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David Card
Pablo Ibarraran
Ferdinando Regalia
David Rosas
Yuri Soares

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ABSTRACT

This paper summarizes the findings from the first randomized evaluation of a job training program in Latin America. Between 2001 and 2005 the government of the Dominican Republic operated a subsidized training program for low-income youth in urban areas. The program featured several weeks of classroom instruction followed by an internship at a private sector firm. A random sample of eligible applicants was selected to undergo training, and information was gathered 10-14 months after graduation on both trainees and control group members. Although previous non-experimental evaluations of similar programs in Latin America have suggested a positive impact on employment, we find no evidence of such an effect. There is a marginally significant impact on hourly wages, and on the probability of health insurance coverage, conditional on employment. Finally, we develop an operational definition of the impact of training on "employability" in the context of a dynamic model with state dependence and unobserved heterogeneity. Consistent with our main results, we find no significant impact of the training program on the subsequent employability of trainees.

David Card
Department of Economics
549 Evans Hall, #3880
UC Berkeley
Berkeley, CA 94720-3880
and NBER
card@econ.berkeley.edu

Pablo Ibarraran
Inter-American Development Bank
1300 New York Avenue
Stop B0700
Washington, DC 20577
pibarraran@iadb.org

Ferdinando Regalia
Inter-American Development Bank
1300 New York Avenue
Washington, DC 20577
ferdinandor@iadb.org

David Rosas
1300 New York Avenue
Washington DC 20577
davidro@iadb.org

Yuri Soares
1300 New York Avenue
Washington DC 20577
yuris@iadb.org

1. Introduction

During the 1990s the Inter-American Development Bank financed a series of innovative training programs throughout Latin America targeted at less-educated youth – a group that faces considerable barriers to labor market success in developing and developed economies.[1] Drawing on lessons from the Job Training Partnership Act in the U.S. and the Youth Training Scheme in Britain, the programs combined classroom training with a subsequent internship period of on-the-job work experience.[2] Unlike earlier training schemes in the region, the programs also placed a heavy emphasis on the private sector, both as a provider of training and as a demander of trainees. Private training contractors were encouraged to participate in the provision of training through a competitive bidding process. Proposals for training programs had to be backed by commitments from local employers to offer internships of at least two months duration.

Among the IABD-sponsored programs, the *Juventud y Empleo* (JE) program in the Dominican Republic was unique in incorporating a randomized design to allow for a highly credible evaluation of the program's effects. This paper summarizes the impacts of JE on a wide range of labor market outcomes, including employment, hours of work, 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, and on their ability to find and hold jobs with health insurance coverage.

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[1] See Heckman, Lalonde, and Smith (1999) for a general overview of training programs, and Betcherman, Olivas and Das (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 Heckman, Lalonde and Smith (1999). Dolton, Makepeace and Treble (1994) describe the Youth Training Scheme.

Our analysis is based on a sample of program participants from the second cohort of the JE program who received training in early 2004.[3] Baseline data were collected from a registration form completed by program applicants prior to randomization. A follow-up survey was administered in the period from May to July of 2005, 10 to 14 months after most trainees had finished their initial course work. Simple experimental comparisons between the people who received treatment and those in the control group suggest a negligible impact on employment, although there is some indication of a possibly larger impact in certain regions of the country. The lack of an overall effect is confirmed by our dynamic models, which show very small effects on employment transition rates. In contrast to the small effects on the likelihood of work, we estimate that JE increased the average monthly earnings of participants by about 10%, although the effects are imprecisely estimated and only marginally significant.

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 3. The data and basic statistics are described in section 4. Section 5 presents the results, and conclusions are presented in Section 6.

2. Previous Research on Labor Market Training Programs

Few public policies have been studied and evaluated as rigorously as job training programs.[4] Most of the existing evidence is derived from programs in the United States and Europe. In the U.S. case, particularly credible evidence is available from randomized

[3] The first JE cohort was a smaller, pilot cohort, for which information was not collected in a rigorous and systematic manner.
[4] 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 emphasized the case for the use of randomized experiments in training program evaluations.

evaluations of the Job Partnership Training Act (see Bloom et al., 1997; GAO, 1996; Heckman, Lalonde, and Smith, 1999), the Job Corps (Burghart et al. 2001), and a series of programs for welfare recipients (Friedlander, Greenberg, and Robins, 1997). One conclusion that emerges from the U.S. and European literature is that the impacts of job training are generally modest, at best. A second key finding is that the effectiveness of training varies with the characteristics of participants and the type of training. For example, many studies have concluded that women benefit more from training than men.[5] On-the-job training is often thought to be more effective than classroom training, although this is by no means a universal finding.[6] Voluntary programs are generally found to be more effective than mandatory programs (Friedlander, Greenberg, and Robins, 1997). Finally, in the case of work experience programs, private sector programs are found to be more effective than public sector programs (Kluve et al, 2006).

With respect to youth, randomized evaluations from the two main programs serving disadvantaged youth in the U.S. – the Job Partnership Training Act (JPTA) and the Job Corps – yield quite different results. The short-run impacts for young women in JPTA are essentially zero (although the longer-term impacts appear to be more positive – see GAO, 1996), while the short-run impacts for young men are negative. In contrast, the Job Corps had a significantly positive effect on both genders. Lee (2005) for example, shows that Job Corp had about a 12 percent effect on earnings three years after training.

The European evidence is far more uncertain (Heckman, Lalonde, and Smith, 1999) in part because of the lack of experimental studies and the wide variation in

[5] See Friedlander, Greenberg, and Robins (1997).
[6] See Heckman, Hohmann, Khoo and Smith (2000). The Job Corps –a largely classroom training program for disadvantage youth– has been found to be relatively effective: see Burghardt et al. (2001).

evaluation methods.[7] Nevertheless, one key finding that emerges from the meta-analysis by Kluve et al. (2006) is that programs serving youth are substantially less likely to show positive impact effects than programs for adults.

Evidence on the effectiveness of training in developing countries is more limited. Betcherman, Olivas and Amit Dar (2004), for example, review 69 impact evaluations of unemployed and youth training programs, only 19 of which are in developing countries. Of those 5 are specific to youth training -- all in Latin America. Betcherman et al. (2004) conclude that training impacts in Latin America are more positive than the impacts of programs in the United States and Europe.[8] 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.[9]

While there are a number of existing studies of training programs in Latin America, to the best of our knowledge all of these have used non-experimental methods – most notably propensity score matching methods. And the positive results notwithstanding, as in European case the variability in methods and data have produced widely varying results, even for the same program. A case in point is Peru's youth

[7] The British YTS case is emblematic of this dispersion in results. Studies such as Main and Shelley (1990) and Main (1991) document positive results on short-term employment in the neighborhood of 11-17 percent. On the other hand, studies such as Whitfield and Bourlakis (1990) find smaller impact on employment, of 4 percent, while Dolton et all (1994) find negative impacts on employment of between 4 and 17 percent. In a recent meta-evaluation of active labor market programs, which is based on a sample of 95 impact evaluations across European countries, Kluve et al (2006) found a tendency for "overly optimistic" results in the non-experimental evaluations.

[8] All of these evaluations (Argentina, Brazil, Chile, Peru and Uruguay) correspond to youth programs that have common features with the Dominican Republic program.

[9] Weller (2004) also looks at Latin America training programs, but in the context of all active labor market programs.

training program: seven evaluations have produced a very wide range of estimated impacts for this program.[10]

Two other problems are common in much of the literature, and are shared by our evaluation of the JE program. First, few studies present estimated impacts beyond two or three years post-training. 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.[11] A second limitation is the paucity of information on program costs, and of other possible program effects, such as general equilibrium spillover or "crowding" effects (Heckman and Carneiro, 2003).

3. LAC Training Programs and *Juventud y Empleo*

a. Background Context

Job training programs 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, many countries adopted a centralized model for training provision, organized through a national training institute (NTI).[12] Program content was usually dictated by NTI's, and services were targeted to more highly skilled workers who were already employed in the sectors favored by the import substitution growth strategy.

Since the abandonment of the import-substitution model in the early 1980s, the role and *modus operandi* of the NTI's have been re-evaluated. NTI's have been under

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[10] These evaluations used data generated for different cohorts. The estimated earnings impact 6 months after treatment ranged from 12% to 60%. Impact at after 18 months ranged from 13% to 40%.
[11] A notable exception is the JPTA evaluation. The GAO obtained data for five years after random assignment (GAO, 1996). These data have unfortunately never been released to other researchers. Longer term data are also available for the British YTS program. Dolton (2004) tracks beneficiaries 11 years after the training was completed.
[12] NTI's in this era included SENA in Colombia, SENAI in Brazil, SNPP in Paraguay, INFOTEP in Dominican Republic,SENATI in Peru and INAFORP in Panama.

pressure to adopt a “demand-driven” model of training, based on a separation between the financing, planning, and delivery of training services, and the active participation of local employers in the selection of providers and program content.

Two influential programs, the Mexican *PROBECAT*, which started in 1984, and *Chile Joven*, which started in 1992, have laid the groundwork for this new generation of programs. In *PROBECAT*, trainees often receive classroom training in the firms where they carry out their internships. Variants of this “Mexican model” were adopted in Central America (Honduras and El Salvador for example) during the late 1990s. Under the alternative “Chilean model,” trainees generally receive technical/vocational training at an independent provider, followed by an internship at a private sector firm. Variants of this plan 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 (JE) was developed and implemented by the Government of the Dominican Republic with financial support from the IADB. The first phase of the program – which ran from 2001 to 2006 – was targeted to low-income youth (ages 18 to 29) with less than a secondary education (i.e. no more than 11 years of completed schooling) who were not currently enrolled in school. Special attention was directed to enrolling women. The stated objective of the program was to increase “employability” of 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 tailoring program content to the needs of local employers.[13]

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 and split into two parts: basic skill 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.

ICAPs were selected through a competitive process. Proposals from potential training providers were required to include written commitments from one or more firms to offer a two-month internship to all trainees graduating from the provider's program. This was supposed to ensure that the ICAP was offering training that would be of value to local employers.[14] The original project design also required ICAPs to follow-up on the trainees during the internship period to provide counseling and technical assistance. In practice, this follow-up was limited.

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

[13] See "*Reform and Labor Training Program"* Project Document (1183/OC-DR). Inter-American Development Bank.

[14] It should be noted that, during program execution, 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 different firms than those originally contacted by the ICAPs

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.

Trainees were not paid during the program, but the program did provide a partial reimbursement of transportation costs and meals, up to a maximum of 50 Dominican Pesos per day (roughly two dollars). The daily stipend was well below the market wage rate available for a typical trainee, who earned about 4,500 Pesos per month in the post-training follow-up survey (more than four times the stipend). The program also provided insurance against accidents on the workplace for all trainees.

c. Implementation

The original JE design specified that individuals interested in receiving training would submit applications through a local employment office of the Minister of Labor, where personal information would be gathered and checked against eligibility rules before being sent to a central office for random assignment. In practice, the local offices did not have the capacity to perform this function, and the enrollment task was taken on by the ICAP's. Staff from the Ministry of Labor and the ICAPs conducted outreach and information programs in the 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*" (announcements by vehicle-mounted loudspeaker), radio advertisements, and contacts with churches and other community groups.

Applicants for a training position 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., unemployment status and education achievement) 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 training.[15] The other 10 were assigned to the control group. ICAPs were allowed to reassign up to five people from the control group to the treatment group, in the event that one or more of the original treatments failed to show up for training ("no-shows") or dropped out within the first two weeks of the course ("dropouts"). No-show and dropout rates were relatively high, and over one-third of the original control group was reassigned to treatment status.

For the second cohort of the JE program, information was submitted by the ICAP's for a total of 8,391 eligible applicants who were applying to receive training in early 2004. Of these 5,802 (69.1%) were originally assigned to the treatment group, and 2,589 (30.9%) were assigned to the control group. A total of 1,011 of the treatments were either dropouts or no-shows, and 966 original members of the control group were re-assigned to the treatment group. Thus, a total of 1,623 people remained in the control group, while 5,757 (=5802-1011+966) were in the treatment group and received at least 2 weeks of training. We refer to these as the "realized" control and treatment groups, respectively.

[15] 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, (iii) both. Due to the difficulties in implementing this scheme the program was simplified to have only one treatment and one control group.

4. Basic Data Description

a. The Evaluation Sample

Although baseline information was collected on all applicants to the JE program, follow-up information was only collected for a subsample of the "realized" treatment and control groups. This subsample was drawn by stratified sampling (using age, gender, and education classes as strata) from administrative lists of the realized treatment and control groups, and includes 563 controls and 786 treatments.[16]

The fact that data were collected from the realized treatment groups, rather than the initial program assignment groups, is potentially problematic. The most serious difficulty is that lack post-program information on people initially assigned to treatment who failed to show up for training, or dropped out less than two weeks. Unless the incidence of dropout and no-show behavior is random, the outcomes of the remaining members of the treatment group may over-state or under-state the average value of training. To assess the likely magnitude of this problem, we present an analysis using the baseline survey data on <u>all</u> originally assigned trainees and controls in the second cohort of the JE program in the next section. A related concern is that people re-assigned from the control group to the treatment group may have been non-randomly selected. Given the nature of this process and available program documents, however, we believe that re-assignment was essentially random.

Originally, the follow-up survey was scheduled to be conducted 6 months after completion of the classroom segment of the training. In practice, however, the survey

[16] From now on we refer to these as "treatment" and "control" groups. The size of the evaluation sample was determined to meet a minimum precision requirement for the difference in means of the employment rates between the treatment and control groups.

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 classroom training program. Because of variation in the date of entry into the program, and variation in the duration of classroom 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.[17]

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 realized 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 74% (=.933 × .848 × .924), which compares favorably with other training programs.[18]

An important complication that arises in the post-program survey is that since the control group members did not enter training, they could not be asked about their activities in the time since program completion. Instead, members of the control group were asked to provide a monthly calendar starting from August/September 2004. Thus, for members of the control group, we have access to information on roughly 7-9 months of data over the period from September 2004 to May-July 2005.

[17] 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% were surveyed 13 months or more after course completion.

[18] Note that the treatment group is defined as those who completed at least two weeks of classroom training. If no-shows and early dropouts are included in the calculation of the completion rate, it falls to 60%, which is still relatively high.

For the dynamic analysis described in Section 5, we make one further adjustment to the sample. Specifically, to ensure that comparisons of the treatment and control groups are not affected by the fact that some treatments were still in classroom training, for the dynamic analysis we limit attention to the subgroup of the treatments in the follow-up survey who had completed their classroom training (or dropped out) by September 2004.[19] Using this criteria we identified a set of 651 members of the treatment group (82.8% of the treatment group included in the evaluation sample). We refer to the sample comprising all 563 controls in the follow-up sample and the 651 treatments in the follow-up sample that completed their classroom training by September 2005 as the "dynamic sample".

b. Basic Sample Characteristics and Tests for Randomness

Table 1 shows some basic characteristics for members of the (realized) treatment and control groups, as well as for a comparison group in the general population taken from the October 2004 labor force survey.[20] We include information collected in the baseline (eligibility) survey completed by all program applicants (denoted as "baseline" characteristics in the table), as well as information on selected characteristics collected in the follow-up survey.

Looking first at the differences between the treatment and control groups, there appears to be only small and unsystematic differences between the groups. For both groups the mean ages are about 22.3 at baseline, and 22.8 at the follow-up survey. The regional distributions of the treatments and controls are also similar, as are the fractions

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[19] The date of ending classroom training has to be imputed for early dropouts.
[20] This group was defined in terms of age (16-29, mimicking the program's eligibility criteria) and region, limiting the sample to those living in the same provinces in which the program operated.

who are male, the mean levels of schooling of both parents, the fractions who report receiving remittances (at either baseline or follow-up), the fractions employed at baseline, the fractions with previous work experience, and average household size. The only notable exception is the distribution of schooling, where – despite the similarity in mean years of schooling – the treatment group appears to have a lower fraction of people with primary education and a higher fraction with secondary schooling than the control group. Given the patterns for parental education, and for years of schooling, we suspect that the slight differences in the fractions with primary versus secondary education are accidental, rather than the result of a failure of randomization, or of the fact that the groups are based on realized program group status, rather than initially assigned status.[21]

Although the comparisons in Table 1 between the realized treatment and control groups suggest that these two groups are very similar, we conducted an additional analysis using data from the baseline survey that are available for everyone in the second cohort of the JE program. Specifically, we compared the characteristics of four subgroups, defined by the combination of initial program assignment and realized assignment: (1) those who were originally assigned to treatment and were either no-shows or dropouts; (2) those who were originally assigned to treatment and actually received treatment; (3) those who were originally assigned to the control group and did not receive treatment; (4) and those who were originally assigned to the control group but were re-assigned to treatment. A sufficient condition for realized program group status to be "as good as random" is that the classification into these four groups is random.

[21] To test randomization more formally, we ran a probit model for being in the realized treatment group on all the baseline characteristics, as well as the follow-up survey characteristics that are arguably unaffected by treatment (parental education, location). We then conducted a test for the joint significance of the covariates, which should be insignificant if assignment is random. The statistic is insignificant when education is measured in years, but marginally significant if it is measured in categories.

Appendix Table 1 presents the means for a set of descriptive statistics for the four groups. Looking across groups, there are few obvious differences by status. The only characteristic that clearly stands out is age: no-shows and dropouts were somewhat more likely to be age 20-24, and less likely to be age 25 or older, than the other groups.

To test the hypothesis of randomness more formally we fit a multinomial logit model for being in each of the four groups, using the age, gender, education, and family status of the applicants, and interactions of these variables, to predict assignment/realized status. The results are summarized in Appendix Table 2. The likelihood ratio test for the hypothesis that all the coefficients of the model are 0 is statistically significant, suggesting that the covariates have some predictive power. In particular, it appears that some of the age effects and their interactions are statistically significant, although the pseudo-R^2 is tiny. This finding suggests that we have to be cautious in inferring the true effect of the JE treatment from comparisons between the realized program group and realized control group.

In view of theses results, in the comparisons below we present both "unadjusted" comparisons of the mean differences between the two groups, and a reweighted difference, which uses the method described by DiNardo, Fortin, and Lemieux (1996) to "balance" the distribution of the characteristics of the two groups.[22] This is a simple semi-parametric alternative to a regression adjustment which will lead to unbiased estimates of the experimental impacts based on realized treatment group status under the assumption that that no-show/dropout behavior was random, conditional on the *observed*

[22] In brief, the method is as follows. Step 1: estimate a logit model for the probability of being in the control group as a function of the baseline and time-invariant characteristics measured in the follow-up survey. Step 2: construct weighted means for the treatment group, using as a weight for a given person the function p/(1-p), where p is the predicted probability he or she is in the control group.

covariates, and that re-assignment from the control group to the program group was also random, conditional on the *observed* covariates. Results from a regression adjusted comparison are quite similar and in the interests of simplicity we report only the unadjusted and reweighted comparisons.

Relative to the comparison group of same-aged people in the October 2004 Labor Force Survey (ENFT), members of the experimental sample are a little less likely to be male, consistent with the stated objectives of the program. People in the experiment also tend to have less-educated parents than those in the overall population. Most noticeably, people in the experiment have lower employment rates and previous work experience (as measured in the baseline survey), reflecting the eligibility requirements of the program.

A third interesting set of contrasts in Table 1 is between responses to similar questions at the baseline and follow-up surveys. For example, in the baseline survey no one in the treatment or control groups reported having post-secondary education, whereas in the follow-up both groups report a 12% rate of post-secondary education. We suspect that this discrepancy reflects under-reporting by applicants who were aware of the eligibility criteria of the program (which specified less than a high school degree for eligibility). A similar under-reporting phenomenon could explain the higher fraction of the sample with reported remittances at the follow-up than at the baseline.[23]

Results

a. Employment

The main goal of the JE program was to increase the "employability" of participants. Hence, a natural yardstick for assessing program success is a comparison of

[23] The remittance question at baseline refers to 2002, while the question in the follow-up survey refers to 2004, and in the ENFT to July-September 2004. We suspect that timing differences cannot account for the rise in remittance rates between baseline and follow-up.

the post-program employment rates of the treatment and control groups, which, under the assumption of random assignment is an unbiased estimate of the average treatment effect. Table 2 reports the employment rates for both groups, as well as the raw and weighted difference. The results clearly show no program impact on participant employment rate[24]: at the time of the follow-up survey 57% of individuals in treatment group were employed versus 56% of those in the control group. The results from the reweighted comparison are even closer to 0. When we disaggregate the results by gender, age, education and region none of the estimated employment impacts are statistically different from 0 at conventional levels. Nevertheless, the point estimates are positive and large enough to be economically significant for the youngest age group (17-19 years old), and for those in the East and Santo Domingo regions.

While the main focus of the JE program was on employment, it is also interesting and important to consider the effects of the program on earnings. To explore these effects, we begin by looking at monthly labor earnings and hours per week.[25] Table 3 shows total monthly labor income for the two groups, assigning 0 earnings for non-workers. Members of the treatment group have monthly total labor earnings which are RD$484 (or 17%) higher than the control group. While this is a large effect, it is imprecisely estimated, reflecting the small samples sizes and the underlying variability in earnings. Examining various subgroups, the estimated earnings impacts are larger for the youngest age group and for residents of Santo Domingo. The impacts also seem to be

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[24] The employment rate is computed at the time of the follow-up survey (May - July 2005). Even controlling for the month of application and for the month of graduation/separation, there are no significant differences between the employment status of treatments and controls.

[25] Additionally, we also examine whether the quality of the job was different as measured by having health insurance. Those results are discussed in the dynamic analysis.

larger for those with some secondary education (a 21% impact versus a 9% for people with only primary education).

Table 4 shows the impacts on hours worked per week. Consistent with the results on the probability of employment, there do not seem to be large or systematic effects on hours of the overall sample or any subgroup. Interestingly, for many groups the estimated effects of JE are negative, though uniformly insignificant.

b. Conditional Impact on Workers

Given the negligible impact on employment and hours per week, but the positive effects on earnings, it is interesting to look at how the JE program affected hourly wages. As pointed out in Lee (2005), a comparison of hourly wages in an experimental setting is problematic when the intervention affects the probability of work. In the case of JE, however, the program appears to have had no effect on employment, implying that wage comparisons between the groups are potentially valid.[26] Table 5 presents results for the overall experimental population. The top row simply reproduces the employment impact from Table 2. The remaining rows show means of income, hours worked, hourly wages, and the probability of health insurance, conditional on working, for the treatment and control groups, as well as the unadjusted and adjusted (reweighted) gaps between them. The JE program appears to have had a marginally significant 10% impact on the hourly wages of participants. No significant differences exist either in hours worked per week

[26] Formally, people who report wages are a selected subset of the population, and, if the experiment effects the probability of working it may change the relative amount of selectivity bias in the observed wages of the two groups. Lee (2005) presents an informative procedure for bounding the size of any wage effects, when there is an employment rate difference. When there is no employment difference, and employment is assumed to be governed by a single index selection model, simple (unadjusted) comparisons of wages are valid.

(conditional on working) or in the probability of obtaining health insurance through the primary job, though the point estimates of the insurance effect are positive.

Tables 6a and 6b conduct the same exercise for different subgroups. Table 6a shows the unadjusted data, whereas 6b shows results using the reweighting procedure to standardize the characteristics of the treatment group back to those of the controls.[27] Across the various subgroups there is no evidence of a significant effect on hours (conditional on working). Likewise, although most of the point estimates are positive, none of the estimated effects on hourly wages are significant. We conclude that the suggestive positive effects on wages seen for the overall sample in Table 5 are relatively evenly distributed across the sample.

Although the estimated impacts on hourly wages and earnings are not statistically significant, the magnitude of the point estimates (around 10%) is relatively large. In particular, the estimated impact on average monthly earnings of those with a job is about RD$440 – or about 38 US dollars per month – with a t-statistic for the adjusted gap of 1.5. The estimated cost of the JE program was about 330 US dollars per trainee. Taking the point estimate at face value, assuming that the employment rate of the trainees (and controls) remains at 55%, and that impact on earnings conditional on employment persists indefinitely, and the initial investment in training costs would be recovered in about two years. 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 on the cost-effectiveness of the JE program.

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[27] Note that the first row of Table 6a corresponds to the third column of Table 5 (raw differences), and the first row of Table 6b corresponds to the last column of Table 5 (reweighted differences).

c. Quality of the Training Institutions

A natural hypothesis about training programs is that higher quality training will have a bigger impact on participant outcomes. 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 in Phase 1 of the JE program, 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.[28] We tried to test whether the impact of training was related to the "quality" of the ICAP by dividing enrollees into those who were assigned to ICAPs with different INFOTEP ratings (treating non-members 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".

d. Dynamic Employment Impacts

So far we have examined the impact of the program at the time of the follow-up survey, which took place between May and July 2005. In this section we focus on employment dynamics, specifically monthly employment outcomes between August

[28] The share of trainees – among those who enrolled with a certified ICAP-- was 37% at ICAP's with low rating, 50% at those with medium rating and 8% at those with a high rating.

2004 and May 2005. As noted earlier, for this purpose, we limit the sample of treatment group members to those that finished or dropped out of the course on or before August 2004. This creates a "balanced" panel of individuals for whom we observe monthly employment status from August 2004 until May 2005.

Figure 1 shows monthly employment rates for the treatment and control groups during each month of this ten month window, along with the difference in employment rates for each month, and a 95% confidence interval around the difference. We present data for the overall sample (top left panel) and for some of the key subgroups in the experiment. As suggested by the estimated employment impacts at the time of the follow-up survey (in Table 2), there is no indication of an overall treatment effect, but there is some indication of positive employment effects for the youngest sample members, and for those in the East region.

We also conducted a similar analysis using information on the dynamic path of the likelihood of having employer-provided health insurance (i.e., being employed at a job that provides health insurance coverage). We interpret this variable as a rough indicator of the quality of the job held at a point in time. (Unfortunately, the surveys did not collect monthly wage data). Figure 2 shows the fractions of people in the treatment and control groups with employer-provided health insurance each month, along with the experimental impact (and a 95% confidence interval). Overall, the treatment group has about a four percentage point higher coverage rate than the control group (19.5% vs. 15.5%), and the gap is marginally significant over most of the post-training window. However, the difference is present only for men; it is negligible for women. Although the estimates are quite noisy, the effect seems to be concentrated among better-educated sample members (with a secondary education) and among those living in Santo Domingo.

e. A Model of Impacts on "Employability"

The designers of the JE program specified "increased employability" as an objective of training. One interpretation of this concept is that training would *raise* the probability of moving from unemployment to employment, and *lower* the probability of moving from employment to unemployment. Building on this interpretation, in this section we develop a simple dynamic model of monthly employment outcomes in the JE evaluation, to determine whether participating in the program had an impact on either probability. 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 model consists of two parts: one for the person's employment status in "month 1" (August 2004) – which we interpret as a period just after the end of training – and another for the rate of employment transitions over the next 9 months.[29] In this setting, the JE program has two types of potential effects: an effect on employment status 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 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 a dummy indicating i's program status (T_i =0 for a control group member and T_i =1 for a program group member). The statistical problem is to develop a model for

[29] Some of the issues in specifying treatment effects in a dynamic setting are described in Ham and Lalonde (1996) and Card and Hyslop (2005).

$$P(y_{i1}, y_{i2}, \ldots y_{i11} \mid T_i, X_i) = P(y_{i1} \mid T_i, X_i) \times P(y_{i2}, \ldots y_{i11} \mid y_{i1}, T_i, X_i)$$

We assume that there is unobserved heterogeneity across the population, represented by the random effect α_i. Under random assignment, the distribution of the random effect is the same in the 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 in month t depends on α_i, on a linear trend (capturing the upward trend in employment we observe in the data from months 1-10) on the X's, and on employment status in the previous month:

$$P(y_{it}=1 \mid y_{it-1}, T_i=0, X_i, \alpha_i) = P(\beta_0 + \beta_1 t + X_i\beta_x + \lambda y_{it-1} + \alpha_i + e_{it} \geq 0)$$

where e_{it} is a logistic random variable that is i.i.d. over time and across people. This implies that

$$P(y_{it}=1 \mid y_{it-1}, T_i=0, X_i, \alpha_i.) = \text{logit}(\beta_0 + \beta_1 t + X_i \beta_x + \lambda y_{it-1} + \alpha_i)$$

where 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 in period t if the person was not working in period t-1 (i.e., an increase in the rate of moving from non-work to work), and a potential increase in the probability of being employed in period t if the person was working in period t-1 (i.e., an increase in the rate of job retention). Formally, we assume that

$$P(y_{it}=1 \mid y_{it-1}, T_i=1, X_i, \alpha_i) = \text{logit}(\beta_0 + \beta_1 t + X_i \beta_x + \lambda y_{it-1} + \varphi_0(1-y_{it-1})$$
$$+ \varphi_1 y_{it-1} + \alpha_i) .$$

The parameter φ_0 represents the effect of the JE program on the probability of moving from non-work to work, while φ_1 is the effect on the probability of job retention.

We assume that the distribution of the random effects can be approximated by a point mass distribution with a small number (3) points of support. Thus, α_i is a random variable that takes on 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.[30] Finally, we assume that the probability that the individual is employed in August 2004 is given by

$$P(y_{i1}=1 \mid T_i, X_i, \alpha_i) = \text{logit}\,(\ \gamma(\alpha_i) + \ \mu\, X_i\, \beta_x \ + \delta T_i\ )$$

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 scalar parameter that "rescales" the effects of the X's in the initial conditions probability model, and δ represents the treatment effect on the probability of employment in month 1.

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, 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 controls. In column 2, we also

[30]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).

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, 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, or to be employed at a job with insurance (row 7). Likewise, older workers have higher probabilities of employment or employment with health insurance (rows 8-9). 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 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 (Figure 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 holding onto such a job.

Some further insight into the predictions of the dynamic model for health insurance coverage are presented 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 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 2 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 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.

5. Interpretation and Conclusions

This paper presents the first evaluation 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 impacts of training on the probability of having a job and on labor earnings. In contrast, we find that the *Juventud y Empleo* (JE) program in the Dominican Republic had no significant effect on employment. There is evidence of an modest (10%) impact on hourly wages and earnings per month (conditional on employment), although the estimated effects are only marginally significant (t=1.5 for monthly earnings). The point estimate is economically significant, and large enough to potentially offset the costs of the JE training in about 2 years, if the impact persisted.

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 were not included in the survey of post-program outcomes. Our analysis suggests that dropouts were not very different than those who completed training, and we use a re-weighting procedure to adjust the available samples of trainees and controls for minor differences in their observed characteristics. It is possible there is some remaining bias in our experimental contrasts, arising from unobserved differences between the dropouts and those who completed training, although we believe theses biases are probably small.

This paper also contributes to the literature by providing an operational definition for "employability", based on transition probabilities between employment and non-employment 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 of 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 JE program in the Dominican Republic, and 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 youths 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.[31]

[31] The Office of Evaluation and Oversight at the IDB is currently working on five quantitative impact evaluations of similar programs, including a natural experiment in Panama and quasi experimental designs for Mexico, Peru, Argentina and Colombia.

Appendix Table 1: Comparisons of Characteristics Between Four Assignment/Realized Treatment Status Groups

Variable	Assigned to Treat. and Received	Assigned to Control and Remained	Assigned to Treat. and No-show/dropout	Assigned to Control and Reassigned
	1	2	3	4
Female	0.545	0.570	0.534	0.528
Years of Schooling	9.255	9.083	9.171	9.459
Primary	0.324	0.386	0.374	0.274
Age	22.246	22.368	22.136	22.034
Student	0.351	0.305	0.403	0.437
Married	0.186	0.215	0.201	0.170
Dependants	0.192	0.193	0.213	0.181
Remittances	0.033	0.035	0.040	0.037
Letrina	0.252	0.274	0.217	0.216
Santo Domingo	0.539	0.553	0.507	0.467
Household Members	4.872	4.826	4.781	4.939
Age 17-19	0.240	0.239	0.231	0.254
Age 20-24	0.505	0.492	0.546	0.510
Age 25+	0.254	0.269	0.223	0.236
Labor Experience	0.151	0.148	0.159	0.164
Employed	0.028	0.025	0.025	0.016
N	4791	1623	1010	941

Appendix Table 2: Tests for Randomness Between Four Assignment Groups

Multinomial logistic regression	**Number of obs**	**=**	**8365**
	LR chi2(63)	**=**	**117.17**
	Prob > chi2	**=**	**0**
Log likelihood = -9464.0928	**Pseudo R2**	**=**	**0.0062**

	Control		No Show/Dropouts		Control-Replacement	
Variable	**Coef.**	**Std. Err.**	**Coef.**	**Std. Err.**	**Coef.**	**Std. Err.**
Female	0.057	0.144	0.286	0.186	0.136	0.182
Years of Schooling	0.009	0.027	0.060	0.034	0.052	0.036
Household Size	-0.009	0.015	-0.027	0.019	0.016	0.019
Married (M)	0.050	0.208	0.069	0.253	0.139	0.257
Primary (P)	0.078	0.171	0.307	0.219	-0.160	0.233
P*F	-0.037	0.128	0.088	0.153	-0.130	0.168
Age 20-24 (A20)	-0.156	0.130	0.406	0.173	0.182	0.164
P*M	0.086	0.160	-0.273	0.194	0.011	0.220
No. Dependents (D)	-0.268	0.186	0.220	0.226	-0.049	0.232
P*D	0.287	0.157	0.162	0.187	0.154	0.212
D*F	-0.119	0.169	-0.117	0.200	0.050	0.216
M*F	0.132	0.197	-0.057	0.228	-0.227	0.243
Age 17-19 (A17)	-0.299	0.154	0.506	0.196	0.116	0.188
A17*P	0.455	0.177	0.001	0.226	0.185	0.238
A17*M	0.173	0.256	0.163	0.351	0.286	0.328
A17*D	0.222	0.264	-0.423	0.360	-0.106	0.338
A17*F	0.070	0.182	-0.601	0.230	-0.089	0.227
A20*P	0.098	0.149	0.114	0.185	0.115	0.205
A20*M	-0.160	0.169	0.264	0.204	-0.040	0.222
A20*D	0.283	0.165	-0.057	0.197	-0.089	0.213
A20*F	0.065	0.152	-0.362	0.191	-0.241	0.193

Note: Multinomial logit model for assignment/realized treatment status. Omitted group is the set of originally assigned trainees who completed training.

Table 1: Basic Characteristics of the Sample

	Treatment Group	Control Group	Comparison Group**
Age (in Years):			
At Baseline	22.3	22.3	--
At Follow-up	22.8	22.8	22.1
Geographic Distribution:			
East	16.0	13.0	7.1
North	18.8	16.7	32.9
Santo Domingo	44.0	45.3	50.0
Southwest	21.1	25.0	10.1
Percent Male	44.7	42.8	49.0
Parental Education:			
Schooling of Father (years)	6.9	7.2	9.4
Schooling of Mother (years)	7.0	6.9	9.3
Distribution of Completed Education			
At Baseline:			
Years of Schooling	9.3	9.2	--
Primary*	30.7	36.9	--
Secondary*	69.3	63.1	--
At Follow-up:			
Years of Schooling	10.7	10.5	9.6
Primary *	15.5	22.7	37.1
Secondary *	72.1	64.8	45.2
Post-secondary	12.3	12.4	17.7
Receive Remittances:			
At Baseline	3.3	4.1	--
At Follow-up	24.7	20.6	8.0
Employed (Baseline)	3.1	3.4	48.9
Previous Work (Baseline)	17.7	15.8	58.8
Household Size	5.0	5.1	4.7

* Denotes statistically significant difference between treatment and control groups in this variable.

** Based on October 2004 ENFT (Labor Force Survey) for the provinces were the training program was offered, for overall population ages 16-29.

Table 2: Employment Status at the Follow-up Survey

Sample:	Treatments	Controls	Raw Difference	Re-weighted Difference
All	57.38%	55.95%	1.43%	0.02%
	1.77%	*2.09%*	*2.74%*	*2.74%*
Men	70.94%	70.54%	0.40%	-0.77%
	2.43%	*2.94%*	*3.81%*	*3.85%*
Women	46.44%	45.03%	1.41%	0.28%
	2.39%	*2.78%*	*3.67%*	*3.65%*
Ages 17 - 19	50.37%	42.59%	7.78%	6.93%
	4.32%	*4.78%*	*6.45%*	*6.35%*
Ages 20 - 24	59.12%	57.64%	1.48%	-0.10%
	2.37%	*2.92%*	*3.75%*	*3.80%*
Age 25+	58.26%	61.68%	-3.42%	-4.46%
	3.35%	*3.77%*	*5.05%*	*5.00%*
Primary Education	56.85%	57.21%	-0.37%	-1.99%
	3.20%	*3.44%*	*4.70%*	*4.59%*
Secondary Education	57.61%	55.21%	2.40%	1.16%
	2.12%	*2.64%*	*3.38%*	*3.43%*
EAST	61.90%	53.42%	8.48%	6.95%
	4.34%	*5.88%*	*7.25%*	*7.50%*
NORTH	60.14%	67.02%	-6.89%	-6.70%
	4.04%	*4.88%*	*6.39%*	*6.41%*
Santo Domingo	58.67%	53.33%	5.34%	4.13%
	2.65%	*3.13%*	*4.09%*	*4.09%*
SOUTHWEST	48.80%	54.61%	-5.81%	-7.46%
	3.89%	*4.21%*	*5.73%*	*5.60%*

Notes: standard errors in italics. In the last column, the mean for the treatment group is a weighted mean, where the weight for a given person is p/(1-p), and p is the estimated probability the person is in the control group, given his/her covariates.

Table 3: Labor Earnings in the Month of the Follow-up Survey

Sample:	Treatments	Controls	Raw Difference	Re-weighted Difference
All	$3,236	$2,752	$484	$273
	$146	*$150*	*$215*	*$208*
Men	$4,750	$4,107	$643	$456
	$251	*$262*	*$373*	*$366*
Women	$2,014	$1,739	$276	$135
	$146	*$153*	*$215*	*$207*
Ages 17 - 19	$2,680	$1,826	$854	$619
	$343	*$321*	*$479*	*$444*
Ages 20 - 24	$3,267	$2,865	$402	$228
	$188	*$209*	*$287*	*$285*
Age 25+	$3,518	$3,157	$362	$128
	$306	*$284*	*$429*	*$403*
Primary Education	$2,890	$2,654	$236	$21
	$238	*$250*	*$346*	*$328*
Secondary Education	$3,389	$2,810	$579	$421
	$183	*$189*	*$273*	*$268*
EAST	$3,181	$2,965	$216	$13
	$325	*$510*	*$577*	*$617*
NORTH	$3,292	$3,727	-$436	-$510
	$327	*$430*	*$534*	*$551*
Santo Domingo	$3,687	$2,712	$975	$781
	$243	*$216*	*$338*	*$320*
SOUTHWEST	$2,287	$2,065	$223	$12
	$270	*$224*	*$358*	*$326*

Notes: standard errors in italics. See note to table 2. The dependent variable is monthly earnings (including 0's for non-earners). The value of earnings is censored at the 99th percentile.

Table 4: Hours of Work Per Week in the Follow-up Survey

Muestra	Treatments	Controls	Raw Difference	Re-weighted Difference
All	23.97	23.39	0.58	-0.73
	0.94	*1.14*	*1.47*	*1.47*
Men	32.50	32.38	0.12	-0.99
	1.46	*1.81*	*2.31*	*2.35*
Women	17.09	16.67	0.42	-0.54
	1.13	*1.34*	*1.74*	*1.73*
Ages 17 - 19	20.53	21.02	-0.49	-1.65
	2.20	*2.85*	*3.54*	*3.49*
Ages 20 - 24	24.59	22.57	2.02	0.80
	1.26	*1.49*	*1.97*	*1.98*
Age 25+	24.87	26.34	-1.47	-2.77
	1.84	*2.15*	*2.83*	*2.78*
Primary Education	24.00	24.27	-0.27	-2.02
	1.69	*2.04*	*2.62*	*2.54*
Secondary Education	23.96	22.88	1.08	0.02
	1.13	*1.35*	*1.78*	*1.80*
EAST	27.12	25.60	1.52	0.37
	2.39	*3.91*	*4.33*	*4.73*
NORTH	22.70	26.78	-4.07	-3.81
	1.94	*2.63*	*3.21*	*3.34*
Santo Domingo	25.72	23.11	2.61	1.33
	1.51	*1.72*	*2.29*	*2.26*
SOUTHWEST	19.07	20.50	-1.44	-2.95
	1.88	*1.95*	*2.72*	*2.57*

Notes: standard errors in italics. See note to table 2. The dependent variable is weekly hours (including 0's for non-workers).

Table 5: Summary of Labor Market Outcomes in the Follow-up Survey

Outcome:	Treatments	Controls	Raw Difference	Re-weighted Difference
Employment Rate	57.38%	55.95%	1.43%	0.02%
	1.77%	*2.09%*	*2.74%*	*2.74%*
Monthly Income (All Jobs)	$ 5,818	$ 5,289	$ 529	$ 438
	$ 195	*$ 202*	*$ 288*	*$ 284*
Hours worked per week (All Jobs)	43.43	44.27	-0.84	-1.11
	0.79	*0.98*	*1.25*	*1.27*
Hourly Wage (All Jobs)	$ 151.19	$ 133.92	$ 17.27	$ 14.50
	$ 9.91	*$ 7.02*	*$ 13.32*	*$ 11.84*
Health Insurance in Primary Job	38.0%	34.8%	3.1%	2.5%
	2.5%	*2.9%*	*3.9%*	*3.9%*

Notes: standard errors in italics. See note to table 2. The sample for employment includes everyone. The sample for income, hours per week, hourly wage, and health insurance includes those with positive earnings and between 10 and 85 hours per week. The value of earnings is censored at the 99th percentile.

Table 6a: Unadjusted Difference For Selected Indicators

	Employment	Monthly Earnings	Hours per Week	Hourly Wage	Health Insurance
All	1.43%	$ 529	-0.84	$ 17.27	3.1%
	2.74%	*$ 288*	*1.25*	*$ 13.32*	*3.9%*
Men	0.40%	$ 757	-0.33	$ 23.33	9.3%
	3.81%	*$ 406*	*1.58*	*$ 21.74*	*5.2%*
Women	1.41%	$ 177	-1.66	$ 8.07	-5.3%
	3.67%	*$ 360*	*1.96*	*$ 10.35*	*5.7%*
Ages 17 - 19	7.78%	$ 1,164	-3.42	$ 33.49	6.9%
	6.45%	*$ 751*	*2.70*	*$ 17.47*	*9.8%*
Ages 20 - 24	1.48%	$ 379	0.83	$ 17.05	-0.8%
	3.75%	*$ 369*	*1.69*	*$ 21.08*	*5.2%*
Age 25+	-3.42%	$ 552	-2.48	$ 10.27	8.2%
	5.05%	*$ 577*	*2.44*	*$ 19.72*	*7.0%*
Primary Education	-0.37%	$ 429	-0.08	$ 12.15	3.4%
	4.70%	*$ 476*	*2.32*	*$ 12.75*	*6.4%*
Secondary Education	2.40%	$ 528	-1.24	$ 18.01	2.6%
	3.38%	*$ 360*	*1.48*	*$ 18.99*	*4.8%*
EAST	8.48%	$ -872	-7.08	$ 3.24	-17.6%
	7.25%	*$ 760*	*3.70*	*$ 15.80*	*10.7%*
NORTH	-6.89%	$ -140	-0.21	$ -10.53	8.1%
	6.39%	*$ 698*	*2.68*	*$ 20.24*	*8.1%*
Santo Domingo	5.34%	$ 858	-1.94	$ 38.73	9.5%
	4.09%	*$ 445*	*1.87*	*$ 26.98*	*5.9%*
SOUTHWEST	-5.81%	$ 969	3.00	$ 2.43	-2.9%
	5.73%	*$ 523*	*2.63*	*$ 16.73*	*8.4%*

Notes: standard errors in italics. See notes to Table 5.

Table 6b: Reweighted Differences for Selected Indicators

	Employment	Monthly Earnings	Hours per Week	Hourly Wage	Health Insurance
All	0.02%	$ 438	-1.11	$ 14.50	2.5%
	2.74%	*$ 284*	*1.27*	*$ 11.84*	*3.9%*
Men	-0.77%	$ 698	-0.41	$ 20.16	9.3%
	3.85%	*$ 399*	*1.62*	*$ 19.09*	*5.2%*
Women	0.28%	$ 90	-2.04	$ 6.91	-6.4%
	3.65%	*$ 359*	*1.99*	*$ 10.38*	*5.8%*
Ages 17 - 19	6.93%	$ 912	-3.79	$ 29.01	7.5%
	6.35%	*$ 731*	*2.72*	*$ 17.22*	*9.5%*
Ages 20 - 24	-0.10%	$ 342	0.86	$ 13.90	-0.3%
	3.80%	*$ 367*	*1.74*	*$ 17.93*	*5.4%*
Age 25+	-4.46%	$ 427	-3.14	$ 10.53	4.9%
	5.00%	*$ 555*	*2.45*	*$ 20.24*	*6.9%*
Primary Education	-1.99%	$ 353	-0.77	$ 12.73	3.2%
	4.59%	*$ 452*	*2.28*	*$ 11.73*	*6.3%*
Secondary Education	1.16%	$ 500	-1.29	$ 15.98	2.3%
	3.43%	*$ 362*	*1.52*	*$ 17.41*	*4.9%*
EAST	6.95%	$ -990	-7.12	$ 0.80	-18.0%
	7.50%	*$ 827*	*3.92*	*$ 15.15*	*11.3%*
NORTH	-6.70%	$ -233	0.43	$ -15.21	8.4%
	6.41%	*$ 737*	*2.72*	*$ 21.95*	*8.2%*
Santo Domingo	4.13%	$ 803	-2.32	$ 35.73	8.5%
	4.09%	*$ 414*	*1.88*	*$ 21.85*	*5.8%*
SOUTHWEST	-7.46%	$ 794	2.22	$ 0.10	-4.4%
	5.60%	*$ 504*	*2.58*	*$ 16.22*	*8.2%*

Notes: standard errors in italics. See notes to Table 5.

Table 7: Employability Model - Estimated Parameters

	Employment	Employed with Health Insurance
Model Parameters		
1. Constant (β_0)	-1.99	-2.43
	(3.43)	(4.36)
2. Trend (β_1)	0.06	-0.03
	(0.02)	(0.03)
3. State-dependence (λ)	4.67	7.00
	(0.15)	(0.31)
4. Treatment Effect if Not Employed in Previous Period (φ_0)	0.03	0.24
	(0.10)	(0.20)
5. Treatment Effect if Employed in Previous Period (φx_1)	0.13	0.18
	(0.14)	(0.27)
6. Treatment Effect in Probability of Employment in Month 8 (δ)	0.07	0.18
	(0.15)	(0.27)
7. Male Dummy in Employment Model	0.73	0.71
	(0.11)	(0.27)
8. Dummy for Age 20-24 in Employment Model	0.37	0.41
	(0.11)	(0.20)
9. Dummy for Age 25+ in Employment Model	0.60	0.57
	(0.13)	(0.25)
10. Loading Factor For Covariates in Model for Employment in Month 8 (μ)	1.33	1.89
	(0.26)	(0.66)
11. Log Likelihood	- 3630.7	- 1536.3
12. Total Number of Parameters	17	17

Note: Models include point-mass random effects, with three points of support. See text. Standard errors in parentheses.

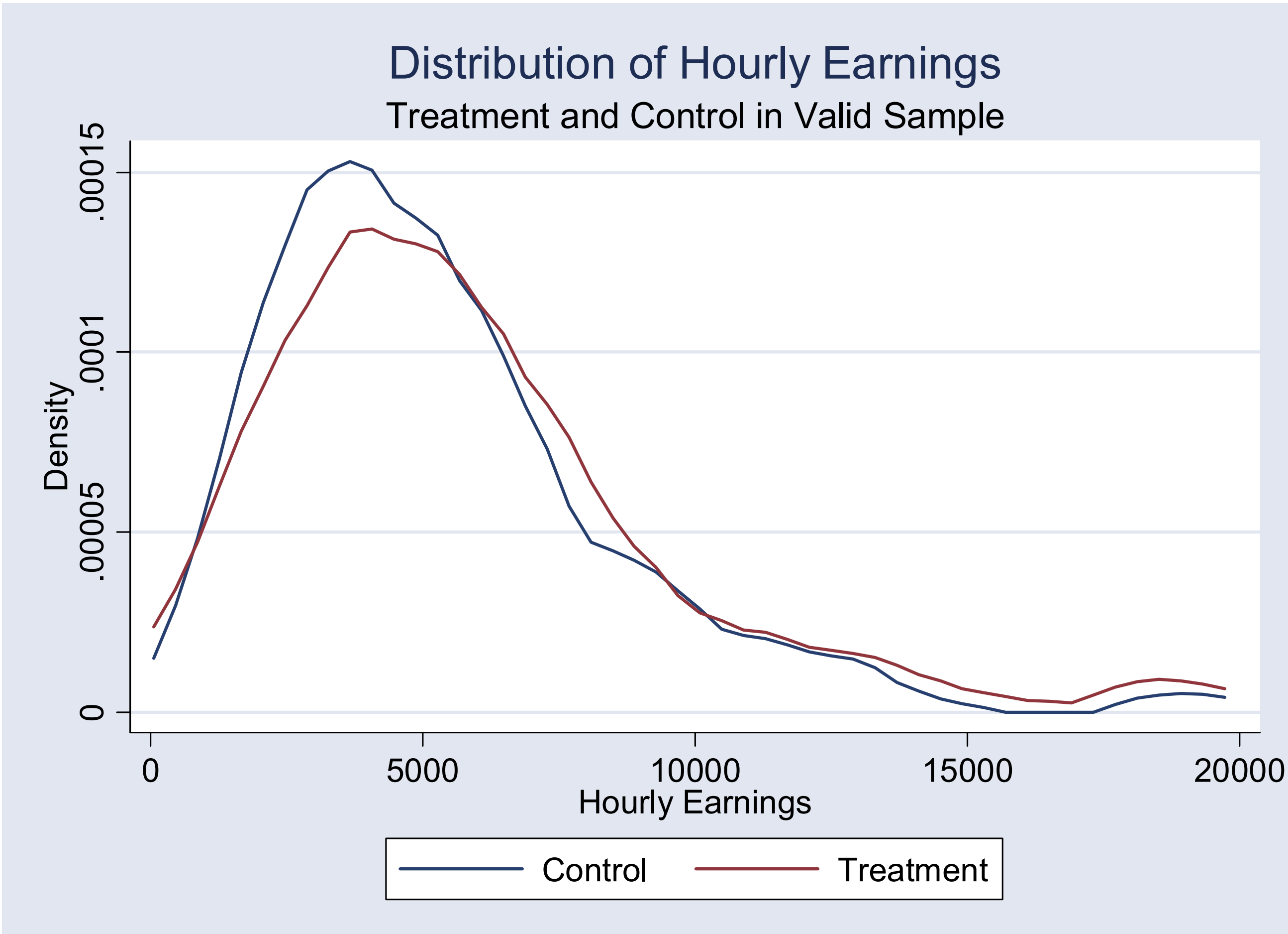

Distribution of Hourly Earnings
Treatment and Control in Valid Sample
Density
0
.00005
.0001
.00015
0
5000
10000
15000
20000
Hourly Earnings
Control
Treatment

Figure 1. Employment Rates

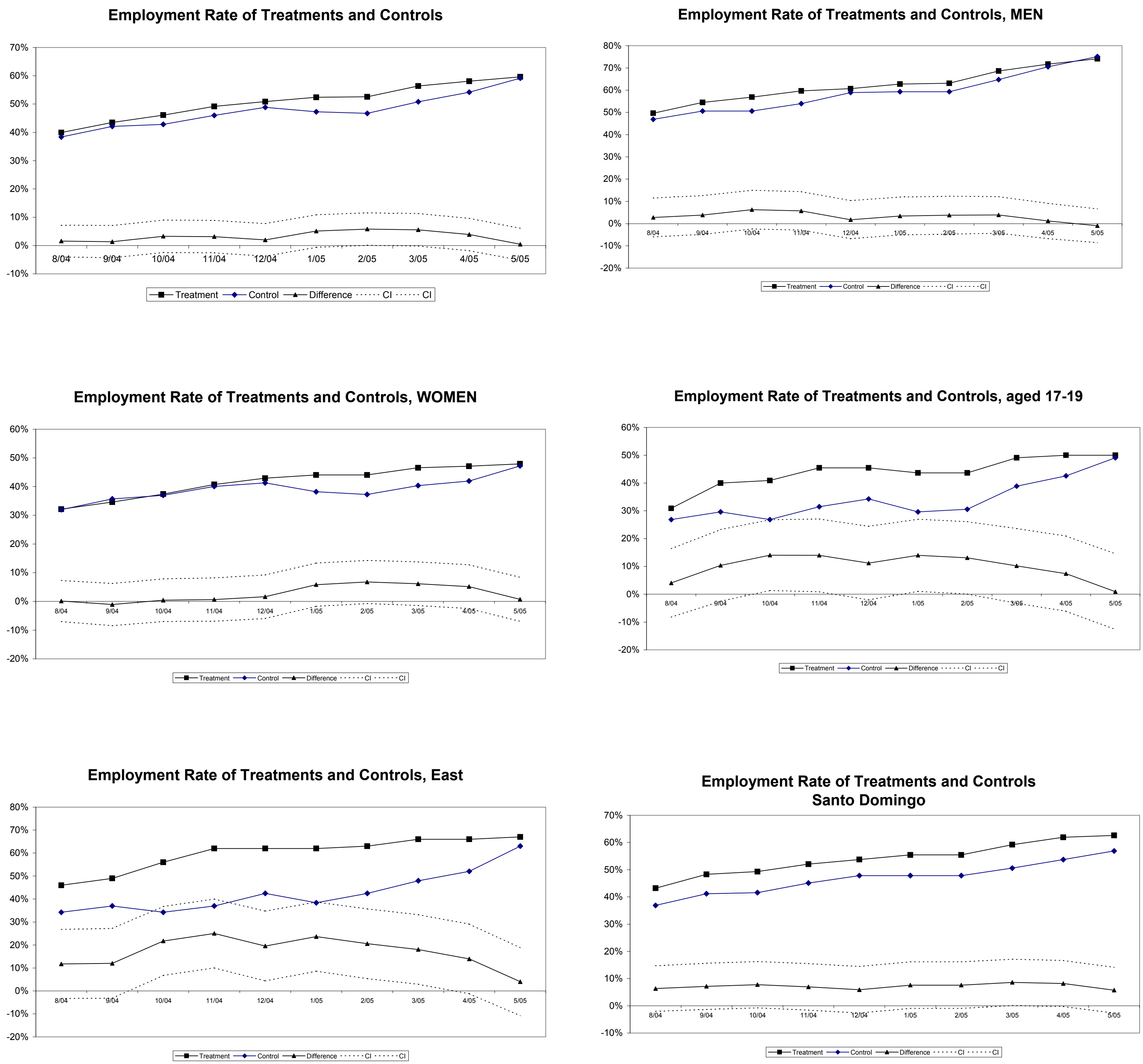


Figure 2. Employer-Provided Health Insurance

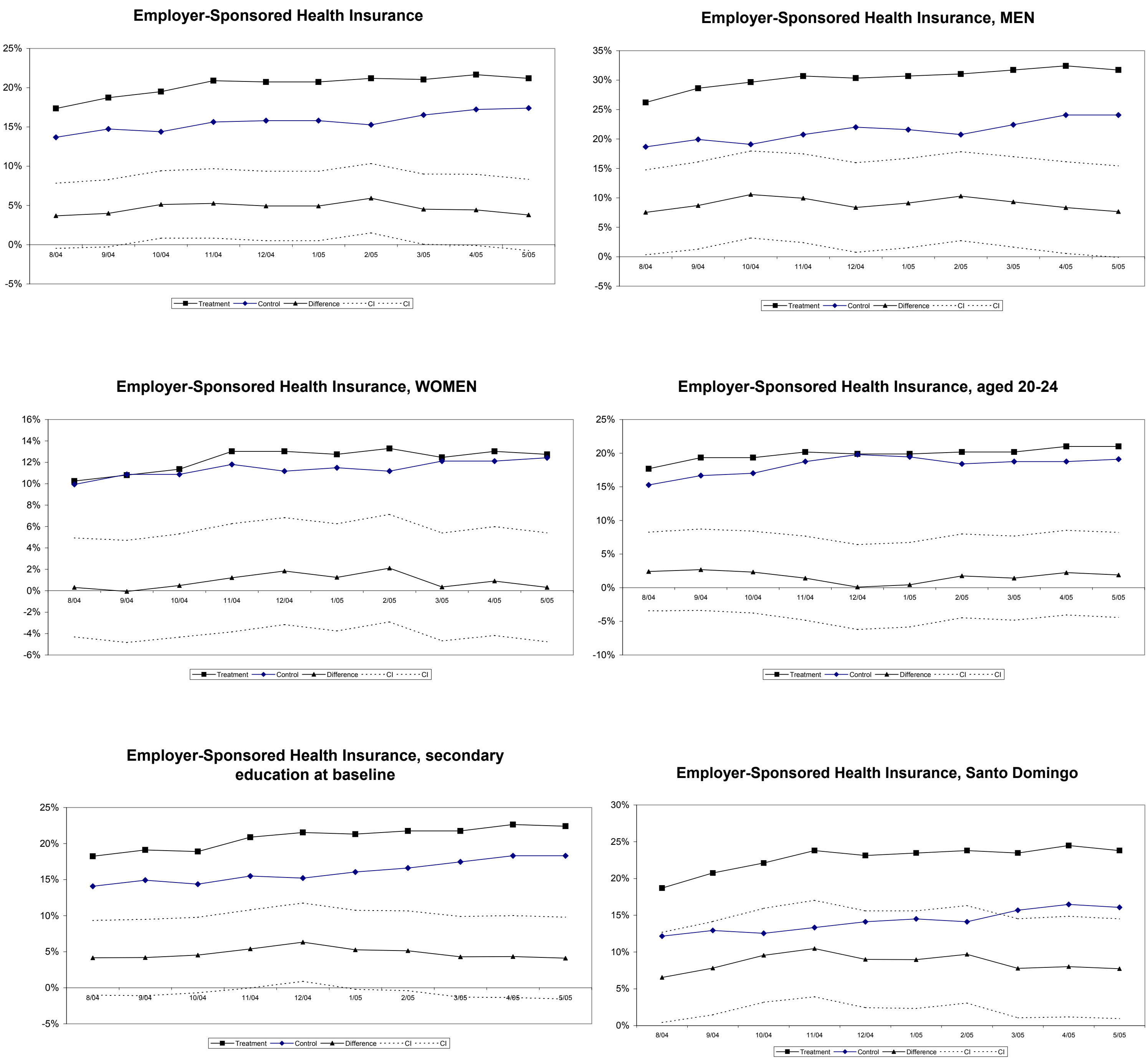
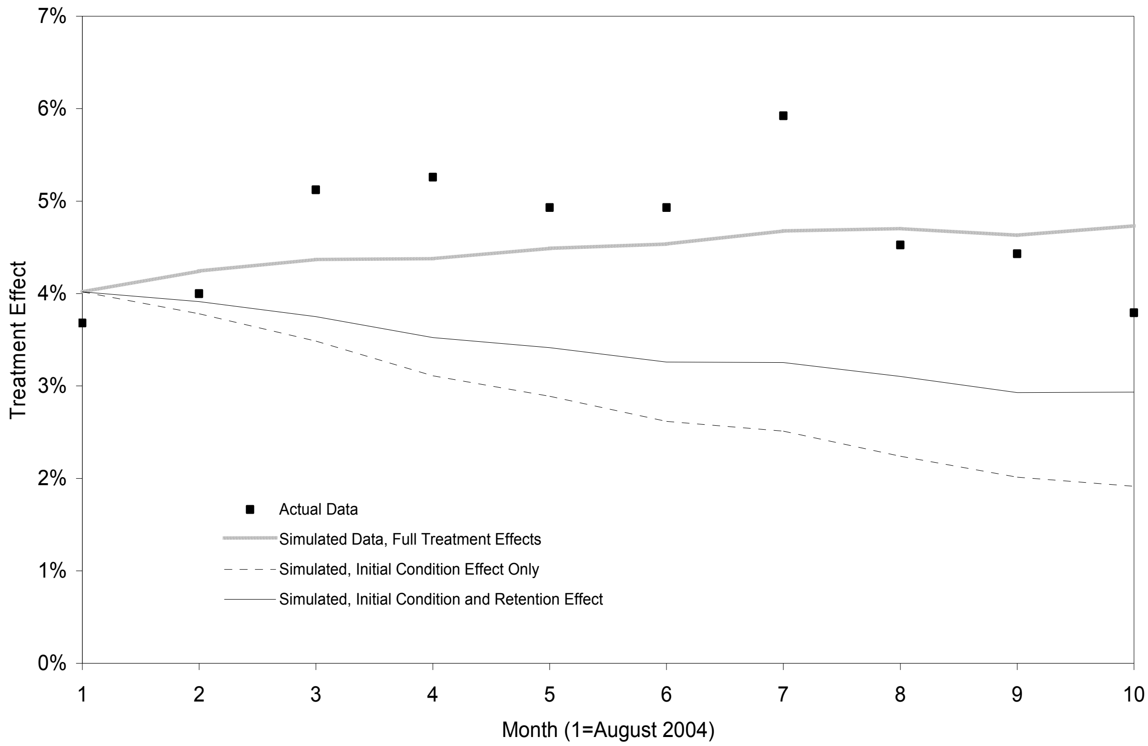


Figure 3. Actual and Simulated Treatment Effects on Probability of Employment with Health Insurance



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