Effects of Youth Training in Developing Countries: Evidence from a Randomized Training Program in Colombia*

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Abstract

This paper evaluates the impact of a randomized training program introduced in Colombia in 2005 on the labor market outcomes of trainees. This is one of a couple of such randomized training trials conducted in developing countries and, as such, it offers a unique opportunity to examine the causal impact of training in a developing country context. We use originally collected data on individuals randomly offered and not offered training. We find that the program raises earnings and employment for both men and women, with larger effects on women. The results are robust to controls for training institution fixed effects and for pre-treatment characteristics. Cost-benefit analysis of these results suggests that the program generates a large net gain, especially for women. Lower bound estimates of the internal rates of return are around 13.5% for women and 4.5% for men.

1. Introduction

Lack of skills is thought to be one of the key limitations for growth in developing countries. However, because the accumulation of human capital through formal schooling takes many years, the catching up process for these countries is often slow. In this context, vocational training may provide remedial education and allow individuals beyond school-attendance age to acquire additional skills that make them more productive in the labor market. Moreover, given the lack or little generosity of government transfers in most developing countries, increased earnings in the labor market following training may be the most effective way of helping those at the bottom of the distribution to come out of poverty. However, while there may be good reasons to advocate the use of training programs in developing countries, there is little reliable evidence on the impact of training on improving the labor market standing of the poor. Indeed, mixed results on the impact of government training programs in the US, the UK and other industrialized countries justifies some a priori skepticism as to whether such interventions can deliver positive and cost-effective results, helping poverty alleviation.[1] A unique intervention in Colombia, combined with a randomized experiment, gives us an almost unique opportunity to offer reliable evidence on the value of training in developing countries.

[1] Randomized evaluations have been conducted for two programs targeted to disadvantaged youth in the U.S.: the Job Partnership Training Act (JTPA) and the Job Corps. The initial results on the impact of JPTA by Orr et al. (1994) show positive impacts of training on men and women. Abadie et al. (2002) reconsider the impact of JTPA training programs on the entire distribution of earnings and find that JTPA had the largest impact on women at low quantiles, while for men the program only had effects at the upper end of the earnings distribution. For Job Corps, Lee (2005) finds positive effects of training on the earnings of both men and women. While the effects for women tend to be larger, analysis of these programs which further disaggregates different groups tends to find insignificant results for some groups, e.g., young men (Carneiro and Heckman (2003)). Cost-benefit analyses based even on positive effects also tend to be mixed and depend on the assumptions about the permanence of the gains from training, with some studies finding that these programs are cost effective and others suggesting that these programs may not be a worthwhile investment for society (see the debate in Heckman and Krueger (2003)).

The program "Jóvenes en Acción" (which translates as Youth in Action) provided 3 months of in-classroom training and 3 months of on-the-job training to young people between the ages of 18 and 25 in the two lowest socio-economic strata of the population. Training institutions in the seven largest cities of the country received applications and were each asked to select more individuals than they had capacity for. Subsequently, the program randomly offered training to as many people as there were slots, among the individuals initially chosen by the training institutions. The remaining youths were then used as a control group not selected into training.[2] The advantage of this design is that it attempts to capture the process of trainee selection as it would take place in practice, rather than force the training institutions to train individuals they would otherwise not choose to train. Moreover, subsequent randomization of individuals by training institution allows us to control for potential selection into training institutions.

Comparisons between those offered and not offered training show that both women and men offered training do better in the labor market. The comparisons between individuals offered and not offered training are known as intention-to-treat (ITT) effects. These intention-to-treat effects are directly related to the average effects of training because there is close full compliance in the case of "Jóvenes en Acción." In particular, the probability of receiving training is about 0.96 for those who are offered training. Few individuals who are not initially offered a slot in a course are eventually trained, and even fewer of those individuals who were offered a slot turn down the opportunity to train. For this reason, in this paper we choose to focus on intention-to-treat effects.

[2] In practice, some training centers did not receive enough applicants to select the extra 50% initially requested. For this reason, the probability of being offered training actually varies by training institution.

Intention-to-treat effects show that women offered training are more likely to be employed and work more days and longer hours. In particular, being offered training increases paid employment by about 14% and increases days and hours worked by about 11%. The offer of training also increases women's monthly salaries by about 30,000 Colombian pesos or by US$15, which is an increase of 35% relative to the average salary during the pre-training period. Moreover, the likelihood of being employed in jobs that offer non-wage benefits and of having a written contract is 0.05 higher for women offered training. Men also benefit from being offered training, but the effects for men are more limited. The offer of training increases men's monthly salaries by 22,600 Colombian pesos, which is an increase of 18% relative to the average salary for men before training. Training offers also double the likelihood of getting a formal sector job and a job with a written contract for men. On the other hand, women and men offered training had shorter tenures by about 1 and a half and 3 months, respectively, compared to those who were not trained, possibly because of the automatic withdrawal from the labor force during the three months of classroom training. Nonetheless, this decreased tenure should be considered as a cost of the program.

A number of evaluations of training programs have been conducted on the basis of randomized experiments in the U.S. These studies sometimes show positive but modest effects of training on earnings and employment for some, but not all groups of individuals. Consequently, given the high cost of these type of programs, cost-benefit analyses generally suggest training programs are not worth investing in unless the gains persist over time (e.g., Heckman and Krueger, 2003; Burghardt and Schochet, 2001; Heckman, LaLonde and Smith, 1999).

The picture, however, could be different in developing countries as one may expect the returns to be higher where the levels of skills of the population are very low to begin with. A number of training programs for disadvantaged and low skilled individuals have been introduced in recent years in many countries, including Argentina, Brazil, Chile, Colombia, Dominican Republic, Panama, Peru and Uruguay, and indeed suggest high returns. However, unlike in the Colombian program we study, in the majority of these programs individuals were not randomized into training. As a consequence, these studies have mostly been evaluated using non-experimental techniques. Consistent with the results in this paper, for the most part, the results from these non-experimental analyses show positive effects on the earnings of women. An exception to these non-experimental evaluations in developing countries is the work by Card et al. (2007) and Ibarrarán and Rosas (2007) for the Dominican Republic and Panama which find positive though insignificant effects on earnings and on the probability of getting a job with health insurance, which are attributed to small sample sizes.

The rest of the paper proceeds as follows. Section 2 provides some background on the basic design and implementation of the program Jóvenes en Acción. Section 3 describes the design and implementation of the randomization, as well as the collection of the data. Section 4 provides descriptive statistics and comparisons between the treatment and control groups at baseline. Section 5 presents first-stage results on the impact of a training offer on actual training. Section 5 then presents estimates of the intention-to-treat effects of the program on labor market outcomes, controlling for training institution fixed effects and pre-treatment characteristics. Section 6 shows cost-benefit analyses for women and men and Section 7 concludes.

2. Background and Description of the Program

In 1998 Colombia was hit by the strongest recession in almost 60 years. While the economy had an average GDP growth of 3% for the entire decade of the 1990s, in 1999 Colombia's GDP growth fell to -6.0%. The economy only recovered to its 3% GDP growth again in 2003.

Given the absence of safety nets in the Colombian economy, in 2001 the Colombian government introduced three new social programs to help those hardest hit by the recession,[3] which were financed with a loan from the World Bank and the IADB. The three programs were "Familias en Acción," "Empleo en Acción," and "Jóvenes en Acción." The first was a conditional cash transfer program, similar to the Progresa program in Mexico, which provides stipends for rural families conditional on sending their children to school and providing health checks to the children. The second was a workfare-type program, similar to "Trabajar" in Argentina, which provided temporary government employment to low-income adults. The third, "Jóvenes en Acción," which is the program evaluated in this study, provided training to young people living in urban areas.

The program "Jóvenes en Acción" reached 80,000 young people (or approximately 50% of the target population) and was given to various cohorts over a period of four years. The first cohort received training in 2002 and the last one in 2005. This analysis evaluates this last cohort.

The program was available to young people between the ages of 18 and 25, who were unemployed and who were placed in the two lowest deciles of the income

[3] It is worth noting that unemployment insurance did not exist in Colombia until 2003 when it was introduced by legislation.

distribution. The program spent US$70 million or US$875 per person and was offered in the seven largest cities of the country: Barranquilla, Bogotá, Bucaramanga, Cali, Cartagena, Manizales and Medellin.

Training consisted of 3 months of classroom training and 3 months of on-the-job training. Classroom training was provided by private and public training institutions, which had to participate in a bidding process to be able to participate in the program. The training institutions were selected based on the following criteria: legal registration, economic solvency, quality of teaching, and ability to place trainees after the classroom phase into internships with registered employers. In 2005, there were a total of 118 training institutions offering 441 different types of courses to 989 classes with a total of 26,615 slots for trainees, which means that the average class had 27 students. Training courses provided vocational skills in a diverse number of occupations.[4] The maximum number of hours of lectures was set at 350 hours for three months (or about 6 hours of lectures during weekdays). Of the participating training institutions 43.2% were for profit and 56.8% were non-profit. Training institutions were paid according to market prices and were paid conditional on completion of training by the participants of the program.

On-the-job training was provided by legally registered companies, which provided unpaid internships to the participants. There were a total of 1,009 companies that participated in the program. These companies operated in manufacturing (textiles, food and beverages, pharmaceuticals, and electricity), retail and trade, and services (including security, transportation, restaurants, health, childcare, and recreation).

[4] Courses included training for: taxi and bus drivers; office assistants; call center operators; nurses' and physicians' assistants; pharmacy assistants; hairdressing and cosmetology assistants; inventory assistants; archival assistants; preschool teacher assistants; cashiers; payroll assistants; assistants for computer installation and maintenance; textile operators; woodcutting machine operators; carpentry assistants, plumber assistants, and electricians' assistants.

The program provided a stipend of about US$2.20 per day to male and female trainees without young children throughout the 6 months in the program to cover for transportation and lunch. The amount was increased to about US$3.00 per day for women with children under 7 years of age to help cover for childcare expenses.

3. Design of the Randomization and Data Collection

3.1. Design and Implementation of the Randomization

It is well understood that, in general, individuals self-select into training and that such selection takes place as a function of their overall ability and/or their perceived returns to training. Thus, identifying the impact of training is often difficult. Strategies such as differences-in-differences (Ashenfelter, 1978) or matching may lead to biased estimates of the parameter of interest for reasons that have been extensively discussed (see Heckman, LaLonde and Smith, 1999). In addition, if the impact of training is heterogeneous in the population, there are many different parameters that could be potentially identified and which may be of greater or lesser relevance for policy. The design of the evaluation defines what parameter can be identified and determines the robustness of the results. As we discuss below, in this study, we had the possibility of randomly offering participants spots into a training program and can therefore identify intention-to-treat effects without strong assumptions.

The training institutions are paid conditional on individuals completing training, which gave them an incentive to choose the best candidates and made them reluctant to randomize. To bypass this potential obstacle, we decided to randomize from within a population of individuals pre-selected by the training institutions out of their entire pool of applicants. Thus, each institution was asked to choose up to 50%

more individuals than they had capacity for, though, in practice, not all training institutions had 50% more applicants than capacity. Then, individuals where randomly offered or not offered a position in a course at each training institution. The randomization was carried out using the special information system set up to register applicants into the program. Since the total number of slots per course was fixed but the number of people pre-selected in the training list varied by training institution, the probability of being offered a spot in a course also differed by training institution.[5]

If the individuals who were initially assigned did not accept the training opportunity, then the training institutions were allowed to fill these slots with the next individual in the lists randomly generated by the information system. In addition, individuals who were not initially offered a slot could request to be released from the waiting list in a particular training institution and to apply to other institutions. In practice, there were only 56 individuals in our sample who did this. This means that although the trainees were randomly assigned for the most part, these 56 individuals (i.e., 1.29% of our sample) who initially did not get assigned to treatment but got trained and the 8 (i.e., 0.18% of our sample) who turned down training may be self-selected and introduce a bias. For this reason, our analysis is based on the initial random assignment.

An important advantage of this study is that the availability of training was randomly assigned among those who chose to apply for training and were selected as suitable by the training institutions. Because of the limited non-compliance we reported above, the direct comparison of outcomes between the group that was offered training and the group that was not, will yield unbiased estimates of the average effect of

[5] The median probability of being offered training was 0.815 and the average probability of being offered training was 0.85 with a standard deviation of 0.12.

offering training on those who apply for it and are accepted by one of the training institutions. This "intention-to-treat" parameter has the advantage that it estimates the effect of training on the population of interest, namely those who would be accepted into the program by those delivering it. Moreover, by asking the training institutions to select more candidates than they had places, the experiment comes closer to identifying the effect following an overall expansion of the program to a population which currently does not have full access.[6]

3.2. Data Collection

Given the design of the randomization described above, the data set used for this evaluation, includes a treatment and a control group, in each of the seven cities covered by the program. There were two stages to the data collection: the first stage conducted a baseline survey which collected information on the individuals in the sample before their participation into the program, and a second stage which conducted a follow-up survey and collected information on individuals after the end of the two components of training (i.e., the classroom and on-the-job training).

Table 1 gives information on the size of the samples and reports the planned and actual number of interviews conducted at baseline and in the follow-up surveys by city. The number of interviews in the proposed sample was based on power tests that we did, which would allow capturing effects on employment and earnings similar in magnitude to those generated by similar programs at the 10% level of significance. This yielded a sample of 3,300 with 1,650 in each group. Moreover, given the expected attrition of close to 24% for program participants and 40% for non-program

[6] In Manizales and Cali, however, several training institutions did not get enough applicants to be able to select more individuals than they had capacity for.

participants, we increased the sample to 2,040 and 2,310 for the treatment and control groups, respectively.

In total, the actual number of interviews in the baseline sample were 2,066 the treatment group and 2,287 in the control group, or more than 100% of the expected interviews for the treatment group and 98% for the control group. The sample was stratified by city and sex, so that 50% of the individuals in the treatment and control groups would be male and 50% would be female in each city. Thus, the random selection of treatment and control individuals for the sample was done conditional on having half males and half females from each training institution. This is important because it allows us to do separate analyses of the program for women and men, which as we will show in the next sections show different effects for the two genders.

The collection of information at baseline (before the provision of training) was carried out in January 2005 either before the beginning of the training program or during the first week of classes to minimize any influence of participation in the program on the interviewees' responses. The follow-up interviews were carried out between August and October of 2006 or between 19 and 21 months after the beginning of the program (since the program started at the end of January 2005) with the idea of allowing at least one year since the completion of the program to evaluate its effectiveness in terms of labor market outcomes.

Since there were concerns with attrition, especially for a highly mobile group of young people in the lowest socio-economic strata of the population, telephone updates were conducted on November 2005 or 4 months after the completion of the program. These telephone follow-ups verified the basic personal information of the baseline interviewees and got up-to-date contact information, including addresses and

telephone numbers, for those who had moved or were about to move. Telephone numbers were available for 4,298 of the 4,353 individuals initially interviewed at baseline, so that there were missing phone numbers only for 55 individuals or 2% of those initially interviewed. Of the ones with a phone number, 3,736 or 85.8% were reached. Of these 163 or 4.36% had moved and it was not possible to get new contact information. Out of the 617 who were not reached, 71% had their phone lines cut off or not working. However, personal visits were then conducted to update the information of these 617 individuals.

The complete follow-up in-person interviews were carried out between 9 and 11 months after the telephone update. The follow-up was conducted using the initial list of individuals in the baseline with the contact information updated by telephone in November. Table 1 presents the number of actual interviews carried out during the follow-up. There were 1,749 and 1,814 treatment and control individuals interviewed in the follow-up approximately one year after the training program finished. This is 85% and 79% of the treatment and control groups relative to the samples in the baseline or 81.8% of the total initial sample.[7] This low attrition rate is remarkable, especially considering that the data collection took place in a less developed country and for a sub-population which is highly mobile.[8]

[7] This attrition rate compares very favorably to the attrition rates found in labor market surveys for developed countries (e.g., the attrition rate for the CPS is around 20%).

[8] Moreover, attrition does not seem to be selective in that the process of attrition was completely unrelated to random training offers in the case of women. In the case of men, those not offered training did appear more likely to leave the sample but the effect is small.

4. Data Description and Baseline Comparisons

4.1. Descriptive Statistics

The survey included three parts. The first part of the survey included collected on the information on the demographic characteristics of the treatment and control individuals, as well as of those living in their households. The second part included questions on education, training, health and general labor market information for all individuals older than 12 years of age living in the households of the treatment and control individuals. The third and most important part of the survey included detailed questions to the treatment and control individuals on their labor market experience during the year prior to the survey. In addition, this part of the survey asked questions to those who participated in the program about their training experience.

Table 2 reports basic descriptive statistics on the pre-treatment and post-treatment demographic characteristics and labor market outcomes of women and men in the treatment and control groups. The Appendix includes a detailed description of how the various variables were constructed. The average age of women and men in the sample before training was 21 years of age. About a quarter of the women and 10% of the men in the sample were married before the program. Educational attainment was low for both genders with both women and men having about 10 years of education and, thus, being high school dropouts. Employment during the year before training is low for both groups in terms of participation (i.e., 0.34 for women and 0.4 for men), in terms of days worked (i.e., 11 for women and 14 for men), and in terms of hours worked (23 for women and 30 for men). The probability of having had a formal sector job during the past year, which includes coverage for pensions, health insurance and/or injury compensation is only 0.07 for women and 0.12 for men. The probability of

having had a job with a written contract is equally low. Moreover, earnings and profits are also low for both women and men. Women's monthly earnings are 86,716 Colombian pesos or earnings of $42.14 per month and $1.4 per day, and men's monthly earnings are 124,647 Colombian pesos or $60.57 per month or $2.02 per day. If these individual earnings were the only source of income in their households, then these individuals would be living in poverty or close to extreme poverty.[9] Profits for the self-employed are even lower.

Table 2 shows an improvement in post-treatment labor market outcomes for both women and men, though we should not interpret these improvements as causal since descriptive statistics pool treatment and control individuals. Instead, the causal effect can be obtained by comparing the treatment and control individuals after being exposed to training. However, before comparing the treatment and control individuals after training, in the next section we present comparisons between treatment and control individuals before training to ensure that post-training differences are not simply capturing pre-existing differences between the two groups.

4.2. Baseline Comparisons

If the randomization was successful, the baseline characteristics of those not offered training (i.e., the control group) and those offered (i.e., the treatment group) should not be significantly different. Moreover, given that individuals were randomized at each training institution and that the probability of treatment differed by training institution, a comparison of outcome variables between the two groups following

[9] Moderate poverty is roughly defined as $2.00 per day and extreme poverty as $1.00 per day (World Development Report, 2001).

training within training institution will provide an estimate of the causal effect of the program.

Table 3 reports differences in demographic characteristics and labor market outcomes between women in the treatment and control groups. The first two columns report descriptive statistics on pre-program characteristics and outcomes for the entire pre-program sample and for the post-program sample only, without controlling for training institution fixed effects. The first column shows that treatment and control group characteristics are, for the most part, very similar for women at baseline, i.e., before the program. The only exceptions are education and age, which show significant but small differences between the two groups. The treatment group has a third of a year more education and is a fifth of a year younger than the treatment group. In addition, those in the treatment group appear to have one more month of tenure in their pre-treatment jobs. To check weather selective attrition could be biasing the results, we also check to see if there are pre-program differences between selected and non-selected individuals who remain in our follow-up sample.[10] As for the full sample, women in the treatment and control groups are very similar in terms of their characteristics in this restricted sample, with the exception of small differences in education and age.

Since individuals were randomized at the level of the training institution, we also compare the characteristics between treatment and control women within training institution. Thus, the last two columns in Table 3 report similar differences between treatment and control women for the two samples, but controlling for training institution fixed effects. The differences in education and tenure between treatment and control women disappear once controlling for institution fixed effects. The difference in

[10] As pointed out above, attrition is unrelated to the offer of training in the case of women, so we should not expect biases due to attrition here.

age now shows that treatment women are 1 month and a half older than control women, but this difference is only significant at the 5% level in the full sample and at the 10% level in the follow-up sample.[11] These comparisons, thus, suggest no noticeable pre-existing differences between women in the treatment and control groups.

Table 4 reports similar differences between those in the treatment and control groups to those reported in Table 3 but for men. These comparisons show that baseline characteristics are also fairly well balanced for men. As for women, there are small differences in education and age between men in the treatment and control groups. However, profits are significantly lower for the treatment group.[12] Also, treated individuals are more likely to have paid employment and less likely to be married, but these differences are smaller and only marginally significant. The comparisons in the restricted follow-up sample show similar differences between men in the treated and control groups. However, all of these differences between the treated and control groups disappear when we control for training institution fixed effects in the last two columns of Table 4.[13] On the other hand, differences controlling for institution fixed effects show that treated individuals have a lower probability of having had a written contract and formal job during the past year, but the differences are only marginally significant. As for women, comparisons between men in the treated and control individuals suggest no noticeable pre-existing differences between the two groups. However, since there are some marginal differences for women and men in the pre-treatment sample, our subsequent analysis presents sensitivity analysis controlling for these pre-treatment differences.

[11] Comparisons controlling for training institution fixed effects also show that treated individuals are more likely to be married, but this difference is only marginally significant.
[12] As explained in the Data Appendix, profits are the earnings of the self-employed net of business-related expenditures.
[13] The results are very similar for the full sample and for the follow-up sample, suggesting that attrition is not generating selection biases.

5. Estimating Program Effects

Given random assignment to treatment, the effect of training on various outcomes can be easily estimated by comparing the difference between those offered and those not offered training. Define by Y_{1i} and Y_{0i} the outcomes for individual i in the training state and the non-training state; $D_i = \{0,1\}$ the indicator of whether an individual chooses to participate in the program; $PS_i = \{0,1\}$ the indicator of whether the individual was pre-selected by the training institution or not; $R_i = \{0,1\}$ the indicator of whether the individual was (randomly) offered a place in the program. Finally, $E\{\cdot\}$ represents expectations. The relevant population is the set of individuals who applied for the program and was pre-selected by the training center. Thus, the effect we estimate is the parameter,

$$\delta = E\{Y_{1it} - Y_{0it} | D_i = 1, PS_i = 1\} = E\{Y_{1it} | D_i = 1, PS_i = 1\} - E\{Y_{0it} | D_i = 1, PS_i = 1\},$$

the first term represents the average outcome of trainees who were pre-selected by a training institution. The second term is the average outcome for trainees who had been pre-selected but not been trained. Because of the randomization each component of the above expression is identified by,

$$E\{Y_{1ti} | D_i = 1, PS_i = 1\} = E\{Y_{1it} | D_i = 1, PS_i = 1, R_i = 1\}$$

and

$$E\{Y_{0ti} | D_i = 1, PS_i = 1\} = E\{Y_{0it} | D_i = 1, PS_i = 1, R_i = 0\}.$$

It follows that a simple comparison of means is an unbiased estimate of δ, the intention-to-treat effect (ITT). In addition, since individuals were randomized at each training institution, it makes sense to compare these means within training institutions. This is equivalent to estimating models of the impact of random

assignment on outcomes, which control for training institution fixed effects. These are only required, however, if the proportion of treated and control individuals differ by training institution and if the training institutions differ in their training effectiveness. As discussed above, the probability of treatment varies by training institution with a mean of 0.85 and a standard deviation of 0.12. Moreover, given the diversity of courses offered, the effectiveness of training is also likely to differ by training institution. We, thus estimate models which control for training institution fixed effects as follows:

$$Y_{it} = \delta R_i + \psi_j + u_{it}, \quad (1)$$

where Y_{it} is the outcome variable; R_i is the indicator of whether someone has been randomized into the program or not and ψ_j are training institution fixed effects; u_{it} is a random error term.

Moreover, the precision of the estimates can be increased by controlling for observable pre-treatment characteristics of individuals, X_{it-1}. In addition, including pre-treatment characteristics helps to control for any remaining pre-existing differences between the treatment and control individuals. The model thus becomes,

$$Y_{it} = \delta R_i + \beta X_{it-1} + \psi_j + u_{it}, \quad (2)$$

where X_{it-1} is the vector of explanatory variables, including pre-treatment age, education, a head status dummy, a marital status dummy, and city effects. The parameter δ should be interpreted as the average effect of offering training to those who request it. Given full compliance, this parameter is directly related to the average effect of treatment on the treated. As shown in the next section, there was indeed close to full-compliance with this program.

5.1. Effects of Offers on Training

The description of the data above suggested that few individuals who were randomly offered training turned down training opportunities and few individuals not initially offered a spot in a training institution eventually got trained. To examine how close random training offers relate to actual training, we estimate a linear probability model of training on a random offer indicator, controlling for training institution fixed effects and pre-treatment characteristics, as follows:

$$T_i = \alpha R_i + \rho X_{it-1} + \varphi_j + \upsilon_i \,, \tag{3}$$

where T_i is an indicator of whether the person got trained or not, φ_j are training institution fixed effects and υ_i is a random error term. Table 5 reports the results of this regression for women and men separately. Random assignment to training increases women's probability of being trained by 0.96 and men's probability of training by 0.97. Thus, there is close to full compliance in the sense that most of those initially assigned to training get trained and most of those not initially assigned to the program remain untrained. Table 6 presents a variant of regression (3) where the dependent variable are the average weekly hours of training instead of an indicator of whether the person was trained. The results suggest increases of about 24 and 27 hours of training weekly for women and men who were randomly assigned to training. Like the results in Table 5, these results show a direct link between random assignment and actual training, suggesting that the ITT effects we report below can be interpreted as the impact of training on various outcomes.

5.2. Program Effects for Women

Table 7 presents intention-to-treat effects of the training program on labor market outcomes, which control for training institution fixed effects and for pre-

treatment characteristics. As mentioned above, given that individuals were randomized within training institutions and that the probability of receiving a random offer differed across training institutions, it is important to condition on training institution fixed effects. In addition, conditioning for pre-treatment characteristics allows us to control for potential pre-existing differences between those offered and non-offered training which should not be attributed to the program.

Panel A of Table 7 presents ITT effects for women, which condition on training institution fixed effects and city effects. These results show positive effects of being offered training on education and all labor market outcomes, with the exception of tenure which declines and profits of the self-employed which are not affected by training.

Panel B presents results which control for training institution fixed effects, as before, and also control for pre-training characteristics. The results conditioning on pre-treatment characteristics are somewhat smaller but remain large and significant. For example, the results show that being offered training increases the probability of employment and paid employment by 0.047 and 0.052, as opposed to the 0.058 and 0.066 found without pre-treatment characteristics. Women's days and hours worked also increase after being offered training by close to a day and two and a half hours. Similarly, the probability of holding a formal job and a job with a written contract now increases by 0.054 for those offered training, in contrast to the 0.07 found before. The results for salaries also show smaller but still sizable effects of being offered training of about 35%. On the other hand, the negative effects of being offered training on tenure are slightly larger and suggest a reduction in tenure of about one month and a half, or half of the time of the classroom training. By contrast, the effect of being offered

training on education disappears when conditioning on pre-training educational attainment, suggesting that the higher education of the treatment group was driven by pre-existing differences between the two groups.

The picture is, thus, one of increased labor market activity with substantially higher earnings and increased employment in the formal sector for women. On the basis of these results, this seems like a highly successful program for young women seeking training in the first place.

5.3. Program Effects for Men

Table 8 reports ITT effects for men, which control for training institution fixed effects and for pre-treatment characteristics. The results in Panel A, which condition on training institution fixed effects, show that training offers increase earnings, the probability of getting a formal sector job and of getting a job with a written contract, and also education. On the other hand, these results show a reduction in tenure of about 3 months and a reduction in profits for the self-employed who are offered training, though the latter is only significant at the 10% level.

To check if these results are driven by pre-existing differences, Panel B controls for pre-training age, household headship, marital status and education. Once these pre-training characteristics are controlled for, the impact of training on education disappears but all other results are robust. Results controlling for training institution fixed effects and pre-training characteristics show positive and significant effects of training on earnings and formal employment. Being offered training increases earnings by 22,603 Colombian pesos or by 18% relative to the pre-treatment earnings. Being offered training also increases the probability of formal employment and of having a written contract by 0.052 and 0.07, respectively. On the other hand, even after

controlling for pre-treatment characteristics, the results continue to show a reduction in tenure of about two and half months or close to the time spent by trainees in the classroom. There is also a negative effect on profits, though this effect is only marginally significant.

To summarize: the results imply that training has had positive labor market impacts for both men and women but possibly through different mechanisms. For women there is a clear effect on employment, days, and hours as well as earnings. We cannot know if the increase in average earnings is due to increased employment or also because of enhanced productivity. We also observe an increase working in the formal sector. For men the picture is different; the estimates imply an increase in earnings and the quality of jobs obtained, but not on employment rates.

5.4. Interpretation of Differences and Magnitudes of the Effects

The results above show larger and more widespread effects of training on women than on men. Women not only experience larger gains than men in terms of earnings following training, but they also experience gains in the quantity of employment in terms of participation, days and hours worked not experienced by men. There are three possible reasons for the differential returns to training for women relative to men. First, the higher return could result from the fact that women have lower levels of formal education to begin with. However, contrasting formal schooling at baseline in Table 2 shows small differences in education between women and men. Moreover, even when we control for education at baseline, the differences between women and men remain. Second, women with children received an additional stipend for child care, which may have freed up additional time for women to devote to training. In fact, women tend to enroll in courses requiring longer hours. While only

16% of women were enrolled in courses with less than 1,000 hours of coursework, 20% of men were enrolled in courses involving less than a 1,000 hours of training. Third, qualitative interviews with the training institutions and trainees suggests that women were more motivated and were more responsible during the classroom training and internship phases of the program. In fact, self-reported information on completion of the program show that 3.3% of men dropped out during the first three months of the program while only 2.4% of women did. Moreover, of those who dropped out during the six months, only 0.4% of women were expelled from the classroom or internship phases, while 1.3% of men were expelled.

These differences in the returns to training between women and men are in line with results for developed countries. However, the returns to training are much higher than those found for the U.S. and other developed countries.[14] One puzzle is why there is such a big payoff to such a short-term intervention. One reason may be that this short-term intervention implies a large increase in schooling in a context where schooling levels are very low. In fact, the 6 months of training provided by the program are associated with a substantial increase of 5% in years of schooling in the Colombian context. A second reason why the returns may be large is that training institutions played the role of intermediaries providing information to both sides of the labor market. On the one hand, the training institutions identified the demand for skills and trained individuals in those skills that were needed most. On the other hand, the training institutions identified qualified individuals for future employers helping to overcome information asymmetries.

[14] See Heckman and Krueger (2003) for a discussion of the impact of training in the U.S.

In what follows, we show that these higher returns to training we find here also imply that these programs are cost-effective in a developing country context.

6. Cost-Benefit Analysis

The simplest way of calculating a lower bound to the benefits is to use the gains in earnings. The results imply a gain of about 30,000 for women and of about 22,000 for men, so there are yearly gains of 360,000 and 264,000 for women and men, respectively. The key question of course is whether these gains are permanent or not. We will consider two scenarios: one in which the gains are permanent but do not grow over time, and a second one in which we assume a 10% depreciation rate of these gains. We assume that the working life of these individuals is another 40 years, given that their average age is about 22 in the data. For women, the 30,000 Colombian pesos increase reflects employment and monthly earnings gains as well as the salary gains from moving to the formal sector for those who do so due to the program. In the case of men, the increase of 22,000 Colombian pesos reflects both monthly earnings gains as well as any salary gains from moving to the formal sector.

Discounting at 2% a year, and assuming the growth rate of earnings is not affected, we obtain a gain for women of 10,044,559 Colombian pesos or about US$4,890 and a gain for men of 7,366,010 Colombian pesos or US$3,586 under the first scenario in which the gains are permanent. Under the more conservative scenario in which we allow the gains to depreciate at a rate of 10%, the gains are of $1,480 for women and of $1,085 for men.

The direct cost of operation of the training program was US$875 per person. In addition there is the cost of lost employment for those who would work at the time of training. We have seen that the loss of time in employment as measured by tenure due

to time participating in the program is about 1 and a half months for women and 3 months for men. This is an overestimate of the employment loss because individuals could get many small jobs in one state and one long job in the other. Loss in tenure may not fully reflect unemployment. Table 2 reports an average monthly salary for women before training of 86,716 Colombian pesos or US$42 per month and of 124,647 Colombian pesos or US$60.57 for men. However, women get a transfer of $2.20 daily if they do not have children under 7 and of $3.00 if they have young children. Under either case, the stipend provided by the program more than compensates the monthly salary they would have gotten without the training, so the opportunity cost for women is zero. For men, the daily stipend of $2.20 implies a monthly transfer of $44, so the opportunity cost for men due to the three months of lost employment is $16.57 per month or $49.71 for the entire period. This implies a total cost of training of US$875 for women and of US$924.7 for men.

Under the first scenario of permanent gains, the net gains for women are of almost $4,015 and of about $2,661 for men. Under the more conservative scenario which allows for depreciation of these gains, the net benefits are of $605 for women and of $160 for men. Thus, net benefits from the program are larger for women, but they are clearly positive for both genders even when we allow for workers to lose some of the skills learned during training. If in addition, we considered the non-wage benefits received due to their increased employment in the formal sector the gains would be even higher for both women and men.

Another way of calculating the effectiveness of the program is to calculate the internal rate of return. The internal rate of return is the rate of return that equates the costs with the gains. Under the first scenario the internal rate of return is 25% for

women and 16% for men, while under the second scenario, which allows for depreciation, the rate of return is 13.5% for women and 4.5% for men. These high rates of return, especially for women, suggest that the training program is a great success.

The high returns to training beg the question as to why more people are not getting trained on their own. In the case of "Jóvenes en Acción", there was a shortage of volunteers for some courses once the program was announced, which suggests that lack of information may be preventing people from obtaining training. In addition, a credible hypothesis is that they cannot finance it. Indeed, for men it would take about 15 months pay to cover the entire cost, based on the average pay at the time of the program. Given the lower salaries for women, the cost of training for women corresponds to about 21 months of work. Moreover, the costs would be even higher for women with children who would need to cover for childcare costs during their participation in the program. It is unlikely that anyone would be able to borrow such an amount without collateral at a reasonable interest rate.

7. Conclusion

The program "Jóvenes en Acción" introduced in Colombia in 2005 offers a unique opportunity to evaluate the causal effect of training on young people with little education in the context of a less-developed country. The program offered vocational training for a total period of 6 months (3 months in classroom and 3 months on-the-job) to young unemployed women and men, who belonged to the lowest two strata in the population and who were for the most part high-school dropouts. Most importantly for the purpose of this evaluation, the program randomly offered training to these young women and men.

The results show that the program had substantial effects on both women and men, but in different ways. In particular, training increased earnings, the probability of employment, days and hours worked, the probability of having a formal job, and the probability of having a job with a written contract for women. The effects on men were smaller and also more limited. In the case of men, training only increased earnings, the probability of having a formal job, and the probability of having a job with a written contract. The most important result for both women and men is the increase in earnings, which is 35% for women and 18% for men. In addition, we find an increase in the probabilities of having a formal sector job and a written contract for both women and men of about 0.05 and 0.07, respectively. There is some loss in work experience for both women and men due to the time in the classroom, which is reflected in loss of tenure for the treatment group. In particular, we find a decrease of about one and a half and three months for women and men, respectively. With the exception of effects initially detected on education, the results are robust to controlling for training institution fixed effects and pre-treatment characteristics. This is reassuring, but not surprising, given the randomized design of the evaluation and the fact that treatment and control samples are reasonably balanced at baseline.

These results constitute the basis for a cost-benefit analysis. Even the most conservative of the cost-benefit calculations, which ignores the benefits associated to the higher probability of being employed in the formal sector and which allows the benefits to depreciate over time, suggest that the net benefits of the program more than justify its existence and possibly its expansion. The lower bound internal rate of return is of 13.5% for women and 4.5% for men. Given the high returns to training, the question remains as to why similar type of programs are not more widespread and why

people do not take advantage of existing training opportunities. Lack of information and credit constraints are two likely causes, but this remains an open question.

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Data Appendix

All information used in this analysis was originally collected for the purpose of evaluating the program "Jóvenes en Acción." The data was collected by enumerators which visited the household of individuals in the treatment and control groups on average 3 times. The survey consisted of three parts. The first part collected information on the characteristics of the household, including demographic characteristics of all members of the household as well as household expenditures. The second part of the survey collected information on education, general labor market experience and health outcomes of all household members over the age of 12. Finally, the last part of the survey collected detailed labor market information exclusively on young individuals assigned either to the treatment or control groups. The information in the filled surveys was scanned; read by computers, and subsequently checked for reading errors.

Earnings and Profits: earnings are the monthly salaries and wages earned in the main job held during the year after having finished training for salaried workers. Profits are the monthly earnings net of costs for the self-employed. We impute zero earnings and profits for all of those who reported being either unemployed or out of the labor force. Both earnings and profits are deflated by a city-specific CPI, which comes from the National Department of Statistics (DANE).

Employment and Paid Employment: the employment variable is an indicator variable which takes the value of 1 if the person reports to have had a job during the year after finishing training or 0 if the person reports being unemployed or out of the labor force. Paid employment is slightly different as it also assigns a value of zero to those who report being employed but who report having zero earnings. There are 176 women and 179 men who report having being employed but having earnings of zero.

Weeks and Hours Worked: the survey asks the weeks worked per month and the hours worked per week on the main job held during the year after finishing training. We impute zero weeks and hours worked for all of those who reported being either unemployed or out of the labor force during the year following the completion of the training program.

Formal Employment: formal employment is an indicator variable which takes the value of 1 if the worker was covered by health insurance, injury compensation, pensions or family subsidies, and zero if the worker did not receive any of these benefits in the main job held during the year after having finished training. We impute zeros for all individuals who report being either unemployed or out of the labor force during the entire year after the completion of the program.

Written Contract: written contract is an indicator variable which takes the value of 1 if the person reports having signed a written contract in the most important job during the year following the completion of the program and zero if the person did not sign a contract in the most important job or was unemployed or out of the labor force during the year following the completion of the program. Note that this is different from having a permanent or a temporary contract, but rather refers to having any type of written contract whatsoever.

Tenure: the tenure on the most important job during the year following the completion of the program is constructed by using the exact dates (month and year) when the person reported ending and starting the most important job held during the year after the completion of the program. For those who reported to still be in the same jobs, the end date used was the month and year of the interview so that tenure spells were incomplete. We also imputed zero tenure spells for all individuals who reported being unemployed or out of the labor force during the year following the completion of the training program.

Table 1: Proposed and Actual Sample Sizes for Pre and Post-Treatment Periods by City

	Proposed Sample		Baseline Sample		Follow-up Sample	
	Treatment	Control	Treatment	Control	Treatment	Control
Bogotá	625	741	642	712	528	530
Medellín	378	441	386	442	333	378
Cali	340	393	344	388	292	312
Barranquilla	211	246	211	256	190	207
Bucaramanga	207	212	204	212	161	146
Manizales	99	93	99	93	81	77
Cartagena	180	184	180	184	164	164
Total	2,040	2,310	2,066	2,287	1,749	1,814

Notes: The table reports the proposed sample sizes for the treatment and control groups based on power tests of a significance difference in earnings and employment between the two groups at the 10 percent level. The baseline sample reports the actual sample sizes before training was provided and the follow-up sample reports the actual size of the sample collected after the training program.

Table 2: Basic Descriptive Statistics for Pre- and Post-Treatment Variables – Women and Men

	Women		Men	
	Pre-Treatment 2004	Post-Treatment 2006	Pre-Treatment 2004	Post-Treatment 2006
Employment	0.465	0.670	0.587	0.830
	(0.499)	(0.470)	(0.493)	(0.376)
Paid Employment	0.340	0.578	0.404	0.720
	(0.474)	(0.494)	(0.491)	(0.449)
Contract (zero if out of work)	0.072	0.229	0.106	0.341
	(0.258)	(0.420)	(0.307)	(0.474)
Formal (zero if out of work)	0.070	0.231	0.120	0.382
	(0.256)	(0.421)	(0.325)	(0.486)
Salary (zero if out of work)	86,716	195,090	124,647	285,444
	(141,180)	(199,680)	(173,206)	(218,708)
Profits (zero if missing)	15,089	14,835	32,228	27,710
	(60,127)	(67,792)	(97,744)	(99,662)
Tenure (zero if out of work)	3.229	6.583	4.151	9.380
	(9.226)	(12.235)	(8.668)	(14.967)
Days (zero if out of work)	11.033	15.606	14.133	20.083
	(12.597)	(12.214)	(12.624)	(10.469)
Hours (zero if out of work)	22.776	33.460	29.864	44.183
	(28.084)	(27.979)	(28.954)	(24.940)
Education	9.978	10.247	10.131	10.316
	(1.872)	(1.677)	(1.773)	(1.721)
Age	21.326	22.869	20.940	22.542
	(2.050)	(2.060)	(2.023)	(2.117)
Married	0.269	0.324	0.109	0.181
	(0.444)	(0.468)	(0.311)	(0.385)
Max N	2,321	1,926	2,028	1,632

Notes: The table reports means and standard deviations of the labor market outcomes and demographic characteristics for the pre-training period and the post-training period, combining treatment and control groups.

Table 3: Baseline Differences between Treatment and Control Groups – Women

	Without Training Institution Fixed Effects		With Training Institution Fixed Effects	
	Pre-Training 2004 (Pre-Training Sample)	Pre-Training 2004 (Post-Training Sample)	Pre-Training 2004 (Pre-Training Sample)	Pre-Training 2004 (Post-Training Sample)
Employment	0.011	0.004	-0.004	-0.005
	(0.022)	(0.024)	(0.006)	(0.006)
Paid Employment	0.025	0.029	-0.004	-0.004
	(0.021)	(0.023)	(0.005)	(0.005)
Contract (zero if out of work)	0.005	-0.001	0.002	0.003
	(0.011)	(0.012)	(0.002)	(0.002)
Formal (zero if out of work)	0.014	0.006	0.002	0.003
	(0.012)	(0.012)	(0.002)	(0.002)
Salary (zero if out of work)	4,719	3,664	739	611
	(6,253)	(6,801)	(857)	(951)
Profits (zero if missing)	3,105	1,145	-131	-433
	(2,826)	(2,991)	(501)	(557)
Tenure (zero if out of work)	1.174**	0.896	-0.079	-0.114
	(0.499)	(0.565)	(0.079)	(0.086)
Days (zero if out of work)	0.585	0.315	-0.150	-0.154
	(0.555)	(0.609)	(0.145)	(0.158)
Hours (zero if out of work)	1.008	0.392	-0.394	-0.465
	(1.241)	(1.357)	(0.347)	(0.378)
Education	0.286***	0.302***	-0.036	-0.043
	(0.088)	(0.093)	(0.040)	(0.044)
Age	-0.202**	-0.239***	0.138**	0.121*
	(0.085)	(0.093)	(0.067)	(0.073)
Married	-0.016	-0.025	0.039*	0.038*
	(0.021)	(0.023)	(0.020)	(0.022)
Max N	2,321	1,926	2,327	1,926

Notes: The table reports differences in mean characteristics between the treatment and control groups with and without controls for training institution fixed effects. The pre-training sample refers to the full sample at baseline. The post-training sample includes only those observations which were observed in the follow-up sample and which were not lost due to attrition. * indicates significance at the 10% level, ** indicates significance at the 5% level, and *** indicates significance at the 1% level.

Table 4: Baseline Differences between Treatment and Control Groups – Men

	Without Training Institution Fixed Effects		With Training Institution Fixed Effects	
	Pre-Training 2004 (Pre-Training Sample)	Pre-Training 2004 (Post-Training Sample)	Pre-Training 2004 (Pre-Training Sample)	Pre-Training 2004 (Post-Training Sample)
Employment	0.012	0.015	-0.006	-0.005
	(0.024)	(0.027)	(0.005)	(0.006)
Paid Employment	0.046*	0.068**	-0.004	-0.002
	(0.025)	(0.027)	(0.007)	(0.008)
Contract (zero if out of work)	-0.023	-0.028	-0.013*	-0.013*
	(0.016)	(0.018)	(0.007)	(0.008)
Formal (zero if out of work)	-0.007	-0.025	-0.013*	-0.012
	(0.016)	(0.019)	(0.007)	(0.008)
Salary (zero if out of work)	-1,817	-381	-3,281	-2,763
	(8,759)	(9,647)	(3,250)	(3,673)
Profits (zero if missing)	-14,051**	-17,724***	618	757
	(5,718)	(6,437)	(847)	(934)
Tenure (zero if out of work)	0.807	0.910*	0.123	0.445
	(0.545)	(0.557)	(0.312)	(0.331)
Days (zero if out of work)	-0.188	-0.050	-0.194	-0.184
	(0.624)	(0.695)	(0.120)	(0.136)
Hours (zero if out of work)	-0.433	-0.322	-0.058	-0.097
	(1.421)	(1.582)	(0.151)	(0.172)
Education	0.340***	0.295***	0.012	0.004
	(0.102)	(0.114)	(0.027)	(0.031)
Age	-0.293***	-0.315***	-0.045	-0.028
	(0.100)	(0.111)	(0.037)	(0.042)
Married	-0.033*	-0.035*	-0.000	-0.001
	(0.019)	(0.021)	(0.002)	(0.002)
Max N	2,028	1,626	2,304	1,632

Notes: The table reports differences in mean characteristics between the treatment and control groups with and without controls for training institution fixed effects. The pre-training sample refers to the full sample at baseline. The post-training sample includes only those observations which were observed in the follow-up sample and which were not lost due to attrition. * indicates significance at the 10% level, ** indicates significance at the 5% level, and *** indicates significance at the 1% level.

Table 5: Effect of Training Offer on Probability of Being Trained

	Women	Men
Training Offer	0.961***	0.965***
	(0.012)	(0.007)
City Effects	Yes	Yes
Pre-treatment Characteristics	Yes	Yes
Training Institution effects	Yes	Yes
R^2	0.92	0.94
N	1,917	1,621

Notes: The table reports the effect of being randomly offered training on the probability of having being trained. Robust standard errors are in parenthesis. *** indicates significance at the 1% level or below.

Table 6: Effects of Training on Hours of Training

	Women	Men
Training Offer	23.78***	26.97***
	(1.78)	(1.92)
City Effects	Yes	Yes
Pre-treatment Characteristics	Yes	Yes
R^2	0.15	0.23
N	1.917	1,621

Notes: The table reports the effect of being randomly offered training on the weekly hours of training received. Robust standard errors are in parenthesis. *** indicates significance at the 1% level or below.

Table 7: Intention-to-Treat Estimates of Effects of Training on Labor Market Outcomes – Women

	Employment	Paid Employment	Formal	Contract	Salary	Profits	Tenure	Days	Hours	Education
					A. Training Institution Fixed Effects					
	0.057***	0.065***	0.068***	0.070***	37,022***	3,487	-1.540***	1.510***	3.332***	0.295***
	(0.021)	(0.023)	(0.020)	(0.020)	(9,184)	(3,080)	(0.561)	(0.560)	(1.284)	(0.075)
R^2	0.138	0.120	0.087	0.102	0.128	0.087	0.102	0.115	0.071	0.164
N	1,917	1,917	1,917	1,917	1,917	1,917	1,906	1,917	1,917	1,908
					B. Training Institution Fixed Effects and Pre-treatment Characteristics					
	0.047**	0.052**	0.053***	0.054***	30,401***	3,465	-1.604***	1.149**	2.540**	0.077
	(0.021)	(0.022)	(0.020)	(0.020)	(9,111)	(2,941)	(0.559)	(0.557)	(1.282)	(0.053)
R^2	0.154	0.140	0.109	0.125	0.152	0.090	0.104	0.137	0.131	0.587
N	1,917	1,917	1,917	1,917	1,917	1,917	1,906	1,917	1,917	1,908

Notes: The table reports the coefficient of the training offer dummy for separate regressions of labor market outcomes. Robust standard errors are reported in parenthesis. All regressions control for training institution fixed effects. The regressions in Panel B control for pre-training age, headship status dummy, marital dummy, education, pregnancy, and city effects. * indicates significance at the 10% level, ** indicates significance at the 5% level, and *** indicates significance at the 1% level.

Table 8: Intention-to-Treat Estimates of Effects of Training on Labor Market Outcomes – Men

	Employment	Paid Employment	Formal	Contract	Salary	Profits	Tenure	Days	Hours	Education
					A. Training Institution Fixed Effects					
	-0.004	0.036	0.050**	0.071***	21,757*	-12,086*	-2.876***	0.093	-1.524	0.251***
	(0.020)	(0.023)	(0.025)	(0.024)	(11,463)	(5,231)	(0.748)	(0.546)	(1.308)	(0.085)
R^2	0.104	0.102	0.102	0.111	0.096	0.085	0.091	0.100	0.051	0.181
N	1,621	1,621	1,621	1,621	1,621	1,621	1,615	1,621	1,621	1,613
					B. Training Institution Fixed Effects and Pre-treatment Characteristics					
	0.002	0.035	0.052**	0.070***	22,603**	-9,326*	-2.538***	0.064	-1.064	0.060
	(0.020)	(0.023)	(0.025)	(0.024)	(11,309)	(5,184)	(0.753)	(0.546)	(1.308)	(0.051)
R^2	0.133	0.115	0.145	0.162	0.117	0.105	0.112	0.125	0.118	0.680
N	1,621	1,621	1,621	1,621	1,621	1,621	1,615	1,621	1,621	1,613

Notes: The table reports the coefficient of the training offer dummy for separate regressions of labor market outcomes. Robust standard errors are reported in parenthesis. All regressions control for training institution fixed effects. The regressions in Panel B control for pre-training age, headship status dummy, marital dummy, education, and city effects. * indicates significance at the 10% level, ** indicates significance at the 5% level, and *** indicates significance at the 1% level.