The Labor Market Impacts of Youth Training in the Dominican Republic

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We report the impacts of a job training program operated in the Dominican Republic. A random sample of applicants was selected to undergo training, and information was gathered 10–14 months after graduation. Unfortunately, people originally assigned to treatment who failed to show up were not included in the follow-up survey, potentially compromising the evaluation design. We present estimates of the program effect, including comparisons that ignore the potential nonrandomness of "no-show" behavior, and estimates that model selectivity parametrically. We find little indication of a positive effect on employment outcomes but some evidence of a modest effect on earnings, conditional on working.

I. Introduction

Since 1992 the Inter-American Development Bank (IADB) has financed a series of innovative training programs throughout Latin America. These

programs target less-educated youth—a group that faces substantial bar-
riers to labor market success in both developing and developed econo-
mies—with the explicit aims of raising participants' job skills and matching
them to suitable employers.[1] Drawing on lessons from evaluations of the
Job Training Partnership Act (JTPA) in the United States and the Youth
Training Scheme in Britain, these programs combine classroom training
with a subsequent internship period of on-the-job work experience.[2] Un-
like 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 contractors are encouraged to participate in
the provision of training through a competitive bidding process. Con-
tractors' proposals need to be backed by commitments from local em-
ployers to offer internships of at least 2 months duration.

Among this round of newly designed programs, the Juventud y Empleo
(JE) program in the Dominican Republic was one of the first to incor-
porate a randomized evaluation design. A similar program in Colombia,
Jóvenes en Acción, also incorporated a randomized design to allow for
the evaluation of training (see Attanasio, Kugler, and Meghir 2009). This
article summarizes the impacts of Juventud y Empleo on a wide range of
labor market outcomes, including employment, hours of work, monthly
earnings, and hourly wages. We also use a simple dynamic model of labor
market transitions to estimate the impacts of JE on the "employability"
of trainees—the primary stated goal of the program—and on the ability
of the trainees to find and hold jobs with health insurance coverage.

Our analysis is based on a sample of applicants for 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. A follow-up
survey was administered in the period from May to July of 2005, some 10–
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

[1] 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.

[2] The Job Training Partnership Act program is described extensively by Heck-
man et al. (1999). Dolton, Makepeace, and Treble (1994) describe the Youth Train-
ing Scheme.

failure to include in the follow-up survey people who were originally assigned to receive training but who failed to show up (or attended for only a very short time). We therefore consider the results from a parametric selection model that incorporates the possibility of a correlation between labor market outcomes in the follow-up survey and the decision to show up for training. These models, and our dynamic models of employment transitions, also show relatively small effects on employment outcomes (though we cannot reject a relatively wide range of positive impacts).

A brief literature review follows this introduction, focusing on previous findings for similar programs, particularly in Latin America. The specifics of the program are presented in Section III, along with information on the intake sample and the randomization process. Our main results as well as a dynamic analysis of the impact of training are described in Section IV. We conclude in Section V.

II. Literature Review

Few public policies have been studied and evaluated as rigorously as job training programs. Most of the existing evidence is derived from programs in the United States and Europe.[3] In the U.S. case, particularly credible evidence is available from randomized evaluations of JTPA (see General Accounting Office 1996; Bloom et al. 1997; Heckman et al. 1998; Heckman, Ichimura, and Todd 1998), Job Corps (Schochet, McConnell, and Burghardt 2003), and a series of programs for welfare recipients (see Friedlander, Greenberg, and Robins 1997). One key conclusion that emerges from the U.S. literature is that the impacts of job training are generally modest. Even this conclusion has to be qualified, since there seems to be substantial heterogeneity in impacts depending on the characteristics of the participants and the type of training.[4] For example, many studies have concluded that women benefit more from training than men (Friedlander et al. 1997). On-the-job training is often thought to be more effective than classroom training, although this is by no means a universal finding. Voluntary programs are generally found to be more effective than mandatory programs (Friedlander et al. 1997). Finally, in the case of work experience programs, private sector programs are thought to be more effective than public sector programs (Kluve 2006; Kluve et al. 2007).

With respect to youth, randomized evaluations from two large pro-

[3] Among the earliest evaluation in the economics literature are the studies by Ashenfelter (1978) and Kiefer (1979). Subsequent studies include the important paper by Lalonde (1986), which underscored the case for the use of randomized experiments in training program evaluations.

[4] As noted by Heckman et al. (1999, 2000), programs can be classified by the type of clients and the type of training. In most cases, clients fall into two categories: youth or the unemployed. Program services can be divided into classroom programs or on-the-job (or "work experience").

grams operated in the United States in the 1990s—JTPA and the Job Corps—yield quite different results. The short-run impacts of JTPA on young women were essentially zero (although the longer-term impacts appear to be more positive; see General Accounting Office 1996), while the short-run impacts for young men were negative. In contrast, the Job Corps appears to have had a significantly positive short-run effect on both genders but little or no long-term effect (Schochet et al. 2003).

The European evidence is even less clear (Heckman et al. 1999), in part because of the lack of experimental studies and the wide variation in evaluation methods.[5] Nevertheless, one key finding that emerges from the meta-analysis by Kluve et al. (2007) is that programs serving youth are less likely to show positive impact effects than programs for adults.

Evidence on the effectiveness of training in developing countries is relatively limited. Betcherman et al. (2004) review 69 impact evaluations of unemployed and youth training programs and conclude that training impacts in Latin America are on average more positive than the impacts of programs in the United States and Europe. Likewise, Ñopo and Saavedra (2003) analyze a sample of training programs in Latin America and conclude that employment and income impacts of the programs tend to exceed the impacts in developed countries.[6] Nevertheless it should be acknowledged that variability in methods and data used in the existing nonexperimental evaluations in Latin America have produced widely varying results, even for the same program. A case in point is Peru's youth training program: seven evaluations have produced a very wide range of estimated impacts for this program.[7]

The only other randomized evaluation of the impact of training in Latin America is provided by Attanasio et al. (2009) in a study of the Colombian Jóvenes en Acción program. They conclude that Jóvenes en Acción (which ran about the same time as the JE program in the Dominican Republic and offered a generally similar training program) had a positive effect on

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[5] Evaluations of the Youth Training Scheme (YTS) in Britain are emblematic of this variation. Main and Shelley (1990) and Main (1991) report positive impacts on short-term employment in the range of 11–17 percentage points. Whitfield and Bourlakis (1990) find a smaller impact on employment, around four points, while Dolton et al. (1994) report negative impacts in the range of −4 to −17 points.

[6] Weller (2004) also looks at Latin America training programs but does so in a broader context.

[7] These evaluations used data for different cohorts of trainees. The estimated earnings impacts at 18 months ranged from 13% to 40%. A recent analysis of Mexican training programs (Delajara, Freije, and Soloaga 2006) finds a similarly wide range of estimated program impacts, depending on the econometric model used.

paid employment and a large positive impact on earnings (a 12%–15% increase relative to the mean of the control group).[8]

Two other shortcomings are present in much of the literature, and these are shared by our evaluation of the JE program. First, few studies provide impact estimates for the period beyond 2 or 3 years after completion of training.[9] In the case of Latin America, existing evaluations tend to focus on impact after 12 or 18 months. As a result, there is considerable uncertainty about the persistence of training effects. A second limitation is the paucity of information on program costs (though Attanasio et al. [2009] is an important exception) and of other possible program effects, such as general equilibrium spillover or "crowding" effects (Heckman and Carneiro 2003).

III. LAC Training Programs and Juventud y Empleo

A. Background

Job training programs have traditionally played a central role in active labor market policies of the Latin American and Caribbean (LAC) region. During the era of import-substitution growth policies (broadly, from the 1940s to the early 1980s), many countries adopted a centralized model for training provision, organized through a so-called national training institute (NTI).[10] Program content was usually dictated by NTIs, and services were targeted to more highly skilled workers who were already employed in the sectors favored by the import-substitution growth strategy.

With the abandonment of import-substitution policies in the early 1980s, NTIs in many countries came under pressure to adopt a "demand-driven" model of decentralized training, with greater participation of local employers in the selection of program content and training providers. A new generation of programs has emerged in many LAC countries, designed by the ministries of labor independently from the NTIs and in accord with the principles of this demand-driven model.

Two influential programs, the Mexican PROBECAT that began in 1984 and Chile Joven that started in 1992, have served as models for this new

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[8] Attanasio et al. (2009) report a modest and statistically insignificant effect on overall employment (including unpaid work) but a larger and significant effect on formal sector employment.

[9] In addition to the results for the Job Corp and JTPA, long-term outcomes are available for a few other experimental evaluations, including the National Supported Work demonstration (Couch 1992) and the California GAIN program (Hotz, Imbens, and Klerman 2006).

[10] For a compilation of experiences in this era, see Cardenas, Ocampo, and Thorp (2001). National training institutes in this era included SENA in Colombia, SENAI in Brazil, SNPP in Paraguay, INFOTEP in the Dominican Republic, SENATI in Peru, and INAFORP in Panama.

generation of programs. In PROBECAT, private sector employers provide both classroom training and a subsidized internship period of on-the-job training. Variants of the "Mexican model" were adopted in Central America (e.g., in Honduras and El Salvador) during the late 1990s. Under the alternative "Chilean model," trainees generally receive technical/vocational training at an independent provider, and this is followed by a subsidized internship at a private sector firm. Variants of this model were adopted in Venezuela and Argentina during the mid-1990s and in Peru, Colombia, Uruguay, and the Dominican Republic in the late 1990s.

B. Juventud y Empleo: Basic Design

Juventud y Empleo was developed and implemented by the Government of the Dominican Republic with financial support from the IADB. During the period from 2001 to 2006 the JE program focused on low-income youths (ages 18–29) with less than a secondary education (i.e., no more than 11 years of completed schooling) who were not enrolled in regular schooling. Special emphasis was placed on enrolling women. The stated objective of the JE program was to increase the likelihood of employment for the lowest-income members of the working-age population by facilitating access to the labor market through training and counseling. According to the program design mandate, this was to be achieved by adapting the nature of training to the demands of local employers (Inter-American Development Bank 1999).

Following the principles of the "Chilean model," the Ministry of Labor outsourced the provision of training services to private training institutions (instituciones de capacitación [ICAPs]). Courses (with a maximum duration of 350 hours) were conducted in the ICAPs' facilities; they were split into two parts: basic skills training and technical/vocational training. Basic skills training was meant to strengthen trainees' self-esteem and work habits, while vocational training was customized to the needs of local employers.

The ICAPs were selected through a competitive bidding process. Proposals from potential training providers were required to include written commitments from at least one local firm to offer 2-month internships to trainees graduating from the provider's program. These were supposed to ensure that the ICAP was offering training that would be of value to local employers.[11] The original project design also required ICAPs to

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[11] It should be noted that delays occurred between the presentation of bids by the ICAPs and the awarding of contracts. By the time trainees graduated, many of the firms that had originally signed an internship agreement with the ICAPs were unable to offer the number of internships initially promised. Therefore, a large proportion of graduating trainees were matched with internships offered by firms different from than those originally contacted by the ICAPs

follow up on the trainees during the internship period to provide counseling and technical assistance. In practice, this follow-up was limited.

An important feature of the JE program is that relatively few of the participating employers appear to have used the internships as a screening channel for recruiting new employees. Indeed, the field team who assisted in implementing the program believes that nearly all interns were let go at the end of their internship. One explanation for this is that the intern's wage costs were fully subsidized. As a result, employers had a strong incentive to fill their slots with new trainees once the subsidy period came to an end.

All potential training providers were required to present training proposals for the courses they would offer. The proposals were evaluated and revised by the National Institute of Technical and Professional Training (Instituto Nacional de Formación Técnica Profesional [INFOTEP]). INFOTEP was also contracted to inspect the selected ICAPs before any training took place and during the training courses. Much less frequently, ICAP personnel also visited some of the firms that were providing internships.

The eligibility requirements for JE specified that enrollees were to be between the ages of 16 and 29, with less than a high school diploma, currently not working or attending school, holding a valid identity card, and willing to work and receive training. Trainees were not paid during the classroom component of the program, but they did receive partial reimbursement for their transportation costs and meals, up to a maximum of 50 Dominican pesos per day or about 1,000 pesos per month (roughly $40). This stipend was well below the typical level of earnings for members of the control group who were working in the follow-up survey (4,500 pesos per month). The program also provided trainees with insurance against workplace accidents.

C. Implementation

The original JE design specified that applicants for training would apply at a local office of the Ministry of Labor, where personal information would be gathered and checked against the eligibility criteria before being forwarded to a central office for random assignment. In practice, the local offices did not have the capacity to perform this function, and enrollment was conducted by the ICAPs. Staff from the Ministry of Labor and the ICAPs conducted outreach programs in poor neighborhoods of the larger urban centers of the Dominican Republic, informing people about the availability of the training courses. The outreach effort included *perifoneo*

(in any case, the graduates performed tasks related to the course they had taken; courses were fairly general and not tailored to specific firms).

(announcements by vehicle-mounted loudspeaker), radio advertisements, and contacts with churches and other community groups.

Applicants for a training position with an ICAP completed a short survey that gathered information on their age, education, and employment status. This information was then used to determine eligibility. Some of the eligibility requirements (e.g., employment status and completed education) were hard to verify, and the rules were apparently known by applicants, leading to reporting problems that we discuss in more detail below. Once a group of 30 eligible applicants was recruited, the ICAP submitted the list of names (selected on a first-come, first-serve basis from the list of those who met the eligibility criteria) to the Ministry of Labor, which randomly selected 20 names to receive the program.[12] The other 10 were assigned to the control group. The ICAPs were allowed to reassign up to five people from the control group to the treatment group in the event that people in the original treatment group failed to show up for training or dropped out within the first 2 weeks of the course. (For simplicity we refer to these individuals as "no-shows," although we do not know what fraction failed to show up vs. dropped out early).

As discussed below, some 17% of those originally assigned to the treatment group failed to show up for training (or dropped out early), and since the original control group was only one-half as large as the original program group, approximately one-third of the controls were reassigned to treatment status. Unfortunately, the exact procedures used by individual ICAPs to reassign applicants from the control group to the program group are unknown. We present some evidence below suggesting that the probability of reassignment was related to individual characteristics, even conditional on the particular training institution. Thus, in some of our analyses we drop the reassigned controls.

D. Applicant Sample: Initial Assignment, Realized Treatment Status, and Follow-up

We obtained a file containing information for all eligible applicants who applied for JE training in early 2004.[13] Characteristics of this population broken down by assignment status are presented in table 1. A total of 2,564 applicants (31%) were originally assigned to the control group (col.

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[12] The original program design called for three treatment groups and one control group. The three treatment groups would have received (i) training and no internship, (ii) internship and no training, or (iii) both. Due to the difficulties in implementing this scheme the program was simplified to have only one treatment (with both the training and the internships components) and one control group.

[13] The data file was constructed from files submitted by the ICAPs. The file includes 8,391 eligible applicants, but in the current study we drop 26 cases with missing data on initial assignment.

Table 1
Baseline Characteristics and Follow-up Status by Initial Assignment

	Originally Assigned to Control Group			Originally Assigned to Treatment		
	All (1)	Not Reassigned (2)	Reassigned to Treatment (3)	All (4)	No-shows (5)	Received Training (6)
Baseline characteristics:						
1. Percent female	55.5	57.0	52.8*	54.3	53.3	54.5
2. Percent ages 17–19	24.5	23.9	25.4	23.8	23.1	24.0
3. Percent ages 20–24	49.8	49.2	51.0	51.1	54.6	50.5*
4. Percent age 25+	25.7	26.9	23.6	24.9	22.3	25.4*
5. Years schooling	9.2	9.1	9.5*	9.2	9.2	9.3
6. Percent primary only	34.4	38.6	27.4*	33.3	37.4	32.4*
7. Percent married	20.6	22.2	17.6*	19.4	20.4	19.1
8. Percent with dependents	18.8	19.3	18.1	19.6	21.3	19.2
9. Number in household	4.9	4.8	4.9	4.9	4.9	4.9
10. Percent get remittances	3.6	3.5	3.7	3.4	4.0	3.3
11. Percent with outdoor toilet	25.2	27.4	21.6*	24.6	21.7	25.2*
12. Percent enrolled	35.3	30.5	43.7*	36.0	40.3	35.1*
13. Percent employed	2.2	2.5	1.6	2.7	2.5	2.8
14. Percent with experience	15.4	14.8	16.4	15.2	15.9	15.1
Post-baseline outcomes:						
15. Percent no-show				17.4	100.0	.0
16. Percent reassigned	36.7	.0	100.0			
17. Percent in follow-up	27.2	34.7	14.2	11.2	.0	13.5
18. No. observations	2,564	1,623	941	5,801	1,010	4,791

NOTE.—Baseline characteristics are taken from that baseline survey administered prior to random assignment. Wave 2 outcomes are taken from a follow-up survey administered approximately 12 months after baseline. Reassigned group members were initially assigned to the control group and were reassigned to treatment to fill slots of no shows. "No shows" are those initially assigned to treatment who failed to enroll.

* Indicates that the difference in mean value of characteristic between reassigned subset and others, or between no-shows and others, is statistically significant at the 5% level.

1), while 5,801 (69%) were assigned to the treatment group (col. 4). Of the original treatment group, 1,011 were "no-shows" (17% of the initial group; see col. 5), while 4,791 are recorded as having received training (col. 6). To fill the places of the no-shows, 941 members of the original control group were reassigned to the treatment group (col. 3), leading to a "realized treatment group" of 5,723 (= 4,791 + 941) and a "realized control group" of 1,623 (col. 2).

Consistent with the aims of the JE program, the baseline data in table 1 show that applicants to the program were over one-half female, were relatively young, had relatively low levels of education, and had very low employment rates at the baseline. Only about 15% reported having had previous work experience. Just under 20% reported being married, and about the same fraction reported having dependents. In an earlier version of this article (Card et al. 2007, table 1) we noted that these characteristics are broadly similar to the characteristics of youth (ages 16–29) in the Dominican labor force, though in the youth population as a whole the employment rate is about 50% and about 60% report having had previous work experience.[14]

Comparisons between those originally assigned to the control group (col. 1 of table 1) and those originally assigned to treatment (col. 4) suggest that the two groups were very similar. In fact, as shown in table 2 (row 1, col. 1), a chi-squared test for the joint significance of the 14 covariates in a logit model predicting treatment/control status is not significant (p-value = 0.77), suggesting that over the entire applicant population random assignment was successfully implemented.[15] Nevertheless, the fractions of applicants assigned to the two groups varied across ICAPs, and as shown by the entry in row 1, column 2 of table 2, a set of ICAP dummies (plus a dummy for sites in Santo Domingo) are highly significant predictors of treatment/control status.[16] In our most general models for the impact of assignment to treatment we therefore include controls for the identities of the individual ICAPs.

[14] We suspect that some JE applicants were aware of the eligibility requirements and underreported their education, current employment, and remittance receipt. As shown in Card et al. (2007, table 1), average education reported in the follow-up survey is about 1 year higher than in the baseline survey, and the fraction reporting remittances from abroad is over 20% (vs. 4% in the baseline).

[15] The covariates used in the logit models (and in the models throughout this article) are the ones listed in table 1, with the addition of a variable measuring age in years and the deletion of one of the three age categories.

[16] There were a total of 33 ICAPs involved in the second cohort of the JE program. In general, ICAPs serve a specific municipality: the only exception is a few larger ICAPs that served trainees in Santo Domingo and other nearby areas. The addition of a dummy for Santo Domingo plus dummies for ICAPs therefore controls for location as well as training institution.

Table 2
Tests for Effects of Individual Covariates and Training Institution Effects in Assignment

	Explanatory Power of:		
	Individual Covariates (14 *df*) (1)	Training Institution/Location (33 *df*) (2)	Individual Covariates Conditional on Institution/Location (14 *df*) (3)
1. Original assignment to treatment and control groups (N = 8,365)	9.9 (.772)	224.3 (.000)	27.4 (.016)
2. No-show conditional on assignment to training (N = 5,801)	47.1 (.000)	696.8 (.000)	33.8 (.002)
3. Training conditional on assignment to control group (N = 2,564)	91.9 (.000)	504.8 (.000)	24.8 (.036)
4. Final assignment to treatment and control groups (N = 7,355)	56.4 (.000)	207.7 (.000)	36.0 (.001)
5. Final assignment to treatment and control groups, excluding reassigned (N = 6,414)	40.0 (.000)	209.1 (.000)	35.5 (.001)
6. Final assignment, conditional on inclusion in follow-up survey (N = 1,345)	21.7 (.085)	89.5 (.000)	22.4 (.071)
7. Final assignment, conditional on inclusion in follow-up survey and excluding reassigned (N = 1,211)	21.8 (.083)	92.1 (.000)	26.0 (.026)

NOTE.—Entries are chi-squared statistics and *p*-values (in parentheses) for the joint significance of the variables in the column heading in a logit model for the classification described in the first column of each row. For example, the entry in row 1, col. 1 is the chi-squared statistic for the null of zero population coefficients on the 14 individual covariates in predicting original assignment to treatment among 8,391 observations in the first cohort of the JE program.

In a randomized evaluation the fact that some members of the treatment group fail to receive treatment does not invalidate the design, provided that all those initially assigned to treatment (or a random subsample) are included in the impact analysis.[17] Unfortunately, in the JE evaluation only people in the realized treatment group were included in the follow-up survey: no attempt was made to contact the no-shows. This oversight causes a problem to the extent that people who remained in the program are systematically different from the no-shows. In fact, comparisons between the two groups (cols. 5 and 6 of table 1) reveal a number of differences, including a gap in the fraction enrolled in school that is significant at the 1% level. As shown in column 1, row 2, of table 2, the individual covariates are significant predictors of no-show status (χ^2 statistic = 47.1 with 14 df). Further investigation reveals that the no-show rate also varied widely across ICAPs, presumably reflecting program quality differences and other factors.[18] This is confirmed by the extremely high chi-squared statistic in column 2, row 2, of table 2. Conditional on ICAP fixed effects, the individual covariates are still significant predictors of no-show behavior (col. 3), but the chi-square is notably smaller than in column 1.

As noted above, the design of the JE evaluation allowed the ICAPs to fill the "empty seats" left open by the no-shows with people from the control group. Comparisons between control group members who were reassigned and those who were not (cols. 2 and 3 of table 1) reveal significant differences in years of schooling, marriage rates, student status, and the fraction with an outdoor toilet, suggesting that the reassignment process was nonrandom. Moreover, as shown in column 1, row 3, of table 2, the covariates are highly significant predictors of reassignment status. The apparent correlation between reassignment rates and individual characteristics is largely but not totally attributable to differences in reassignment rates across ICAPs and differences in the characteristics of enrollees at each ICAP. Once ICAP dummies are included in the assignment prediction model, the predictive powers of the individual characteristics fall, though they are still jointly significant at conventional levels (p-value = 0.036). Given these results it may be plausible to interpret the realized control group as a random subset of the originally assigned control group, conditional on the identity of the training institution, though caution is obviously required.

Simple comparisons between the realized control group and the realized

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[17] When some people assigned to treatment fail to receive treatment or some people assigned to the control group get treatment on their own, it is conventional to refer to the difference in outcomes between the treatment and control groups as an estimate of the "intention to treat" effect (e.g., Angrist, Imbens, and Rubin 1996).

[18] Some ICAPs apparently failed to monitor their attendance records closely enough to detect no-shows and to conduct reassignment.

treatment group would provide the basis for valid inferences about the JE program if no-show behavior was random and if reassignment from the control group to the treatment group was random. Although we have already shown some evidence against the former hypothesis, it is useful to directly test for differences between the realized assignment groups. The test statistics presented in row 4 of table 2 show that the individual covariates are significant predictors of realized treatment group status, either alone (col. 1) or conditional on ICAP fixed effects (col. 3). Thus, it appears that differences between the realized treatment and control groups must be interpreted cautiously. In the next section we develop and implement a selection correction procedure that addresses the non-random nature of the no-show process. We also present estimates that exclude members of the control group who were reassigned to the trainee group. As shown in row 5 of table 2, the test results for the predictability of assignment status are very similar whether we include or exclude the reassigned controls.

Owing to budget limitations, the follow-up survey that forms the basis for our evaluation was administered to only a subset of the realized control and treatment groups in the JE evaluation.[19] Overall, as shown in row 17 of table 1, 35% of the realized control group and 14% of the realized treatment group completed the follow-up survey. Thus, for our analysis of post-training outcomes we have a sample of 1,345 observations: 563 people who were originally assigned to the control group and not reassigned (i.e., realized controls); 648 people who were originally assigned to treatment and completed at least 2 weeks of the program; and 134 people who were reassigned from the control group to the treatment group.

Table 3 shows the characteristics of the realized control and treatment groups who completed the follow-up survey. Given the relatively small

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[19] A target sample size for the follow-up survey was determined by consultants hired by the program's executing unit before we were involved in the project. The target size was set to achieve a precision level of 3% for the estimated employment effect of the program, assuming a 50% employment rate among the control group, a confidence level of 95%, and a design effect due to stratification of 0.9, and assuming that a universe of 6,000 beneficiaries and 3,000 controls was available. (The latter assumption was incorrect: only about 4,800 beneficiaries and 1,630 controls were available.) These calculations yielded a sample size of 828 beneficiaries and 728 controls, which was the basis for the field work. A survey firm attempted to reach the target sample sizes (drawing names in order from a randomized list), but response rates were relatively low, and eventually the survey firm exhausted the control sample. The firm stopped field operations when 782 beneficiaries and 563 members of the control group were surveyed. Final response rates for the survey were very similar for the treatment and control groups (63.2% and 61.1%, respectively). The survey was administered in the respondents' homes.

Table 3
Characteristics of Evaluation Sample

	Realized Control	Realized Treatment Group	
	Group (1)	All (2)	Exclude Reassigned (3)
Baseline characteristics:			
1. Percent female	57.5	55.0	55.9
2. Percent ages 17–19	24.5	23.8	23.8
3. Percent ages 20–24	48.1	50.3	49.7
4. Percent ages 25+	27.3	26.0	26.5
5. Years schooling	9.2	9.3	9.3
6. Percent primary only	36.9	30.8	31.2
7. Percent married	21.0	17.4	18.5
8. Percent with dependents	18.7	23.0	24.1
9. No. in household	5.0	5.0	5.0
10. Percent with remittances	4.1	3.3	2.5
11. Percent with outdoor toilet	25.4	25.8	26.5
12. Percent enrolled	33.9	39.1	38.9
13. Percent employed	3.4	3.1	3.1
14. Percent with experience	14.4	16.2	15.3
Post-baseline outcomes:			
15. Percent employed at follow-up	56.0	57.4	56.3
	(2.1)	(1.8)	(1.9)
16. Monthly earnings at follow-up	2,677	3,133	2,961
	(149)	(146)	(148)
17. No. observations	563	782	648

NOTE.—See note to table 1. The realized control group includes those initially assigned to the control group and not reassigned. The realized treatment group includes those initially assigned to training who were not no-shows as well as those who were reassigned controls. Monthly earnings values are top-coded at 25,000 (the third highest value in the data). Standard errors are in parentheses.

sample sizes there are no significant differences in the mean characteristics of the realized control group (col. 1) and either the overall realized treatment group (col. 2) or the realized treatment group excluding the reassigned controls (col. 3). Interestingly, however, even in the smaller follow-up sample the ICAP dummies are significant predictors of treatment/control status, reflecting differences across ICAPs in the rates of assignment to the original treatment and control groups, as well as differences in no-show and reassignment rates.

IV. Impact Estimates and Extensions

A. Design of Follow-up Survey

Originally, the follow-up survey for the JE evaluation was scheduled to be conducted 6 months after completion of the classroom segment of the training. In practice, the survey was conducted between May and July of 2005. As part of the survey, members of the treatment group were asked to provide monthly information on their activities, starting from the month that they completed (or left) their program. Because of variation in the date of entry into the program and variation in the duration of training, the number of months of post–classroom training data available for members

of the treatment group ranges from 1 to 18, with a median of 13 months.[20] Members of the control group were asked to provide their monthly employment status starting from August/September 2004, which roughly corresponds to the median completion date for the trainees.

Information on the treatment group members who completed the follow-up survey enables us to estimate the fractions of the trainees in JE who completed the various phases of treatment. A total of 93.3% of the treatment group completed their classroom training, while 6.7% did not. Of the completers, 84.8% started an internship. Finally, of those who started the internship, 92.4% completed it. Thus, the completion rate for the entire classroom and internship program was 0.74 ($= 0.933 \times 0.848 \times 0.924$), which compares favorably with other training programs.[21] Members of the control group had no access to the JE, and there are no comparable training programs in the Dominican Republic for disadvantaged youths. Thus, the difference in the fractions of the realized treatment and control groups who completed the entire course of treatment is 0.74, which could be used to convert the "intention to treat" estimates presented below into estimated "local average treatment effects" for the compliers (i.e., members of the originally assigned treatment group who showed up for training and members of the reassigned control group).

B. Employment and Earnings: Basic Impacts

Our two basic measures of labor market outcomes in the follow-up survey are an indicator for being employed at the date of the survey and labor market earnings in the month prior to the survey in all jobs (which are equal to zero for nonworkers).[22] As shown in rows 15 and 16 of table 3, the realized treatment group had a slightly higher employment rate (0.574 vs. 0.560) and somewhat higher average earnings (3,133 pesos/month vs. 2,677 pesos/month). The 1.5 percentage point difference in employment rates is not statistically significant ($t = 0.5$), while the 455 peso difference in monthly earnings is significant at conventional levels ($t = 2.13$). Interestingly, the outcomes for the treatment group excluding the reassigned controls (col. 3) are slightly less positive, and they are not statistically significantly different from the outcomes from the control group.

As noted in the previous section, an important problem in the JE evaluation is the absence of follow-up data for the no-shows. This omission

[20] For 2.9% of trainees the survey was conducted less than 6 months after course completion; 14% were surveyed 6–9 months after; 21.6% between 10–12 months after; and 61.5% 13 months or more after.

[21] If no-shows and early dropouts are included in the calculation of the completion rate, it falls to 0.60, which is still relatively high.

[22] Just under 3% of those who are recorded as working report zero earnings. All those with earnings are recorded as employed.

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.[23] 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). Specifically, since the employment rate for the no-shows at the time of the follow-up survey has to lie between 0.0 and 1.0, the actual employment rate for the entire group who were assigned to treatment has to lie between $(1 - \rho)E^R$ and $\rho + (1 - \rho)E^R$, where ρ is the fraction of no-shows in the population who were assigned to treatment and E^R is the employment rate of those who remained in training (i.e., the "non-no-shows"). In the overall training cohort, $\rho = 0.150$ (including the reassigned controls in the trainee group). Using 57.4 as an estimate of E^R, the lower and upper bounds are 48.8 (delta-method standard error = 1.5) and 63.8 (standard error = 1.5). Thus, upper and lower bound estimates of the impact of training are −7.2 (standard error = 2.6) and +7.8 (standard error = 2.6)—too wide to be very informative about the effect of training.[24]

As an alternative to nonparametric bounds, we fit a series of regression models and parametric selection models, summarized in table 4. For reference, the first row of the table presents the unadjusted impacts on employment and monthly earnings. Rows 2 and 3 present estimates from linear regression models that include the 14 individual covariates presented in tables 1 and 3, along with 11 dummies for municipality (row 2) or 33 dummies for ICAP and residence in Santo Domingo (row 3). The addition of these covariates leads to slightly smaller impacts on employment for models that use the entire realized treatment group (col. 1) but slightly larger impacts for models that exclude the reassigned controls (col. 2). Thus, with the addition of individual covariates and either municipality or ICAP indicators the estimated employment effects are very similar from the two samples. Adding the covariates somewhat attenuates the estimated training effect on earnings for the sample that uses the entire realized treatment group (col. 3), reducing the magnitude enough to push the t-statistic below 2.

The regression-adjusted treatment effects in rows 2 and 3 of table 4 are valid under the twin assumptions that selection into the realized assignment groups is independent of any unobserved determinants of employment or earnings in the follow-up period, conditional on the observed

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[23] In principle the mean outcomes of the realized control group may also differ from the counterfactual mean that would have been realized if none of the control group had been reassigned. We ignore this problem for the moment but return to it later.

[24] This bound is based on the assumption that reassignment of the control group to training was random. If we drop the reassigned group from the realized treatment group, the bounds are −9.5 (2.6) and 7.9 (2.6).

Table 4
Impacts of Assignment to Training on Employment Outcomes

	Impact on Employment Rate		Impact on Monthly Earnings	
	All (1)	Exclude Reassigned (2)	All (3)	Exclude Reassigned (4)
A. OLS models fit to observations in follow-up survey only:				
1. No covariates	1.5	.4	455	284
	(2.7)	(2.9)	(213)	(211)
2. With covariates (individual covariates and 11 region effects)	1.1	.7	415	294
	(2.7)	(2.8)	(201)	(200)
3. With covariates and ICAP effects	1.3	1.1	390	288
	(2.7)	(2.9)	(205)	(204)
4. Reweighted (weight function depends on covariates and ICAP)	.7	.2	392	264
	(2.7)	(2.9)	(215)	(213)
B. Joint models for participation in training if assigned to treatment and outcomes in follow-up survey:				
5. Estimating correlation between equations				
a. Treatment effect*	7.7	6.3	471	389
	(5.2)	(6.3)	(517)	(474)
b. Correlation between latent errors (ρ)	−.60	−.49	−.05	−.09
	(.67)	(.66)	(.35)	(.33)
6. Using fixed values of correlation between equations implied treatment effects*:				
a. $\rho = -.4$	5.8	5.4	878	731
b. $\rho = -.2$	3.5	3.1	642	510
c. $\rho = .2$	−1.5	−1.9	190	78
d. $\rho = .4$	−4.1	−4.5	−45	−141
Mean of dependent variable	56.8	56.2	2,942	2,829

NOTE.—Standard errors are in parentheses. Entries in panel A are coefficients of assignment to treatment dummy in linear models for the probability of employment at follow-up survey (cols. 1 and 2) and the monthly labor earnings at follow-up survey (cols. 3 and 4). Entries in panel B are coefficients of assignment to treatment dummy for outcome of interest in a two-equation model for the event of attending training if assigned and labor market outcome in the follow-up survey. These models include individual covariates and 11 region effects. Joint models in cols. 1 and 2 combine a Probit model for attending training if assigned to treatment, and a Probit model for employment at the time of follow-up. Joint model in cols. 3 and 4 combine Probit model for attending training if assigned and linear model (with normally distributed error) for income. See the text for the complete description of the joint models. Models in cols. 1 and 2 include reassigned control group members in the treatment group. Models in cols. 3 and 4 exclude these individuals.
    * Reported treatment effect for employment is marginal effect on probability of employment.

covariates, and that our parametric regression models are correctly specified. To address concerns about the latter assumption, we implemented the reweighting procedure suggested by DiNardo, Fortin, and Lemieux (1996), using a logit model with the same controls as in the regression models in row 3 of table 4 to predict the probability of being observed in the realized treatment group, and then using these predicted probabilities to reweight the realized control group. Row 4 of table 4 shows

the implied estimates of the intention to treat effect (formed by sub-
tracting the mean outcome for the realized treatment group, minus the
reweighted mean for the realized control group). The estimated effects
on employment are a little smaller than the corresponding regression
estimates in row 3, while the effects on earnings are very close to the
regression estimates.

Based on the results in table 2 and our understanding of the imple-
mentation of the JE program, we believe that the most serious selection
issue is the potential nonrandomness of no-show behavior. To evaluate
this issue more formally, we fit joint models for the event of showing up
for training and for the labor market outcome in the follow-up period.
For employment, the joint model consists of two Probit equations, with
a correlation between the latent errors in the no-show and employment
equations. For earnings, the joint model consists of a Probit equation and
a linear regression model, with a correlation between the latent error in
the no-show equation and the residual component of earnings.[25] An issue
in these models is how to treat the expected outcomes for people who
were reassigned from the control group to the treatment group. Perhaps
the most natural assumption is that the reassigned controls who completed
training are selected in the same way as the originally assigned trainees
who completed training.[26] As an alternative, we drop the reassigned con-
trol group and fit the joint model to the originally assigned treatment
group and the realized controls.

Joint estimates of the intention to treat effect and the correlation be-
tween the no-show equation and outcome equation are presented in rows
5a and 5b of table 4.[27] Given the absence of any exclusion restrictions
between the selection equation and the outcome equation, the joint models
are unable to precisely estimate the two parameters. In all cases, however,

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[25] Note that we assume that there is no selection bias in the outcomes of the
realized control group. The models are slightly nonstandard because we observe
no-show behavior for everyone initially assigned to treatment but employment
outcomes only for the subset included in the follow-up survey. We build the
likelihood assuming that there were fixed probabilities of inclusion in the fol-
low-up survey ($563/1{,}623 = 0.35$ for the realized control group and $782/5{,}732 = 0.14$ for the realized treatment group).

[26] If the reassigned control group is nonrandomly selected because they had
to be available for training when called by the ICAP, then those who remained
in the control group are also nonrandomly selected. The extent of selection
depends on the fraction of the original control group who were offered reas-
signment and failed to take up the offer, and it is arguably small. For example,
assuming a non-takeup rate of 17% (equal to the no-show rate for the original
treatment group) and using the fact that 36.7% of the control group was reas-
signed, the implied fraction of the realized control group who were offered
treatment and failed to take it up is 12%.

[27] These models include the individual covariates and the region effects in-
cluded in the models in row 2 of table 4.

the point estimates suggest that people who completed training are neg-atively selected, as would be true if potential trainees who managed to find a regular job on their own were more likely to drop out of the program.[28] As a result, estimated treatment effects from the joint models are all larger in magnitude than the corresponding ordinary least squares (OLS) and reweighted estimates, though they are very imprecise.

As an alternative to a fully specified joint selection model, we follow Altonji, Elder, and Taber (2005) and compare the sensitivity of the estimate of the treatment effect to different assumptions about the correlation parameter (ρ). The entries in rows 6a and 6d in table 4 show the implied estimates for values of $\rho = -0.4, -0.2, 0.2$, and 0.4. (The estimates with $\rho = 0$ are virtually identical to the estimates from the corresponding OLS models shown in row 2.) Estimates of the effect of assignment to training on the probability of employment are increasingly positive for larger negative values of ρ and increasingly negative for larger positive values of ρ. However, estimates of the effect on earnings remain positive for values of ρ less than about one-third.

C. Impacts by Subgroup

A recurrent theme in the existing training literature is that there is sub-stantial heterogeneity in the impact of training. To explore this issue in the JE context, we divided the evaluation sample into two groups of approx-imately equal size by gender, age, education, and location and compared impacts for each subgroup. The results are summarized in table 5. In view of the limited sample sizes, we use the full sample of realized treatments and controls, and we report results from models with no covariates and from the specification used in row 2 of table 4, which includes controls for individual characteristics and municipality (but not for ICAPs) . The es-timated impacts on employment are all fairly close to zero, and there are no significant differences by gender, age, education, or location. The es-timated impacts on monthly earnings are fairly similar for men and women and for younger and older workers, but they show interesting patterns by education and location. In particular, the overall impact on earnings seems to be generated by a large positive effect for better-educated workers (adjusted impact $= 807$; $t = 2.54$) coupled with a minimal effect for the less educated. Cut by location, we also find a relatively large positive effect for residents of Santo Domingo (adjusted impact $= 804$; $t = 2.71$)

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[28] We also estimated the impacts of assignment to training separately for each ICAP and correlated these impacts with the no-show rate among those initially assigned to training. The estimated impacts are slightly negatively correlated with the no-show rate at the ICAP (weighted correlation of no-show rate with employment impact $= -0.10$; correlation with earnings impact $= -0.13$), which is consistent with the idea that those who completed training were negatively selected.

Table 5
Labor Market Outcomes and Estimated Impacts of Assignment to Treatment by Subgroup

	Mean Employment Rates		Estimated Assignment to Treatment Effects		Mean Monthly Earnings		Estimated Assignment to Treatment Effects	
			Unadjusted	With Covariates			Unadjusted	With Covariates
	Controls (1)	Treatments (2)	(3)	(4)	Controls (5)	Treatments (6)	(7)	(8)
By gender:								
1. Males (N = 591)	70.2 (3.0)	70.2 (2.4)	−.1 (3.8)	−.5 (3.9)	3,945 (259)	4,547 (252)	603 (373)	552 (370)
2. Females (N = 754)	45.4 (2.8)	47.0 (2.4)	1.6 (3.7)	2.0 (3.8)	1,743 (156)	1,975 (145)	232 (214)	248 (217)
By age:								
3. Age ≤ 21 (N = 670)	51.2 (2.9)	54.9 (2.6)	3.7 (3.9)	4.0 (3.9)	2,457 (208)	2,918 (201)	461 (293)	446 (284)
4. Age 22 (N = 675)	61.0 (3.0)	59.8 (2.4)	−1.2 (3.8)	−1.4 (3.9)	2,913 (214)	3,335 (210)	422 (310)	411 (296)
By education:								
5. Education ≤ 9 (N = 663)	57.1 (2.9)	55.3 (2.6)	−1.8 (3.9)	−2.8 (3.9)	2,574 (197)	2,598 (177)	24 (266)	−18 (254)
6. Education 10 (N = 682)	54.7 (3.0)	59.4 (2.4)	4.6 (3.9)	4.3 (3.8)	2,785 (225)	3,628 (255)	843 (330)	807 (318)
By location:								
7. Santo Domingo (N = 699)	54.3 (2.8)	59.6 (2.5)	5.2 (3.7)	3.7 (3.7)	2,455 (179)	3,585 (233)	1,130 (304)	804 (297)
8. Elsewhere (N = 646)	58.0 (3.1)	55.3 (2.5)	−2.7 (4.0)	−.1 (3.9)	2,956 (249)	2,692 (174)	−264 (296)	−20 (273)
By education and location:								
9. Santo Domingo and education 10 (N = 373)	51.0 (4.0)	61.5 (3.3)	10.5 (5.2)	10.3 (5.2)	2,461 (260)	4,056 (342)	1,594 (462)	1,345 (455)
10. All others (N = 972)	57.8 (2.4)	55.9 (2.1)	−2.0 (3.2)	−2.2 (3.2)	2,759 (181)	2,776 (150)	17 (234)	23 (220)

NOTE.—Estimated standard errors are in parentheses. Assignment to treatment effects are estimated from linear models with no additional covariates (cols. 3 and 7) or with the same list of covariates included in the models in row 2 of table 4 (cols. 4 and 8).

coupled with a minimal effect for those outside the capital city. If we compare 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 this is interesting, we 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.

We also estimated a series of quantile regressions of earnings on a dummy for assignment to treatment (Heckman, Smith, and Clements 1997). Under the assumption that training preserves the rank of different individuals in the earnings distribution, the estimate for a specific quantile can be interpreted as the causal effect of training on earners at that quantile (e.g., Bitler, Gelbach, and Hoynes 2006). These models showed relatively stable treatment effects beyond the 50th percentile for the overall sample and larger but also relatively stable effects across quantiles for better-educated applicants and those from Santo Domingo. Based on these results, we infer that the JE program raised rates of earnings (for those that found a job) relatively homogeneously, with most of the effect concentrated among a subset of better-educated applicants in the capital city.

D. Impacts on Wages, Hours, and Health Insurance

So far the pattern of evidence suggests that the JE program had little or no effect on the likelihood of employment but that it had a modest positive impact on earnings. Given these results, it is useful to examine how the program affected the components of earnings (i.e., wages vs. hours). Comparisons of wages or other conditional outcomes are problematic when the intervention affects the probability of work. In the case of JE, however, the program appears to have had little or no employment effect, implying that wage comparisons between the groups are potentially valid.[29] Table 6 summarizes a series of models fit to hours per week conditional on working, log monthly earnings, and log hourly wages (both conditional on working) and on the probability that the individual was offered employer-sponsored health insurance (with zeros for nonworkers). Overall, it appears that the JE program had no effect on hours per week but had a 7%–10% effect on hourly wages and monthly earnings conditional on working. As with the level of monthly earnings, the impacts on log wages and log earnings conditional on working are on the

[29] Formally, people who report wages are a selected subset of the population, and if the experiment affects the probability of working it may change the relative amount of selectivity bias in the observed wages of the two groups. Lee (2008) presents an informative procedure for bounding the size of any wage effects when there is an employment rate difference. When there is no employment gap and employment is determined by a single index selection model, simple (unadjusted) comparisons of wages are valid.

Table 6
Impacts of Assignment to Treatment on Other Labor Market Outcomes

	Mean Outcomes		Estimated Assignment to Treatment Effects			
			All Realized Treatments		Excluding Reassigned	
	Controls (1)	Treatments (2)	Unadjusted (3)	With Covariates (4)	Unadjusted (5)	With Covariates (6)
1. Hours/week conditional on working	39.5 (1.1)	39.7 (.9)	.2 (1.4)	−.7 (1.4)	−.2 (1.5)	−1.1 (1.5)
2. Log monthly earnings conditional on working (× 100)	826.1 (4.4)	837.1 (3.7)	11.0 (5.8)	9.4 (5.5)	9.5 (5.9)	8.3 (5.7)
3. Log hourly wage conditional on working (× 100)	477.9 (4.0)	486.6 (3.4)	8.6 (5.2)	9.9 (5.4)	7.5 (5.4)	9.6 (5.6)
4. Have employer health insurance (unconditional; %)	16.9 (1.6)	19.9 (1.4)	3.1 (2.2)	2.7 (2.2)	2.1 (2.2)	2.2 (2.3)
5. Average employment in 10 months after training	47.6 (1.7)	51.0 (1.6)	3.4 (2.3)	2.4 (2.3)	2.5 (2.4)	2.2 (2.4)

NOTE.—Estimated standard errors are in parentheses. Assignment to treatment effects are estimated from linear models with added covariates (cols. 3 and 5) or with the same list of covariates included in the models in row 3 of table 4 (cols. 4 and 6). Samples for hours/week exclude people with zero or missing hours (N = total sample size = 754 using all realized treatments and N = 680 excluding reassigned treatment group). Samples for log monthly earnings and log hourly wage exclude people with zero or missing earnings or hours (N = 744 using all realized treatments; N = 660 excluding reassigned treatment group). Samples for average employment in 10 months after training exclude trainees who had not left training by August 2004 (N =1,211 using all realized treatments; N = 1,097 excluding reassigned treatment group).

margin of conventional significance levels. They are also robust to inclusion or exclusion of the subset of trainees who were reassigned from the control group. The estimated impacts on the probability of health insurance (an indicator of "job quality" and also a marker for employment in the formal sector) are also positive, but these are less significant, with t-ratios of around 1. Finally, we also show in row 5 of the table the effect of JE on the fraction of months worked in the 10 months between completion of the training program and the follow-up survey. (We use the period from August 2004 to May 2005, excluding trainees who were not out of training by August 2004.) This analysis shows a slightly larger point estimate of the effect of training on post-program employment, though the estimates are still all insignificant.

Although the estimated impacts of JE on hourly wages and log earnings are not statistically significant, the magnitudes of the point estimates are economically important. In particular, a 10% increase in earnings, conditional on working, is equivalent to a treatment effect on earnings in the month prior to the survey of about RD$270 (or US$10 per month)—similar to the range of estimates in table 4. The estimated cost of the JE program was about US$330 per trainee. Thus, a 10% impact on wages is potentially in the range where the program could be considered cost effective. Unfortunately, given the imprecision of the estimated earnings impacts and the absence of longer-term follow-up data, it is impossible to reach a definitive conclusion regarding the cost-benefit performance of the JE program.

E. Quality of the Training Institutions

A natural hypothesis is that higher-quality training will have a bigger impact on participant outcomes.[30] Information on the quality of different ICAPs was obtained from a supervision system set up by INFOTEP (the National Training Institute). For each ICAP we know whether or not it was a member of the INFOTEP network, and if so, the quality grade assigned by INFOTEP for the institution. Of the 33 ICAPs contracted for training, 22 were certified by INFOTEP and 11 were not (however, 80% of trainees attended a certified ICAP). Among the certified ICAPs, 10 received the minimum grade, 6 received a medium rating, and 3 received the maximum rating.[31] We tried to test whether the impact of training

[30] This hypothesis was validated in a similar program in Peru by Chong and Galdo (2006), who have a large data set of trainees and training provider characteristics. They document larger positive impacts for higher-quality training providers. Burghardt and Schochet (2001) present data on the variation in Job Corp effectiveness by site.

[31] The share of trainees—among those who enrolled with a certified ICAP—was 37% at ICAPs with a low rating, 50% at those with a medium rating, and 8% at those with a high rating. The remaining 5% of trainees attended one of the two certified ICAPs that were not rated by INFOTEP.

was related to the "quality" of the ICAP by dividing enrollees into those who were assigned to ICAPs with different INFOTEP ratings (treating nonmembers as a fourth category). To account for local variation in other unobserved factors that may be correlated with quality, we assigned the controls to the ICAPs they would have trained with if they had been in the treatment group. Comparisons between treatment and control outcomes within each quality group showed no evidence of a large or systematic "quality effect."[32]

F. Dynamic Employment Impacts

The designers of the JE program specified "increased employability" as an objective of training. One interpretation of this goal is that training would raise the probability of moving from nonemployment to employment and lower the probability of moving from employment to nonemployment. In this section we use retrospective data on monthly employment outcomes collected in the follow-up survey to test whether people assigned to JE training had different employment transition rates than members of the control group. We also use a similar model to examine the effects of the JE program on transitions into and out of jobs with employer-provided health insurance—the only measure of "job quality" for which we have a continuous record of monthly outcomes in the period between the end of training and the follow-up survey.

While the results from the preceding analysis show little or no effect of training on the likelihood of employment at the time of the follow-up survey, this does not necessarily mean that the program had negligible impacts on employment (or health insurance) transition rates. As emphasized by Ham and Lalonde (1996), a program like JE that requires trainees to withdraw from the regular labor force can have an effect on employment status at the close of the program (i.e., when trainees finish their apprenticeship). In the presence of state dependence, this "initial conditions effect" will continue to influence subsequent labor market outcomes over and above any program impact on post-program transition rates. A full understanding of the impact of the program therefore requires a model that can disentangle the initial conditions effect from the post-program effects on transition rates.

For our dynamic analysis, we restrict attention to members of the realized treatment group who had completed their training (or dropped out) by August 2004. This restriction eliminates some 17% of the treatment group, leaving us with a final sample of 651 people in the realized treatment group and 563 people in the realized control group, all of whom

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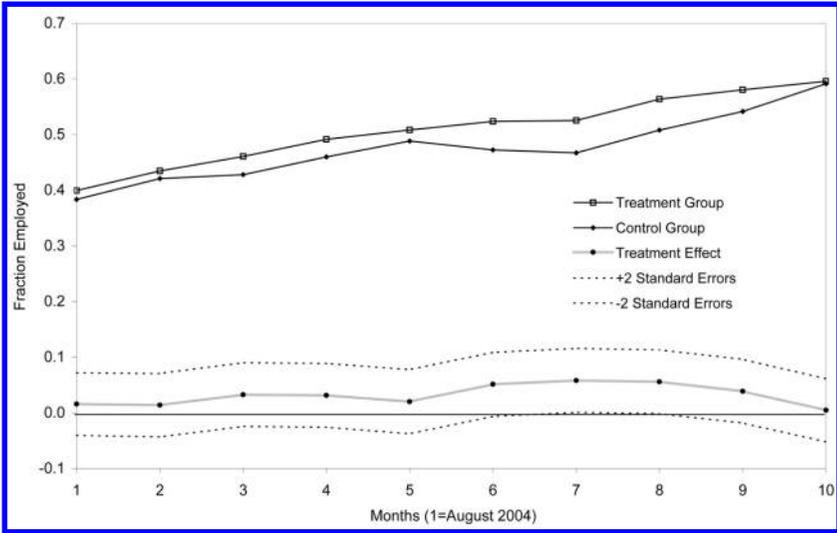[32] The results from these analyses are available upon request.

FIG. 1.—Employment rates

reported employment data from August 2004 to May 2005.[33] Figure 1 shows average monthly employment rates for the treatment and control groups during each month of this period, along with the differences in employment rates for each month, and a 95% confidence interval around the differences. As suggested by the estimated employment impacts at the time of the follow-up survey (in tables 3 and 4), there is no indication of an overall treatment effect by the end of the period, though the impacts in months 6–8 are positive and marginally significant.

Figure 2 is a similar graph of the fractions of people in the treatment and control groups with employer-provided health insurance. On average the treatment group has a 3–4 percentage point higher coverage rate than the control group over most of the post-training window, suggesting a small positive impact of training that tails off slightly by month 10 (the month referred to for the analysis in table 6).

Our dynamic model of the effect of the JE program has two key components: a model for the initial condition in "month 1" (August 2004)—which we interpret as a period just after the end of training—and a second model for the rate of employment transitions over the next 9 months. In

[33] The follow-up survey was administered between May and July of 2005. Thus, everyone in the survey reports data for the calendar period from September 2004 to May 2005, though depending on the month they were surveyed this period may have ended just prior to the survey, 1 month before, or 2 months before.
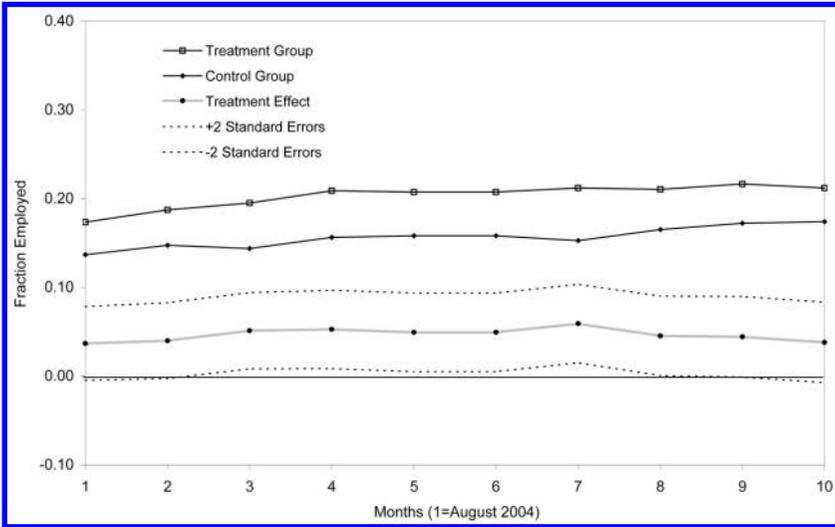
Fig. 2.—Fractions with employer-provided health insurance

this setting, the JE program has two types of potential effects: an effect on employment (or health insurance coverage) in month 1, which could be negative if training takes someone out of the labor force, and an effect on the subsequent transition probabilities.

To proceed, let y_{it} represent the employment/health insurance status of person i in month t, let X_i represent a set of observed baseline covariates for individual i, and let T_i be an indicator for program status. The statistical problem is to develop a model for

$$\Pr(y_{i1}, y_{i2}, \ldots y_{i10}|T_i, X_i) =$$

$$\Pr(y_{i1}|T_i, X_i) \times P(y_{i2}, y_{i3}, \ldots y_{i10}|y_{i1}, T_i, X_i). \tag{1}$$

We assume that there is unobserved heterogeneity across the population, represented by the random effect α_i. For simplicity, we assume that the distribution of the random effects is identical for the realized treatment and control groups.

In the absence of the JE program, we assume that in months 2–10, the probability that person i is employed (or has insurance) in month t depends on α_i and on a linear trend, the observed X's, and employment/coverage status in the previous month:

$$\Pr(y_{it} = 1|y_{it-1}, T_i = 0, X_i, \alpha_i) =$$

$$\Pr(\beta_0 + \beta_1 t + X_i\beta_x + \lambda y_{it-1} + \alpha_i + e_{it} \geq 0), \tag{2}$$

where e_{it} is an independent and identically distributed logistic random variable. This implies that

$$\Pr(y_{it} = 1 | y_{it-1}, T_i = 0, X_i, \alpha_i) =$$

$$\text{logit}(\beta_0 + \beta_1 t + X_i \beta_x + \lambda y_{it-1} + \alpha_i), \tag{3}$$

where $\text{logit}(z) = \exp(z) / [1 + \exp(z)]$ is the logistic distribution function.

For people in the treatment group we assume that exposure to treatment potentially increases "employability." This is captured by two treatment effects: a potential increase in the probability of being employed or covered by health insurance in period t if the person was not working/covered in period $t - 1$, and a potential increase in the probability of being employed or covered in period t if the person was working or covered in period $t - 1$. Formally, we assume that

$$\Pr(y_{it} = 1 | y_{it-1}, T_i = 1, X_i, \alpha_i) = \tag{4}$$

$$\text{logit}(\beta_0 + \beta_1 t + X_i \beta_x + \lambda y_{it-1} + \phi_0 (1 - y_{it-1}) + \phi_1 y_{it-1} + \alpha_i).$$

The parameter ϕ_0 represents the effect of the JE program on the probability of moving from nonwork to work (or noncoverage to coverage), while ϕ_1 is the effect on the probability of remaining employed (or covered by insurance).

We assume that the distribution of the random effects can be approximated by a point mass distribution with three mass points. Thus, α_i is a random variable that takes the values $\{\alpha_1, \alpha_2, \alpha_3\}$ with probabilities $\{\pi_1, \pi_2, \pi_3\}$. We jointly estimate the location of the mass points and their probabilities.[34] Finally, we assume that the probability that the individual is employed or covered in August 2004 is given by

$$\Pr(y_{i1} = 1 | T_i, X_i, \alpha_i) = \text{logit}(\gamma(\alpha_i) + \mu X_i \beta_x + \delta T_i), \tag{5}$$

where $\gamma(\alpha_i) = \gamma_j$ (for $j = 1, 2, 3$) represent unrestricted constants for each point of support of the random effect, μ is a parameter that rescales the index of effects of the covariates, and δ represents the treatment effect on the probability of employment/coverage in month 1.[35] We experimented with models that allow completely independent coefficients for

[34] The use of a point-mass distribution to approximate the distribution of unobserved heterogeneity was popularized in econometrics by Heckman and Singer (1984). Our model is similar to ones used in Card and Sullivan (1988) and Card and Hyslop (2005).

[35] One of the points of support is normalized to have $\alpha_i = 0$, since there is an unrestricted constant in the employment model (2). There is no constant in the initial conditions model (5) so each point of support has a separate value for γ_j.

Table 7
**Parameter Estimates from Dynamic Models for Employment
and Health Insurance**

Model Parameter	Model for Employment during Month (1)	Model for Health Insurance during Month (2)
1. Constant (β_0)	−1.99	−2.43
	(3.43)	(4.36)
2. Trend (β_1)	.06	−.03
	(.02)	(.03)
3. State dependence parameter (λ)	4.67	7.00
	(.14)	(.31)
4. Treatment effect if not employed in previous period (ϕ_0)	.03	.24
	(.10)	(.20)
5. Treatment effect if employed in previous period (ϕ_1)	.13	.18
	(.14)	(.27)
6. Treatment effect on initial condition in August 2004 (δ)	.07	.33
	(.15)	(.20)
7. Dummy for males	.73	.71
	(.11)	(.27)
8. Dummy for ages 20–24	.37	.41
	(.11)	(.20)
9. Dummy for ages 25+	.60	.57
	(.13)	(.25)
10. Loading factor for covariates in model for initial condition (μ)	1.33	1.89
	(.26)	(.66)
11. Log likelihood	−3,630.7	−1,536.3
12. No. parameters	17	17

NOTE.—Standard errors are in parentheses. Models include mass point random effects with three points of support. Each mass point j has a probability (π_j), an intercept (α_j) in the monthly outcome model, and an intercept (γ_j) in the probability of the outcome in August 2004 (the initial condition). For the employment model, the mass points are ($\pi_j, \alpha_j, \gamma_j$) = (0.159, 0, −0.060), (0.782, −1.337, −1.921), (0.059, 2.011, 2.171). For the health insurance model, the mass points are ($\pi_j, \alpha_j, \gamma_j$) = (0.064, 0, −0.989), (0.928, −2.660, −3.687), (0.008, 2.155, 1.870).

the X's in the model for the initial condition but found that the "single index" restriction imposed by (5) fits well.[36]

   We fit a number of versions of this model to the sequences of monthly employment outcomes of the treatment and control groups, including models without any covariates, and other specifications with controls for various combinations of gender, age, education, and region. Estimates from a representative specification are presented in column 1 of table 7. This model includes three observed characteristics: a dummy for males,

[36] For example, the chi-square statistic comparing the restricted and unrestricted employment models is 1.6 with 2 df. We estimated models with four and five points of support and found only small (and statistically insignificant) increases in the log likelihood (−3,630.54 and −3,630.51, respectively, relative to −3,630.71 for the baseline model). More importantly, the estimates of the other parameters are very similar whether we include three, four, or five points of support.

a dummy for ages 20–24, and a dummy for ages 25 and older (with the omitted category being ages 17–19). The main parameter estimates are very similar from specifications with no covariates or with a longer list of covariates. In column 2, we also show estimates from a parallel specification fit to the sequence of indicators for having a job with employer-provided health insurance.

   As one might expect if the impacts of training on getting a job and on getting a good job are alike, the parameter estimates from the two models are similar, though there are some interesting differences. Consistent with the patterns in figures 1 and 2, the model in column 1 of table 7 has a positive trend, while the trend in the model for employment with insurance is negligible (see row 2). Males are more likely to be employed in any month and to be employed at a job with insurance (row 7). Likewise, older workers have higher probabilities of employment or employment with health insurance (rows 8 and 9). The estimates of the "loading factor" μ (row 10) suggest that the covariates combine in a similar way to affect the probabilities of employment in months 2–9 and in month 1. Finally, both outcomes exhibit large and statistically significant state dependence: the estimate of λ is 4.67 for employment and 7.00 for employment with insurance.

   Given the absence of a large or systematic gap in the employment rates of the treatment and control groups (fig. 1), it is not surprising that the estimated treatment effects for employment are small and imprecise (rows 4–6). The point estimates suggest that any treatment effect is concentrated on the job retention rate, though the t-statistic is only about 1. The estimated treatment effects for the probability of having a job with health insurance are larger, though still relatively imprecise. Training appears to have raised the probability of holding a job with health insurance during August 2004 ("month 1"), as well as the rates of moving into a job with insurance and of holding onto such a job.

   Further insights into the predictions of the dynamic model for health insurance coverage can be discerned in figure 3. This figure shows the actual difference between the treatment and control groups in the likelihood of a job with insurance (shown by the black squares), as well as the predicted differences from the model (the heavy line). We also show the predicted difference under the assumption that treatment only affected the "initial condition" in month 1 (the dashed line) and under the assumption that treatment only affected the initial condition and the probability of retaining a job with health insurance (the lighter solid line). Looking at month 10 (i.e., May 2005), the predicted treatment effect is around 4.5 percentage points, of which about two points can be attributed to the impact of treatment on health insurance status in month 1, another point can be attributed to the impact of treatment on the likelihood of retaining a job with insurance, and the remained (about 1.5 points) can be attributed to the treatment effect on the likelihood of moving from
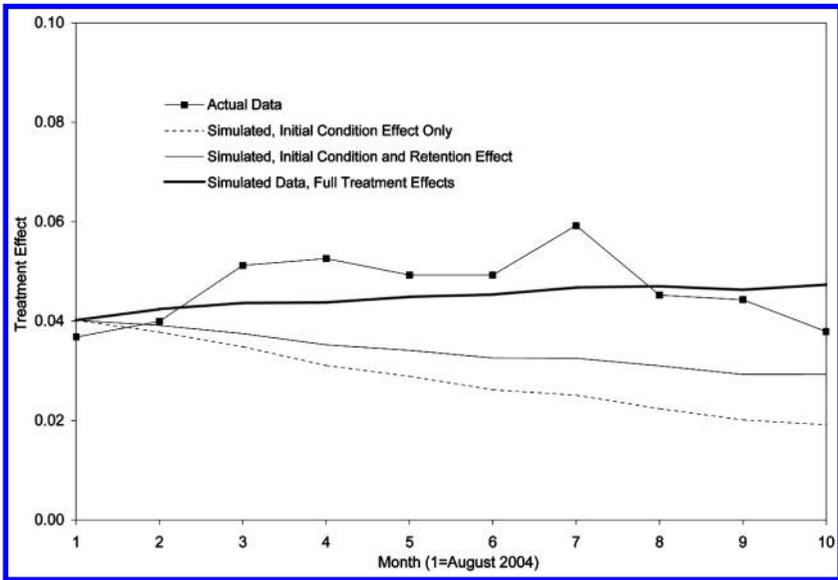
FIG. 3.—Actual and simulated treatment effects on probability of health insurance.

no insurance to insurance. The relatively large contribution of the initial insurance status in month 1 suggests that training helped the trainees move to better jobs almost immediately—perhaps through employment at the firm that offered on-the-job training. Nearly one-half of the overall effect on the likelihood of holding a job with health insurance at the end of the follow-up period is attributed to this initial condition effect.

V. Interpretation and Conclusions

This article presents one of the first evaluations based on an experimental design for a job training program in Latin America. Previous evaluations of similar programs, based on observational designs, typically report positive and statistically significant impacts of training on the probability of having a job and on labor earnings. In contrast, we find that the Juventud y Empleo program in the Dominican Republic had no significant effect on employment. There is evidence of a modest (10%) impact on earnings per month (conditional on employment), although the estimated effect is only marginally significant ($t = 1.5$).

Although our evaluation is based on a randomized design, in the implementation of the experiment some people who were initially assigned to training dropped out, and they were not included in the survey of post-program outcomes. The design was also complicated by the pro-

cedure of reassigning control group members to the treatment group—a process that was implemented site by site, introducing additional complexity. We address these potential problems by using a richly specified regression model to adjust for observable differences in the observed treatment and control groups and by fitting a parametric selection model that incorporates potential correlation between the propensity to drop out and the unobserved determinants of labor market outcomes in the follow-up survey. We also present results with and without the subgroup of trainees who were reassigned from the initial control group. It is possible that there are remaining biases in our experimental contrasts, although taking the evidence as a whole we believe these biases are probably small. Nevertheless, the credibility of the overall evaluation is not as high as it would have been with better implementation and a simpler design.[37]

This article also contributes to the literature by providing an operational definition for "employability," based on transition probabilities between employment and nonemployment status. Building on this definition, we fit a logistic model with state dependence and unobserved heterogeneity for the observed employment transitions of the treatment and control groups. The results of the model suggest that the JE program had no significant impact on trainee employability, although a similar model shows a modest impact on job quality, as measured by the probability of holding of a job that offers health insurance.

Our finding that the Juventud y Empleo training program had (at best) relatively modest effects on participants' labor market outcomes is consistent with the results from evaluations in many developed countries. Although it may be possible to improve the effectiveness of the Juventud y Empleo program in the Dominican Republic, and of similar programs in other Latin American and Caribbean countries, it is unlikely that programs of this nature, operating under similar financial and operational constraints, can fully address the many barriers and problems faced by disadvantaged youth in the region. In any case, the results from this evaluation suggest that it is important that job training programs be closely tracked and rigorously evaluated.

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[37] These complications highlight the importance of a careful implementation of experimental designs: random assignment is the first step, but the treatment of noncompliance, the reassignment procedures, and reliable and complete data collection on a representative sample of the set of eligible participants over which the random assignment took place are equally important.

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