



*Office of Evaluation and  
Oversight*

# Impact Evaluation of PROJoven Youth Labor Training Program in Peru

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Ex-Post Project  
Evaluation Report:  
OVE/EPPER-04/06  
July 2006







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## PREFACE

In 1995 the ILO presented to the Peruvian's Labor Minister and to the Vice-President a proposal to implement a youth training program based on programs that were being implemented in Chile and Argentina. In 1997 the IDB financed a Technical Cooperation for the preparation of the project, which was implemented between 1998 and 2003 without the support of the Bank. Later, in 2004 a the IDB started to finance the ongoing PROJOVEN program, largely on the basis of the original design.

The overall objective of the program was to help economically disadvantaged youths, of 16-24 years old, to enter into the formal labor market by providing them with training and an opportunity to acquire work experience, which is based on the needs of the private sector. An underlying objective was to galvanize the training market by fostering competition among the training institutions (TI). The specific objectives were to increase the employment rates, jobs quality, hours worked per week and incomes (monthly and hourly) of the beneficiaries.

A relevant feature of PROJOVEN is that, since its original design, it has a good evaluation component that allows to measure the impact of the program on key labor market indicators, such as employment, paid employment, formal-sector employment, hours worked per week, hourly wages and monthly earnings. Based on this data, OVE decided to analyze the impact of the IDB-designed operation.

Regarding PROJoven impacts for beneficiaries, our overall estimates suggest that there are positive and statistically significant effects in terms of paid jobs and formal employment probabilities, as well as in terms of monthly earnings. 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.

Despite international evidence on this sort of training programs, we find that PROJoven has high positive impacts in terms of earnings. Our 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 more conservative estimators we find much lower program effects but still are well above 12%, and as high as 30%.

We argue that, at least in part, this is the result of the concordance between the courses and real labor demand requirements in the labor market, and that beneficiaries must be hired for their internships under Agreements of Youth Labor Training, which provides better job conditions and pay at least the minimum wage. 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 months 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 unobservables.

These robust impacts on employment are enough to recoup the costs of the program. When analyzed together with the conservative estimated impacts on earning, we find that the program has a positive rate of return under a wide set of scenarios, suggesting that this small scale operation has been an efficient investment.

Regarding the impacts of PROJoven on the vocational training market, the most important finding is that the upward trend in participation is associated to the incorporation of new locations outside of Lima. Surprisingly, in Lima the trend turns out slightly downward. This raises questions about the payoff for participation in the Program. The second important finding is that the largest group of participants, and the fastest growing outside of Lima, are in RECAP but do not bid for courses. This suggests motivations for participation that are different than obtaining courses. We suggest that accreditation may be one important rationale and find some econometric evidence in support. However, we also suggest that more analysis of this phenomenon is needed.

We specifically explored and tested three potential impacts of the Program on the training market: specialization among participating institutions, closer links between ECAPs and firms, and accreditation effects. Through analysis of course offerings by training entities we find strong evidence of a trend towards greater specialization. We also explore some descriptive evidence on links between ECAPs and firms, merging data from RECAP and that from the survey of ECAPs conducted by the consultant team. The evidence suggests that PROJoven has significant effects in furthering connections between ECAPs and firms. Finally, we tested the hypothesis of an accreditation motive in participation in the Program, estimating an econometric model of the decision to bid or not bid for courses from PROJoven. Although some evidence indicates the existence of accreditation effects, we suggest that this is an area for future research.

The document also describes some features of the new IDB-funded operation, which are still at an early implementation stage and thus cannot be evaluated. Hence, based on the results of the impact evaluation the recommendations are to continue the close monitoring and evaluation of the program, and to improve in the data gathering system. The new program contemplates the use of surveys at different stages in order to assess the value-added of different stages of the program. This would be a central contribution and should be supported by the Bank.



## I. INTRODUCTION

### A. Purpose of the EPPR and ex post evaluation<sup>1</sup>

Within the monitoring and evaluation (M&E) framework of the Inter-American Development Bank (IDB), the Office of Evaluation and Oversight (OVE) undertakes the independent evaluation of Bank interventions, in accordance with the Bank's policy on ex-post evaluation of operations approved in 2003. For the 2005 ex post evaluation cycle projects under the following three themes have been selected: (i) Job Training Programs; (ii) Rural Roads; and (iii) Science and Technology. OVE has reviewed the bank's interventions on the labor market since the early 1990s, specifically on job training for the unemployed or underemployed, and has selected eleven programs to evaluate. Individual evaluations will be produced for each country, and a thematic evaluation will be done based on the case studies.

This document constitutes a comprehensive evaluation of the "Youth Labor Training Program" (PROJOVEN), which was designed by the IDB in 1996 through a technical cooperation and later financed by the Peruvian government (from 1997 to 2003). The IDB is currently financing a second stage of PROJOVEN (PE0241).

It is too early to evaluate the impact of the IDB-financed intervention of PROJOVEN, which started in 2005. However, OVE considers that it is useful to determine the impact of the previous calls. The report presents the basic information about the original program: the context under it was developed, the basic design features (inputs, outputs, outcomes) and the underlying model. The EPPR contributes to enhance the general knowledge of the Bank by taking a detailed look at the achievement of the development objectives of the first stage of the project.

The report also analyses the ongoing program, PE0241. Particularly, the emphasis is put on the analysis of the main modifications that were introduced to the original design in order to improve the impacts, the evaluability and the problems that affected the execution of the program.

Following the introduction and the description of the evaluation strategy followed by OVE, the document will analyze the context in which the program operated in section II: this section describes the problem(s) (along with its causes and consequences) to which the project was responding, as well as other relevant context. Section III will present a theory based evaluation of the project design, analyzing the implicit model behind the program, as well as the logical consistency and empirical evidence supporting such a model. Sections IV and V- will discuss the findings of the evaluation of the original program, which was not financed by the IDB, for the process and the development outcomes of the program. Section VI will analyze the main modifications of the original design that were introduced by the new stage of the program that is being finance by the Bank. The evaluability and the preliminary results of the program are also presented.. Section VII, will present a general discussion about both stages of the program.

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<sup>1</sup> Part of the section that concerns the context and the problem tackle and also the sections about the description and the impacts of the original program are based on the background report commissioned by OVE to GRADE.

Finally, Section VIII includes the conclusions and recommendations that follow from the evaluation.

## **B. Brief description of the objective and activities of the program**

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 IDB, that had financed the Chilean program<sup>2</sup>, 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.

The overall objective of the program was to help economically disadvantaged youths, of 16-24 years old, to enter into the formal labor market by providing them with training and an opportunity to acquire work experience, which is based on the needs of the private sector. An underlying objective was to galvanize the training market by fostering competition among the training institutions (TI). The specific objectives were to increase the employment rates, jobs quality, hours worked per week and incomes (monthly and hourly) of the beneficiaries. The number of beneficiaries was set at 160,000. Eventually, the Peruvian government decided to choose other funding sources for the program and the goals of the program were reduced.

In 2004, the Bank approved the project PE0241 to finance five more public calls of the program. The overall objective of Peru's "Youth Labor Training Program" (PE0241) is the following: "to help provide economically disadvantaged youths between the ages of 16 and 24 with access to the formal labor market. This is to be accomplished by means of specific activities designed to provide vocational training and work experience, in conjunction with labor intermediation, orientation and information services, which will meet the business sector's and labor market's requirements. The program is also intended to foster competition and efficiency in the training services market by promoting greater interaction between training providers and firms in the private sector. The program's specific objectives are to help raise the labor participation rate, job quality, and wage levels of young people in the target group"<sup>3</sup>. The project maintains the fundamental objectives of PROJoven. It introduced some improvements in the original design, such a labor intermediation activities, and increased the coverage (in terms of the number of beneficiaries and also of cities where it operates). However, some structural problems of the

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<sup>2</sup> The program adopted a design implemented before in Chile (CHILE JOVEN). Its main characteristic was a new mechanism to provide training based on private provision of courses and with the contents determined in a demand-driven fashion. Another salient characteristic of PROJOVEN was that the design included an impact evaluation component.

<sup>3</sup> Paragraph 2.1.

original design were not solved. For example the new design did not consider the institutionalization and thus the sustainability of the program.

The following table shows the basic data for the new stage of the project:

Project	<b>Youth Labor Training Program (PE0241)</b>
Loan Number	<b>1534/OC-PE</b>
Executing unit	<b>PROJOVEN, MTPS</b>
Approval date	<b>02/18/2004</b>
Current disbursement expiration date	<b>03/26/2008</b>
US \$ (IDB)	<b>18,000,000</b>
US \$ (Country)	<b>8,000,000</b>

### C. Evaluation strategy

As we already mentioned, it is too early to evaluate the impact of the project PE0241 because its implementation started in 2005. However, OVE considers that it is useful to have a comprehensive evaluation of the previous calls, for it was designed with the IDB intervention based on the program of ChileJoven and also because the previous impacts evaluations of the program used different methods and they obtained results that are not strictly comparable across the different rounds. Moreover, the new stage of the program is a close continuation of the original program to be evaluated. Thus, the EPPR analyses the design of the new stage, with particular emphasis on the modifications that were introduced, and the implementation process.

There is evaluation data<sup>4</sup> from the first (pilot), second, fourth, sixth and eighth rounds of PROjoven. This data has been used in several studies<sup>5</sup> to evaluate PROjoven impacts on a wide arrange of outcomes, such as employment and unemployment status, labor status transitions, weekly working hours, and labor earnings, among others. Some salient issues emerged from these studies: (i) there is a large variability of the impacts found due to the different use of different techniques (see table I.1); (ii) a comprehensive and homogenous evaluation of the different rounds does not exist and (iii) the selection bias on non observable effects is not sufficiently treated. Therefore, a homogenous evaluation that applies the same methods and criteria across samples is necessary in order to have a better understanding of the program's impacts across sub-groups of interest and over the years (rounds). On the other hand, the impact of the program in the vocational training market has not been evaluated.

The general goal of our re-examination of PROjoven is to provide a comprehensive and homogenous evaluation of the different rounds where evaluation data are available. This homogenous evaluation will explore heterogeneity of program impacts by gender, age and

<sup>4</sup> The program developed a quasi-experimental design for the impact evaluation. This design has been refined over the years. Baseline information is collected for beneficiaries and control groups. Both groups are surveyed again 6, 12 and 18 months after the completion of the training courses. The selection of the control group is based on the following variables: age, sex, education, poverty level and geographic residence.

<sup>5</sup> For example, Galdo (2000), Burga (2003), Chacaltana and Sulmont (2003), Ñopo, Saavedra and Robles (2002), Ñopo y Saavedra (2003) and PROJOVEN (2004).

education, comparing the results over the different rounds and across cities where the program was implemented. The specific objectives of this new evaluation are:

- a. To provide estimates of program impacts in a consistent format for all the previous cohorts of program beneficiaries on several outcomes of interest such as labor market insertion, earnings and occupational segregation. We will explore the heterogeneity of impacts for relevant sub-groups and over time.
- b. To conduct an exploratory analysis on the issue of selection bias. Given the availability of panel and repeated cross-sectional data, we will implement a longitudinal variant of matching to address the potential problem of selection on time invariant unobserved characteristics.
- c. To provide a Cost-Benefit analysis.
- d. To conduct a first analysis of PROjoven's impacts on the Vocational Training Market.
- e. To provide lessons learned from the PROjoven experience.

**Table I.1: Heterogeneity of results in the previous evaluations of PROjoven**

Study	Public call	Return on monthly incomes		
		6 months after	12 months after	18 months after
Galdo (2000)	1	60%	NA	NA
Burga (2003)	2	NA	100%	NA
Nopo, Saavedra and Robles (2002)*	6	18%	NA	NA
Chacaltana and Sulmont (2003)	2	45%	NA	40%
	4	38%	54%	51%
Nopo and Saavedra (2003)	6	48%	34%	31%
PROjoven (2004)	8	58%	65%	NA

*Source:* Chacaltana and Sulmont (2003). Completed by OVE.

*Notes:* \* = hourly income. NA = non available.

## II. CONTEXT AND PROBLEM TACKLED

This section analyzes the context under which the program was design and it operated. The main objective is to assess if the subprogram tackled a real development problem that affected the country and if this problem continued during the implementation. Thus, in the case of PROJOVEN it is necessary to analyze the macro-economic situation, the labor market results, especially for the disadvantaged youths; and how the labor training system functioned.

Overall, the first stage of PROJOVEN started being design during a period of high economic growth but it operated during an economic recession that affected the labor demand and the labor market results. The most affected by the negative labor market situation, in terms of unemployment rates for example, were the poorest young people. The target population concerned more than one million of people with most of them that were underemployed. Moreover, the country did not have an articulated and efficient training system. Instead, Peru had an ample labor training supply with public and private training institutions that offered training with different levels of quality. The target population had disadvantages to access the labor training and those who accessed had higher probabilities to receive low quality training.

On the opposite, the second stage was design and is being implementing during a period of economic growth. However, the negative labor market situation of the target population and the deficiencies of the labor training supply have not changed.

### A. Macroeconomic and labor market context

The Program was designed during a vigorous economic recovery that was generated by 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 equilibria and reduce inflation<sup>6</sup>. 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 (see figure II.1)<sup>7</sup>.

Most of the execution of the program occurred during the period of economic recession (1997-2001). Thus, the labor demand was not very dynamic and thus the creation of news jobs was low.

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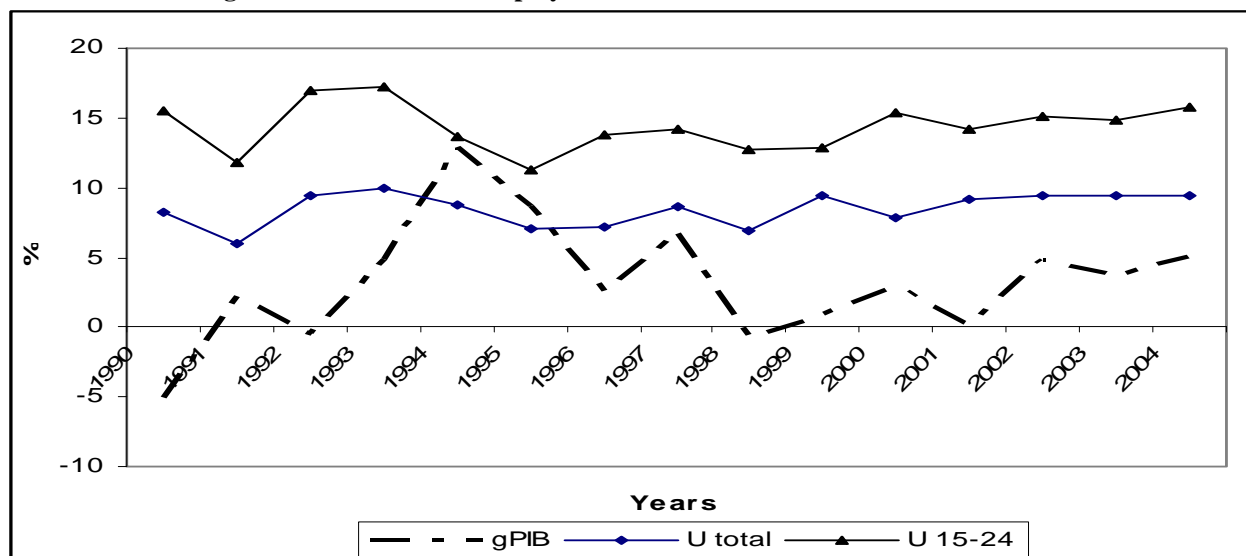
<sup>6</sup> A detailed account of policies during this period can be found in Jaramillo and Saavedra (2005).

<sup>7</sup> 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).

On the contrary, the second stage of PROjoven was launched during the economic recovery that started in 2002 and continued through 2005.

Between 1992-1997 the employment growth followed growth in output. However, the growth of employment was 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%. Its participation in the labor force was also large, accounting for more than one-fourth of it<sup>8</sup>.

**Figure II.1: GDP and unemployment rates evolution between 1990 and 2004**



Source: OIT (2005) and WDI

Table II.1 summarizes the employment situation of young people 15-24 years old one year after the implementation of PROjoven (1997). We observe that the active population was composed of 1,867,000 youths 15-19 years old and of 1,650,000 youths 20-24 years old; 1,345,000 of them were poor (the proportion of poor was 42% in the 15-19 years old group and 34% in the 20-24 years old group). The majority of the 15-19 years old were inactive (62%) while the 20-24 years old had a higher proportion of active people (67%). Most of the active youths had an employment but were underemployed. This was particularly the case for the youngest and for the poorest. In fact, among the poor youths underemployment was higher for the 20-24 years old. On the other hand, the unemployment rate was almost the same for both age groups but was also higher for the poorest. Finally, the inactivity rate was higher for the poorest. This was particularly the case for the 20-24 years old (30%). Between the period 1998-2001, the economic recession increased the rates of unemployment, decreased the generation of formals jobs and reduced the average quality of employment. Moreover, the quality of the jobs was affected during the nineties. According to the OIT (OIT 2005) between 1990 and 2001 the share of the

<sup>8</sup> For detailed descriptions of the situation of youth in the labor and education markets see Saavedra and Chacaltana, 2001; Arróspide and Egger, 2000.

informal sector in the total employment increased from 54% to 59%<sup>9</sup> while the share of the temporary jobs in the private formal sector increased from 19% to 53%.

The economic recovery that started in 2002 has not significantly improved the labor situation of the target population. For example, in 2004, the informal sector represented around 58% and more than 40% of the Peruvians considered that the lack of employment was their main preoccupation (ENAHO 2004).

**Table II.1: Labor indicators of the young people by socio-economic status in 1997**

	15-19 years old			20-24 years old			All young people		
	No poor	Poor	Total	No poor	Poor	Total	No poor	Poor	Total
<b>Absolute values</b>									
PET	1,079	788	1,867	1,092	557	1,650	2,172	1,345	3,517
PEA	457	255	712	801	310	1,112	1,258	566	1,824
Employed	404	202	606	732	245	976	1,136	447	1,583
Underemployed	283	149	432	396	194	590	679	343	1,021
Adequately employed	121	53	175	336	51	387	457	104	561
Unemployed	53	53	106	70	66	136	122	119	241
Inactive	623	533	1,155	291	247	538	913	780	1,693
Student	478	392	871	147	81	228	625	474	1,099
No student	144	140	285	144	166	310	288	306	594
<b>Indicators</b>									
Participation rate	42.3	32.4	38.1	73.4	55.7	67.4	57.9	42.0	51.9
Inactivity rate	13.4	17.8	15.2	13.2	29.7	18.8	13.3	22.8	16.9
Unemployment rate	11.5	20.8	14.8	8.7	21.2	12.2	9.7	21.0	13.2
Underemployment rate	61.9	58.3	60.6	49.4	62.4	53.0	53.9	60.6	56.0

*Source: Saavedra and Chacaltana, 2001*

*Note: Only the urban areas are represented*

## B. The education system and the training market

By the time when the program was design the educational system was in a poor state and it seemed quite clear that it was not fulfilling its role in preparing Peruvian youth for the realities of the labor market<sup>10</sup>. A significant effort in training was needed to support disadvantaged youth in making room for them in an increasingly competitive labor market (see box II.1). Two elements, considerable disadvantages for youth in insertion in the labor market and a poorly performing system of basic education, provided the rationale for a broad-ranging training program focused on the poor, such as PROjoven.

### Box II.1: A note on the Peruvian educational system

The expansion of the educational system in the seventies and eighties resulted, in the nineties, with youth having more years of education than previous generations<sup>11</sup>. However, this came with a declining trend in public education

<sup>9</sup> According to Garcia (2001) the annual growth rates of the informal and formal employment between 1990 and 2000 were 1.2% and 4.8%.

<sup>10</sup> The quality of education declined in the public education system, which affected the poor people because they couldn't afford to attend the private education system.

<sup>11</sup> Between 1972 and 1993 the proportion of young people (15-29 years old) with primary education decreased from 48% to 24%. During the same period, the proportion of young people with a post secondary education increased from 5% to 23% (Saavedra and Chacaltana 2001).

expenditure per-student and thus a drop in the quality of education.<sup>12</sup> Further, according to ENAHO in 1997 less than 10% of youth coming from poor households had any formal education beyond secondary.

In Peru, the educational system is organized in the basic educational system (eleven years of schooling), formed by the primary level (first through sixth grade) and secondary level (from seventh to eleventh grade) levels; and the post-secondary system, formed by universities (5 year-colleges) that offer college degrees, and other higher education institutions (usually 3-year or 4-year education) that offer technical degrees and vocational training. Basic education is mandatory, and most of the schools and enrollment are managed by the public sector, which represent above 80% of total enrollment. Post-secondary education is optional, and the public sector represents more than half of the supply.

On average, the evidence gathered from language and math test to evaluate the achievement of students from different grades reveal that Peruvian schools are performing poorly. For instance, results for fourth grade students from the language test in the National Evaluation of School Achievement of 2001 (ENRE-2001) are discouraging. In the test of reading comprehension only 31% of the students in urban areas obtained scores in to the sufficient performance level, whereas 54% of students obtained a score that locates them in the below-the-basic level. In rural areas, only 11% of the students of fourth grade obtained a score equivalent to a sufficiency level, whereas 81% were below the basic level. For grammar rules, the results are still more discouraging: the percentage of students with a level of sufficient performance is practically zero, both in urban and rural areas. In urban areas, 62% of the students obtained scores below the basic level and only 38% attained the basic level; in rural areas these figures were 86% and 14%, respectively.

For students of eleventh grade, who were completing the secondary level, results in ENRE-2001 were also disappointing. In the language test, 63% of the students obtained scores in the below-the-basic level. In math, the results were even worse, 83% of all eleventh graders obtained scores in the below-the-basic level. Even more, there were dramatic differences between students from public and private schools. While the percentage of private students in the below-the-basic performance level of the language test was 31%, the figure for public school students was 69%. In math, 88% of public schools students and 55% of private schools students were in the below-the-basic level. Given that children from poor families attend public primary and secondary schools, the most vulnerable groups receive education of very low quality, which generates serious challenges in terms of the skills with which these individuals enter the labor market.

In addition to the learning deficiencies that these tests reveal, other indicators reveal the serious problems faced by the educational system in Peru. Although the educational system has improved its coverage in infrastructure terms during the last decade and enrollment in the primary level has improved in most of the 24 departments, enrollment in secondary level is still low, especially in rural areas. There still persist serious drop-out problems in secondary, and extra-age at primary and secondary. Although the extra-age problem has been reduced during the nineties, we still have a long way to go. In 1999, the percentage of students of primary schools in the correct grade for their age was just 67%, while at secondary schools it was only 36%. Even tough, these figures represent an improvement of approximately 10% with respect to the situation observed in 1993.

The country did not have, and still lacking, a well-defined structured training system; instead, it has an important number of training modalities, programs and institutions with different quality levels. The Ministry of Labor and the Ministry of Education share the responsibility of this supply of training (see box II.2). Several studies indicate that the deficiencies of the basic educational system are reproduced in the post-secondary educational system. In particular, recent research on the supply side of the vocational training system in Peru indicates that there are: a) a strong expansion of the supply of vocational training, reflected by the increase in the number of and enrollment in public/private ITs and CEOs during the last decade, b) a high heterogeneity in the quality of the institutions that provide training services, c) little or negligible regulation on the training market, d) information problems regarding the quality of education institutions and the demand for qualifications, and e) a mismatch between the supply of labor qualifications and

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<sup>12</sup> In 1990 expenditure per student was half of that in 1970 (Saavedra et al. 1996).



the demand of these qualifications by firms and companies. Few training institutions consider the firms or sector needs before to elaborate their courses.

In this context, it is of great relevance to assess how adequate and pertinent the post-secondary education institutions are. One open question is whether the vocational training education improves the productivity of the labor force or if it works only as a signaling device. On this regard, one argument in favor of PROjoven is that this intervention concentrates on human capital investments that are pertinent, in the sense that there exists a demand for the qualifications provided by the training institutions (ECAPs).

**Table II.2: Type of training institution attained in 1997 by age group and socio-economic situation (%)**

	15-19 years old			20-24 years old			All youngs		
	No poor	Poor	Total	No poor	Poor	Total	No poor	Poor	Total
CEO	26.0	22.6	24.8	30.3	35.7	31.9	28.6	29.4	28.9
Sector service	4.0	3.6	3.9	5.2	4.4	5.0	4.7	4.0	4.5
IST	24.9	17.1	22.0	35.7	33.4	35.1	31.5	25.5	29.6
University	6.8	4.3	5.9	8.0	6.1	7.5	7.5	5.2	6.8
Secondary School	29.7	41.8	34.1	7.6	8.9	8.0	16.3	24.7	19.0
Firm	0.5	1.3	0.8	5.2	2.6	4.5	3.4	2.0	2.9
Other	8.0	9.5	8.5	7.9	8.9	8.2	7.9	9.1	8.3
<b>Total</b>	<b>387.0</b>	<b>224.0</b>	<b>611.0</b>	<b>600.0</b>	<b>240.0</b>	<b>841.0</b>	<b>988.0</b>	<b>646.0</b>	<b>1452.0</b>
<b>Total (%)</b>	<b>100.0</b>	<b>100.0</b>	<b>100.0</b>	<b>100.0</b>	<b>100.0</b>	<b>100.0</b>	<b>100.0</b>	<b>100.0</b>	<b>100.0</b>

*Source: Saavedra and Chacaltana, 2001*

*Note: Only the urban areas are represented*

During the nineties, the government tried to stimulate the training offered by firms and introduced measures oriented to young people in the labor legislation<sup>13</sup>. Before the labor reform of 1990, the Peruvian legislation included only one modality of labor contract for the young people, which was that of pre-professional internship. This modality was reserved exclusively to the students with higher education. Two new modalities were included by the labor reform: (a) the agreement of young vocational training (“convenio de formación laboral juvenil”) which is oriented to the 16-25 years old; and (b) the contract of learning (“contrato de aprendizaje”) which is oriented to the 14-24 years old with at least a primary education. However, these new modalities were not widely used by firms. According to Saavedra and Chacaltana (2001) they represented less than 4% of the total formal employment and they concerned only 5% of the young people working in formal firms<sup>14</sup>. On the other hand, the labor market reform had negative indirect consequences on the training offered by firms because it increased the labor mobility and reduced the duration of employment (by 37% between 1990 and 1999)<sup>15</sup>. Firms were less motivated to offer training to their employees because the risk of turnover increased<sup>16</sup>.

<sup>13</sup> However, the government didn't introduce fiscal incentives to motivate the participation of firms in training.

<sup>14</sup> In 2000, 9,328 pre-professional internship agreements and 37,432 young vocational training agreements were signed. In 1998, 64,144 contracts of learning were signed.

<sup>15</sup> According to Saavedra and Chacaltana (2001) turnover rates are higher for young people. The authors found that 53% of young people changed its labor status during the year and only 17% of young workers were employed for more than one year.

<sup>16</sup> Chacaltana and Garcia (2002) found that firms with higher rates of rotation invest on average 28% less in training than firms with lower turnover rates.

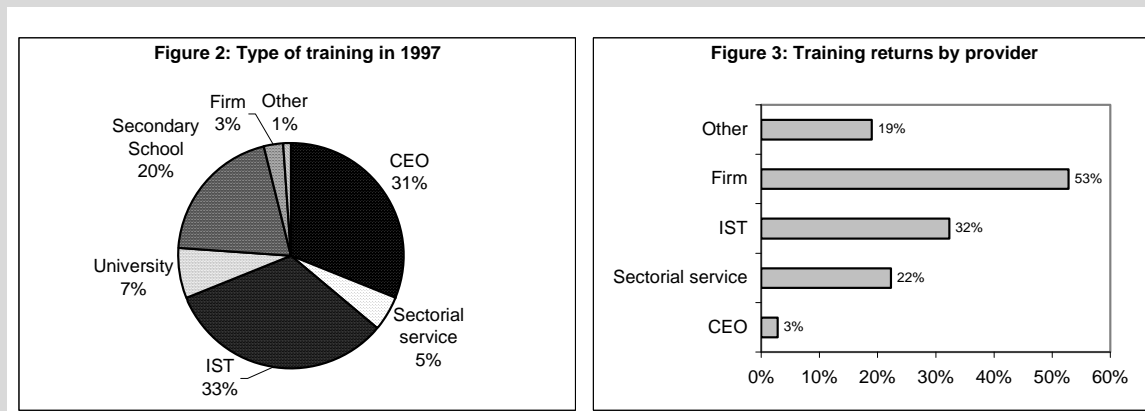
### Box II.2: The labor training system

In the decade prior to the implementation of PROjoven, the Peruvian training market had expanded significantly, particularly through the growth of the private sector, though the public sector still had more than a third of training institutions<sup>17</sup>. The sector was essentially unregulated and efforts to introduce quality standards through certification moved extremely slowly. The IDB's program with the Ministry of Education for the reform of technical education started in mid-nineties contemplated a component focused on certification. However, no certification system currently exists. As a result, the system is quite heterogeneous as far as quality and generally lacks a connection with the productive sector. In addition, it is unevenly distributed geographically.

Labor training supply can be divided in two main components: (1) the formal training which is integrated by Technical Institutions (ITs); Occupational Education Centers (CEOs), Sectorial Services (SS)<sup>18</sup>; Institutions of Higher Education (IHE); and Secondary Schools that offer technical education; and (2) the non-formal training that is offered by NGOs, firms, public programs and by some universities.

Figure 2 and figure 3 show that in 1997 the majority of young people that received training attained the TIS and CEO. However, incomes training returns were higher for young people that received on-the-job training.

Chacaltana and Garcia (2002) found that in 1998 only 23% of the industrial firms offered training to their employees. Most of these firms were medium or large firms<sup>19</sup>. The fact that small firms did not offer training to their employees is problematic because the employment in firms with less than 10 employees represented more than 70% of the total employment.



*Source: Saavedra and Chacaltana, 2001*

*Note: Only the urban areas are represented*

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 (see table II.2). 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 (urban) survey applied to 1,112 graduates from 123 Technical Institutes (Institutos Superiores Tecnológicos – IST) in 1996 finds that there exists a high degree of

<sup>17</sup> Saavedra and Chacaltana (2001) found that the number of Technical Institutions and Occupational Education Centers increased from 996 to 2,470 between 1986 and 1996. The number of students attending these training institutions increased from 342,000 in 1993 to 420,000 in 1996. In 1996, 60% of these institutions were in the private sector.

<sup>18</sup> This service exists in the industrial, communications, commerce and tourism sectors. For example, the industrial training service named SENATI (*Servicio Nacional de Adiestramiento en Trabajo Industrial*) was created in 1961. This service is financed by industrial firms (with more than 20 employees) that have to pay a monthly payroll tax of 0.75%. SENATI offers “técnico operativo”, “técnico medio” and “técnico superior”. The first type of training is oriented to young people (14-24 years old) without any qualification and also to non-qualified or half-qualified young workers.

<sup>19</sup> Only 9% of the micro enterprises and 19% of the small enterprises invest in training. These proportions were 40% and 64% for the medium and large firms. On average firms spent US\$ 712 per trainee.

heterogeneity among these institutions. The study suggests that 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 ISTs from other Peruvian cities. In particular, graduates from a high quality IST earns on average 46% more than graduates from low quality ISTs. In other cities this difference in earnings is about 17%. 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).

### **C. The original program design: a theory based evaluation**

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 ECAPs. Beneficiaries attend the training courses for three months and the cost of the courses is covered by PROjoven. The second is an internship phase at private firms where trainees acquire on-the-job experience. During this phase each beneficiary interns at a private firm for three months and receives a market wage for their skills level, which is paid by the firm. After these three months the firm may or may not hire the trainee.

PROjoven beneficiaries are youth between 16 and 24 years old, with low levels of formal education and without or minimum labor market experience, currently unemployed, underemployed or out of the labor force. The beneficiaries primarily come from poor families. The selection of beneficiaries takes place at PROjoven headquarter or at PROjoven local centers. The program is voluntary and operates on a first-come first-serve basis.

ECAPs are pre-selected by PROjoven on the basis of past training experience, administrative capacity and the adequacy of the courses provided. ECAPs should also provide their 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 to provide vocational training<sup>20</sup>.

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.

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#### **D. Description of the program**

It should be noted that the original design contemplated a much larger program than what was achieved. Initially it was planned that the program should benefit directly 160,000 youths between 16 and 24 years old in five years of operation. Two reasons are associated to this rescaling. First, after the pilot, the Program was supposed to be financed through an IDB loan that was ready for approval in 1997. However, the Peruvian government decided not to proceed with the operation<sup>21</sup>. Second, the first call suggested that there were supply side constraints for such a fast growth in the Program<sup>22</sup>.

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<sup>23</sup>. However, greater emphasis was 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-à-vis its predecessors. Instead, in order to ensure that practical training in the firm occurs, the Program requires training entities to obtain letters of intent from private sector firms to provide internships/practical training to beneficiaries of the Program. Although 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 ECAP is contingent upon students completing their practical training phase.

Another feature in which PROjoven has departed from prior experiences is the targeting strategy. While Chile Joven relies solely on self-targeting, Peru's PROjoven uses a combination 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 card 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. Although any youth can apply to participate in the Program, only those that meet the minimum criteria are accepted.

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<sup>21</sup> The program spent approximately US\$17 million between 1997-2001. The original budget was fixed to around US\$80 million.

<sup>22</sup> It seems that these problems were reinforced by the economic recession that started in 1997.

<sup>23</sup> 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 youths at risk.

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% of participants were non-poor, by the fifth call the figure had dropped to 9.8%. These numbers compare favorably *vis-á-vis* those for Chile Joven (Marín, 2003).

## **1. Administration<sup>24</sup>**

PROjoven is executed by a Coordinating Unit (CU), within the Labor Ministry, enjoying financial and administrative autonomy. Its main functions are planning activities, managing their execution, and monitoring and evaluating the performance of the Program. Work is divided into the following 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 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.

## **2. Processes: selection of eligible and beneficiary groups**

Before the selection of program eligible and beneficiaries takes place, PROJoven personnel carryout several activities in order to provide information about the program to its target population and to promote the enrollment of potential beneficiary youngsters. To this end, the program put 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, radio, and by using printed ads in the press, and also distributing pamphlets and other printed materials in localities where poverty rates are higher.<sup>25</sup> 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 several steps, selection procedures and filters. The ECAPS play an important role during this process (see Chart II.1 and Box II.3).

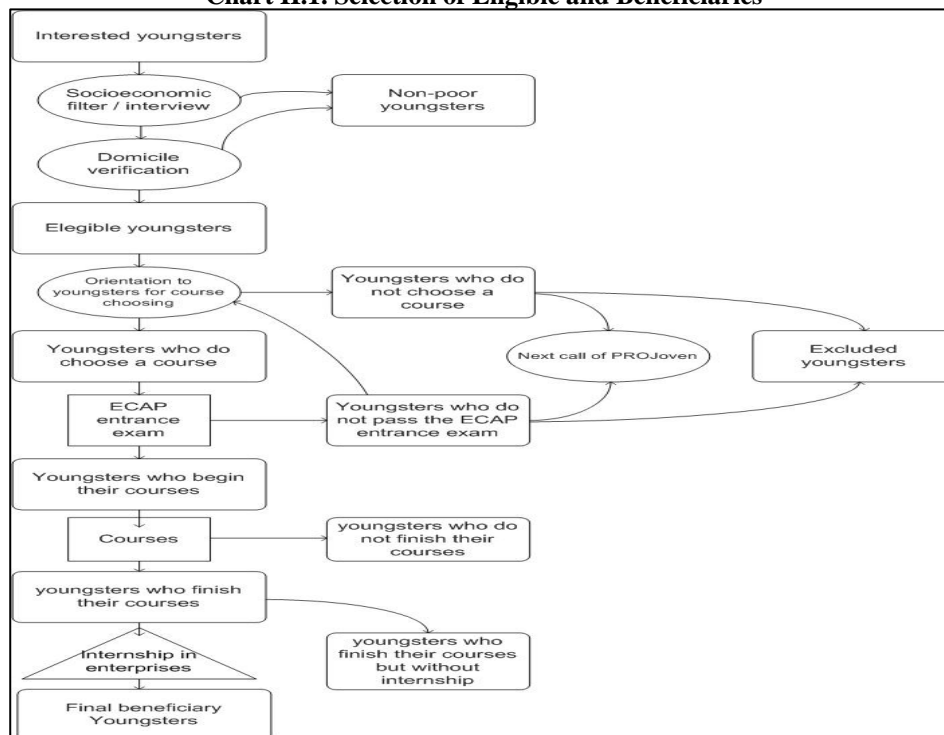
The information generated and acquired by PROjoven during the process of selection is collected in the Registry of Eligible and Beneficiaries. This Registry comprises information on the accreditation process as well as on the selection of beneficiaries. This way there is a link between each beneficiary in the Program's database and the ECAP she received training from. 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.

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<sup>24</sup> This section is based largely on Arróspide and Egger (2000).

<sup>25</sup> The localities where promotion and dissemination 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.

**Chart II.1. Selection of Eligible and Beneficiaries**



**Box II.3: The selection process of eligible and beneficiaries**

At a first moment, all the youngsters that show-up to the PROJoven headquarter or decentralized offices receive a ticket to go through an accreditation interview, in which they need to show they are actually poor. In general, the interview takes place about a month after the ticket is received, and the process continues until the pool of eligible individuals is twice the course vacancies for the public call. Some of these young people never return to the accreditation interview and no registry on those people is kept. Those who return to the interview respond to a battery of socioeconomic and demographic questions and are required to show with documents that the information they are giving is accurate. For instance, they are required to present their identification or military card, and utility bills from recent months to confirm address and place of residence.

Information collected during the interview is used to compute a “poverty index” based on a pre-specified algorithm. Based on the results from the algorithm, social workers from the program can decide whether these young people belong to the target population of the program, that is, to the eligible group (*acreditados*). In order to verify the results of the procedure and that the information collected during the interviews is reliable, a sample of youngsters from those interviewed 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 accredited or eligible youngsters are selected, they are asked to choose among a list of training courses that will be supplied by the training institutions –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. In the potential situation some youngsters feel unable to choose, either because, for instance, they don’t like the menu of courses or because they don’t wish to continue the process; they are offered the possibility to show-up again during the next public call and are exonerated from the accreditation stage.

Within days after the orientation talk, eligible youngsters are sent to the ECAPs. At this stage the training centers choose beneficiaries. Each ECAP employs its own procedures, such as entry tests, personal interviews, or any combination of these. 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

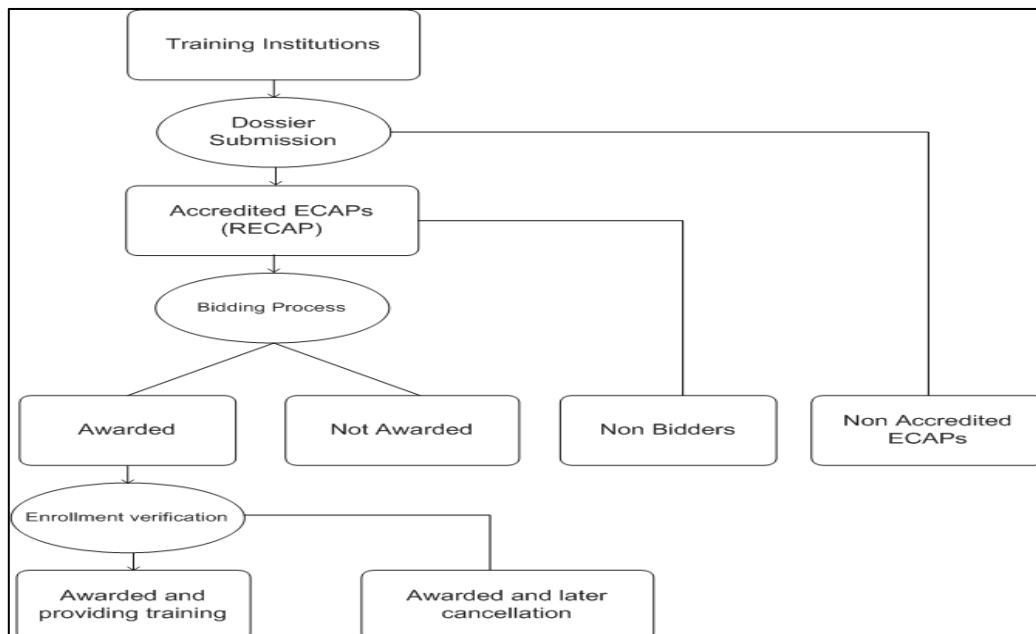
of drop outs during the first week of courses, deserters are replaced by other beneficiaries. However, if desertion occurs after one week there is no replacement and PROJoven does not pay the ECAP the cost of that vacancy. Individuals in the eligible group who are not admitted by the ECAPs of their choice 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.

GRADE found that only a fraction of about 60% to 70% of the eligible beneficiaries is finally selected as beneficiaries at the ECAPs selection process. An analysis of the observables differences between eligible non-beneficiaries and beneficiaries of the program showed that there are some statistically significant differences between both groups. Overall, they found that those in the beneficiary group are less likely to be high school drop-outs and more likely to have acquired a high school diploma. Years of schooling are slightly higher among beneficiaries, but the difference is highly 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% level. GRADE found no significant differences between beneficiaries and eligible non-beneficiaries on other observable dimensions such as age, percent of female youngsters, current school enrollment or whether they worked during the last week prior to the recollection of information. Finally, and as expected the accreditation score (computed using the formula provided in the Accreditation Forms) turned out as not statistically significant between eligible non-beneficiaries and beneficiaries.

### 3. Selection of Training Centers (ECAPs)

In order to participate in the program the ECAPs must be accredited too. Only training institutions that succeed in the accreditation process are considered in Registry of Training Centers (Registro de Entidades de Capacitación –RECAP) and are allowed to bid for courses during the public call. This process of accreditation of ECAPS precedes the selection of the eligible and beneficiary groups (see Chart II.2 and Box II.4) .

Chart II.2: Selection of ECAPs



#### **Box II.4: The accreditation of the ECAPS**

To obtain accreditation, ECAPs must present a dossier following specific instructions provided in the RECAP regulations. The information provided by ECAPs is evaluated according to an algorithm 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, and the commitment with private companies to provide on-the-job training for their students. Each public call has its own procedures and regulations, which define the criteria to accept training institutions into the RECAP, that is, they specify the selection algorithm, which can vary over time. The results of this selection process has three possibilities: accredited (apta), non-accredited (no apta), and under observation (observada –border line cases in which additional verification of the information is required). ECAPs that are accredited enter the RECAP. They can participate in the public call to bid training courses that will be paid by PROjoven. During the bidding process ECAPs offer courses (syllabus), in packages of sections (hour, classes) which are ranked and chosen according to the procedures established in the RECAP regulation for each public call. In the end, accredited ECAPs can be classified into four groups: a) those with awarded courses providing training (adjudicadas), b) those with awarded courses with later cancellation (adjudicadas anuladas), and finally c) those with not awarded courses (no adjudicadas)<sup>26</sup>. It is possible that even after being accredited and registered in RECAP some ECAPs do not participate in the bidding process (non bidders)<sup>27</sup>.

Once the courses have been awarded comes the selection process of beneficiaries. This process is made as soon as social workers of PROjoven conclude the selection of the eligible group. 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.

#### **4. The model behind the program**

Publicly funded job training is a policy instrument within the Active Labor Market Policies framework. In the literature there are several definitions of these policies, from those “aiming at improving the access of unemployed to the labor market and jobs, job-related skills and the functioning of the labor market” (Martin, 2000) to “activities intended to increase the quality of labor supply, to increase labor demand; or to increase the matching of workers and jobs” (Betcherman et. al, 2000). It is possible to identify the central elements as a direct intervention of the government aimed at impacting the functioning of the labor market, centered around two

<sup>26</sup> The reasons for cancellation could be low or null enrollment or sanctions to the ECAP due to serious offenses.

<sup>27</sup> The longitudinal analysis of RECAP data confirms that this is happening more often in recent calls.



issues: improving the opportunities for the unemployed and improving the skills of the labor force.

Within the set of active labor market policies, training is one of the most common instruments. It has several modalities (training for unemployed, displaced or active workers) and it is used to address various issues. Training programs are intended to impact on labor supply, by providing or updating relevant skills to the population, with the ultimate goal of increasing employment and incomes. In some cases, training is closely linked with intermediations services.

Labor markets have various important failures, which could justify government intervention to increase its efficiency. These failures include imperfect and asymmetric information, the lack of complete contracts (the prohibition of long-term binding contracts in labor relations), and externalities (that arise insofar as training creates knowledge, which may be considered a public good). Additionally, a major political economy rationale for these programs is to create support for economic reform; an added motivation is that of equity concerns.

With this background, the rationale behind job training programs could seem straightforward. Several purposes could be pursued:

- a. To provide new knowledge or abilities (enhance human capital or employability)
- b. To serve as a labor intermediation instrument
- c. To integrate trainees to the social institutions (enhance social capital)

These purposes are not mutually exclusive, for example, training and counseling programs could aim at meeting two or all of the objectives. In any case, the ultimate goal of a training program for the unemployed or underemployed is to insert the trainee in the labor market. The programs usually stress that they do not provide jobs, but rather with elements (theoretical training and practical experience) to improve the chances of a successful participation in the labor market; i.e. they promote employability (which, ultimately, should translate into employment). This was the case of PROJOVEN in Peru. For example, the Loan Document (LD) of the PE0241 mentions that: “the young people participating in the program will increase their productivity and employability thanks to the resulting improvement in their basic competencies, work attitudes, knowledge of the labor market and of existing labor demand, and self-esteem”<sup>28</sup>.

The underlying assumption is that people are not able to find a job because they lack the skills that are demanded by the productive sector, and/or because they lack relevant labor market experience. For example, in Peru one of the reasons mentioned by the LD in order to support the pertinence of the new stage of the program is that: “given the poor quality of primary and secondary education, the acquisition of job skills and/or work attitudes that will improve young people’s prospects in the labor market are closely related to their chances of gaining access to post-secondary training”<sup>29</sup>. The LD also mentions that the introduction of labor intermediation activities will reduce: “the overall cost (in terms of time and accuracy) of matching up young job seekers with positions that become available as a function of demand on the labor market. This

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<sup>28</sup> Paragraph 4.9.

<sup>29</sup> Paragraph 1.26.

effort is expected to reduce the amount of time that individual job seekers are unemployed, employers' recruitment costs, and non-functional staff turnover caused by the placement of people in positions for which they are not qualified".

Succinctly, the assumptions behind these programs can be described as follows:

- a. Unemployment is due to a lack of the skills demanded by the productive sector
- b. The skills needed could be acquired in short-term courses
- c. Courses are relevant and pertinent to local labor markets

On the second assumption, as in most similar programs, it is assumed that this is the case. The program aims at providing training at the semi-skilled level, so it is considered that three months of intensive training in a particular trade or occupation could be enough to acquire the basic necessary skills to perform that occupation. Although it is unlikely that people will acquire relevant skills in three months, for some of these occupations three months could provide the basic skills to start a career in that trade. If the primary purpose was to increase the human capital of the trainee, it is uncertain whether three-month part-time courses could reasonably aim at providing with enough skills or techniques to effectively improve the productivity of workers. However, the course was for semi-skilled positions, for which the basic skills could be acquired in a short course. In general, what is stressed is that basic social skills for the labor market can be provided, thus increasing the social capital of participants. Also, the internship experience allows the trainee to have real world contact with the labor market<sup>30</sup>. Besides, participation in the program also constitutes a valuable signal for potential employers, thus helping to reduce information asymmetries in the labor market.

On the other hand, if the stress is around labor intermediation or social capital enhancement, then the training component serves more as a function of signaling or of socialization, and the improvements to human capital are not as important. For example, in Chile and Argentina the purpose of the courses was to attend at-risk populations, in the sense of them being marginalized from the main social institutions<sup>31</sup>. In these programs it is usually argued that even if the economy is able to create jobs these groups could be marginalized due to the uncertainty that employers face when considering whether to hire someone from the target population. In this sense, the program is able to provide a signal (in terms of the trainee being certified or accredited) in order to correct for asymmetries of information.

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<sup>30</sup> During the interviews one of the coordinators of the program mentioned that the purpose of the program was to offer a minimum first labor experience to the beneficiaries ("horas de vuelo").

<sup>31</sup> This purpose justifies the intervention of the government in the financing of the training required by private firms.

### Box II.5: Demand-Driven Models

In the early 1990s an innovative program was implemented in Chile. The “*Chile Joven*” was the first demand-driven model financed by the IDB, and had the following salient features:

- a) Focalization: youth and disadvantaged unemployed population with low chances of inserting into the formal labor market
- b) Training is intended not only to increase the human capital of participants, but mainly to increase their social capital and to increase the employability of participants.
- c) Training is completed by an internship in a firm. This phase is supposed to allow the participants to gain valuable experience in a formal sector job
- d) The training institutions have to contact private sector firms to detect the demand, and these firms agree to provide internships to trainees for two to three months
- e) The model is market-oriented for it relies on the market to reveal the demand for training (the participation of the private firms by providing internships is supposed to guarantee the pertinence of the courses) and the provision of courses is determined competitively through the functioning of a market of training institutions.

(For a further discussion, see OVE’s Approach Paper and the Meta-Evaluation –forthcoming in 2006)

The third assumption about the relevance and pertinence of training courses is crucial. An important critique to the traditional provision of training by national training institutions is that the content of the courses is driven by the supplier of these courses. Even if efforts are made to periodically consult with the productive sectors in order to determine their training needs, in many cases it was considered that those large public institutions in charge of the financing, planning and provision of training were not respondent to the needs of the productive sectors, and that they operated inefficiently and had little incentives to improve their performance<sup>32</sup>.

One of the main innovations from the Chilean experience was the use of market mechanisms to provide the training and to guarantee the relevance of the courses (see Box II.5: Demand Driven Models). To assure that courses were relevant, proposals needed to be presented along with a letter from a private firm stating that the content of the course was agreed with the Training institutions, and that the firm was committed to providing internship opportunities for trainees. This mechanism implies that training institutions are crucial for the functioning of the demand-driven feature of the program. For this to happen, training institutions need to be solid institutions capable of developing course contents and of contacting the private sector in order to determine its needs and to adjust their courses to the requirement of those demanding training. In Chile this was the case before *Chile Joven* was implemented (see Box II.6). Peru, improved this mechanism because. *PROjoven* does not finance the internship phase in order to secure the relevance of the on-the-job training.

Perhaps the most questionable assumption behind the basic model that supports this type of intervention is to suggest that unemployment or inactivity is due (largely) to supply problems. These models operate under the assumption that job creation is not the main issue. In the face of a poor rate of employment growth, perhaps these programs accept the consensus that a stable macroeconomic and fiscal policy, together with deregulation, free trade promotion and other basic pro-market reforms would be the ideal climate and best policy for job creation. This, conceivably, is the most important risk of this program: that the economy does not grow fast enough to create the jobs necessary to place the trainees. We do not know if the original design of *PROjoven* considered this risk but the execution of the program occurred during a context of

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<sup>32</sup> The exception to this is Brazilian SENAI, which is managed by the private sector, thus assuring the relevance of its operations.

an economic downturn (see section 1). On the contrary, the PE0241 was designed and has been executing during a period of economy recovery. The logical framework (LF) considers as an important assumption in order to meet the goal of the program that the economic conditions have to remain stable enough to facilitate government policies designed to help reduce poverty and employment problems by investing in human capital development strategies<sup>33</sup>.

#### **Box II.6: Peculiarities from Chile Joven**

The first demand-driven IDB-financed program was in Chile in 1992. Closely after, together the ILO and the IDB promoted and implemented similar programs in Venezuela (1993), Argentina (1994), Paraguay (1994) and Peru (1996). Chilean inspired programs have also been implemented in Panama (1999), the Dominican Republic (1999), Colombia (2002), Honduras (2004) and Haiti (2005).

There are several peculiarities from Chile that are not present in many other countries, and that should alert on the replication of the Chilean experience without a thorough consideration. Among them:

- a) Existence of a solid regulatory agency for training policies (SENCE, created in 1976)
- b) The separation of financing, design and provision of public sponsored training had been in place for over twenty five years.
- c) The existence of private training institutions in a competitive market used to work closely with the productive sector
- d) A stock problem: it was considered that about 200,000 people were at risk. This group was formed by young people that dropped school due to the crisis from the mid 80s. It was assumed that this problem would not persist, that new pools of at-risk people would not appear
- e) There was a massive effort to solve the stock problem. The dimension of the project was large in order to solve this issue.

In this sense, from a Chilean perspective, the innovation of *Chile Joven* was not the mechanism, but rather that it was one of the first social programs that used the institutional infrastructure created by SENCE in 1976.

(For a further discussion, see OVE's Approach Paper, the EPPER for *Chile Joven* and the Meta-Evaluation – forthcoming in 2006)

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<sup>33</sup> Paragraph 1.2 mentions that: “the economy has performed well during 2003. GDP registered a 4.8% growth rate for the first 8 months of the year; the inflation rate continues to be one of the lowest in the region; the country has no more than a small deficit on its balance-of-payments current account; and its country risk is also one of the lowest among the Latin American economies. The economy does however, face challenges in 2 areas: the consolidation of its fiscal stability; and the achievement of sustained growth over the long term”.

### III. OUTPUTS

As we already mentioned, the original design of PROJoven contemplated to provide vocational training to 160,000 economically disadvantaged youths but unexpected supply and side constraints limited the scope of the program. Between 1996 and 2003, PROJoven provided vocational training to approximately 34,000 young people<sup>34</sup> in ten major cities across the country (Lima, Callao, Arequipa, Trujillo, Chiclayo, Cusco, Piura, Huancayo, Chimbote and Iquitos)<sup>35</sup>. The majority of these youths completed the technical training (96%) and 75% of those who went to the next stage finished the program (under the terms of a youth employment arrangement<sup>36</sup>). Also, during this period, a total of 428 ECAPS participated in the program; approximately 230 of them participated at least once in the program. These ECAPS provided 1,786 vocational courses (Table III.1).

**Table III.1: Evolution of PROJoven during the period 1996-2003**

Public call	Year	ECAPS	Courses	Beneficiaries					
				All		Graduates		Job-training	
				#	%	#	%	#	%
1	1996	14	75	1,505	96	1,450	96	1,201	83
2	1997	19	96	1,807	96	1,729	96	1,443	83
3	1998	22	122	2,243	96	2,146	96	1,762	82
4	1998	39	140	2,671	95	2,527	95	2,056	81
5	1999	42	171	3,075	96	2,945	96	2,267	77
6	2000	41	203	3,651	95	3,481	95	2,768	80
7	2001	59	220	4,178	97	4,052	97	3,106	77
8	2001	59	266	5,157	97	5,010	97	3,880	77
9	2002	77	292	5,942	97	5,788	97	4,668	81
10	2002	27	76	1,795	97	1,736	97	1,590	92
11	2003	29	125	2,312	96	2,226	96	NA	NA
<b>Total</b>		<b>428</b>	<b>1786</b>	<b>34,336</b>	<b>96</b>	<b>33,090</b>	<b>96</b>	<b>24,741</b>	<b>75</b>

Source: PROJoven

#### A. Impacts

Regarding PROJoven impacts for beneficiaries, the estimates obtained 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 that are study. Female youngsters and 16-20 year old 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. Also, 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.

<sup>34</sup> The program approximately spent US\$17 million during this period, with around 40% of this sum being drawn from local resources and the rest from the international cooperation.

<sup>35</sup> The direct cost by beneficiary was around US\$500.

<sup>36</sup> Under this agreement firms have to pay a minimum monthly wage. In 2003, the minimum monthly wage was around US\$120.

PROJoven has high positive impacts in terms of earnings. The DID estimates suggest that program impacts on monthly earnings and on censored monthly earnings seem unreasonably large when compared to international evidence.

Regarding the impacts of PROJoven on the vocational training market, the most important finding is that the upward trend in participation is associated to the incorporation of new locations outside of Lima. Surprisingly, in Lima the trend turns out slightly downward. This raises questions about the payoff for participation in the Program. The second important finding is that the largest group of participants, and the fastest growing outside of Lima, are in RECAP but do not bid for courses. This suggests motivations for participation that are different than obtaining courses. GRADE suggests that accreditation may be one important rationale and find some econometric evidence in support. However, they also suggest that more analysis of this phenomenon is needed.

There is a trend towards greater specialization of the ECAPs that participated in the program. Also, the evidence suggests that PROJoven has significant effects in furthering connections between ECAPs and firms. Also, ECAPs participating in PROJoven are of greater quality than those rejected by the Program.

## B. Impacts on the beneficiaries

### 1. 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) 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:<sup>37</sup>

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

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*.

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<sup>37</sup> 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  $D = 0$ .

### a) Sources of selection bias in PROJoven

In the context of PROJoven, there are at least two potential sources of selection bias. These factors are of particular importance in light of the findings in Nopo et al. (2001) on the Ashenfelter's dip for employment and earnings among PROJoven beneficiaries.

First, even when the control 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 (twice) to the Registration Centers to be recognized as eligible and to the ECAPs to be selected as beneficiaries. Thus, applicants –and beneficiaries in the treatment sample– may be systematically different from their control counterparts on those unobserved characteristics that make applicants more prone to seek treatment.

Second, the selection of beneficiaries depends on ECAPs criteria, 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 choose trainees, it is likely that the beneficiary group is systematically different from the rejected eligible group in some other characteristics. 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 screaming”.

Therefore, traditional cross-sectional methods (either regression or matching) need to be modified to identify program impacts without bias.

### b) Dealing with selection bias in the context of PROJoven

We will 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 control 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.<sup>38</sup> Denoting by  $X$  the relevant set of observable characteristics, the identification assumption can be expressed as  $Y_0 \perp D | X$  where the symbol  $\perp$  denotes independence, and  $Y_0$  is the observed outcome for control units.<sup>39</sup> 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:  $P(X) = \Pr(D = 1 | X)$ ); that is:

$$(Y_0 \perp D) | X \Rightarrow (Y_0 \perp D) | P(X)$$

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<sup>38</sup> 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).

<sup>39</sup> 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:

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

However, as we have discussed, in the context of PROJoven it is likely that program participation depends on both observable and unobservable characteristics, and thus, we can not impose CIA in its cross-sectional version.

A potential solution to the self-selection problem is to assume that the systematic and unobserved differences between treatment and control 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:

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

where  $t$  and  $t'$  represent the time periods after and before 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 there are systematic unobserved differences between treatment and control units, 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:

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

where  $\Delta_{DIDM}$  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  $s = t', t$ ,<sup>40</sup> and  $Y_{0si}$  represent the outcome for comparison units at time  $s$ .  $I_1$  and  $I_0$  denote the set of treatment and comparison units respectively,  $S$  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 will report estimates using the kernel matching version of propensity score matching. The kernel estimator matches treatment units to a kernel weighted average of comparison units. This can be thought as a non-parametric regression of the 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 and  $h_n$  is a bandwidth parameter.

The evaluation data from PROJoven is well suited for this strategy for two reasons. First, the quasi-experimental design of the evaluation data places comparison units (untreated potential beneficiaries) in the same local labor market as those in the treatment sample. Second, both treatment and comparison units were administered the same survey instrument, and so data from both types of units are fully comparable.

### c) Outcomes of interest

Our re-examination of PROJoven impacts will analyze several outcomes of interest to the program and funding agencies:

- a. **Employment.** This outcome indicates whether the youth is employed, no matter if she is working as an unpaid family workers (which are classified as working only when they work ten or more hours a week).
- b. **Paid employment.** This outcome variable indicates whether the youth is employed in a paid job.
- c. **Formal employment.** 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 pension fund (*seguro social*).
- d. **Earnings: monthly, hourly.** Earnings outcomes are constructed using information for the main job only. Hourly earnings are constructed by dividing monthly earnings by the four times the total number of hours worked a week. 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.

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<sup>40</sup> In the evaluation of PROJoven  $t$  would represent any of the follow-up periods and  $t'$  the baseline.

- e. **Working hours.** Weekly hours of work, computed using information for the main job.

**d) Conditioning variables**

In order to guarantee the conditional independence assumption, we will estimate a propensity score using a rich as possible set of conditioning variables. In this regard, we will follow the international literature and the experience from previous PROJoven evaluations to control for several pre-treatment characteristics such as:

- a. Demographic characteristics: gender, age, marital status, whether the individual have children, education.
- b. Background characteristics: poverty index, parents' education.
- c. Labor market history: labor experience, earnings history.

Of particular importance is the individual's prior employment history and the evaluation data from PROJoven provide information up to six months before treatment. We will exploit the availability of these employment histories to control for potential problems of selection bias as mentioned before. The international evidence reveals the existence of the Ashenfelter's dip before program participation. Heckman and Smith (2004) show, in the context of the JTPA, that controlling for employment trajectories is crucial to guarantee the conditional independence assumption in non-experimental studies of training programs.

**2. Evaluation data**

PROJoven has collected evaluation data on its first, second, fourth, sixth and eighth rounds of operation. These data were specially designed with the purpose of providing relevant information for the evaluation of PROJoven program impacts.

These data consist of a baseline survey –which records pre-treatment information– and three follow-up surveys carried out six, twelve and eighteen months after both the learning and on-the-job training components of the treatment concluded. This panel data structure is available for two samples of individuals in each of the rounds with an evaluation component:

- a. **The treatment sample.** From the universe of beneficiaries a stratified random sample is extracted, the stratification depends on gender, age and district of residence.
- b. **The control sample.** Once the treatment sample is selected, a sample of controls (eligible non-participants) 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 neighbors in the same block based on characteristics that would make them eligible. These individuals would otherwise be selected at the first stage of selection at PROJoven Registration Centers, which is guaranteed because during the field work the same protocols to select eligible individuals are used to choose control 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 three months prior to program enrollment.

The follow-up evaluation data 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.

#### **a) Employment history data**

Information on employment histories is of particular relevance in the current setting because of the possibility of an Ashenfelter's dip. Thus, controlling for the trajectories of employment status over the period before enrollment into PROJoven might be important.

In addition to the core questionnaires in the baseline and follow-up surveys, there exists a retrospective questionnaire on employment histories. This questionnaire is applied to both treatment and control units in the evaluation data, and gathers information for the previous six to nine months to the time of each evaluation survey. These employment histories contain month-by-month information on labor status that is, whether the individual was employed, looking for a job or out of the labor force in each of the previous months. For those employed at any given month, information on occupational category, firm size and monthly earnings is collected.

However, it is important to mention that there are differences in the way these data is collected with respect to the data collected using the core questionnaires of baseline and follow-up surveys. In particular, in the employment history section, youths are asked to provide self-reports on their employment status (employed, unemployed, out-of-the labor force), and employment characteristics at each previous month. In the core questionnaires, employment status is constructed using a set of questions designed to identify those who are working for pay, unpaid family workers but working more than ten hours a week, and unpaid family workers working less than ten hours, which are classified as unemployed. Additionally, to identify those who are unemployed, a set of questions are asked to verify that unemployed youths are actively looking for a job.

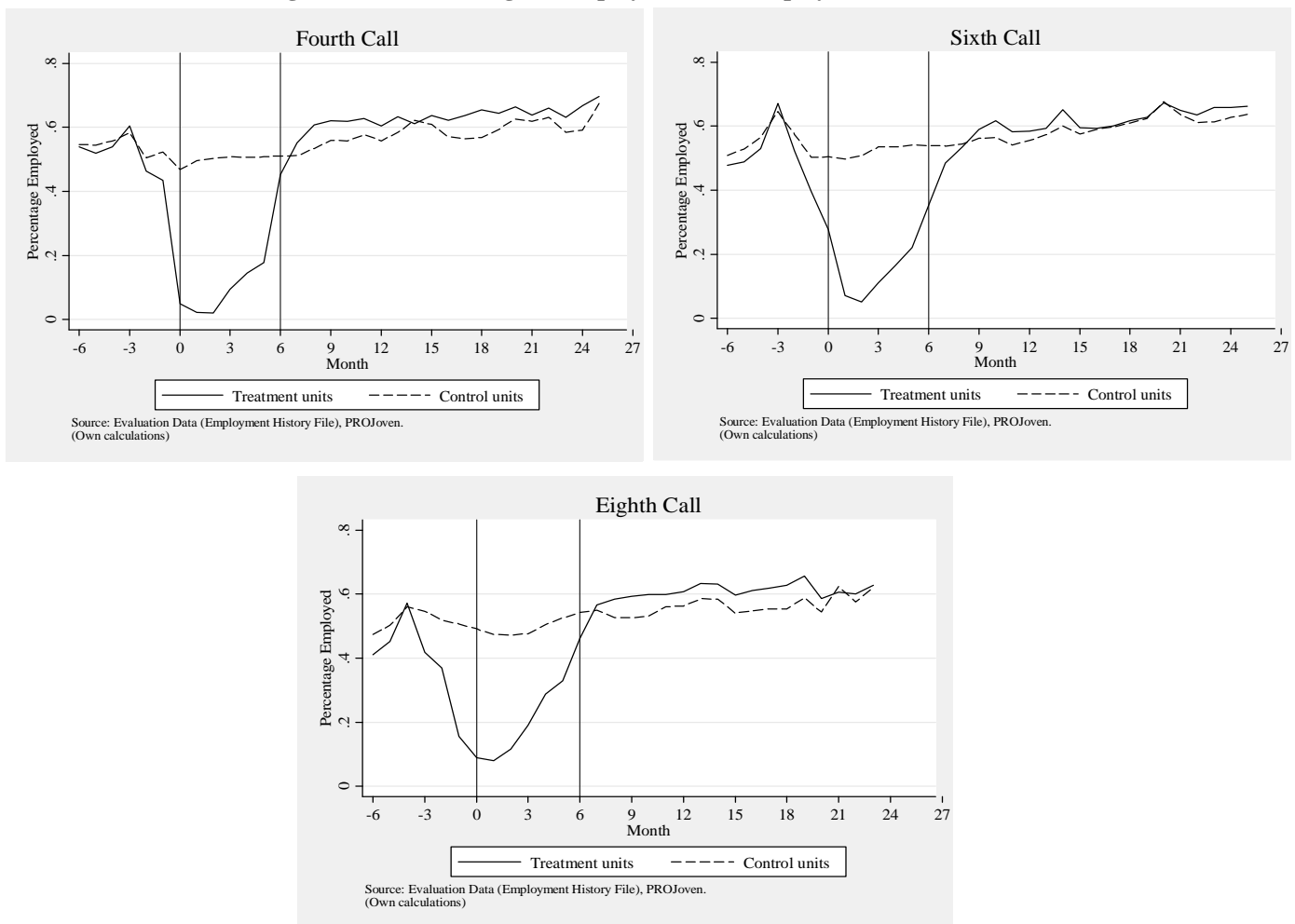
For public calls 4, 6, and 8, these data exist for the baseline and well as for the follow-up surveys. Unfortunately, for the first and second public calls data on employment histories are not available for the baseline surveys. For these evaluation data, employment histories exist in magnetic version only for the three follow-up surveys. During workshops with PROJoven's personnel we have been informed that the baseline employment histories questionnaires were applied but were never processed into a dataset. The reasons explaining or justifying this are unclear. Thus, it is not possible to control for previous employment history in the first and second public calls data.

Figure III.1 displays average employment rates for the treatment and control group, using the employment history data. The six-month period of the training (three months at ECAPs and three months at private firms) is marked by the two vertical lines. These employment rates suggest that employment fell in the months before the baseline survey, and so they reveal the existence of an

Ashenfelters’s Dip in these data. Using the employment history data available for the fourth, sixth, and eighth public calls we are able to construct dummy variables that indicate whether the each youth from the treatment and control groups is working or not every month covered by the baseline and follow-up surveys. Given that there are differences in the employment rates between treated and untreated youth before the training begins, these dummies will be included in the regressions to construct the propensity scores in order to “balance” the samples on these variables.

For these three calls for which we can use the retrospective information we find that the employment status variable obtained from the core baseline questionnaire differs from the employment status obtained from the retrospective questionnaire. In particular, the difference is more pronounced for the one to three month period before treatment begins where the percentage of employed beneficiaries decline while that of controls remain stable. We have argued that these differences might be related to the more stringent questions asked to interviewees in the core questionnaire. Thus, we should regard the less stringent information from the employment history file only as approximate measures. Nevertheless, an interesting point to raise here is that the after treatment trends are quite stable.

**Figure III.1: Percentage of Employed Youth Employment histories data**



## **b) Advantages of PROJoven evaluation data**

These evaluation data provide several advantages.

First, the control group is chosen using the same protocol applied to identify eligible youngster.

Second, both treatment and control group individuals come from the same neighborhoods. In this regard it is likely that they both face the same distances and transportation costs to PROJoven headquarters or recruitment centers and also to their potential place or work. 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 treatment and control 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 timing of the baseline and follow-up surveys is the same for the treatment and control units. Thus the possibility of timing biases, such as seasonal differences in earnings or employment, are minimized.

## **c) Disadvantages of PROJoven evaluation data**

It has to be recognized that the data also entail some disadvantages. First, even when the control 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 (twice) to the Registration Centers to be recognized as eligible and to the ECAPs to be selected as beneficiaries. Thus, applicants –and beneficiaries in the treatment sample– may be systematically different from their control counterparts on those unobserved characteristics that make applicants more prone to seek treatment.

Second, the selection of beneficiaries depends on ECAPs own procedures, 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 choose trainees, it is likely that the beneficiary group is systematically different from the rejected eligible group in some other characteristics. 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's likely that ECAPs perform some sort of “cream screaming”.

Third, the procedures to choose a control when there is none in the same neighborhood block of the treatment counterpart or when there are multiple controls to choose from are unclear. Finally, 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 to find good quality controls.

As we will report later, even when the data balance treated and control units in terms of some observable characteristics, there are statistically significant differences in terms of monthly earnings in the baseline surveys between treated and controls in all the public calls' evaluation

data. In particular, the data show that at the time of the baseline survey, real monthly earnings for the treatment group are consistently lower than those of their control counterparts across all public calls. 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 than an average potential program beneficiary.

We think there are also other possible explanations for these differences. The first is that the targeting mechanism implemented to select program beneficiaries effectively "cream skims" the poorest youth that show up at PROJoven. When field workers go to the neighborhood of the treatment sample to find the controls, it is quite possible to find similar youth but they will, on average, have higher monthly earnings than program beneficiaries. Remember that the targeting mechanism of PROJoven allows for an initial self-selection of youngsters into the eligible group. It is possible that from the universe of those who otherwise would qualify as eligible based on the socioeconomic criteria used by PROJoven, only a particular sample decide to register at the program. These could be the more motivated, but also those with the lower quality jobs or worse labor market prospects. A second possible explanation, related to our first claim, is that some key variables such as whether the youngster has a paid job or not, and her earnings and hours of work, are not used to select the control units. This is so because the operative costs of finding a quasi experimental clone increase as the number of matching characteristics increases. A third potential explanation is related to the time when evaluation data are collected. For treated youngsters these data are usually recorded during the initial weeks of courses at ECAPs centers and collect information about the youngster characteristics and labor market outcomes during the month before treatment. The fieldwork to find control units and to collect their baseline information is carried out at the same calendar time, also asking for the information during previous month just as for the treated. However, given that in many cases program beneficiaries already know that they have been selected as such, their employment status and earnings may be observationally different just because they leave low quality jobs once they realize they have been admitted at 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.

We suggest two ways to overcome the problems we have discussed. The first, and usually the preferred solution to these problems, is to undertake an experimental evaluation. The experimental design avoids the problems of self-selection, both in terms of observable and unobservable characteristics. But it also has its own problems and limitations too. Here the key questions are how and when to implement the randomization of eligible units into the treatment and control groups, especially because PROJoven only selects eligible youngsters and the final decision of whether an eligible youngster becomes a program beneficiary is a decision of the ECAPs which behave strategically to select the best candidates. 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 randomize beneficiaries instead of selecting them. Additionally, given that under an experimental design the control group is denied treatment, it is quite possible that gathering baseline and follow-up information could imply some extra difficulties as randomized out youngsters may not want to cooperate. Finally, the total cost of implementing an experimental evaluation may be higher than the current costs of pursuing the quasi experimental evaluations. This should also be considered as a decision variable.

The second venue is to remain under the quasi-experimental design but to improve the way controls are selected by increasing the number of matching characteristics, and to increase the size of the control 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 control 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 differences between these 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.

Given the way the selection of eligible and beneficiaries is designed, one open possibility that should not imply large 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 control groups under the current design, these youngsters are equivalent in their motivation to participate in PROJoven and are also equivalent in the observables that accredited them as potential beneficiaries. 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. However, 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 may be used later in the evaluation as instruments for program participation, that would allow to implement 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 controls is that PROJoven could apply the baseline survey two or three months before training begins and before eligible youngsters realize whether they have granted a course vacant, at the same time the socioeconomic information used to grant eligibility is collected by program officials.

#### **d) Construction of panels**

To control for potential biases arising from selection on unobserved characteristics, we have proposed a difference-in-difference (DID) procedure. To this end we have constructed panels for all the evaluation datasets available to us. For any given public call with evaluation data the procedure we implemented is as follows:

- a. Extract relevant information (outcomes variables and individual characteristics) from each survey dataset for treatment and control units. (Datasets are organized by public call, city, baseline vs. follow-ups, and treatment vs. control groups).
- b. From the Registry of Eligible and Beneficiaries, extract data on individual characteristics for the pre-treatment period that is not contained in the baseline evaluation data (most of the variables used in the accreditation process) and merge to data for the treatment sample.
- c. Merge baseline to follow-up data for treatment and control units separately.

- d. Stack together the panels for treatment and control units.
- e. Once the overall panel is put together we merge it to the employment histories dataset when the latter data exit.
- f. We retain individuals that are present in all the four evaluation surveys and have non-missing information on the employment histories dataset.

The protocol just described was applied separately by city when applicable (4<sup>th</sup>, 6<sup>th</sup> 8<sup>th</sup> public calls).

We exploit the panel dimension of the evaluation data to implement the longitudinal version of propensity score matching. However, we also compare the results from this procedure to those obtained from unconditional (naive) and linear regression DID methods. The rationale for these comparisons is that each of these non-experimental methods imposes different assumptions to the data generating process. 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.

### 3. Results

We exploit the panel dimension of the evaluation data to implement the longitudinal version of propensity score matching. However, given that the evaluation data are non-experimental, we also report cross-sectional (CS) estimates to compare the results from this procedure to those obtained from the DID method and also using linear regressions instead of matching (results reported in the appendix).<sup>41</sup> 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. As we mentioned earlier, the control units in the evaluation data are similar to the treatment units in terms of age, gender, education, and employment status (employed, unemployed, out-of the labor force) at the time of the baseline survey. The equivalence in terms of age, gender, education and employment status is the result of the instrument design to find control units.

We begin by reporting time trends for the outcome variables between evaluation surveys and across public calls. Figures A.1-3 plot the outcome variables of interest for the baseline and the three follow-up surveys (denoted by Post-1, Post-2, and Post-3). For almost all of the outcomes, (paid jobs, formal jobs, monthly earnings, hourly earning and hours of work per month) the values these variables take for treatment units are below or very near to those for controls in the baseline survey. However, treatment units perform better than control units in the follow-up surveys. In the latter surveys, the outcomes for treatment units are above of those for control units.

A first important thing to bear in mind to interpret the results presented later is that during the first public call there were many more unpaid family workers in the treatment group than in the

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<sup>41</sup> We perform conditional linear DID and CS regressions using the same conditioning variables as in the propensity scores. We arrive at the same qualitative conclusions using these alternative methods.



control group. The information from the baseline survey show that 35% of youngsters in the treatment group had a paid job, while nearly 50% among youngsters in the control group were paid workers. However, six months after the practical stage of PROJoven has been completed, these figures turned to 59 and 55% respectively. In the second follow-up the percentage of paid job youngsters in the control group, begin to fall, this also happens for the treatment group but in the third follow-up survey. Nevertheless, the final values of these percentages are above their initial values in the baseline for both groups, but the interesting point is that the percentage of paid jobs among treatment units remains above than the corresponding figure for control units. A similar pattern emerges in the following public calls, with the difference that the initial gap in paid employment between treatment and control units was much lower.

A second important thing to keep in mind is that both treatment and control units have similar probabilities of being employed in the formal sector. We use the legal definition of formal employment as being a job with any of the following characteristics: whether the employee signed a formal employment contract, she receives health insurance, or she has entitlement to a retirement fund pension. After participating in PROJoven, program beneficiaries experienced an increase in the likelihood of being formally employed relative to the control group.

#### **a) Kernel matching difference-in-difference estimates**

We report the results of the difference-in-difference and cross-section estimates version of propensity score matching in Tables A.1 to A.5 applied to the evaluation data from the five public calls we are analyzing. Each set of tables report the DID and CS estimates, for discrete outcomes such as being employed or not, we report the difference in the estimated probability. For continuous outcomes such as earnings and hours of work, we report the point estimates of the difference between treatment and controls youths as well as the percent effect computed as the point estimate divided by the average of the corresponding outcome variable in the control group.<sup>42</sup> When dealing with the DID point estimates we use the average of the relevant outcome for the control group in the baseline survey to compute the percent effect. When dealing with the CS point estimates we use the average of the outcome for control group in the corresponding follow-up survey to compute the percent effect. We report estimates for the full sample, and explore impact heterogeneity by gender and age.

All the estimates we present used the Epanechnikov kernel to compute counterfactuals. Regarding the common support, we impose the common support condition following the procedure proposed by Heckman, Ichimura and Todd (1997, 1998) and Heckman, Ichimura, Smith and Todd (1998). In particular, we compute our estimates on the support region defined by a trimming rule of 5%. Bootstrapped standard errors, based on 200 replications are reported in parentheses, percent effects for continuous outcomes are reported in *italics* face..

#### **b) Employment rates**

We first explore the effects of program participation on employment rates. Panel (A) from Table A.1 reports the DID kernel estimates for the full sample of each public call. We find no

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<sup>42</sup> That is, we compute the percent gain (or loss) in the outcomes for the treated with respect to their control counterparts.

statistically significant effects of PROJoven on employment in the first, fourth, and sixth public calls. In the second public call, the only positive and statistically significant estimate is that for the second follow-up survey (twelve months after the training), which represents an increase of 11% on the employment probability for program beneficiaries. In the eighth public call, we find positive and statistically significant effects both in the first and second follow-up surveys. These estimates suggest that for beneficiaries from the eighth public call, employment probabilities increased 8.6% by the first follow-up (six months after the on-the-job training stage) and 7.3% by the second follow-up with respect to control units between as a consequence of participation in PROJoven.

For males, we find no statistically significant program effects on employment for any of the five public calls we analyze as results from Table A.2 indicates. However, for female youths we do find statistically significant PROJoven effects on employment in the second, sixth and eighth public calls, as the estimates from Table A.3 suggest. During the second public call, the probability of employment increased by 21% in the first follow-up and by 19% in the second follow-up; the estimate for third follow-up is also positive but the standard error is high enough to make it statistically significant only at the 15% level. In the sixth public call, we estimate that employment probabilities increase by 7.3% between the baseline and first follow-up, and 8.9% between the baseline and first third-up, these two estimates are both statistically significant at conventional levels. For the eighth public call we also find positive and statistically significant program impacts on employment probabilities, these increase by 13% by the first follow-up and 11% by the second follow-up. When splitting the sample by age groups, we do not find employment effects, neither for 16-20 year olds nor for 21-25 year olds.

### **c) Paid employment**

In Peru as in many other developing countries, there is a large fraction of people engaged in unpaid family jobs, particularly among youths. Thus, many of those who are counted as employed, are unpaid workers. In panel (B) of Tables A.1 to A.5 we explore the impacts of PROJoven on paid employment, that is, excluding from the definition of employment previously analyzed those youths who were working as unpaid family workers. For this outcome, we find positive and statistically significant program impacts in all the public calls except the fourth. After program participation, beneficiaries from the first public call experienced an increase in their probabilities of being employed as paid workers. The statistically significant point estimates are 17%, 19% and 20% in the first, second and third follow-up surveys respectively. In the second public call, the paid job probabilities increase by 9 and 14% between the baseline and first and second follow-up surveys respectively. In the sixth and eighth public calls, these probabilities increase by 5 to 8% on average.

When we split the sample by gender, we do not find statistically significant effects on paid employment for male youths. However, for females we find that PROJoven boosts the probabilities of being a paid worker with positive and large effects, see results reported on panel (B) from Table A.3. These results are consistent with the fact that among women there is a larger fraction of unpaid family workers than among men. We also find that PROJoven has positive impacts on the probability of being a paid worker for 16-20 year olds (first, second, sixth and eighth calls) but not for 21-25 year old, both using DID and CS estimators. This is expected as long because younger individuals likely have lower labor market experience, so the program

might have bigger impacts among them than among individual (relatively) more acquainted with the labor market.

#### **d) Quality of employment: formal jobs versus informal jobs**

We also explore the change on quality of employment conditions after PROJoven participation using a definition of formal jobs. We define formal jobs, as is common in the literature on the labor market in Peru, as those with either a formal contract, with access to social security, or with accident, or health insurance. Panel (C) from Table A.1 reports the kernel matching estimates on this outcome for the full sample of treatment and control units. For all the public calls under study, we find positive and statistically significant effects of PROJoven on the probabilities of being a formal worker. Depending on the specific public call and the baseline/follow-up DID or CS comparisons the estimated program effects on formal jobs probabilities vary in a range between 7 to 18%. For instance, in the first public call the probability of having a formal job after PROJoven for program beneficiaries with respect to untreated youth increases by 18% in the first follow-up, 15% in the second follow-up, and 13% in the third follow-up. Our estimates also suggest that the effects of PROJoven on formal employment probabilities decrease from the initial to later calls, although the point estimates remain positive and statistically significant. In the eighth public call, the increase on formal jobs probabilities for program beneficiaries after participation in PROJoven was 7% in the first follow-up and 9% in the second follow-up.

An important factor that may help explain these results is that when PROJoven beneficiaries get their internships, the internship firms must hire them under the legal terms of the Agreement of Youth Labor Training (*convenio de formación laboral juvenil*). These agreements are in place to promote youth labor training by allowing firms to hire youths 16 to 25 under special conditions regarding the labor contract. These agreements replace the labor contract, do not grant social security, but cover 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 firms. Given that many of PROJoven beneficiaries are still working for their internship firms under these agreements during the first follow-up surveys six months after having completed the training provided by PROJoven, it is expected to observe positive impacts in terms of formal employment as well as in terms of paid employment and earnings as we will comment on later.

However, the positive effects we find on formal employment are not only explained by the presence of these agreements. One important finding is that in every public call under study, the positive program impact on formal employment probabilities remains positive and statistically significant, even after 18 months after beneficiaries completed the training, a period when the agreements with the internship firms must have expired. Despite that in almost all the calls we find a declining pattern on the gains in terms of formal employment, these impacts did not completely fade out. We argue that even if the program does not contribute to increase beneficiaries' productivity, it is likely that having worked for a legal firm contributes to the likelihood of getting better jobs for program beneficiaries.

Splitting the sample by gender (Tables A.2 and A.3), we find positive and statistically significant effects for both male and females beneficiaries with respect to their control counterparts. However, our estimates suggest that the effects in terms of formal employment were higher for treated women than for men. These results are consistent with the fact that youth women face worse labor conditions. Therefore, it is likely that women benefit the most. On the other hand, when we explore treatment effect heterogeneity by age (Tables A.4 and A.5), we find that PROJoven has slightly bigger impacts on the probabilities of being in a formal job among 16-20 year olds than among 21-25 year olds.

**e) Continuous outcomes: monthly and hourly earnings and weekly working hours**

For the continuous outcomes, monthly and hourly earnings, and weekly working hours, we estimate program effects in terms of both uncensored and censored outcomes. We also provide estimates computed the as percent effect by dividing the point estimate by the average outcome for the control group. We estimate program effects using censored outcomes because both in the treatment and control groups we have individuals that were unpaid workers, unemployed or out of the labor force during the surveys. For these individuals, obviously, there are no data on earnings or hours of work. We report our estimates for those who were employed during the surveys and reported positive earnings and hours of work (uncensored outcomes); for unpaid the unemployed or out of the labor force individuals we treat these outcomes as missing. We also report estimates for the whole sample of treated and control units. To do this, we set the outcome equal to zero when the youth was not working (censored at zero outcomes) and compute the DID and CS kernel estimators over the whole set of observations. The estimated program impacts using censored earnings and hours of work account both for the effect on earnings and hours, and for the employment and employment quality effects of the program. Panels (D), (E), and (F) report the estimated effects for monthly earnings, hourly earnings, and weekly hours worked respectively, using the uncensored variables. Panels (G), (H), and (I), report the estimated effects for the censored variables.

In general we find that PROJoven has high positive and statistically significant effects in terms of monthly earnings (uncensored) across all the public calls we study, both using the DID and CS estimators. However the CS estimates are lower than the DID estimates. This is expected because pre-treatment earnings are much lower among treatment units than among their control counterparts in part because a higher proportion of beneficiaries were unpaid family workers before.

However, it is important to comment on the magnitude of the estimated program effects we find that compared to international standards are large. In particular, we find that for the first public call the impact of PROJoven on monthly earnings was the highest. The estimated DID program effects were 60% 6 months after treatment, 44% after 12 month of treatment, and 39% after 18 months after treatment. The corresponding CS estimates were 30%, 12%, and 7% (not statistically significant), respectively. For the other public calls we also find large positive and statistically program effects on earnings, except in the sixth call where CS were not statistically significant. We also find that the effect on earnings does not disappear when going from the first to the third follow-up surveys.

Our estimates also suggest that the program impacts on uncensored monthly earnings measured as a percent affect decrease between the first and fourth calls, and the increase in the sixth and eighth calls when using the first follow-up. However, the time patterns of earnings gains are less clear when using the second and third follow-up surveys.

When splitting the sample by gender we find that both the point estimates and the percent effect estimates of PROJoven impacts are higher for female youths than for their male counterparts. For males, the estimates suggest that there are no statistically significant effects in the second and fourth public calls. On the other hand, for females, we find statistically significant and positive program effects for every public call. Among females, 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 16-20 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 ever found. Is it possible to reconcile results from the international experience with the experience of PROJoven? Could six month of training be so effective? We argue that several factors may help explain these results.

A first important feature of the program is that its beneficiaries targeted to be among the poorest youngsters from urban areas. Even if no productivity effects materialize, having participated in the program may induce some sort of credentialism effect. On the other hand, by design the training courses are designed to provide beneficiaries skills required by firms from the productive sector. The relationship between courses' contents and actual firms' demand for skills is reinforced by the internship stage of the training. Remember that participating firms must pay the trainees, so they are not free labor. This concordance between the skills ECAPs teach their students and actual requirements on human resources quality might be generating real improvements in productivity for program beneficiaries so these youngsters are likely to meet labor demand requirements at real firms.

On the other hand, given that beneficiaries are hired under the aforementioned agreements of youth labor training, which imply otherwise better quality jobs than those accessible to poor youngsters. These agreements not only improve on employment conditions (health and accident insurance coverage, etc.), but also imply that 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 the program may entail a credentials effect instead of or in addition to potential productivity enhancement. In this sense, the program may help beneficiaries to engage in formal jobs, instead of unpaid family jobs or sporadic informal jobs. This is of particular importance for females and kids that have recently completed basic education and have no labor experience in a formal private firm at all.

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 facts that some beneficiaries would still be working in their internship firm under the youth labor agreement 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 monthly earnings.

In terms of hourly earnings (uncensored), we also find positive and statistically significant effects of PROJoven, both in terms of the DID and CS estimates. However we find, in general, fewer statistically significant effects on hourly earnings compared to monthly earnings. When looking at different subgroups, we find that these estimates are higher for women and for 16-20 year olds. On the other hand, when looking at the censored hourly earnings for the overall sample, we find that most of the estimates are statistically significant and positive. Again, we find lower CS estimates but these are still positive and statistically significant. When splitting by gender we find that for men beneficiaries only the DID estimates for the first, sixth, and eight calls are statistically significant, no effect is found for the second and fourth public calls nor using the CS estimates. On the contrary, for women we do find high positive and statistically significant program impacts, 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%. We also find that the program is most effective among 16-20 year olds beneficiaries.

For uncensored weekly hours of work, panel F in the tables, we find positive and statistically significant PROJoven impacts in the overall sample using the DID estimator in the first and eight calls for all the baseline/follow-up comparisons. 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 estimate we find correspond to the first follow-up of the eight 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 youngsters we find positive impacts on hours for the first and eight public calls, in particular in the first follow-up both the DID and CS estimates are positive 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 eight calls for the 16-20 cohort and for the fourth and sixth calls for the 21-25 cohort.

Finally, for censored hours of work we find positive estimates for the overall sample but not for every follow-up survey. For instance, only find positive and statistically significant DID and CS estimated effects on censored hours during the first public call using the third follow-up data, during the second and fourth public calls using the second follow-up data, and during the eight public call using the first and second follow-up data. We also find treatment effect heterogeneity by gender: for men beneficiaries we find that statistically significant estimates are negative,

while for women beneficiaries are 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 eight call when no effects are found for the 21-25 year olds.

#### **f) Additional discussion**

After reviewing the evidence provided by PROJoven evaluation data we conclude that despite that the quasi-experimental design allows to get a control group statistically equivalent to treated youngsters on some relevant observable characteristics, it is likely that self-selection on the part of beneficiaries and selection on other unobservable characteristics (to us) on the part of ECAPs might be important. We use DID methods in an attempt to remove any time invariant secular trend between treated and controls that may confound the true program effects. However, we also find that pre-treatment differences on earnings between treated and controls may be the result of the timing of the baseline survey and reference periods used to collect pre-treatment data. If we assume that the difference in pre-treatment earnings between beneficiaries and controls are only born by this timing, the DID estimates overestimate program impacts. If these are only mechanical differences and not the result of selection on unobservables, CS estimates are cleaner. Using the CS estimators we still find positive but smaller program effects estimates on earnings.

However, there is still the possibility that the control quasi-experimental control group was not equivalent to the treated group. We have discussed earlier on the way controls are obtained and some suggestions on how to improve the quasi-experimental design. We also believe that after ten years of operation and all the evidence gathered on the PROJoven experience, it is necessary to improve its evaluation system. We believe that is extremely important to improve on the construction of the control group. One possibility is to move to an experimental design, but this would imply to modify the way PROJoven operates and its relationship with ECAPs. 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.

Additionally, we believe that it is necessary to designing 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 valuation data we use to perform this study. For instance, it is no clear that there exist an official version of these data. We have received up to four different versions of the data depending on the public call, involving different numbers of observations or with an incomplete list of variables. 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 its information. Sometimes what information or database is stored depends on decisions made in a call-by-call fashion. We believe this could easily be improved by the program.

We will see later in the document that the new stage of PROJoven lost the opportunity to introduce relevant modifications on the data and on the evaluation component of the program.

### **C. 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.

#### **1. Benefits data**

We estimate two types of benefits. First, 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, benefits perceived because of participation in PROJoven, that is the gains in terms of employment opportunities and earnings for beneficiaries with respect to the control 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 months.

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% rate per year; under a neutral scenario, we assume that benefits decrease at a 25% rate per year; while under an optimistic scenario, we assume that benefits decrease at a 10% rate per year.



**Table III.2: Per capita benefits and costs, and Internal Rates of Return**

	Public Call				
	First	Second	Fourth	Sixth	Eight
<b>A Beneficiaries</b>	1,505	1,807	2,671	3,651	5,157
<b>B Benefits 1</b>					
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	-	30
Post-2 CS	49	101	29	-	40
Post-3 CS	40	41	50	-	40
<b>C Costs 2</b>					
Operative costs	2,682	2,670	2,202	1,427	1,085
Stipends given to beneficiaries	467	439	385	419	243
Opportunity costs	63	89	91	124	79
<b>D Internal Rate of Return using DID estimates of benefits</b>					
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
<b>E Internal Rate of Return using CS estimates of benefits</b>					
Pessimistic 3	0.0	0.0	0.0	0.0	1.7
Neutral 4	4.0	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% rate per year.

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

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

## 4. Results

Table III.2 presents the results of our calculations. Even in the pessimistic scenario the simulated IRR are above 4%. 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% (1996), 22.5 and 3.3% (1997), 29.3 and 4.3% (1998), 22.0 and 3.7% (2000), and 23.2 and 3.1% (2001).

#### **D. Impact on the vocational training market**

One open question is how PROjoven may be affecting the vocational training market. A major goal of PROjoven is to provide incentives to make this market more dynamic and to improve the overall quality of training services. It is likely that these potential benefits vary across regional labor and vocational training markets, and so it is of interest to have an idea of what is happening along these dimensions. We intended to investigate whether the composition of participating vocational training institutions is changing over time and in which directions. We addressed questions such as: Are individual ECAPs reorienting their course offerings as a consequence of PROjoven's intervention? Have they diversified their course offerings or, alternatively, have they tended to specialize? Have new ECAPs emerged as a consequence of PROjoven? In addition, we did cross-sectional analysis of the quality of ECAPs participating in PROjoven and its effects on the outcomes of the Program. In this direction we constructed quality indices, which we then plugged into the programs outcomes equations. This allowed us to examine the impact of the quality of training institutions on PROjoven's results. Thus, we conducted a longitudinal and a cross-sectional analysis, both based on quantitative data. The entire analysis is display in the final report of GRADE.

The main results of the longitudinal analysis are: (1) there is an upward trend in participation that is associated to the incorporation of news locations outside Lima. Surprisingly, in Lima the trend turns out slightly downward. This raises questions about the payoff for participation in the program. (2) The largest group of participants, and the fastest growing outside Lima, are in RECAP but do not bid for courses. This suggests motivations for participation that are different than obtaining courses; accreditation may be one important rationale and find some econometric evidence in support. (3) Participation by type of training institution is dominated by CEOs, ISTs and NGOs. The CEOs is the largest group while the other two groups has similar share in the number of institutions participating. Together these three groups account for about 2/3 of total participating institutions. (4) Smaller institutions (with less than 20 employees) get a small share of courses, slightly above 6%<sup>43</sup>.

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<sup>43</sup> There is only one study on the firms that participated in the Program (Projoven, 2004), and it is limited to a sample of firms that participated in the 11<sup>th</sup> call. The study lacks a comprehensive analysis of the demand side and its overall quality is doubtful. The main objective of the study is: "to know and analyze the perception of the businessman about to the program across the performance of the youths in the company and the existing link between the ECAP and the company". In fact, the authors randomly surveyed 60 firms selected from the universe of 281 firms that participated in the call 11. The random selection was stratified by economic sector. Three economic sectors were selected: textiles and confections (54), administration (37) and manufacture services (8). The main results of the survey are: (1) the medium and large firms correspond to 43% of the total number of firms; (2) for 30% of the firms it was their first participation in the program while 43% had been working between 1 and 3 years; (3) 65% of the firms received 1-5 beneficiaries of the program; (4) 52% of the firms declared that the youths remained more than 90 days; (5) Surprisingly, 10% of the large firms didn't pay their trainees; (6) the majority of firms in the 3 economic sectors considered that level of generic competencies of the trainees was good; (7) 63% of the firms learned about the program from the ECAPS; (8) 70% of firms didn't have any participation in the

There are significant differences in several aspects, particularly in links with firms and management and curricular aspects between training institutions participating and not participating in PROjoven. The evidence presented suggests that the quality of firms participating in PROjoven is greater than that of those not accepted in the RECAP. Among those participating it is harder to find differences, particularly between bidders that are awarded courses and those that are not. Between bidders and non-bidders links with firms is the most important factor behind their differences, those bidding having greater links with firms. This is an expected result since PROjoven's rules require bidders to have established links with firms, at least as far as written commitments to offer internships. What this analysis underlines, however, and that in the previous subsection as well, is that these links go beyond this aspect. Generally, the intensity of involvement with firms is much greater among bidders than non-bidders.

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elaboration of the courses offered by the ECAPS; (9) 62% of the firms received a monitoring telephone call from the ECAPS.

## IV. THE PROGRAM PE0241

As we already mention the Peruvian government requested the IDB support in order to finance a new stage of PROJOVEN. The program PE0241 was approved in 2004 and started its implementation in 2005. The program has suffered important delays in its execution and in two years it has only disbursed 10% of its approval amount. Most of these delays are related to political and institutional problems that impeached a normal execution.

This section put emphasis on the analysis of the main modifications that were introduced in the original design of PROJOVEN in order to improve its results and impacts, the ex-ante evaluability and the implementation results, mainly the main problems that are affecting the execution.

Overall the PE0241 introduced some modifications and increased the scope and the coverage of the program. For example, labor intermediation activities were considered. However, even if the scope and coverage of the new stage were increased they stayed relatively modest compare to the size of the target population and the importance of the development problem. The PE0241 did not try to transform the overall training system. The LD only mentions that in order to improve the efficiency and transparency of training it is also necessary to complement the private training with: “a public regulatory structure responsible for the setting and applying relevance and quality standards to be met by the training programs”<sup>44</sup>. In fact, it was maybe more pertinent to help the country to develop an efficient national training system based on the lessons learned from the first stage of PROjovent and also from other countries (such as the Chilean experience)<sup>45</sup>.

The program did not solve important problems of the original design. Particularly, the PE0241 did not consider the institutionalization of the program and its long-term sustainability. Moreover, the new stage did not improve the evaluation component of the program neither the problems of the evaluation data.

### A. Modifications introduced to the original design

#### 1. Objectives

The LD of the PE0241 mentions that the new program will: “have the same fundamental objectives as PROjovent, but its coverage will be broadened and the design improvements will be made with a view to increasing its impact”<sup>46</sup>. According to the LD the main reasons for the IDB’s participation in this new program are: (i) given the bad quality of the education, the returns of professional training are important, (ii) many of the economically disadvantage youths do not have access to training and those who have access receive a low quality training; (iii) the assessments made to PROjovent show positive impacts on employment and income indicators;

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<sup>44</sup> Paragraph 1.29.

<sup>45</sup> For example, the program could plan to condition the disbursements to improvements of the overall training system or to the implementation of national training policies.

<sup>46</sup> Paragraph 2.1.

(iv) the model of the program can be applied to the overall labor training system; and (v) the program can be expanded because there are at least 428,000 potential beneficiaries.

The main objective of the program is: “to help provide economically disadvantaged young people between the ages of 16 and 24 with access to the formal market (...). The program is also intended to enhance the competence and market efficiency of training institutions by promoting greater interaction between those institutions and businesses in the productive sector. The program’s specific objectives are to help raise the labor participation rate, job quality, and wage levels of young people in the target group”.

The beneficiaries have to be young people with little or no working experience, who are unemployed, underemployed or inactive. The program is open to youths with complete and incomplete secondary education and with less than 480 hours of training. The number of beneficiaries was set to 47,000 in five years. The coverage of the program was also expanded to the following cities: Cajamarca, Ica, Juliaca, Huanuco, Tarapoto, and Ayacucho.

## **2. Components**

The LD identified that it was necessary to introduce some changes in the original design in order to have better results and impacts. Thus, the PE0241 introduced some modifications. Especially, few changes were considered in order to improve the quality of the training, the job placement, the targeting process and the evaluation component:

- a. New mechanisms in order to enhance the quality and impact of training services based on closer ties among the program, the ECAPS, and the firms. The new stage of PROjoven put emphasis on ensuring a lower dropout rate during the on-the-job training phase. The following measures were adopted: (i) a new payment system where the ECAPS have to pay 50% at the end of each program’s phase, (ii) a more flexible labor agreement whereby trainees are able to work under young training agreement for shifts of minimum of 6 hours per day; (iii) a more efficient roster of ECAPS by improving the feedbacks procedures; (iv) to monitor the on-the-job training phase; and (v) technical assistance to strengthen the capability of the monitoring team of the program.
- b. To improve the self-targeting process and to increase the job placement rate of beneficiaries by the introduction of labor intermediation and liaison services. According to the LD, the national intermediation system (SIL)<sup>47</sup> has to provide: “a predetermined sequence of activities, job-search coaching, and job matching services to the program in order to help its beneficiaries position themselves in the labor market”<sup>48</sup>.
- c. In order to reduce the withdrawal of participants and to expand efficiently the coverage of the program measures were implemented to strengthen and improve targeting. Particularly, the new program has to reinforce ties between the program

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<sup>47</sup> The SIL is under the responsibility of the Red-CIL-PROempleo and is structured on the basis of a network composed of 17 private and public intermediations centers.

<sup>48</sup> Paragraph 1.40.

and the community organizations, improve the candidate registration forms and the procedures for monitoring the selection and the accreditation process.

- d. To improve the evaluation component by the introduction of a diagnostic appraisal and outcome evaluations. Particularly, new tests are introduced in order to measure the value added by each training phase in terms of skills and work attitudes. On the other hand, if the impact evaluation component has to continue using a non-experimental design, few modifications were introduced to reduce the bias generated by the unobservable characteristics.

The PE0241 has the following components and sub-components:

**a) Component 1: Vocational instruction in ECAPS and businesses**

The objective is to provide labor training to beneficiaries in ECAPS and firms.

The program plans to offer 2,350 courses for 45,600 young people in the technical phase. It is estimated<sup>49</sup> that 85% of the beneficiaries who complete this phase will continue to the next phase of the program (approximately 38,750 youths). The stipends to be paid per class attended are fixed at US\$1.40 for the participants in Lima and at US\$0.80 for trainees outside of Lima. Additional subsidies, of US\$1.10 for Lima and of US\$0.80 elsewhere, are also planned for the beneficiaries who are mothers.

**b) Component 2: Selection, monitoring, and follow-up of ECAPS**

This component includes the following three subcomponents: (i) preparation of a roster of ECAPS and course selection; (ii) monitoring and oversight of ECAPS; and (iii) feedback and technical assistance.

**c) Component 3: Labor intermediation and information**

The objective is: “to provide program beneficiaries with labor intermediation and information services as a means of reducing the overall cost (in terms of time and accuracy) of matching up young job seekers with positions that become available as a function of demand on the labor market. This effort is expected to reduce the amount of time that individual job seekers are unemployed, the employers’ recruitment costs, and the non-functional staff turnover caused by the placement of people in positions for which they are not qualified”<sup>50</sup>.

This component includes the following subcomponents: (i) information activities; (2) job-search coaching; (iii) job matching assistance; and (iv) technical assistance in connection with the SIL.

**d) Component 4: Evaluation and monitoring**

The objective is: “to evaluate the program’s outputs, outcomes, and impacts”.

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<sup>49</sup> The LD does not explain how these estimations were made.

<sup>50</sup> Paragraph 2.15.

This component will include the following components: (i) evaluation of outputs; (ii) evaluation of outcomes; and (iii) evaluation of impacts. The LD mentions that the baseline for these evaluations will be constructed from the information gathered at the beginning of the program execution.

The evaluation of outcomes will include different tests. First a test of generic, crosscutting competencies will be administered to a sample group of beneficiaries for at least two rounds. Two other tests will measure the acquisition of basic competencies by the beneficiaries such as: “(i) the ability to understand and follow written, oral and symbolic instructions; and (ii) the ability to solve the types of arithmetic problems that arise on a daily basis”<sup>51</sup>. Finally, the LD does not detail the improvements that will be introduced to the design of the impact evaluation.

### 3. Budget

Table IV.1 shows that the new stage of PROjoven costs US\$26 million, of which US\$18 million are funded by the IDB and US\$8 million by the country. The budget allocated to the training component (component 1) represents 60% of the total amount and is 94% financed by the IDB. The cost of the three other components (US\$3.4 million) is smaller than the administrative costs (US\$3 million).

**Table IV.1: Total Costs of the program by components and products**

Total cost of the PE0241 program (US\$ thousands)			
Investment Categories	IDB	Local	Total
<b>1. Vocational instruction in TIs and businesses</b>	<b>14,805</b>	<b>900</b>	<b>15,705</b>
Targeting	300	700	1,000
Training in TIs	5,825	200	6,025
Training in businesses	5,390	-	5,390
Stipends for beneficiaries	3,290	-	3,290
<b>2. Selection, monitoring, and follow-up of TIs</b>	<b>1,990</b>	<b>-</b>	<b>1,990</b>
Preparation of roster and selection	850	-	850
Monitoring and oversight	710	-	710
Technical assistance	430	-	430
<b>3. Communications Strategy</b>	<b>-</b>	<b>700</b>	<b>700</b>
Information services	-	80	80
Job-search coaching	-	180	180
Job matching	-	390	390
Technical assistance	-	50	50
<b>4. Evaluation and monitoring</b>	<b>700</b>	<b>50</b>	<b>750</b>
Evaluation of outputs	-	50	50
Evaluation of outcomes	500	-	500
Evaluation of impacts	200	-	200
<b>Administration</b>	<b>-</b>	<b>3,600</b>	<b>3,600</b>
<b>Audits</b>	<b>200</b>	<b>-</b>	<b>200</b>
<b>Contingencies</b>	<b>305</b>	<b>800</b>	<b>1,105</b>
Contingencies	305	800	1,105
<b>Financial Costs</b>	<b>-</b>	<b>1,950</b>	<b>1,950</b>
Interest	-	1,700	1,700
Credit Fee	-	250	250
<b>Total</b>	<b>18,000</b>	<b>8,000</b>	<b>26,000</b>
<b>% By Source</b>	<b>69%</b>	<b>31%</b>	<b>100%</b>

Source: Loan Document

<sup>51</sup> Paragraph 2.23.

#### 4. Evaluability

The PE0241 has low levels of evaluability<sup>52</sup>. The diagnostic identifies the potential beneficiaries and the development problem: the low employability of low-income youths. However, the diagnostic does not provide clear evidence about the causes of the development problem. For example, the LD provides a poor analysis about the causes of the high unemployment of the target population. It does not provide enough evidence, from a careful analysis of the household surveys, to justify the intervention as a solution to the problem of unemployment, underemployment and inactivity. In fact, it gives an incomplete description of the Peruvian labor market situation and particularly of the employment situation of the youths in 2002. Therefore, even if the expected results at the end of the program seem to be linked to the development problem it is difficult to verify the pertinence of the components. For example, the diagnostic does not mention the number of poor young people that is entering every year into the labor market (the flow). Instead, it only mentions that there are 428,000 potential beneficiaries (the stock). Moreover, the diagnostic does not provide sufficient information about the first phase of the program. Especially, the heterogeneity of the impacts on the beneficiaries and also the impacts on the training market (demonstrative effect) of the first rounds of PROjoven are not considered. The former analysis was important to evaluate the pertinence of the second purpose identified in the logical framework (see table IV.2)<sup>53</sup>.

There is an inconsistency between the goal and purposes of the LD and those of the logical framework (LF). The LD considers as overall and secondary objectives of the program what are considered as purposes in the LF (see figure IV.1). Also, according to the LF the goal of the program is: “to help to increase the employability of low income youths”<sup>54</sup>. However, the LD only mentions the term employability at the end of the section A named “objectives and

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<sup>52</sup> Evaluable projects are defined as those that clearly identified a problem, proposed a logical intervention to address the problem, had adequate indicators to determine progress, and monitored those indicators during project execution to determine whether the anticipated degree of progress was being achieved (4.7 of RE-300). See also RE-275, both available at [www.iadb.org/ove](http://www.iadb.org/ove).

<sup>53</sup> Other limitations of the diagnostic are the following: (1) the analysis of the economic context is incomplete. For example, the main reasons of the economic recovery are not analyzed. (2) The description of the Peruvian labor market situation in 2002 is incomplete. For example, the level of segmentation in the labor market is not indicated neither the decomposition of the main labor indicators, by gender, geographical sectors, age groups or education levels. (3) The articulation of the education system, especially the current links between the formal secondary education and the training system, is not explained. (4) More information is needed about the 2 similar training programs currently implemented in Peru. The potential links of these programs with PROjoven are not analyzed. The same is true for the other IDB programs that are related to PROjoven. (5) The description of similar labor training programs (with the same model) in the region does not indicate the main modifications and improvements introduced by each country in order to improve programs design, results and impacts. (6) The traditional institutional instability in Peru and the problems confronted by the ministry of labor, which is one of the smallest and poorest ministries, are not considered. (7) The reasons of the large range of results found by previous evaluations of PROjoven as well as the methodological shortcomings and recommendations made by these studies are not mentioned.

<sup>54</sup> Without giving a detailed definition of the term “employability”. A footnote specifies that employability means: “the set of qualities that enables a person to obtain and keep a job and to adapt to its evolving demands (Sulmont 1986)”.



description”<sup>55</sup>. Finally, the specific objectives in the LD correspond to some of the goal indicators in the LF.

The logic of the program is affected by the problems mentioned before. For example, the LD considers that the first stage of PROJOVEN had significant impacts. It does not consider that the previous evaluations used different evaluations methods and found different results. Actually, it was more pertinent to undertake a comprehensive evaluation of the different rounds of PROJOVEN to have a better idea of the causes of the strong heterogeneity of the results. The preparation of the PE0241 should incorporate the findings of an evaluation of the previous rounds of PROJOVEN on the labor training supply. Also, it is not clear why the bank decided to restraint its intervention to a limited number of beneficiaries (45,000) when the stock of the target population was approximately ten times higher. Also, the pertinence of the second purpose is not evident because the demonstration effect should have been produced during the execution of the first phase of PROJOVEN (from 1996 to 2003). If that happened, thus it was maybe more pertinent for the PE0241 to try to reform the entire training system of the country. Moreover, the future institutionalization of the program is not considered. Projoven is considered as a “bubble”. On the other hand, the LD does not consider the characteristics of the labor demand in Peru where more than 70% of the firms are micro or small. Also, it does not consider the size of the informal sector that accounts for more than 50% of total employment. Both issues can affect the execution of the program. For example, these problems can make difficult for the program to find a large number of firms that are determined to offer paid internships (and this could be the underlying reason for not expanding the size of the program).

The products are related to the components and there are outcomes and outputs indicators but many of them suffer from the absence of baselines and milestones<sup>56</sup>.

The identification of risks is incomplete. The LD identified three internal risks and none external risks<sup>57</sup>. No mitigation activities for these risks are planned in the program. Important external risks that can affect the program execution are not considered. Particularly, neither institutional and political risks nor a possible economic downturn are mentioned. However, these risks are included as assumptions in the LF.

In terms of the evaluation strategy, the LD mentions that a mid term evaluation<sup>58</sup> and a final evaluation are required. Moreover, as we already mentioned the fourth component is devoted to the evaluation and monitoring of the program. Particularly, new tests are introduced to measure the value added by each of both phases of the program (courses and internship) in terms of skills

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<sup>55</sup> The LD mentions that: “helping young beneficiaries gain entry into the labor market involves enhancing their employability”.

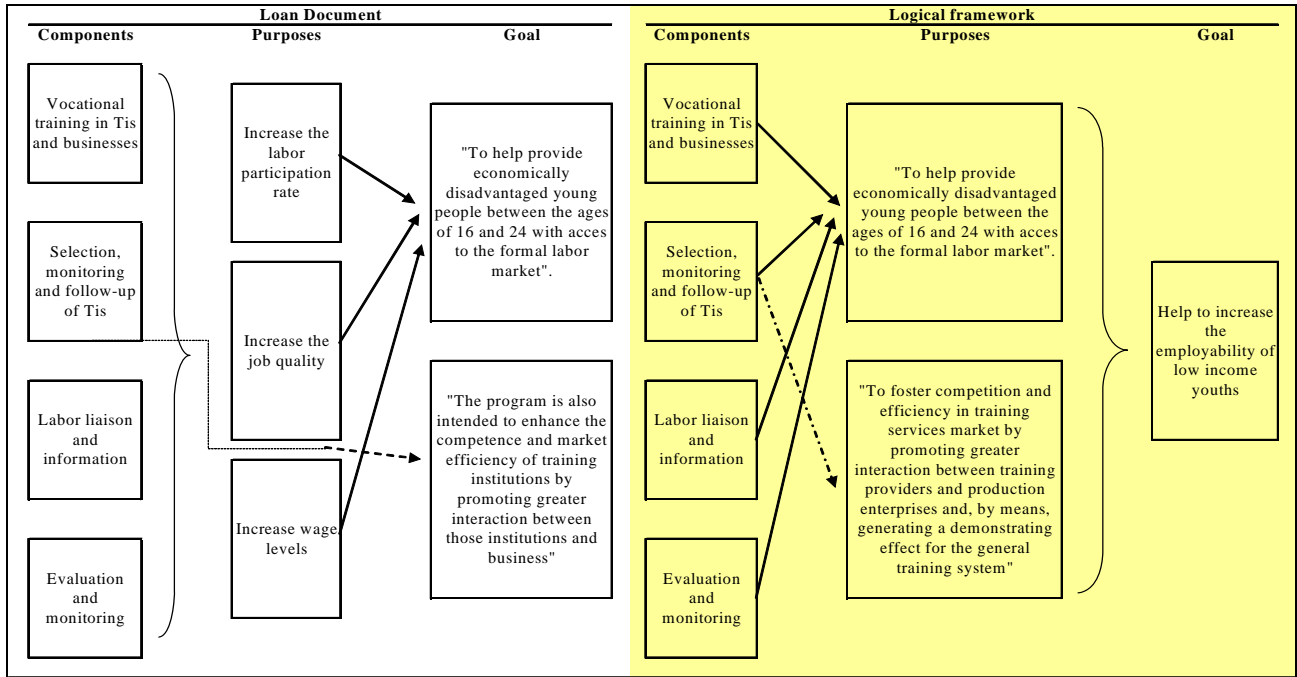
<sup>56</sup> The 3 PPMRs that are available for the program (December 2004, June and December 2005) maintain the same outcomes, outputs and indicators that were previously defined in the LD. On the contrary, the second purpose identified by the LF is not considered.

<sup>57</sup> Paragraph 4.11 mentions the following: “the main challenge to be met during the program’s execution is to maintain proper targeting of the beneficiary group and to select TIIs that meet clearly defined technical requirements (...) Raising the number of beneficiaries above the levels served thus far by PROJOVEN could introduce another element of risk for this program’s execution”.

<sup>58</sup> Its objective is: “to check on the achievement of benchmarks and to assess the operational scheme for the various program components”. Paragraph 3.36.

and work attitudes. However, the PE0241 lost the opportunity to integrate substantive modifications on the evaluation component. For example, as we already mentioned instead of developing an experimental impact evaluation in order to eliminate the selection bias, the PE0241 preferred to only introduce some modifications to the quasi-experimental design of the impact evaluation. These modifications were not sufficient to improve the evaluation of the impacts of the program.

**Figure IV.1: Differences between the LD and the LF**



**Table IV.2: Results framework**

Goal	Indicators	Baselines	Milestones	Targets
Help to increase the employability of low income youths	Employment rate Wage income Job quality Gender segregation (Duncan Index)	NA NA NA NA	NA NA NA NA	6% higher 18 higher higher 0.12 lower
<b>Purposes</b>				
1) to help provide economically disadvantage youths between the ages of 16-24 with access to the formal market. This is to be accomplished by means of specific activities designed to provide vocational training and work experience, in conjunction with labor liaison, orientation and information services, that will meet the business sector's and labor market	% of beneficiaries that improve their skills % of beneficiaries that improve their generic competencies % of beneficiaries that become more knowledgeable	NA NA NA	NA NA by cycle: 6300 (1) , 7200 (2) , 8100 (3), 9900 (4), 10800 (5)	NA NA 0.9
2) To foster competition and efficiency in training services market by promoting greater interaction between training providers and production enterprises and, by means, generating a demonstrating effect for the general training system	% of beneficiaries receiving liaison services that are placed by the SIL The TI performance score average improve	NN NA	by cycle: 940 (1) , 1099 (2) , 1270 (3), 1564 (4), 1710 (5) NA	24% NA
<b>Component 1</b>				
Vocational instruction in TIs and businesses for low-income youths	% of beneficiaries that complete the training phase % of beneficiaries that complete the internship phase % of beneficiaries that receive a stipend and/or subsidies % of targeting errors	NA NA NA NA	by cycle: 95% (1) , 96.3% (2) , 97% (3), 97.7% (4), 98% (5) by cycle: 73.9% (1) , 78.9% (2) , 88.9% (3), 88.9% (4), 88.9% (5) NA NA	97% on average 85% on average 100% Lima: 8% of screening errors and 12% for under coverage errors. Regions: 5% and 6%
<b>Component 2</b>				
TI training is linked to the production sectors's needs, which are reflected in internships that upgrade young people's skills	Number of TI that are placed on the roster based on their institutional and performance cyclical scores % of the courses that are selected on the basis of the evaluations % of the TI placed on the roster that receive methodological support materials % of the TI that receive assistance	NA NA NA NA	NA NA NA NA	130-150 100% 100% 75%
<b>Component 3</b>				
The labor liaison and information system provides effective support for the program beneficiaries entry into the labor market	% of the beneficiaries that receive information about the program from the SIL % of the beneficiaries that receive job-search coaching from the SIL % of the beneficiaries who have not completed their internship or who have not found a job that receive job-matching services from the SIL	NN NN NN	by cycle: 7000 (1) , 8000(2) , 9000 (3), 11000 (4), 12000 (5) by cycle: 6730 (1) , 7760 (2) , 8730 (3), 10670 (4), 11640 (5) by cycle: 3970 (1) , 4593 (2) , 5197 (3), 6400 (4), 7000 (5)	100% 97% 60%
<b>Component 4</b>				
The evaluation and monitoring system generates reliable data and outputs, outcomes and impacts achieved by the program, thus providing feedback for the program and permitting its operations to be modified on the basis of those evaluations	Monitoring and recording studies Evaluation of outcomes of two cycles Evaluation of client satisfaction Evaluation of impact	NN NN NN NN	by cycle: August2005 (1) , February 2006 (2) , July 2006 (3), July 2007 (4), November 2007 (5) by cycle: Competency evaluation instrument July 2005 (1) , Competencies evaluation January 2006 (2) , Competencies evaluation June 2006 (3) by cycle: July 2005 (2) , June 2006 (3) by cycle: 1st impact eval report (3): 1st measurement Sept 2006 (1) , 2nd meas March 2007 (2) , Third meas Sept 2007, 2nd impact eval report (3) : 1st meas. Feb 2007, 2nd meas. Aug 2007, 3rd meas. Feb 2008	Completed 2 Completed Completed

*Source: Logical framework*

*Note: NA = non available and NN = not required*

## **B. Problems detected during the implementation**

The execution of the program has suffered delays most of which are related to political and institutional problems that occurred in 2005<sup>59</sup>. Currently, two years after its approval, the program has only disbursed 10% of the original total amount (component 1 has only disbursed 14%, component 2: 11% and component 4: 3%). The PPMR of December 2005 indicates that at the end of the year the program attained 78% of its targets expected and disbursed 55% of the amount planned for this period<sup>60</sup>.

Both PPMRs of 2005 mention the institutional and political problems that affected the execution of the program in 2005. Both instruments introduced those problems into the list of key assumptions related to development objectives. Nevertheless, both instruments maintain a satisfactory note in the implementation progress summary classification. On the contrary, the PPMRs do not mention the internal changes that also occurred in 2005<sup>61</sup>. These changes affected the role of supervision and monitoring of the Bank, making the political capture of the program easier.

Finally, the PPMRs also mention that some administrative problems provoked delays in the implementation of the component 3. What is happening is that the CIL is not an executing agency. Therefore it is complicated to transfer the resources necessary to execute the activities related to the program.

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<sup>59</sup> Recent political appointments have made the program management very volatile. In 2005, the program had 5 different coordinators and none of those who were employed in January remained in the program by the end of the year. Moreover, some evidence about corruption also appeared in 2005. In October a television report showed that the brother of the new Minister of Labor opened an illegal office of PROjoven in the locality of Carabayllo in Lima. In this office fake PROjoven officials made the promise to the youths that they would be directly accepted into the program. Allegedly, the purpose behind this illegal activity was to get support for the brother's mayoral election campaign. The television report also showed that the brother participated, without any authorization, in some internal meetings at PROjoven's headquarters. The tv report can be found at: [www.Agenciaperu.com/investigacion/2005/oct/almery.htm](http://www.Agenciaperu.com/investigacion/2005/oct/almery.htm).

<sup>60</sup> However, according to the data analyzer the program has only finished the activities related to the first round (round number 12). These delays are not entirely assimilated by the last PPMR because this instrument maintained January 2006 as the date for the planned mid-term evaluation.

<sup>61</sup> The country office specialist was transferred to the Bank's headquarters mid 2005 and its replacement only arrived to Peru in January 2006. A consultant was put partially in charge of the program during the second semester but was charged with other responsibilities a few months later after he won an administrative staff position. It is interesting to note that even if the first specialist in charge of the program left Peru mid 2005, the last PPMR continues mentioning his name as the bank's official in charge of the program.

**Table IV.3: Current disbursements, by components and products**

<b>Budget modifications during the execution</b> (US\$ thousands)			
<b>Investment Categories</b>	<b>Approved Current</b>	<b>Disbursed</b>	<b>%</b>
<b>1. Vocational instruction in Tis and businesses</b>	<b>14,805</b>	<b>120</b>	<b>14%</b>
Targeting	300	36	12%
Training in TIs	5,825	60	1%
Training in businesses	5,390	-	0%
Stipends for beneficiaries	3,290	24	1%
<b>2. Selection, monitoring, and follow-up of Tis</b>	<b>1,990</b>	<b>78</b>	<b>11%</b>
Preparation of roster and selection	850	26	3%
Monitoring and oversight	710	42	6%
Technical assistance	430	9	2%
<b>3. Communications Strategy</b>	<b>-</b>	<b>-</b>	<b>0%</b>
Information services	-	-	0%
Job-search coaching	-	-	0%
Job matching	-	-	0%
Technical assistance	-	-	0%
<b>4. Evaluation and monitoring</b>	<b>700</b>	<b>6</b>	<b>3%</b>
Evaluation of outputs	-	-	0%
Evaluation of outcomes	500	-	0%
Evaluation of impacts	200	6	3%
<b>Administration</b>	<b>-</b>	<b>-</b>	<b>0%</b>
<b>Audits</b>	<b>200</b>	<b>-</b>	<b>0%</b>
<b>Contingencies</b>	<b>305</b>	<b>-</b>	<b>0%</b>
Contingencies	305	-	0%
<b>Financial Costs</b>	<b>-</b>	<b>-</b>	<b>0%</b>
Interest	-	-	0%
Credit Fee	-	-	0%
<b>Revolving fund</b>	<b>-</b>	<b>1,622</b>	<b>0%</b>
<b>Total</b>	<b>18,000</b>	<b>1,825</b>	<b>10%</b>

*Source: LSMS*

### C. Outputs

- 4.2 The last PPMR indicates that 15,208 youths attended PROjoven regional offices and that 11,098 of them received an accreditation (see table IV.4). At the end of the process of selection, only 5,213 beneficiaries remained and were able to initiate the training. This number of beneficiaries is smaller than the number expected in the LD<sup>62</sup>. For this first call, 278 courses were contracted and 144 ECAPs were selected.
- 4.3 The second round was expected to occur during the second semester of 2005. However, it has been delayed. The last PPMR mentions that for this call 135 ECAPs were selected between May and July 2005 and that the selection process of courses started on August 31. A total number of 15,310 youths attended PROjoven regional offices. Currently, these youths are in the selection process for the 8,667 vacancies that are available (instead of the 9,000 planned in the LD). The training courses were expected to start in February 2006.

<sup>62</sup> Paragraph 3.7 of the loan document indicates that the program will serve approximately 9,000 youths per cycle.

**Table IV.4: Original and current outputs of the first component**

Public calls	Logical framework			PPMR of december 2005				
	Graduates	Job-training	Timeline	TIs	Courses	Graduates	Job-training	Final date expected
1	6,650	4,914	First semester 2005	144	278	5,031	3,639	January 2006
2	7,700	6,075	Second semester 2005	135	351	NA	NA	August 2006
3	8,730	7,760	First semester 2006	NA	NA	NA	NA	NA
4	10,750	9,555	Second semester 2006	NA	NA	NA	NA	NA
5	11,760	10,450	First semester 2007	NA	NA	NA	NA	NA
<b>Total</b>	<b>45,590</b>	<b>38,754</b>				<b>5,031</b>	<b>3,639</b>	

*Source: Logical framework and PPMR of December 2005*

#### **D. Discussion**

The PE0241 is a close continuation of the first stage of PROJOVEN even if some modifications of the original design were introduced in order to improve the impacts of the program. However, important deficiencies of the original design were not solved.

The long-term sustainability of the program was not considered by the PE0241 and, in fact, it is not guaranteed. PROJOVEN is now in its tenth year. However, the program does not have an institutional sustainability. The program is a sort of “bubble” inside the public training system and is not integrated with other similar programs<sup>63</sup> and with agencies and ministries involved in the nationwide training network. On the other hand, the long continuity of the program 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 rare feature of the program is that until recently it has had a quite stable and technical management team. Until two years ago, only four individuals had occupied the position of program coordinator; which compares with the also four coordinators that the program has had so far this year. 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.<sup>64</sup> 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,

<sup>63</sup> For example, the paragraph 1.23 mentions the training program CAPLAB that provides training to lower income groups. Also, Chacaltana and Sulmont (2003) describe other interesting training programs such as: PROFECE, PASE, FORTE PE, FONDOEMPLEO. Like PROJOVEN these programs introduced innovative mechanisms in their designs in order to improve the quality of their training courses.

<sup>64</sup> 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.

accounting for less than 1% of the central government budget.<sup>65</sup> 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 appointment in February this year 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.

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. Also, though over time the investment made in PROjoven is considerable, the Program has never enjoyed long-term financing. 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 Ministry's own sources of revenue. The recent loan contract with IDB (US\$18 million) 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 have been 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 may be large because of PROjoven's demanding project cycle<sup>66</sup>.

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<sup>65</sup> In the second half of the nineties, during PROjoven's growth, the Labor Ministry represented 0.6% of the central government budget.

<sup>66</sup> An example of the political importance of PROjoven is that it was selected as the best social program of the country in 2005 by the journalist association of the Congress (more information at: <http://www.projoven.gob.pe/contactenos/noticias.htm>)

## V. CONCLUSIONS

PROjoven is in its 10 year of execution. In 2004, the Bank approved the project PE0241 to finance five more public calls of the program. The PE0241 opted to maintain the overall objective of the program and the main features of the original design. It extended the number of beneficiaries (from approximately 6,000 to 9,000 beneficiaries per call, which is still insufficient to solve the problem of a stock of young poor people that are unemployed, inactive or underemployed --approximately 400,000). The program also introduced some changes to the design in order to improve the relevance of the training courses, the job placement rate of the beneficiaries, the targeting process and the evaluation component. However, it lost the opportunity to solve other relevant issues of the original design. Particularly, it did not consider the lack of institutional sustainability of the program. In fact, the new program maintains the underlying objective of the original program, which is the demonstrative effect on the general training system. However, this demonstrative effect should have been produced during the execution of the first phase of PROjoven because the program was launched almost eight years before. Also, new tests were introduced by the new operation in order to reduce the gap of evidence about the quality of the technical training offered by the ECAPS. However, no measure was introduced in order to improve the supervision and the monitoring process of the ECAPS. Moreover, the PE0241 recognized the limitations of the quasi-experimental evaluation design but instead of implementing an experimental design it only introduced some improvements in the matching procedure.

The evaluation of the original program arrived 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.

There are 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 bidding. 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 eight public call. The recommendation for the Program 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 a 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 control 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 CS 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 control 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.

Regarding PROJoven impacts for beneficiaries, 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%, and as high as 30%.

We argue that, at least in part, this is the result of the concordance between the courses and real labor demand requirements in the labor market, and that beneficiaries must be hired for their internships under Agreements of Youth Labor Training, which provides better job conditions and pay at least the minimum wage. 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 unobservables.

Regarding the impacts of PROJoven on the vocational training market, we first analyzed trends in participation trends in participation. The most important finding here is that the upward trend in participation is associated to the incorporation of new locations outside of Lima. Surprisingly, in Lima the trend turns out slightly downward. This raises questions about the payoff for participation in the Program. The second important finding is that the largest group of participants, and the fastest growing outside of Lima, are in RECAP but do not bid for courses. This suggests motivations for participation that are different than obtaining courses. We suggest that accreditation may be one important rationale and find some econometric evidence in support. However, we also suggest that more analysis of this phenomenon is needed.

Participation by type of institution is dominated by CEOs, ISTs and NGOs. The first of these is the largest group while the other has a similar share in the number of institutions participating. Together these three groups account for about two-thirds of total participating institutions. Also we find that smaller institutions, with less than 20 employees, get a larger share of participation, including courses allocated, accounting for more than three quarters of these. In the opposite side, institutions with more than 50 employees get a small share of course, slightly above 6%.

We specifically explored and tested three potential impacts of the Program on the training market: specialization among participating institutions, closer links between ECAPs and firms, and accreditation effects. Through analysis of course offerings by training entities we find strong evidence of a trend towards greater specialization. We also explore some descriptive evidence on links between ECAPs and firms, merging data from RECAP and that from the survey of ECAPs conducted by the consultant team. The evidence suggests that PROJoven has significant effects in furthering connections between ECAPs and firms. Finally, we tested the hypothesis of an

accreditation motive in participation in the Program, estimating an econometric model of the decision to bid or not bid for courses from PROJoven. Although some evidence indicates the existence of accreditation effects, we suggest that this is an area for future research.

Finally, through the construction of quality indices we find that training institutions participating in PROJoven are of greater quality than those rejected by the Program. We also find significant differences among those in RECAP between bidders and non bidders. Specifically, greater links with firms characterize the institutions bidding.

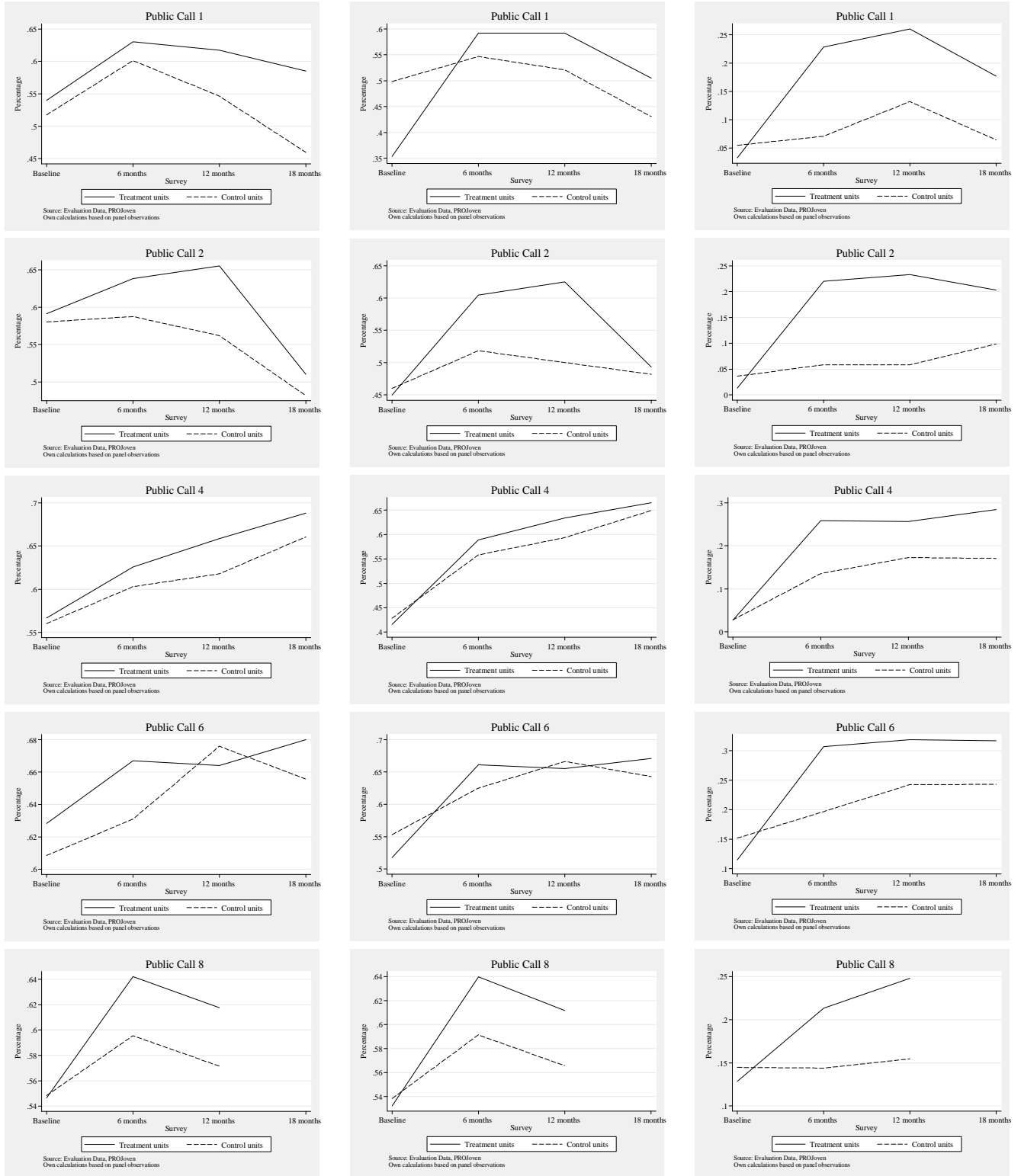
# APPENDIX A: FIGURES AND TABLES

## Figure A.1: Employment and paid jobs percentages

### A. Employment

### B. Paid Jobs

### C. Formal Jobs

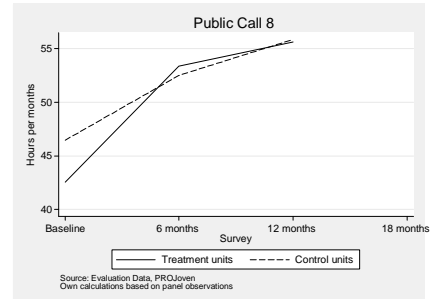
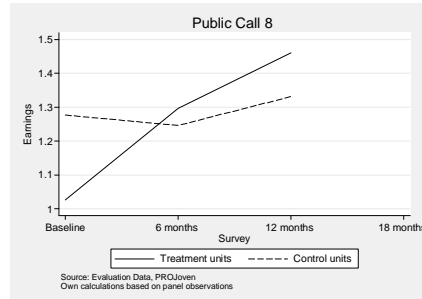
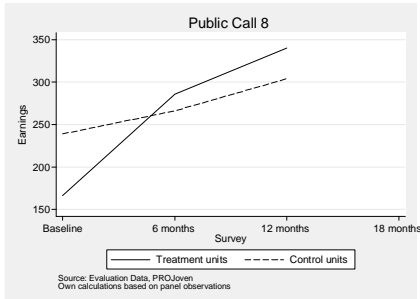
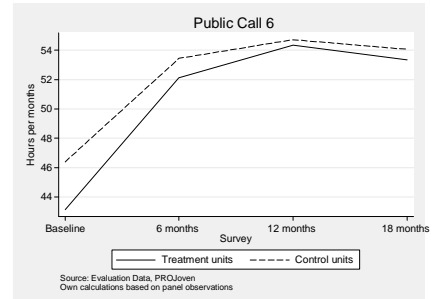
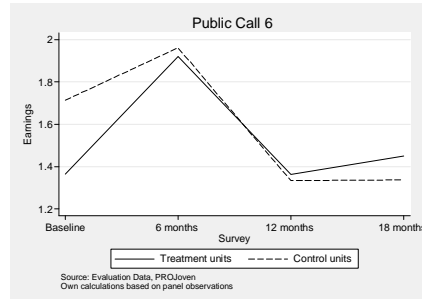
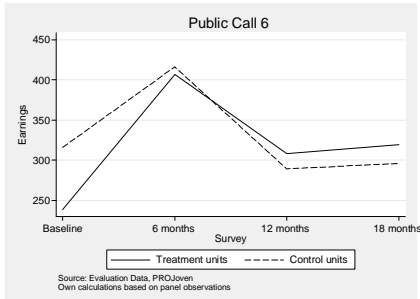
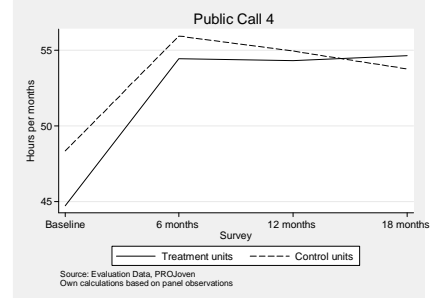
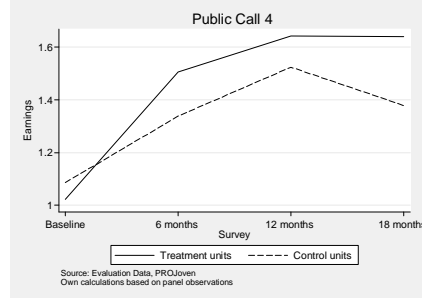
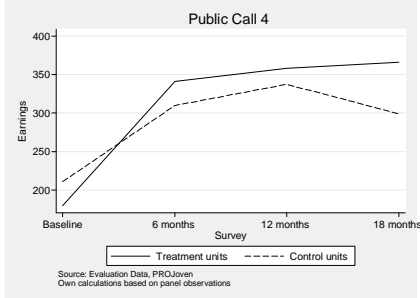
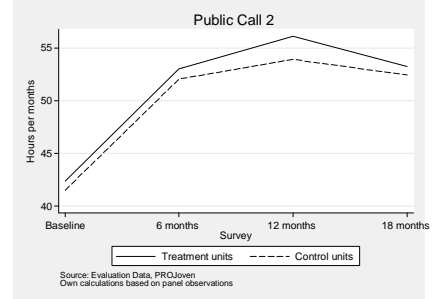
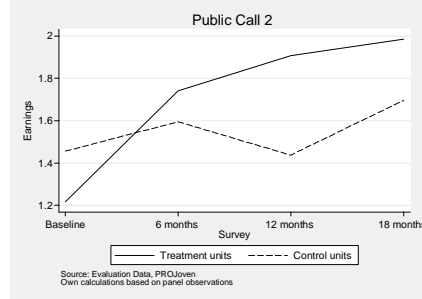
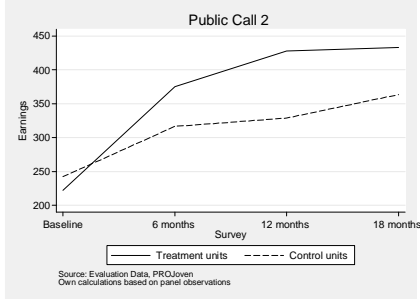
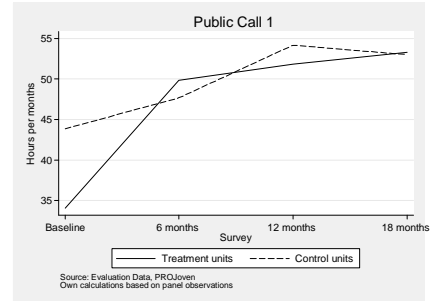
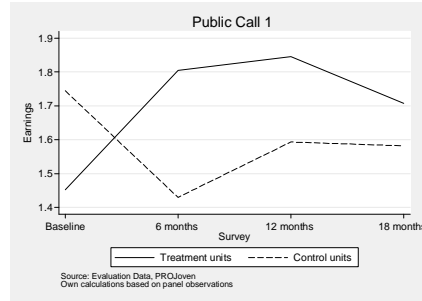
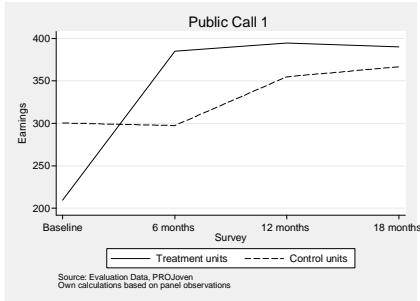


**Figure A.2: Evolution of real earnings and weekly working hours in the main job**

**A. Monthly earnings**

**B. Hourly earnings**

**C. Weekly working hours**



Source: Evaluation Data, PROJoven  
Own calculations based on panel observations

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Own calculations based on panel observations

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Own calculations based on panel observations

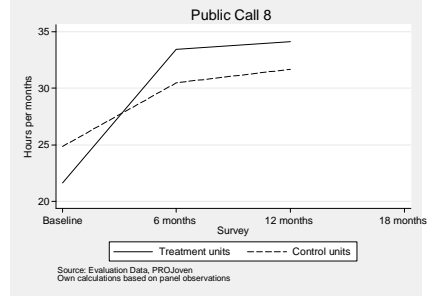
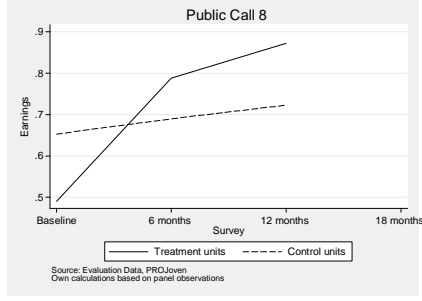
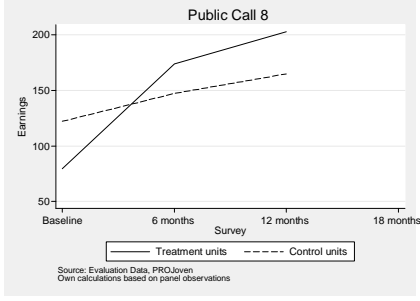
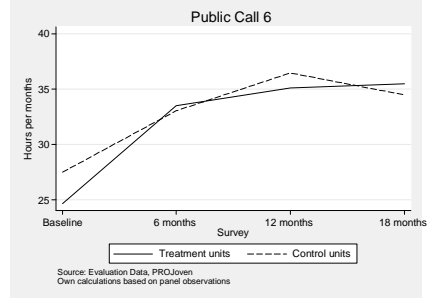
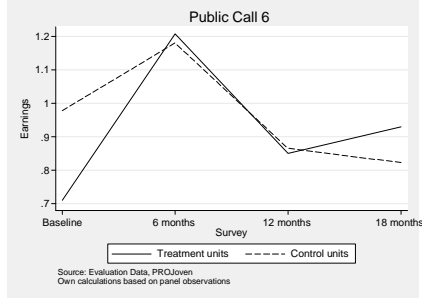
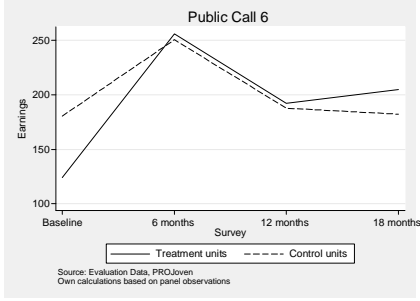
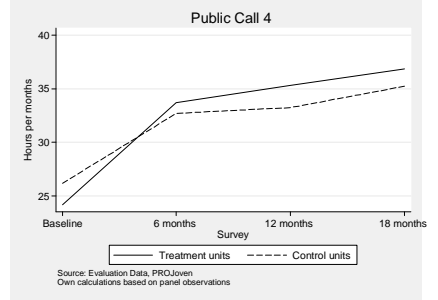
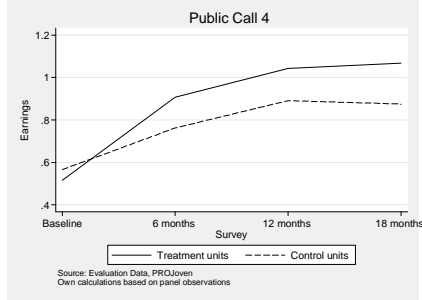
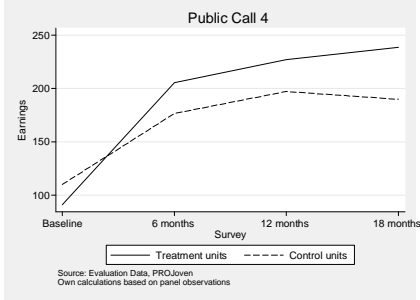
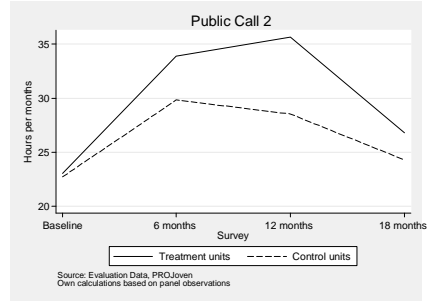
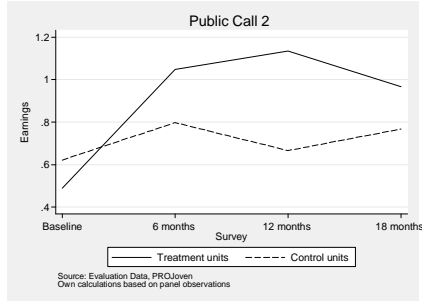
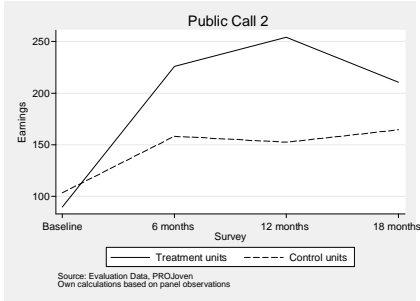
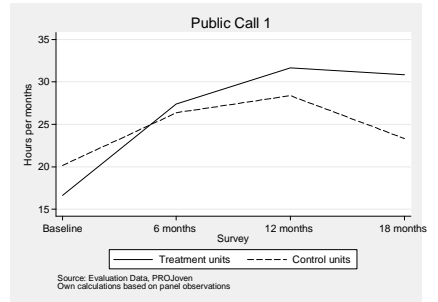
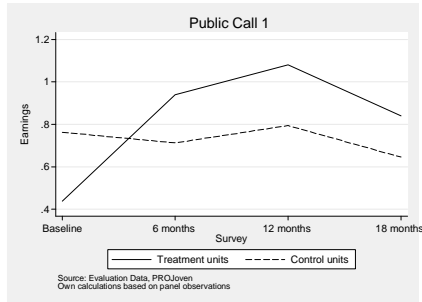
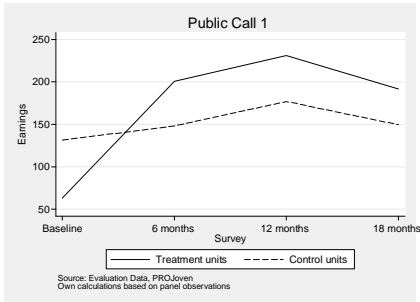
Source: Evaluation Data, PROJoven  
Own calculations based on panel observations

**Figure A.3: Evolution of real earnings and weekly working hours in the main job (Censored)**

**A. Monthly earnings**

**B. Hourly earnings**

**C. Weekly working hours**



**TableA.1: Full sample matching estimates**  
(standard errors in parentheses)

	First Call		Second Call		Fourth Call		Sixth Call		Eighth Call	
	DID	CS	DID	CS	DID	CS	DID	CS	DID	CS
<b>A. Employment</b>										
Post-1	-0.02 (0.05)	0.02 (0.04)	0.04 (0.06)	0.05 (0.04)	0.03 (0.04)	0.02 (0.03)	0.03 (0.03)	0.04 (0.02)	0.05 (0.03)	0.05 (0.02)
Post-2	0.01 (0.06)	0.05 (0.04)	0.08 (0.05)	0.09 (0.04)	0.04 (0.04)	0.03 (0.03)	-0.02 (0.03)	-0.01 (0.02)	0.06 (0.03)	0.06 (0.02)
Post-3	0.08 (0.06)	0.12 (0.04)	0.01 (0.06)	0.02 (0.05)	0.03 (0.04)	0.02 (0.03)	0.02 (0.03)	0.03 (0.02)		
<b>B. Paid Jobs</b>										
Post-1	0.17 (0.05)	0.04 (0.04)	0.09 (0.06)	0.08 (0.04)	0.05 (0.04)	0.03 (0.03)	0.08 (0.03)	0.04 (0.02)	0.06 (0.03)	0.05 (0.02)
Post-2	0.19 (0.06)	0.06 (0.04)	0.14 (0.06)	0.13 (0.04)	0.06 (0.04)	0.04 (0.03)	0.03 (0.03)	-0.01 (0.02)	0.06 (0.03)	0.05 (0.02)
Post-3	0.20 (0.06)	0.07 (0.04)	0.01 (0.06)	0.00 (0.05)	0.03 (0.04)	0.01 (0.03)	0.08 (0.03)	0.04 (0.02)		
<b>C. Formal Jobs</b>										
Post-1	0.18 (0.03)	0.16 (0.03)	0.18 (0.03)	0.16 (0.03)	0.12 (0.03)	0.12 (0.02)	0.14 (0.02)	0.11 (0.02)	0.09 (0.02)	0.07 (0.02)
Post-2	0.15 (0.04)	0.12 (0.03)	0.19 (0.03)	0.17 (0.03)	0.08 (0.03)	0.08 (0.02)	0.11 (0.02)	0.07 (0.02)	0.11 (0.02)	0.09 (0.02)
Post-3	0.13 (0.03)	0.11 (0.03)	0.12 (0.03)	0.10 (0.03)	0.11 (0.03)	0.11 (0.03)	0.11 (0.02)	0.08 (0.02)		
<b>D. Real Monthly Earnings</b>										
Post-1	181.2 (31.1)	90.1 (19.7)	87.0 (26.9)	63.7 (21.0)	59.1 (17.2)	31.5 (14.1)	66.4 (23.0)	-10.9 (19.8)	93.5 (11.9)	27.4 (8.9)
% effect	60.3	30.3	35.9	20.1	28.0	10.2	21.0	-2.6	39.2	10.3
Post-2	132.8 (28.6)	41.8 (16.7)	119.3 (31.3)	96.1 (27.2)	46.7 (18.9)	19.1 (15.8)	95.4 (17.1)	18.0 (11.7)	104.4 (12.5)	38.3 (10.0)
% effect	44.2	11.8	49.2	29.2	22.1	5.7	30.2	6.2	43.8	12.6
Post-3	116.1 (35.2)	25.0 (27.7)	85.6 (36.4)	62.4 (33.3)	96.4 (19.9)	68.8 (17.3)	99.2 (16.0)	21.8 (10.6)		
% effect	38.6	6.8	35.3	17.2	45.7	23.0	31.4	7.4		
<b>E. Real Hourly Earnings</b>										
Post-1	0.65 (0.22)	0.38 (0.10)	0.41 (0.17)	0.17 (0.13)	0.21 (0.09)	0.16 (0.07)	0.30 (0.13)	-0.04 (0.11)	0.28 (0.07)	0.06 (0.05)
% effect	37.4	26.8	28.3	10.5	19.2	12.2	17.7	-2.1	22.3	4.6
Post-2	0.53 (0.22)	0.26 (0.09)	0.71 (0.18)	0.46 (0.16)	0.16 (0.10)	0.12 (0.08)	0.37 (0.09)	0.03 (0.06)	0.36 (0.07)	0.13 (0.05)
% effect	30.1	16.1	48.5	32.2	14.8	7.5	21.7	2.1	27.9	9.6
Post-3	0.40 (0.21)	0.13 (0.11)	0.50 (0.19)	0.25 (0.17)	0.30 (0.11)	0.25 (0.09)	0.46 (0.09)	0.11 (0.05)		
% effect	22.9	8.1	34.0	14.8	27.3	18.2	26.8	8.5		
<b>F. Weekly Working Hours</b>										
Post-1	11.7 (3.3)	2.2 (2.1)	0.1 (3.3)	1.1 (2.1)	2.3 (2.2)	-1.4 (1.5)	2.1 (1.7)	-1.3 (1.2)	5.3 (1.7)	2.0 (1.1)
% effect	26.6	4.6	0.2	2.1	4.8	-2.4	4.4	-2.4	11.5	3.8
Post-2	7.1 (2.9)	-2.4 (1.9)	1.2 (3.3)	2.2 (2.3)	3.3 (2.2)	-0.4 (1.5)	3.0 (1.5)	-0.4 (1.0)	3.4 (1.7)	0.0 (1.1)
% effect	16.2	-4.4	2.9	4.1	6.8	-0.7	6.5	-0.7	7.3	0.1
Post-3	9.6 (2.8)	0.2 (1.9)	-0.3 (3.3)	0.7 (2.3)	4.8 (2.3)	1.2 (1.6)	2.6 (1.6)	-0.7 (1.1)		
% effect	22.0	0.3	-0.7	1.4	10.0	2.2	5.6	-1.4		

continues...

**Table A.1**

	First Call		Second Call		Fourth Call		Sixth Call		Eighth Call	
	DID	CS	DID	CS	DID	CS	DID	CS	DID	CS
<b>G. Real Monthly Earnings (censored)</b>										
Post-1	115.9	49.2	77.5	63.2	49.0	28.4	59.5	3.1	69.7	30.0
	(21.0)	(17.3)	(22.1)	(18.6)	(14.9)	(13.0)	(14.8)	(12.9)	(9.4)	(7.7)
% effect	88.2	33.2	74.9	39.9	44.5	16.1	33.0	1.2	57.3	20.4
Post-2	115.6	49.0	115.7	101.4	49.6	29.0	59.5	3.1	79.7	40.0
	(22.5)	(18.3)	(25.2)	(22.6)	(14.9)	(12.6)	(12.9)	(9.5)	(10.6)	(9.1)
% effect	88.0	27.7	111.8	66.5	45.1	14.7	33.0	1.6	65.5	24.2
Post-3	106.5	39.9	55.1	40.8	70.4	49.8	80.1	23.7		
	(23.3)	(19.4)	(26.3)	(23.5)	(16.0)	(14.2)	(13.1)	(10.1)		
% effect	81.0	26.6	53.2	24.8	64.0	26.3	44.4	13.0		
<b>H. Real Hourly Earnings (censored)</b>										
Post-1	0.53	0.21	0.37	0.23	0.19	0.14	0.29	0.02	0.26	0.10
	(0.12)	(0.08)	(0.12)	(0.10)	(0.07)	(0.06)	(0.08)	(0.07)	(0.05)	(0.04)
% effect	69.1	30.1	58.9	28.9	33.7	18.4	29.8	1.9	39.4	15.2
Post-2	0.57	0.26	0.60	0.47	0.20	0.15	0.25	-0.02	0.31	0.16
	(0.13)	(0.09)	(0.13)	(0.11)	(0.08)	(0.07)	(0.07)	(0.05)	(0.05)	(0.04)
% effect	74.6	32.3	97.1	70.2	35.9	17.1	25.6	-2.1	47.5	21.7
Post-3	0.50	0.18	0.31	0.18	0.24	0.19	0.38	0.11		
	(0.12)	(0.08)	(0.13)	(0.11)	(0.08)	(0.07)	(0.07)	(0.05)		
% effect	65.0	28.4	50.3	23.1	42.4	21.7	39.1	13.9		
<b>I. Weekly Working Hours (censored)</b>										
Post-1	3.5	0.4	3.5	3.8	3.6	1.0	3.6	0.5	6.7	3.7
	(2.9)	(2.3)	(3.3)	(2.4)	(2.5)	(1.9)	(1.5)	(1.2)	(1.6)	(1.3)
% effect	17.2	1.7	15.2	12.6	13.8	2.9	12.9	1.5	26.9	12.2
Post-2	5.6	2.6	6.8	7.1	4.5	1.9	1.6	-1.4	6.1	3.1
	(3.2)	(2.5)	(3.3)	(2.5)	(2.3)	(1.7)	(1.7)	(1.2)	(1.6)	(1.3)
% effect	27.7	9.0	29.7	24.7	17.2	5.6	5.9	-3.9	24.6	9.9
Post-3	10.1	7.0	1.5	1.8	4.4	1.7	4.4	1.3		
	(3.1)	(2.5)	(3.4)	(2.7)	(2.3)	(1.8)	(1.6)	(1.2)		
% effect	49.9	30.1	6.6	7.4	16.7	4.9	15.9	3.8		



**Table A.2: Male sample matching estimates**  
(standard errors in parentheses)

	First Call		Second Call		Fourth Call		Sixth Call		Eighth Call	
	DID	CS	DID	CS	DID	CS	DID	CS	DID	CS
<b>A. Employment</b>										
Post-1	-0.08 (0.08)	-0.04 (0.05)	-0.17 (0.08)	-0.14 (0.06)	-0.05 (0.05)	-0.07 (0.04)	-0.02 (0.04)	0.00 (0.03)	-0.03 (0.04)	-0.02 (0.03)
Post-2	-0.05 (0.08)	-0.02 (0.06)	-0.02 (0.09)	0.01 (0.06)	-0.02 (0.05)	-0.03 (0.04)	-0.07 (0.04)	-0.05 (0.03)	0.00 (0.04)	0.00 (0.03)
Post-3	-0.03 (0.08)	0.01 (0.06)	-0.07 (0.09)	-0.04 (0.06)	-0.01 (0.05)	-0.03 (0.04)	-0.07 (0.03)	-0.05 (0.02)		
<b>B. Paid Jobs</b>										
Post-1	0.12 (0.09)	-0.01 (0.06)	-0.12 (0.09)	-0.08 (0.07)	-0.05 (0.06)	-0.09 (0.04)	0.03 (0.04)	0.00 (0.03)	-0.03 (0.04)	-0.02 (0.03)
Post-2	0.12 (0.08)	-0.01 (0.06)	0.01 (0.09)	0.05 (0.06)	-0.01 (0.06)	-0.05 (0.04)	-0.02 (0.04)	-0.06 (0.03)	0.00 (0.04)	0.01 (0.03)
Post-3	0.13 (0.09)	-0.01 (0.06)	-0.08 (0.09)	-0.04 (0.06)	0.00 (0.05)	-0.04 (0.04)	-0.01 (0.04)	-0.04 (0.02)		
<b>C. Formal Jobs</b>										
Post-1	0.13 (0.06)	0.11 (0.05)	0.16 (0.05)	0.12 (0.05)	0.13 (0.04)	0.12 (0.04)	0.09 (0.03)	0.08 (0.03)	0.05 (0.03)	0.05 (0.02)
Post-2	0.07 (0.06)	0.05 (0.05)	0.18 (0.05)	0.13 (0.05)	0.09 (0.04)	0.08 (0.04)	0.06 (0.04)	0.05 (0.03)	0.08 (0.03)	0.08 (0.03)
Post-3	0.09 (0.06)	0.07 (0.05)	0.11 (0.06)	0.07 (0.05)	0.13 (0.04)	0.12 (0.04)	0.06 (0.04)	0.05 (0.03)		
<b>D. Real Monthly Earnings</b>										
Post-1	182.1 (35.5)	60.7 (25.9)	90.5 (43.1)	62.8 (36.4)	28.6 (24.5)	-6.4 (20.4)	50.1 (28.0)	-34.4 (23.6)	79.2 (17.7)	8.6 (12.1)
% effect	52.9	18.0	32.8	17.8	11.8	-1.8	14.0	-7.4	29.0	2.8
Post-2	122.8 (35.7)	1.4 (26.7)	105.1 (47.6)	77.4 (42.6)	23.7 (26.2)	-11.3 (22.7)	95.9 (21.7)	11.4 (15.2)	106.4 (18.3)	35.8 (14.3)
% effect	35.7	0.3	38.0	19.3	9.8	-2.9	26.8	3.5	39.0	10.3
Post-3	124.7 (37.3)	3.3 (28.8)	61.4 (57.8)	33.7 (53.6)	65.4 (23.5)	30.4 (19.6)	100.0 (20.4)	15.5 (14.1)		
% effect	36.2	0.8	22.2	7.7	27.1	9.0	28.0	4.7		
<b>E. Real Hourly Earnings</b>										
Post-1	0.91 (0.20)	0.39 (0.12)	0.54 (0.26)	0.20 (0.22)	0.19 (0.12)	0.13 (0.08)	0.28 (0.14)	-0.12 (0.11)	0.31 (0.09)	0.03 (0.06)
% effect	46.7	25.1	34.9	10.9	16.1	9.0	14.5	-5.8	21.7	2.1
Post-2	0.61 (0.18)	0.09 (0.11)	0.86 (0.33)	0.51 (0.30)	0.02 (0.12)	-0.03 (0.09)	0.48 (0.10)	0.08 (0.07)	0.40 (0.09)	0.12 (0.06)
% effect	31.4	5.2	54.9	31.5	2.1	-2.0	25.4	5.8	28.0	7.9
Post-3	0.59 (0.20)	0.07 (0.12)	0.45 (0.30)	0.10 (0.27)	0.12 (0.12)	0.06 (0.09)	0.60 (0.10)	0.20 (0.07)		
% effect	30.0	3.8	29.1	5.1	10.5	4.3	31.6	14.2		
<b>F. Weekly Working Hours</b>										
Post-1	10.0 (3.8)	-1.9 (2.4)	-2.2 (4.4)	-0.9 (3.1)	-1.2 (2.8)	-5.6 (1.9)	1.3 (2.0)	-2.0 (1.4)	2.1 (2.1)	-1.2 (1.4)
% effect	21.6	-3.8	-5.1	-1.6	-2.3	-9.5	2.7	-3.6	4.5	-2.2
Post-2	8.8 (3.7)	-3.1 (2.3)	-1.6 (4.2)	-0.2 (2.8)	3.2 (2.7)	-1.2 (1.6)	0.8 (1.9)	-2.4 (1.3)	3.3 (2.2)	0.0 (1.4)
% effect	19.0	-5.5	-3.6	-0.4	6.3	-2.0	1.7	-4.3	7.0	0.0
Post-3	9.2 (4.2)	-2.7 (2.5)	-1.2 (4.2)	0.2 (2.6)	4.4 (2.8)	0.0 (2.0)	0.0 (2.0)	-3.3 (1.2)		
% effect	19.8	-4.7	-2.7	0.3	8.6	0.1	-0.1	-5.8		

continues...

**Table A.2**

	First Call		Second Call		Fourth Call		Sixth Call		Eighth Call	
	DID	CS	DID	CS	DID	CS	DID	CS	DID	CS
<b>G. Real Monthly Earnings (censored)</b>										
Post-1	135.2	33.1	8.8	10.4	8.8	-23.0	30.7	-33.0	48.0	1.5
	(34.4)	(27.4)	(38.4)	(32.1)	(24.0)	(20.7)	(25.6)	(21.6)	(14.8)	(11.6)
% effect	67.1	15.1	6.5	4.4	5.8	-8.8	12.7	-9.8	29.0	0.7
Post-2	107.1	5.1	66.5	68.1	14.3	-17.5	50.6	-13.1	70.1	23.7
	(35.7)	(29.4)	(46.3)	(41.5)	(24.7)	(21.0)	(20.0)	(14.9)	(16.7)	(13.9)
% effect	53.2	1.9	49.0	28.1	9.4	-6.1	20.9	-5.2	42.4	9.8
Post-3	103.9	1.8	-1.7	-0.1	40.4	8.5	57.5	-6.2		
	(35.7)	(30.0)	(44.0)	(39.3)	(24.6)	(21.0)	(18.5)	(11.9)		
% effect	51.6	0.8	-1.3	0.0	26.6	3.2	23.7	-2.4		
<b>H. Real Hourly Earnings (censored)</b>										
Post-1	0.75	0.23	0.06	-0.03	0.10	0.02	0.17	-0.13	0.20	0.01
	(0.19)	(0.13)	(0.22)	(0.19)	(0.11)	(0.08)	(0.12)	(0.10)	(0.08)	(0.06)
% effect	65.5	22.0	8.4	-2.1	13.5	1.6	13.5	-8.6	22.6	1.0
Post-2	0.62	0.09	0.47	0.39	0.01	-0.07	0.27	-0.03	0.27	0.08
	(0.19)	(0.13)	(0.25)	(0.22)	(0.11)	(0.09)	(0.10)	(0.06)	(0.08)	(0.06)
% effect	54.0	8.2	62.0	39.6	1.8	-5.6	21.2	-3.0	31.3	8.2
Post-3	0.58	0.05	0.08	-0.01	0.07	-0.02	0.38	0.07		
	(0.17)	(0.13)	(0.22)	(0.19)	(0.12)	(0.09)	(0.09)	(0.06)		
% effect	50.5	5.5	10.1	-1.1	9.0	-1.3	29.1	6.4		
<b>I. Weekly Working Hours (censored)</b>										
Post-1	2.3	-4.5	-8.3	-7.1	-3.3	-6.9	-0.2	-2.3	1.0	-2.1
	(4.4)	(3.4)	(5.0)	(3.5)	(3.4)	(2.8)	(2.3)	(1.7)	(2.2)	(1.7)
% effect	8.1	-12.6	-29.7	-17.7	-9.9	-15.8	-0.6	-5.7	3.4	-5.2
Post-2	4.2	-2.6	0.4	1.6	1.3	-2.3	-2.9	-5.0	2.7	-0.4
	(4.5)	(3.2)	(5.5)	(4.3)	(3.6)	(2.7)	(2.4)	(1.8)	(2.3)	(1.7)
% effect	14.8	-6.8	1.5	4.1	4.1	-5.3	-9.1	-11.4	9.1	-1.0
Post-3	6.2	-0.6	-3.3	-2.1	1.4	-2.2	-3.2	-5.2		
	(4.6)	(3.5)	(5.0)	(3.8)	(3.3)	(2.4)	(2.2)	(1.5)		
% effect	21.5	-1.8	-11.9	-7.0	4.4	-4.9	-9.8	-11.8		

**Table A.3: Female sample matching estimates**  
(standard errors in parentheses)

	First Call		Second Call		Fourth Call		Sixth Call		Eighth Call	
	DID	CS	DID	CS	DID	CS	DID	CS	DID	CS
<b>A. Employment</b>										
Post-1	0.04 (0.07)	0.07 (0.05)	0.20 (0.08)	0.19 (0.06)	0.10 (0.05)	0.09 (0.04)	0.08 (0.04)	0.08 (0.03)	0.13 (0.04)	0.12 (0.03)
Post-2	0.10 (0.07)	0.13 (0.05)	0.15 (0.08)	0.14 (0.06)	0.09 (0.05)	0.09 (0.04)	0.03 (0.04)	0.03 (0.03)	0.11 (0.04)	0.09 (0.03)
Post-3	0.17 (0.07)	0.20 (0.05)	0.08 (0.08)	0.07 (0.05)	0.06 (0.05)	0.06 (0.04)	0.10 (0.04)	0.10 (0.03)		
<b>B. Paid Jobs</b>										
Post-1	0.22 (0.07)	0.08 (0.05)	0.25 (0.08)	0.21 (0.06)	0.14 (0.05)	0.12 (0.04)	0.12 (0.04)	0.08 (0.03)	0.14 (0.04)	0.12 (0.03)
Post-2	0.27 (0.07)	0.13 (0.05)	0.22 (0.08)	0.18 (0.06)	0.11 (0.06)	0.10 (0.04)	0.08 (0.04)	0.03 (0.03)	0.11 (0.04)	0.09 (0.03)
Post-3	0.28 (0.07)	0.13 (0.05)	0.09 (0.07)	0.04 (0.05)	0.06 (0.05)	0.04 (0.04)	0.15 (0.04)	0.11 (0.03)		
<b>C. Formal Jobs</b>										
Post-1	0.21 (0.04)	0.20 (0.03)	0.20 (0.04)	0.19 (0.04)	0.11 (0.03)	0.12 (0.03)	0.19 (0.03)	0.14 (0.02)	0.12 (0.02)	0.09 (0.02)
Post-2	0.20 (0.04)	0.19 (0.04)	0.20 (0.04)	0.19 (0.03)	0.08 (0.03)	0.09 (0.03)	0.15 (0.03)	0.10 (0.02)	0.13 (0.02)	0.10 (0.02)
Post-3	0.17 (0.03)	0.15 (0.03)	0.13 (0.04)	0.13 (0.04)	0.09 (0.03)	0.10 (0.03)	0.16 (0.03)	0.11 (0.02)		
<b>D. Real Monthly Earnings</b>										
Post-1	176.8 (51.8)	126.1 (29.6)	113.2 (31.7)	72.0 (24.6)	93.6 (24.7)	74.1 (20.0)	90.9 (34.8)	28.2 (30.1)	119.0 (14.7)	51.9 (10.9)
% effect	73.8	51.4	53.9	26.8	52.9	29.8	35.5	8.3	60.0	24.2
Post-2	146.7 (50.1)	96.0 (23.9)	169.0 (35.1)	127.8 (29.2)	83.8 (24.8)	64.3 (21.1)	97.1 (23.4)	34.4 (16.6)	121.0 (18.2)	53.9 (15.2)
% effect	61.2	34.4	80.4	53.4	47.4	23.9	38.0	14.6	61.0	22.2
Post-3	127.8 (57.8)	77.1 (36.8)	140.5 (42.3)	99.3 (37.7)	136.4 (35.2)	116.9 (31.8)	110.0 (25.3)	47.3 (18.7)		
% effect	53.3	27.3	66.9	35.4	77.1	46.6	43.0	20.0		
<b>E. Real Hourly Earnings</b>										
Post-1	0.33 (0.45)	0.39 (0.13)	0.41 (0.20)	0.18 (0.14)	0.28 (0.15)	0.22 (0.11)	0.31 (0.22)	0.08 (0.19)	0.29 (0.09)	0.10 (0.07)
% effect	23.0	31.3	30.0	13.6	27.5	18.0	21.8	4.7	26.3	9.1
Post-2	0.39 (0.48)	0.44 (0.13)	0.70 (0.20)	0.48 (0.14)	0.36 (0.16)	0.30 (0.13)	0.23 (0.14)	0.00 (0.09)	0.37 (0.11)	0.18 (0.08)
% effect	26.7	32.6	51.7	38.8	35.9	22.7	16.1	-0.2	33.4	15.9
Post-3	0.21 (0.48)	0.26 (0.15)	0.69 (0.23)	0.47 (0.17)	0.50 (0.19)	0.44 (0.17)	0.28 (0.14)	0.05 (0.09)		
% effect	14.3	19.0	51.1	35.8	50.1	36.4	19.7	4.1		
<b>F. Weekly Working Hours</b>										
Post-1	13.0 (4.7)	7.1 (2.7)	2.8 (4.6)	2.7 (2.9)	5.2 (3.1)	3.1 (2.1)	3.0 (2.7)	0.0 (1.8)	9.5 (2.7)	5.2 (1.7)
% effect	32.3	15.6	7.1	5.3	11.6	6.0	6.6	0.1	21.0	10.5
Post-2	4.5 (4.7)	-1.4 (2.9)	5.1 (5.0)	5.0 (3.3)	3.5 (3.4)	1.4 (2.4)	4.7 (2.7)	1.8 (1.7)	5.0 (2.6)	0.7 (1.7)
% effect	11.1	-2.8	12.9	10.4	7.7	2.6	10.4	3.4	11.1	1.3
Post-3	11.1 (4.7)	5.2 (3.4)	0.2 (4.6)	0.1 (3.0)	4.3 (3.5)	2.2 (2.2)	6.2 (2.7)	3.2 (1.7)		
% effect	27.7	11.2	0.4	0.2	9.6	4.3	13.6	6.4		

continues...

**Table A.3**

	First Call		Second Call		Fourth Call		Sixth Call		Eighth Call	
	DID	CS	DID	CS	DID	CS	DID	CS	DID	CS
<b>G. Real Monthly Earnings (censored)</b>										
Post-1	106.9 (25.4)	67.9 (21.1)	131.8 (23.8)	104.4 (20.8)	81.1 (17.2)	67.3 (14.9)	83.8 (20.9)	39.2 (18.5)	87.9 (10.4)	53.4 (8.9)
% effect	137.7	72.8	165.2	104.2	104.8	61.0	70.5	23.9	103.1	58.9
Post-2	130.2 (25.2)	91.2 (20.7)	149.3 (25.8)	121.8 (22.4)	81.1 (20.8)	67.3 (18.6)	66.3 (15.4)	21.7 (11.7)	88.3 (11.0)	53.8 (9.3)
% effect	167.7	84.6	187.1	141.1	104.8	53.6	55.8	17.3	103.6	53.5
Post-3	113.0 (25.1)	74.1 (20.9)	99.2 (25.1)	71.8 (22.5)	94.2 (25.0)	80.4 (23.5)	101.3 (17.0)	56.7 (13.8)		
% effect	145.6	94.2	124.4	69.7	121.8	62.5	85.3	51.7		
<b>H. Real Hourly Earnings (censored)</b>										
Post-1	0.40 (0.14)	0.23 (0.10)	0.60 (0.13)	0.43 (0.10)	0.28 (0.09)	0.23 (0.08)	0.37 (0.12)	0.17 (0.11)	0.31 (0.06)	0.19 (0.05)
% effect	85.4	49.1	115.3	87.8	64.5	43.0	55.6	20.1	64.8	40.2
Post-2	0.60 (0.15)	0.43 (0.10)	0.66 (0.14)	0.50 (0.11)	0.37 (0.12)	0.32 (0.11)	0.21 (0.08)	0.01 (0.06)	0.34 (0.06)	0.22 (0.05)
% effect	127.6	81.9	128.0	113.2	85.3	51.8	32.1	1.6	70.6	46.3
Post-3	0.48 (0.15)	0.31 (0.09)	0.48 (0.13)	0.32 (0.10)	0.39 (0.13)	0.34 (0.12)	0.38 (0.09)	0.17 (0.07)		
% effect	102.0	80.6	93.3	67.2	89.2	54.2	56.7	31.1		
<b>I. Weekly Working Hours (censored)</b>										
Post-1	4.9 (3.7)	4.6 (2.9)	13.5 (4.2)	12.4 (3.3)	9.2 (3.0)	7.2 (2.3)	7.3 (2.4)	3.3 (1.8)	11.5 (2.1)	8.3 (1.6)
% effect	35.9	23.7	71.4	55.4	43.6	29.7	32.0	12.6	55.1	36.7
Post-2	7.5 (3.9)	7.2 (3.2)	12.0 (4.4)	10.8 (3.3)	7.4 (3.3)	5.4 (2.5)	6.1 (2.4)	2.2 (1.8)	9.0 (2.1)	5.8 (1.6)
% effect	54.9	34.2	63.0	50.6	35.2	21.1	26.9	7.4	43.0	23.9
Post-3	13.2 (3.7)	12.8 (2.9)	6.2 (4.1)	5.0 (3.1)	6.1 (3.3)	4.1 (2.6)	11.7 (2.4)	7.8 (1.7)		
% effect	96.1	87.5	32.8	25.7	29.2	14.8	51.7	31.3		

**Table A.4: 16-20 year olds sample matching estimates**  
(standard errors in parentheses)

	First Call		Second Call		Fourth Call		Sixth Call		Eighth Call	
	DID	CS	DID	CS	DID	CS	DID	CS	DID	CS
<b>A. Employment</b>										
Post-1	-0.01 (0.07)	0.04 (0.05)	0.12 (0.08)	0.09 (0.05)	0.03 (0.05)	0.02 (0.04)	0.04 (0.03)	0.07 (0.02)	0.07 (0.03)	0.07 (0.03)
Post-2	0.01 (0.07)	0.06 (0.05)	0.12 (0.08)	0.09 (0.06)	0.04 (0.05)	0.04 (0.04)	-0.03 (0.04)	0.00 (0.03)	0.06 (0.03)	0.06 (0.02)
Post-3	0.07 (0.07)	0.12 (0.05)	0.02 (0.08)	-0.01 (0.06)	0.05 (0.05)	0.04 (0.04)	0.01 (0.04)	0.04 (0.03)		
<b>B. Paid Jobs</b>										
Post-1	0.23 (0.07)	0.08 (0.05)	0.15 (0.08)	0.13 (0.06)	0.05 (0.05)	0.03 (0.04)	0.08 (0.03)	0.06 (0.02)	0.08 (0.03)	0.07 (0.03)
Post-2	0.22 (0.07)	0.06 (0.05)	0.16 (0.08)	0.13 (0.06)	0.05 (0.05)	0.03 (0.04)	0.02 (0.04)	0.01 (0.03)	0.06 (0.03)	0.06 (0.02)
Post-3	0.22 (0.07)	0.07 (0.05)	0.00 (0.09)	-0.03 (0.06)	0.04 (0.05)	0.02 (0.04)	0.07 (0.04)	0.05 (0.03)		
<b>C. Formal Jobs</b>										
Post-1	0.20 (0.04)	0.18 (0.03)	0.24 (0.04)	0.22 (0.04)	0.11 (0.03)	0.11 (0.03)	0.12 (0.03)	0.09 (0.02)	0.10 (0.02)	0.08 (0.02)
Post-2	0.16 (0.05)	0.13 (0.04)	0.19 (0.04)	0.18 (0.04)	0.09 (0.04)	0.09 (0.04)	0.10 (0.03)	0.07 (0.02)	0.11 (0.02)	0.09 (0.02)
Post-3	0.12 (0.04)	0.09 (0.03)	0.14 (0.05)	0.13 (0.05)	0.10 (0.03)	0.10 (0.03)	0.09 (0.03)	0.06 (0.03)		
<b>D. Real Monthly Earnings</b>										
Post-1	194.1 (36.2)	121.8 (25.0)	82.3 (31.4)	70.0 (24.1)	50.6 (23.7)	37.8 (19.7)	87.7 (27.0)	22.4 (23.3)	105.8 (14.3)	40.9 (9.8)
% effect	71.6	45.1	34.0	23.9	27.7	12.6	31.9	5.9	46.3	16.2
Post-2	117.9 (30.6)	45.6 (18.0)	96.8 (38.8)	84.5 (33.1)	38.3 (24.4)	25.5 (20.3)	100.0 (19.4)	34.7 (14.0)	118.9 (16.2)	54.0 (12.3)
% effect	43.5	14.0	40.0	25.0	20.9	7.8	36.3	13.1	52.0	18.8
Post-3	104.0 (36.4)	31.7 (25.7)	20.5 (48.6)	8.2 (42.2)	86.9 (31.0)	74.1 (28.3)	79.2 (19.2)	13.9 (13.3)		
% effect	38.3	9.9	8.5	2.1	47.5	24.7	28.8	4.9		
<b>E. Real Hourly Earnings</b>										
Post-1	0.68 (0.17)	0.43 (0.10)	0.57 (0.20)	0.29 (0.16)	0.28 (0.12)	0.26 (0.08)	0.45 (0.16)	0.10 (0.13)	0.36 (0.07)	0.13 (0.05)
% effect	43.6	31.0	38.2	20.3	28.7	21.2	28.2	5.9	29.3	11.3
Post-2	0.39 (0.17)	0.14 (0.10)	0.61 (0.18)	0.33 (0.13)	0.26 (0.14)	0.24 (0.12)	0.46 (0.11)	0.11 (0.07)	0.40 (0.08)	0.17 (0.06)
% effect	25.4	9.3	40.8	23.1	26.6	17.1	28.8	9.2	32.8	13.5
Post-3	0.37 (0.19)	0.12 (0.11)	0.30 (0.26)	0.02 (0.21)	0.30 (0.17)	0.28 (0.15)	0.42 (0.11)	0.07 (0.07)		
% effect	24.1	8.4	20.2	1.1	30.4	20.2	26.2	5.6		
<b>F. Weekly Working Hours</b>										
Post-1	15.3 (4.0)	6.3 (2.3)	-3.0 (4.4)	0.9 (2.9)	-3.0 (2.9)	-2.9 (1.9)	0.8 (2.1)	-1.3 (1.3)	5.4 (1.9)	1.8 (1.3)
% effect	35.5	14.1	-7.5	1.7	-6.6	-5.1	1.8	-2.4	11.8	3.3
Post-2	9.6 (3.8)	0.6 (2.3)	-0.4 (4.4)	3.4 (3.1)	-1.3 (3.3)	-1.1 (2.0)	0.6 (2.1)	-1.5 (1.3)	3.9 (1.9)	0.3 (1.3)
% effect	22.3	1.1	-1.1	6.2	-2.8	-2.1	1.3	-2.7	8.5	0.5
Post-3	11.5 (3.8)	2.4 (2.3)	-3.4 (4.3)	0.5 (2.9)	0.1 (2.9)	0.2 (1.9)	0.1 (2.2)	-1.9 (1.3)		
% effect	26.6	4.9	-8.4	1.0	0.2	0.4	0.3	-3.5		

continues...

**Table A.4**

	First Call		Second Call		Fourth Call		Sixth Call		Eighth Call	
	DID	CS	DID	CS	DID	CS	DID	CS	DID	CS
<b>G. Real Monthly Earnings (censored)</b>										
Post-1	140.8 (25.5)	82.3 (20.9)	102.8 (28.3)	85.1 (22.9)	34.9 (19.7)	23.3 (17.6)	71.6 (19.7)	29.9 (17.4)	77.8 (10.9)	42.7 (9.0)
% effect	<i>130.1</i>	<i>64.8</i>	<i>104.5</i>	<i>61.5</i>	<i>41.0</i>	<i>13.8</i>	<i>52.3</i>	<i>14.1</i>	<i>73.1</i>	<i>33.3</i>
Post-2	105.7 (26.1)	47.2 (22.2)	105.1 (32.8)	87.3 (29.4)	39.4 (19.8)	27.8 (18.0)	57.1 (13.7)	15.4 (10.3)	85.1 (11.7)	50.0 (9.9)
% effect	<i>97.7</i>	<i>30.1</i>	<i>106.8</i>	<i>58.9</i>	<i>46.2</i>	<i>15.1</i>	<i>41.7</i>	<i>9.7</i>	<i>79.9</i>	<i>33.8</i>
Post-3	99.4 (23.4)	40.9 (18.3)	30.5 (36.4)	12.8 (32.6)	62.2 (23.0)	50.6 (21.1)	61.8 (14.7)	20.1 (11.9)		
% effect	<i>91.9</i>	<i>34.3</i>	<i>31.0</i>	<i>6.9</i>	<i>73.0</i>	<i>27.9</i>	<i>45.1</i>	<i>12.3</i>		
<b>H. Real Hourly Earnings (censored)</b>										
Post-1	0.61 (0.13)	0.31 (0.10)	0.54 (0.16)	0.36 (0.12)	0.19 (0.09)	0.16 (0.07)	0.36 (0.11)	0.14 (0.09)	0.31 (0.05)	0.17 (0.04)
% effect	<i>98.1</i>	<i>48.1</i>	<i>89.6</i>	<i>54.2</i>	<i>42.6</i>	<i>23.0</i>	<i>46.2</i>	<i>13.8</i>	<i>54.2</i>	<i>28.2</i>
Post-2	0.48 (0.13)	0.19 (0.10)	0.53 (0.15)	0.35 (0.12)	0.23 (0.10)	0.20 (0.09)	0.27 (0.08)	0.05 (0.05)	0.33 (0.06)	0.19 (0.04)
% effect	<i>78.0</i>	<i>25.2</i>	<i>87.2</i>	<i>56.1</i>	<i>50.8</i>	<i>25.0</i>	<i>34.7</i>	<i>6.3</i>	<i>58.1</i>	<i>28.8</i>
Post-3	0.47 (0.12)	0.17 (0.08)	0.26 (0.18)	0.08 (0.15)	0.24 (0.12)	0.21 (0.10)	0.32 (0.08)	0.10 (0.05)		
% effect	<i>75.9</i>	<i>31.9</i>	<i>43.2</i>	<i>9.8</i>	<i>53.8</i>	<i>25.4</i>	<i>40.9</i>	<i>12.8</i>		
<b>I. Weekly Working Hours (censored)</b>										
Post-1	5.9 (3.6)	3.6 (2.7)	6.4 (4.5)	5.5 (3.4)	0.1 (3.2)	-0.7 (2.5)	3.3 (2.1)	1.5 (1.6)	7.9 (2.0)	5.0 (1.6)
% effect	<i>31.8</i>	<i>14.9</i>	<i>28.8</i>	<i>19.3</i>	<i>0.5</i>	<i>-2.2</i>	<i>13.9</i>	<i>4.9</i>	<i>34.7</i>	<i>17.4</i>
Post-2	6.0 (3.9)	3.7 (3.2)	8.0 (4.4)	7.1 (3.7)	2.2 (3.2)	1.4 (2.5)	0.2 (2.1)	-1.5 (1.7)	7.0 (2.0)	4.0 (1.5)
% effect	<i>32.3</i>	<i>13.9</i>	<i>35.8</i>	<i>24.6</i>	<i>9.7</i>	<i>4.2</i>	<i>1.1</i>	<i>-4.4</i>	<i>30.5</i>	<i>13.4</i>
Post-3	10.2 (3.7)	8.0 (2.7)	1.3 (4.6)	0.3 (3.6)	2.5 (3.1)	1.7 (2.3)	2.4 (2.2)	0.7 (1.7)		
% effect	<i>55.5</i>	<i>38.9</i>	<i>5.6</i>	<i>1.2</i>	<i>11.1</i>	<i>5.0</i>	<i>10.4</i>	<i>2.1</i>		

**Table A.5: 21-25 year olds sample matching estimates**  
(standard errors in parentheses)

	First Call		Second Call		Fourth Call		Sixth Call		Eighth Call	
	DID	CS	DID	CS	DID	CS	DID	CS	DID	CS
<b>A. Employment</b>										
Post-1	-0.09 (0.09)	-.01617 (0.07)	0.00 (0.08)	0.02 (0.06)	0.03 (0.06)	0.01 (0.05)	0.00 (0.04)	-0.01 (0.03)	0.00 (0.05)	0.00 (0.04)
Post-2	-0.02 (0.09)	.05127 (0.06)	0.05 (0.08)	0.07 (0.06)	0.05 (0.06)	0.03 (0.04)	-0.01 (0.04)	-0.02 (0.03)	0.05 (0.05)	0.05 (0.04)
Post-3	0.05 (0.09)	0.13 (0.07)	0.03 (0.09)	0.05 (0.07)	0.02 (0.06)	0.00 (0.04)	0.03 (0.04)	0.02 (0.03)		
<b>B. Paid Jobs</b>										
Post-1	0.02 (0.09)	-0.02 (0.07)	0.07 (0.08)	0.05 (0.06)	0.08 (0.07)	0.02 (0.05)	0.07 (0.04)	-0.01 (0.03)	0.01 (0.05)	0.00 (0.04)
Post-2	0.11 (0.09)	0.07 (0.06)	0.11 (0.09)	0.09 (0.06)	0.10 (0.06)	0.03 (0.05)	0.04 (0.04)	-0.03 (0.03)	0.06 (0.05)	0.06 (0.04)
Post-3	0.14 (0.09)	0.10 (0.07)	0.06 (0.09)	0.05 (0.07)	0.06 (0.06)	0.00 (0.04)	0.08 (0.04)	0.01 (0.03)		
<b>C. Formal Jobs</b>										
Post-1	0.16 (0.06)	0.15 (0.05)	0.12 (0.05)	0.08 (0.05)	0.14 (0.04)	0.13 (0.04)	0.17 (0.04)	0.14 (0.03)	0.08 (0.05)	0.07 (0.03)
Post-2	0.13 (0.06)	0.11 (0.05)	0.20 (0.06)	0.17 (0.05)	0.05 (0.04)	0.05 (0.04)	0.13 (0.04)	0.10 (0.03)	0.09 (0.05)	0.08 (0.04)
Post-3	0.16 (0.05)	0.14 (0.05)	0.12 (0.05)	0.08 (0.04)	0.14 (0.04)	0.13 (0.04)	0.15 (0.04)	0.11 (0.03)		
<b>D. Real Monthly Earnings</b>										
Post-1	162.4 (52.0)	40.5 (37.0)	87.0 (43.9)	47.1 (36.9)	69.4 (28.2)	26.9 (24.7)	28.6 (34.0)	-57.6 (28.4)	65.9 (22.4)	-3.8 (16.4)
% effect	48.6	12.2	35.9	13.7	28.3	8.2	7.8	-12.2	25.3	-1.3
Post-2	168.2 (55.2)	46.3 (37.9)	133.7 (48.6)	93.7 (42.4)	55.2 (28.1)	12.7 (23.9)	84.4 (26.7)	-1.9 (17.6)	70.6 (28.3)	0.9 (24.5)
% effect	50.3	11.8	55.1	29.4	22.5	3.6	23.0	-0.6	27.1	0.3
Post-3	151.4 (62.1)	29.5 (48.5)	150.8 (57.8)	110.8 (52.7)	111.6 (25.5)	69.0 (20.8)	125.7 (25.2)	39.4 (17.3)		
% effect	45.3	7.0	62.1	33.4	45.6	23.2	34.2	12.5		
<b>E. Real Hourly Earnings</b>										
Post-1	0.72 (0.41)	0.36 (0.19)	0.25 (0.25)	0.04 (0.20)	0.12 (0.14)	0.05 (0.10)	0.08 (0.18)	-0.24 (0.15)	0.09 (0.14)	-0.13 (0.10)
% effect	36.5	24.0	17.8	2.2	9.6	3.2	4.4	-10.8	6.6	-8.6
Post-2	0.82 (0.45)	0.46 (0.20)	0.79 (0.33)	0.58 (0.30)	0.04 (0.14)	-0.03 (0.12)	0.25 (0.12)	-0.08 (0.08)	0.24 (0.16)	0.02 (0.12)
% effect	41.6	27.5	56.3	39.7	3.0	-1.9	13.3	-5.3	16.9	1.2
Post-3	0.56 (0.42)	0.20 (0.19)	0.57 (0.29)	0.36 (0.26)	0.34 (0.13)	0.27 (0.10)	0.53 (0.13)	0.20 (0.09)		
% effect	28.4	11.5	40.4	21.1	27.7	19.3	28.2	14.3		
<b>F. Weekly Working Hours</b>										
Post-1	7.1 (4.8)	-4.5 (3.1)	4.7 (4.7)	1.2 (3.3)	8.6 (3.4)	1.1 (2.2)	3.8 (2.6)	-1.1 (1.9)	5.9 (3.3)	2.3 (2.3)
% effect	16.0	-8.7	10.8	2.3	16.9	2.0	7.9	-2.1	12.7	4.5
Post-2	5.1 (4.8)	-6.6 (3.2)	3.8 (4.7)	0.3 (3.0)	7.7 (3.5)	0.2 (2.4)	6.3 (2.4)	1.3 (1.7)	2.6 (3.1)	-1.1 (2.1)
% effect	11.4	-11.4	8.8	0.6	15.2	0.4	12.8	2.4	5.5	-1.9
Post-3	10.7 (4.7)	-0.9 (3.1)	5.9 (4.8)	2.4 (3.4)	9.1 (3.2)	1.6 (2.0)	6.2 (2.3)	1.3 (1.4)		
% effect	24.0	-1.6	13.5	4.8	18.0	3.1	12.7	2.3		

continues...

**Table A.5**

	First Call		Second Call		Fourth Call		Sixth Call		Eighth Call	
	DID	CS	DID	CS	DID	CS	DID	CS	DID	CS
<b>G. Real Monthly Earnings (censored)</b>										
Post-1	70.9	3.4	70.9	51.6	69.7	31.6	42.0	-33.0	49.1	-3.1
	(37.1)	(30.0)	(35.9)	(31.5)	(24.4)	(20.9)	(28.3)	(23.5)	(19.5)	(15.5)
% effect	43.1	1.9	64.4	28.0	46.9	16.7	16.3	-10.3	29.1	-1.5
Post-2	133.2	65.7	118.3	99.0	63.6	25.5	64.1	-10.9	69.4	17.2
	(38.2)	(31.1)	(39.1)	(34.4)	(25.6)	(22.2)	(23.4)	(17.2)	(21.9)	(18.7)
% effect	80.9	31.9	107.4	62.7	42.8	11.7	24.8	-4.5	41.2	7.9
Post-3	122.5	55.0	97.0	77.7	88.8	50.7	106.4	31.4		
	(43.3)	(36.5)	(39.1)	(35.3)	(24.3)	(20.5)	(22.3)	(16.5)		
% effect	74.4	28.5	88.1	57.0	59.8	24.9	41.2	14.6		
<b>H. Real Hourly Earnings (censored)</b>										
Post-1	0.34	0.10	0.25	0.14	0.20	0.09	0.16	-0.15	0.10	-0.08
	(0.24)	(0.15)	(0.20)	(0.17)	(0.12)	(0.09)	(0.15)	(0.12)	(0.11)	(0.09)
% effect	35.0	12.0	39.0	14.7	27.5	10.7	12.4	-9.5	11.0	-8.2
Post-2	0.67	0.43	0.65	0.54	0.17	0.05	0.21	-0.10	0.26	0.08
	(0.25)	(0.17)	(0.22)	(0.19)	(0.13)	(0.11)	(0.12)	(0.09)	(0.12)	(0.09)
% effect	69.8	49.5	100.8	74.0	22.3	5.2	16.0	-8.9	28.9	8.6
Post-3	0.50	0.26	0.41	0.30	0.31	0.20	0.47	0.16		
	(0.24)	(0.15)	(0.20)	(0.17)	(0.12)	(0.09)	(0.11)	(0.08)		
% effect	51.9	32.7	63.2	42.7	41.5	20.7	35.5	16.5		
<b>I. Weekly Working Hours (censored)</b>										
Post-1	-1.6	-4.5379	3.5	3.3	8.6	3.2	3.9	-0.8	4.1	0.8
	(4.6)	(3.6)	(4.8)	(3.6)	(3.8)	(3.0)	(2.7)	(2.0)	(3.1)	(2.4)
% effect	-7.1	-15.411	15.2	10.3	27.5	10.1	11.3	-2.1	13.5	2.3
Post-2	3.8	0.8	5.6	5.3	8.1	2.6	4.2	-0.5	4.7	1.4
	(4.9)	(3.7)	(5.0)	(3.6)	(3.7)	(2.9)	(2.6)	(1.9)	(3.3)	(2.5)
% effect	16.7	2.7	24.1	18.9	25.7	7.7	12.0	-1.3	15.3	3.7
Post-3	9.6	6.7	5.0	4.8	7.0	1.6	6.9	2.1		
	(4.9)	(3.9)	(4.9)	(3.8)	(3.6)	(2.8)	(2.5)	(2.0)		
% effect	42.4	24.3	21.6	23.0	22.4	4.3	19.7	5.7		











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