Youth Training in the Dominican Republic: New Evidence from a Randomized Evaluation Design*

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1.   Introduction[1]

Since 1992, the Inter-American Development Bank has financed a series of innovative training programs throughout Latin America. These programs target less-educated youth – a group that faces substantial barriers to labor market success in both developing and developed economies– with the explicit aims of raising participants' job skills and matching them to suitable employers.[2] Drawing on lessons from evaluations of the Job Training Partnership Act in the U.S. and the Youth Training Scheme in Britain, these programs combine classroom training with a subsequent internship period of on-the-job work experience.[3] Unlike earlier training schemes in the region, they also place a heavy emphasis on the private sector, both as a provider of training and as a demander of trainees. Private training firms compete for public funds with proposals that need to be backed by commitments from local employers to offer internships.

Of these programs, the *Juventud y Empleo* (JE) program (Youth and Employment Program) in the Dominican Republic was the first to incorporate a randomized evaluation design in the operation of the program. Card et al. (2011) report of the evaluation of one of the early cohorts of the program, which will be discussed below. A similar program in Colombia, *Jóvenes en Acción,* also incorporated a randomized design to allow for the evaluation of the last cohort of trainees of the program (see Attanasio, Kugler, and Meghir (2011)). This paper summarizes the impacts on labor market outcomes (employment, hours of work, monthly earnings and hourly wages) of a revamped version of JE on a recent cohort of trainees.

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[1] Several sections of this document closely follow Card et al. (2011), which presents the results of the evaluation of an earlier cohort of this program.

[2] See Heckman, Lalonde, and Smith (1999) for a general overview of training programs, and Betcherman, Olivas and Dar (2004) for a recent summary that includes some evaluations of developing country training programs.

[3] The Job Training Partnership Act program is described extensively by Heckman, Lalonde and Smith (1999). Dolton, Makepeace and Treble (1994) describe the Youth Training Scheme.

Our analysis is based on a sample of applicants for the cohort of trainees that participated in a version of the JE program that was modified following the first impact evaluation. The cohort under study applied to receive training during 2008. Baseline data were collected from applicants prior to random assignment. A follow-up survey was administered in winter of 2010-2011, some 18 to 24 months after most trainees had finished their initial course work. Unlike the first evaluation of the JE program where the randomized design was compromised by the failure to include no-shows and early dropouts in the follow-up survey, the implementation of this evaluation was based on a sample of all the individuals that participated in the random assignment. The new evaluation also benefits from a larger sample (4,039 individuals) compared to the initial evaluation (that had 1,345), and it covers a longer post-program period (18-24 months versus 10-14months after graduation).

The next section provides the specifics of the program, in particular the changes introduced since the previous evaluation, as well as a description of the results and limitations of the previous evaluation and the basic design features of this evaluation. Section 3 presents data that illustrate the randomization process and that describe the flow of the program. Preliminary results are described in section 4, followed by conclusions in section 5.

2. *Juventud y Empleo:* **the project, the experimental evaluation design and its results.**

The *Juventud y Empleo (JE)* program – Youth and Employment Program – is a Dominican ALMP that aims to improve the labor market insertion of youngsters between 16-29 years of age that did not complete high school. It operates since 2001 and it was the first job training program in Latin America to incorporate a randomized evaluation component when the project was designed.

The program offers a wide range of job training courses such as administrative assistant, baker, hair stylist, clerk, car repairer, barman, and so on. The Ministry of Labor outsources the provision of training services to private training institutions (*Centros Operadores del Sistema*, COS) that are registered and approved by the national training institution (*Instituto Nacional de Formación Técnico Profesional*, INFOTEP). Courses (with duration of 225 hours) are conducted in the COS facilities and split into two parts: basic or life skills training (75 hours) and technical/vocational training (150 hours). Basic skills training is meant to strengthen trainees' self- esteem and work habits, while vocational training was meant to be customized to the technical training needs of local employers. Training at the COS is followed by an internship in a private sector firm, which is supposed to be contacted by the COS in order to develop training programs tailored to the labor demand of the firm.

Youngsters are identified by the COS according to their preferred vocation and the availability of the desired course. Once they complete 35 potential participants, the COS sends the names and identification numbers to the Program's Coordinating Unit (PCU) that randomly selects those who are offered the training course. From a group of potential participants, 20 are randomly assigned to a treatment group and are notified by phone the date and conditions under which the course will work. The other fifteen youngsters are randomly assigned to the control group but five of them are randomly ordered on a waiting list that is used in the case that a participant does not show up. In addition, if no one from the waiting list was called to fill a no-show spot, he or she would be destined to be part of the control group.

The original impact evaluation of this program (Card et al. (2011)) was based on a sample of applicants of the second cohort of the JE program who applied to receive training in early 2004. Baseline data

were collected from applicants prior to random assignment through registration forms that were completed at the COS. A follow-up survey was administered in the period from May to July 2005; some 10 to 14 months after most trainees had finished their initial course work. Simple comparisons between trainees in the follow-up survey and members of the control group show little impact on employment, although there is some evidence of a modest impact (10%) on wages. Unfortunately, however, the randomized design of the JE evaluation was potentially compromised by the failure to include in the follow-up survey people who were originally assigned to receive training but failed to show up (or attended for only a very short time).

Moreover, in spite of the fact that the random assignment was well implemented centrally at the project central office, compliance was not perfect: some of the winners of the lottery (intended to be treated) did not participate in the training (either they did not show up or dropped at some point), and some that were selected to the control group ended up taking the training (either as replacements of drop-outs and no-shows or by some other reason). Imperfect compliance is the norm in voluntary programs in which individuals decide whether or not to participate if offered the program.

Setting

Introducing a general notation, let Z_i represent the random assignment of each youngster i where $Z_i = 1$ are those randomly assigned to the treatment group and $Z_i = 0$ are assigned to the control group. Similarly, let D_i represent the final treatment status with $D_i = 1$ for those who attended the course and $D_i = 0$ who did not do so.

In the cohort that was analyzed in Card et al. (2011) there was a total of 2,564 applicants (31%) that were randomly assigned to the control group ($Z_i = 0$), while 5,801 (69%) were randomly assigned to the treatment group ($Z_i = 1$).[4] Of the original treatment group, 1,011 were "no-shows" (17% of the initial group with the pair $Z_i = 1$ and $D_i = 0$), while 4,791 are recorded as receiving training ($Z_i = 1$ and $D_i = 1$). To fill the places of the no-shows, 941 members of the original control group were reassigned to the treatment group ($Z_i = 0$ and $D_i = 1$), leading to a "realized treatment group" of 5,723 (=4,791+941 with $D_i = 1$) and a "realized control group" of 1,623 ($D_i = 0$).

As shown in Card et al. (2011), comparisons between those originally assigned to the control group and those originally assigned to treatment suggest the two groups were very similar. The results of that evaluation suggest that the realized treatment group had a slightly higher employment rate (0.574 versus 0.560, not statistically significant with t==0.5) and somewhat higher average earnings (3,133 Pesos/month versus 2,677, which is significant at conventional levels with t=2.13).

Card et al. (2011) deal with the problem caused by the failure to follow-up no shows (i.e. those with $Z_i=1$ and $D_i=0$) through selection correction models and by showing with the baseline data that the characteristics of the no-shows were similar from those of the replacements. They also exclude the reassigned controls from alternative specifications and the results hold. In any case, the estimated impacts on employment are all fairly close to zero and there are no significant differences by gender, age, education, or geographic location. The estimated impacts on monthly earnings are fairly similar for men and women, and for younger and older workers, but show interesting patterns by education and geographic location. In particular, the overall impact on earnings seems to be generated by a large

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[4] During the first phase of the program the randomization in different proportions: 20 assigned to treatment, 10 to controls.

positive effect for better-educated workers (adjusted impact = 807, t=2.54) coupled with a minimal effect for the less-educated. In terms of regional variation, the evaluation finds a relatively large positive effect for residents of Santo Domingo (adjusted impact = 804, t=2.71) coupled with a minimal effect for those outside the capital city. If one compares better-educated applicants in Santo Domingo to all others the results are even more striking: this subgroup accounts for virtually all of the observed positive impact on monthly earnings. While interesting it is important to note that these findings must be interpreted cautiously, since the subsample of largest impact was determined after the fact, rather than based on an ex ante analysis plan.

The lack of data for the no-shows in the follow-up survey means that the observed mean outcomes for the realized treatment group are potentially biased estimates of the means for everyone who was initially assigned to treatment.[5] In a correct setting with follow-up information for everyone, the comparison of the mean wages and employment conditions between the original treatment and control groups yields the Intention to Treat Effect – ITT, i.e. the impact of being assigned to treatment without considering the final participation status (a no-show is a primary treated participant but a non-treated one once the program is competed).

This second evaluation focuses on a modified version of the program and its evaluation design. While the core of the project (two-stage training followed by an internship) is maintained and the evaluation is still based on random assignment, there were some important changes:

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[5] However, in the case of employment, a simple bound can be constructed that is completely agnostic about the behavior of the missing no-show group (Manski, 1989).

- COS are supposed to work closer to the firms that provide the internship in order to develop tailored courses to train people for real vacancies

- The life-skills section of the training was revamped. Firms argued that what they value most from training were the general job-readiness / life-skills rather than the technical training.[6]

- Random assignment was done on a larger sample for each course (20 treatments, 15 controls).

3.   Design and implementation of the second impact evaluation of *Juventud y Empleo*

Random assignment was done on a group of potential participants identified by the COS that meet certain criteria: 1. youngsters between 16 to 29 years of age; 2. belong to the poorest households of the country; 3. not currently attending school; 4. with incomplete high school education or less; 5. currently unemployed, under-employed or occupationally inactive; 6. hold a Dominican identity card[7]. The program receives the information from the COS and proceeds to verify that none of the applicants had been registered before. For each course, the COS submits data of 35 eligible and interested youngsters that are randomly selected and then divided into two groups. The first one is formed by 20 youngsters that are offered the program. The second one (15 youngsters) is assigned to the control group.

If someone of those offered the program does not respond or drops out before the tenth day of the ongoing classes, the COS may replace up to 5 slots with those belonging to the control group. The

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[6] Actually, a complementary evaluation based on another cohort of trainees led by the World Bank analyzed the impact of providing only life-skills versus the traditional training, and th preliminary findings suggest that there is no valued added of the technical training. A qualitative analysis by Fazio (2011) presents additional evidence that firms value more the life skills component than the technical training.

[7] The membership to the poorest household of the country was strictly related to the location of the youngsters across the country and a normative priority established by the national government. In a targeting report, it was revealed that 72% of the postulants met the location criteria but only 40% were poor (Morillo, 2010).

replacements are supposed to be randomly selected by the PCU who provides the names directly to the COS.

As in the first evaluation, despite having an ideal initial configuration of the treatment and control groups due to the successful randomization, there is imperfect compliance due to (non-random) decisions by COS and by participants. The following table clarifies the setting[8]:

Table 1: Classification of participants by assignment and treatment status.

	Selected in the Lottery, $Z_i=1$	Not selected in the Lottery, $Z_i=0$
Participated in the program, $D_i=1$	"Complier" Beneficiaries	Replacements / Always Takers
Did not participate in the program, $D_i=0$	No-shows , dropouts / Never Takers	"Complier" Control group

Source: Authors

During the registration process, the program identified 10,309 applicants that met the selection criteria, with the following distribution according to the administrative data:

Table 2: Classification of treatment groups.

	Selected in the Lottery, $Z_i=1$	Not selected in the Lottery, $Z_i=0$	TOTAL
Participated in the program, $D_i=1$	4,937	977	5,914
Did not participate in the program, $D_i=0$	977	3,418	4,395
TOTAL	5,914	4,395	10,309

Source: Administrative data.

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[8] We refer to the typical assumptions of the *LATE Theorem* shown by Angrist and Pischke (2009) regarding the independence, exclusion, fist stage and monotonicity. Especially, we exclude from the analysis the existence of *defiers* which is derived from the monotonicity assumption according to Imbens and Rubin (1993). The latter means that there are no participants of the PJE that adopted an opposite treatment to the one that they were assigned.

The reason why the number of never-takers equals the number of always-takers is because the PCU tried to maintain the number of participants per course.

4. Estimating the impact of the program

The follow-up data was collected on a sample of those who participated in the lottery and thus is suitable for the estimation of the effect of the program on those to whom a course was randomly offered to. This is the Intention to Treat Effect (ITT) that estimates the impact of offering the JE program, regardless of what happened after the random assignment (some youngsters finally declined to attend the courses or dropped out during the first week of classes, while some of those assigned to the control group ended up receiving the treatment).

Formally the ITT is defined in terms of the differences of the expectations of an outcome (Y_i) given the random assignment:

$$ITT = E[Y_i \mid Z_i = 1] - E[Y_i \mid Z_i = 0] \tag{1}$$

Because it yields the causal effect of Z_i (Duflo *et al*, 2006), the estimation of the ITT includes all the group of youngsters that participated in the random assignment including those for which $D_i \neq Z_i$ (the pairs $D_i = 1 \mid Z_i = 0$ and $D_i = 0 \mid Z_i = 1$, formed by those who took the course given that they were randomly assigned to the control group and those that did show up or dropped out given that they belong to the treatment group, respectively). Therefore, one may expect that the effect of offering the JE becomes smaller as long as the proportion of those with $D_i \neq Z_i$ increases. If the training has a

positive impact and we compare the Zi=1 with Zi=0 but less than 100% of the former took the course and more than 0% of the latter participated in the program, then the comparison yields an underestimation of the "true" program impacts. This is why under imperfect compliance of the random assignment the ITT leads to the dilution of the impact of the program.

Under certain conditions[9], it is also possible to estimate the impact of the program on the compliers, i.e. those that took the course because they were selected in the lottery. This is known as the estimation of the Local Average Treatment Effect (LATE) which yields a larger impact than the ITT, since it basically assumes that any difference in the outcome between Zi=1 and Zi=0 is due to the larger fraction of the former that participated in the training. To see how this is related to the ITT we can present equation 1 in terms of the expected effect of the program for compliers times the compliance rate (π_c)[10]:

$$ITT = E[Y_i \mid Z_i = 1] - E[Y_i \mid Z_i = 0] = E[Y_i(1) - Y_i(0) \mid compliers] \cdot \pi_c \qquad (2)$$

If we rearrange equation 2, the effect on compliers is observed as a transformation of the ITT:

$$LATE = E[Y_i(1) - Y_i(0) \mid compliers] = ITT / \pi_c \qquad (3)$$

Similarly, the compliance rate π_c can be shown as the slope of the regression of D_i on Z_i:

$$\pi_c = E[D_i \mid Z_i = 1] - E[D_i \mid Z_i = 0] \qquad (4)$$

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[9] Supra.

Substituting equation 4 on equation 3 the *LATE* is finally represented as an Instrumental Variable (IV) estimator where the *ITT* is obtained as a reduced form while π_c as a first stage (Angrist and Pischke, 2009):

$$LATE = \{E[Y_i \mid Z_i = 1] - E[Y_i \mid Z_i = 0]\} / \{E[D_i \mid Z_i = 1] - E[D_i \mid Z_i = 0]\} \qquad (5)$$

Thus, the estimation of the *LATE* for JE *compliers* yields the effect of the program on those whom were finally treated since they were randomly assigned to be; otherwise they would not have been recruited to attend the courses (Z_i is an instrument unrelated with Y_i but related to D_i).

Methods

The coefficients represented by equations 1 and 5 are estimated with Ordinary Least Squares (OLS) and Two Stage Least Squares (2SLS), respectively. Additionally, some other controls and interactions are included in the regression equations (gender, location, education, etc.) in order to estimate heterogeneous impacts as well (see appendix 1).

In the first case the estimating equation to measure the ITT on each outcome Y_i is:

$$Y_i = \beta_0 + \beta^{ITT} \cdot Z_i + \Sigma \beta_\kappa{}^{ITT} \cdot Z_i \cdot X_{\kappa,i} + \Sigma \beta_j \cdot X_j + e_i \qquad (6)$$

Where β_0 is the constant[11], β^{ITT} is equivalent to equation 1; β_κ^{ITT} is the heterogeneous ITT for the interaction of Z_i with some other characteristics, $X_{\kappa,i}$; β_j represents the coefficient of some control variables regarding location, gender and education denoted by X_j; finally, e_i is the random error.

In the second case, the estimation of the *LATE* requires the specification of the reduced form and first stage of the IV by a 2SLS equation:

$$Y_i = \beta_0 + \beta^{LATE} \cdot \widehat{D}_i + \Sigma \beta_\kappa^{LATE} \cdot \widehat{D} \cdot X_{\kappa,i} + \Sigma \beta_j \cdot X_j + e_i \tag{7}$$

$$\widehat{D} = \beta_0 + \beta z \cdot Z_i \tag{8}$$

Where β_κ^{LATE} corresponds to equation 5 while equations 7 and 8 are estimated simultaneously and they comply with the typical assumptions on IV and 2SLS (Angrist and Pischke, 2009).

Data description

The baseline data was collected at the registration stage at each COS, and it is available for all those eligible and interested to participate in the program, a total of 10,309 youth. The following table shows some characteristics from the baseline survey and a t-statistic for equality of means between Z_i groups as an evidence of randomness. It also specifies the means for D_i groups divided by the random assignment.

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Table 3: Basic characteristics at baseline.

Characteristic	Zi = 1 (a)	(Di = 0, 1 / Zi = 1) Di = 1	(Di = 0, 1 / Zi = 1) Di = 0	Zi = 0 (b)	(Di = 0, 1 / Zi = 0) Di = 1	(Di = 0, 1 / Zi = 0) Di = 0	Difference (Zi) (a) - (b)	t
Age	22.03	22.00	22.18	21.99	21.75	22.06	0.04	0.59
Gender (male = 1)	0.37	0.36	0.40	0.38	0.41	0.37	-0.01	-1.35
Marital status (married = 1)	0.24	0.24	0.26	0.24	0.22	0.25	0.00	-0.03
Number of children	0.71	0.70	0.73	0.70	0.66	0.71	0.01	0.42
Attend school (currently)	0.23	0.24	0.19	0.23	0.22	0.23	0.01	0.76
Incomplete elementary	0.20	0.20	0.22	0.20	0.20	0.21	0.00	-0.3
Complete elementary	0.05	0.05	0.04	0.05	0.06	0.05	0.00	-0.2
Incomplete high school	0.55	0.56	0.54	0.58	0.55	0.54	-0.03	1.09
Complete high school	0.04	0.04	0.03	0.03	0.03	0.03	0.00	0.79
More than high school	0.00	0.00	0.00	0.00	0.01	0.00	0.00	0.02
Missing education	0.04	0.04	0.05	0.04	0.04	0.04	0.00	-0.65
No data on education	0.11	0.11	0.12	0.12	0.12	0.12	-0.01	-1.19
Fraction with prior work experience	0.20	0.19	0.24	0.22	0.25	0.22	-0.02	1.37
Currently employed	0.04	0.04	0.06	0.04	0.04	0.04	0.00	0.15
Currently salaried worker	0.02	0.01	0.02	0.02	0.01	0.02	0.00	-0.23
Currently unemployed	0.53	0.53	0.53	0.52	0.55	0.52	0.00	0.28
ICV Score (0 to 100)*	62.81	62.78	62.97	62.93	62.91	62.94	-0.12	-0.59
- ICV (Type I)	0.02	0.03	0.01	0.02	0.02	0.02	0.00	0.95
- ICV (Type II)	0.03	0.03	0.02	0.03	0.03	0.03	0.00	-0.74
- ICV (Type III)	0.26	0.26	0.27	0.25	0.25	0.25	0.01	1.2
- ICV (Type IV)	0.08	0.07	0.08	0.08	0.07	0.08	0.00	0.12
- ICV (Type V)	0.53	0.53	0.54	0.56	0.55	0.56	-0.03	-2.33**
- ICV (Type VI)	0.08	0.08	0.08	0.07	0.07	0.07	0.01	2.26**
Urban areas	0.89	0.89	0.89	0.89	0.88	0.90	0.00	-0.47
Live in Santo Domingo	0.24	0.24	0.25	0.24	0.24	0.24	0.00	0.27
Receives remittances	0.11	0.10	0.11	0.11	0.10	0.11	0.00	-0.31
Observations	5,914	4,937	977	4,395	977	3,418		

Source: PJE baseline data and administrative records.
[a]: Stands for *Indice de Calidad de Vida* (Living Quality Index). It is divided into 6 levels from the poorest to the richest.
Note: Means, differences and t-statistics are calculated by linear regression with robust standard errors. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

Most of the participants are women (62%) and nearly a half of them have an incomplete high school education. Ninety percent of participants live in urban areas and about a quarter live in Santo Domingo. The average age is 22 years and about 22% attended school at the time of the baseline. As shown in the last column in the table, randomization worked as most of the characteristics are balanced.

It is interesting to note that only 4% of youngsters declared to be employed at the baseline prior to the beginning of the courses and that 52% of them were unemployed (hence, the 44% were inactive). This may be due to the requirements of the selection process that demanded an inactive or unemployed labor situation, and may also an expression of the Ashenfelter's dip, i.e. that both groups received a shock that increased unemployment levels right before the program started. According to the National Labor Force Survey (known as ENFT, *Encuesta Nacional de Fuerza de Trabajo*), in 2008 the employment rate for youngsters ages 16-29 with less than complete high school education was 43%.

After the completion of the courses, a follow-up household survey was carried out between November 2010 and February 2011 (18 to 24 months later) to a random sample of the 10,309 initial registered youngsters[12]. The sample size was set at 5,000 to detect an 8% increase in income with a power of 0.8 and an attrition of 30% of the sample[13]. This sample has 3,250 randomly assigned to the treatment group ($Z_i = 1$) and 1,750 randomly assigned to the control group ($Z_i = 0$).

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tional government. In a targeting report, it was revealed that 72% of the postulants met the location criteria but only 40% were poor (Morillo, 2010).

on part of the survey. In fact 55.6% and 55.2% of the control and treatment groups were surveyed in December 2010, respectively.

[13] We used the Stata command *sampsi*.

The questionnaire for the follow-up household survey was put together by an interdisciplinary team from the the Ministry of Labor of the Dominican Republic, the Inter-American Development Bank and the World Bank. It includes 15 modules that involve household composition, labor history, assets, time use, courses and the internship, consumption, health status, risk aversion, future expectations, pregnancy history, dwelling materials and basic skills (non-cognitive skills and self-esteem). Nonetheless, this version of the impact evaluation focuses on the core labor market outcomes such as employment, unemployment and its characteristics.

Most of the questions that measure labor market outcomes are based on the ENFT that is carried out twice a year by the Central Bank of the Dominican Republic. Some questions were modified and adapted to the JE evaluation and youth population; the basic indicators of labor participation are generated following the ENFT and allow performing an external validation of our data.

The follow-up survey

About 80% of the follow-up sample was located at their households, with virtually no difference between those selected and those not selected in the lottery (in the case of $Z_i = 1$ and those $Z_i = 0$, 80.8% and 80.4% of them were interviewed, respectively). This compares favorably to the first evaluation, where the attrition rate was larger and unbalanced between beneficiaries and members of the comparison group (the rates attrition rates were 35% and 45%, respectively).

We verified the similarity of the interviewed treatment and control groups. We also inspected how different are the basic characteristics of those that were interviewed from those that were not interviewed. Appendix 1 contains a table (A1) that compares the sample of those intended to be

interviewed at home and the final interviewed group of 2,629 and 1,407 randomly assigned to the treatment and control groups, respectively. Table A1 shows that the original sample and the realized one have similar characteristics at the baseline. Nonetheless, comparing the similarity among the original and the realized groups for the same random assignment (Z_i) there is a small unbalance in the poverty indicator (ICV) for the control groups in spite of the fact that the differences are close to zero. Regarding the treatment group (column *(c) - (e)*), the most significant difference emerged from school attendance and the location in urban areas for which the disparities are statistically significant but substantially very small. Hence, we assume that attrition was random and/or that if affected equally both those selected and not selected in the lottery.

Rearrangement of treatment groups and course characteristics

The follow-up survey included questions to confirm whether the respondent participated in the program or not. Although the PCU has administrative data on this, the COS enjoyed some degree of control on who takes the course. Also, in the process of replacing no-shows and/or dropouts some members of the Zi=0 group were contacted as replacements, and some of them may have declined to participate but are still classified as Zi=0, Di=0 instead of Zi=1,Di=0.

Youngsters were not classified as treatment or control based on administrative data prior to the follow-up survey, and pollsters did not have that information. They were asked if the COS contacted them after the registration with the aim of notifying them that their selection was successful and to inform them the date and time when the course would start. Table 5 presents the answers of this question, which imply a rearrangement of the D_i groups:

Table 4: Notification from COS
by result of the lottery

		Were you notified to start the course?		
	$Z_i = 0$		$Z_i = 1$	
Yes	614	46.9%	2,335	92.9%
No	695	53.0%	178	7.1%
Total	1,309		2,513	

Source: Follow-up survey.

Based on the follow-up survey, however, we find discrepancies between the administrative records of who participated in the program and the responses from the youngsters. Even allowing for replacements, a sizeable fraction of the "complier control-group" reported to have taken the course and revealed their opinions about their participation in the life and technical skills training and internship phases of the program.

In table 5 the administrative records had revealed that 22% of the randomly assigned control group ($Z_i = 0$) and 100% of the treatment group ($Z_i = 1$) were contacted by the COS and were taught the courses. Nonetheless, only 92.9% of those selected in the lottery reported that they had been called to participate in the course, and 47.1% of those not selected by the lottery report being contacted by a COS to participate in the program. This shows that compliance was low (in particular for the Zi=0 group). However, the lottery did have a strong impact on the probability of participating and thus constitutes a very strong instrument.[14]

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[14] Another filter was introduced. We wanted to make sure that those that were not contacted by the COS did not manage to participate in the program by other method. Hence they were asked if in spite of the fact that they were not called for

At the end the final composition of the treated and non-treated groups revealed a sharp difference among them, especially for those randomly assigned to the control group. For the latter, the administrative records reported something quite distinct from the information collected with the follow-up survey. Table 5 compares the administrative records to the findings of the follow-up survey and evidences that while 511 youngsters from the couple $D_i = 1 \mid Z_i = 0$ manifested that they had taken the program, in the administrative record only 284 of them were registered as attendees.

Table 5: Final composition of the treatment groups.

		Follow-up survey		Administrative data	
		$Zi = 0$	$Zi = 1$	$Zi = 0$	$Zi = 1$
D_i	0	89	102	316	232
	1	511	2,219	284	2,089
TOTAL		600	2,321	600	2,321

Source: Follow-up survey.

From the group of youngsters that were recruited by the COS, 89.4% of them completed the theoretical stage of the course and 92% of those started the internship. About 95% of interns ended this second stage of the program and received a certificate as JE graduates.

Regarding the characteristics of the firms where the internships took place, 74% of interns were placed at their preferred internship; internships lasted an average of 5.8 weeks. Similarly, as shown in table 7, 50% of interns worked at firms with less than 10 employees. Another 23.7% worked at firms with 51 or more employees.

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attending classes, they ended up receiving the course in any case. Therefore, approximately 30 cases from the control group with these characteristics were accepted by the COS to take the course and complete the internship.

Table 6: Internship's firm size.

Number of employees	
Does not know.	0.1%
1 employee	0.6%
2 - 4 employees	22.9%
5 - 10 employees	26.4%
11 - 19 employees	12.5%
20 - 30 employees	9.3%
31 - 50 employees	4.2%
more than 51 employees	23.7%

Source: Follow-up survey.

The first evaluation of JE reported that only 22% of trainees got a job in the firm where they completed their internship. The program was modified with the expectation that courses would be tailored to real vacancies and thus that a higher fraction of trainees would be offered jobs in the internship firms. Preliminary results suggest that about 40% of trainees received an offer at the internship firm, and of this number only half accepted it.

Table 7: Where you offered a job?

Firm size	Yes, and accepted the offer	Yes, but rejected the offer	No
Does not know.	0.0%	0.0%	100.0%
1 employee	7.8%	15.7%	76.5%
2 - 4 employees	12.2%	17.8%	69.9%
5 - 10 employees	13.5%	17.6%	68.9%
11 - 19 employees	18.2%	18.4%	63.4%
20 - 30 employees	20.4%	16.9%	62.7%
31 - 50 employees	23.1%	16.4%	60.5%
more than 51 employees	20.5%	14.1%	65.3%
TOTAL	16.5%	16.8%	66.7%

Source: Follow-up survey.

Table 7 shows that firms with between 31-50 employees are more likely to offer jobs to interns (40% of interns at those firms received an offer). Also, a large proportion of the job offers were rejected by the youngster: 17% of the rejections occurred because the intern did not like the activity they were assigned, 15.6% found a better job, 9.3% changed their activity and the rest (58%) had other reasons.

Outcomes

The labor market outcomes in the follow-up survey are presented in tables 8 and 9. Table 8 shows the employment rate by assignment and participation status. Overall, there are only minor differences between Zi=0 and Zi=1: employment is 59.0% and 59.7% respectively. Conditional on working, there are some minor differences as the share of salaried workers is slightly higher (59.0% versus 56.5%) and the share overed by labor risk insurance is 18% for Zi=1 and 16% for Zi=0. Those originally assigned to treatment were also more likely to have a written contract (22.3% versus 20.4%), and they had slightly higher monthly labor earnings.

Table 8: Employment characteristics in the follow-up survey

Outcome	$D_i = 0 \mid Z_i = 0$	$D_i = 1 \mid Z_i = 0$	$Z_i = 0$	$D_i = 0 \mid Z_i = 1$	$D_i = 1 \mid Z_i = 1$	$Z_i = 1$
Employed	56.7%	62.9%	**59.0%**	50.5%	61.2%	**59.7%**
Agriculture and mining	1.5%	2.5%	**1.8%**	0.8%	1.6%	**1.4%**
Industry	8.7%	9.8%	**9.1%**	7.9%	8.0%	**8.0%**
Services	89.8%	87.8%	**89.1%**	91.3%	90.4%	**90.5%**
Duration of current job (months)	16.5	15.8	**16.2**	17.4	15.2	**15.5**
Permanent Job	68.9%	66.3%	**67.8%**	67.2%	67.6%	**67.6%**
Employed at large firms	17.0%	25.8%	**20.5%**	24.5%	20.9%	**21.4%**
Salaried workers	53.7%	60.8%	**56.5%**	54.7%	59.5%	**59.0%**
Workers without pay	3.8%	5.8%	**4.6%**	3.1%	4.6%	**4.5%**
Self-employed workers	36.5%	28.9%	**33.5%**	29.7%	31.9%	**31.6%**
Workers with labor risk insurance	13.8%	19.5%	**16.0%**	19.3%	17.8%	**18.0%**
Workers with health insurance	25.1%	32.2%	**28.0%**	27.1%	29.6%	**29.3%**
Workers with written contract	17.2%	25.2%	**20.4%**	21.4%	22.4%	**22.3%**

Weekly worked days	5.0	5.2	**5.1**	5.1	5.1	**5.1**
Weekly worked hours	38.5	40.9	**39.4**	37.0	40.1	**39.7**
Wants to work more hours	70.7%	68.4%	**69.8%**	69.3%	69.6%	**69.5%**
Wants to change current job	80.8%	81.2%	**81.0%**	71.4%	80.2%	**79.2%**
Workers seeking for other job	27.3%	30.1%	**28.4%**	25.0%	31.7%	**30.8%**
Monthly wage (Dominican peso)	4,669.6	5,059.8	**4,824.9**	5,481.2	5,069.6	**5,116.8**
Hourly wage (Dominican peso)	42.3	38.4	**40.7**	45.8	41.5	**42.0**

Source: Follow-up survey.

Note: Outcomes are conditional on employment status. a: Large firms are those that employ 51 or more employees. b: One Dominican Peso = 0.026 US Dollar (April, 2011).

Labor force participation is examined in table 9, which shows that inactivity is larger for those assigned to treatment: in the case of females employment increases, while for males it is unemployment that offsets the decrease in inactivity (which is smaller than for females).

Table 9: Labor force participation status in the follow-up survey.

Outcome	$D_i = 0 \mid Z_i = 0$	$D_i = 1 \mid Z_i = 0$	$Z_i = 0$	$D_i = 0 \mid Z_i = 1$	$D_i = 1 \mid Z_i = 1$	$Z_i = 1$
All						
Employed	56.7%	62.9%	**59.0%**	50.5%	61.2%	**59.7%**
Unemployed	18.8%	22.2%	**20.0%**	11.8%	24.5%	**22.7%**
Inactive	24.5%	14.9%	**21.0%**	37.6%	14.2%	**17.6%**
Male						
Employed	72.0%	79.6%	**75.1%**	54.4%	76.2%	**72.9%**
Unemployed	9.7%	14.3%	**11.2%**	4.5%	19.5%	**14.8%**
Inactive	18.3%	6.1%	**13.7%**	41.1%	4.4%	**12.3%**
Female						
Employed	47.9%	51.1%	**49.0%**	48.1%	52.5%	**51.9%**
Unemployed	15.8%	27.7%	**19.2%**	4.5%	27.5%	**20.5%**
Inactive	36.3%	21.2%	**31.8%**	47.5%	20.0%	**27.6%**

Source: Follow-up survey.

Impacts

Given that the experimental design was well implemented, the analysis of the impacts of the program is straightforward. Here we report the ITT and the LATE for the following labor market outcomes:

- Employment
- Employment with health insurance
- Monthly earnings
- Hourly wage
- Labor force participation

The impacts are reported for the overall population and for the following groups:

- Female
- Male
- Residents of Santo Domingo
- Age 16-19
- Age 20-24
- Age 24+
- Incomplete elementary
- Complete elementary
- Incomplete secondary

Tables 10 and 11 present the basic outcome indicators of this evaluation. Table 10 reports the ITT estimates β^{ITT} from equation 6 for different population groups. The first column shows that, overall, the only impacts are on labor force participation, which increases in 3.2 percentage points. Moving about on that row, we find that the increase in labor force participation holds for females but not for males, is larger in Santo Domingo and for the younger group of participants. These results suggest that the program is being successful in reaching the inactive youth and encouraging them to participate in the labor market. However, once in the labor market there assignment to the program does not have positive impacts.

The fourth column shows the results for Santo Domingo, and there the results are different: there is a significant impact on employment of eight percentage points, an impact on the share receiving health insurance through the employer, and the monthly earning. Note that the outcomes are not conditional on employment because the randomness comes from the assignment to treatment and not from the employment status.

Table 9 shows the LATE estimates, which as explained above are larger as to compute them one basically assigns all the difference between Zi=1 and Zi=0 to the difference in the rate with Di=1 in each group, which is about 0.5 so the estimates are larger by a factor of two approximately. The basic interpretation holds, with the following caveat: the impacts on those who took the course because they were assigned to receive it are larger, but also limited to the same outcomes and subgroups.

Table 8: ITT estimation on selected outcomes.

Intention to Treat Effect	(1)	(2)	(3)	(4) Santo	(5)	(6)	(7)	(8) Incomplete	(9) Complete	(10) Incomplete
Outcomes	All	Female	Male	Domingo	Age 16-19	Age 20-24	Age > 24	Elementary	Elementary	High School
Employed	0.0059	0.0268	-0.0214	0.0798**	0.0247	0.0014	-0.0319	0.0104	0.0332	-0.0059
	(0.0176)	(0.0228)	(0.0234)	(0.0343)	(0.0366)	(0.0298)	(0.0363)	(0.0384)	(0.0731)	(0.0226)
Control mean	0.590	0.490	0.751	0.530	0.567	0.599	0.613	0.579	0.571	0.600
Employed with health insurance	0.0101	-0.0008	0.0320	0.0473**	0.0204	0.0145	-0.0127	0.0292	-0.0518	0.0075
	(0.0119)	(0.0138)	(0.0222)	(0.0223)	(0.0273)	(0.0209)	(0.0257)	(0.0260)	(0.0553)	(0.0156)
Control mean	0.165	0.128	0.225	0.150	0.172	0.173	0.144	0.130	0.171	0.182
Monthly earnings	187.63	197.51	264.13	1,084.80***	661.55*	78.24	-209.90	293.71	105.41	-59.54
	(159.58)	(152.48)	(294.04)	(392.63)	(357.52)	(234.58)	(324.96)	(318.08)	(534.00)	(204.78)
Control mean	2836	1905	4340	2653	2521	2839	3123	2508	2312	3064
Hourly earnings	0.89	1.40	0.70	2.77	7.70**	0.95	-4.31	-1.46	3.75	-0.30
	(1.74)	(1.92)	(3.16)	(4.63)	(3.81)	(2.52)	(4.31)	(4.15)	(4.11)	(2.22)
Control mean	23.89	18.08	33.26	26.65	18.35	23.48	29.92	22.53	15.58	25.31
Labor force participation	0.0319**	0.0406**	0.0218	0.0770***	0.0669**	0.0318	-0.0036	0.0176	0.0553	0.0279
	(0.0139)	(0.0197)	(0.0166)	(0.0274)	(0.0284)	(0.0214)	(0.0277)	(0.0317)	(0.0702)	(0.0183)
Control mean	0.790	0.728	0.890	0.769	0.773	0.791	0.803	0.765	0.757	0.804
Observations	4,010	2,504	1,506	953	846	1,529	843	798	202	2,269

Source: PJE baseline and follow-up data.

Note: Clustered standard errors at course level in parenthesis. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

Table 9: LATE estimation on selected outcomes.

Local Average Treatment Effect - LATE	(1)	(2)	(3)	(4) Santo Domingo	(5)	(6)	(7)	(8) Incomplete Elementary	(9) Complete Elementary	(10) Incomplete High School
Outcomes	All	Female	Male	Santo Domingo	Age 16-19	Age 20-24	Age > 24	Incomplete Elementary	Complete Elementary	Incomplete High School
Employed	0.0121	0.0527	-0.0476	0.1452**	0.0492	0.0028	-0.0648	0.0197	0.0624	-0.0119
	(0.0362)	(0.0449)	(0.0521)	(0.0625)	(0.0724)	(0.0606)	(0.0735)	(0.0726)	(0.1367)	(0.0457)
Control mean	0.548	0.479	0.665	0.497	0.513	0.561	0.567	0.529	0.623	0.554
Employed with health insurance	0.0209	-0.0015	0.0712	0.0860**	0.0406	0.0295	-0.0258	0.0556	-0.0976	0.0152
	(0.0244)	(0.0271)	(0.0488)	(0.0393)	(0.0540)	(0.0425)	(0.0519)	(0.0488)	(0.1048)	(0.0317)
Control mean	0.141	0.114	0.186	0.147	0.159	0.154	0.112	0.0956	0.164	0.160
Monthly earnings	385.98	388.34	588.15	1,973.46***	1,316.92*	159.84	-425.77	558.76	198.45	-120.99
	(327.92)	(300.72)	(652.64)	(718.11)	(710.48)	(478.03)	(655.97)	(598.28)	(997.21)	(415.48)
Control mean	2612	1808	3973	2847	2402	2476	2912	2419	2727	2715
Hourly earnings	1.84	2.75	1.56	5.04	15.33**	1.93	-8.75	-2.78	7.07	-0.60
	(3.57)	(3.78)	(7.01)	(8.38)	(7.58)	(5.15)	(8.78)	(7.89)	(7.79)	(4.51)
Control mean	23.07	18.96	30.04	28.17	17.86	24.29	25.96	19.75	18.10	24.13
Labor force participation	0.0657**	0.0798**	0.0486	0.1401***	0.1332**	0.0650	-0.0072	0.0335	0.1041	0.0567
	(0.0284)	(0.0383)	(0.0367)	(0.0495)	(0.0562)	(0.0434)	(0.0561)	(0.0596)	(0.1300)	(0.0369)
Control mean	0.715	0.668	0.795	0.702	0.718	0.702	0.722	0.643	0.738	0.741
Observations	4,010	2,504	1,506	953	846	1,529	843	798	202	2,269

Source: PJE baseline and follow-up data.

Note: Clustered standard errors at course level in parenthesis. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

A further analysis into the effects in Santo Domingo is presented in Table 10, which shows the results from breaking the sample by gender. The findings are quite interesting:

- The results on employment are driven by the impacts on females.

- The results on employer-provided health insurance are driven by impact on males. This finding is consistent with the results from the first impact evaluation.

- There results on monthly earnings hold for both groups. They are smaller in absolute levels but more significant for women, but in relative terms they are similar.

Table 10: ITT and LATE estimation on selected outcomes by gender in Santo Domingo

Santo Domingo	ITT		LATE	
	Female	Male	Female	Male
Employed	0.0956**	0.0502	0.1619**	0.1083
	(0.0457)	(0.0431)	(0.0776)	(0.0926)
Control mean	0.471	0.651	0.450	0.594
Employed with health insurance	0.0134	0.1192***	0.0227	0.2570***
	(0.0279)	(0.0443)	(0.0467)	(0.0926)
Control mean	0.129	0.193	0.123	0.198
Monthly earnings	815.69**	1,686.13*	1,382.37**	3,636.46**
	-367.14	-857.41	-640.45	-1,829.35
Control mean	2,059	3,877	2,158	4,279
Hourly earnings	4.3297	0.5107	7.3377	1.1015
	(5.3010)	(9.1945)	(9.0163)	(19.6192)
Control mean	22.32	35.58	24.82	35.12
Labor force participation	0.0810**	0.0680*	0.1372**	0.1467*
	(0.0376)	(0.0394)	(0.0627)	(0.0832)
Control mean	0.738	0.835	0.686	0.736
Observations	644	309	644	309

Source: PJE baseline and follow-up data.
Note: Clustered standard errors at course level in parenthesis. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

5. Conclusions

[TO BE COMPLETED]

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Appendix 1

Table A1: Basic characteristics at baseline for original sample to interviewed and interviewed at home (realized).

Characteristic	Baseline Zi = 1 (a)	Zi = 0 (b)	Follow-up (original) Zi = 1 (c)	Zi = 0 (d)	Diff. (c) - (d)	t	Follow-up (realized) Zi = 1 (e)	Zi = 0 (f)	Diff. (e) - (f)	t	Original vs. realized Diff. (c) - (e)	t	Diff (d) - (f)	t
Age	22.0	22.0	22.2	22.1	0.14	1.41	22.03	21.90	0.14	1.22	0.22	0.51	0.21	0.24
Gender (male = 1)	0.37	0.38	0.37	0.38	-0.01	-0.92	0.37	0.38	-0.01	-0.74	0.00	0.08	0.00	0.18
Marital status (married = 1)	0.24	0.24	0.24	0.24	0.00	0.34	0.24	0.23	0.01	0.84	0.00	0.35	0.01	1.22
Number of children	0.71	0.70	0.70	0.69	0.02	0.59	0.71	0.70	0.01	0.30	0.00	0.36	-0.01	-0.91
Attend school (currently)	0.23	0.23	0.23	0.23	0.00	0.05	0.24	0.24	0.00	0.21	-0.01	2.21**	-0.01	1.08
Incomplete elementary	0.20	0.20	0.20	0.20	0.00	-0.08	0.20	0.20	0.00	-0.36	0.00	-1.33	0.00	-0.18
Complete elementary	0.05	0.05	0.05	0.05	0.00	0.20	0.05	0.05	0.00	0.06	0.00	0.2	0.00	0.48
Incomplete high school	0.55	0.58	0.56	0.55	0.01	0.81	0.57	0.56	0.02	0.92	-0.01	1.78*	0.00	0.74
Complete high school	0.04	0.03	0.03	0.03	0.00	0.52	0.03	0.03	0.00	-0.05	0.00	-0.65	0.00	1.07
More than high school	0.00	0.00	0.00	0.00	0.00	1.25	0.00	0.00	0.00	1.53	0.00	1.58	0.00	-0.25
Missing education	0.04	0.04	0.04	0.04	0.00	-0.49	0.04	0.04	0.00	-0.08	0.00	-1.93*	0.01	-2.14
No data on education	0.11	0.12	0.11	0.12	-0.01	-0.08	0.11	0.12	-0.01	-1.24	0.00	0.34	0.00	-0.08
Number of jobs prior to PJE	0.25	0.23	0.23	0.25	-0.01	-1.03	0.23	0.24	-0.01	0.54	0.00	0.24	0.00	-0.67
Currently employed	0.04	0.04	0.04	0.04	0.00	-0.20	0.04	0.04	-0.01	-0.74	0.00	0.93	0.00	0.95
Currently salaried worker	0.02	0.02	0.01	0.02	0.00	-1.01	0.01	0.02	0.00	-0.83	0.00	0.05	0.00	0.24
Currently unemployed	0.53	0.52	0.53	0.52	0.01	0.51	0.53	0.51	0.01	0.80	0.00	0.55	0.00	0.54
ICV*	62.8	62.9	62.9	62.9	-0.02	-0.05	62.8	62.6	0.14	0.43	0.13	1.60	0.29	2.87**
- ICV (Type I)	0.02	0.02	0.02	0.02	0.00	0.69	0.02	0.02	0.00	0.12	0.00	1.01	0.00	3.54***
- ICV (Type II)	0.03	0.03	0.03	0.03	0.00	0.32	0.03	0.03	0.00	-0.55	0.00	1.07	0.00	1.86*
- ICV (Type III)	0.26	0.25	0.26	0.24	0.02	1.21	0.26	0.25	0.01	0.69	0.00	0.47	-0.01	1.55
- ICV (Type IV)	0.08	0.08	0.08	0.08	0.00	-0.18	0.08	0.08	0.00	-0.05	0.00	0.21	0.00	-0.14
- ICV (Type V)	0.53	0.56	0.54	0.57	-0.03	-1.83*	0.53	0.55	-0.02	-1.27	0.01	1.35	0.01	2.08**
- ICV (Type VI)	0.08	0.07	0.08	0.07	0.01	1.50	0.08	0.07	0.02	1.64	0.00	0.45	0.00	-0.47
Urban areas	0.89	0.89	0.90	0.90	0.00	-0.25	0.89	0.89	0.00	-0.47	0.01	3.94***	0.01	1.91*
Live in Santo Domingo	0.24	0.24	0.24	0.24	0.00	-0.25	0.24	0.24	0.00	0.05	0.00	-0.43	0.01	-1.06
Recieves remittances	0.11	0.11	0.10	0.10	0.00	0.10	0.11	0.10	0.00	0.41	0.00	0.62	0.00	0.45
Observations	5,914	4,395	3,250	1,750			2,629	1,407						

Source: PJE baseline and follow-up data.

[a]: Stands for *Indice de Calidad de Vida* (Living Quality Index). It is divided into 6 levels from the poorest to the richest.

Note: Means, differences and t-statistics are calculated by linear regression with robust standard errors. ***: significant at 1%; **: significant at 5%; *: significant at 10%.

Appendix 2

Table A2: ITT estimation on Employment.

Intention to Treat Effect - ITT Variables	(1)	(2)[a]	(3)	(4)	(5)	(6)
Z_i	0.0069	0.0056	0.0073	0.0252	0.1086	0.0194
	(0.0177)	(0.0184)	(0.0170)	(0.0227)	(0.1076)	(0.0373)
Z_i * Male = 1				-0.0471		
				(0.0317)		
Z_i * Age					-0.0046	
					(0.0049)	
Z_i * Complete elementary						-0.0056
						(0.0766)
Z_i * Incomplete high school						-0.0244
						(0.0418)
Z_i * Incomplete elementary						0.0254
						(0.0978)
Z_i * More than high school						-0.2791*
						(0.1496)
Z_i * Missing education						0.0726
						(0.0797)
Z^i * No education data						-0.0071
						(0.0607)
Age			0.0067***	0.0067***	0.0097**	0.0066***
			(0.0023)	(0.0023)	(0.0038)	(0.0023)
Male = 1			0.2357***	0.2662***	0.2356***	0.2356***
			(0.0158)	(0.0262)	(0.0158)	(0.0158)
Incomplete elementary			0.0085	0.0098	0.0078	0.0119
			(0.0382)	(0.0382)	(0.0382)	(0.0632)
Incomplete high school			0.0156	0.0160	0.0150	0.0313
			(0.0205)	(0.0204)	(0.0205)	(0.0362)
Complete high school			0.0111	0.0112	0.0113	-0.0057
			(0.0466)	(0.0466)	(0.0468)	(0.0873)
More than high school			0.0723	0.0703	0.0696	0.2984***
			(0.1046)	(0.1049)	(0.1043)	(0.0844)
Missing education			0.1331***	0.1333***	0.1321***	0.0848
			(0.0430)	(0.0429)	(0.0428)	(0.0702)
No education data			-0.0218	-0.0215	-0.0219	-0.0174
			(0.0291)	(0.0291)	(0.0291)	(0.0490)
Constant	0.5899***	0.5950***	0.3580***	0.3463***	0.2928***	0.3523***
	(0.0152)	(0.0159)	(0.0595)	(0.0597)	(0.0865)	(0.0647)
Observations	4,036	3,583	4,010	4,010	4,010	4,010
R-squared	0.0000	0.0000	0.0563	0.0568	0.0566	0.0569
Mean dep. var for control group	0.590	0.590	0.590	0.590	0.590	0.590

Source: PJE baseline and follow-up data.

Note: Clustered standard errors at course level in parenthesis. ***: significant at 1%; **: significant at 5%; *: significant at 10%. a: estimations for those whose information on education is answered.