FINAL REPORT

DO THE POOREST AMONG THE POOR BENEFIT LESS FROM ACTIVE LABOR MARKET PROGRAMS? EVIDENCE FROM PERU'S PROJOVEN

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1. Introduction

The paradigm of the representative agent, which assumes that a public policy has the same impact on all treated individuals, has produced a vast array of empirical work that focuses on mean treatment impacts (see the survey in Heckman, Lalonde, and Smith 2001). Theory, however, predicts treatment heterogeneity in the impact of training and welfare programs on earnings and income (e.g., Bitler, Gelbach, and Hoynes, 2006).

Most of the existing literature on heterogeneous impacts is based on social experiments carried out in developed countries. For instance, Heckman, Smith, and Clemens (1997), find strong evidence that heterogeneity is an important feature of impact distributions using experimental data from the National Job Training Partnership Act Study (JTPA). Black, Smith, Berger, and Noel (2002) using experimental data from the Kentucky WPRS program find that the estimated impact of treatment varies widely across quantiles of the outcome distributions. Bitler, Gelbach, and Hoynes (2004, 2006) find strong evidence against the common effect assumption using experimental data from the Connecticut's Job First Waiver program and Self-Sufficiency Project in Canada.

For developing countries, we have scant evidence about heterogeneity in program evaluation. Djebbari and Smith (2005) and Dammert (2006) are, to the best of our knowledge, the first studies that study heterogeneous impacts from conditional cash transfer programs in Latin America. The authors find that program impacts on wealth and nutrition are greater for households who were at the higher level of wealth and nutrition prior to the program.

In this paper, we analyze whether the poorest among the poor benefit the same from active labor market programs that targets disadvantaged youths in Peru. This research question is particularly relevant in the Peruvian case where the training system excels in reproducing initial poverty conditions among youngsters (Jaramillo et al. 2007, Valdivia 1997). We use a non-experimental training program, the Youth Training Program PROJOVEN, which has provided training to around 40,000 disadvantaged young individuals aged 16 to 24 since 1996. The availability of data for five different public calls in a program operating for almost a decade has created an extraordinary opportunity to estimate the overall heterogeneity of program impacts across households' relative wealth status.

To estimate heterogeneous treatment impacts across the wealth distribution, we construct an index based on the household asset information that PROJOVEN collects in order to assess eligibility of applicants. The basic methodology consists in approximating socioeconomic level through a household's asset index, which is based on principal component analysis of a determined number of asset variables. The method used here provides a simple technique for creating a long-run wealth proxy in the absence of either income or expenditure data. It has been used in examining wealth differences in socioeconomic outcomes such as education attainment (Filmer and Pritchett 2001), mortality, morbidity and utilization of health facilities (Gwatkin, Rutstein, Johnson, Pande, and Wagstaff 2000, Bonilla-Chacin and Hammer 1999), and fertility, and contraceptive use (Stecklov, Bommier, and Boerma 1999). We report a large difference in the mean values across quantiles in the estimated wealth index distribution. This result suggests that the principal component method applied to the PROJOVEN data has the ability to sort individuals into different percentiles of the population.

We have four main findings. First, we find strong evidence about the effectiveness of the PROJOVEN program. We find large overall point estimates for monthly earnings and relative small impacts for the employment outcome. The results are particularly robust for females who show much higher treatment impacts than male participants for both earnings and employment outcomes. Second, quantile treatment effects (QTE) show strong heterogeneity in the impacts of the PROJOVEN program. In fact, 30 percent of the participants in the program report identically zero treatment effects, whereas those individuals between the 40 and 70 quantiles report the highest treatment impacts. For quantiles 80 to 90, the treatment group earnings exceed the comparison group earnings but they eventually become smaller.

Third, both parametric OLS models and semiparametric matching models do not reject the null hypothesis that treatment does not vary with the individuals' initial poverty level. This is a steady result for all public calls and independent of whether we measure the impacts 6 or 18 months after the program. Put differently, the PROJOVEN program does not reproduce the large wealth gaps observed in the Peruvian labor market. Fourth, there is evidence of Ashenfelter's dip in this program. Parametric estimators that are consistent when the model of program participation stipulates pre-program earnings dip show that our results are robust to alternative identifying assumptions.

The remainder of the paper is organized as follows. In section 2 we briefly discuss the institutional analysis of the PROJOVEN program. Section 3 provides an overview of the program design and operation. We then present the evaluation data in section 4. Section 5 presents the QTE treatment impacts. In section 6 we present the principal component analysis of household's wealth status. Our main results appear in section 7. Section 8 concludes.

2. Institutional Analysis of PROJOVEN

2.1 Macroeconomic and Labor Market Context

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

Employment growth followed growth in output, though not equitably for different social or demographic groups. Specifically, both unemployment and underemployment rates for youth more than doubled those for adult workers. Thus, this one group seemed

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

[2] Note that Chile Joven was also 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).

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 are a sizable part of total population, slightly above 30 percent. Its participation in the labor force is also large, accounting for more than one-fourth of it (see Appendix Table A.1).[3]

From 1992 to 1997, labor force participation (LFP) grew at a rate of 2.3 percent per year. The labor market absorbed the increase in labor supply mainly trough job creation. In effect, after 1992 labor demand took a growing pace with a strong increase in the employment rate, which more than compensated the increase in the working-age population; thus, the increase in LFP was absorbed by an increasing labor demand.

The financial market and trade liberalization contributed to reduce the relative price of physical capital, and thus allowed firms to acquire new capital and hire high-skilled workers, a complement factor in production. At the same time, the deregulation of the labor market allowed firms to replace expensive older workers with more educated younger workers willing to accept lower wages. However, as we will see below, education quality is unequally distributed among youth, being the poorest in considerable disadvantage. The PROJOVEN program precisely aimed to give extra support to poor youth, so that they can improve their chances of benefiting from current labor market trends.

After a sharp fall in the late eighties, between 1992 and 1997 real monthly earnings grew at a 3.3 percent per year, along with the rise in GDP and labor demand. Concurrently, the dispersion of real earnings was also increasing. In terms of relative earnings, measured with respect to average earnings, by 1997 both women and younger

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

men with more education improved their relative situation with respect to the period 1986-1989. At the same time, by 1997 older workers and specially men with low levels of education experienced the bigger drop in their relative position compared to their situation in 1986-89. These upward trends on relative earnings for women and skilled workers can be explained by an increase in the relative demand biased towards women and high-skilled workers, which more than compensated the increase on the relative supply of these groups (Saavedra 1996a and 1996b, Diaz 1999). Given these trends, pertinent training may increase youth income, in addition to increasing their chances of a job placement. Below we test whether the PROJOVEN program managed to produce any of these potential effects.

2.2 The Education and Training Sectors

The expansion of the educational system in the seventies and eighties did translate in nineties youth having more years of education than previous generations. However, this came with a declining trend in public education expenditure per-student and thus a drop in the quality of education.[4] Further, according to ENAHO, in 1997 less than 10 percent of youth coming from poor households had any formal education beyond secondary. The education 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. Although youth from poor households have access to training, they use it with less frequency than youth from non-poor households and tend to attend less qualified institutions in the context of a highly heterogeneous and largely unregulated training market (Jaramillo and Díaz 2007). A significant effort in training was needed to support disadvantaged youth in making room for them in an increasingly competitive labor market. Thus, two elements,

[4] In 1990 expenditure per student was half of that in 1970 (Saavedra et al. 1996).

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.

2.3 Youth and Poverty in Urban Peru

This section describes in broad strokes the poverty situation in urban Peru for both the general population and, specifically, for youth. In addition to provide context on the poverty situation in urban Peru, this descriptive analysis is useful for two main purposes. First, being aware of youth relative poverty situation (relative to total population) is essential to evaluate the appropriateness of the program, that is, if its focus on poor youth is in accordance with the disadvantaged situation of this group. Second, specific analysis of the situation of the urban youth in poverty, the target population of PROJOVEN program, provides insights about inequalities within this group and therefore about potential for heterogeneous impacts of PROJOVEN on their beneficiaries, which is the central topic of this paper. Thus, below we explore education, labor market status and labor income among poor urban youth.

As set out in Appendix Table A.1, total urban population of Peru is around 18 million people, of which a high 25% is between 16 and 24 years old. In the capital of the country, Metropolitan Lima, this percentage is higher (31%), possibly reflecting its role as a pole of attraction for young migrants.[5] In all the urban areas, men and women are fairly equally distributed within the young population.

[5] The most widely accepted classification of Peru's geography identifies three regions: Coast, Highlands and the Amazonic Region. Representative household survey data is available for these three regions, in addition to Metropolitan Lima. PROJoven has provided vocational training to approximately 42,000 youngsters in ten major cities across the country: Trujillo, Chiclayo, Piura and Chimbote (Coast); Arequipa and Huancayo (Highlands); Iquitos (Amazonic Region) and Lima and Callao (Metropolitan Lima).

An important aspect concerning training programs is the educational level of the target population. This is summarized in Appendix Table A.2. It is remarkable the relatively high educational attainment among urban youth: 97% has completed elementary school, while 70% has finished secondary education and more than one fourth has some kind of post secondary education. Among the latter, half of them have at least some non-university tertiary education and the other half has some university education. An interesting fact is the percentage of women with secondary and post secondary education is higher, although only marginally so, than the percentage of men with such educational level.

Though general educational attainment is relatively high, differences are significant across the distribution of income. This can be gathered from a comparison between the lowest and the highest income quintiles. As shown in Appendix Table A.3, the percentage of youth with complete secondary education is 72% in Metropolitan Lima and 60% in the urban area for youth belonging to the lowest income quintile, while it reaches 90% for both areas for youth in the highest income quintile. Similarly, the percentage of youth with post secondary education is 31% in Metropolitan Lima and 25% in the urban area for the lowest income quintile while around 66% for both areas for the highest income quintile. On the other hand, the gender inequality goes in the opposite direction for each quintile. In the lowest income quintile, women have on average lower educational levels, while the opposite occurs in the highest quintile, where the percentage of women with complete secondary is twenty percentage points higher than that for men and the percentage of women with post secondary education almost doubles that of men.

As a result, an interesting finding is that inequality between quintiles is much greater among women that among men.

Labor market status of target age population is also a crucial aspect to consider for training programs. In urban Peru, youth represents 23% of total labor force and its activity rate is 45%. Appendix Table A.4 presents a comparison between youth and total labor force. The activity rate of youth is about two-thirds that among total population, but the gap is narrower among women. This has to do with the fact that younger cohorts of women tend to participate more in the labor market. It stands out that the portion of labor force currently (at the moment of the survey) with a job is significantly lower for youth, in other words unemployment is more prevalent among youth. This fact supports the focus of active labor market policies on this disadvantaged group. As far as the distribution of occupation by gender, no significant differences are found between youth and the labor force as a whole.

As far as indicators of welfare, the average levels of monthly expenditures and income among Peruvian urban household are US$ 549 and US$ 641, respectively; while in per capita terms, US$ 115 and US$ 134. There are clear differences between Metropolitan Lima and total urban areas, the former showing higher levels of both expenditure and income.[6]

In order to have an idea of distributional inequality, we estimated the same indicators for the lowest and the highest income quintiles (Appendix Table A.5). These indicators reveal the high levels of inequality existent in urban Peru. The average monthly expenditure (per household and per capita) is on average 8 times higher for the

[6] It must be pointed out that these data are in nominal terms. Since the cost of living is higher in Lima, the actual difference between Lima and total urban area is not as large.

highest income quintile than for the lowest income quintile. On the income side, the difference is even bigger, being the average monthly income 14 times higher for the highest quintile than for the lowest quintile. It is important to notice that income inequality is even greater in Metropolitan Lima, being on average 17 times higher for the highest quintile than for the lowest quintile. Therefore, the capital of the country, the area with higher income levels, has also the highest inequality levels.

The large income gaps hold for the young population as well. In Appendix Table A.6, it can be seen that the average monthly labor income for the highest quintile is on average 11 times higher than for the lowest income, individuals in the latter quintile earning around US$ 30 per month.

Income distribution among the PROJOVEN's target population, roughly the two lowest income quintiles, constitutes relevant information for the present study, since income heterogeneity is a potential source of heterogeneous effects. Appendix Table A.7 presents income distribution indicators for youth and households of the two lowest income quintiles, which are useful to highlight the inequality both between these income quintiles and within each of them. It must be pointed out that average labor income of youth belonging to the second lowest income quintile is more than two times the income of the most disadvantaged youth. Also, inside each income quintile significant inequality also shows up. It must be noted that inequality is higher inside the lowest income quintile, with a standard deviation representing around 40% of the mean, than inside the second lowest income quintile, in which the standard deviation represents around 20% of the mean. Income dispersion among households is similar to income dispersion among youth.

Income inequality typically goes together, to some extent, with health and education inequality; and all three of them can potentially influence the benefit level obtained from PROJOVEN program. This fact already suggests the idea of heterogeneity in PROJOVEN's impacts. On the other hand, the relationship between youth income and household wealth has yet to be established. We do this below with data from PROJOVEN.

In sum, this sub-section has described some relevant features of PROJOVEN's target population. Among youth, average educational attainment is relatively high, but unequally distributed: poorer youth tend to have significantly lower educational levels. As far as labor market status, close to half of the Peruvian urban youth are active and, as it is typical in labor markets of both developed and non-developed economies, their unemployment rates are systematically higher than for those in the total labor force. Income levels are particularly low for those youth in the poorest quintile, but significant variance is found both within and between the two lowest quintiles. This means that there is vast room for active labor market policies targeted to this group, such as training programs, oriented to increase the chances of job placement and to improve the earnings of their beneficiaries. It also indicates that not all participants are on equal foot to take advantage of PROJOVEN, suggesting potential for heterogeneous impacts of PROJOVEN on their beneficiaries across the distribution of pre-treatment earnings.

3. The PROJoven Program

3.1 Goals and Treatment

The Youth Training Program PROJOVEN is an ongoing active labor market policy that seeks to improve the productivity and employability of disadvantaged youth through labor training services. The PROJOVEN program was designed as a demand-driven program, with public and private training institutions competing for public resources through international bidding processes. Since its creation in 1996, and for almost a decade, over 40,000 out-of-school unemployed poor individuals aged 16 to 24 years old have been selected as beneficiaries of PROJOVEN, and a total of 542 training institutions have participated, providing more than 2,160 vocational courses.[7]

The PROJOVEN program provides funding for basic training in low-skill occupations. The treatment consists of a mix of formal and on-the-job training organized into two sequential phases. The first consists of 300 hours of classes at the training center locations roughly five hours per day for three months. The program covers the full cost of the courses. In the second phase, training institutions must place trainees into a paid, on-the-job training experience in private manufacturing firms for an additional period of three months. In this phase, trainees are supposed to receive no less than the minimum wage paid by the firm providing the internship.

To ensure the paid, on-the-job training experience, the program relies on a demand-driven mechanism that stipulates that all training centers must present, as part of their offers, formal agreements with private firms that guarantee a paid, on-the-job training for each beneficiary. This design requires a strong match between the content of

[7] General equilibrium effects for the PROJOVEN program are highly unlikely because the modest number of trainees with respect to the Peruvian youth poor population (less than 1 percent in 10 years of operation).

the training courses and the firm's labor skill requirements. It supposes a strict coordination between the training institutions and firms when designing and implementing the training courses. As a result, the coverage of this training program is limited because of its costly design and relatively intense package of services.

3.2 The Selection of Training Services

The selection of training services follows a two-step standardized process. The first step focuses on the selection of training institutions. Before determining the eligibility of prospective young beneficiaries, the program operator opens a training directory called RECAP where all training institutions that want to participate in the program have to enroll. To be part of the RECAP, the training centers must pass a minimum quality threshold following standardized instruments that mostly evaluate the legal status (formality) and the existence of some acceptable level of human resources and infrastructure.

In the second step, the program operator invites institutions enrolled in the RECAP to participate in public bidding processes where the selection of training courses rather than training institutions takes place. The selection of the training courses relies on bidding processes that targets the relatively best training courses at the best competing prices. The program operator selects those courses with the relative highest scores at the best competing prices. The number of selected courses depends on the available training slots that are determined ex-ante.

3.3 The Beneficiary Selection Process

The beneficiaries' selection process consists of multiple stages governed by different actors: target individuals, bureaucrats, and training centers. Figure 1 shows the dynamic of this process. The program awareness strategy (position A) constitutes the first formal effort to reach out to the target population and aims to inform potential participants about the program's benefits and rules. This first filter, under the responsibility of the program operator through local training offices, focuses only on those neighborhoods with a high concentration of households below the poverty line. Those prospective participants attracted by the expected benefits and perceived opportunity costs of participation voluntarily show up in the registration centers (position B) where qualified personnel determine their eligibility status. A standardized targeting system based on five key observable variables (poverty status, age, schooling, labor market status, and pre-treatment earnings) determines who is eligible and who is not. The low percentage of targeting errors shows the combination of self-targeting with individual assessment through objective indicators has been quite effective.[8] This process concludes when the total number of eligible individuals exceeds by around 90 percent the total number of slots available in each public call on a first-come-first-served basis.

The eligibility status does not guarantee participation in the program. Program enrollment depends on both training centers' and applicants' willingness to pursue the application process to its conclusion. The program operator invites eligible individuals to an orientation process (position C), where they choose the courses they want to attend on a first-come-first-served basis. This process concludes when the number of eligible individuals exceeds by 75 percent the number of available slots in each course. Finally, the program operator sends to the training institutions this final pool of eligible applicants (position D). This is the only step where training institutions intervene in the selection of

[8] Targeting errors have been documented to be below 10 percent (Arróspide and Egger 2000).

participants. Because of the high dropout rate, as we will see below, on average the training institutions have a limited role in selecting beneficiaries.

4. Dropping Out and Program Participation

About half of those youth eligible and registered to participate in PROJOVEN drop out before being sent to a training institution (Table 1). This is large considering the rather short period of time, two to three months, between registering and being assigned to a course.[9] The existence of a database of eligibles where it is possible to identify beneficiaries from non-beneficiaries provides an opportunity to study the choice between participating and dropping out.

Conceptually, one would expect that those dropping out are the ones facing the highest costs from participating. Costs of participating include the opportunity cost of time, transportation, materials and other goods associated to attending the course. Opportunity costs will be higher for those who have relatively more human capital accumulated, those who already have a job or those who would have to travel longer distances in order to reach the training institution. While human capital is probably associated to the less poor among the poor, having a job is not necessarily. Transportation costs will be more onerous for the poorer participants, both in absolute terms, because they will tend to live in the more marginal urban areas, where training institutions participating in PROJOVEN tend not to be located, and relatively, because they will represent a larger portion of their household budgets. This is also the case for materials

[9] In the case of the widely studied JTPA program dropout rates were around 30 percent (Heckman Lalonde and Smith 2001).

and other goods participants will need to use to properly attend the course. This may include from notebooks and pencils to proper shoes or shirt.

Thus, we may conclude two things from the previous discussion. First, we expect that beneficiaries and dropouts will differ in several characteristics associated with the individuals' human capital and poverty status. Second, while dropping out may be associated to greater human capital, it may also be positively correlated with the poverty level of the youth and her household. Thus, the poorest among the poor may be self-selecting themselves out of PROJOVEN.

In order to test these ideas we use the database on PROJOVEN's eligibles. Table 1 compares the characteristics of eligible beneficiaries and eligible non-beneficiaries (dropouts) for the different rounds for which data are available and for the pooled data. Although differences are not large, we do find that beneficiaries differ from non-beneficiaries in some dimensions.

Dropouts are on average older and a greater percentage of them are men for all public calls considered, being the sixth call the only exception. Also, dropouts are on average less educated than beneficiaries. For all public calls a smaller percentage of non-beneficiaries have secondary education (incomplete or complete secondary), and for most calls and the pooled data also a smaller percentage of them have tertiary education. Evidence is not clear regarding to education level of the head of household. About labor characteristics, we find for all public calls that a higher percentage of dropouts were employed when registering for the program.

Table 1 also compares household assets for beneficiaries and non-beneficiaries. The household assets refer to features of the dwelling: floor materials, ceiling materials and toilet characteristics. Regarding floor materials, results for first and sixth call suggest that beneficiaries have floor of lower quality materials than non-beneficiaries. In contrast, dropouts have ceiling of lower quality materials for all calls, except for second and the sixth calls, and poorer toilet facilities for all calls, except for the first and second.

To identify which characteristics are associated with the decision to participate in the program, a probit model was estimated, participation being the dependant variable and the same variables presented in Table 1 the covariates. We use dwelling characteristics as indicators of socioeconomic status. The base values for the dwelling characteristics are the poorer ones. Table 2 sets out the results[10]. Sex is a significant predictor for all calls, being men more prone to drop out, as expected. Age is significant for all calls, except the first and second and the results indicate an inverse relation between age and participation. Schooling is also a significant predictor. Eligibles with highest education level tend to participate in the program. This is a counterintuitive result, as these youth has the highest opportunity cost. A possible explanation of this is that we may be capturing here the effect of socioeconomic status, as lower educational attainment is associated with greater poverty. Employment appears to be significant only in the sixth and eighth call, being the employed eligibles, as expected, more prone to drop out. Regarding dwelling characteristics, at least one of them suggests that lower socio-economic status explains dropping out in all public calls except in the first, while the eighth call, the one with more observations and with more covariates available, reports most clearly the inverse relation between poverty and participation.[11]

[10] We do not estimate a probit model for the pooled data since the definitions of the covariate values are not the same for each call.
[11] We must point out that first call is the one with fewer observations and with fewer covariates available.

In sum, results in this section suggest that a main determinant of participation in the program is the socioeconomic status of the participant, as, among those that are informed about the program and go through the registration process, it is the poorer who tend to drop out. Thus, exclusion in this case works through self-selection out of the program. Given the rules of the program concerning the number of slots available in the program, the large proportion of dropouts allow us to rule out cream-skimming on the average, though higher quality training institutions attracting a larger share of beneficiaries may be in a position to exercise selection. Overall, this is important new evidence that has not been considered in previous evaluations of PROJOVEN and that needs to be taken into account when assessing the equity aspects of the intervention.

5. The Evaluation Data

From 1996 to 2004, the period for which we currently have data, the PROJOVEN evaluation datasets consist of 10 different sub-samples associated with five different cohorts of beneficiaries receiving treatment in Lima, and five corresponding comparison group samples.[12] The beneficiary subsamples are selected by the program operator from a stratified random sample of the population of participants corresponding to the first, second, fourth, sixth, and eighth rounds of the program.[13]

Individuals in the corresponding comparison subsamples are selected from a random sample of "nearest-neighbor" households located in the same neighborhoods as

[12] These periods extend from November 1996 to April 1997; February 1998 to July 1998; March 1999 to August 1999; June 2000 to December 2000; and August 2001 to January 2002, respectively. The program operator constructs the baseline and follow-up datasets without any intervention from the training institutions to guarantee a technical and objective evaluation of the program.
[13] The total number of participants in these five rounds is 1,507, 1,812, 2,274, 2,583, and 3,114, respectively. The corresponding number of treated individuals in the random sample is 299, 321, 343, 405, and 421.

those participants included in the evaluation sample aimed at reaching the same target population. In particular, once the treatment group individuals are selected, a sample of comparison group individuals is selected by a survey fielded in the same poor neighborhoods where individuals from the treated group reside. The program operator uses the same eligibility instruments applied to the treatment sample and by pairing each beneficiary to a random neighbor who has the same sex, age, schooling, employment status, and initial poverty condition. The neighborhood dimension may have the ability to control some unobservables, including geographic segregation, transportation costs, and firms' location, which may affect the propensity to work and the potential outcomes. This costly evaluation design greatly ameliorates support problems in the data as we see later.

For each treated and untreated cohort combination, we have panel data collected in 4 rounds including a baseline and 3 follow-up surveys taken 6, 12, and 18 months after the program. The baseline survey provides rich information for all variables that define the eligibility status applied to treatment and comparison group individuals at the same calendar time. It also contains demographics and labor-market information. In fact, relevant factors affecting both the propensity to participate in the program and labor market outcomes are available. There is information, for example, on education attainment, marital status, number of children, parents' schooling, and participation in welfare programs.

The labor-market module includes information about labor force participation, experience, monthly earnings, working hours, occupation, firm's size, and participation in previous training courses. At the household level, we have information about family size, family income, and household's density rate. In addition, the datasets provide detailed information on dwelling characteristics including source of drinking water, toilet facilities, and house infrastructure (type of materials used in the floor, ceiling, and walls), which is used to build a wealth index. Moreover, the follow-up surveys provide detailed labor-market information for both treated and comparison groups, using the same definitions and variables as the baseline instruments, which minimize potential biases due to misalignment in the measurement of variables, and thus, overcoming one of the main criticisms when solving the evaluation problem with non-experimental data (Heckman, Ichimura, Smith, and Todd 1998).

5.1 Comparison of Pre-Treatment Sample Means

Table 3 compares the baseline means of several covariates for the treatment and comparison samples for each one of five different cohorts. Column 2 shows the means using the pooled sample and columns 3 to 7 show the means for the five different rounds. In terms of demographic and socioeconomic characteristics, Panel A shows the effectiveness of the "neighborhood" strategy to balance the distribution of covariates that determine the eligibility status. Both groups have the same average age (19), sex ratio (42 percent are males), and schooling attainment (85 percent have completed high school). The p-values for all cohorts under analysis do not reject the null hypothesis of equality of means. The data show, however, that both marital status and children variables have different distributions. About 90 percent of the participants are single and only 14 percent have children, which differ from the comparison sample, which has a lower proportion of single people (77 percent) and a higher proportion of individuals with offspring (25 percent). Estimated p-values reject the null hypothesis of equality of means in all cases.

Panel B compares labor market characteristics for treatment and comparison samples. Both groups have the same proportion of individuals in and out of the labor force. Approximately 52, 25, and 22 percent of individuals were employed, unemployed, and out of the labor force, respectively. These non-significant differences are consistent across all cohorts as is backed by the p-values. The type of work depicts a somewhat different pattern. A higher proportion of comparison individuals were working in the formal private sector (63 versus 54 percent) whereas a higher proportion of treated individuals were non-paid family workers (17 versus 10 percent). A comparison of monthly earnings also shows that treated units receive on average smaller earnings than their counterpart comparison sample, which is a steady result across all public calls.

Panel C compares households and dwelling characteristics. On average family income is somewhat smaller for treated individuals, although the p-values show mixed results across different public calls. In addition, the analysis of dwelling characteristics shows that a higher proportion of treated individuals live in houses with somewhat better infrastructure and access to flush toilet and piped water. These differences, however, are not significant for several cohorts. Finally, the father's schooling attainment is similar in both samples. The p-values do not reject the null hypothesis of equality of means for most of the categories.

In summary, Table 3 show that the treatment and comparison group individuals are similar in several dimensions, including sex, age, schooling, employment, father's education, previous training, and family size. This result reveals the efficacy of the "nearest-neighbor" approach to construct the comparison sample because of the balance of all variables that define eligibility status between treated and untreated groups. On the other hand, the data also reveal some significant differences in three key variables: marital status, presence of children, and unpaid family workers. The proportion of no-parent single individuals is much larger in the treatment group than in the comparison group, which suggests that those eligible individuals that do not applied to the program cannot afford the cost of losing current earnings because of their marital status. Similarly, a large proportion of treated individuals worked as unpaid family workers or housekeepers relative to the comparison group. These variables need to be incorporated in any econometric strategy that intends to eliminate selection bias.

6. Heterogeneous Treatment Impacts in PROJOVEN

The impact of the PROJOVEN program on earnings and employment of beneficiaries has been studied in partial evaluations of the program.[14] These studies focus on average treatment impacts that assume that everyone benefits the same from the program. The reported mean treatment impacts on monthly earnings range from 12 to 100 percent, whereas the mean gains on the probability of employment is estimated between 0 and 15 percentage points. These "common effect" estimates emerge from different econometric techniques and different parameters of interest, which prevents the comparison of the point estimates across different calls.[15]

To investigate whether the poorest among the poor benefit the same from the program, we first study whether there is heterogeneity in the distribution of treatment

[14] These are: Galdo (1998) for the first public call; Burga (2001) for the second public call; Chacaltana (2003) for the second and fourth public call; and Ñopo, Saavedra, and Robles (2003) for the sixth public call.

[15] For instance, Ñopo, Saavedra, and Robles (2002) implement nearest-neighbor matching and difference-in-differences parametric estimators, whereas Burga (2001) uses kernel regression matching.

impacts in the PROJOVEN program using quantile regression (Koenker and Bassett 1978).

6.1 Quantile Treatment Effects (QTE)

The quantile regression has been used to address heterogeneous treatment impacts in the context of training programs (Heckman, Smith, and Clements 1997, Abadie, Angrist, and Imbens 2002, and Friedlander and Robins 1997); welfare reform programs (Bitler, Gelbach, and Hoynes 2004a, 2004b); conditional cash transfer programs (Djebbari and Smith 2005, Dammert 2006); and profiling unemployment insurance programs (Black et al. 2003). The appeal for this semiparametric approach relies on its flexibility to accommodate observed and unobserved heterogeneity (Djebbari and Smith 2005) and on the evidence that intra-group variation in quantile treatment effects greatly exceeds the inter-group variation in mean impacts (Bitler et al. 2004a).

This technique provides a convenient framework for examining how the impact varies at different quantiles of the untreated outcome distribution. Let Y_1 and Y_0 denote the outcome of interest in the treated and untreated states with corresponding *CDFs* $F_1(y) \equiv \Pr[Y_1 \leq y]$ and $F_0(y) \equiv \Pr[Y_0 \leq y]$. Denote the *qth* quantiles of each distribution $y_j^q = \inf\{y : F_j(y) \geq q\}, j = 0,1$. Thus, we can define the quantile treatment effect as $\Delta^{QTE} = y_1^q - y_0^q$.[16] The identification follows from assuming perfect positive dependence (rank invariance), which implies that an observed individual would maintain his rank in the distribution regardless of his or her treatment status (Heckman et al. 1997).

In order to analyze distribution aspects of the PROJOVEN program impact, I use quantile regression of Y (monthly earnings) on an intercept and a discrete variable T

[16] For instance, estimating the quantile treatment effect at the 0.50 quantile involves taking the sample median for the treatment group and subtracting the sample median for the control group.

indicating whether the observation belongs to the treatment or comparison group. The impact estimate for a given quantile q distribution is the coefficient on the treatment indicator from the corresponding quantile regression. Under the null hypothesis of a common effect, these impact estimates should not vary over quantiles. To address the non-experimental nature of our data, we use the inverse propensity score-weighting (Bitler et al. 2004). This procedure corrects for bias in estimating quantiles of the counterfactual treated and control distributions, with the simple differences of sample adjusted quantiles then serving as consistent estimates of the population (see a formal proof in Firpo 2003).

To implement the weighting approach we estimate the propensity score that predicts the probability that the individual i is in the treatment group conditional on a rich set of baseline covariates, $P(T=1 \mid X=x)$. We estimate a logit model subject to the balancing test suggested by Dehejia and Wahba (1999). Parametric propensity score models that pass standard balancing tests are regarded as valid because they balance the distribution of pre-treatment covariates between matched units conditional on the propensity score (Lechner 2000).[17] The set of conditioning covariates include common demographic variables (sex, age, schooling, marital status, and number of children); labor market outcomes (past monthly earnings, employment status, type of work, previous training courses, duration of previous training); household characteristics (number of members, members / number of rooms, running water, drainage system, dwelling's quality materials); and father's educational attainment.

[17] It is important to indicate, however, that multiple versions of the balancing test exist in the literature, and little is know about their statistical properties or the relative efficiency among them.

Table 4 reports the coefficients and standard errors for logit models estimated separately for each public call. As expected, the covariates used to construct the comparison samples (age, sex, schooling, and employment status) are not significant predictors for program participation as they are balanced between treatment and comparison groups. In general, past earnings, experience, type of work, dwelling characteristics, father's education, and family density rate are the most important predictors of participation in the PROJOVEN program. The estimates also show that married individuals and people with offspring are less likely to participate, although the coefficients are not significant for some public calls.

Denoting the estimated propensity score for person i as $\hat{p}_i(x)$, we define the inverse propensity score-weighting as,

$$\hat{w}_i = \frac{\hat{p}_i(x)}{1 - \hat{p}_i(x)} \tag{1}$$

which are used to estimate the effects of treatment on the treated (Imbens 2004).[18] Under this approach the weighted empirical *CDFs* for Y_1 and Y_0 is given by

$$\hat{F}_1(y) = \frac{(1/n_1)\sum_{i=1}^{n_1} \hat{w}_i I(Y_1 \leq y)}{\sum_{i=1}^{n_1} \hat{w}_i} \quad \text{and} \quad \hat{F}_0(y) = \frac{(1/n_0)\sum_{i=1}^{n_0} \hat{w}_i I(Y_0 \leq y)}{\sum_{i=1}^{n_0} \hat{w}_i} \tag{2}$$

where n_1 *and* n_o are the number of treated and comparison observations and *I(.)* is an indicator variable.

6.2 QTE Estimates for Monthly Earnings

Figure 2 plots the quantiles treatment impacts for monthly earnings 6, 12, and 18 months after the program and for each public call separately. The associated dotted lines

[18] Alternative, Bitler et al. (2004) estimate $\hat{w}_i = \frac{T_i}{\hat{p}_i(x)} + \frac{1-T_i}{1-\hat{p}_i(x)}$ to uncover treatment effects for the entire population.

represent two-sided 90 percent confidence intervals. Overall, we find large and positive treatment impacts for earnings in all public calls and periods of time. The estimates depict large degree of treatment heterogeneity. For instance, 6 months after the program the QTE impacts range between [-S.17, S.172], [S.0, S.107], [S.0, S.185], [S.0, S.158], and [S.0, S.218] for the 1st, 2nd, 4th, 6th, and 8th public call, respectively.

These point estimates show clearly that not everyone benefits the same from the PROJOVEN program. In fact, participants in the first 30 quantiles of pre-treatment earnings report identically zero treatment effects, whereas those individuals between the 40 and 70 quantiles report the highest treatment impacts. For quantiles 80 to 90, the treatment group earnings exceed the comparison group earnings but they eventually become smaller. This particular QTE earnings distribution is steady across all public calls and, with the exception of the 1st public call; there is no evidence that suggest the treatment heterogeneity decreases (increases) over time. These findings suggest that households with lowest pre-treatment earnings do not benefit at all from the program.

Unlike welfare reform programs in which QTE heterogeneity is broadly consistent with static labor supply theory (Bitler et al. 2004), the identification of the source of QTE heterogeneity for the training treatment impacts is not direct. To test whether the individuals' wealth status explains this heterogeneity, we need to incorporate explicitly in our econometric analysis measures of wealth under the assumption that the initial wealth condition varies across individuals in the data.

7. Measuring Household's Wealth

To investigate the role of household economic status on PROJOVEN's heterogeneous treatment impacts, socioeconomic level data of beneficiaries is needed. Because household income or expenditure data are not readily available for treatment and comparison group individuals, we construct an asset index based on household asset information that PROJoven collects to assess eligibility of applicants. This method has been used in examining wealth differences in socioeconomic outcomes such as education attainment (Filmer and Pritchett 2001), mortality, morbidity and utilization of health facilities (Gwatkin, Rutstein, Johnson, Pande, and Wagstaff 2000, Bonilla-Chacin and Hammer 1999), and fertility, and contraceptive use (Stecklov, Bommier, and Boerma 1999).

In contrast to expenditure data that is highly variable and sensitive to transitory fluctuations (Jalan and Ravallion 1998), the asset index is more stable (Fields 1998 and Skoufias 1999) and contains less measurement error (Filmer and Pritchett 2001) when predicting household's economic status. Two additional features are noteworthy. First, by aggregating household assets, the index represents a proxy for long-run economic status rather than a measure either of current welfare or of poverty. In fact, we are only establishing a relative measure -households' ranking within the distribution- which in the context of our empirical problem makes sense since all treated and comparison individuals are by definition below the poverty line.

Second, the weight each asset receives in the construction of the index is not grounded theoretically. Thus, it is recommendable to perform empirical validation exercises to establish the robustness (internal validity) of the index. Evidence for India suggests that this approach is a robust valid measure of household wealth (Filmer and Pritchett 2001, Gwatkin et al. 2000), and comparable to the results emerging from consumption expenditures in a sample of nineteen countries (Wagstaff and Watanabe 2003).

7.1 Constructing the Wealth Index

From January 1996 to October 2004, the period for which we currently have data, the evaluation datasets cover about 3,500 treated and untreated poor individuals corresponding to the first, second, fourth, sixth, and eight rounds of the PROJOVEN program. The baseline surveys include information on 21 poverty indicators that can be grouped in four types: characteristics of the household's dwelling (6 indicators for the building materials used, 2 indicators for toilet facilities, 2 indicators for the source of drinking water, 2 indicators about rooms in the dwelling); household landownership, with 2 indicators; household's participation in welfare programs; and parent's education attainment.[19]

To aggregate this various asset indicators into one variable to proxy for household wealth, we use the statistical procedure of principal components that determine the weights for asset variable. This simple technique essentially consolidates the data around the covariance structure of the variables under the assumption, in this particular context, that household's long-run wealth explains the maximum variance-covariance in the wealth variables. Intuitively, it extracts from a set of variables those few orthogonal linear combinations of all the variables that capture the largest amount of information that is common to all of the variables (maximum variance). Then, it finds the second linear

[19] It is noteworthy to mention that there is no restriction on participation in any welfare program for those prospective applicants to the PROJOVEN program.

combination for the variables, orthogonal to the first, with maximum remaining variance, and so on. The first linear combination is called the first principal component of the set of variables.

The mathematical steps to perform the principal component analysis are detailed in Smith (2002) and Pande and Yazbeck (2002). Suppose we have a set of *k* variables (assets) associated to each individual *i*, $x_{1i}, \ldots, x_{ki}$. After normalizing the variables by its mean and standard deviation, we express each scaled variable as a linear combination of a set of underlying components for each individual *i,*

$$\begin{aligned} x_{1i} &= v_{11}X_{1i} + \ldots.. + v_{1k}X_{ki} \\ &\ldots.. \\ x_{ki} &= v_{k1}X_{1i} + \ldots.. + v_{kk}X_{ki} \end{aligned} \tag{3}$$

where *v* are the coefficients associated to the unobserved *X* components for each variable. To solve this system of equations, the Principal Component technique solve the equations $(R-\lambda_k I)v_k = 0$ for λ_k and v_k, where *R* is the covariance matrix between the variables *x*, λ_k are the eigenvalues of *R* and v_k their associated eigenvectors. Finally, the weights or "scoring factors", w_k, are recovered by inverting the system implied by Equation (3), which yield a set of estimates for each of the *k* principal components,

$$\begin{aligned} X_{1i} &= w_{11}x_{1i} + \ldots.. + w_{1k}x_{ki} \\ &\ldots.. \\ X_{ki} &= w_{k1}x_{1i} + \ldots.. + w_{kk}x_{ki} \end{aligned} \tag{4}$$

Once the asset index is obtained for each individual in the dataset, the individuals are ranked by their asset index score and divided into quintiles. Table 5 reports the weighs (or scoring factors) from the principal component analysis implemented separately for each round of the program. The mean of the index is 0 for all rounds with standard deviation in the range 1.46 to 1.93. Because the index ranks the households within each distribution, the weights differ from round to round although we observe similar patterns (signs) for all variables. In general, the characteristics of the household's dwelling receive the highest weights across all rounds. Because all the variables (except household members / rooms in the household) are categorical ones, it is easy to interpret the weights: a move from 0 to 1 changes the index by a factor equal to weight / standard deviation (reported in columns 3, 6, 9, 12, and 15). For instance, Column 3 shows that a treated or comparison individual that lives in a household with flush toilet has a wealth index higher by 1.005 than one who does not. Similarly, Column 6 shows that individuals living in households with low-quality walls (matting) have a wealth index lower by -0.89 than those who do not.

The last three rows of Table 5 report the mean wealth index for three different groups of individuals that according to the value of their index are assigned to the bottom quartile ("poorest"), second and third quartile ("poor"), and upper quartile ("less poor"). The difference in the mean index between the "poorest" and "less poor" individuals is remarkable: 3.99, 4.79, 3.97, 3.80, and 3.84 units for the first, second, fourth, sixth, and eight round, respectively. Moreover, the difference in the mean index between the "poorest" and "poor" individuals reaches 2.18, 3.77, 2.44, 2.23, and 2.03 units, respectively. These results suggest that the principal component method applied to the PROJOVEN data has the ability to sort individuals into different percentiles of the population.

7.2 Robustness Test

To evaluate the internal validity of the wealth index we investigate the mean distribution of the asset variables across the different percentiles of the PROJOVEN population. We expect that the "poorest" group individuals have the lowest level of asset ownership whereas the "less poor" group individuals present the highest level.

Table 6 reports the average asset ownership across the bottom (25 percent), middle (50 percent), and upper (75 percent) quartiles for all public calls of the PROJOVEN program. We find, as expected, that the asset ownership differ consistently across these groups of individuals in all rounds of the program. By looking at the first three columns, we observe for instance that whereas only the 3.3 percent of the "poorest" individuals have access to potable water, this percentage increases to 68 percent for the "poor" individuals and to 97 percent to the "less poor" individuals. Likewise, the house ownership increases from 36 percent ("poorest") to 85 percent ("poor") and 99 percent ("less poor") in the second round of the program. Also, 62 percent of the "poorest" individuals in the fourth round of the program live in houses with low-quality walls (matting) versus 22 percent for the "poor" individuals and 0 percent for the "less poor" individuals.

8. Treatment Impacts across Wealth Status

In this section we explore the heterogeneity of the impacts as a function of the estimated wealth index. It considers variation in treatment impacts through the interaction of the treatment indicator with the estimated long-run wealth status. If the PROJOVEN targeting mechanism is effective, one expect the poorest individuals will benefit the most from the program.

Let $Y_1(q)$ be the potential outcome in the treatment state $(T = 1)$ for an individual who is in the wealth quantile *q* and let $Y_0(q)$ be the potential outcome in the untreated state $(T = 0)$. We observe the pairs $(Y_1(q), T_1)$ and $(Y_0(q), T_0)$ but never $(Y_1(q), T_0)$ or $(Y_0(q), T_1)$. Because of this missing data problem, we cannot identify for any particular individual the treatment gains $\Delta_i = (Y_1(q) - Y_0(q))$. We focus, instead, on the average treatment impacts for subgroups of the population defined by the wealth index.

Our parameter of interest is the impact of treatment on the treated, which estimates the mean effect of attending training course rather than not participating on the individuals who attend the course:

$$\Delta_{TT} = E(Y_1(q) - Y_0(q) \mid T = 1) = E(Y_1(q) \mid T = 1) - E(Y_0(q) \mid T = 1). \qquad (5)$$

While $E(Y_1(q) \mid T = 1)$ may be estimated from the observed treatment sample, the right-hand side of the equation (5) contains the missing data $E(Y_0(q) \mid T = 1)$. Using the outcomes of untreated individuals to approximate the missing counterfactual yield the well-known selection bias because of differences in the distribution of observed and unobserved characteristics between *T=1* and *T=0.*

Because program participation in PROJOVEN depend on both observed and unobserved characteristics which leads to self-selection, we can proceed under the assumption that the distribution of systematic and unobserved differences varies across *T=1* and *T=0* but not over time within groups, which is the standard assumption of difference-in-differences models. This approach, however, may be sensitive to the specific definition of the ‘before” period if we observe a drop in the mean earnings of participants prior to program entry (Ashenfelter 1978).

Figure 3 depicts the earnings trajectory for the treatment and comparison groups for the 4th, 6th, and 8th public calls. The evidence suggests the existence of Ashenfelter’s Dip in the PROJOVEN program that may upward bias the conventional parametric difference-in-difference estimates. Instead, we use an alternative econometric estimator that is consistent when the model of program participation stipulates pre-program earnings dip. This is the so-called semi difference-in-differences model (LaLonde 1986). We use a regression-based estimator of the difference between the post-treatment earnings of treatment and comparison group members, holding constant the level of pre-treatment earnings and a set of control variables.

We compare treatment and comparison individuals through a linear regression of the outcome variables *Y* (i.e., monthly earnings and employment) on the treatment status *(T)* and interactions between *T* and dummy variables indicating whether the individual *i* is in the upper, middle, or bottom quartile of the wealth index distribution,

$$y_{it} = \delta_0 + \delta_1 W_{4i} + \delta_2 W_{2i} + \beta_0 T_i + \beta_1 T_i * W_{4i} + \beta_2 T_i * W_{2i} + \gamma y_{i,t-1} + X_{it}'\alpha + \varepsilon_{it} \qquad (6)$$

The individuals in the “poorest” group (W_1) are the omitted group and, therefore, the implicit counterfactual. The interaction terms are expected to be positive if individuals from “less poor” households (W_4 and W_2) benefit more from the program than individuals living in the “poorest” households (W_1). Equation (6) also controls for other baseline household and individual characteristics (X) to account for empirical differences in the covariate distribution between treatment and comparison groups. The X-vector includes sex, age, schooling, marital status, offspring, and pre-treatment participation in training courses. This parametric approach estimates the effect of a treatment under the assumptions of selection on observables and that simply conditioning linearly on the covariates suffices to eliminate selection bias.

We test whether the program impact along the wealth index is the same for all individuals by testing the following hypothesis

$$Ho : \beta_1 = \beta_2 = 0.$$

Rejecting this null hypothesis is evidence of heterogeneous program impacts emerging from differences in individuals' wealth status.

8.1 Parametric Estimates

Tables 7 and 8 report the estimation results for monthly earnings and employment outcomes 6, 12, and 18 months after the program. We do not reject the null hypothesis that treatment does not vary with the individuals' initial poverty level. This is a stable result for all public calls and independent of whether we measure the impacts 6, 12 or 18 months after the program. This result suggests that the strong treatment heterogeneity emerging from the QTE approach is not due to the variation in the initial poverty level of the beneficiaries. These parametric estimates also confirm that the PROJOVEN program has strong positive impacts on the monthly earnings of beneficiaries although they decrease somewhat over time. On the contrary, the average impacts on the employment variable are small and even negative for the second call (6 and 18 months after the program), fourth call (12 months after the program), and sixth call (12 months after the program).

The interaction of the treatment variable with a gender indicator report statistically significant varying effects for males and females. Hence, we re-do the parametric wealth analysis separately for males and females after pooling the data across all public calls to avoid large sampling variability due to small sample sizes. Two patterns emerge from Table 9. First, the earnings and employment treatment estimates are larger for females rather than males. This result suggests that the positive treatment impacts found in the PROJOVEN program is driven by the performance of female beneficiaries. In particular, the differences found in the employment variable are striking. Second, we do not reject again the null hypothesis that treatment does not vary with the individuals' initial poverty level for both males and females. This is a consistent result in the short- and medium-term. The p-values in all cases are above 0.10. It is noteworthy, however, the sign of the interaction terms for males (mostly negative) and females (mostly positive), which suggest that the "poorest" males and the "less poor" females benefit more from the program.

8.2 Matching Treatment Impacts across Wealth Status

We exploit the panel structure of the evaluation data by implementing both difference-in-difference and cross-section propensity score matching methods. In general, propensity score matching methods are better equipped to deal with the pre-treatment earnings dip after forcing one to compare individuals with the same pre-treatment observable characteristics. This approach also allow us to relax any linear assumption that may mask the earnings-wealth relationship by taking weighted averages over the outcomes of observationally similar untreated individuals.

Difference-in-differences matching methods (Heckman et al. 1997) are conditional semiparametric versions of the widely used parametric approach. This method solves the evaluation problem by subtracting the before-after change in untreated outcomes from the before-after change for treatment outcomes. The identifying assumption justifying this matching estimator is that there exists a set of conditioning variables X such that

$$E(Y_{0t}(q) - Y_{0t'}(q) \mid X, T = 1) = E(Y_{0t}(q) - Y_{0t'}(q) \mid X, T = 0)\,. \qquad (7)$$

where *t'* and *t* refer to before and after the start of the program. This conditional independence assumption (CIA) ensures that after conditioning on a rich set of observable variables, the outcomes for treated and untreated individuals follow a parallel path.

Matching methods force us to compare comparable individuals by relying on the common support assumption

$$\Pr(T = 1 \mid X) < 1 \text{ for all } X. \qquad (8)$$

The support condition ensures that for each X satisfying assumption (7) there is a positive probability of finding a match for each treatment individual. Otherwise, if there are X for which everyone received treatment, then it is not possible for matching to construct the counterfactual outcomes for these individuals. In this sense, matching forces us to compare comparable individuals in a way that standard regression methods do not.

Under conditions (7) and (8), we estimate the treatment impacts by computing first the counterfactual outcome for each treatment unit using a weighted average of the comparison units' outcomes over the common support region, and then averaging these results over the treatment group sample

$$\Delta^{DID} = \frac{1}{n_1} \sum_{i \in n_1 \cap S_p} \left\{ [Y_{1t}(q) - Y_{0t'}(q)] - \left\{ \sum_{j \in n_0 \cap S_p} W(\| i - j \|)[Y_{0t}(q) - Y_{0t'}(q)] \right\} \right\}. \qquad (9)$$

where n_1 and n_0 are the sample of treatment and comparison individuals, S_p is an indicator function that takes the value 1 for individuals in the common support region (0 otherwise) and $W(\|i-j\|)$ is the key weighting function that depends on the Euclidian distance between each comparison group individual and the treatment group individual for which the counterfactual is being constructed. We estimate the counterfactual outcome $\sum W(\|i,j\|)[Y_{0t}(q)-Y_{0t'}(q)]$ using local linear regression methods that were developed in the early 1990s by Fan (1992) and have more recently been considered in the evaluation literature by Heckman, Ichimura, Smith, and Todd (1998). This nonparametric approach relies on standard kernel weighting functions that assign greater weight to individuals who are similar (in terms of the estimated propensity score $\hat{p}$), and is more efficient than local constant regression methods because of its lower boundary bias in regions of sparse data.[20]

The price to be paid for the greater flexibility of local linear matching is the selection of the bandwidth parameter that achieves the best possible trade-off between bias and variance (Imbens 2004). We choose the bandwidth h to minimize the approximation to the MISE (of the estimated counterfactual mean regression function) associated with a particular bandwidth given by

$$\text{MISE}(h) = \arg\min_h \left(\frac{1}{n_0} \sum_{j=1}^{n_0} \left((Y_{0jt} - Y_{0jt'}) - \hat{m}_{-j}(\hat{p}(x_j), h) \right)^2 \right). \tag{10}$$

where $\hat{m}_{-j}(\hat{p}(x_j), h)$ denotes the estimated conditional mean function for the untreated outcome evaluated at $\hat{p}(x_j)$ using all of the untreated units except unit "*j*". The benefit

[20]The local constant regression presents bias inversely proportional to the density distribution of the untreated sample. This feature may be problematic because typically the mass of the treated individuals is located in regions where the number of untreated ones is sparse.

of this cross-validation approach comes from using out-of-sample forecasts rather than in-sample fit to guide the bandwidth choice. This approach implicitly weights the MISE calculation by the distribution of estimated propensity scores in the untreated sample. Operationally, this approach proceeds via a grid search over a set of candidate bandwidths specified in advance.[21]

8.3 Matching Estimates

Tables 10 and 11 present difference-in-difference (DID) and cross-section (CS) matching estimates for monthly earnings and employment outcomes reported separately for each public call. In our estimation we only include the observations inside the overlapping empirical support region. To impose the support condition we follow the "trimming" procedure proposed by Heckman et al. (1997).

The upper panel (A) depicts short-run treatment impacts whereas the lower panels (B and C) present medium-term impacts. Within each panel, three different parameters of interest are presented: the average treatment effect on those located on the top quantile of the wealth index ("less poor"), the average treatment effect on those located in the second and third quantile ("poor"), and the average treatment effect on those located in the bottom quantile ("poorest"). In all cases, we estimate the counterfactuals using the full set of comparison group observations. The point estimates for the treatment impacts are presented along with their corresponding bootstrapped standard errors estimated with 200 replications.

Four patterns emerge. First, all matching estimates show the PROJOVEN program is an effective, active labor-market initiative. For instance, 6 months after the program the earnings treatment impacts on the treated ranges from S.27 to S.107 soles for

[21] The grid for the bandwidth search equals [0.05, 0.10,…, 2].

individuals located on the top quantile of the wealth index and from S.44 to S.104 soles for individuals located in the bottom quantile of the poverty distribution. The employment effects are also positive for most public calls although small relative to earnings impacts.

Second, there is no evidence that link in any systematic way households' wealth status to the size of the treatment impacts. In fact, the matching estimates reinforce the OLS findings regarding the no rejection of the null hypothesis that treatment does not vary with the individuals' initial poverty level. In this aspect, the PROJOVEN program is very effective in not reproducing commonly observed wealth gaps on labor outcomes in the Peruvian labor market.

Third, the cross-section matching estimates are lower than the difference-in-difference estimates. This is explained by the existence of Ashenfelter's dip in the PROJOVEN data although these differences are modest. Finally, in almost all public calls the point estimates also indicate a downward trend in the evolution of the earning gains over time, which is consistent with theoretical predictions emerging from human capital models (Becker, 1993).

Table 12 reports matching estimates for males and females separately using the pooled data. The qualitative conclusions are similar to those emerging from the OLS estimation. In particular, females show much higher treatment impacts than males in both the short- and medium-term. In addition, we do not find any systematic relationship between treatment impacts and pre-treatment household' wealth status for neither of these two groups.

9. Summary of Findings and Policy Discussion

The following are the main findings in the analysis:

1. The large proportion of dropouts allows us to explore the determinants of participation in the program among eligible individuals. An important determinant of participation in the program is the socioeconomic status of the participant: poorer eligible youth tend to drop out. Thus, exclusion in this case works through self-selection out of the program. Given the rules of the program concerning the number of slots available in the program, the large proportion of dropouts allow us to rule out cream-skimming on the average, though higher quality training institutions attracting a larger share of beneficiaries may be in a position to exercise selection.

2. The data reveal the effectiveness of the "neighborhood" strategy to balance the distribution of covariates that determine the eligibility status. Both groups have the same average age (19), sex ratio (42 percent are males), and schooling attainment (85 percent have completed high school). The p-values for all cohorts under analysis do not reject the null hypothesis of equality of means. On the other hand, the data also reveal some significant differences in three key variables: marital status, presence of children, and unpaid family workers. The proportion of single individuals is much larger in the treatment group than in the comparison group, which suggests that those eligible individuals that did not apply to the program cannot afford the cost of losing current earnings because of their marital status. Similarly, a large proportion of treated individuals worked as unpaid family workers or housekeepers relative to the comparison group. These variables need to be incorporated in any econometric strategy that intends to eliminate selection bias.

3. The quantile treatment effects (QTE) show strong heterogeneous impacts in the PROJOVEN program. In fact, the first 30 pre-treatment earnings quantiles of the participants in the program report identically zero treatment effects, whereas those individuals between the 40 and 70 quantiles report the highest treatment impacts. For quantiles 80 to 90, the treatment group earnings exceed the comparison group earnings but they eventually become smaller. These findings suggest that households with lowest pre-treatment earnings do not benefit at all from the program. Unlike welfare reform programs in which QTE heterogeneity is broadly consistent with static labor supply theory (Bitler et al. 2004), the identification of the source of QTE heterogeneity for the training treatment impacts is not straightforward. To test whether the individuals' wealth status explains this heterogeneity, we need to incorporate explicitly in our econometric analysis measures of wealth under the assumption that exist variation in the wealth status across individuals in the data.

4. The difference in the mean wealth index between the "poorest" and "less poor" individuals is remarkable: 3.99, 4.79, 3.97, 3.80, and 3.84 units for the first, second, fourth, sixth, and eight round, respectively. Moreover, the difference in the mean wealth index between the "poorest" and "poor" individuals reaches 2.18, 3.77, 2.44, 2.23, and 2.03 units, respectively. These results suggest that the principal component method applied to the PROJOVEN data has the ability to sort individuals into different wealth percentiles of the population.

5. The evidence suggests the existence of Ashenfelter's Dip in the PROJOVEN program that may upward bias the difference-in-difference estimates.

6. Both parametric OLS models and semiparametric matching models do not reject the null hypothesis that treatment does not vary with the individuals' initial poverty level. This is a steady result for all public calls and independent of whether we measure the impacts 6 or 18 months after the program. This result suggests that the strong treatment heterogeneity emerging from the QTE approach is not due to the variation in the initial poverty level of the beneficiaries. Put differently, the PROJOVEN program does not reproduce the wealth gaps observed in the Peruvian labor market.

7. The earnings treatment impacts are relatively higher than the employment treatment impacts for most public calls and rounds of the program. In addition, these treatment estimates for both earnings and employment decrease over time.

8. The interaction of the treatment variable with a gender indicator report statistically significant varying effects for males and females: the earnings and employment treatment estimates are larger for females rather than males. This result suggests that the positive treatment impacts found in the PROJOVEN program is driven by the performance of female beneficiaries.

Policy discussion

The Youth Training Program PROJOVEN corresponds to a new array of demand- driven training programs implemented in Latin America in the mid-1990s in the midst of structural reforms in the labor markets. Similar programs have been implemented in Argentina (Proyecto Joven), Chile (Chile Joven), Uruguay (Opcion Joven), and Colombia (Youth Training Program). Therefore, our findings about the effectiveness of this particular program exceed the evaluation of the Peruvian labor market. This "last generation" of active labor market policies is based on market-based approaches where public resources are assigned to training institutions via public bidding processes. In this context, knowing whether this program produces the desired impacts or not constitutes a test about the effectiveness of market-based approaches to improve the employability and productivity of disadvantaged individuals.

Several of the findings presented in this report are of interest to policy makers. First, policy makers interested in enhancing equity aspects of social programs should be interested in the process of participation. While targeting and self-selection may ensure that non-poor do not participate in programs intended for the poor, they do not ensure that the poorest among the poor participate. The evidence from PROJOVEN suggests that some effort should be invested in the latter direction as well, since self-selection out of the program by the poorer has been shown to be an issue. Identification of the factors that prevent the poorer from participating would be an important step in order to establish strategies that are effective in incorporating them. We suggest that it may be both opportunity costs, be it through employment or doing chores around the household, or direct costs of participating (transportation, clothing, materials, etc.), but lack the empirical evidence to back up these suspicions.

Second, regarding the impact of the program, overall results indicate that PROJOVEN's design is not only an effective mechanism to enhance employability of youth from poor households that participate in the program, but it is also equity enhancing among them as the evidence indicates similar returns for participants along varying poverty levels. This result is likely related to the demand-driven mechanism, which is a central feature of the program, and which conditions part of the payoff to training institutes to placing the youth in a private firm for a minimum of three months.

This conditioning on training institutes motivates them to produce course contents more adequate to what firms are expecting from young workers. Although our evidence indicates that there is little space for average cream-skimming, this is associated with the large proportion of dropouts. Thus, countries or sub-national governments looking to incorporate training as part of their policies to support the poor would do good in looking at PROJOVEN's design.

Third, impacts are largest for women, which suggests that interventions such as PROJOVEN are relevant options for policy makers interested in reducing labor market gender gaps. This is possibly associated with the fact that because of discrimination and other factors women face greater difficulties in getting access to proper employment. Within this context, exposing the participant to a package of basic training and a practical experience in the firm seems to go a long way towards changing the labor market prospects of the young women participating in the program. It should be noted that PROJOVEN's design includes a stipend for single mothers to cover costs of childcare. This information may also be important for the discussion of which groups should this type of policy target in the context of tight public budgets.

Fourth, PROJOVEN seems better fit to improve earnings of participants than changing their employment status, as impacts on earnings are fairly consistently higher than on employment. This result suggests that it is harder to produce changes in employment status than in earnings. In other words, while the training dose seems adequate to produce changes in earnings it does not seem to work the same for employment. Thus, if the goal of the government is to improve employment opportunities of those youth who do not have a job, policy makers should consider alternative treatments. Short of starting a different program, it may be a good idea to experiment with a training module within the PROJOVEN setting specifically oriented to this goal.

Finally, an also relevant policy issue concerns the sustainability of treatment impacts. Our evidence shows that PROJOVEN impacts tend to decline, but remain positive up to eighteenth months after treatment. These estimates should be compared to the program's cost to have a reliable cost- benefit analysis.

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Appendix Table A.1. Peru: Distribution of Young Population in Urban Areas, 2004

	Total Population	Young Population (16 - 24 years old)			
		Total	% of Total Population	Men	Women
Coast	4 835 669	977 059	20%	489 222	487 837
Highlands	3 485 632	776 987	22%	393 322	383 665
Amazonic Region	1 674 874	324 080	19%	168 956	155 123
Metropolitan Lima	7 942 245	2 427 399	31%	1 180 795	1 246 603
Total Urban Area	17 938 420	4 505 525	25%	2 232 296	2 273 229

Source: National Household Survey (ENAHO) 2004.

Appendix Table A.2. Peru: Educational levels among urban youth (17-24 year-old), 2004

Educational level	Metropolitan Lima		Urban Area	
	Men	Women	Men	Women
No formal education	0.46%	0.17%	0.40%	0.39%
Incomplete elementary school	1.68%	2.06%	2.45%	3.17%
Complete elementary school	2.77%	4.29%	3.79%	4.39%
Incomplete secondary education	27.09%	19.73%	27.22%	23.77%
Complete secondary education	44.70%	48.92%	41.25%	40.51%
Incomplete non-university tertiary education	7.20%	7.30%	8.09%	8.89%
Complete non-university tertiary education	3.43%	3.59%	4.13%	4.95%
Incomplete university education	12.10%	12.35%	11.80%	12.09%
Complete university education	0.58%	1.59%	0.85%	1.83%
Post-degree education	0.00%	0.00%	0.02%	0.01%

Source: ENAHO 2004. Elaborated by authors.

Appendix Table A.3: Peru: Educational Level among Urban Youth.

Lowest and highest income quintiles

Educational level	Metropolitan Lima				Urban Area			
	Lowest Income Quintile		Highest Income Quintile		Lowest Income Quintile		Highest Income Quintile	
	Men	Women	Men	Women	Men	Women	Men	Women
No formal education	0.0%	0.0%	0.0%	0.0%	1.0%	0.9%	0.0%	0.0%
Incomplete elementary school	8.3%	3.2%	0.0%	0.0%	6.8%	6.6%	0.0%	0.1%
Complete elementary school	0.0%	0.0%	1.4%	0.0%	5.4%	6.3%	3.7%	0.0%
Incomplete secondary education	21.6%	23.0%	18.6%	0.0%	23.1%	29.2%	16.7%	0.0%
Complete secondary education	33.5%	47.6%	33.1%	14.7%	37.1%	33.4%	29.8%	16.4%
Incomplete non-university tertiary education	20.0%	5.3%	7.4%	2.9%	13.0%	8.1%	6.9%	3.1%
Complete non-university tertiary education	0.0%	6.5%	14.2%	19.4%	3.7%	6.5%	17.3%	21.1%
Incomplete university education	16.5%	9.8%	10.8%	42.3%	9.5%	6.3%	9.4%	36.5%
Complete university education	0.0%	4.6%	14.6%	20.8%	0.3%	2.7%	16.2%	22.7%

Source: ENAHO 2004. Elaborated by authors.

Appendix Table A.4. Peru: Urban Labor Force

	Activity Rate			Occupied labor force		
	Men	Women	Total	Men	Women	Total
Total labor force						
Metropolitan Lima	78.3%	58.0%	67.6%	93.3%	90.7%	92.1%
Urban Area	79.2%	59.7%	69.1%	93.6%	91.9%	92.8%
Young labor force						
Metropolitan Lima	38.7%	36.0%	37.3%	86.6%	83.3%	85.0%
Urban Area	49.0%	41.5%	45.2%	88.0%	85.4%	86.8%

Source: ENAHO 2004. Elaborated by authors.

Appendix Table A.5. Peru: Average Expenditure and Income per quintile

	Metropolitan Lima		**Urban Population**	
	Lowest Income Quintile	Highest Income Quintile	Lowest Income Quintile	Highest Income Quintile
Average monthly expenditure per household (US$)	116	957	108	884
Average monthly income per household (US$)	77	1332	82	1154
Average monthly per capita expenditure per household (US$)	31	254	29	234
Average monthly per capita income per household (US$)	21	339	22	307

Source: ENAHO 2004. Elaborated by authors.

Appendix Table A.6. Peru: Average Monthly Labor Income per quintile for Youth

	Metropolitan Lima		Urban Population	
	Lowest Income Quintile	Highest Income Quintile	Lowest Income Quintile	Highest Income Quintile
Average Monthly Income of Youth (16 - 24 years old) (US$)	32	338	30	337

Source: ENAHO 2004. Elaborated by authors.

Appendix Table A.7. Peru: Income Distribution for the Two Lowest Income Quintiles

	Minimum Monthly Income (US$)	Maximum Monthly Income (US$)	Mean (US$)	Standard Deviation (US$)
Youth				
Metropolitan Lima				
Lowest Income Quintile	6	47	32	11
Second Lowest Income Quintile	47	93	75	15
Urban Area				
Lowest Income Quintile	1	47	30	13
Second Lowest Income Quintile	47	93	73	14
Household				
Metropolitan Lima				
Lowest Income Quintile	0	124	77	36
Second Lowest Income Quintile	127	211	173	25
Urban Area				
Lowest Income Quintile	0	124	82	31
Second Lowest Income Quintile	124	211	172	51

Source: ENAHO 2004. Elaborated by authors.

Figure 1. Beneficiary Selection Process
PROJOVEN, Lima 1996 to 2003

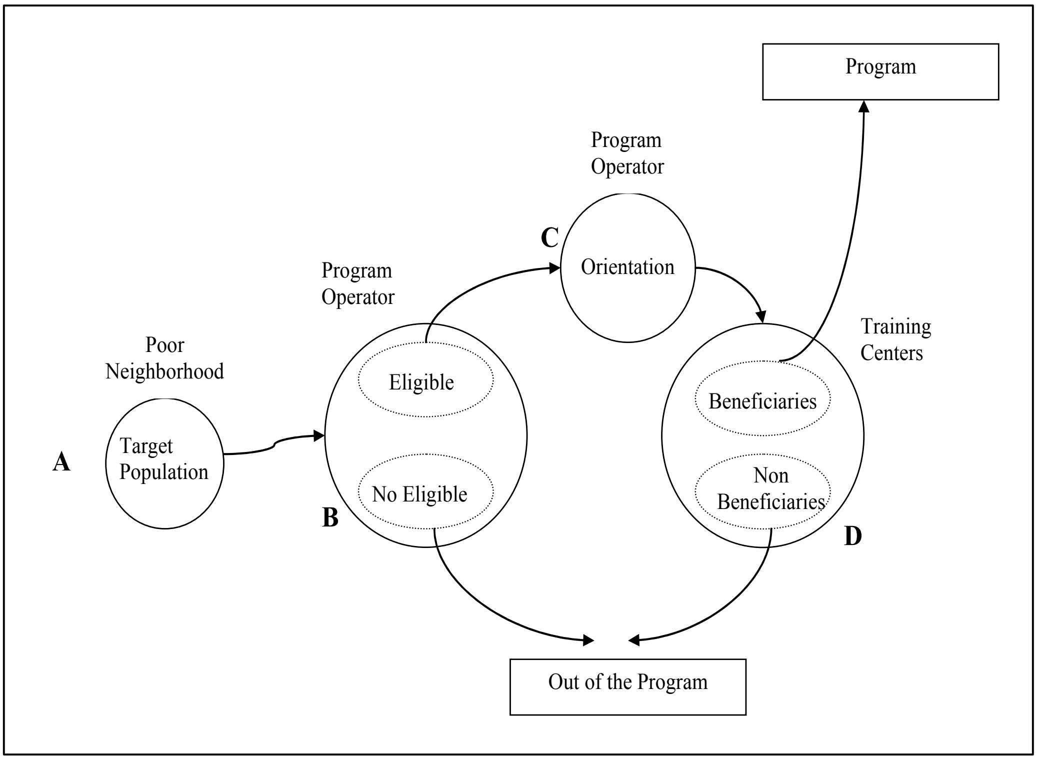
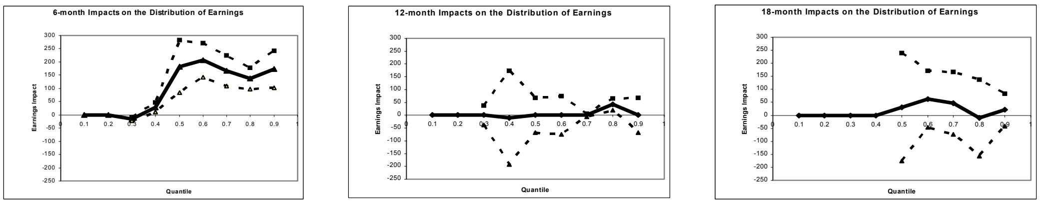
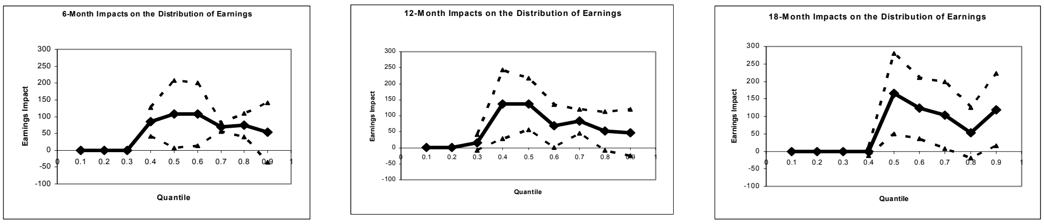


Figure 2: Quantile Treatment Effects
PROJOVEN, Lima 1996-2003

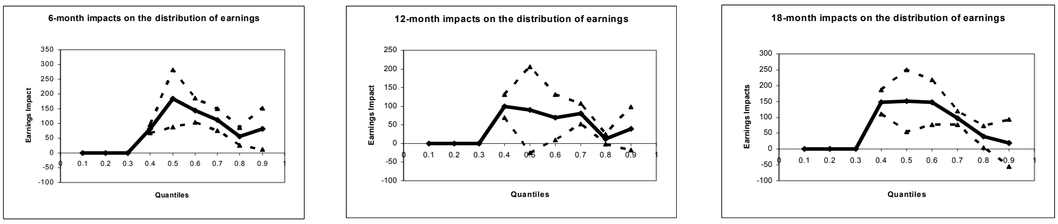
1st public call



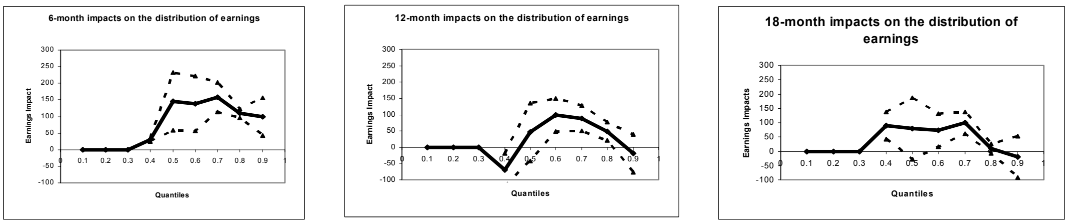
2nd public call



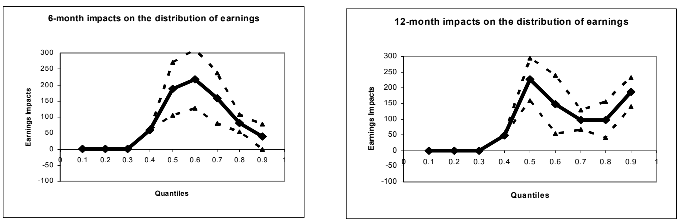
4th public call



6th public call



8th public call



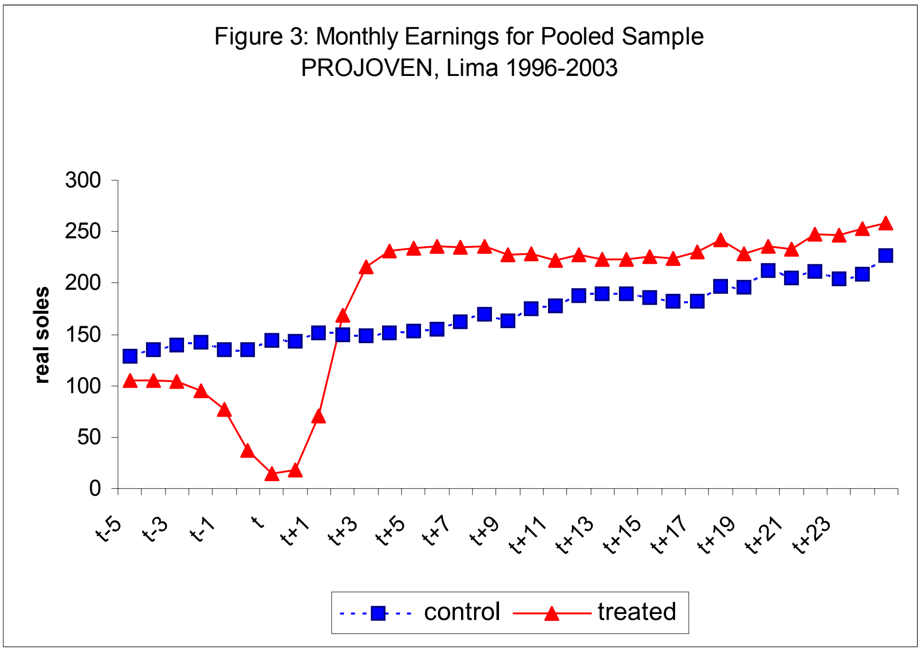


Figure 3: Monthly Earnings for Pooled Sample
PROJOVEN, Lima 1996-2003
real soles
300
250
200
150
100
50
0
t-5
t-3
t-1
t
t+1
t+3
t+5
t+7
t+9
t+11
t+13
t+15
t+17
t+19
t+21
t+23
control
treated

Table 1: Summary Statistics within eligibles
PROJOVEN, Lima 1996-2004

	Pooled data		1st public call		2^{nd} public call		4th public call		6th public call		8th public call	
	parti-cipants	dropouts	parti-cipants	dropouts	Parti-cipants	dropouts	parti-cipants	dropouts	parti-cipants	dropouts	parti-cipants	dropouts
Age and Sex												
Age (average)	20.02	20.27	19.45	19.57	20.32	20.32	21.07	21.22	19.33	19.55	19.98	20.09
Sex(%)	42.88	45.35	44.10	47.90	43.58	48.91	41.35	44.56	42.63	39.42	43.11	47.03
Schooling (%)												
Primary at most	1.25	3.17	0.75	1.96	1.73	2.19	1.03	2.28	1.57	4.90	1.10	4.03
Secundary at most	96.16	94.83	89.19	86.58	97.32	96.98	98.27	97.42	98.39	95.00	95.83	93.85
Tertiary	2.59	2.00	10.06	11.46	0.95	0.83	0.70	0.30	0.04	0.10	3.07	2.12
Schooling household head (%)												
Primary at most	-	-	-	-	-	-	-	-	38.34	36.56	46.18	45.51
Secundary at most	-	-	-	-	-	-	-	-	56.55	58.85	50.87	50.26
Tertiary	-	-	-	-	-	-	-	-	5.11	4.59	2.94	4.23
Labor information (%)												
Employed	25.74	27.54	18.80	19.66	20.87	21.85	21.94	23.81	27.17	29.88	33.59	37.37
Household assets (%)												
Floor												
Floor: high-quality mat.	35.48	36.90	57.15	60.30	4.02	3.18	1.36	1.33	0.75	1.07	0.81	0.40
Floor: medium-quality mat.	-	-	-	-	65.68	65.75	22.22	23.89	22.06	24.73	22.23	22.95
Floor: medium low-quality mat.	-	-	-	-	-	-	0.98	1.10	0.75	0.71	0.61	0.84
Floor: low-quality mat.	64.52	63.10	42.85	39.70	30.30	31.07	75.43	73.68	76.45	73.48	76.34	75.81
Ceiling												
Ceiling: high-quality mat.	34.18	33.12	38.66	38.21	43.25	43.01	34.04	31.70	28.23	29.12	31.59	27.98
Ceiling: medium-quality mat.	-	-	-	-	33.65	34.20	44.40	46.23	41.25	42.48	41.49	43.67
Ceiling: medium low-quality mat.	-	-	-	-	21.76	21.48	19.13	20.21	26.98	24.89	24.17	24.35
Ceiling: low-quality mat.	65.82	66.88	61.34	61.79	1.34	1.30	2.44	1.86	3.54	3.52	2.75	3.99
Toilet												
Toilet: inside the household	79.84	74.87	67.71	70.08	67.97	67.67	22.08	21.05	62.49	61.19	62.23	58.96
Toilet: inside the building	-	-	7.87	6.52	4.80	6.31	7.74	6.11	5.27	6.94	7.80	7.70
Toilet: outside the building	-	-	-	-	21.43	21.06	-	-	27.57	25.80	25.47	27.31
Toilet: no service	20.16	25.13	24.42	23.39	5.80	4.95	70.18	72.85	4.68	6.07	4.50	6.03
N	11,159	10,094	1,601	1,073	1,792	1,918	2,133	2,637	2,543	1,961	3,090	2,505
%	52.51	47.49	59.87	40.13	48.30	51.70	44.72	55.28	56.46	43.54	55.23	44.77

Table 2: Coefficient Estimates from Probit Models for Program Participation within eligibles
PROJOVEN, Lima 1996-2004

	Coefficients									
covariates	1st public call		2nd public call		4th public call		6th public call		8th public call	
	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value
constant	-0.0639	0.831	-0.0188	0.950	-0.0691	0.784	-0.235	0.300	-0.648***	0.004
Age and Sex										
age	-0.0116	0.257	-0.00135	0.878	-0.0167**	0.033	-0.0209***	0.008	-0.0151*	0.055
sex	-0.0869*	0.083	-0.132***	0.002	-0.0712*	0.056	0.0990**	0.011	-0.0964***	0.005
Schooling										
secondary at most	0.658***	0.004	0.155	0.299	0.485***	0.001	0.737***	0.000	0.817***	0.000
tertiary	0.573**	0.016	0.23	0.384	0.998***	0.001	0.118	0.878	1.059***	0.000
Schooling household head										
secondary at most_hh							-0.0588	0.140	-0.00167	0.962
tertiary_hh							0.0181	0.843	-0.231**	0.014
Labor information										
employed	-0.00967	0.879	-0.0029	0.955	-0.0366	0.409	-0.0878**	0.041	-0.0763**	0.033
Household assets										
Floor										
floor: high-quality mat.	-0.100*	0.071	0.211*	0.082	-0.039	0.806	-0.216	0.285	0.453**	0.047
floor: medium-quality mat.			0.0416	0.424	-0.0875**	0.049	-0.0767*	0.091	-0.0442	0.284
floor: medium low-quality mat.					-0.115	0.529	0.089	0.693	-0.206	0.307
Ceiling										
ceiling: high-quality mat.	0.0567	0.307	-0.0263	0.887	-0.112	0.388	-0.0804	0.453	0.260***	0.008
ceiling: medium-quality mat.			-0.0187	0.919	-0.175	0.174	-0.0661	0.530	0.181*	0.061
ceiling: medium low-quality mat.			-0.00397	0.983	-0.188	0.155	-0.0035	0.974	0.239**	0.017
Toilet										
toilet: inside the household	-0.0419	0.501	-0.106	0.266	0.0498	0.273	0.193**	0.025	0.177**	0.025
toilet: inside the building	0.0971	0.361	-0.274**	0.031	0.184**	0.013	0.0355	0.756	0.180*	0.064
toilet outside the building			-0.0791	0.428			0.214**	0.017	0.117	0.150
Observations	2674		3710		4770		4504		5595	

Note: * 10% significance, ** 5% significante, ***1% significance

Table 3: Summary Statistics
PROJOVEN, Lima 1996-2004

	Pooled data		1st public call		2nd public call		4th public call		6th public call		8th public call	
	treated	comparison	treated	comparison	treated	comparison	treated	comparison	treated	comparison	treated	comparison
A. Socio-Demographic												
age	19.64	19.75	19.75	20.24	20.24	20.23	20.19	19.96	19.42	19.66	18.75	18.73
sex (%)	42.94	42.53	43.62	43.29	44.03	44.15	40.7	40.92	42.72	42.46	43.64	42.20
schooling (%)												
incomplete primary	0.87	0.72	1.67	0.68	0.00	0.00	0.60	0.54	1.78	1.64	0.28	0.61
complete primary	4.43	6.20	5.36	7.21	4.63	5.84	4.38	6.77	5.04	7.12	2.89	3.97
incomplete high school	8.80	7.95	7.71	7.9	8.27	7.14	13.16	10.29	9.49	8.49	5.49	5.50
complete high school	85.64	85.00	85.23	84.19	86.09	86.66	81.50	82.11	83.67	82.73	91.32	89.90
marital status (%)												
single	91.26	77.34	91.27	69.41	90.72	76.62	90.90	77.23	89.02	77.53	94.21	85.01
married and/or cohabitating	8.17	22.04	8.38	29.89	8.60	22.40	9.09	22.76	10.38	21.64	4.62	14.37
other	0.56	0.60	0.33	0.68	0.66	0.97	0.00	0.00	0.59	0.82	1.15	0.61
have children (%)	14.16	25.84	15.10	31.95	14.56	30.19	15.05	23.57	15.72	26.84	10.69	17.73
number of children	1.21	1.29	1.37	1.33	1.15	1.3	1.22	1.34	1.22	1.28	1.05	1.13
B. Labor information												
work status (%)												
have a job	51.50	52.11	50.34	51.89	53.97	55.52	48.9	49.32	54.30	54.25	50.00	49.85
unemployed	26.03	26.57	26.51	30.24	26.82	25.97	25.71	25.75	18.40	19.18	32.66	33.03
out of labor force	22.47	21.33	23.15	17.87	19.21	18.51	25.39	24.93	27.30	26.58	17.34	17.13
kind of work (%)												
self-employed	10.42	10.90	17.11	18.90	12.58	10.06	6.26	8.67	10.08	12.38	6.93	5.50
worker in private sector	27.34	32.22	16.44	28.17	28.47	30.51	27.58	28.72	29.97	32.87	32.94	40.67
worker in public sector	0.37	0.48	0.33	1.10	0.66	0.32	0.62	0.00	0.00	0.54	0.28	0.61
unpaid family worker/ housekeeper	18.22	9.81	24.16	4.12	20.19	18.50	17.86	12.73	19.28	9.86	10.69	3.36
monthly earnings	91.43	127.39	73.97	142.00	102.54	126.00	99.84	115.10	89.82	131.83	90.57	123.00
participation in training courses	22.65	23.13	20.13	23.71	19.53	22.72	31.97	24.39	27.59	22.19	14.16	22.62
hours of training	58.02	56.64	60.66	36.60	25.15	40.13	105.00	40.28	81.08	84.90	17.83	76.95
C. Parent's schooling												
father (%)												
complete high school	27.00	31.99	—	—	27.15	26.95	23.20	23.31	27.30	36.44	30.06	41.59
mother (%)												
complete high school	18.02	21.77	—	—	17.88	18.83	15.99	17.34	15.54	23.84	22.54	27.22
N	1602	1660	298	291	302	308	319	369	337	365	346	327

Table 4: Coefficient Estimates from Balanced Logit Models for Program Participation
PROJOVEN, Lima 1996-2004

	Coefficients									
covariates	1st Public Call		2nd Public Call		4th Public Call		6th Public Call		8th Public Call	
A. Socio-demographic	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value	Coeff.	p-value
constant	-3.090	0.081	-3.113	0.005	-4.119	0.000	1.250	0.422	-3.560	0.001
age	0.086	0.108	0.140	0.002	0.101	0.031	0.019	0.701	0.104	0.039
sex	0.104	0.671	-0.177	0.402	0.057	0.770	0.022	0.910	0.139	0.487
schooling										
incomplete primary	2.662	0.008	1.649	0.215	1.541	0.310	0.019	0.980	0.804	0.647
incomplete high school	0.715	0.227	0.258	0.628	1.140	0.345	0.214	0.653	0.504	0.409
complete high school	0.214	0.646	-0.419	0.331	0.606	0.186	-0.282	0.476	0.073	0.880
marital status										
single	0.289	0.831	1.019	0.007	1.050	0.004	-0.055	0.962	1.117	0.011
married and/or cohabitating	-1.349	0.307	0.571	0.587	----	----	-0.528	0.639	2.335	0.023
have children	-0.465	0.404	0.294	0.636	0.493	0.391	0.162	0.801	0.603	0.553
number of children	0.151	0.615	-0.901	0.048	-0.460	0.257	-2.959	0.175	-1.383	0.137
B. Labor information										
have a job	-0.913	0.312	-0.609	0.209	-0.969	0.136	-1.027	0.115	0.130	0.901
unemployed	-0.744	0.018	-0.108	0.704	-0.361	0.156	0.048	0.859	-0.118	0.657
kind of work										
self-employed	1.490	0.105	1.764	0.002	0.589	0.427	2.040	0.004	0.459	0.672
worker in private sector	1.334	0.155	1.438	0.006	1.092	0.117	1.936	0.005	-0.284	0.783
unpaid family worker /housekeeper	2.911	0.001	0.781	0.051	1.257	0.040	1.847	0.002	1.120	0.270
monthly earnings	-0.006	0.000	-0.004	0.000	-0.002	0.097	-0.004	0.000	-0.002	0.033
participation in training courses	-1.217	0.006	0.675	0.144	-0.262	0.382	0.612	0.013	0.722	0.098
hours of training	0.005	0.006	-0.006	0.026	0.004	0.001	-0.001	0.038	-0.007	0.002
C. Household characteristics										
household members	0.098	0.026	-0.084	0.040	0.493	0.391	-0.087	0.022	-0.053	0.158
household members/rooms in house	0.115	0.126	0.016	0.810	0.182	0.001	-0.070	0.365	0.318	0.000
floor : high-quality materials	1.658	0.000	-0.316	0.184	0.466	0.079	-0.080	0.715	-0.133	0.497
ceiling: high-quality materials	1.361	0.000	0.111	0.602	0.724	0.001	0.197	0.339	0.476	0.025
walls: high-quality materials	-1.130	0.000	0.057	0.833	-0.511	0.021	0.254	0.200	0.067	0.729
drinking water piped into house	0.894	0.002	1.625	0.000	1.731	0.000	-2.777	0.000	----	----
flush toilet	-0.403	0.172	-0.415	0.103	-0.770	0.000	-0.487	0.260	0.084	0.644
D. Father's schooling										
no education	----	----	-0.615	0.072	-0.403	0.318	-0.104	0.889	----	----
incomplete primary	----	----	-0.388	0.585	0.129	0.862	0.636	0.345	0.116	0.875
complete primary	----	----	-0.134	0.625	0.286	0.239	0.130	0.580	0.308	0.187
complete high school	----	----	-0.141	0.612	0.054	0.846	-0.320	0.182	-0.231	0.323
higher education	----	----	0.237	0.546	0.327	0.372	0.549	0.184	0.293	0.495
N	589		610		688		702		673	
R^2	0.34		0.16		0.18		0.17		0.14	

Table 5: Wealth Index Estimates
PROJOVEN, Lima 1996-2003

	1st public call			2nd public call			4th public call			6th public call			8th public call		
	mean	weights	weight / std. dev	mean	weights	weights / std. dev	mean	weights	weights / std. dev	mean	weights	weights / std. dev	mean	weights	weights / std. dev
Floor: high-quality materials (concrete)	0.40	0.33	0.68	0.65	0.37	0.77	0.24	0.09	0.20	0.19	0.12	0.31	0.25	0.06	0.14
Floor: low-quality materials (earthen)	—	—	—	0.32	-0.39	-0.83	0.76	-0.09	-0.20	0.79	0.07	0.16	0.73	0.00	-0.01
Ceiling: high-quality materials (concrete)	0.25	0.42	0.96	0.40	0.30	0.62	0.31	0.31	0.67	0.27	0.33	0.73	0.28	0.46	1.02
Ceiling: low-quality materials (matting)	—	—	—	0.22	-0.33	-0.79	0.51	-0.31	-0.62	0.29	-0.33	-0.73	0.52	-0.29	-0.58
Walls: high-quality materials (concrete)	0.61	0.48	0.98	0.75	0.38	0.87	0.68	0.02	0.05	0.51	0.31	0.61	0.51	0.33	0.66
Walls: low-quality materials (matting)	—	—	—	0.10	-0.26	-0.90	0.26	0.35	0.79	0.08	-0.25	-0.92	0.05	-0.18	-0.82
Flush toilet in the house	0.62	0.49	1.00	0.68	0.34	0.74	0.44	-0.28	-0.57	0.65	0.56	1.16	0.58	0.49	0.99
Pit Toilet/ latrine	—	—	—	0.06	-0.19	-0.81	—	—	—	0.33	-0.54	-1.15	0.28	-0.50	-1.11
Drinking water piped into the house	0.59	0.49	0.99	0.69	0.25	0.54	0.42	0.33	0.66	—	—	—	—	—	—
No drinking water	—	—	—	0.10	-0.18	-0.60	0.25	-0.37	-0.85	—	—	—	—	—	—
Household members/rooms in the house	3.18	-0.06	-0.04	2.43	-0.16	-0.09	3.53	0.08	0.05	2.82	0.00	0.00	2.95	0.02	0.01
Own house	—	—	—	0.69	0.10	0.22	0.77	0.39	0.93	—	—	—	—	—	—
Invaded land	—	—	—	0.20	-0.13	-0.32	0.18	-0.42	-1.11	—	—	—	—	—	—
Participating in welfare program	0.42	-0.06	-0.13	—	—	—	—	—	—	—	—	—	0.48	0.06	0.12
No education (father)	—	—	—	0.02	0.00	0.02	0.02	-0.03	-0.20	0.02	0.02	0.16	0.03	-0.06	-0.31
Complete primary schooling (father)	—	—	—	0.17	-0.02	-0.04	0.23	-0.04	-0.09	0.20	0.05	0.12	0.18	0.16	0.42
Incomplete high school or higher (father)	—	—	—	0.19	-0.02	-0.05	0.20	-0.03	-0.08	0.22	-0.06	-0.13	0.22	0.08	0.18
Complete high school (father)	—	—	—	0.27	0.00	-0.01	0.23	0.09	0.21	0.32	0.00	-0.01	0.36	-0.17	-0.35
Higher than high school (father)	—	—	—	0.08	0.03	0.12	0.08	0.04	0.14	0.06	0.02	0.07	0.04	-0.08	-0.39
Wealth Index quartile 1 ("poorest")	-2.09			-2.83			-2.21			-2.10			-1.98		
Wealth Index quartile 2 & 3 ("poor")	0.10			0.45			0.23			0.23			0.06		
Wealth Index quartile 4 ("less poor")	1.90			1.96			1.76			1.70			1.87		

Table 6: Means of Wealth Assets
PROJOVEN, Lima 1996-2003

	1st public call			2nd public call			4th public call			6th public call			8th public call		
	Poorest	Poor	Less poor	Poorest	Poor	Less poor	Poorest	Poor	Less poor	Poorest	Poor	Less poor	Poorest	Poor	Less poor
	q=1	q=2 & 3	q=4	q=1	q=2 & 3	q=4	q=1	q=2 & 3	q=4	q=1	q=2 & 3	q=4	q=1	q=2 & 3	q=4
Floor: high-quality materials (concrete)	0.148	0.344	0.777	0.123	0.744	0.994	0.168	0.266	0.273	0.089	0.167	0.353	0.179	0.313	0.198
Floor: low-quality materials (earthen)	—	—	—	0.871	0.198	0.000	0.832	0.731	0.727	0.000	0.014	0.023	0.006	0.009	0.012
Ceiling: high-quality materials (concrete)	0.000	0.118	0.783	0.026	0.308	0.942	0.046	0.249	0.686	0.006	0.142	0.803	0.000	0.122	0.884
Ceiling: low-quality materials (matting)	—	—	—	0.581	0.386	0.045	0.543	0.581	0.314	0.520	0.312	0.000	0.665	0.696	0.035
Walls: high-quality materials (concrete)	0.074	0.679	0.993	0.265	0.867	1.000	0.237	0.749	1.000	0.218	0.524	0.775	0.231	0.513	0.785
Walls: low-quality materials (matting)	—	—	—	0.303	0.042	0.000	0.618	0.217	0.000	0.162	0.076	0.000	0.139	0.032	0.000
Flush toilet in the house	0.040	0.722	0.972	0.226	0.750	1.000	—	—	—	0.011	0.796	1.000	0.017	0.684	0.919
Pit Toilet/ latrine	—	—	—	0.181	0.026	0.000	0.734	0.445	0.145	0.933	0.184	0.000	0.884	0.116	0.000
Drinking water piped into the house	0.033	0.689	0.972	0.387	0.711	0.942	0.121	0.393	0.773	—	—	—	—	—	—
No drinking water	—	—	—	0.226	0.078	0.000	0.590	0.197	0.000	—	—	—	—	—	—
Household members/rooms in the house	3.385	3.239	2.890	3.184	2.347	1.815	3.318	3.472	3.838	2.849	2.892	2.624	2.965	2.869	3.115
Own house	—	—	—	0.619	0.666	0.812	0.358	0.855	0.994	—	—	—	—	—	—
Invaded land	—	—	—	0.297	0.214	0.078	0.584	0.058	0.000	—	—	—	—	—	—
Participating in welfare program	0.513	0.445	0.283	—	—	—	—	—	—	—	—	—	0.445	0.472	0.512
No education (father)	—	—	—	0.019	0.023	0.013	0.023	0.023	0.012	0.017	0.023	0.029	0.064	0.023	0.023
Complete primary schooling (father)	—	—	—	0.200	0.162	0.169	0.277	0.240	0.180	0.156	0.204	0.231	0.150	0.168	0.244
Incomplete high school or higher (father)	—	—	—	0.219	0.195	0.169	0.220	0.188	0.192	0.263	0.235	0.162	0.168	0.238	0.227
Complete high school (father)	—	—	—	0.271	0.289	0.234	0.156	0.237	0.308	0.346	0.303	0.329	0.486	0.345	0.244
Higher than high school (father)	—	—	—	0.052	0.065	0.117	0.052	0.092	0.087	0.045	0.059	0.069	0.069	0.038	0.017

Table 7: OLS Treatment Impacts for Monthly Earnings
PROJOVEN, Lima 1996-2003

	6-months		12-months		18-months	
	coeff.	std. err.	coeff.	std. err.	coeff.	std. err.
1st Public Call						
Treatment	92**	38	60*	36	80**	39
Treatment*q4	16	55	-44	52	-8	56
Treatment*q2	-40	45	-19	43	-21	46
Ho: β_1=β_2=0	0.249		0.583		0.790	
Baseline Mean	142		142		142	
2nd Public Call						
Treatment	42	39	122**	44	73	50
Treatment*q4	-13	57	-108	65	-50	73
Treatment*q2	34	47	-29	53	-35	59
Ho: β_1=β_2=0	0.348		0.167		0.804	
Baseline Mean	126		126		126	
4th Public Call						
Treatment	49	37	-16	38	14	41
Treatment*q4	-23	51	24	52	32	56
Treatment*q2	-19	44	33	45	35	49
Ho: β_1=β_2=0	0.915		0.825		0.958	
Baseline Mean	115		115		115	
6th Public Call						
Treatment	60*	32	10	32	-8	40
Treatment*q4	-22	45	-38	45	59	57
Treatment*q2	-9	39	38	38	56	49
Ho: β_1=β_2=0	0.738		0.052		0.948	
Baseline Mean	131		131		131	
8th Public Call						
Treatment	66**	27	66*	35	-----	-----
Treatment*q4	5	39	31	50	-----	-----
Treatment*q2	9	33	35	43	-----	-----
Ho: β_1=β_2=0	0.902		0.932		-----	
Baseline Mean	123					
Pooled data						
Treatment	61**	15	49**	16	40*	21
Treatment*q4	-7	21	-22	23	10	29
Treatment*q2	-3	18	12	19	10	25
Ho: β_1=β_2=0	0.825		0.088		0.996	
Baseline Mean	127		127		127	

Table 6: OLS Treatment Impacts for Employment
PROJOVEN, Lima 1996-2003

	6-months		12-months		18-months	
	coeff.	std. err.	coeff.	std. err.	coeff.	std. err.
1st Public Call						
Treatment	0.052	0.082	0.082	0.083	0.1961**	0.084
Treatment*q4	-0.068	0.120	-0.100	0.121	-0.097	0.123
Treatment*q2	-0.048	0.098	-0.124	0.099	-0.138	0.100
Ho: β_1=β_2=0	0.848		0.816		0.704	
Baseline Mean	0.520		0.520		0.520	
2nd Public Call						
Treatment	-0.032	0.090	0.008	0.083	-0.1547*	0.090
Treatment*q4	0.010	0.133	0.002	0.122	0.190	0.132
Treatment*q2	0.141	0.108	0.147	0.100	0.212	0.108
Ho: β_1=β_2=0	0.264		0.177		0.848	
Baseline Mean	0.550		0.550		0.550	
4th Public Call						
Treatment	0.007	0.080	-0.047	0.078	0.018	0.077
Treatment*q4	-0.064	0.109	0.019	0.107	0.096	0.106
Treatment*q2	0.035	0.096	0.081	0.094	-0.010	0.093
Ho: β_1=β_2=0	0.287		0.492		0.243	
Baseline Mean	0.490		0.490		0.490	
6th Public Call						
Treatment	0.010	0.073	-0.095	0.071	0.1679**	0.071
Treatment*q4	0.006	0.104	0.038	0.100	-0.128	0.101
Treatment*q2	0.044	0.089	0.143	0.086	-0.104	0.087
Ho: β_1=β_2=0	0.669		0.226		0.789	
Baseline Mean	0.540		0.540		0.540	
8th Public Call						
Treatment	0.062	0.070	0.042	0.071	-----	-----
Treatment*q4	0.051	0.099	0.086	0.101	-----	-----
Treatment*q2	0.111	0.085	0.082	0.087	-----	-----
Ho: β_1=β_2=0	0.484		0.970		-----	
Baseline Mean	0.500					
Pooled Data						
Treatment	0.028	0.034	0.003	0.034	0.0746*	0.040
Treatment*q4	-0.012	0.049	0.010	0.048	-0.016	0.057
Treatment*q2	0.051	0.042	0.061	0.041	-0.028	0.048
Ho: β_1=β_2=0	0.140		0.222		0.801	
Baseline Mean	0.520		0.520		0.520	

Table 9: OLS Earnings and Employment Effects by Sex, Pooled Data
PROJOVEN, Lima 1996-2003

	Earnings Impacts						Employment Effects					
	6-months		12-months		18-months		6-months		12-months		18-months	
	coeff.	std. err.	coeff.	std. err.	coeff.	std. err.	coeff.	std. err.	coeff.	std. err.	coeff.	std. err.
	Males						Males					
Treatment	45*	25	17	27	42	33	-0.040	0.049	-0.061	0.048	0.012	0.058
Treatment*q4	-21	35	-42	38	-18	45	-0.039	0.069	-0.023	0.067	-0.015	0.080
Treatment*q2	-39	30	1	33	-77*	40	0.000	0.060	0.030	0.058	-0.070	0.070
Ho: $\beta_1=\beta_2=0$	0.5754		0.2043		0.1187		0.519		0.359		0.425	
Baseline Mean	178		178		178		0.620		0.620		0.620	
	Females						Females					
Treatment	72**	18	72**	19	37	27	0.072	0.047	0.044	0.047	0.1182**	0.054
Treatment*q4	4	26	-6	27	29	38	0.013	0.068	0.041	0.067	-0.017	0.078
Treatment*q2	25	22	23	23	75	32	0.0943*	0.057	0.091	0.057	0.006	0.065
Ho: $\beta_1=\beta_2=0$	0.3463		0.2269		0.1745		0.166		0.387		0.728	
Baseline Mean	88		88		88		0.440		0.440		0.440	

Table 10: Matching Treatment Impacts for Monthly Earnings
PROJOVEN, Lima 1996-2003

	1st public call		2nd public call		4th public call		6th public call		8th public call		Pooled Data	
	DID	CS	DID	CS	DID	CS	DID	CS	DID	CS	DID	CS
Panel A: 6 Months after the Program												
Poorest (q=1)	118	104	79	56	84	94	82	83	51	44	87	69
	(42)	(38)	(60)	(60)	(52)	(48)	(34)	(33)	(37)	(37)	(22)	(23)
Poor (q=2&3)	96	62	86	83	72	64	64	58	76	60	78	63
	(37)	(33)	(34)	(33)	(43)	(35)	(29)	(22)	(23)	(28)	(13)	(14)
Less Poor (q=4)	117	107	45	27	81	84	32	33	21	41	61	53
	(73)	(53)	(47)	(50)	(51)	(42)	(28)	(34)	(42)	(40)	(18)	(17)
Baseline Mean	142	142	126	126	115	115	131	131	123	123	127	127
Panel B: 12 Months after the Program												
Poorest (q=1)	24	-11	119	78	20	7	2	-5	32	26	51	28
	(52)	(53)	(52)	(54)	(68)	(57)	(61)	(49)	(44)	(42)	(22)	(23)
Poor (q=2&3)	31	-11	90	79	62	40	42	33	114	98	74	60
	(48)	(44)	(44)	(38)	(43)	(34)	(26)	(31)	(30)	(32)	(18)	(18)
Less Poor (q=4)	3	-76	51	20	83	55	-30	-33	38	58	34	30
	(36)	(45)	(60)	(59)	(58)	(47)	(44)	(42)	(50)	(38)	(21)	(21)
Baseline Mean	142	142	126	126	115	115	131	131	123	123	127	127
Panel C: 18 Months after the Program												
Poorest (q=1)	74	69	78	39	38	39	22	19	—	—	60	40
	(50)	(45)	(52)	(56)	(49)	(41)	(41)	(36)	—	—	(26)	(21)
Poor (q=2&3)	52	40	116	109	67	71	37	27	—	—	67	56
	(54)	(40)	(37)	(36)	(46)	(36)	(32)	(30)	—	—	(20)	(17)
Less Poor (q=4)	34	52	80	60	67	81	23	19	—	—	55	47
	(81)	(68)	(60)	(62)	(53)	(45)	(42)	(36)	—	—	(25)	(27)
Baseline Mean	142	142	126	126	115	115	131	131	123	123	127	127

Table 11: Matching Treatment Impacts for Employment
PROJOVEN, Lima 1996-2003

	1st public call		2nd public call		4th public call		6th public call		8th public call		Pooled Data	
	DID	CS	DID	CS	DID	CS	DID	CS	DID	CS	DID	CS
Panel A: 6 Months after the Program												
Poorest (q=1)	0.031	0.031	0.002	0.0518	0.054	0.1041	0.044	0.0814	0.095	0.0911	0.037	0.089
	(0.136)	(0.099)	(0.131)	(0.107)	(0.125)	(0.102)	(0.125)	(0.120)	(0.069)	(0.091)	(0.057)	(0.044)
Poor (q=2&3)	0.046	-0.068	0.150	0.0931	0.074	0.0458	0.043	0.0673	0.159	0.1616	0.105	0.095
	(0.101)	(0.062)	(0.116)	(0.0693)	(0.095)	(0.078)	(0.090)	(0.065)	(0.084)	(0.071)	(0.048)	(0.037)
Less Poor (q=4)	0.025	0.027	-0.036	-0.0888	0.031	0.0054	-0.037	0.0273	-0.047	0.0194	-0.003	0.006
	(0.196)	(0.098)	(0.124)	(0.073)	(0.116)	(0.112)	(0.095)	(0.088)	(0.104)	(0.089)	(0.053)	(0.050)
Baseline Mean	0.52	0.52	0.55	0.55	0.49	0.49	0.54	0.54	0.50	0.50	0.52	0.52
Panel B: 12 Months after the Program												
Poorest (q=1)	0.018	0.0897	0.055	0.1194	-0.024	0.0085	-0.095	-0.0542	-0.072	-0.0090	-0.016	0.041
	(0.147)	(0.104)	(0.113)	(0.086)	(0.123)	(0.092)	(0.193)	(0.109)	(0.108)	(0.107)	(0.054)	(0.042)
Poor (q=2&3)	0.038	0.0452	0.189	0.1312	0.150	0.0830	0.036	0.0475	0.033	0.0501	0.102	0.096
	(0.117)	(0.096)	(0.128)	(0.092)	(0.115)	(0.075)	(0.079)	(0.055)	(0.111)	(0.079)	(0.052)	(0.038)
Less Poor (q=4)	0.052	0.0575	0.025	-0.0369	0.180	0.0886	-0.091	-0.0314	-0.019	0.0501	0.018	0.034
	(0.291)	(0.191)	(0.109)	(0.094)	(0.191)	(0.130)	(0.133)	(0.089)	(0.097)	(0.079)	(0.054)	(0.046)
Baseline Mean	0.52	0.52	0.55	0.55	0.49	0.49	0.54	0.54	0.50	0.50	0.52	0.52
Panel C: 18 Months after the Program												
Poorest (q=1)	0.180	0.2485	-0.109	-0.0758	-0.038	0.0068	0.182	0.1279	—	—	0.042	0.081
	(0.160)	(0.100)	(0.118)	(0.077)	(0.143)	(0.109)	(0.172)	(0.105)	—	—	(0.075)	(0.053)
Poor (q=2&3)	0.134	0.1381	0.238	0.1574	0.129	0.0885	0.064	0.0523	—	—	0.137	0.109
	(0.135)	(0.102)	(0.109)	(0.063)	(0.114)	(0.088)	(0.096)	(0.065)	—	—	(0.058)	(0.042)
Less Poor (q=4)	0.148	0.1482	0.067	-0.0072	0.239	0.19333	0.036	0.0285	—	—	0.123	0.073
	(0.272)	(0.171)	(0.122)	(0.128)	(0.187)	(0.135)	(0.129)	(0.090)	—	—	(0.071)	(0.059)
Baseline Mean	0.52	0.52	0.55	0.55	0.49	0.49	0.54	0.54	0.50	0.50	0.52	0.52

Table 12: Matching Treatment Impacts for Earnings and Employment by Sex, Pooled Data
PROJOVEN, Lima 1996-2003

	Earnings						Employment					
	6 months		12 months		18 months		6 months		12 months		18 months	
	DID	CS	DID	CS	DID	CS	DID	CS	DID	CS	DID	CS
	Males						Males					
Poorest (q=1)	55	59	-1	5	40	34	-0.066	0.054	-0.117	-0.012	-0.096	0.018
	(37)	(33)	(38)	(34)	(46)	(42)	(0.088)	(0.057)	(0.092)	(0.070)	(0.086)	(0.074)
Poor (q=2&3)	26	29	31	38	2	1	-0.017	0.024	0.011	0.030	-0.009	0.030
	(29)	(26)	(30)	(24)	(36)	(33)	(0.052)	(0.052)	(0.063)	(0.050)	(0.091)	(0.070)
Less Poor (q=4)	15	45	-13	15	17	22	-0.130	-0.016	-0.123	-0.061	-0.073	-0.002
	(37)	(31)	(30)	(27)	(44)	(41)	(0.074)	(0.057)	(0.080)	(0.053)	(0.091)	(0.060)
Baseline Mean	178	178	178	178	178	178	0.62	0.62	0.62	0.62	0.62	0.62
	Females						Females					
Poorest (q=1)	110	80	81	54	74	42	0.124	0.118	0.081	0.076	0.115	0.097
	(19)	(23)	(24)	(23)	(36)	(34)	(0.076)	(0.069)	(0.099)	(0.076)	(0.082)	(0.070)
Poor (q=2&3)	117	95	100	85	112	93	0.207	0.158	0.190	0.141	0.216	0.143
	(15)	(15)	(20)	(17.00)	(18)	(26)	(0.057)	(0.057)	(0.059)	(0.047)	(0.064)	(0.054)
Less Poor (q=4)	91	67	66	52	85	58	0.106	0.038	0.152	0.084	0.237	0.131
	(25)	(21)	(28)	(26)	(44)	(42)	(0.085)	(0.077)	(0.081)	(0.086)	(0.102)	(0.074)
Baseline Mean	88	88	88	88	88	88	0.44	0.44	0.44	0.44	0.44	0.44