# Experimental evidence on the long term impacts
# of a youth training program[1]

Pablo Ibarrarán*

Jochen Kluve**

Laura Ripani***

David Rosas Shady***

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# Abstract

Identifying the right human capital investments for disadvantaged young adults is a key policy concern worldwide. While many studies on the (very) short-term impacts of youth skills training programs exist, there is almost no rigorous evidence on the long-run effects of these policies outside the US. This paper presents the results of a large-scale randomized controlled trial of a youth training program, estimating treatment effects six years after random assignment, including long-term labor market trajectories of young people. We are able to track a representative sample of more than 3,200 youths at the six-year follow-up. The intervention is prototypical of many skills training programs worldwide, and has been implemented at scale in the Dominican Republic for more than a decade. Our empirical findings indicate mixed results: on the one hand, we document significant impacts on the formality of employment, particularly for men, and impacts for both men and women in Santo Domingo, the capital. The long-term analysis shows that these impacts are sustained and growing over time. On the other hand, there are no significant impacts on average employment; which appears consistent with the low unemployment in countries with high informality and no unemployment insurance. Looking at the local labor market context, the analysis suggests that skills training programs work better in more dynamic local contexts, where there is actual demand for the skills provided.

Pablo Ibarrarán
Laura Ripani
David Rosas Shady
Inter-American Development Bank
1300 New York Avenue
Washington, DC, 20577
pibarrraran@iadb.org
laurari@iadb.org
davidro@iadb.org

Jochen Kluve
Humboldt-Universität zu Berlin and RWI
School of Business and Economics
Spandauer Str.1
10178 Berlin, Germany
jochen.kluve@hu-berlin.de

1. <u>Introduction</u>

Investments in human capital are decisive for the economic success of individuals in the labor market. In developed and developing countries alike, however, there are disadvantaged groups in need of support to upgrade, modernize or simply build their skills. The fact of being disadvantaged can arise for several reasons, such as lack of access to education, or dropping-out of education; a history or background of poverty; and/or being unemployed and looking for a job. Job training programs of various types have been implemented in many countries to address issues of skills shortages, to help jobseekers find jobs, and to increase individual productivity and labor earnings.

A large number of program evaluation studies over the last decades have addressed the effectiveness of these training programs in terms of increasing participant earnings and employment probability (for systematic overviews of the evidence see Heckman et al. 1999, Greenberg et al. 2003, Betcherman et al. 2004, Kluve 2010, Card et al. 2010, 2015). A key result emerging from the overall evidence is that the timing of measuring program impacts matters: short-term effects during the first year post-program are often small or even negative, reflecting the "lock-in" nature of a skills-enhancing treatment. With increasing time between the end of the program and measurement of the effects, the size of the effects and their positive significance generally tends to increase. Card et al. (2010, 2015) e.g. show that medium- and long-run estimates of training impacts are more likely to be positive and significant than the shorter term estimates. One caveat of this finding is, however, that the number of available long-run estimates is small, and that the "long-run" is typically defined only as impacts measured more than 24 months after the program. Thus, the importance of the pattern notwithstanding, this type of evidence also implies that our knowledge on long-term impacts remains quite limited.

In fact, only very few studies of long-term impacts of training programs exist, such that there is little knowledge regarding the question whether the human capital investment contained in these – typically post-secondary – training programs has any significant returns over an extended time horizon. Our paper contributes to filling this knowledge gap by investigating the long-term effects of a youth training program using a Randomized Controlled Trial (RCT). The empirical analysis is based on experimental data tracing 3,279 young people six years after random assignment. Our study is

implemented in the Dominican Republic and covers a cohort of the training program *Juventud y Empleo* (i.e. "youth and employment"). A previous cohort of the previous version of the program served to evaluate the short-term impacts also using an RCT (Card et al. 2011), and short-term results for the training cohort serving for our long-term experiment are presented in Ibarrarán et al. (2014). Along with a parallel study by Attanasio et al. (2015) for Colombia, our paper is one of the very few experimental long-term impact evaluations of a youth training program outside the US, where a series of large-scale experimental evaluations investigated the impacts of the National Supported Work Demonstration in the 1970s, JOBSTART in the 1980s, and Job Corps in the 1990s. The *Juventud y Empleo* program is interesting and relevant to study, because it is prototypical of many youth training programs worldwide, and it has been implemented at scale for more than a decade.

As our review of the literature shows (section 2), the evidence on long-term impacts of job training programs is quite limited and inconclusive, despite the policy relevance of these programs. On the one hand, a series of studies for Germany uses non-experimental identification strategies on specific cohorts of registered unemployed and generally finds that training programs – at least the more intensive ones – have positive long-term labor market outcomes. These programs are not targeting specific age groups among the unemployed. On the other hand, the large-scale experimental studies in the US look specifically at youths and generally find that these programs at best have very small long-term earnings impacts. This led some authors at the time to conclude that despite the efforts that went into the experimental evaluations there is no known way to make training programs for disadvantaged youths work (Bloom et al. 1997).

Our paper contributes to this debate by producing new evidence on the long-run impacts of a job training program. The intervention itself is less intensive than many training programs in developed countries, but constitutes a sizeable human capital input combining classroom training and on-the-job training for disadvantaged youths. Our study has several key advantages: (i) we can rely on experimental data, (ii) we succeed in tracing a representative and comparatively large sample at the 6-year follow-up, (iii) together with Attanasio et al. (2015) it is the first such study outside the US, and since the 1990s, (iv) and we study a prototypical job training implemented at scale.

The empirical analysis shows and discusses the representativeness of our sample at the long-term follow-up and presents intention-to-treat, average treatment, and local average treatment effect estimates of program impacts. We find persistent effects on the formality of employment, in particular for men, but not on overall employment. Young individuals in Santo Domingo, the capital, also benefit significantly in terms of labor earnings. The empirical results therefore suggest that the skills investment of the program may not bring about large overall impacts, but that it does have a significant impact on the probability of being formally employed and on labor earnings in an urban labor market. Moreover, the (small) positive impacts found for subgroups are generally sustained and growing over time.

The paper is organized as follows. Section 2 reviews the literature on long-term impacts of training programs. Section 3 describes the *Juventud y Empleo* program, gives a summary of the previous findings on short-term impacts, and explains the experimental design and the long-term data collection. The empirical results are presented in section 4. Section 5 concludes.

2. Related literature

The evidence to date on the long-run effects of job training programs is quite limited and can be grouped into three types of studies.[2] The first type of studies looks at job training programs in the US; these studies investigate impacts for the set of well-known US employment training programs that were evaluated using large-scale experimental designs. A second type of studies looks at the long-run effects of training for the unemployed (youth and adult) in Germany; these studies are based on administrative data and use non-experimental identification strategies. Third, there is one parallel study by Attanasio

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[2] Note that our review of the related literature focuses specifically on long-run impact studies. There is a large number of impact evaluations of (youth) training programs in developing and developed countries that estimate short-run effects, typically during the first year after end of program participation; these studies are contained and discussed e.g. in the reviews and meta-analyses provided by Heckman et al. (1999), Greenberg et al. (2003), Ibarrarán and Rosas (2009), Card et al. (2010, 2015), and Kluve (2010).

et al. (2015) looking at long-run impacts of youth training in Colombia. We will review the three types of studies in turn.

Couch (1992) uses Social Security data to track annual earnings for the treatment and control groups in the National Supported Work (NSW) demonstration for 8 years following the end of the program. The NSW provided subsidized employment opportunities – essentially work experience and on-the-job training – to individuals "severely handicapped in the labor market" and operated experimentally at 10 sites across the US from 1975 to 1979. The study finds significant earnings impacts (in the range of USD 400 – 500 p.a.) for Aid to Families with Dependent Children (AFDC) recipients during the period 4 to 8 years after treatment, but does not find any significant earnings effects for the youth target group (sample sizes are not reported for both target groups). That is, for the entire 8-year post-program period 1979-1986 the annual earnings impact estimates for young individuals are close to zero in size and always insignificant, indicating that the program did not help this group into a better position in the labor market.

The JOBSTART demonstration was implemented between 1985 and 1988 in 13 sites and specifically aimed at providing evidence on what works for low-skilled, economically disadvantaged young people (Cave et al. 1993). The evaluation used an experimental design on the eligible population of 17- to 21-year old, economically disadvantaged school dropouts with poor reading skills. The treatment group participated in education and vocational training, and received job placement assistance; the total average duration of program activities amounted to 400 hours (with wide variation, cf. Cave et al. 1993). The evaluation uses a sample of 1,941 youths who were surveyed in the short- and medium-term (12, 24, and 48 months after random assignment). Whereas educational outcomes – i.e. passing the General Educational Development (GED) examination or completing high school – were significantly improved, labor market outcomes were not enhanced: after the expected "lock-in" effect in the first year, at the end of the survey period treatment group average earnings "appeared to overtake those of controls […, but] the magnitude of these impacts was disappointing and they were not statistically significant according to the usual tests." (Cave et al. 1993)

The Job Corps program is similar to JOBSTART but more intensive: disadvantaged youths between the ages of 16 to 24 receive academic education, vocational training, and other services (e.g. counseling and life skills training) during an average period of 8 months (again varying widely) in a residential setting (Schochet et al. 2008). Placement services are also provided. The large-scale experimental study uses a sample of 11,131 eligible youths that applied to the program in the mid-1990s. Survey data were collected at baseline and at 12-, 30-, and 48-month follow-up interviews. The results indicate negative earnings impacts during the first 5 quarters after random assignment (the "lock-in" phase), before treatment group youths catch up with the control group and display significantly higher earnings during the 3rd and 4th year after random assignment.[3] This significant medium-term impact found in the survey data disappears, however, when looking at the long-term impacts (years 5 to 8 after random assignment) on earnings and employment using annual Social Security records (Schochet et al. 2008). Hence, despite the intensity of the Job Corps intervention, the long-term impacts appear to be small. This finding, however, has to be interpreted against the counterfactual of the evaluation, which is given by a randomized-out control group of youths who to a large extent were offered and took part in alternative training programs (ibid.).

Hotz et al. (2006) analyze the impacts of California's Greater Avenues to Independence program (GAIN) for up to 9 years after random assignment. A key part of the study is an assessment of the relative effectiveness of alternative strategies for designing welfare-to-work programs: one approach uses human capital development through education and vocational training, and a second approach focuses on labor force attachment through job search assistance. Different from the above studies that assess long-term impacts for disadvantaged youths, GAIN targeted adults on welfare, and contained a mandatory component (see Hotz et al. 2006 for details). The analysis finds that in the long run the human capital development approach yields higher employment rates for participants than the labor force attachment approach. Overall, however, the long-run experimental impact estimates (7-9 years) on employment rates are statistically significant from zero only for one of the four counties analyzed.

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[3] This is not the case for all youths. Flores-Lagunes et al. (2010) find that Hispanic youth did not experience earnings gains like whites and blacks – despite similar increases in human capital – and show that this relates to the different (higher) levels of local labor market unemployment rates they face.

This evidence on long-run impacts of training programs in the US is complemented by three studies for Germany using administrative data based on specific cohorts each: inflows into unemployment during 1993 and 1994 (Fitzenberger and Völter 2007), inflows into training for the unemployed from January 1992 to June 1993 (Lechner et al. 2011), and inflows into unemployment within the period July 1999 to December 2001 (Osikominu 2012). Given the non-experimental nature of the data, all three studies use econometrically involved variants of identifying treatment effects under unconfoundedness. The results are generally encouraging: Fitzenberger and Völter (2007) find significantly positive long-run impacts – up to 7.5 years after start of the program – on participants' employment probability for a comprehensive classroom training program. Lechner et al. (2011) find significantly positive employment impacts for up to 8 years after the end of the program for the more comprehensive training programs. Osikominu (2012) considers impacts for up to 5 years after registering as unemployed, and finds the longer programs to be effective in creating stable employment spells and higher earnings.

Finally, the parallel study by Attanasio et al. (2015) looks at the *Jovenes en Accion* ("youth in action") program in Colombia. The authors link the data from the initial experimental evaluation (Attanasio et al. 2011) to administrative long-term follow-up data. The results indicate that the program had a positive and significant effect on the probability to work in the formal sector, and the treatment group contributed more months to social security during the analyzed period. The effects are more pronounced for women.

3.  The *Juventud y Empleo* Program: random assignment and data collection

Youth labor market insertion represents a challenge for the majority of Latin American and Caribbean (LAC) countries. According to ILO (2014), one out of five young individuals are neither working nor studying, and among those who are employed more than half are in the informal sector. To address this situation, LAC governments have a long tradition of implementing programs that offer short-term job training services to youths living in urban areas. These programs are supposed to offer training that responds to the skills needs of the productive sector (Ibarraran and Rosas 2009, Gonzalez et al. 2012). The interventions combine technical skills training (of approximately three months) in lower-skilled

professions with a subsequent internship period to provide on-the-job work experience (of around three months). The majority of the programs also comprise a short component of soft skills training.

There is some evidence about the short-term impacts of these training interventions. In general, the existing impact evaluations find zero or modest impacts on overall employment, but positive impacts on job quality (formal employment) and earnings. Also, the evidence suggests large heterogeneity in results by gender (Ibarrarán and Rosas 2009, Urzúa and Puentes 2010, Gonzalez et al. 2012). These results differ from those for similar programs in developed countries, where, first, youth training programs rarely show any positive impacts at all, and second, impact estimates generally do not differ by gender (Card et al. 2010).

Since 2001, the Dominican Republic has been implementing one of the previously described programs, which is named *Juventud y Empleo* ("youth and employment"). The Juventud y Empleo program has been rigorously evaluated in the past, because it considered an experimental design since its inception (Card et al. 2011). This emphasis on rigorous impact evaluation is striking in LAC, since few randomized controlled trials to evaluate social policies or labor programs exist. Also, the program has been characterized by using the findings from earlier evaluations to introduce improvements in its conceptual and operative design.

Juventud y Empleo targets youths between 16 to 29 years of age that are living in poor neighborhoods and that are not attending school. Other targeting criteria are that they should have, at most, incomplete high school education; and they should be unemployed, under-employed or occupationally inactive at the moment of the registration in the program; and hold a Dominican identity card. The program offers skills training courses that last 225 hours: 150 hours devoted to teach a wide range of low-skill qualifications, such as administrative assistant, hair stylist, or mechanic; and 75 hours devoted to improve the soft skills of participants (mainly, work habits and self-esteem). Courses are followed by a three-month period internship in a private firm. Both the registration of beneficiaries and the identification of firms are the responsibility of private training institutions (*Centros Operativos del Sistema*, COS) that have been previously approved by the national training institution (INFOTEP for its acronym in Spanish). Participants receive a monetary stipend of around US$3 per day from the

government during both phases of the program. They also receive an insurance against workplace accidents.

## 3.1 Previous evaluations

Juventud y Empleo is the first labor training program with an experimental impact evaluation in LAC. Card et al. (2011) estimate program effects using a sample of youth that applied to the program in 2004. Follow-up data were collected in 2005, approximately 10-14 months after trainees had finished the program. No impacts on employment but a modest positive impact on wages and formality for men were found. The evaluation had a relatively small sample and a few other limitations. In particular, compliance was imperfect: 17.4 per cent of youths randomly assigned to training did not show up for the course. Moreover, the no-show behavior was selective and the study did not collect follow-up data for the no-show group. In addition, 36.7 per cent of youths originally assigned to the control group were re-assigned to the treatment group to replace the no-shows. Whereas, clearly, such a replacement procedure can be implemented rigorously by randomly assigning control group units as replacements – as we do in the current study (see section 3.2) – in that first evaluation the training institutions were allowed to choose the replacements, and they did so in a selective manner (Card et al. 2011).

A second evaluation of the program was performed by Ibarrarán et al. (2014), again focusing on short-term effects of the program, but using a later cohort of participants. Specifically, the cohort under study in the second evaluation comprises youths who registered in the COS training centers in 2008, and follow-up data were collected between November 2010 and February 2011, some 18-24 months after participants had finished their course. In light of the experiences made in the first evaluation, the study is based on an improved procedure of random assignment and data collection that also forms the basis of our analysis (section 3.2).

The results of the evaluation are mixed; as in the first study, program impacts on overall employment are negligible but there is an impact on job quality for men. Specifically, the impact on the probability of formal employment for males is a 17 per cent increase. Moreover, the study finds a positive impact of 7 per cent on monthly earnings, conditional on being employed. Both of these impacts are stronger

in Santo Domingo, the capital. Looking at secondary outcomes, the study finds positive impacts on participants' perceptions and expectations about the future, particularly for women. The program also has an impact on the development of soft skills, mainly in the following dimensions: leadership skills, persistency of effort, and conflict resolution (Ibarrarán et al. 2014).

Despite the fact that the second evaluation managed to solve the methodological problems of the first impact study, estimates of the short-term effects only provide limited information on the program's effectiveness. First, it is imperative to investigate whether the overall relatively modest effects as well as the stronger effects for some subgroups are sustained in the long run. Second, this is particularly interesting against the background of the generally disappointing results on long-term impacts for youth training programs in developed countries (recall section 2). And third, it is important to assess long-term impacts specifically in LAC, since the series of youth training programs in the region has been generally perceived as a success, even though this conclusion is derived from short-term impact estimates only.

## 3.2 Random assignment and survey implementation

The evaluation design of the Juventud y Empleo program is strongly linked to its targeting method. In the first stage, for each training course they offer, the COS training centers identify 35 young people that meet the eligibility criteria described above. In the second stage, the Juventud y Empleo Program Coordination Unit (PCU; *Unidad Coordinadora de Programas*) receives the information about the youths that registered for the course from the COS training centers and proceeds to verify that none of the applicants has registered before. In the third step, the program runs a lottery in which each of the 35 youths is randomly assigned to one of two groups. The first one is formed by 20 youths who are invited to attend the training course and the second one by 15 youths who are assigned to the control group; their identification numbers are locked in order to guarantee that they will not be registered again in the case of any other attempt.

If any of the 20 youths assigned to the treatment group gives no response when called to attend the course or if they drop out before the tenth day of classes, the COS may replace up to 5 slots with

youths from the control group. This group of five people is again randomly selected out of the 15 in the control group by the PCU, who provides the names of the up to five replacements directly to the COS.[4] Hence, given this procedure and the initial configuration of random assignment to treatment and control groups, we can define four groups in the data: (A) the *beneficiaries*: those who were randomly assigned to the treatment group and actually attend and complete the course, (B) the *no-shows*: those who were randomly assigned to the treatment group but fail to show up or fail to complete at least two weeks of the course, (C) the *replacements*: those who were randomly assigned to the control group and then randomly chosen to replace no-shows in the training course, (D) the *controls*: those who were randomly assigned to the control group.

The data for the long-term impact evaluation were collected in three waves, one baseline survey at random assignment in 2008 and two follow-up surveys: the short-term follow-up survey between November 2010 and February 2011 (18 to 24 months after graduating from the program), and the long-term follow-up survey between September and December 2014 (six years after the treatment). Both follow-up surveys were administered using comprehensive face-to-face interviews (the full questionnaires are available upon request). It has to be emphasized that substantial effort went into tracking youths at the long-term follow-up, using all resources available (family, friends, neighbors) to establish contact and make possible the interview.[5]

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[4] This procedure solves a problem that is not uncommon in randomized trials of training or similar programs: typically some share of the units assigned to the treatment group may not show up or drop out very early, while at the same time strong incentives may exist to fill all available slots (e.g. if the program implementing institution is paid to serve a certain number of people). It is then crucial to avoid a non-random process of filling-up the free slots with control group units, e.g. as happened in the first Juventud y Empleo study, when the training institutions themselves selected the replacements, biasing both treatment and control groups (Card et al. 2011). A related approach is the "waitlist" procedure used e.g. in Hirshleifer et al. (2014), in which eligible units are assigned to treatment group, control group or waitlist, and implementing institutions can replace no-shows choosing units from the waitlist. While the waitlist units are excluded from the analysis (as their selection into training is non-random), the advantage of the procedure is that it maintains a valid control group. Our procedure has the additional advantage that also the replacements can be included in the analysis, since they are randomly drawn from the control group – this was feasible because, if replacements were needed, it could be put into practice very quickly at the COS training course level (i.e. the cell of 35 eligible youths).

[5] Data collection proceeded using several incentives and instruments for quality assurance. For instance, external advisors were hired to supervise the field experiment, and the data processing system was implemented using the Computer Assisted Field Editing (CAFÉ) methodology. A system of double entry was used in 20 per cent of the cases to ensure that the CAFÉ methodology was working as expected. Finally, monetary incentives of RD$600 (around 15 dollars) per respondent were offered in order to minimize attrition.

At baseline in the year 2008, 10,309 eligible applicants were randomly assigned to 5,914 treated and 4,395 control units. Following the (expected) occurrence of no-shows and the replacement procedure delineated above, the eventual distribution of the entire baseline sample into the four groups was as follows: (A) 4,937 beneficiaries, (B) 977 no-shows, (C) 977 replacements, (D) 3,418 controls. The short-term follow-up targeted a random sample of 5,000 out of the 10,309 young people who had initially registered.[6] This target sample had 3,250 individuals from the treatment group and 1,750 from the control group. From this sample, 4,033 individuals were found and interviewed, 2,626 of the treatment group and 1,407 of the control group (Ibarrarán et al. 2014). These formed the target sample for our long-term follow-up in 2014: From the 4,033 individuals interviewed in 2010, 3,279 were found and have complete surveys: 2,163 individuals in the treatment group and 1,116 in the control group (as initially assigned). In sum, in both follow-up surveys about 80 percent of the sample were located at their households, and this percentage was balanced between treatment and control groups.

If $Z_i$ represents the random assignment of each youth $i$ ($Z_i = 1$ assigned to the treatment group and $Z_i = 0$ assigned to the control group) and $D_i$ the final treatment status ($D_i = 1$ attended the course and $D_i = 0$ do not), Table 1 shows the distribution of the long-term follow-up between the four groups in 2014.

**Table 1. Participants by lottery assignment and treatment status, long-term follow-up sample**

|  | Selected in the Lottery, $Z_i$=1 | Not selected in the Lottery, $Z_i$=0 |
|---|---|---|
| Participated in the program, $D_i$=1 | Group A: Beneficiaries, N=1,901 | Group C: Replacements, N=438 |
| Did not participate in the program, $D_i$=0 | Group B: No-shows, N=262 | Group D: Controls, N=678 |

Source: 2014 long-term follow-up survey.

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[6] The sample size was set at 5,000 to detect an 8 percent increase in income with a power of 0.8 and an attrition of 30 percent of the sample.

## 3.3 Identification strategy and data

Given the randomized experiment described above, the first step in our analysis is to estimate the causal effect of $Z_i$ on labor market outcomes using an Intention to Treat (ITT) analysis, i.e. a linear regression of the outcome on $Z_i$. This approach uses the full sample of treatment (A and B) and control group units (C and D).[7] Since youths in the replacements group were eventually randomly selected to take part in the course, we also estimate Average Treatment on the Treated (ATT) effects that compare the combination of the groups A and C – both of which effectively received the treatment – with the group D as control group (i.e. the "pure" randomized-out control units). In both cases, the regressions include fixed effects for training institutions COS, and robust standard errors are computed using clusters defined by the course within which randomization took place. Finally, we also estimate the Local Average Treatment Effects (LATE), in which participation is instrumented by randomization status.

Ibarrarán et al. (2014) show that the complete cohort (N=10,305)[8] as well as both the target sample (N=5,000) and the realized sample (N=4,033) at short-term follow-up are balanced in baseline characteristics of the treatment and control groups. Table 2 validates the long-term follow-up and shows that, at baseline, there are no significant differences between the long-term follow-up sample (N=3,279) and the rest of the sample (N=7,026). As shown, the characteristics of the sample that we track in 2014 are statistically equivalent to the rest of the 2008 training cohort. The same holds if we focus on differences at baseline between the 2014 sample and the rest of the sample within the treatment and control groups.[9]

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[7] An alternative definition of the ITT in this set-up would be to compare A, B, and C together against D. Estimation results for this ITT are reported in Appendix Table A1.

[8] The complete cohort of 10,305 individuals comes from the original randomization group of 10,309, taking out four individuals that did not have identification numbers.

[9] The tables showing the descriptive statistics within treatment and control groups at baseline for the 2014 sample and the rest are available upon request. They find the same results in terms of balancing as the overall sample shown in table 2. As a robustness check, and additional validation of the long-term follow-up sample, we re-estimated the regressions for the short-term analysis in Ibarrarán et al. (2014) restricting the sample to the 3,279 observations with complete long-term data in 2014, and the results remain essentially unchanged.

After having shown that the long-term follow-up sample is representative of the complete cohort, we proceed to show that, within the 2014 sample, there is balance in the characteristics of treated and control youths (as defined by the lottery) as well as between participants and non-participants. That is, using the definition of the four groups presented above, Table 3 shows the mean characteristics at baseline for each of the groups defined by the assignment/participation matrix (Table 1), as well as for the groups defined by the lottery (AB vs CD) and by participation (AC vs D). The results show that balance is maintained in the 2014 sample. In the first comparison (based on the results of the lottery) there is only one unbalanced variable out of 25, and in the second comparison (based on participation) there are only two unbalanced variables (but only at the 10% significance level). In sum, these tables show that the long-term follow-up data are representative of the whole cohort, and that the data are balanced between the four subgroups defined by lottery assignment and participation, essentially also validating the replacement procedure.

**Table 2. Representativeness of the long-term follow-up sample, characteristics at baseline**

| | Mean | | |
| --- | --- | --- | --- |
| | Sample 2014=0 | Sample 2014=1 | P-value |
| Age | 21.54 | 21.51 | 0.61 |
| Age 16-19 | 0.35 | 0.35 | 1.00 |
| Age 20-24 | 0.43 | 0.44 | 0.49 |
| Age >24 | 0.22 | 0.21 | 0.41 |
| % Women | 0.63 | 0.62 | 0.41 |
| % Married | 0.03 | 0.03 | 0.70 |
| Number of people in HH | 4.36 | 4.53 | 0.00 |
| Number of children | 0.70 | 0.70 | 0.88 |
| % Currently attending school | 0.22 | 0.24 | 0.03 |
| Fraction with prior work experience | 0.16 | 0.17 | 0.46 |
| Worked during last 2 years | 0.21 | 0.21 | 0.79 |
| Rosenberg (0 to 30) [a] | 23.85 | 23.95 | 0.25 |
| Urban areas | 0.90 | 0.88 | 0.04 |
| Lives in Santo Domingo | 0.25 | 0.23 | 0.02 |
| Receives remittances | 1.89 | 1.90 | 0.43 |
| Owns home | 0.06 | 0.06 | 0.20 |
| Concrete, brick or wood walls | 0.97 | 0.97 | 0.54 |
| Concrete or zinc ceilings | 1.00 | 1.00 | 0.22 |
| Cement, ceramic or wood floors | 0.98 | 0.98 | 0.94 |
| % connected to aqueduct | 0.50 | 0.49 | 0.39 |
| % Proper sanitation | 0.99 | 0.99 | 0.31 |
| % Garbage collection | 0.83 | 0.85 | 0.07 |

|  |  |  |  |
|---|---|---|---|
| % Refrigerator | 0.71 | 0.72 | 0.41 |
| % TV | 0.88 | 0.88 | 0.76 |
| % Wash Machine | 0.72 | 0.72 | 0.61 |
| % Car | 0.14 | 0.15 | 0.30 |
| % AC | 0.02 | 0.02 | 0.99 |
| % Computer | 0.09 | 0.08 | 0.59 |
| % Electricity Generator | 0.08 | 0.09 | 0.75 |
| Observations | 7,026 | 3,279 | 10,305 |

Sources: Long-term follow-up survey, baseline survey.

Notes: The "Sample 2014=1" group denotes the long-term follow-up sample available for analysis. The "Sample2014=0" group comprises the remaining observations from the full cohort of 10,305 youths that formed part of the random assignment in 2008.

[a] The Rosenberg Scale is a tool used in clinical-psychometric practice to measure self-esteem levels. It was first introduced in 1965 and revised in 1989 (see Rosenberg 1989). Higher scores on the scale indicate greater self-esteem. The balancing test for the scale is based on the 9,692 observations with available data -3,096 of them in the follow-up sample. The complete set of balancing tests for all the variables for this subsample yield the same conclusions and are available upon request.

**Table 3. Baseline characteristics of subgroups defined by lottery assignment and participation: long-term follow-up sample**

| Characteristic | Mean | | | | | | | P-value | |
|---|---|---|---|---|---|---|---|---|---|
| | A | B | C | D | AB | CD | AC | AB/CD | AC/D |
| Age | 21.99 | 22.27 | 21.79 | 22.03 | 22.03 | 21.94 | 21.96 | 0.470 | 0.604 |
| Age 16-19 | 0.19 | 0.19 | 0.22 | 0.22 | 0.19 | 0.22 | 0.20 | 0.050 | 0.170 |
| Age 20-24 | 0.53 | 0.54 | 0.54 | 0.50 | 0.53 | 0.52 | 0.53 | 0.292 | 0.116 |
| Age >24 | 0.27 | 0.27 | 0.24 | 0.28 | 0.27 | 0.26 | 0.27 | 0.555 | 0.601 |
| % Women | 0.62 | 0.65 | 0.59 | 0.63 | 0.63 | 0.61 | 0.62 | 0.511 | 0.645 |
| % Married | 0.24 | 0.24 | 0.25 | 0.23 | 0.24 | 0.23 | 0.24 | 0.603 | 0.360 |
| Number of people in HH | 4.50 | 4.49 | 4.45 | 4.64 | 4.50 | 4.56 | 4.49 | 0.341 | 0.071 |
| Number of children | 0.69 | 0.80 | 0.63 | 0.73 | 0.70 | 0.69 | 0.68 | 0.783 | 0.230 |
| Currently attending school | 0.25 | 0.21 | 0.24 | 0.24 | 0.24 | 0.24 | 0.25 | 0.898 | 0.823 |
| Incomplete elementary | 0.19 | 0.21 | 0.18 | 0.20 | 0.19 | 0.19 | 0.19 | 0.938 | 0.493 |
| Complete elementary | 0.05 | 0.04 | 0.04 | 0.06 | 0.05 | 0.05 | 0.05 | 0.874 | 0.573 |
| Incomplete high school | 0.58 | 0.56 | 0.55 | 0.58 | 0.58 | 0.57 | 0.57 | 0.612 | 0.892 |
| Complete high school | 0.04 | 0.03 | 0.04 | 0.03 | 0.03 | 0.03 | 0.04 | 0.966 | 0.295 |
| More than high school | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.00 | 0.401 | 0.235 |
| Missing education | 0.04 | 0.04 | 0.04 | 0.03 | 0.04 | 0.04 | 0.04 | 0.991 | 0.750 |
| No data on education | 0.10 | 0.12 | 0.14 | 0.10 | 0.10 | 0.12 | 0.11 | 0.202 | 0.651 |
| Prior work experience | 0.17 | 0.19 | 0.16 | 0.16 | 0.17 | 0.16 | 0.17 | 0.365 | 0.603 |
| Currently employed[a] | 0.04 | 0.04 | 0.06 | 0.04 | 0.04 | 0.05 | 0.04 | 0.224 | 0.912 |
| Currently salaried worker[a] | 0.01 | 0.01 | 0.02 | 0.02 | 0.01 | 0.02 | 0.01 | 0.225 | 0.241 |
| Currently unemployed[a] | 0.52 | 0.55 | 0.50 | 0.51 | 0.53 | 0.51 | 0.52 | 0.417 | 0.810 |
| ICV Score (0 to 100)[b] | 62.66 | 62.70 | 63.25 | 62.29 | 62.66 | 62.66 | 62.77 | 0.993 | 0.288 |
| Rosenberg (0 to 30)[c] | 24.00 | 23.89 | 24.09 | 23.73 | 23.99 | 23.87 | 24.02 | 0.385 | 0.075 |
| Urban areas | 0.89 | 0.87 | 0.86 | 0.90 | 0.88 | 0.88 | 0.88 | 0.944 | 0.117 |
| Lives in Santo Domingo | 0.31 | 0.30 | 0.28 | 0.32 | 0.31 | 0.31 | 0.31 | 0.662 | 0.603 |
| Receives remittances | 0.11 | 0.08 | 0.10 | 0.11 | 0.10 | 0.11 | 0.10 | 0.806 | 0.551 |
| Observations | 1,901 | 262 | 438 | 678 | 2163 | 1,116 | 2,339 | | |

Sources: Long-term follow-up survey.

Notes: Groups as defined by lottery assignment and participation: A = beneficiaries, B = no-shows, C = replacements, D = controls. See also section 3.2 and Table 1. AB = treatment group according to random assignment, CD = control group according to random assignment; these groups are used to identify ITT program effects. AC = all youths that actually received the treatment, AC vs. D used to identify ATT program effects.

[a] Tests for current job status were run for the available observations: 1,708 from group A, 230 group B, 375 group C, 606 group D, 1,938 AB, 981 CD, 2,083 AC.

[b] The test for ICV scores was run for the available observations: 1,592 from group A, 221 group B, 364 group C, 574 group D, 1,813 AB, 938 CD, 1,956 AC.

[c] The test for Rosenberg scores was run for available observations: 1,795 from group A, 251 group B, 409 group C, 641 group D, 2,046 AB, 1,050 CD, 2,204 AC.

Balancing tests for all variables with the same sub-sample yield the same conclusions and are available upon request..

Table 4 displays sample characteristics and raw outcomes at the time of the long-term follow-up in 2014. Again, we group the data based on the subgroups defined above, to illustrate the summary statistics in relation to the two alternative identification strategies: the first based on the results from the lottery (AB vs CD, the ITT), and the second considering actual participation in the program (AC vs D, the ATT). Table 4 shows that the age at the time of the long-term follow-up is 28 years on average, so the group is at the upper end of the youth age range (if we consider youths as individuals between 15 and 29 years of age), entirely in line with the timing of the survey six years after random assignment.

Half of the sample is married (in contrast with 20 per cent at baseline), and about 32 per cent are heads of household. Looking at gender separately, the respective probabilities for men and women to be married at the long-term follow-up are 0.41 and 0.56, and to be head of household the probabilities are 0.42 and 0.25. In terms of demographics, the only statistically significant difference is in marital status, due to a higher share of males being married in the treatment group: those males assigned to participate in the program (AB) and those that completed the program (AC) are more likely to be married in 2014 than the control group. A plausible mechanism for this result lies in the positive impacts on quality of employment (explored below) that may in turn impact positively the probability of getting married.

Looking at raw outcomes, overall there is little indication of statistically significant differences in the comparison of the groups assigned to treatment vs. control, and actual participation vs. control. The average employment rate is 73 per cent, with no statistically significant differences across those randomly assigned to training (AB vs CD) or across treatment participants and controls (AC vs D). The employment rate is higher for men than for women, and it is overall substantially higher than at the short-term follow-up in 2010 (when it was around 62 per cent). In part this can be explained already by an upward sloping employment-age profile that would be expected in general; and specifically against the background of high levels of informality in the Dominican labor market, generating pressure on youths to look for work. We will explore these employment patterns further when we look at the long-term labor market trajectories of the treatment and control groups in section 4.2.

Regarding employment characteristics, almost 90 per cent of young individuals work in services, and the average tenure on the job is about 20 months (with a slightly longer tenure for those selected in the

lottery). About 56 per cent have a permanent job (but only 21 per cent have a written contract), 44 per cent are salaried workers and 22 per cent work at large firms. About half of the workers express their desire to change their current job, but only 19 per cent were seeking for another job at the time of the survey. Average monthly labor income (calculated with zero earnings for those not working) is RD$5,300, the equivalent of USD$120.

**Table 4. Sample characteristics and raw outcomes at long-term follow-up**

| Characteristic/Outcome | Mean | | | | P-value | |
|---|---|---|---|---|---|---|
| | AB | CD | D | AC | AB/CD | AC/D |
| Age | 27.93 | 27.94 | 28.00 | 27.89 | 0.94 | 0.48 |
| % Women | 0.62 | 0.63 | 0.63 | 0.62 | 0.48 | 0.37 |
| % Head of Household (all) | 0.34 | 0.30 | 0.35 | 0.31 | 0.04 | 0.03 |
| % Head of Household (women) | 0.26 | 0.25 | 0.28 | 0.25 | 0.52 | 0.53 |
| % Head of Household (men) | 0.46 | 0.40 | 0.50 | 0.40 | 0.03 | 0.01 |
| % Married | 0.50 | 0.50 | 0.49 | 0.50 | 0.97 | 0.40 |
| % Currently attending school | 0.20 | 0.22 | 0.20 | 0.22 | 0.25 | 0.34 |
| Years of education | 11.48 | 11.53 | 11.29 | 11.60 | 0.66 | 0.01 |
| Employed (women) | 0.66 | 0.65 | 0.64 | 0.65 | 0.41 | 0.61 |
| Employed (men) | 0.86 | 0.86 | 0.87 | 0.85 | 0.80 | 0.52 |
| Employed (all) | 0.74 | 0.72 | 0.72 | 0.73 | 0.34 | 0.82 |
| Agriculture and mining | 0.02 | 0.02 | 0.01 | 0.02 | 0.57 | 0.45 |
| Industry | 0.10 | 0.08 | 0.11 | 0.08 | 0.05 | 0.01 |
| Services | 0.88 | 0.90 | 0.87 | 0.90 | 0.04 | 0.03 |
| Duration of current job (months) | 21.03 | 18.67 | 20.52 | 19.26 | 0.03 | 0.34 |
| Permanent job | 0.57 | 0.56 | 0.55 | 0.57 | 0.65 | 0.28 |
| Employed at large firms | 0.21 | 0.22 | 0.22 | 0.22 | 0.39 | 0.87 |
| Salaried workers | 0.44 | 0.44 | 0.42 | 0.45 | 0.75 | 0.25 |
| Unpaid workers | 0.01 | 0.01 | 0.01 | 0.01 | 0.40 | 0.88 |
| Self-employed | 0.22 | 0.22 | 0.22 | 0.22 | 0.77 | 0.87 |
| Workers w/labor risk insurance | 0.11 | 0.13 | 0.11 | 0.13 | 0.11 | 0.07 |
| Workers w/ health insurance | 0.25 | 0.28 | 0.24 | 0.28 | 0.12 | 0.02 |
| Workers w/written contract | 0.20 | 0.21 | 0.19 | 0.22 | 0.34 | 0.13 |
| Weekly worked days | 5.72 | 5.75 | 5.79 | 5.72 | 0.59 | 0.34 |
| Weekly worked hours | 29.31 | 28.42 | 28.85 | 28.85 | 0.35 | 1.00 |
| Wants to work more hours | 0.47 | 0.47 | 0.47 | 0.47 | 0.70 | 0.83 |
| Workers seeking another job | 0.21 | 0.19 | 0.20 | 0.19 | 0.21 | 0.79 |
| Monthly wage (Dominican peso) | 5357.84 | 5285.06 | 4999.80 | 5417.94 | 0.75 | 0.12 |
| Hourly wage (Dominican peso) | 37.44 | 39.78 | 34.57 | 40.16 | 0.46 | 0.14 |
| Observations | 2,163 | 1,116 | 678 | 2,339 | 3,279 | 3,017 |

Source: 2014 long-term follow-up survey.
Notes: Outcomes are not conditional on employment status. Groups as defined by lottery assignment and participation: A = beneficiaries, B = no-shows, C = replacements, D = controls. See also section 3.2 and Table 1. AB = treatment group according to random assignment, CD = control group according to random assignment; these groups are used to identify ITT program effects. AC = all youths that actually received the treatment, AC vs. D used to identify ATT program effects.
a: Large firms are those that employ 51 or more employees -- b: One Dominican Peso = 0.0228 USD (November 2014)

## 4    Empirical results

### 4.1 Long-term impacts

Tables 5 through 7 present the long-term impact estimates of the Juventud y Empleo job training program on labor market outcomes, using the experimental design described in the previous section. It is important to recall that the sample is representative for the young people that this program serves, and that the large sample size at the six-year long-term follow-up and the virtue of random assignment allows to provide precise estimates of the intervention effects. It is also worth noting that Juventud y Empleo is not a pilot program, but has been running for more than a decade as a publicly funded active labor market program in the Dominican Republic.

The results tables 5 and 6 (displaying ITT and ATT estimates, respectively) show several patterns. First, the overall average impacts on employment and earnings in the long-run remain close to zero in size and insignificant. Second, there is heterogeneity in the impact estimates, indicating, in particular, significant treatment-control differences for several stratifications of the sample population by socio-demographic characteristics. One key result is that there is a positive long-term impact of the program on the quality of employment for men, as measured by the job characteristic "employed with health insurance" (ITT in Table 5: 8 percentage points, i.e. an impact of 25 per cent).[10] This finding implies a sustained positive impact on formality that is consistent with and continues what was found in the short-run evaluation: in the first follow-up the impact was significant and of smaller magnitude (17 per cent).

The long-term impact is largest in Santo Domingo, where there is also an important impact for women. The overall point estimate of 7.3 percentage points in Santo Domingo, representing an impact of 31 per cent, is higher in the case of men (11.8 percentage points) than for women (6.3 percentage points), though in relation to the mean outcomes of controls (19.1 for females, 33.3 for males) the per cent increase is similar. In Santo Domingo, there is also a positive effect on the probability of having a

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[10] Having a job with health insurance or written contract are used as proxies of formality. Given the high informality rates in the Dominican Republic, having a formal job makes a sizeable difference in the career path of young people.

**Table 5. Long-term impacts of the "Juventud y Empleo" training program on labor market outcomes: ITT**

| Outcome | (1) All | (2) Women | (3) Men | (4) Santo Domingo | (5) Santo Domingo women | (6) Santo Domingo Men | (7) Age 16-21 | (8) Age >21 |
|---|---|---|---|---|---|---|---|---|
| *Employed* | -0.0143 | -0.0162 | 0.0028 | 0.0250 | 0.0345 | 0.0203 | -0.0035 | -0.0256 |
| | (0.0152) | (0.0222) | (0.0192) | (0.0315) | (0.0423) | (0.0453) | (0.0224) | (0.0239) |
| Mean control group | 0.740 | 0.665 | 0.860 | 0.719 | 0.658 | 0.838 | 0.739 | 0.741 |
| *Employed with health insurance* | 0.0256 | 0.0001 | 0.0802*** | 0.0732*** | 0.0625* | 0.1176* | 0.0227 | 0.0178 |
| | (0.0158) | (0.0201) | (0.0278) | (0.0276) | (0.0326) | (0.0632) | (0.0249) | (0.0228) |
| Mean control group | 0.254 | 0.220 | 0.309 | 0.240 | 0.191 | 0.333 | 0.295 | 0.210 |
| *Employed with written contract* | 0.0173 | 0.0110 | 0.0383 | 0.0441* | 0.0318 | 0.0744 | 0.0211 | 0.0033 |
| | (0.0141) | (0.0176) | (0.0259) | (0.0246) | (0.0296) | (0.0523) | (0.0219) | (0.0197) |
| Mean control group | 0.200 | 0.163 | 0.258 | 0.196 | 0.164 | 0.256 | 0.232 | 0.164 |
| *Monthly earnings* | -99.7006 | 82.1841 | -91.8111 | 299.6451 | 913.9499** | -292.7177 | 44.4220 | -312.0953 |
| | (222.9148) | (221.2317) | (477.2564) | (391.4541) | (371.6096) | (935.1086) | (311.1643) | (337.2968) |
| Mean control group | 5358 | 3772 | 7888 | 5519 | 3692 | 9031 | 5549 | 5145 |
| *Ln Monthly earnings* | 0.0166 | -0.0081 | 0.0631 | -0.0333 | 0.0234 | -0.0633 | 0.0631 | -0.0290 |
| | (0.0396) | (0.0600) | (0.0542) | (0.0817) | (0.1259) | (0.1064) | (0.0554) | (0.0616) |
| Mean control group | 8.701 | 8.498 | 8.930 | 8.784 | 8.531 | 9.111 | 8.743 | 8.654 |
| *Labor force participation* | -0.0030 | -0.0049 | 0.0060 | 0.0287 | 0.0398 | 0.0119 | 0.0041 | -0.0100 |
| | (0.0115) | (0.0182) | (0.0101) | (0.0205) | (0.0305) | (0.0173) | (0.0161) | (0.0176) |
| Mean control group | 0.883 | 0.832 | 0.963 | 0.877 | 0.831 | 0.966 | 0.879 | 0.887 |
| Observations | 3,279 | 2,041 | 1,238 | 1,020 | 677 | 343 | 1,729 | 1,549 |

Notes: Regressions use the full sample of groups A and B (treated) and C and D (controls). All specifications control for training institutions (COS) and 10 administrative regions. Standard errors in parentheses. Significance levels are indicated by *10%, **5%, and ***1%.

**Table 6. Long-term impacts of the "Juventud y Empleo" training program on labor market outcomes: ATT**

| Outcome | (1) All | (2) Women | (3) Men | (4) Santo Domingo | (5) Santo Domingo women | (6) Santo Domingo Men | (7) Age 16-21 | (8) Age >21 |
|---|---|---|---|---|---|---|---|---|
| *Employed* | 0.0033 | 0.0148 | -0.0103 | 0.0313 | 0.0575 | -0.0261 | -0.0030 | 0.0183 |
| | (0.0182) | (0.0255) | (0.0231) | (0.0364) | (0.0476) | (0.0509) | (0.0266) | (0.0279) |
| Mean control group | 0.724 | 0.641 | 0.865 | 0.705 | 0.635 | 0.855 | 0.737 | 0.711 |
| *Employed with health insurance* | 0.0437** | 0.0273 | 0.0816*** | 0.0861** | 0.0969** | 0.0639 | 0.0360 | 0.0488* |
| | (0.0194) | (0.0240) | (0.0314) | (0.0342) | (0.0398) | (0.0683) | (0.0290) | (0.0249) |
| Mean control group | 0.237 | 0.202 | 0.298 | 0.221 | 0.162 | 0.348 | 0.286 | 0.187 |
| *Employed with written contract* | 0.0260 | 0.0178 | 0.0449 | 0.0447 | 0.0407 | 0.0315 | 0.0224 | 0.0172 |
| | (0.0169) | (0.0207) | (0.0284) | (0.0292) | (0.0360) | (0.0566) | (0.0266) | (0.0226) |
| Mean control group | 0.189 | 0.155 | 0.246 | 0.189 | 0.155 | 0.261 | 0.225 | 0.151 |
| *Monthly earnings* | 387.6807 | 304.0351 | 622.0150 | 1,029.5949** | 1,059.2345** | 1,124.5911 | 557.4660 | 142.6572 |
| | (268.7737) | (276.8782) | (535.6966) | (477.5475) | (456.7620) | (1,166.3988) | (351.6033) | (389.7139) |
| Mean control group | 5000 | 3599 | 7368 | 4879 | 3443 | 7961 | 5213 | 4778 |
| *Ln Monthly earnings* | 0.0451 | 0.0056 | 0.1120* | 0.0466 | 0.1082 | 0.0426 | 0.0829 | -0.0240 |
| | (0.0516) | (0.0786) | (0.0647) | (0.1031) | (0.1612) | (0.1072) | (0.0660) | (0.0788) |
| Mean control group | 8.677 | 8.492 | 8.889 | 8.725 | 8.479 | 9.050 | 8.728 | 8.621 |
| *Labor force participation* | 0.0127 | 0.0162 | 0.0030 | 0.0384 | 0.0485 | 0.0070 | 0.0195 | 0.0115 |
| | (0.0137) | (0.0206) | (0.0131) | (0.0244) | (0.0312) | (0.0255) | (0.0206) | (0.0190) |
| Mean control group | 0.872 | 0.819 | 0.960 | 0.866 | 0.824 | 0.957 | 0.867 | 0.877 |
| Observations | 3,017 | 1,872 | 1,145 | 938 | 625 | 313 | 1,601 | 1,415 |

Notes: Regressions use the sample of groups A and C (treated) and D (controls). All specifications control for training institutions (COS) and 10 administrative regions. Standard errors in parentheses. Significance levels are indicated by *10%, **5%, and ***1%.

written contract – another measure of formality – of 23 per cent. This impact is significant at the 10 per cent level and measured for men and women together. Overall, the long-term impacts on formality are substantial and show that the program has an important effect in helping youth get and keep good jobs. Table 5 also shows that, in Santo Domingo, there is a statistically significant impact on the earnings for women, representing an increase of 25 per cent in the treatment over the control group.

The results of estimating the ATT impacts (Groups A and C vs. D) shown in Table 6 are consistent with the findings from the ITT analysis (and also borne out by the alternative definition of the ITT in Appendix Table A1): there is an important impact on formality (measured by having employer provided health insurance, in the order of 18 per cent), which is particularly strong for men (27 per cent) and in Santo Domingo (39 per cent over the control group mean). In this specification, the impact for men is not concentrated in Santo Domingo, where women do have a very large impact (9.7 percentage points or 60 per cent over the control group mean). The impact on earnings in Santo Domingo, in particular for women, is also statistically and economically meaningful, of about 30 per cent. For men the point estimate is similar, but the sample is much smaller and the percentage difference relative to the control group is also smaller (14 per cent, insignificant).

Finally, we report results from a LATE analysis in Table 7, using random assignment as instrument for participation. As expected, the coefficients are larger than in the ITT analysis shown in Table 5, by a factor of close to two (LATE coefficients are the result of dividing the ITT coefficients by the difference between participation of lottery winners – approximately 87 per cent – and the participation rate of those who did not win the lottery – about 40 per cent). Statistical significance is largely unchanged, and the interpretation is that for those that participated in the program due to the lottery, the impact on formality is substantial. In the case of men, the impact is in the order of 17 percentage points, representing an increase of 52 per cent over the mean outcome of the control group. This difference is even stronger in Santo Domingo, representing a 26 percentage point or 70 per cent increase in formality.[11]

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[11] We conducted a series of robustness checks by e.g. re-estimating the regressions including course fixed effects, and controlling for three quality levels of the COS training institutions provided by the Program Coordination Unit. The specifications essentially yield the same results and are available upon request.

**Table 7. Long-term impacts of the "Juventud y Empleo" training program on labor market outcomes: LATE estimates**

| Outcome | (1) All | (2) Women | (3) Men | (4) Santo Domingo | (5) Santo Domingo Women | (6) Santo Domingo Men | (7) Age 16-21 | (8) Age >21 |
|---|---|---|---|---|---|---|---|---|
| *Employed* | -0.0296 | -0.0325 | 0.0061 | 0.0485 | 0.0627 | 0.0449 | -0.0074 | -0.0517 |
| | (0.0311) | (0.0438) | (0.0400) | (0.0599) | (0.0747) | (0.0967) | (0.0465) | (0.0471) |
| Mean control group | 0.733 | 0.649 | 0.878 | 0.736 | 0.655 | 0.899 | 0.741 | 0.725 |
| *Employed with health insurance* | 0.0531* | 0.0002 | 0.1724*** | 0.1424*** | 0.1137** | 0.2603** | 0.0485 | 0.0361 |
| | (0.0322) | (0.0395) | (0.0588) | (0.0516) | (0.0573) | (0.1327) | (0.0518) | (0.0448) |
| Mean control group | 0.243 | 0.198 | 0.319 | 0.234 | 0.170 | 0.364 | 0.293 | 0.191 |
| *Employed with written contract* | 0.0358 | 0.0221 | 0.0824 | 0.0857* | 0.0580 | 0.1648 | 0.0449 | 0.0066 |
| | (0.0288) | (0.0346) | (0.0540) | (0.0467) | (0.0525) | (0.1117) | (0.0457) | (0.0387) |
| Mean control group | 0.193 | 0.158 | 0.252 | 0.191 | 0.145 | 0.283 | 0.238 | 0.146 |
| *Monthly earnings* | -206.3394 | 165.3628 | -197.2966 | 582.4215 | 1,663.0461** | -648.1405 | 94.6817 | -631.2885 |
| | (454.8568) | (434.8564) | (991.5577) | (740.9820) | (660.6672) | (1,988.6684) | (645.8421) | (665.6751) |
| Mean control group | 5041 | 3617 | 7496 | 5208 | 3673 | 8309 | 5261 | 4817 |
| *Ln Monthly earnings* | 0.0359 | -0.0171 | 0.1394 | -0.0694 | 0.0449 | -0.1454 | 0.1344 | -0.0638 |
| | (0.0839) | (0.1220) | (0.1153) | (0.1657) | (0.2317) | (0.2375) | (0.1142) | (0.1300) |
| Mean control group | 8.681 | 8.488 | 8.906 | 8.738 | 8.504 | 9.032 | 8.738 | 8.619 |
| *Labor force participation* | -0.0062 | -0.0099 | 0.0128 | 0.0558 | 0.0724 | 0.0263 | 0.0087 | -0.0203 |
| | (0.0234) | (0.0359) | (0.0211) | (0.0386) | (0.0533) | (0.0368) | (0.0334) | (0.0345) |
| Mean control group | 0.870 | 0.815 | 0.965 | 0.873 | 0.825 | 0.970 | 0.869 | 0.871 |
| Observations | 3,279 | 2,041 | 1,238 | 1,020 | 677 | 343 | 1,729 | 1,549 |

Notes: Regressions use the full sample. All specifications control for training institutions (COS) and 10 administrative region. Standard errors in parentheses. Significance levels are indicated by *10%, **5%, and ***1%.

Table A2 in the Appendix constructs two versions of a formality index as an outcome: "strong" formality defined as employment with both health insurance and a written contract, and "weak" formality as employment with either health insurance or a written contract. The results indicate that, while outside of Santo Domingo there is no measurable impact on "strong" formality, "weak" formality is significantly and positively influenced by the program on average and for almost all subgroups.
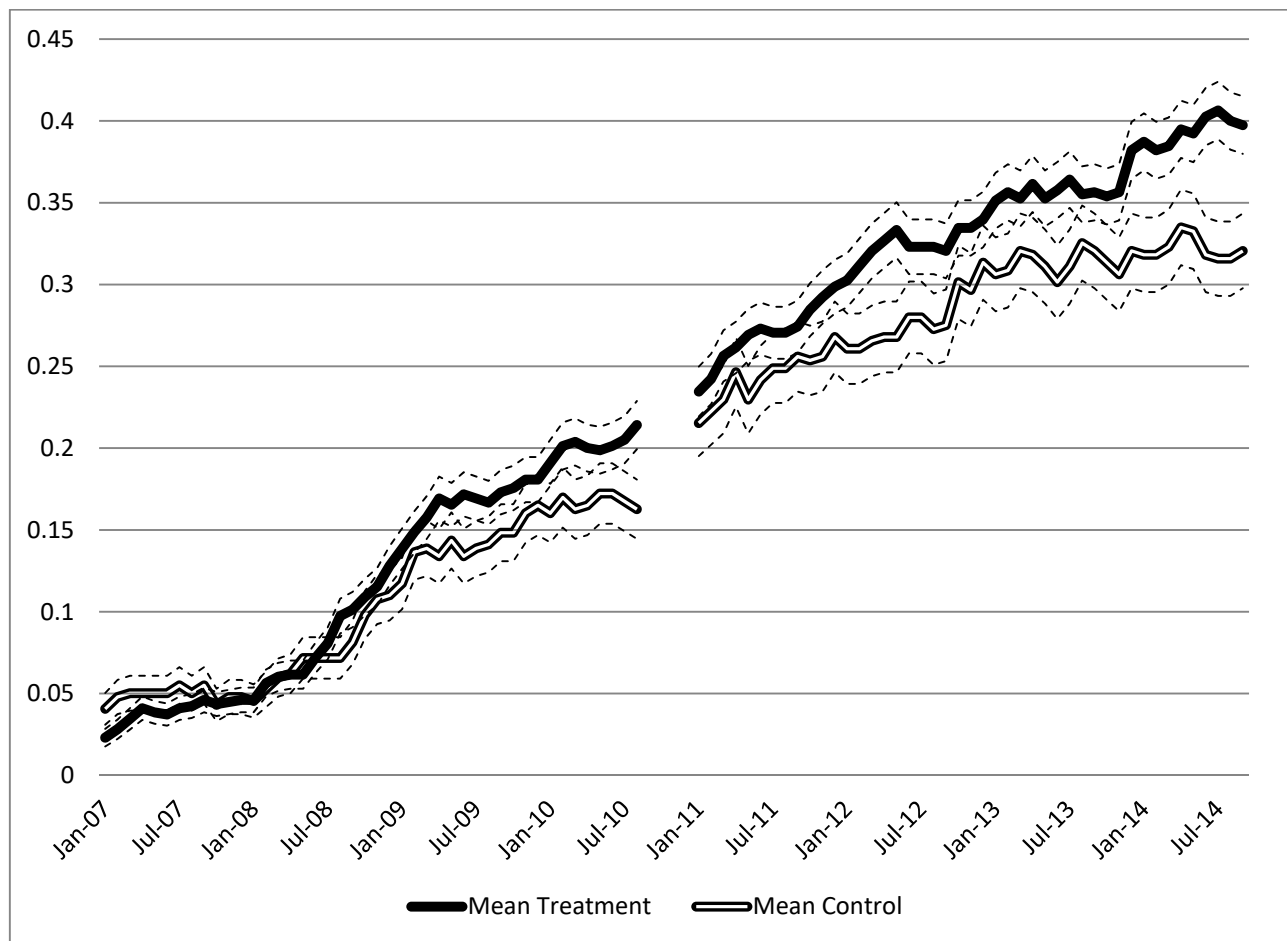
4.2 Labor market trajectories

The persistent long-term impact of the Juventud y Empleo training program on formality for men is the most significant and consistent result in all the specifications. The coefficients in Tables 5 through 7 are estimated for the point in time six years after the treatment. In addition to assessing the impacts at the long-term follow-up, it is of interest to look at the evolution of labor market outcomes over time: specifically, we can use a set of retrospective questions on labor market status in the short-term and long-term surveys to construct labor market trajectories on a monthly basis over the full time horizon (with a very small gap of four months from September to December 2010 not covered by the data due to the timing of the surveys). For this purpose, we merge the short-term and long-term data to combine individual trajectories.

We construct these labor market trajectories for the formal employment rate, as this has been identified as the most relevant outcome in the previous section. Figure 1 depicts the long-term trajectory of formal employment for men, separately for the treatment and control groups. The figure shows several patterns. First, the percentage of men that hold formal sector jobs increases over time for both treatment and controls. It starts at around 5 per cent for both groups before the start of the program, and increases over the six years to approximately 32 per cent for the control group and 40 per cent for the treatment group. Second, the trajectories indicate a slight lock-in effect: the treatment group employment rate is lower than the control group employment rate initially, but the curves soon intersect once the program is over. Third, from that point in time onward the average monthly employment rate for men in the treatment group lies always above the average for the control group. Fourth, this difference is not statistically significant for a substantial time period, but the gap widens and becomes statistically significant during the last 1 to 1.5 years. This indicates that the initially small

gains are consistent and increase over time.

**Figure 1. Labor market trajectory: Formal employment rate – ITT men**



Notes: The figure depicts monthly averages for treatment and control groups from the time of random assignment until long-term follow-up. Dashed lines correspond to 95% confidence intervals. The labor market trajectories are constructed from merging retrospective information collected in the 2010 and 2014 surveys. Due to the timing of the two surveys, the 4-month-period from September to December 2010 is not covered by the data.

Figures A1 to A6 in the appendix depict corresponding labor market trajectories for several other (sub-) samples: for the full sample as ITT and ATT; for women; for youths in Santo Domingo; and separately for the younger age group (<= 21 years of age) and the older age group (>21 years of age). Overall, the figures strengthen several of the patterns seen in Figure 1: First, a continuously increasing trend in employment rates over time for all (sub-) samples, in both treatment and control groups. Second, a lock-in effect during the year 2008, such that the treatment group trajectory typically crosses the

control group trajectory during the year 2009. Third, for the years three to six after treatment in all (sub-) samples the treatment group has a consistently higher employment rate than the control group – a rather pronounced pattern, despite the fact that the confidence intervals indicate that the difference is often not statistically significant. However, fourth, in several of the graphs the treatment-control difference does become significantly positive during the last 1 to 1.5 years prior to the long-term follow-up. Overall, this suggests that program gains do develop over time and do persist.

4.3 Local labor market context

Local labor market conditions may play a role in determining labor market outcomes and also the effectiveness of labor market interventions, as is the case e.g. for Job Corps in the US (Flores-Lagunes et al. 2010). In the main specifications (Tables 5 to 7, also Figure A4) we find that it is particularly youths in Santo Domingo, the capital, that benefit from participation in Juventud y Empleo. We investigate the role of local labor market further by looking at results by the three Dominican macro-regions, "Cibao" (North), "Sur" (South), and "Este" (East), where the latter excludes Santo Domingo. The corresponding regression results are reported in Tables A3 to A5 in the appendix.

In general, the results are not very strong, but do show some coarse pattern by macro-region. First, the North is the region for which we find significantly negative impacts on overall employment and labor force participation, driven by the results for the young women in the sample. For women themselves, also the impact on monthly earnings is significantly negative. At the same time, the North is the one region with a significantly higher rate of youths being in education: the overall average probability to report "currently being studying" is 6.2 percentage points or 11.1 per cent higher in the North (base probability 54.8 per cent, numbers not reported in the table). This pattern would be in line with the lower level of labor market outcomes, in particular for the labor force participation rate. Since marriage rates or number of children are not significantly different between regions, these are unlikely to be key explaining factors.

Looking at the South, almost all estimated coefficients are not significantly different from zero, the sole exception being a positive impact on formality for males. Different from the results for North and

South, the regressions for the East do show the main impacts we saw for the overall sample on formality (Table A2), driven mainly by the male population. One might argue that these results for the East are in line with the positive findings for the Santo Domingo subpopulation, in a way that also the region surrounding the capital benefits more strongly than other parts of the country. This may have to do mostly with the fact that the capital is characterized by the most dynamic labor market of the country, such that a skills training program may be more beneficial in a context where these skills are actually in demand.

5   Conclusions

Experimental evidence on long-term impacts of (youth) training programs had not been produced outside the US until now, and the large-scale experiments in the US date back two decades. This lack of rigorous evidence on such an important policy question – the longer-term effectiveness of human capital interventions for disadvantaged youths – is surprising. Together with a parallel study for Colombia (Attanasio et al. 2015), this paper contributes to closing this knowledge gap.

The evidence from the earlier experimental US studies points to long-run impacts that are small positive at best, but generally tend to be close to zero. In the case of Job Corps, a comprehensive intervention for the most disadvantaged youths, positive medium-run impacts on earnings do not seem to be sustained in the longer run. Against the background that some of these programs are comparatively intensive (and costly), these are certainly not encouraging results for the conception and design of youth training programs.

In the case of this new experimental long-term impact evaluation, we find a series of interesting results: because of the program, young men seem to have a better start of their careers, in the formal sector, and urban women improve their earnings. Specifically, our results show that there is a statistically significant long-run impact on the formality of employment for men (as measured by jobs with health insurance benefits, and a "weak formality index") participating in the Juventud y Empleo program. This effect of the Juventud y Empleo program was previously reported in the short-run evaluations, and

it is important to see that this impact is sustained over a long time horizon. Moreover, the long-term labor market trajectories we construct suggest that the effect is growing over time. Such lasting impacts in the quality of employment can make an important difference in the employment experience of young people and their lifetime labor market trajectories.

Second, the ITT and ATT estimates also provide evidence of sustained earnings impacts for female youths in the country's most important urban labor market, Santo Domingo. This, together with the impacts on formality for males, suggests that the returns to skills investments may be particularly relevant in the context of a more dynamic labor market where the demand for these skills is higher. The East region surrounding the capital also seems to benefit from this local labor market context (although to a lesser extent), whereas for the other two macro-regions North and South this does not seem to be the case.

Finally, for the full sample, the long-term impacts on labor earnings and overall employment probability are frequently close to zero in size and not statistically significant, which is compatible with the US studies. This result also has to be seen against the background that the total skills investment implied by the program is not very large compared to training programs in developed countries.

Overall we interpret our results as indicating that training programs for disadvantaged youth can have positive long-term outcomes: the Juventud y Empleo training improves formality in a context of high informality; and it seems to increase earnings in that part of the labor market in the Dominican Republic that is comparatively dynamic and where actual demand for skills exists. It is important to highlight the fact that a program of this kind has impacts on formality: given the high rate of informality of young people in LAC, impacts on the probability of being formally employed may change the path on which young people start their careers and can have lifelong impacts.

These findings are relevant for a much broader set of countries, since a multitude of economies worldwide face similar types of labor markets, and challenges for youths. Moreover, the training offered here, while not as comprehensive as the very intensive – and costly – interventions analyzed in previous research on long-term impacts (e.g. Job Corps in the US) does combine a sizeable investment

in both classroom and on-the-job training, and is thus comparable and similar to many youth interventions that are used across countries. Moreover, additional external validity comes from the fact that this is not a pilot intervention or pilot evaluation, but a rigorous long-term study on an active labor market program that is part of the set of public policies offered in a developing country to improve labor market opportunities for youth.

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**Figure A1. Labor market trajectory: Formal employment rate – ITT full sample**
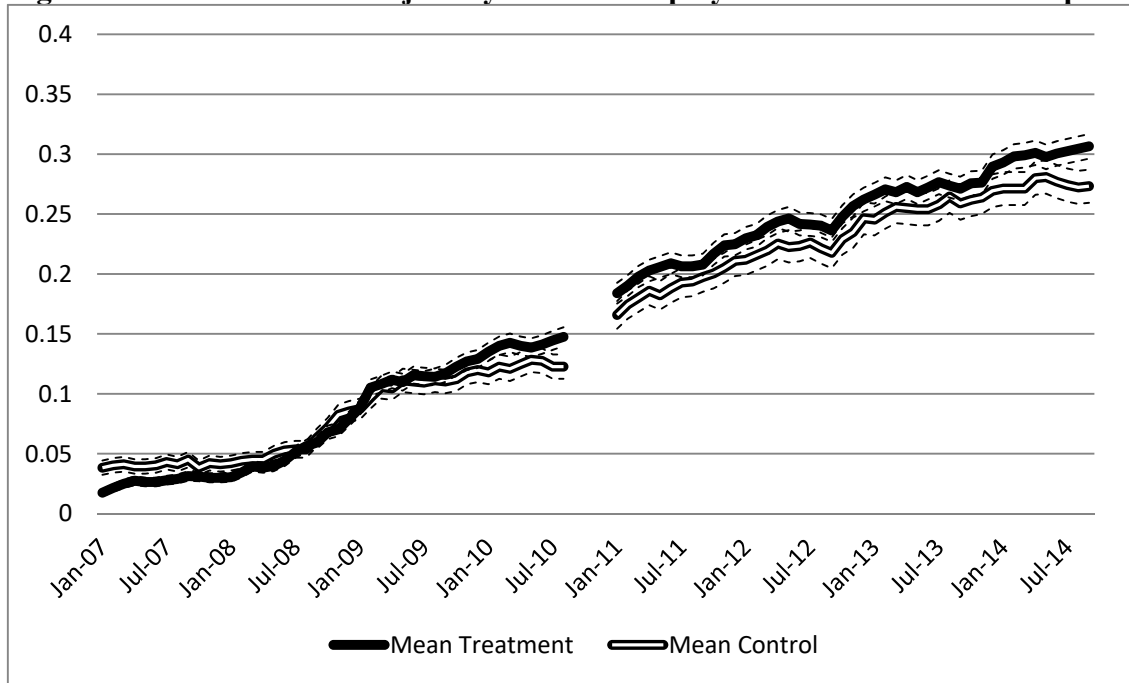


Notes: See notes to Figure 1.

**Figure A2. Labor market trajectory: Formal employment rate – ATT full sample**



Notes: See notes to Figure 1.

**Figure A3. Labor market trajectory: Formal employment rate – ITT women**
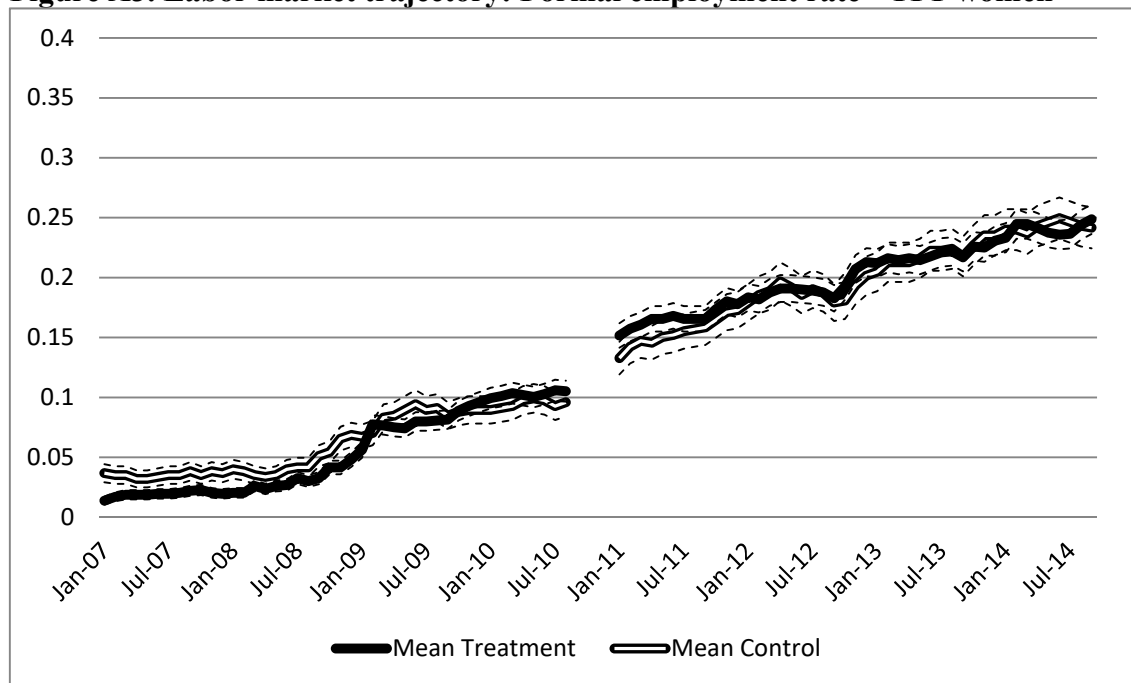


Notes: See notes to Figure 1.
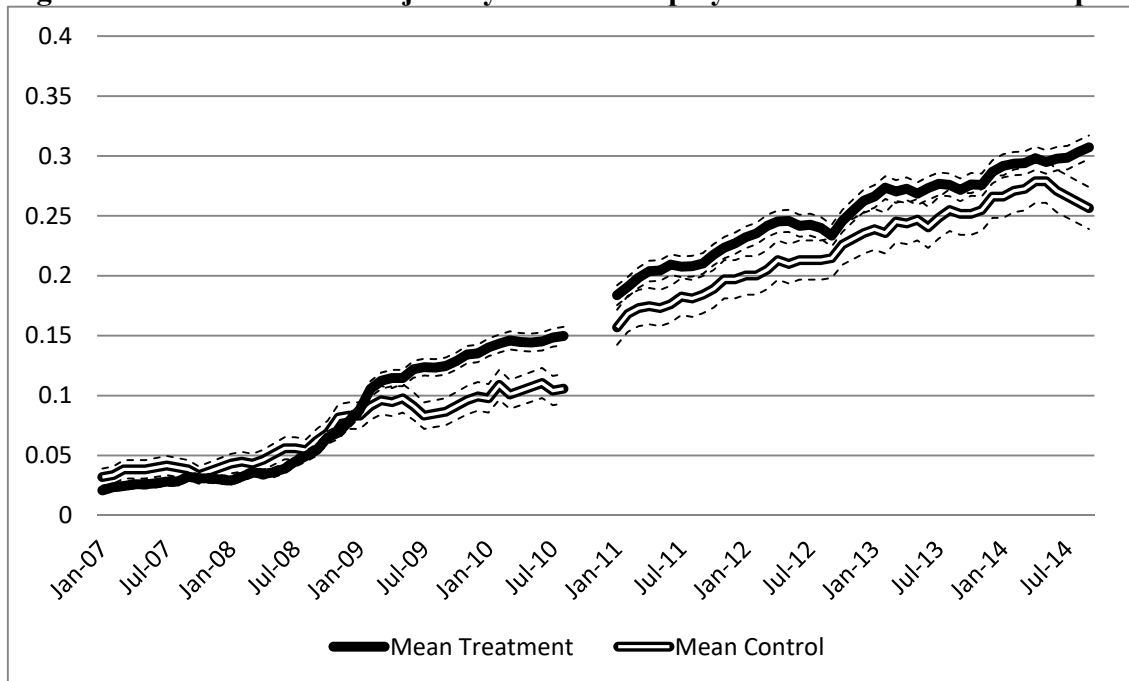
**Figure A4. Labor market trajectory: Formal employment rate – ITT Santo Domingo**



Notes: See notes to Figure 1.

**Figure A5. Labor market trajectory: Formal employment rate – ITT age group <=21 years**
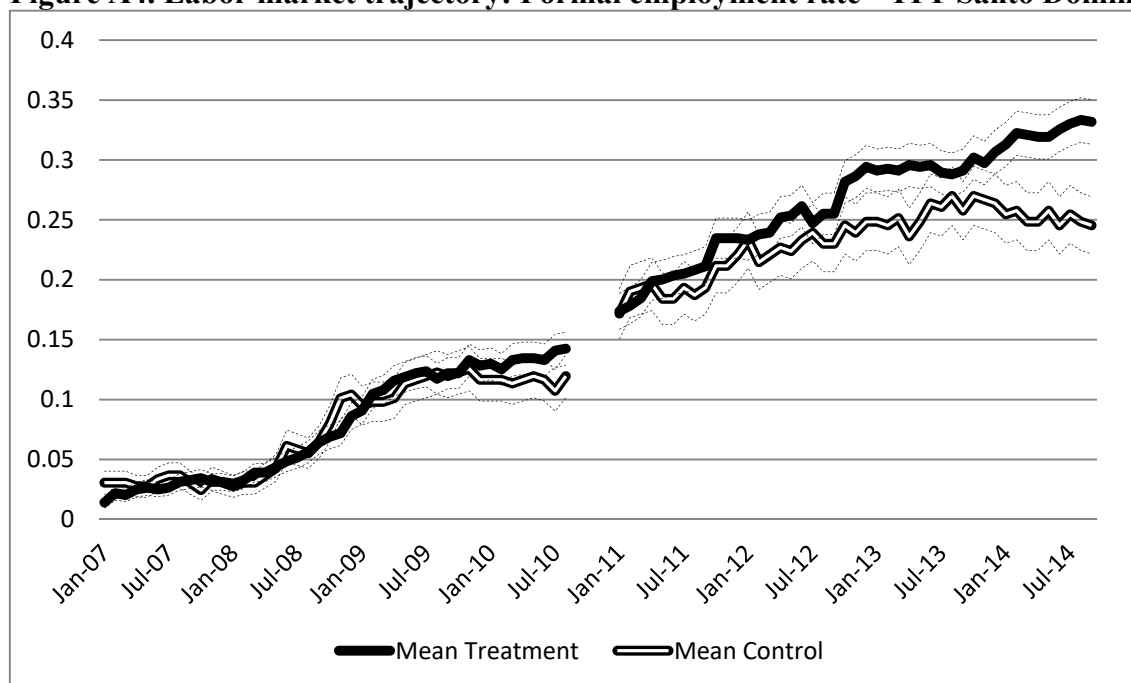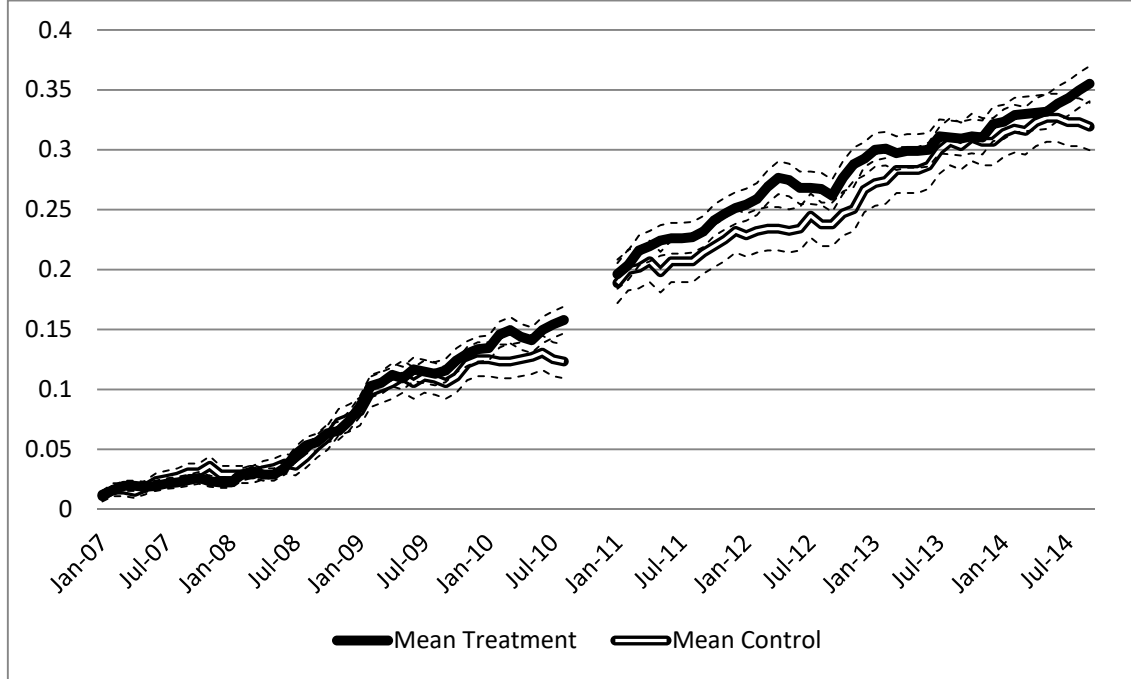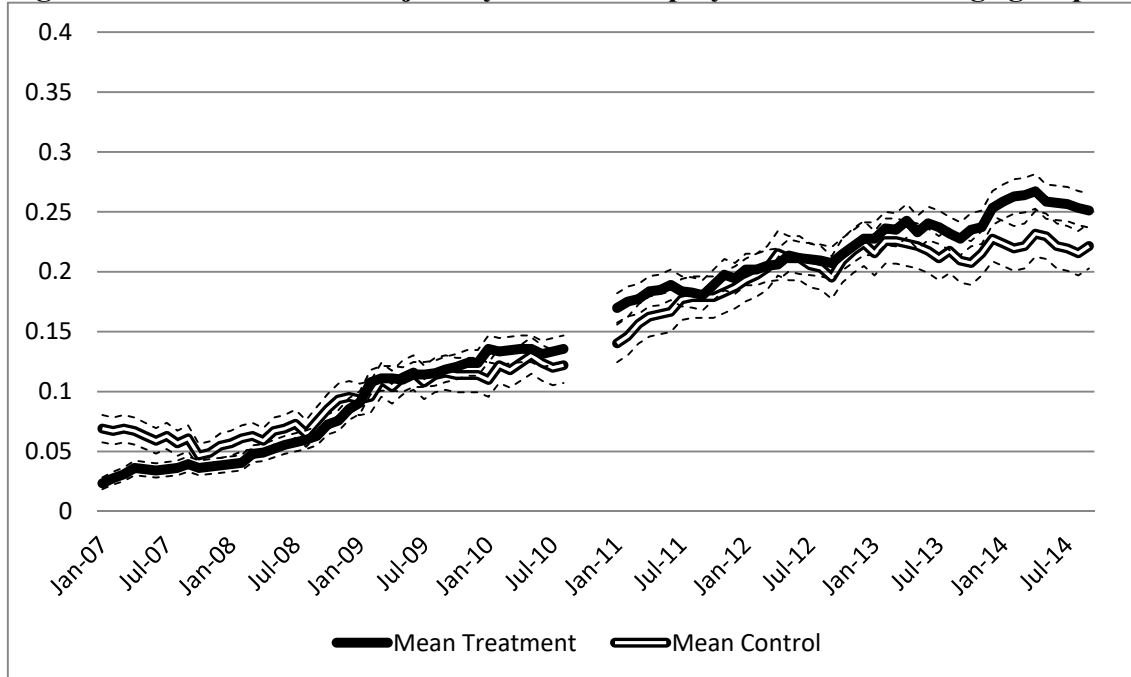


Notes: See notes to Figure 1.

**Figure A6. Labor market trajectory: Formal employment rate – ITT age group > 21 years**



Notes: See notes to Figure 1.

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**Table A1. Long-term impacts of "Juventud y Empleo" on labor market outcomes: ITT alternative definition**

| Outcomes | (1) Overall | (2) Women | (3) Men | (4) Santo Domingo | (5) SD Women | (6) SD Men | (7) 16 to 21 y.o. | (8) 22 y.o. or more |
|---|---|---|---|---|---|---|---|---|
| **Employed** | 0.0048 | 0.0165 | -0.0059 | 0.0393 | 0.0614 | -0.0089 | -0.0029 | 0.0208 |
| | (0.0180) | (0.0254) | (0.0225) | (0.0362) | (0.0476) | (0.0491) | (0.0264) | (0.0271) |
| Mean control group | 0.724 | 0.641 | 0.865 | 0.705 | 0.635 | 0.855 | 0.737 | 0.711 |
| **Employed with health insurance** | 0.0395** | 0.0228 | 0.0776** | 0.0810** | 0.0896** | 0.0618 | 0.0295 | 0.0474* |
| | (0.0191) | (0.0235) | (0.0311) | (0.0339) | (0.0388) | (0.0680) | (0.0291) | (0.0243) |
| Mean control group | 0.237 | 0.202 | 0.298 | 0.221 | 0.162 | 0.348 | 0.286 | 0.187 |
| **Employed with written contract** | 0.0248 | 0.0180 | 0.0403 | 0.0405 | 0.0334 | 0.0373 | 0.0231 | 0.0175 |
| | (0.0169) | (0.0207) | (0.0284) | (0.0299) | (0.0366) | (0.0562) | (0.0269) | (0.0224) |
| Mean control group | 0.189 | 0.155 | 0.246 | 0.189 | 0.155 | 0.261 | 0.225 | 0.151 |
| **Monthly earnings** | 340.7264 | 267.7623 | 575.0730 | 1,007.0935** | 1,032.3056** | 1,030.7811 | 470.0322 | 143.6105 |
| | (264.6333) | (272.7473) | (520.8847) | (461.8425) | (438.0657) | (1,084.0954) | (347.7381) | (381.8319) |
| Mean control group | 5000 | 3599 | 7368 | 4879 | 3443 | 7961 | 5213 | 4778 |
| **Ln Monthly earnings** | 0.0411 | 0.0020 | 0.1060* | 0.0482 | 0.1134 | 0.0322 | 0.0719 | -0.0202 |
| | (0.0508) | (0.0774) | (0.0635) | (0.1014) | (0.1570) | (0.1047) | (0.0672) | (0.0764) |
| Mean control group | 8.677 | 8.492 | 8.889 | 8.725 | 8.479 | 9.050 | 8.728 | 8.621 |
| **Labor force participation** | 0.0106 | 0.0143 | 0.0045 | 0.0374 | 0.0460 | 0.0107 | 0.0165 | 0.0094 |
| | (0.0137) | (0.0207) | (0.0130) | (0.0242) | (0.0317) | (0.0239) | (0.0204) | (0.0190) |
| Mean control group | 0.872 | 0.819 | 0.960 | 0.866 | 0.824 | 0.957 | 0.867 | 0.877 |
| **Observations** | 3,279 | 2,041 | 1,238 | 1,020 | 677 | 343 | 1,729 | 1,550 |

Notes: Regressions use the full sample of groups A, B & C (treated) and D (controls). All specifications control for training institutions COS) and region (10 regions responding to political administrative division). Clustered standard errors at course level in parentheses. Significance levels are indicated by *10%, **5%, and ***1%.

**Table A2. Long-term impacts of "Juventud y Empleo": ITT for "strong" and "weak" formality index**

| Outcomes | (1) Overall | (2) Women | (3) Men | (4) Santo Domingo | (5) SD Women | (6) SD Men | (7) 16 to 21 y.o. | (8) 22 y.o. or more |
|---|---|---|---|---|---|---|---|---|
| *Strong Formality index* | 0.0100 | -0.0001 | 0.0362 | 0.0442* | 0.0328 | 0.0824 | -0.0004 | 0.0135 |
| (Having both Health insurance & Written Contract) | (0.0134) | (0.0164) | (0.0259) | (0.0228) | (0.0271) | (0.0521) | (0.0212) | (0.0190) |
| Mean control group | 0.174 | 0.138 | 0.230 | 0.164 | 0.129 | 0.231 | 0.210 | 0.134 |
| *Weak Formality index* | 0.0330** | 0.0112 | 0.0824*** | 0.0731** | 0.0615* | 0.1096* | 0.0444* | 0.0104 |
| (Either Health insurance or Written Contract) | (0.0163) | (0.0208) | (0.0275) | (0.0298) | (0.0352) | (0.0615) | (0.0254) | (0.0230) |
| Mean dep. var for control group | 0.280 | 0.245 | 0.337 | 0.272 | 0.227 | 0.359 | 0.317 | 0.240 |

Notes: Regressions use the full sample of groups A and B (treated) vs. C and D (controls). All specifications control for training institutions COS) and region (10 regions responding to political administrative division). Clustered standard errors at course level in parentheses. Significance levels are indicated by *10%, **5%, and ***1%.

**Table A3. Long-term impacts of "Juventud y Empleo" on labor market outcomes by macro-region: ITT**

| Outcome | (1) North | (2) North women | (3) North men | (4) South | (5) South women | (6) South men | (7) East | (8) East women | (9) East men |
|---|---|---|---|---|---|---|---|---|---|
| *Employed* | -0.0843*** | -0.1371*** | 0.0009 | 0.0021 | 0.0056 | 0.0028 | -0.0007 | 0.0188 | -0.0163 |
| | (0.0269) | (0.0412) | (0.0310) | (0.0293) | (0.0450) | (0.0406) | (0.0315) | (0.0444) | (0.0364) |
| Mean control group | 0.797 | 0.750 | 0.854 | 0.684 | 0.590 | 0.854 | 0.767 | 0.672 | 0.908 |
| | | | | | | | | | |
| *Employed w/ health insurance* | -0.0184 | -0.0833* | 0.0653 | 0.0106 | -0.0180 | 0.0847** | 0.0297 | 0.0198 | 0.0504 |
| | (0.0352) | (0.0467) | (0.0546) | (0.0285) | (0.0379) | (0.0397) | (0.0350) | (0.0430) | (0.0575) |
| Mean control group | 0.279 | 0.275 | 0.285 | 0.197 | 0.168 | 0.250 | 0.316 | 0.273 | 0.379 |
| | | | | | | | | | |
| *Employed w/ written contract* | -0.0221 | -0.0352 | -0.0025 | 0.0258 | 0.0202 | 0.0476 | 0.0172 | 0.0103 | 0.0424 |
| | (0.0291) | (0.0408) | (0.0467) | (0.0282) | (0.0325) | (0.0548) | (0.0314) | (0.0375) | (0.0540) |
| Mean control group | 0.217 | 0.188 | 0.254 | 0.138 | 0.104 | 0.198 | 0.260 | 0.211 | 0.333 |
| | | | | | | | | | |
| *Monthly earnings* | -782.6251 | -899.2740** | -469.1724 | -51.8795 | -252.2339 | 697.1834 | 135.2489 | 343.3590 | -154.0295 |
| | (481.1298) | (394.1821) | (1,035.7267) | (378.4305) | (439.4946) | (778.2400) | (540.4879) | (599.0169) | (986.6684) |
| Mean control group | 5909 | 4250 | 7952 | 4409 | 3270 | 6462 | 5545 | 3993 | 7829 |
| | | | | | | | | | |
| *Ln Monthly earnings* | 0.0075 | -0.0379 | 0.0558 | 0.0711 | -0.0837 | 0.2359* | 0.0264 | 0.0292 | 0.0474 |
| | (0.0702) | (0.1094) | (0.0956) | (0.0782) | (0.1064) | (0.1259) | (0.0888) | (0.1333) | (0.1099) |
| Mean control group | 8.721 | 8.497 | 8.943 | 8.582 | 8.458 | 8.728 | 8.686 | 8.494 | 8.891 |
| | | | | | | | | | |
| *Labor force participation* | -0.0666*** | -0.1108*** | 0.0042 | 0.0305 | 0.0308 | 0.0302 | -0.0057 | 0.0070 | -0.0247* |
| | (0.0214) | (0.0344) | (0.0241) | (0.0265) | (0.0432) | (0.0203) | (0.0226) | (0.0371) | (0.0134) |
| Mean control group | 0.910 | 0.887 | 0.938 | 0.833 | 0.763 | 0.958 | 0.916 | 0.859 | 1 |
| Observations | 834 | 481 | 353 | 769 | 491 | 278 | 656 | 392 | 264 |

Notes: Regressions use the corresponding full samples for the Dominican Republic's three macro-regions "Cibao" (North), "Sur" (South), and "Este" (East), where the latter excludes Santo Domingo. All specifications control for training institutions (COS) and administrative sub-region (10 total). Standard errors in parentheses. Significance levels are indicated by *10%, **5%, and ***1%.

**Table A4. Long-term impacts of "Juventud y Empleo" on labor market outcomes by macro-region: ATT**

| Outcome | (1) North | (2) North women | (3) North men | (4) South | (5) South women | (6) South men | (7) East | (8) East women | (9) East men |
|---|---|---|---|---|---|---|---|---|---|
| *Employed* | -0.0768** | -0.1194** | -0.0025 | 0.0203 | 0.0378 | -0.0226 | 0.0478 | 0.0644 | 0.0092 |
| | (0.0354) | (0.0507) | (0.0416) | (0.0309) | (0.0455) | (0.0435) | (0.0435) | (0.0619) | (0.0516) |
| Mean control group | 0.806 | 0.761 | 0.857 | 0.670 | 0.567 | 0.862 | 0.730 | 0.629 | 0.902 |
| | | | | | | | | | |
| *Employed w/ health insurance* | -0.0222 | -0.0706 | 0.0453 | 0.0176 | -0.0023 | 0.0725 | 0.1037** | 0.0586 | 0.1827** |
| | (0.0392) | (0.0548) | (0.0539) | (0.0375) | (0.0456) | (0.0573) | (0.0435) | (0.0554) | (0.0704) |
| Mean control group | 0.291 | 0.284 | 0.299 | 0.195 | 0.167 | 0.246 | 0.261 | 0.243 | 0.293 |
| | | | | | | | | | |
| *Employed w/ written contract* | -0.0474 | -0.0633 | -0.0123 | 0.0227 | 0.0182 | 0.0395 | 0.1034** | 0.0681 | 0.1712*** |
| | (0.0328) | (0.0490) | (0.0420) | (0.0315) | (0.0319) | (0.0659) | (0.0427) | (0.0522) | (0.0641) |
| Mean control group | 0.242 | 0.216 | 0.273 | 0.135 | 0.100 | 0.200 | 0.198 | 0.171 | 0.244 |
| | | | | | | | | | |
| *Monthly earnings* | -545.6250 | -744.9065 | 46.2285 | 112.4076 | -135.0655 | 475.4241 | 1,006.2458 | 814.0878 | 843.0617 |
| | (544.3540) | (511.5383) | (995.1156) | (489.2831) | (551.8664) | (787.7755) | (696.3942) | (714.5601) | (1,455.9153) |
| Mean control group | 5925 | 4289 | 7795 | 4315 | 3234 | 6311 | 5001 | 3688 | 7244 |
| | | | | | | | | | |
| *Ln Monthly earnings* | 0.0315 | -0.0674 | 0.1230 | 0.0412 | -0.0552 | 0.1326 | 0.0435 | -0.0405 | 0.1474 |
| | (0.1076) | (0.1775) | (0.1278) | (0.0954) | (0.1331) | (0.1331) | (0.1050) | (0.1387) | (0.1662) |
| Mean control group | 8.723 | 8.526 | 8.901 | 8.580 | 8.440 | 8.745 | 8.666 | 8.543 | 8.818 |
| | | | | | | | | | |
| *Labor force participation* | -0.0409 | -0.0524 | -0.0050 | 0.0129 | -0.0072 | 0.0266 | 0.0499 | 0.0826 | -0.0214* |
| | (0.0248) | (0.0426) | (0.0285) | (0.0292) | (0.0457) | (0.0269) | (0.0335) | (0.0519) | (0.0116) |
| Mean control group | 0.909 | 0.875 | 0.948 | 0.843 | 0.783 | 0.954 | 0.874 | 0.800 | 1 |
| Observations | 755 | 427 | 328 | 719 | 458 | 261 | 605 | 362 | 243 |

Notes: Regressions use the corresponding samples of groups A and C (treated) and D (controls) separately for the Dominican Republic's three macro-regions "Cibao" (North), "Sur" (South), and "Este" (East), where the latter excludes Santo Domingo. All specifications control for training institutions (COS) and administrative sub-region (10 total). Standard errors in parentheses. Significance levels are indicated by *10%, **5%, and ***1%.

**Table A5. Long-term impacts of "Juventud y Empleo" on labor market outcomes by macro-region: LATE estimates**

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
|---|---|---|---|---|---|---|---|---|---|
| Outcome | North | North women | North men | South | South women | South men | East | East women | East men |
| *Employed* | -0.2036*** | -0.3720*** | 0.0019 | 0.0035 | 0.0094 | 0.0048 | -0.0018 | 0.0431 | -0.0472 |
| | (0.0645) | (0.1113) | (0.0645) | (0.0000) | (0.0000) | (0.0667) | (0.0767) | (0.0987) | (0.1016) |
| Mean control group | 0.787 | 0.739 | 0.853 | 0.681 | 0.575 | 0.878 | 0.722 | 0.620 | 0.887 |
| *Employed w/ health insurance* | -0.0444 | -0.2259* | 0.1398 | 0.0183 | -0.0303 | 0.1460** | 0.0740 | 0.0453 | 0.1457 |
| | (0.0826) | (0.1204) | (0.1178) | (0.0000) | (0.0000) | (0.0645) | (0.0850) | (0.0938) | (0.1614) |
| Mean control group | 0.270 | 0.246 | 0.304 | 0.196 | 0.157 | 0.268 | 0.284 | 0.250 | 0.339 |
| *Employed w/ written contract* | -0.0534 | -0.0956 | -0.0055 | 0.0444 | 0.0340 | 0.0820 | 0.0429 | 0.0235 | 0.1226 |
| | (0.0675) | (0.1041) | (0.0970) | (0.0000) | (0.0000) | (0.0880) | (0.0768) | (0.0826) | (0.1536) |
| Mean control group | 0.225 | 0.204 | 0.255 | 0.149 | 0.118 | 0.207 | 0.210 | 0.180 | 0.258 |
| *Monthly earnings* | -1,890.6513* | -2,439.5573** | -1,003.9579 | -89.2155 | -424.7839 | 1,201.4129 | 337.1953 | 785.6898 | -444.9637 |
| | (1,137.6799) | (1,018.7876) | (2,157.1896) | (0.0000) | (0.0000) | (1,328.6650) | (1,318.1609) | (1,328.5032) | (2,712.5867) |
| Mean control group | 5592 | 4066 | 7717 | 4348 | 3129 | 6622 | 4906 | 3615 | 6989 |
| *Ln Monthly earnings* | 0.0181 | -0.1092 | 0.1167 | 0.1263 | -0.1463 | 0.4270* | 0.0684 | 0.0655 | 0.1453 |
| | (0.1654) | (0.0000) | (0.1943) | (0.0000) | (0.0000) | (0.2428) | (0.2229) | (0.2839) | (0.3200) |
| Mean control group | 8.708 | 8.501 | 8.937 | 8.576 | 8.409 | 8.769 | 8.676 | 8.546 | 8.828 |
| *Labor force participation* | -0.1610*** | -0.3007*** | 0.0090 | 0.0525 | 0.0518 | 0.0520 | -0.0143 | 0.0161 | -0.0715** |
| | (0.0535) | (0.0987) | (0.0503) | (0.0000) | (0.0000) | (0.0334) | (0.0548) | (0.0818) | (0.0364) |
| Mean control group | 0.889 | 0.852 | 0.941 | 0.847 | 0.784 | 0.963 | 0.870 | 0.790 | 1 |
| Observations | 834 | 481 | 353 | 769 | 491 | 278 | 656 | 392 | 264 |

Notes: Regressions use the corresponding full samples for the Dominican Republic's three macro-regions "Cibao" (North), "Sur" (South), and "Este" (East), where the latter excludes Santo Domingo. All specifications control for training institutions (COS) and administrative sub-region (10 total). Standard errors in parentheses. Significance levels are indicated by *10%, **5%, and ***1%.