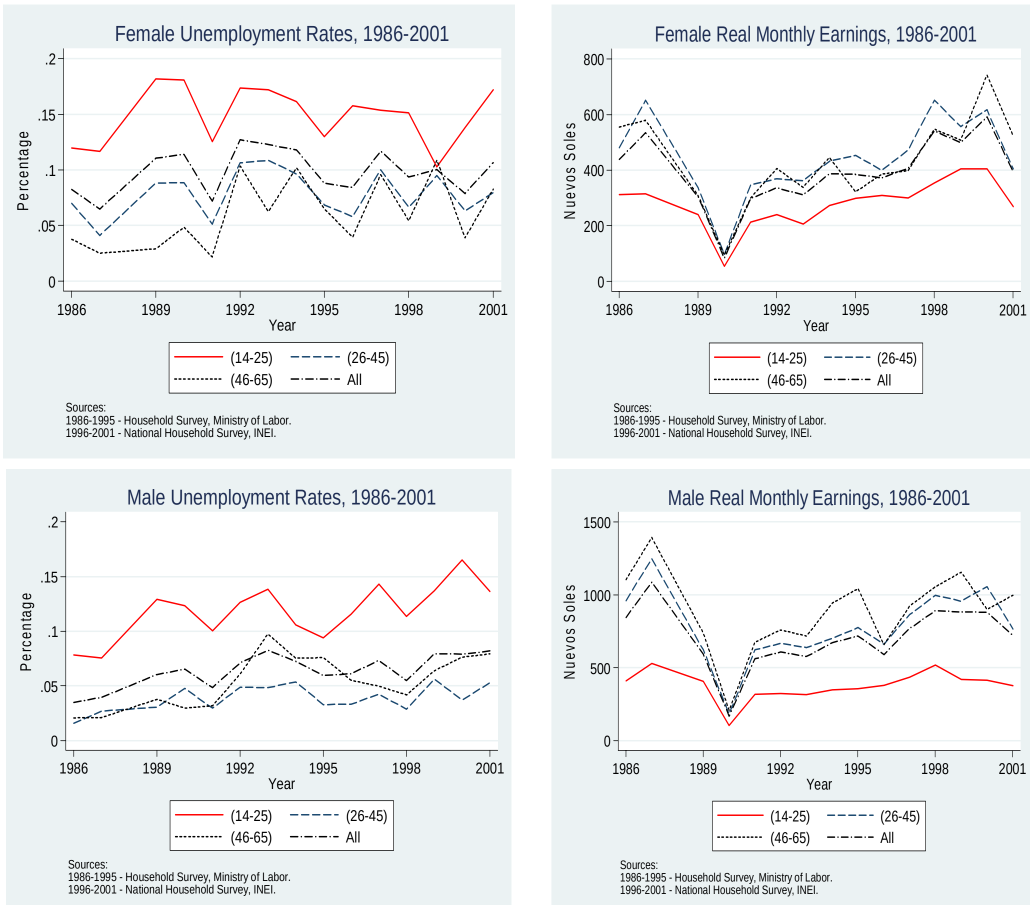


Figure 3. Employment rates among PROJoven treatment and comparison youngsters

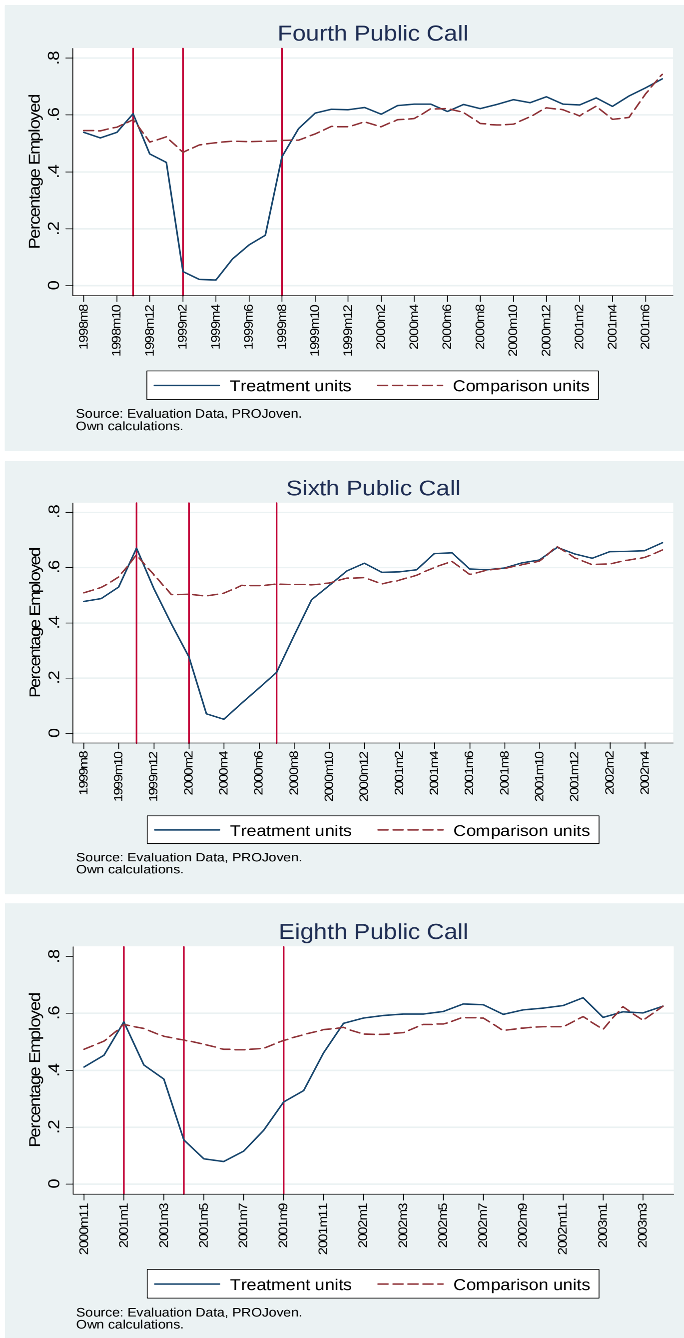


Figure 4. Outcomes: employment, paid jobs, and formal employment rates

A. Employment | B. Paid Jobs | C. Formal Jobs

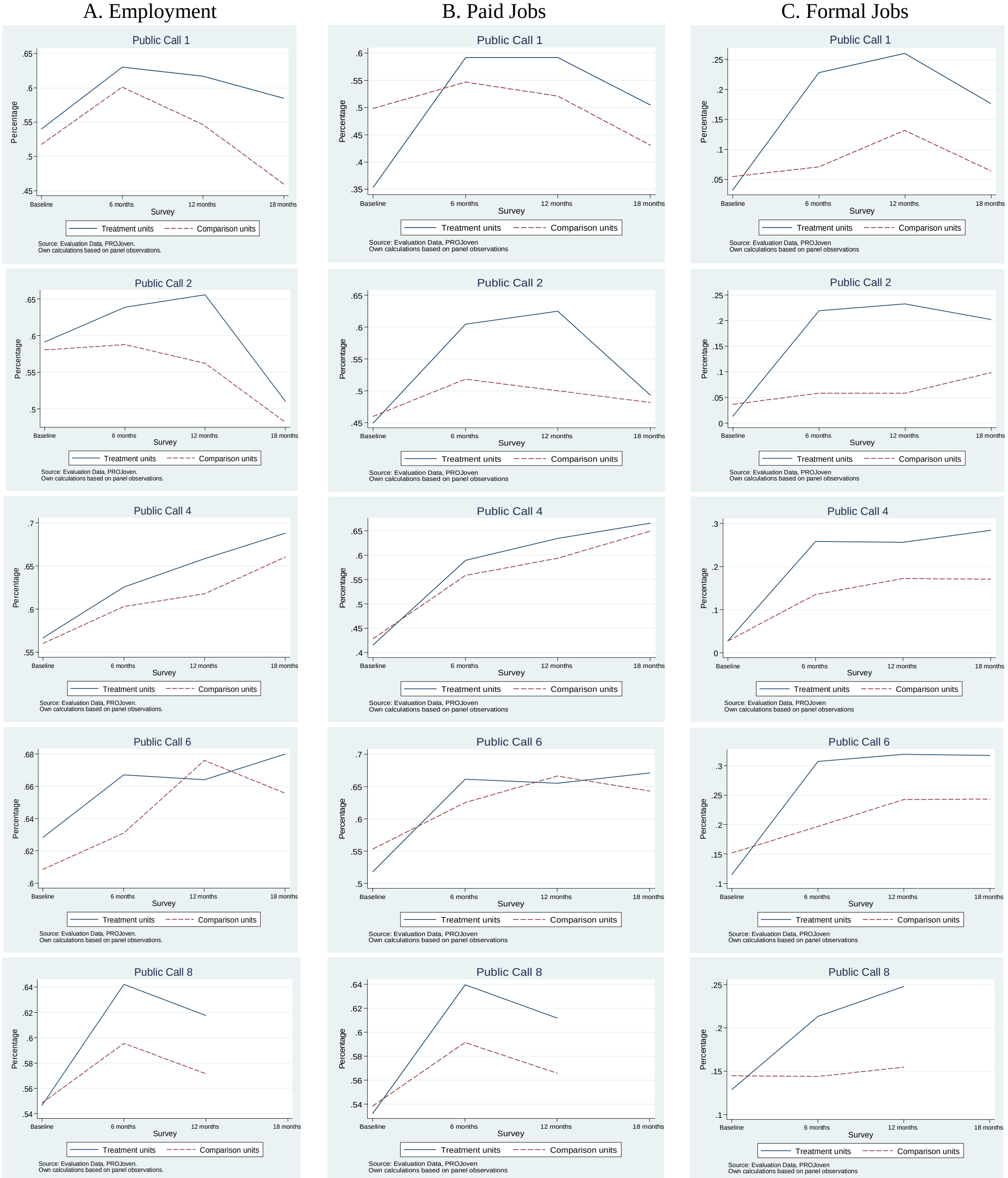


Figure 5. Outcomes: real earnings and weekly working hours in the main job

A. Monthly earnings B. Hourly earnings C. Weekly working hours

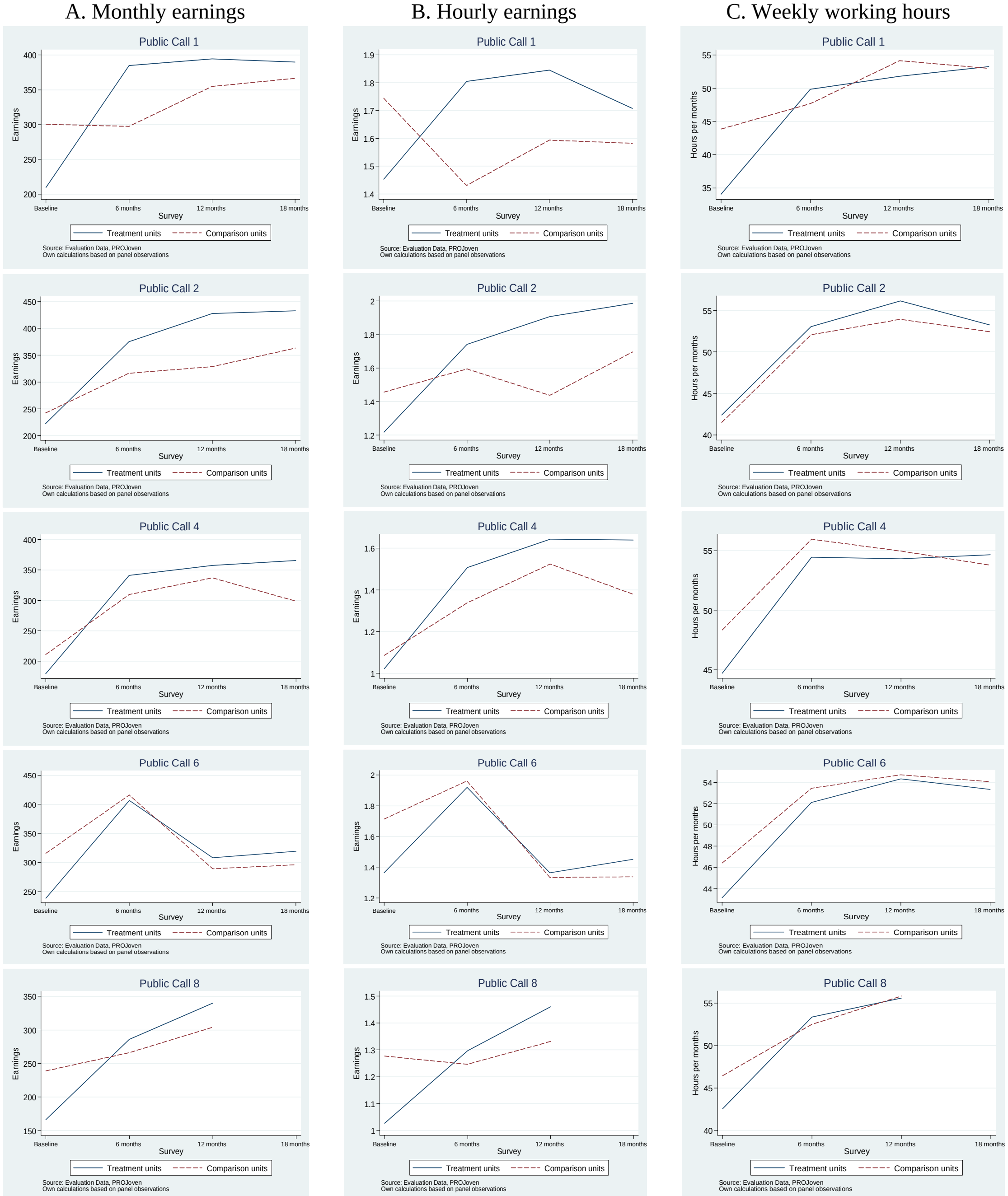


Figure 6. Outcomes: censored real earnings and weekly working hours in the main job

A. Monthly earnings | B. Hourly earnings | C. Weekly working hours

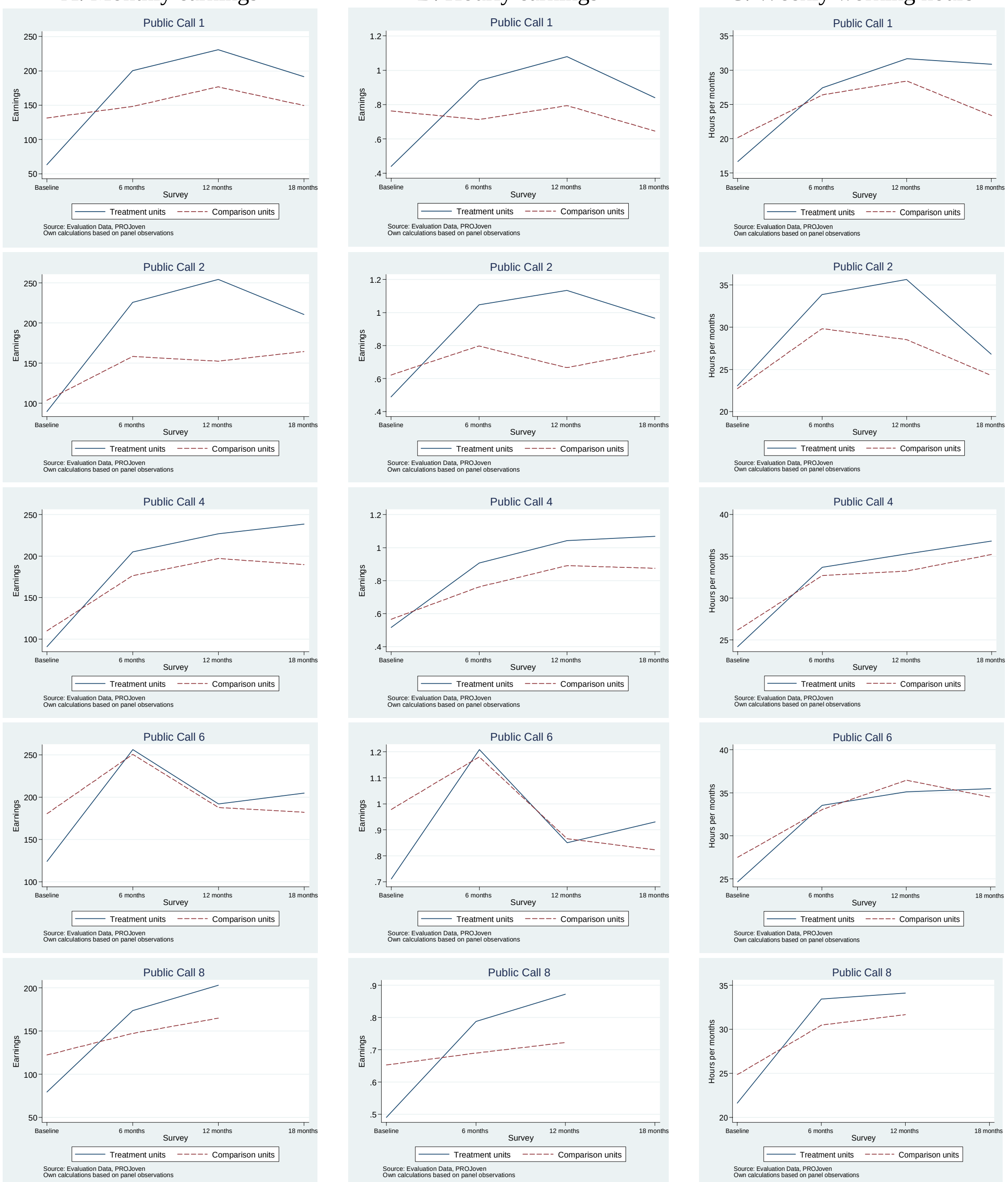
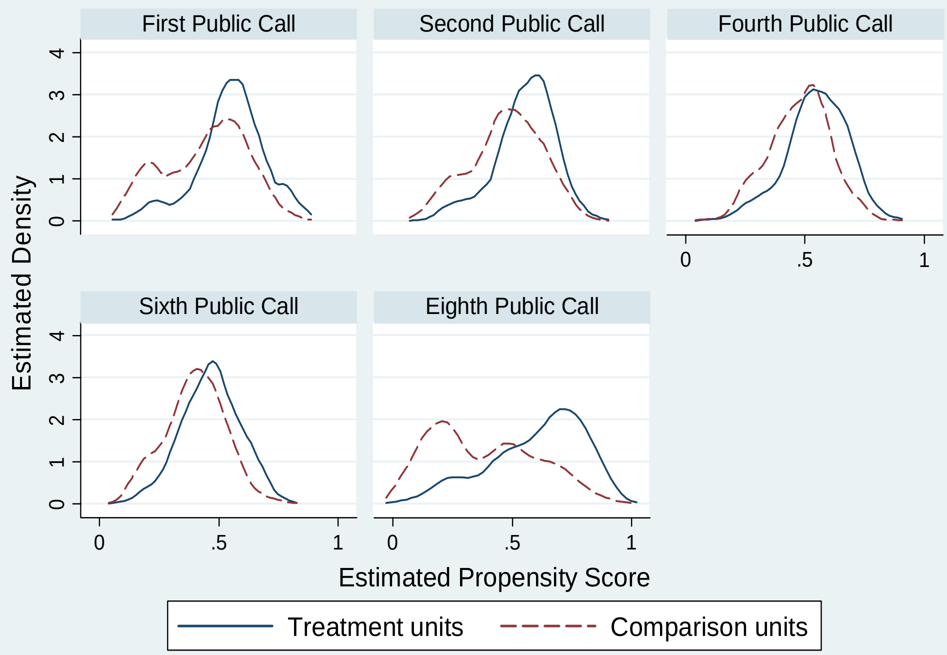


Figure 7. Estimated Propensity Scores for participation in PROJoven



Source: Evaluation Data, PROJoven.
Own calculations.

APPENDIX

Estimated marginal effects from Probit regressions for program participation used to compute the propensity score
(Standard errors in parentheses)

	First	Second	Fourth	Sixth	Eighth
Sex (male=1)	-0.117	-0.219	0.047	0.010	-0.006
	(0.126)	(0.119)	(0.075)	(0.043)	(0.024)
Age	7.704	-6.604	-16.640	-8.506	-20.764
	(20.163)	(23.815)	(14.693)	(9.789)	(15.424)
Age^2/10	-6.812	5.024	12.755	5.869	15.684
	(15.000)	(17.472)	(10.907)	(7.326)	(11.850)
Age^3/100	2.554	-1.683	-4.296	-1.782	-5.248
	(4.930)	(5.664)	(3.577)	(2.422)	(4.024)
Age^4/1000	-0.347	0.210	0.538	0.202	0.657
	(0.604)	(0.685)	(0.437)	(0.298)	(0.510)
Secondary	-0.035	-0.103	0.006	-0.045	-0.030
	(0.072)	(0.093)	(0.060)	(0.028)	(0.024)
Single	0.365**	0.260**	0.263**	0.173**	0.237**
	(0.065)	(0.072)	(0.053)	(0.040)	(0.044)
Have children	-0.001	-0.145*	-0.019	-0.094*	-0.028
	(0.081)	(0.069)	(0.057)	(0.041)	(0.046)
Mother's schooling secondary	0.032	-0.081	-0.009	0.010	0.192**
	(0.061)	(0.058)	(0.033)	(0.022)	(0.026)
Poverty score			-0.006	0.001	-0.130**
			(0.005)	(0.003)	(0.024)
Poverty score^2					0.037**
					(0.007)
Lima			0.003	0.111**	-0.086**
			(0.039)	(0.030)	(0.032)
Trujillo			-0.008	0.019	-0.015
			(0.049)	(0.034)	(0.036)
Chiclayo				-0.017	-0.072
				(0.039)	(0.040)
Cusco				-0.037	
				(0.037)	
Huancayo					-0.039
					(0.041)
Out of labor force in baseline	0.104*	-0.073			
	(0.052)	(0.053)			
Employed t-1			-0.174**	-0.085**	-0.492**
			(0.048)	(0.026)	(0.024)
Employed t-2			0.010	-0.056	0.117**
			(0.053)	(0.030)	(0.039)
Employed t-3			0.169**	0.219**	-0.115**
			(0.054)	(0.032)	(0.039)

	First	Second	Fourth	Sixth	Eighth
Employed t-4			-0.056	-0.041	0.204**
			(0.054)	(0.036)	(0.031)
Employed t-5			-0.052	0.025	0.002
			(0.058)	(0.040)	(0.034)
Employed t-6			0.050	0.023	0.033
			(0.053)	(0.035)	(0.034)
Earnings (log) past six months			-0.008	-0.021**	
			(0.009)	(0.007)	
Sex * Mother's schooling	0.098	0.276**			
	(0.086)	(0.077)			
Sex * Secondary	0.033	0.013	-0.081		
	(0.131)	(0.128)	(0.081)		
Sex * Earnings (log) past six months				-0.011	
				(0.008)	
Observations	622	570	1112	2340	2383
LR chi2	70.37	47.91	80.16	171.73	582.04
P-value	0.00	0.00	0.00	0.00	0.00
Obs. Prob.	0.50	0.52	0.52	0.43	0.49
Pred. Prob.	0.50	0.52	0.52	0.42	0.49
Correctly classified	62.86	62.46	61.60	63.21	70.21

* significant at 5%; ** significant at 1%

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