

Subsidizing Vocational Training for Disadvantaged Youth in Colombia: Evidence from a Randomized Trial[†]

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This paper evaluates the impact of a randomized training program for disadvantaged youth introduced in Colombia in 2005. This randomized trial offers a unique opportunity to examine the impact of training in a middle income country. We use originally collected data on individuals randomly offered and not offered training. The program raises earnings and employment for women. Women offered training earn 19.6 percent more and have a 0.068 higher probability of paid employment than those not offered training, mainly in formal-sector jobs. Cost-benefit analysis of these results suggests that the program generates much larger net gains than those found in developed countries. (JEL I28, J13, J24, O15)

Lack of skills is thought to be one of the key determinants of major social problems, such as unemployment, poverty, and crime as well as a key limitation to growth in developing countries. Education programs, mostly targeted at reducing the cost of attending school, have, thus, been at the heart of developing country policies. While early interventions that reduce the cost of education and improve the quality of education at the primary and secondary levels may be key for long-term poverty alleviation (see, e.g., Pedro Carneiro and James J. Heckman 2003), these interventions may arrive too late for those who are already close to the end of their schooling or are in their early post-schooling years.¹

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[†]To comment on this article in the online discussion forum, or to view additional materials, visit the article page at <http://www.aeaweb.org/articles.php?doi=10.1257/app.3.3.188>.

¹Innovative interventions in developing countries include: subsidies to attend private schools (see, e.g., Joshua Angrist et al. 2002; Angrist, Eric Bettinger, and Michael Kremer 2006; Bettinger, Kremer, and Juan E. Saavedra 2008; and Felipe Barrera-Osorio et al. 2011), conditional cash or in-kind transfers to families who send their kids to school (see, e.g., Attanasio et al. 2005; Jere R. Behrman, Piyali Sengupta, and Petra Todd 2005; Paul Glewwe

Training programs are a potential solution to the problem of lack of skills for individuals who have already left the formal schooling system. However, while there are good reasons to advocate the use of training programs for youth, there is little reliable evidence on the impact of training on improving the labor market standing of the poor in developed countries and even less in the context of middle- and low-income countries. Indeed, mixed results of careful evaluations of government training programs in the United States, the United Kingdom, and other industrialized countries justify some a priori skepticism as to whether such interventions can deliver positive and cost-effective results, helping poverty alleviation in middle- and low-income countries.²

The picture, however, could be different in middle- and low-income countries, as one may expect the returns to training to be higher where the levels of skills of the population are low to begin with. Moreover, specialized skills are all the more valuable in low- and middle-income countries, where access to good jobs in the formal sector is often limited to more educated workers. A number of training programs for disadvantaged workers have been introduced in recent years in several Latin American countries in the hope of increasing the level of skills of the poor and helping them gain access to better jobs. Argentina, Brazil, Chile, Colombia, the Dominican Republic, Panama, Peru, and Uruguay have all introduced training programs for disadvantaged youth, and evidence from these countries suggests positive returns.³ However, these programs have largely been evaluated using nonexperimental techniques casting some doubt on the validity of the estimates, which could be biased if there is selection into the program on the basis of unobservables.⁴

An intervention in Colombia, combined with a randomized experiment, gives us an almost unique opportunity to offer reliable evidence on the value of training in middle-income countries. The program *Jóvenes en Acción* (which translates as Youth in Action) was introduced between 2001 and 2005 and provided three months of in-classroom training and three months of on-the-job training to young people between the ages of 18 and 25 in the two lowest socioeconomic strata of the population. Training institutions in the seven largest cities of the country chose the courses to be taught as part of the program and received applications. Each institution was then asked to select more individuals than they had capacity for in each of the classes it offered. Subsequently, the program randomly offered training to as many people as there were slots available in each class from among the individuals

and Pedro Olinto 2004; and Christel Vermeersch and Kremer 2005), and teacher incentives and extra teaching time aimed at increasing quality (see, e.g., Abhijit V. Banerjee et al. 2007; and Kartik Muralidharan and Venkatesh Sundararaman 2009).

²See Robert J. LaLonde (1995) and Heckman, LaLonde, and Jeffrey A. Smith (1999) for surveys of the literature; see Carneiro and Heckman (2003) and Heckman and Alan B. Krueger (2003) for a general discussion of human capital policies; and see Harry Holzer (2007) for a discussion of employment policies for the poor. Also, see LaLonde (1986), David Card and Daniel Sullivan (1988), and John Burghardt and Peter Z. Schochet (2001) for some selected studies of randomized training programs in the United States.

³See Gordon Betcherman, Karina Olivás, and Amit Dar (2004); Victor Elías et al. (2004); and Card et al. (2007).

⁴Most studies evaluating the impact of vocational training in Latin America try to eliminate selection biases by using standard matching methods. The study by Alberto Chong and Jose Galdo (2006) for Peru compares the effects of higher and lower quality training on labor market outcomes using difference-in-differences parametric and ridge matching approaches. A related study by Ofer Malamud and Cristian Pop-Eleches (2010) instead compares the effects of vocational and general education in a transition economy by using a regression discontinuity design.

initially chosen by the training institutions. The remaining youths were then used as a control group not selected into training. 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. This means that the results focus on the population of individuals good enough to be accepted into such a program.

The results we obtain show large program effects, especially for women. Youths, and in particular women, offered the training do better in the labor market than those not offered training. The regression estimates we report are the variance weighted averages of the intention to treat (or offer of treatment) parameter across different courses and training centers. These are probably very close to the average treatment effect for the population that opted into the experiment because the degree of compliance is about 97 percent. 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. Alternative weights for averaging the treatment effects across courses make little difference.

The program has differential impacts on women and men. On the former, we find sizable and significant impacts on the probability of employment and paid employment, on the number of hours worked, and on wages. In particular, the probability of paid employment increases by close to 7 percent, hours per week by almost 3 hours, and wages by close to 20 percent. By contrast, we find that none of these outcomes is significantly affected for men. However, we find that the program has a significant impact on formality for both men and women. Trained male youths are 6 percent more likely to hold a formal contract and 5 percent more likely to have formal employment, while trained women are 8 percent more likely to have a contract and 7 percent more likely to hold formal employment. Male formal wages increase by 23 percent, while the formal wages of women increase by a staggering 33 percent.

The credibility of these results hinges upon the validity of the randomization and the possibility of comparing the treatment and control samples. The availability of baseline data allows us to test whether the two samples are balanced. Our investigation shows that the randomization yielded a substantially balanced sample for women, while there is some indication of slight imbalances for men. In the case of men, we also observe rates of attrition that are slightly different between treatment and control samples. We discuss these issues below and add some words of caution in the interpretation of our results for men.

As we discuss below, given the nature of the experiment, it is not trivial to decompose the observed impacts into productivity effects versus changes in the composition of employed individuals. It is clear, however, that the impacts of the training program we are studying are positive, relatively large, and significant for women. These results stand in strong contrast to most of the results obtained in developed countries and, in particular, in the United States (see, e.g., Heckman and Krueger 2003; Burghardt and Schochet 2001; and Heckman, LaLonde, and Smith 1999). In these countries, the effects are often small, if at all positive, and it is often unclear whether training programs are worth implementing from a cost-benefit perspective. On the other hand, our results are consistent with nonexperimental evaluations of training programs for disadvantaged youth introduced in recent years in a number

of Latin American countries including Argentina, Brazil, Chile, Colombia, the Dominican Republic, Panama, Peru, and Uruguay. Like the results in our paper, for the most part, the results from these nonexperimental analyses show positive effects on earnings, especially for women. The only evaluation of a training program in a developing country based on a randomized trial that we know of is the work by Card et al. (2011) on a program in the Dominican Republic, which also finds positive, though insignificant, effects on earnings and on the probability of getting a job with health insurance of similar magnitudes that we find here. The authors attribute the insignificant effects to their small sample sizes.⁵

The rest of the paper proceeds as follows. Section I provides some background on the basic design and implementation of the program Jóvenes en Acción. Section II describes the experimental design, as well as the collection of the data. Section III provides descriptive statistics and comparisons between the treatment and control groups at baseline. Section IV presents our results. Section V shows cost-benefit analyses, and Section VI concludes.

I. 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 gross domestic product (GDP) growth of 3 percent for the entire decade of the 1990s, in 1999 Colombia's GDP growth fell to -6.0 percent. The economy only recovered to 3 percent GDP growth again in 2003.

Given the absence of safety nets in the Colombian economy and the devastating effect that the recession was having on the poorest segments of the population, in 2001 the Colombian government introduced three new social programs to help those hardest hit by the recession,⁶ which were financed with loans from the World Bank and the Inter-American Development Bank. The three programs were Familias en Acción, Empleo en Acción, and Jóvenes en Acción.

In this paper, we evaluate Jóvenes en Acción, which provided subsidized training to poor young people living in urban areas.⁷ The program, Jóvenes en Acción, reached 80,000 young people (or approximately 50 percent 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 received training in 2005. This analysis evaluates this last cohort, which is the one that was randomly assigned to training.⁸

⁵Jonas Hjort et al. (2009) discuss the work they are currently conducting on a randomized training trial in Kenya.

⁶It is worth noting that unemployment insurance did not exist in Colombia until 2003.

⁷The two other programs targeted different populations. Familias en Acción 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. Empleo en Acción was a workfare type program similar to Trabajar in Argentina, which provided temporary government employment to low income adults.

⁸The World Bank and IADB loans that financed the three programs required the Colombian government to evaluate their impacts. The decision of who was to carry out the evaluation was made by an international panel of policy evaluation experts after an open bidding of proposals. The authors of this paper were part of a consortium, which was selected to carry out the evaluation of Youth in Action. Delays in the contracting of the evaluations implied that preliminary data collection and analysis only started in 2004. This preliminary data allowed us to explain the shortcomings of a nonexperimental evaluation to the Government. After long negotiations with the administrators of the program and the Department of National Planning, they agreed to allow us to randomize individuals into training in

The program was targeted 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 distribution. The program spent US\$60 million or US\$750 per person and was offered in the seven largest cities of the country: Barranquilla, Bogotá, Bucaramanga, Cali, Cartagena, Manizales, and Medellín.

Training consisted of three months of classroom training and three months of on-the-job training. Classroom training was provided by private 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 114 training institutions offering 441 detailed types of courses to 989 classes with a total of 26,615 slots for trainees, which means that the average class had 27 students. The vocational skills provided by the courses were very diverse. Appendix Table A1 provides the distribution of courses further grouped into 70 categories. The greatest number of courses was offered in administrative occupations (such as sales, secretarial work, marketing, warehouse and inventory work, and archival work). However, there were also a large number of courses in manual occupations (such as seamstresses, electricians, and cooking assistants), as well as courses in fairly skilled occupations including (IT specialists, data entry, surveyors, and accountant assistants). Private training institutions played a fundamental role in determining what courses were offered, how they were marketed, and how they were designed. The average number of hours of training per instructor was about 7.56 hours per day.⁹ Of the participating training institutions 43.2 percent were for profit and 56.8 percent were nonprofit. 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). The internships offered an average of 5.19 daily hours of on-the-job training (with a standard deviation of 0.53).

The program provided a cash transfer 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, which was provided conditional on participation in the program. The amount was increased to about US\$3.00 per day for women with children under 7 years of age to help cover childcare expenses.

2005 in a manner that would be acceptable to the training institutions and the participants. The scheme we devised, and which we describe below, was accepted by the government and subsequently implemented.

⁹While we do not have information from trainees on hours of classroom training, we have information from training institutions on the total number of hours an instructor teaches a year. The hours are reported in brackets from 0–1,000, 1,001–5,000, and 5,001 or more. We take the minimum within each category to estimate the average number of hours per instructor. While this is a rough imputation, we do not use this information in our analysis.

II. Experimental Design and Data Collection

A. Experimental Design

As a rule, the earnings of trainees and nontrainees are unlikely to be directly comparable for reasons that have been extensively discussed (see Heckman, LaLonde, and Smith 1999). Random assignment allows us to overcome selection bias in the evaluation of Youth in Action.

The randomization worked as follows. For each class that was oversubscribed, each site or training institution was instructed to select a list of up to 50 percent more applicants than they had capacity for. The population at risk of random assignment were, thus, all applicants, and the risk sets were all classes in each site (i.e., site-by-class).¹⁰ Applicants were randomly assigned to available places on January 18, 2005 using the special information system set up to register applicants into the program. About 10 percent of men and 8 percent of women applicants were assigned at later dates, as we discuss below. We do not use these applicants in our analysis. This does not introduce any bias, as we use the original randomization. Since the total number of slots per class was fixed but the extent of oversubscription differed by site and class, the probability of being offered a spot in a class differed by training institution.¹¹ However, any potential self-selection into sites is eliminated in our analysis because we control for site-by-course effects. If initially assigned individuals did not accept the training opportunity, then training institutions were allowed to fill these slots with the next individual in the class 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 class and could apply to other classes within the same training institutions or in other institutions. In practice, there were only 56 individuals in our sample who did this. This means that although, for the most part, the trainees were randomly assigned, these 56 individuals (i.e., 1.29 percent of our sample), who initially did not get assigned to treatment but got trained, and the 8 (i.e., 0.18 percent of our sample) who turned down training may be self-selected and introduce a bias. Although the low level of noncompliance is unlikely to introduce significant bias, our analysis is based on the initial random offer of training and not on actual training, unlike Card et al. (2011).

Another advantage of this study is that the availability of training was randomly assigned among those who chose to apply for training and who were selected as suitable by the training institutions. Moreover, by asking training institutions to select more candidates than they had places for, the experiment comes closer to identifying the effect following an overall expansion of the program to a population that currently does not have full access.

¹⁰While individuals were randomly assigned to each class at each training institution, our data only have information on each type of course offered by each training institution. Thus, if a training institution offered two classes for seamstresses or three classes for data entry assistants, we cannot compare treatment and control individuals within each class, but rather within each course, i.e., within the two seamstress classes in a site or within the three data entry assistant classes in a site. Thus, in our analysis we will be able to control for site-by-course effects rather than site-by-class effects.

¹¹The median (mean) probability of being offered training was 0.815 (0.85) with a standard deviation of 0.12.

B. Data Collection

Since this was a large-scale experiment, it was not possible to interview the entire population at risk. Instead, random samples were collected from the applicant lists provided by the training institutions, stratified by initial treatment offer, so that roughly half the sample is in the treatment group and half is in the control group. Aside from stratifying by treatment offer, we also stratified by city and sex, with equal numbers of women and men in each city, to allow us to do separate analysis by gender.

We conducted power calculations such that the size of the survey would be able to detect effects similar to those found in other programs, based on a 10 percent level of significance. This yielded a sample of 3,300 with 1,650 in each group. Taking an *ex ante* pessimistic view on attrition,¹² we increased the sample to 2,040 and 2,310 for the treatment and control groups, respectively.

We conducted two surveys. The baseline survey collected information on the individuals in the sample before their participation in the program. The follow-up survey collected information on individuals after the end of the classroom and on-the-job training.

The baseline sample includes 2,066 individuals in the treatment group and 2,287 controls. The baseline data was collected 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 interviewees' responses. Since the baseline interviews for the small number of individuals assigned after January 18 were conducted after the courses were already under way, we do not include the 8.99 percent and 9.5 percent nonrandomly assigned individuals in the baseline and follow-up samples.¹³

The follow-up interviews were carried out between August 2006 and October 2006 or between 13 and 15 months after the conclusion of the program. However, since there were concerns with attrition, especially for a highly mobile group of young people in the lowest socioeconomic strata of the population, we conducted telephone updates four months after the completion of the program in November 2005. 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 percent of those initially interviewed. Of those with a phone number, 85.8 percent were reached. Of these only 4.36 percent had moved, and we were unable to get new contact information. Out of the 617 who were not reached by phone, 71 percent had their phone lines cut off or not working and, thus personal visits were conducted to update the information of these individuals.

¹²The expected attrition used was 24 percent for the program participants and 40 percent for the nonprogram participants.

¹³To check the robustness of our results, we also experimented with the possibility of eliminating training institutions that signed up more than 10 percent of individuals after January 18. The motivation for such a strategy is that these institutions might be trying to get around the experimental design. The results we obtained with such a reduced sample are very similar to those that we report. We also tried leaving out any institutions with any individuals assigned after January and leaving out institutions that had more than 5 percent of individuals assigned after January. The results were similar, but somewhat less precise.

TABLE 1—IMPACT OF TREATMENT ON LIKELIHOOD OF CONTINUING IN THE SAMPLE

	Women		Men	
	(1)	(2)	(3)	(4)
Treatment	0.0169 (0.0175)	0.0157 (0.0175)	0.0713 (0.0204)	0.0723 (0.0207)
Baseline characteristics	No	Yes	No	Yes
Test of joint significance	—	F -test = 1.07 p -value = 0.379	—	F -test = 0.97 p -value = 0.465
Observations	2,133	2,130	1,823	1,818

Notes: The table reports the coefficient of an indicator of whether the person continues in the sample in the follow-up survey on the training offer dummy. Robust standard errors are reported in parentheses. All regressions control for site-by-course fixed effects. The regressions in columns 2 and 4 also control for the following pretreatment characteristics: age, education, marital status, employment, paid employment, salary, self-employment earnings, whether working in the formal sector, whether working with a contract, days worked per month, and hours worked per week.

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 updated contact information. In total, there were 3,549 individuals interviewed in the follow-up survey, which corresponds to 81.5 percent of the total initial sample. 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 percent). More importantly, we need to consider whether treatment and control individuals attrite differentially in the follow-up survey. Table 1 reports results from a regression of the probability of continuing in the sample on an indicator of whether the person was initially assigned to training and site-by-course fixed effects. In addition, we estimate a similar regression, which also includes baseline characteristics. The results for women show no relation between continuing in the sample and an offer of training in either specification. Moreover, baseline characteristics are neither individually nor jointly significantly correlated with the likelihood of continuing in the sample. Selection into the sample thus does not appear to be a problem for women. By contrast, the results for men show that treated individuals are 0.07 more likely to continue in the sample. This could bias the results for men and implies that we need to be careful in the interpretation of these results. It is not clear, however, in which direction the bias will go. There would be positive selection if those that attrite were less motivated. On the other hand, there could be negative selection if those that attrite have better outside options and enumerators cannot find them because these are the individuals who found jobs. At the same time, we find that baseline characteristics are not correlated with the likelihood that men continue in the sample.

III. Data Description and Baseline Comparisons

A. Descriptive Statistics

The baseline and follow-up surveys collected information on demographic characteristics, education, training, health, and general labor market information for all

TABLE 2—BASIC DESCRIPTIVE STATISTICS OF PRETREATMENT AND POST TREATMENT VARIABLES

	Women		Men	
	Pretreatment 2004	Post treatment 2006	Pretreatment 2004	Post treatment 2006
Employment	0.47	0.67	0.58	0.83
Paid employment	0.34	0.58	0.40	0.72
Contract (zero if out of work)	0.07	0.23	0.10	0.34
Formal (zero if out of work)	0.07	0.23	0.12	0.38
Wage and salary earnings (zero if out of work)	84,929 (139,733)	196,411 (200,604)	121,372 (169,862)	285,446 (219,247)
Self-employment earnings (zero if missing)	15,273 (58,967)	14,582 (67,713)	31,395 (96,774)	27,038 (97,755)
Tenure (zero if out of work)	3.26 (9.56)	6.46 (12.12)	4.02 (8.25)	9.31 (14.98)
Days worked per month (zero if out of work)	11.1	15.7 (12.2)	13.8 (12.6)	20.0 (10.5)
Hours worked per week (zero if out of work)	22.9	33.7 (28.1)	29.0 (28.7)	44.1 (25.0)
Education	10.1	10.3 (1.6)	10.20 (1.70)	10.3 (1.68)
Age	21.3 (2.04)	22.9 (2.05)	21.0 (2.04)	22.5 (2.12)
Married	0.27	0.32	0.104	0.18
Max observations	1,769	1,769	1,468	1,468

Notes: The table reports means and standard deviations of the labor market outcomes and demographic characteristics for the pre- and post-training period combining treatment and control groups. The statistics relate to the group that was observed in both baseline and follow up.

individuals older than 12 years of age living in the households of the treatment and control individuals. In addition, the survey included detailed questions on the labor market experience of treated and control individuals during the year prior to the survey.

Table 2 reports basic descriptive statistics on pre-treatment and post-treatment demographic characteristics and labor market outcomes of women and men who were observed in the baseline and follow-up surveys. In Table 2, we do not distinguish between treatment and control samples. The labor market variables include employment status, hours, days, earnings, and the quality of jobs. We distinguish between employment and paid employment. We also distinguish between earnings from wage and salary employment and earnings from self-employment reported in Colombian pesos. As we discuss below, our earnings, tenure, days, and hours measures all include zeros for those not working. Two interesting outcomes that we consider in our analysis are whether the worker is employed in a formal sector job or not and whether she has a contract. These two measures are indicator variables which take the value of one if an individual is employed in the formal sector and has a written contract, and zero if she is not working at all or works in the informal sector or without a written contract. This distinction between the formal and informal sector is important in middle- and low-income countries where being in the formal sector implies access to pensions, health, and other benefits as well as better working conditions. Appendix A includes a detailed description of how the various variables were constructed.

The average age of women and men in the sample before training was around 21 years of age. About 54 percent of the sample is female. Close to one-fifth of the individuals in the sample were married before the program started. Educational attainment among individuals in the sample is low. Average education was about 10 years of education before participation in the program and, thus, on average, the individuals in the sample were high school dropouts. Employment during the year before training is low in terms of participation (i.e., the probabilities of employment and paid employment are close to 0.5 and 0.35), in terms of days worked per month (i.e., almost 12 days/month), and in terms of hours worked per week (i.e., about 25 hours/week). Similarly to youth unemployment and employment in urban labor markets in Colombia during that period, employment rates rise considerably between the baseline and the follow-up surveys.¹⁴ 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 0.08. The probability of having had a job with a written contract is equally low. Moreover, wage and salary earnings and self-employment earnings are also low. Monthly wage and salary earnings are 95,417 Colombian pesos (COP), or US\$40.37/month or US\$1.35/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, as defined by the World Bank. Self-employment earnings are even lower.

To get a sense of how our sample compares to the overall target population, we computed descriptive statistics from the 2005 National Household Surveys (NHS). In particular, we computed descriptive statistics for individuals between 18 and 25 years of age, living in the 7 cities where Jóvenes en Acción was implemented, and living in households in the lowest two deciles of the income distribution in the 2005 NHS. Some of the statistics are remarkably close. For example, the mean age in this sample is 21 years, and the share of women is 55.6. However, individuals in the NHS sample do better in some dimensions and worse in other dimensions compared to those in our Youth in Action sample. Those in the NHS sample have less education (9.2 years), are less likely to be employed (0.24), and are less likely to have a written contract (0.26). On the other hand, those in the NHS sample are more likely to be employed in the formal sector (0.17) and to have longer tenure (5.3 months). These comparisons, offer an ambiguous picture on the relative labor market position and human capital of our target population relative to the one surveyed by the NHS. Moreover, similar patterns hold for men and women, so that we cannot determine whether men or women are unambiguously negatively or positively selected into the program.

B. Baseline Comparisons

If the randomization was successful, the baseline characteristics of those not offered training (i.e., the control group) and those offered training (i.e., the treatment group) should not be significantly different, at least within courses. They could, however, differ between training institutions and courses because these may differ in their quality, and applicants may sort by training institution or courses on the basis

¹⁴United Nations' statistics show a decline in youth unemployment from 25 percent to 22.7 percent during that time period.

of tastes, ability, and other variables. For this reason, we allow for site-by-course fixed effects in all our calculations, although we should note that it does not make much difference for the results we obtain.

Table 3 reports differences in demographic characteristics and labor market outcomes between the treatment and control groups at baseline, separately for women and men. Columns 1 and 3 report the pre-intervention mean in the control sample for women and men, respectively. Columns 2 and 4, instead, report the estimated difference between treatment and control, with its estimated standard error. Overall, we notice that the two samples are remarkably balanced, indicating that the randomization worked quite well. The only exceptions are education for women, where the treatment sample seems slightly better educated, and the fraction of paid employment for men, where treated men seemed more likely to have had paid employment at baseline than men in the control group. However, it is important to point out that when we conduct a test of joint significance of differences of all the baseline characteristics, we cannot reject that the characteristics of women in the treatment and control groups are the same. The F -statistic for women is 1.54. By contrast, the F -statistic for men is 2.61, so that we marginally reject that the baseline characteristics of treated and control men are the same. In particular, the baseline imbalance for men points to a slight positive selection bias.¹⁵

As we mentioned above, there were some indications of irregularities in the assignment protocol in some institutions, which seemed to be more prevalent among men. For instance, the number of candidates assigned to control and treatment groups after the official deadline was significantly larger for men than for women. Although this particular fact does not explain the imbalance of the sample we use (which excludes such individuals), it might be an indication of some problem in the randomization protocol for men.¹⁶ We should stress, however, that the measured baseline imbalances for men are not very large.

To summarize, we conclude that while the randomization proved successful for women, there seem to be some question marks with regard to the experimental integrity for men. Because of these concerns with random assignment of men, as well as the differential attrition of men from our sample, the results for this group should be interpreted with some caution.

IV. Estimating Program Effects

Define by Y_{1i} and Y_{0i} the outcomes for individual i in the training state and the non-training state. $R_i = \{0, 1\}$ is an indicator of whether the individual was (randomly)

¹⁵ Baseline comparisons are similar to those when we consider different samples. For example, we consider a smaller sample that leaves out "problematic" training institutions, where more than 10 percent of the trainees were assigned to treatment after the pre-established deadline.

¹⁶ The administrators who ran the randomization indicated that some training institutions indicated having problems getting enough individuals to sign up for certain classes by January 18, so that they signed up afterward. In particular, administrators indicated that since there were more women than men signing up for the program, this seemed to be more problematic for classes that appealed to men (e.g., electrician training versus beautician training). Also, we were informed that since training institutions were paid on the basis of who completed the program, some training institutions sent updated lists after January 18 to the administrators to get new assignments under the argument that they did not have sufficient candidates in the initial draw.

TABLE 3—BASELINE DIFFERENCES BETWEEN TREATMENT AND CONTROL GROUPS

	Women		Men	
	Control mean	Treatment-control difference	Control mean	Treatment-control difference
Employment	0.460	0.012 (0.025)	0.556	0.038 (0.029)
Paid employment	0.328	0.018 (0.024)	0.358	0.069* (0.029)
Contract (0 if no work)	0.067	0.007 (0.013)	0.103	0.001 (0.018)
Formal (0 if no work)	0.061	0.014 (0.013)	0.125	-0.013 (0.019)
Wage and salary earnings (0 if no work)	84,798	255 (7,189)	119,204	3,992 (10,072)
Self-employment earnings (0 if missing)	13,183	4,066 (3,090)	32,287	-10,853 (5,912)
Tenure (0 if no work)	2.66	1.15 (0.50)	3.45	1.02 (0.55)
Days worked per month (0 if no work)	10.95	0.256 (0.645)	13.40	0.768 (0.745)
Hours worked per week (0 if no work)	22.58	0.536 (1.44)	28.0	1.90 (1.69)
Education	10.0	0.214** (0.076)	10.1	0.16 (0.093)
Age	21.40	-0.13 (0.105)	21.05	-0.11 (0.12)
Married	0.275	-0.019 (0.023)	0.121	-0.032 (0.018)
Test of joint significance	$F(11, 1347) = 1.54$ $p\text{-value} = 0.11$		$F(11, 1062) = 2.61$ $p\text{-value} = 0.003$	
Observations	1,769		1,468	

Notes: The table reports the difference in each variable between the treatment and control groups, controlling for site-course fixed effects. The last row reports the F -statistics and p -value of tests of differences of all of the variables.

** Significant at the 1 percent level.

* Significant at the 5 percent level.

offered a place in the program following preselection by a training institution. Finally, $E\{\cdot\}$ represents expectations.

Given the design of the program, the average outcome for those offered treatment is equal to the average outcome for those randomly offered a place in a course C , i.e., $E\{Y_{1i}|C\} = E\{Y_i|C, R_i = 1\}$, where Y_{1i} represents the outcome in the treated state, and Y_i represents the observed outcome. Similarly, the counterfactual for this population may be estimated using the average outcome for those randomized out of a course, i.e., $E\{Y_{0i}|C\} = E\{Y_i|C, R_i = 0\}$, where Y_{0i} represents the outcome in the control state. By virtue of random assignment, the difference between these two expectations is the program effect on the treated in training course C ,

$$\delta_T = E\{Y_{1i} - Y_{0i}|C\} = E\{Y_i|C, R_i = 1\} - E\{Y_i|C, R_i = 0\}.$$

With full compliance, the above parameter is the average treatment effect among those volunteering for the program, which can be interpreted as the average treatment on the treated in the general population of youth. With less than full compliance the effect is an intention to treat. In our case, the compliance rate is 97 percent.

In our experiment, the randomization took place at the course level across many different courses. The treatment effects we present are weighted averages of the effects across many different courses, based on the within groups estimator, which gives variance-weighted estimates, i.e.,

$$(1) \quad \hat{\delta} = \frac{\sum_C P_C(1 - P_C)(\bar{Y}_{1C} - \bar{Y}_{0C})}{\sum_C P_C(1 - P_C)},$$

where P_C is the proportion allocated to treatment in the sample for a training course C ; \bar{Y}_{1C} is the average outcome (e.g., wages, employment) for those qualified applicants randomly offered training in course C ; and \bar{Y}_{0C} is the average outcome for those qualified applicants randomly denied training in course C . The sum is taken over all training courses, and the parameter is the weighted average of the program effects across training courses. If P_C is the same across all clusters, then this becomes the simple difference of means between all treated and all control individuals, respectively. This simple comparison of weighted means is an unbiased estimator of the program effect. We also explore alternative weights, such as weighing by the probability of assignment to treatment. Since P_C is very similar across courses, alternative weights make little difference.¹⁷

It is also straightforward to control for observable pretreatment characteristics. Including these pretreatment characteristics increases the precision of the estimates and may help control for any remaining baseline imbalances, although these are insignificant for women and otherwise small. Thus, below, we report estimates from the following regressions:

$$(2) \quad Y_{ij} = \alpha R_i + \tau_j + \rho X_i + v_{ij},$$

where Y_{ij} is an outcome for person i in site and course j ; τ_j are site-by-course fixed effects; and v_{ij} is a random error term. X_i are pretreatment characteristics, which are included in an additional set of regressions, and which include age, education, marital status, employment, paid employment, salary, self-employment earnings, whether working in the formal sector, whether working with a contract, days worked per month, and hours worked per week. For the case of binary variables, we estimate a conditional logit, which controls for site-by-course fixed effects (Gary Chamberlain 1980). All results of treatment effects are presented separately for women and men.

¹⁷In practice, P_c is close to 50 percent for most but not all clusters.

TABLE 4A—TREATMENT EFFECTS OF TRAINING ON FEMALE EMPLOYMENT AND EARNINGS

	Labor supply					Earnings	
	Employment	Paid employment	Days/month	Hours/week	Tenure	Wage and salary earnings	Self-employment earnings
Control means	0.637	0.550	14.84	31.82	7.14	177,161	11,970
<i>Panel A. Course fixed effects</i>							
	0.061 (0.024)	0.071 (0.025)	1.46 (0.620)	3.41 (1.41)	-1.53 (0.632)	39,369 (9,955)	2,267 (3,588)
Observations	1,769	1,769	1,769	1,769	1,758	1,769	1,769
<i>Panel B. Course fixed effects and pretreatment characteristics</i>							
	0.054 (0.022)	0.068 (0.023)	1.172 (0.614)	2.866 (1.398)	-1.43 (0.619)	34,668 (9,743)	1,950 (3,568)
Observations	1,367	1,474	1,767	1,767	1,756	1,767	1,767

Notes: The table reports the coefficient of the training offer dummy for separate regressions of labor market outcomes. Robust standard errors are reported in parentheses. All regressions control for course fixed effects. The regressions in panel B, in addition, control for the following pre-training baseline characteristics: age, education, marital status, employment, paid employment, salary, self-employment earnings, whether working in the formal sector, whether working with a contract, days worked per month, and hours worked per week. The effects relating to the binary variables in panel B have been estimated using a conditional logit. The reported numbers are marginal effects evaluated at the average course probability and then averaged over the sample.

A. Employment and Earnings Effects

Table 4A presents treatment effects on employment and earnings for women. Panel A reports effects that take into account site-by-course fixed effects, while panel B, in addition, controls for pretreatment characteristics.

Employment increases significantly by 6.1 percentage points and paid employment increases by 7.1 percentage points. This reflects into a significant increase in days worked per month and hours per week. Part of the cost of training for the individual is reflected in the lost tenure, which is estimated to be about -1.5 and is significant. In other words, the controls did find jobs earlier, though not much earlier given that treated individuals were in training and thus out of the labor force for six months. Salary earnings increase significantly by nearly COP\$40,000, which corresponds to 22 percent control of women's earnings. The change in self-employment earnings, albeit positive, is small (at COP\$2,000) and not statistically different from zero. Panel B shows that all of these effects are slightly smaller when we control for pretreatment characteristics, which is consistent with a successful randomization. The effects on employment and paid employment when controls are added are 5.4 percentage points and 6.8 percentage points, while the effects on hours and tenure are 2.87 and -1.43 . The effect on salaries with controls shows an increase of 19.57 percent.¹⁸

¹⁸The unweighted estimates for women are almost indistinguishable from the ones employing the variance weights in the within groups estimator. The unweighted estimates show increases in employment and paid employment of 5.3 percentage points and 6.2 percentage points, increases in days and hours worked of 1.17 and 2.77, and

At this point, it is useful to consider how we can interpret the comparisons of earnings (which include zeros) between the treatment and control groups. The treatment effect we report is $E(L_i S_i | R_i = 1) - E(L_i S_i | R_i = 0)$, where S_i stands for earnings (salary), and L_i is one for workers and zero for nonworkers. The salary for those out of work should be interpreted as potential salary. This effect can be decomposed as:

$$\begin{aligned} (3) \quad & E(L_i S_i | R_i = 1) - E(L_i S_i | R_i = 0) \\ &= [E(S_i | L_i = 1, R_i = 1) - E(S_i | L_i = 1, R_i = 0)] \times \Pr(L_i = 1 | R_i = 0) \\ &+ [\Pr(L_i = 1 | R_i = 1) - \Pr(L_i = 1 | R_i = 0)] \times E(S_i | L_i = 1, R_i = 1). \end{aligned}$$

This expression shows that earnings increases will occur because of increased employment (the last term, which can be estimated) and/or because of the increased earnings of those employed. However, we cannot conclude that the program had an effect on productivity, even if the first term is nonzero. The overall impact on earnings is given by a combination of productivity effects and changes in the composition of those who are employed. Given that we observe impacts on employment, these composition effects are potentially important. To take a closer look, at this point we need to introduce some notation. Individuals can be split up into four groups: those who would work regardless of the program ($L(1) = 1, L(0) = 1$), i.e., the always-takers; those who would never work ($L(1) = 0, L(0) = 0$); those switching into work due to the program, i.e., the compliers, ($L(1) = 1, L(0) = 0$); and those switching out of work because of the program ($L(1) = 0, L(0) = 1$). Because of the randomization, the size of each of those sets is independent of the assignment to treatment.

We can use this last fact and an additional assumption to obtain some bounds on the productivity effects of the program. In particular, a useful and plausible assumption is that of “monotonicity,” that is that the program may induce individuals to work but does not discourage individuals from work (in other words, individuals who would have worked without the program would also work with the program).¹⁹

an increase in salary earnings close to 18 percent. The estimates weighted by the probability of being a treatment observation in the sample (P_c) show increases in employment and paid employment of 6.2 percentage points and 7.5 percentage points, increases in days and hours worked of 1.24 and 3.33, a decrease in tenure of -1.217 , and an increase in salary earnings close to 20.75 percent.

¹⁹David S. Lee (2009) uses a similar assumption to establish bounds of the impact of a training program on earnings in the United States.

Under this assumption, we can further decompose and simplify the first term in square brackets on the right-hand side of equation (3) as²⁰

$$\begin{aligned}
 (4) \quad & E(S|L = 1, R = 1) - E(S|L = 1, R = 0) \\
 &= [E(S|L(1) = 1, L(0) = 0, R_i = 1) - E(S|L(1) = 1, L(0) = 0, R_i = 0)] \\
 &\quad \times \frac{\Pr(L(1) = 1, L(0) = 0)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \\
 &+ [E(S|L(1) = 1, L(0) = 1, R_i = 1) - E(S|L(1) = 1, L(0) = 1, R_i = 0)] \\
 &\quad \times \frac{\Pr(L(1) = 1, L(0) = 1)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \\
 &+ [E(S|L(1) = 1, L(0) = 0, R_i = 0) - E(S|L(1) = 1, L(0) = 1, R_i = 0)] \\
 &\quad \times \frac{\Pr(L(1) = 1, L(0) = 0)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]}.
 \end{aligned}$$

The first two terms on the right-hand side of equation (4) represent the effect of the program on the earnings of compliers and of those who would work irrespective of the program, respectively. This is the productivity effect of training. The last term of equation (4), instead, captures the change in average earnings induced by the change in the composition of the employed. The first two productivity terms could be zero, and the overall effect could still be positive (or negative) depending on the composition of those moving in and out of work as a result of the program. Thus, although the positive effect we found does mean that the program caused average earnings to rise, the mechanism by which this happens is not revealed by this sort of approach because changes in employment composition cannot be controlled for. Equation (4) can be used to obtain bounds on the productivity effects. We do so by bounding the last term of this equation. In particular, we notice that (as proven in Appendix B)

$$\begin{aligned}
 &\frac{\Pr(L(1) = 1, L(0) = 0)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \\
 &= \frac{\Pr(L = 1|R = 1) - \Pr(L = 1|R = 0)}{\Pr(L = 1|R = 1)},
 \end{aligned}$$

²⁰The details of this derivation are contained in Appendix B.

which can be estimated from the data. We can then bound $[E(S|L(1) = 1, L(0) = 0, R_i = 0) - E(S|L(1) = 1, L(0) = 1, R_i = 0)]$ by considering the distribution of wages in the control group.

Returning to the estimated impacts, using equation (3) and the results for women in Table 4A, we can compute $E(S_i|L_i = 1, R_i = 1) - E(S_i|L_i = 1, R_i = 0)$, the earnings component of the effect, which is the left-hand side of equation (4). This turns out to be COP\$20,654.5²¹ which is substantial but, in light of the discussion above, may not reflect a productivity increase. Under the “monotonicity” assumption, however, we can bound the effect on productivity. Taking as the lower bound of earnings the lower 10 percent of observed positive earnings among non-trainee women and the upper as the top 10 percent, the bounds to the productivity effect under the monotonicity assumption are $\{-11,899.75, 53,208.75\}$ Colombian pesos.²²

The bounds are, unfortunately, quite wide and include zero as a possibility. One can plausibly make some additional assumptions to narrow them, however. If one assumes that the nonprogram earnings of the “always workers” are at least as high as the nonprogram earnings of the 6.8 percent of individuals that were switched from nonwork to work, then the selection term will be nonpositive and the left-hand side of equation (4) will be a lower bound for the effect of the program on productivity. Using, again, the distribution of earnings among the controls to get a bound on the selection term, we would get that the effect of the program on productivity would be bound between COP\$20,654.5 and COP\$53,208.75.

The treatment effects for men are presented in Table 4B. Here, none of the effects are significant at the 5 percent level, except the reduction in tenure of the program participants by about 3 months. This is true whether we condition on pretreatment characteristics or not. Thus, we have no evidence that the program had any employment or earnings effects for males; rather it appears to have cost them in terms of lost earnings. However, given the potential attrition and sample selection biases, we must be cautious with the interpretation of the results for men.

B. Effects on Formal Sector Employment and Earnings

In Latin America, like in other middle-income countries, there is a large share of workers employed in the shadow or informal economy, with no coverage of mandatory benefits. About 45 percent of all workers in Colombia are employed in jobs in the informal sector, in which they do not receive nonwage benefits, such as health insurance, pensions, or injury compensation (see, e.g., Kugler 1999, 2005). Moreover, earnings are lower, on average, in the informal sector. While some of the earnings differences between the two sectors can be attributed to differences in skills and/or self-selection by ability of workers into the formal

²¹ $20,654.5 = [34,668 - 0.068 \times (177,161 + 34,668)/(0.55 + 0.068)]/0.55$.

²² We compute the bounds as $\{20,654.5 - [(E(S(p(0.90))) - E(S(p(0.10)))) \times 0.0646]/(0.06747)\} < \text{Productivity Effect} < \{20,654.5 - [(E(S(p(0.10))) - E(S(p(0.90)))) \times 0.0646]/(0.06747)\}$, where $E(S(p(q)))$ is the mean salary for those in the q th quantile, and where $[\Pr(L = 1 | R = 1) - \Pr(L = 1 | R = 0)] = 0.0646$ and $\Pr(L = 1 | R = 1) = 0.6747$. For a discussion of bounds with selection effects see Charles F. Manski (1994) and Richard Blundell et al. (2007).

TABLE 4B—TREATMENT EFFECTS OF TRAINING ON MALE EMPLOYMENT AND EARNINGS

	Labor supply					Earnings	
	Employment	Paid employment	Days/month	Hours/week	Tenure	Wage and salary earnings	Self-employment earnings
Control means	0.833	0.689	20.06	45.15	11.05	265,292	35,435
<i>Panel A. Course fixed effects</i>							
	-0.025 (0.022)	0.024 (0.027)	-0.56 (0.62)	-2.37 (1.48)	-3.18 (0.91)	15,466 (12,819)	-11,610 (5,959)
Observations	1,468	1,468	1,468	1,468	1,464	1,468	1,468
<i>Panel B. Course fixed effects and pre-treatment characteristics</i>							
	-0.027 (0.030)	0.013 (0.029)	-0.545 (0.618)	-2.27 (1.471)	-2.772 (0.900)	13,690 (12,819)	-6,731 (5,839)
Observations	791	1,037	1,464	1,464	1,460	1,464	1,464

Notes: The table reports the coefficient of the training offer dummy for separate regressions of labor market outcomes. Robust standard errors are reported in parentheses. All regressions control for course fixed effects. The regressions in panel B, in addition, control for the following pre-training baseline characteristics: age, education, marital status, employment, paid employment, salary, self-employment earnings, whether working in the formal sector, whether working with a contract, days worked per month, and hours worked per week. The effects relating to the binary variables in panel B have been estimated using a conditional logit. The reported numbers are marginal effects evaluated at the average course probability and then averaged over the sample.

and informal sectors, wage differences remain between the two sectors even after controlling for observed and unobserved characteristics of workers, and these are often attributed to the willingness of firms in the formal sector to pay above market-clearing wages. Generally, formal sector jobs are thought to be better for workers. However, probably because of regulations, such as the minimum wage, access to these jobs is often limited to those with more skills, so that increased education and training are often seen as ways to gain entry into these jobs. It is, thus, important to ask whether the training intervention introduced by Youth in Action improved access to better paying jobs in the formal sector.

Tables 5A and 5B show treatment effects on the probability of formal employment, defined as employment covered by health, pension, and injury compensation benefits, as well as on the probability of having a written contract. Not being employed in the formal sector includes the unemployed and those in the informal sector.

For both men and women, there is a significant impact of the program on working in the formal sector (as opposed to either not working at all or working in the informal sector). Thus, for women much of the gain in employment was into formal jobs. Men seem to have shifted from informal employment to formal employment, but as explained above, we prefer to interpret the effects on men cautiously because of potential biases due to attrition and because of the initial imbalance. It is possible that some of the trainees were kept on by the firms in which they undertook their on-the-job training. For both men and women this shift has also been reflected in significantly higher formal earnings, although only for women has this meant higher

TABLE 5A—TREATMENT EFFECTS OF TRAINING ON FEMALE FORMAL EMPLOYMENT AND EARNINGS

	Contract	Formal employment	Formal salary	Informal salary
Control means	0.191	0.197	79,623	97,537
<i>Panel A. Course fixed effects</i>				
	0.079 (0.021)	0.071 (0.022)	32,194 (9,352)	7,174 (8,337)
Observations	1,769	1,769	1,769	1,769
<i>Panel B. Course fixed effects and pretraining characteristics</i>				
	0.078 (0.019)	0.069 (0.020)	26,604 (9,150)	8,064 (8,280)
Observations	1,173	1,202	1,767	1,767

Notes: The table reports the coefficient of the training offer dummy for separate regressions of labor market outcomes. Robust standard errors are reported in parentheses. All regressions control for course fixed effects. The regressions in panel B, in addition, control for the following pre-training baseline characteristics: age, education, marital status, employment, paid employment, salary, self-employment earnings, whether working in the formal sector, whether working with a contract, days worked per month, and hours worked per week. The effects relating to the binary variables in panel B have been estimated using a conditional logit. The reported numbers are marginal effects evaluated at the average course probability and then averaged over the sample.

average earnings overall.²³ In addition, formal sector workers receive nonwage benefits, which are paid through payroll taxes. This is an additional gain from training as long as nonwage benefits are not fully shifted to workers as lower wages. However, as noted in Kugler and Maurice Kugler (2009), only about 20 percent of payroll taxes are passed on to workers as lower wages in the Colombian context, so that a large part of the nonwage benefits are accrued by the workers.

C. Discussion and Interpretation of the Results

To summarize, the program seems to have strong impacts for women.²⁴ In particular, for women, we find large effects on employment, earnings, and formality. The effects on males are confined to an impact on formality and have to be interpreted with caution.

These are important results that stand in contrast to results obtained in evaluations of training programs in developed countries. However, as it is perhaps unavoidable, our exercise is not exempt from the need for some qualifications. First, as pointed out above, while attrition turns out to be low and balanced for women, men from the

²³ An alternative explanation for the higher formal sector earnings received by young treated workers is that these workers are simply earning temporarily higher earnings because they have steeper age-earnings profiles in the formal sector that eventually flatten. However, when we run a regression of earnings on age and age squared, and the interaction of age and its quadratic term with a formal sector dummy, we find that the interaction terms are not individually or jointly significant.

²⁴ Our results are consistent with other results which show that women in Colombia do better than men in terms of educational outcomes (e.g., Angrist et al. 2002).

TABLE 5B—TREATMENT EFFECTS OF TRAINING ON MALE FORMAL EMPLOYMENT AND EARNINGS

	Contract	Formal employment	Formal salary	Informal salary
Control means	0.286	0.341	144,977	120,314
<i>Panel A. Course fixed effects</i>				
	0.061 (0.028)	0.053 (0.030)	30,777 (13,569)	-15,310 (11,315)
Observations	1,468	1,468	1,468	1,468
<i>Panel B. Course fixed effects and pretraining characteristics</i>				
	0.070 (0.025)	0.058 (0.026)	33,156 (13,583)	-19,465 (11,446)
Observations	1,115	1,149	1,464	1,464

Notes: The table reports the coefficient of the training offer dummy for separate regressions of labor market outcomes. Robust standard errors are reported in parentheses. All regressions control for course fixed effects. The regressions in panel B, in addition, control for the following pre-training baseline characteristics: age, education, marital status, employment, paid employment, salary, self-employment earnings, whether working in the formal sector, whether working with a contract, days worked per month, and hours worked per week. The effects relating to the binary variables in panel B have been estimated using a conditional logit. The reported numbers are marginal effects evaluated at the average course probability and then averaged over the sample.

control group are more likely to leave the sample. Moreover, for men, the hypothesis of baseline equality between treatment and control characteristics is rejected, albeit with very small differences across the samples. Thus, it is conceivable that the finding of no effect for men could be the result of bias. For women, on the other hand, attrition was balanced across treatment and control groups, and we cannot reject that the baseline characteristics are the same. Moreover, for women, the impacts are large and significant.

In addition, there is the issue of decomposing the observed effect on earnings into employment and productivity effects (which we discussed above) and between employment and formal employment.

We have seen that, for women, we find sizable employment effects, both for formal jobs and for all jobs, while for men the impact is visible only for formal employment. A pessimistic view of these results is that they are induced by “queue jumping” rather than new job creation. That is, the trainees might be replacing other individuals that would have been hired in any case by the firm. Unfortunately, formally, there is not much we can say about this possibility. The experiment was not designed to address this issue. Different designs might have allowed a more definitive answer.

However, our design and its implementation in the field can provide some hints to the fact that queue jumping might not be a big concern. The instructions for the training institutions were to present 45 eligible individuals, of which 30 would be randomly offered training. In practice, though, there was variation in the size of the original lists. As a consequence, the probability of receiving treatment varies across training institutions. One can think that training institutions for which the list of applicants were longer, operated in local environments characterized by a

TABLE 6—DIFFERENTIAL IMPACT OF TREATMENT IN COURSES WITH LOW DEMAND

	Employment	Paid employment	Formal employment	Contract
<i>Panel A. Women</i>				
Treated	0.057 (0.036)	0.063 (0.040)	0.091 (0.033)	0.105 (0.030)
Treated × high probability of treatment	-0.006 (0.061)	0.009 (0.063)	-0.053 (0.0820)	-0.074 (0.090)
Observations	1,367	1,474	1,202	1,173
<i>Panel B. Men</i>				
Treated	0.013 (0.036)	0.063 (0.034)	0.040 (0.045)	0.025 (0.050)
Treated × high probability of treatment	-0.083 (0.081)	-0.140 (0.103)	0.033 (0.059)	0.076 (0.050)
Observations	791	1,037	1,149	1,115

Notes: The table reports marginal effects from a conditional logit of the probability of being selected and the interaction of being selected with a dummy for high probability of treatment in a course, where the high probability dummy is defined as those in courses with a probability of treatment in the course above the mean probability of treatment in the sample. The reported marginal effects evaluated at the average course probability and then averaged over the sample. Robust standard errors are reported in parentheses. All regressions control for course fixed effects. In addition, the regressions control for the following pre-training baseline characteristics: age, education, marital status, employment, paid employment, salary, self-employment earnings, whether working in the formal sector, whether working with a contract, days worked per month, and hours worked per week.

tighter labor market, that is, in a situation where there was more of an opportunity for queue jumping. Therefore, one can think of the probability of being treated (which varied across training institutions) as being inversely related to the opportunity for queue jumping (although this variable is not necessarily exogenous). We can then interact this probability with the treatment indicator to check whether the size of the estimated effects vary inversely with the probability of being treated. If the impact declines with the probability of being treated, this could be an indication that the employment impacts reflect, at least in part, queue jumping. We implement this idea by dividing training institutions into those with the probability of treatment above the median and those with the probability of treatment below the median. We then interact above-the-median and treatment indicators. Panels A and B of Table 6 report the results of regressions that include the interactions with the above/below the median indicator for women and men, respectively. Neither the results for women in panel A nor those for men in panel B show evidence that the effects of the program on overall and formal employment are bigger in environments with tighter labor markets. We find that there are no significantly different impacts in the two types of labor markets. If anything, the comparisons of regression and ITT estimates in footnote 18 suggest that program effects may be greater in environments with loose labor markets. We therefore conclude that queue jumping is not likely to be a problem.

Given the positive impacts we encounter, it is useful to relate our results to others in the literature. The only other similar intervention with a randomized trial in a developing country is the program in the Dominican Republic analyzed by Card et al. (2011), so it is of interest to provide a brief comparison of this program with

Jóvenes en Acción. The programs are similar; the only difference is that in Colombia the internship lasted three months instead of two months, as it did in the Dominican Republic. Card et al. (2011) find no employment effect, but they do find an earnings effect of 10–17 percent (depending on the method used). The effect they find is not precisely estimated, however. They also find positive but insignificant effects on formality. The differences in our results could be explained by our larger sample size and/or differences in the programs and the contexts.

Our results are consistent with the findings in Bettinger et al. (2007) who show that vouchers to attend vocational schools, which like the direct subsidy here reduce the costs of training, led to increased labor market participation and hours worked.

V. Cost-Benefit Analysis

The simplest way of calculating a lower bound to the benefits of the program is to use the gains in wage and salary earnings. The results in panel B of Table 4A imply a gain for women of about COP\$416,000 per year, which reflect employment and monthly earnings gains, as well as salary earnings gains from moving to the formal sector. 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 percent annual 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. Discounting at 5 percent a year, and assuming the growth rate of earnings is not affected, we obtain a gain of US\$3,805 for women 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 percent annually, the gains are of US\$1,478.²⁵

The direct cost of operation of the training program, including a stipend for the trainees was US\$750 per person. The program caused a loss of tenure of 1.43 months, which, when evaluated at baseline, is another US\$62, which we add just to be conservative, in case the stipend underestimates the opportunity cost of training. Thus, under the first scenario of permanent effects, the net lifecycle gains for women are \$2,993. Under the more conservative scenario, which allows for depreciation of these gains, there is a net benefit of US\$666. The corresponding internal rates of return are 35 percent and 21.6 percent, respectively, which show, in a different way, how effective the program has been for women.

These gains do not factor in the nonwage benefits obtained from working in the formal sector. On the other hand, they assume that women will work for a full 40 years. If women have interrupted careers because of children the gains would be lower. However, it may well be the case that the program will increase job attachment, either directly or through the shift toward the formal sector. With a one-off experiment it is not possible to firmly quantify these factors. Finally, we have not

²⁵ All conversions to US dollars are made at a rate of US\$1 = COP\$1,970.00.

factored in the welfare loss of raising funds for the program through distortionary taxation. But overall, for women, the program may generate substantial benefits.

For men, there are no easily measurable gains from the program. There is the shift to the formal sector, which carries with it nonmonetary benefits and possibly better longer term job attachment, but we have little information to evaluate this. Also, we prefer to be cautious, interpreting the results for men because of the potential biases due to attrition and the initial imbalance.

While it is possible that the program encouraged the creation of new jobs by increasing the supply of qualified workers and by improving intermediation in the labor market, some of the gains for program participants could have come at the expense of displacing some nonparticipants. For example, when the training program approached firms to participate in the program they may have refrained from hiring in the open market so as to create an internship position. However, we do not find evidence of displacement above. On the other hand, the potential employees that would have been hired could probably find jobs elsewhere, as one would expect in a reasonably competitive market. The problem may be more important if the training program is large relative to the labor market. This can cause general equilibrium effects on wages, reducing the returns. Moreover, the employment effects we observe could be due to labeling of the program employees. This could be some sort of certification effect, where workers have been screened for basic skills and honesty by the program. Of course, this activity could have value in itself and should not be completely discounted. However the point remains that for a deeper understanding of the effects of the program we need to understand better the operations of the labor market and how a scaled up program will affect it.²⁶

The high returns to training for women 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, it would take about 22 months of pay to cover the entire cost based on the average pay at the time the program was initiated. 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.

VI. 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

²⁶Blundell et al. (2004) consider the possibility that a UK labor market program, which offered job search assistance and placement services, could have caused displacement. They do this by comparing estimates of effects obtained between regions that implemented and did not implement the program to those obtained by looking out for outcomes of eligible and ineligible individuals within the region. More generally a program that randomized the intensity of treatment across randomly chosen treatment regions could have provided some information on the possible displacement effects and the implications of scaling up.

little education in the context of a middle-income country. The program offered vocational training for a total period of six months (three months in classroom and three months on-the-job) to young unemployed men and women, who belonged to the lowest two strata in the population and who were, for the most part, high school dropouts. Most important for the purpose of this evaluation, the program randomly offered training to these young men and women.

The results show that the program had substantial effects for women. In particular, training increased wage and salaried earnings and the probability of having paid employment. Salaried earnings increased by close to 20 percent for women alone. As is standard in these interventions, there is some loss in work experience due to the time in the classroom, which is reflected in loss of tenure for the treatment group. In particular, we find a decrease in tenure of a little over one month. The results are robust to controlling for site-by-course fixed effects and pretreatment 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 for women at baseline.

Our results show that training offers increase earnings both due to increased employment and due to increases in productivity and access to better jobs. We find an increase in the probabilities of having a formal sector job and a written contract of 0.053 and 0.066, respectively, suggesting that part of the increased earnings for those trained is likely due to access to better jobs. For men the shift from informal to formal work is the only discernible effect of the program, but even this effect we interpret with caution due to potential biases due to attrition and initial imbalances for men.

These results constitute the basis for a cost-benefit analysis. Even the most conservative of the cost-benefit calculations, which ignore the benefits associated with the higher probability of being employed in the formal sector, and which allow the benefits to depreciate over time, suggest that the net benefits of the program more than justify its existence and possibly its expansion. Under this pessimistic scenario, the internal rate of return is 21.6 percent for women. Given the high returns to training for women, the question remains as to why similar types 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.

By most standards, including cost-benefit criteria, Youth in Action is a success for women. This contrasts with results obtained in industrialized countries, such as the United States, the United Kingdom, and others, as discussed earlier. A priori, there is little reason to expect that in such different contexts the results should be similar. However, it is still useful to highlight what aspects of this program may have contributed to making it successful.

First, the program provided six months of specific skills in certain sectors or occupations in the classroom and on the job, suggesting an important specific human capital component to the training. Second, private sector institutions—some for profit and some nonprofit—offered the classroom training and chose, designed, and marketed the courses to the firms providing the internships. Training institutions, thus, had to offer courses that provided skills for which there was demand in the

labor market. There is already some evidence that both these aspects are important for the success of training programs.²⁷ Third, the internships allowed both firms and workers to obtain information on the other side of the market. From the employers' side, the internships allow firms to acquire information on the quality of workers without having to commit with a written contract subject to the high dismissal costs in Colombia. From the workers' side, the internships provide information on jobs just becoming available that are not announced through formal channels, as well as information on what sort of skills are required for a job.

Our results are consistent with the reasoning underlying the recent recommendation by the Economic Commission for Latin America and the Caribbean (ECLAC) "[to establish] a national training and skills development system which provides internships in business and links to employers" as a solution to the youth unemployment problem in the region (Martin Hopenhayn 2002). Given these perceptions, it would be worthwhile to further explore the causal impact of on-the-job versus classroom training on youth labor market success, as well as the differential impact of training versus job search assistance directly designed to improve matches.

APPENDIX A: 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 who visited the households of individuals in the treatment and control groups on average three 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.

Employment and Paid Employment.—The employment variable is an indicator variable which takes the value of one if the person reports to have had a job during the year after finishing training or zero 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 in the main job held during the year after finishing training.

²⁷Barbara Sianesi (2001), for example, shows that among the Swedish programs the ones relating to wage subsidies and internships are the most successful. Blundell, Lorraine Dearden, and Meghir (1996) show that private employer-provided training is the one with positive returns.

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 one 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 one 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, 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.

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

Formal and Informal Wage and Salary Earnings.—Formal wage and salary earnings are the monthly salaries and wages earned in the main formal job held during the year after having finished training for salaried workers. Similarly, informal wage and salary earnings are the monthly salaries and wages earned in the main informal job held during the year after having finished training for salaried workers. We impute zero earnings for those who reported being either unemployed or out of the labor force. For formal earnings, we impute zero earnings for all of those who were identified as employed in the informal sector. By contrast, for informal earnings, we impute zero earnings for all of those who were identified as employed in the formal sector. As before, earnings are deflated by a city-specific CPI.

TABLE A1—DISTRIBUTION OF TRAINING COURSES

Course name	Number of courses	Course name	Number of courses
Inventory and warehouse assistant	18	Agricultural machinery mechanic	1
Taxi/bus driver	2	Cashiers' assistant	2
Electrician	16	Plumbers' assistant	2
Management assistant	3	Seamstress/industrial textile prod.	17
Security guard/building maintenance	8	Library assistant	3
Secretary/administrative assistant	34	Gas station assistant	3
Sales representative	43	Environmental assistant	3
Bakery assistant	5	Organic waste processor	1
Kitchen/cooking assistant	13	Industrial production operator	4
Archival assistant	18	Flower cultivation	5
Pharmacy assistant	6	Metal fabrication	3
Doctor/dentist/nurse assistant	12	Construction operator	1
Carpenter	7	Sports referee	1
IT assistant	13	Senior citizens assistant	6
Clinical lab assistant	2	Marketing assistant	20
Auto/motorcycle mechanic assistant	11	Meat processor	6
Human resources assistant	5	Cleaning services	5
Welding assistant	1	Cattle farming	1
Graphic design assistant	5	Organic farming	1
Refrigeration equipment assistant	1	Waste processor	1
Data entry assistant	14	Packing operator	1
Client relations assistant	16	Shoe repair services	1
Upholster	1	Florist	1
Wooden furniture painter assistant	3	Journeyman	2
Preschool teacher assistant	10	Tourism assistant	1
Accounting assistant	10	Wooden machine operator	1
Foreign trade assistant	2	Molding and foundry worker	2
Beautician	4	Vocational training teacher	2
Mail delivery assistant	10	Journalism assistant	1
Real estate assistant	2	Bank teller	3
Busboy/waiter/waitress	10	Physical rehabilitation	1
Recreation assistant	11	Food processing	4
Call center/telemarketing assistant	5	Quality control assistant	1
Surveyor assistant	9	Worker safety assistant	1
Gas installations	1		

APPENDIX B: DERIVATION OF EQUATION (4)

We further decompose the first term in brackets in equation (3) in the text as

$$\begin{aligned}
 & E(S|L = 1, R = 1) - E(S|L = 1, R = 0) \\
 &= E(S|L(1) = 1, L(0) = 0, R_i = 1) \\
 &\quad \times \frac{\Pr(L(1) = 1, L(0) = 0)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \\
 &+ E(S|L(1) = 1, L(0) = 1, R_i = 1) \\
 &\quad \times \frac{\Pr(L(1) = 1, L(0) = 1)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]}
 \end{aligned}$$

$$\begin{aligned}
& - \left\{ E(S|L(1) = 0, L(0) = 1, R_i = 0) \right. \\
& \quad \times \frac{\Pr(L(1) = 0, L(0) = 1)}{[\Pr(L(1) = 0, L(0) = 1) + \Pr(L(1) = 1, L(0) = 1)]} \\
& \quad + E(S|L(1) = 1, L(0) = 1, R_i = 0) \\
& \quad \left. \times \frac{\Pr(L(1) = 1, L(0) = 1)}{[\Pr(L(1) = 0, L(0) = 1) + \Pr(L(1) = 1, L(0) = 1)]} \right\}.
\end{aligned}$$

By suitable subtractions and corresponding additions, this can be rearranged as follows:

$$\begin{aligned}
& E(S|L = 1, R = 1) - E(S|L = 1, R = 0) \\
& = [E(S|L(1) = 1, L(0) = 0, R_i = 1) - E(S|L(1) = 1, L(0) = 0, R_i = 0)] \\
& \quad \times \frac{\Pr(L(1) = 1, L(0) = 0)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \\
& \quad + [E(S|L(1) = 1, L(0) = 1, R_i = 1) - E(S|L(1) = 1, L(0) = 1, R_i = 0)] \\
& \quad \times \frac{\Pr(L(1) = 1, L(0) = 1)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \\
& \quad + [E(S|L(1) = 1, L(0) = 0, R_i = 0) - E(S|L(1) = 0, L(0) = 1, R_i = 0)] \\
& \quad \times \frac{\Pr(L(1) = 0, L(0) = 1)}{[\Pr(L(1) = 0, L(0) = 1) + \Pr(L(1) = 1, L(0) = 1)]} \\
& \quad + E(S|L(1) = 1, L(0) = 0, R_i = 0) \\
& \quad \times \left\{ \frac{\Pr(L(1) = 1, L(0) = 0)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \right. \\
& \quad \quad \left. - \frac{\Pr(L(1) = 0, L(0) = 1)}{[\Pr(L(1) = 0, L(0) = 1) + \Pr(L(1) = 1, L(0) = 1)]} \right\} \\
& \quad + E(S|L(1) = 1, L(0) = 1, R_i = 0) \\
& \quad \times \left\{ \frac{\Pr(L(1) = 1, L(0) = 1)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \right. \\
& \quad \quad \left. - \frac{\Pr(L(1) = 1, L(0) = 1)}{[\Pr(L(1) = 0, L(0) = 1) + \Pr(L(1) = 1, L(0) = 1)]} \right\}.
\end{aligned}$$

The first two parts are the causal elements, while the last three parts are the various sources of selection bias.

Assuming monotonicity, i.e., $\Pr(L(1) = 0, L(0) = 1) = 0$. This gives the following expression:

$$\begin{aligned}
 & E(S|L = 1, R = 1) - E(S|L = 1, R = 0) \\
 &= E(S|L(1) = 1, L(0) = 0, R_i = 1) \\
 &\quad \times \frac{\Pr(L(1) = 1, L(0) = 0)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \\
 &\quad + E(S|L(1) = 1, L(0) = 1, R_i = 1) \\
 &\quad \times \frac{\Pr(L(1) = 1, L(0) = 1)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \\
 &\quad - E(S|L(1) = 1, L(0) = 1, R_i = 0).
 \end{aligned}$$

However, we can write this in a more useful way:

$$\begin{aligned}
 & E(S|L = 1, R = 1) - E(S|L = 1, R = 0) \\
 &= [E(S|L(1) = 1, L(0) = 0, R_i = 1) - E(S|L(1) = 1, L(0) = 0, R_i = 0)] \\
 &\quad \times \frac{\Pr(L(1) = 1, L(0) = 0)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \\
 &\quad + E(S|L(1) = 1, L(0) = 0, R_i = 0)] \\
 &\quad \times \frac{\Pr(L(1) = 1, L(0) = 0)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \\
 &\quad + [E(S|L(1) = 1, L(0) = 1, R_i = 1) - E(S|L(1) = 1, L(0) = 1, R_i = 0)] \\
 &\quad \times \frac{\Pr(L(1) = 1, L(0) = 1)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \\
 &\quad + E(S|L(1) = 1, L(0) = 1, R_i = 0)] \\
 &\quad \times \frac{\Pr(L(1) = 1, L(0) = 1)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \\
 &\quad - E(S|L(1) = 1, L(0) = 1, R_i = 0).
 \end{aligned}$$

Rearranging the last two terms, we get

$$\begin{aligned}
 & E(S|L = 1, R = 1) - E(S|L = 1, R = 0) \\
 &= [E(S|L(1) = 1, L(0) = 0, R_i = 1) - E(S|L(1) = 1, L(0) = 0, R_i = 0)] \\
 &\quad \times \frac{\Pr(L(1) = 1, L(0) = 0)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \\
 &\quad + E(S|L(1) = 1, L(0) = 0, R_i = 0)] \\
 &\quad \times \frac{\Pr(L(1) = 1, L(0) = 0)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \\
 &\quad + [E(S|L(1) = 1, L(0) = 1, R_i = 1) - E(S|L(1) = 1, L(0) = 1, R_i = 0)] \\
 &\quad \times \frac{\Pr(L(1) = 1, L(0) = 1)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \\
 &\quad - E(S|L(1) = 1, L(0) = 1, R_i = 0)]. \\
 &\quad \times \left\{ 1 - \frac{\Pr(L(1) = 1, L(0) = 1)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \right\}.
 \end{aligned}$$

Noting that

$$\begin{aligned}
 & \left\{ 1 - \frac{\Pr(L(1) = 1, L(0) = 1)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \right\} \\
 &= \left\{ \frac{\Pr(L(1) = 1, L(0) = 0)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \right\},
 \end{aligned}$$

we can rearrange again to get

$$\begin{aligned}
 (4) \quad & E(S|L = 1, R = 1) - E(S|L = 1, R = 0) \\
 &= [E(S|L(1) = 1, L(0) = 0, R_i = 1) - E(S|L(1) = 1, L(0) = 0, R_i = 0)] \\
 &\quad \times \frac{\Pr(L(1) = 1, L(0) = 0)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]}
 \end{aligned}$$

$$\begin{aligned}
& + [E(S|L(1) = 1, L(0) = 1, R_i = 1) - E(S|L(1) = 1, L(0) = 1, R_i = 0)] \\
& \times \frac{\Pr(L(1) = 1, L(0) = 1)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \\
& + [E(S|L(1) = 1, L(0) = 0, R_i = 0) - E(S|L(1) = 1, L(0) = 1, R_i = 0)] \\
& \times \frac{\Pr(L(1) = 1, L(0) = 0)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]}.
\end{aligned}$$

The first two terms are causal effects on compliers and always takers, respectively, while the last term is a selection/composition bias term.

To be able to use equation (4) to bound the effects, we first note the following equalities:

$$\Pr(L(1) = 1, L(0) = 1) + \Pr(L(1) = 1, L(0) = 0) = \Pr(L = 1 | R = 1)$$

$$\Pr(L(1) = 1, L(0) = 1) + \Pr(L(1) = 0, L(0) = 1) = \Pr(L = 1 | R = 0).$$

Using the monotonicity assumption, we can then rewrite the expression that multiplies the brackets in the first and third term in equation (4) as follows:

$$\begin{aligned}
& \frac{\Pr(L(1) = 1, L(0) = 0)}{[\Pr(L(1) = 1, L(0) = 0) + \Pr(L(1) = 1, L(0) = 1)]} \\
& = \frac{\Pr(L = 1 | R = 1) - \Pr(L = 1 | R = 0)}{\Pr(L = 1 | R = 1)}.
\end{aligned}$$

Thus, the term in square brackets in equation (4) can be bounded by using the mean salary in the tenth and ninetieth percentile for those in the control group who have paid employment. Rearranging equation (4) and substituting the term above, we get that the lower and upper bounds of the causal effects are

$$\begin{aligned}
& \left\{ E(S|L = 1, R = 1) - E(S|L = 1, R = 0) \pm \right. \\
& \quad \left. [E(S(p(0.90))) - E(S(p(0.10)))] \right. \\
& \quad \left. \frac{\Pr(L = 1 | R = 1) - \Pr(L = 1 | R = 0)}{\Pr(L = 1 | R = 1)} \right\}.
\end{aligned}$$

It is reasonable to assume that the “always takers” are at least as productive as the “switchers,” so that the average earnings without treatment of the “always takers” are at least as high as the average earnings without treatment of the switchers. Thus,

our estimates of the productivity effect of training would be a lower bound, and the upper bound would be given by the expression above which adds the second term.

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1. Orazio Attanasio, , Adriana Kugler, , Costas Meghir. 2011. Subsidizing Vocational Training for Disadvantaged Youth in Colombia: Evidence from a Randomized Trial. *American Economic Journal: Applied Economics* 3:3, 188-220. [[Abstract](#)] [[View PDF article](#)] [[PDF with links](#)]