

Training Programs for Unemployed in Developing Countries: Casual Jobs or Better and Sustained Jobs?

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Abstract

Since few job offers received by unemployed individuals in developing countries are for sustained jobs that can lead to greater human capital accumulation, those getting back to work are likely to experience, within a short span of time, another spell of unemployment. The effectiveness of active labor market programs in helping individuals back to work in these countries is only part of the feedback required by policy makers. A knowledge of their effectiveness in helping workers achieve good job matches and in helping individuals find a 'sustained' job -as opposed to 'any job' is also required.

Data sets used for evaluations in developing countries do not lend themselves to the measure of impacts of programs on the re-employment dynamics of trainees. An exception is a data set collected for an evaluation conducted in 1994 to a cohort of participants in a training program targeted at the unemployed in Mexico. In addition to having a control group with eligible individuals that did not participate in the program, it is the only one with longitudinal data covering not only the length of unemployment episodes after the training of the respondent, but also the duration of his/her employment spells. This paper contributes to the empirical literature of evaluation of active labor markets policies in developing countries by using Mexico's data to illustrate how a program's impact on weeks needed by participants to find a job and on time spend in that job must be estimated. This requires of the estimation of hazard functions for survival analysis. Our results illustrates how failure to distinguish between impact on finding a 'sustained' job and on finding 'a job' can mislead conclusions about program effectiveness. They also show that failure to adequately deal with the effect of possibly omitted determinants in the estimation of hazard functions (unobserved heterogeneity of individuals) can also imply inconsistent estimates, thereby erroneously implying that the program is not effective in improving employability of participants.

1. Introduction

Open and prolonged lapses of unemployment are unaffordable for most participants in labor markets of developing countries. Since few job offers received by unemployed individuals are for sustained jobs that can lead to greater human capital accumulation, those getting back to work are likely to experience, within a short span of time, another spell of unemployment. Their acceptance of casual jobs is associated with lack of means

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to search for employment opportunities in which they can be more productive. Information asymmetries aggravate this problem and can be a reason for workers trapped in a sequence of casual jobs and unemployment spells: stigma effects on employment offers for sustained and relatively better paid jobs often depend on applicants' job histories².

These features of their labor markets explain why the effectiveness of active labor market programs in helping individuals back to work is only part of the feedback required by policy makers. A knowledge of their effectiveness in helping workers achieve good job matches and in helping individuals find a 'sustained' job -as opposed to 'any job' is also required.

This latter requirement has not been adequately dealt in evaluation studies dealing with the performance of this kind of programs in developing countries³. Available studies have dealt exclusively with the program's impact on wages and on time to find a job and/or on the probability of finding one. Moreover, data sets used for evaluations of programs aimed to unemployed workers in developing countries do not lend themselves to measuring their impact on re-employment dynamics of their beneficiaries. This is because they do not have longitudinal data covering employment spells after program participation.

An exception is a data set for an evaluation of participants in a training program targeted at the unemployed in Mexico⁴. In addition to having a control group with eligible individuals that did not participate in the program, it is the only one with longitudinal data covering not only the length of unemployment episodes after the training of the respondent, but also the duration of his/her employment spells.

This paper contributes to the empirical literature of evaluation of active labor markets policies in developing countries by using Mexico's data to illustrate how a program's impact on weeks needed by participants to find a job and on time spend in that job must be estimated. This requires of statistical techniques (hazard functions for survival analysis).⁵

² Severance payments for job termination tend to characterize these countries' labor codes; an extension of the model with adverse selection deployed by Kugler and St. Gilles (2004) suggests that the higher the costs of ending a job, the more reluctant employers would be to offer sustained jobs to individuals considered to be 'lemons', *viz.* to those without long spells in previous employments,.

³ Along with demands for transparency of public spending, there has been a growing consensus in these countries that the future of their active labor market programs should be decided based on adequate measurements of their impact. This occurs at times because international agencies, such as IDB, ADB or World Bank, may require it as part of their financial contribution.

⁴ This program is called PROBECAT (the Spanish acronym for *Programa de Becas de Capacitación para Desempleados*). It had yearly registration of less than 200 000 persons in 1994. *Cfr.* Revenga *et. al.* (1994) and was expanded twofold when the country entered a major recession in 1995. The number of trainees increased in the subsequent years to reach a peak number of 590 000 in 2000.

⁵ *Cfr.* Meyer (1990), where the development and advantages of this estimation strategy to labor market analysis has been illustrated. His strategy, in turn, is an extension of Heckman and Singer (1984) approach. Related studies for developed countries that use this technique are Ham and LaLonde (1996), Bonnal *et. al.* (1997) and Eberwein *et. al.* (1997) & (2002)).

The results obtained indicate that the average impact on working days attributed to program participation is sixty nine on female participants and forty three on male participants. They suggest that even when post-training wages remain at pre-treatment levels, benefits in yearly income earnings of participants might be enough to compensate a large part of the costs of the program. Our analysis also illustrates two misleading results obtained when the effect of possibly omitted determinants in the exit from unemployment (unobserved heterogeneity of individuals) is not accounted for. One of them is that female heads of household spend more time looking for jobs than other women in the labor market. The other one is an inconsistent estimate of the parameters of the hazards function, thereby erroneously implying that the program is not effective in improving employability of participants.

The structure of the paper is as follows: the data set is described in section 2, with an emphasis on information about re-employment dynamics. We also explain there the procedure that matches a trainee with a member of the non-participant group. The statistical framework used in this evaluation is presented and discussed in section 3. Section 4 presents the results and remarks about the impact of the program on its beneficiaries and about cost-benefit analysis, as applied to this type of program. The final section presents conclusions.

2. Characteristics of the Data set

Re-employment dynamics

We use a data set that registered the re-employment dynamics of participants in a program and of members of an appropriate control group. The program consisted of courses and training offered to unemployed individuals with previous working experience, for an average of three months, in publicly funded institutions associated with either the Ministries of Education or Labor.⁶

In addition to time needed to find a job after their training, the survey registered whether respondents were still in their first post-training job at the moment of the interview (September 1994).⁷

If that was not the case, it registered the length of time during which they kept that job. The survey also captured the number of weeks in unemployment already experienced prior to joining the program.

The key feature of this survey is that it enabled us to construct job histories for each interviewed person. Figure 1 illustrates an example of an individual that is interviewed at point in time t_5 . It shows that, prior to joining the program, he had already experienced a period of unemployment, u_0 . His job history shows that after the end of his training (which occurred at t_0), he experienced an initial unemployment spell, u_1 , (which captures how fast he found a job). At time t_1 , he found a job, e_1 , where he was employed by a number of weeks represented by the difference between t_5 and t_1 .

(Figure 1)

⁶ Participants received allowances equivalent to one minimum wage while enrolled in the program, plus transportation and partial health insurance coverage.

⁷ Their training began on the first quarter of 1993.

The survey was applied to 1786 trainees and to 437 members of a control group. Of these, 1432 participants were males and 354 females; corresponding figures in the control group were 273 and 164.

Survivor rates in employment and unemployment

Data on the time spent in each job status revealed that after finishing their training, 34% of male participants had already found a job within a month; one out of three was still unemployed by the end of the fourth month; one out of four remained unemployed by the middle of the year and 12% spent more than 360 days unemployed.

(Table 1)

This appears in the first column of table 1, which shows the proportion of men that remained unemployed after finishing training. In turn, figures in the second column of table 1 indicate that, while 76% of them stayed in their job for at least four months, only two out of three men lasted longer than six months in their first post-training job and only half of them stayed for at least one year. In contrast, figures from the third and fourth columns show that the unemployment rates of female participants were significantly higher during the periods examined. Barely half of these women had found a job within six months and 32% remained unemployed a year after finishing the training. In addition, although employment retention rates for these women were similar in pattern to those of the men, the survival rates for each date were relatively lower for women.

Matching procedure

The control group for this evaluation was comprised of eligible persons that did not participate in the program. Unlike members of control groups that characterize experimental evaluations, they were not sampled from the set of individuals that intended to participate but were refused enrollment into the program. Their answers are needed to infer counterfactual outcomes for participants, namely what the beneficiaries of the program would have experienced had they not participated. In Figure 2 two job histories are drawn. The one below represents an hypothetical scenario of what would have happened with the individual represented in the upper part, if he had not participated. The case illustrated there is consistent with an effective program. In his counterfactual job history, the individual would have had only a casual job with duration given by the difference between t_3 and t_2 , which took him longer to found.

The Empirical survivor functions corresponding to members of the control group (last columns of table 1) indicate the percentage of individuals that stayed in each job status.

Due to the non-experimental nature of this evaluation, we used a matching method to pair each trainee with a member of a comparison group with similar pre-program observable characteristics. The assumption that justifies the use of matching methods for an unbiased evaluation of this program is this: conditional on a vector of observable characteristics, W , the employment and unemployment spells of the set of non-participants have the same distributions that participants would have experienced had they had not participated in the program. In view of the large number of pre-treatment observable characteristics, we applied the propensity score method variant of matching

(Rosenbaum and Rubin (1983)). This variant has the advantage of reducing the dimensionality of the matching problem down to matching on one scalar, while considering the importance of all pretreatment variables included in the analysis. This scalar is the propensity score, $P(W)$, defined as the probability of participation in the program conditional on pre-treatment observable characteristics.

A logit-regression was estimated to have each individual's propensity score, $P(W)$. We incorporated as predictor variables in the logit regression the following pre-program observable characteristics of individuals (results relegated to the appendix): geographic zone where the individual is located; four demographic characteristics: age, family position, education and civil status; time spent without a job previous to their training; three characteristics of previous job: part or full time, formal or informal sector⁸ and whether the individual was wage earner or self-employed; reason why previous job was left (three possibilities): 1) marriage, care of children or relatives, 2) contract finishing or plant closure and 3) unsatisfied with the job or left to study; We also included occupation in their last job.

We identified for each of the treatment group members a corresponding match from the set of control group, subject to the following two requirements: both of them had to be of the same sex and the absolute differences in their propensity score values should not be no larger than .01. When there was more than one control candidate for a trainee, the matched person was randomly selected among non-participants fulfilling this *criterion*.

Following this method we matched 1390 couples for men and 322 for women. In table 2 we present the parameters of the distribution of these two groups. Working with these subsets of the original sample, we have treatment and control groups that are distributed almost as if they were obtained from a "balanced experiment". The comparison of the number of trainees that could be included in our analysis with those in the original data set implied working with 89.5% of available men and available 86.3% women. The incurred wastage of information is the cost to be paid for not having to worry about biases caused by differences in the support of the distribution of $P(W)$ for each group, or in the shape of the distribution over the common support.

(Table 2)

3. Specification of the model

To calculate the impact on re-employment dynamics of the program's beneficiaries, we follow the semiparametric estimation procedure originally proposed and used by Meyer (1990)- namely, a piece-wise proportional hazard model that allows for unobserved heterogeneity correction. We discuss first the general characteristics of these models and then the specific assumptions to address heterogeneity.

hazard function specification

Survival models take as the point of departure the definition of a nonnegative continuous random variable T , which represents the spell duration with a density function, $f(t)$. This function $f(t)$ has a corresponding survivor function, $S(t)$, simply defined as $1-F(t)$, i.e. as the probability that duration will equal or exceed the value t (where $F(t)$ is the distribution function). The hazard function, $h(t)$, is given by:

⁸ Formal sector is defined as having access to social security and other non-wage benefits.

$$h(t) = \frac{f(t)}{S(t)} \quad (1)$$

In this relationship, $h(t)$ can be interpreted as an exit rate or escape rate from the state in consideration. It is the limit (as Δt tends to zero) of the probability that a spell terminates in interval $(t, t+\Delta t)$, given that the spell has lasted t periods⁹.

The ‘mixed proportional hazard’ specification estimated in the next section has two parts: a ‘baseline’ hazard and a ‘systematic part’ which takes form of an exponential function. Thus, the hazard rate is multiplicative in all the separate elements of the covariates. In addition, we specify that the systematic part is composed of two parts: observed individual characteristics, X , and a binary dummy variable, Z , indicating whether or not the individual is in the treatment group,¹⁰ viz:

$$h(t | X, Z) = h_0(t) \exp(X\mathbf{a} + Z\mathbf{b}) \quad (2)$$

The baseline hazard, $h_0(t)$, captures the common hazard among individuals in the population, while the systematic part captures the individual observed heterogeneity through the effect of a set of covariates on the hazard rate. As it is done in Meyer (1990) we allow for a flexible duration dependence in our estimation by using a step function for $h_0(t)$. That is, we estimate a piecewise-linear baseline hazard with a number of breakpoints.

In turn, β is the vector of parameters to be estimated and so is the scalar λ . The larger the exponential of the parameters in β are, the more probable it is that the individual with characteristics represented by X will exit the job status, given that the spell has lasted t periods.

Semiparametric specification of unobserved heterogeneity and time dependence

Also following Meyer (1990) in the specification of unobserved heterogeneity across individuals we assume that, if this is present, it is independent of the covariates, its distribution can be approximated with two points of support and that it enters the hazard function multiplicatively. Additionally, in view of the multi-spell nature of the re-employment dynamics under analysis, we assume that it is independent across spells.¹¹ Hence, the hazard function to be estimated becomes:

$$h(t | X, Z) = h_0(t) \Omega_i \exp(X\mathbf{a} + Z\mathbf{b}) \quad (3)$$

⁹ Some people who started a spell of employment/unemployment in a given job status may still have been in the same status when they were last interviewed. Data for these people are called censored, and they would constitute a problem for a standard regression model where the dependent variable was the length of the spell. If we exclude people with unfinished spells, we throw away part of the data set and introduce a serious bias against people with longer and more recent spells in each of the job statuses. Duration models have the distinct advantage of being able to handle censored data effectively (Kiefer (1988).

¹⁰ This assumes that the different services provided by the multidimensional nature of the training program are adequately captured by a single binary variable.

¹¹ By construction, the duration of the first post-training job, t_e , starts after the moment at which the first spell of unemployment t_{u1} is realized. A multispell specification of these models would have enable us to capture the dependence between states by including t_{u1} as an additional covariate in the hazard for t_e . *Cfr.* Van den Berg (2000) and Calderón-Madrid and Trejo (2001).

Where O_i is a random variable that is assumed to be independent of X and Z . As it is commonly done, we assume a specific shape for the distribution of the variable O_i . In our estimations, the covariates of the systematic part X were: individual characteristics such as head of household, level of formal education, age, and marital status, as well as the following ones: time spent without a job before the date in which training programs started; characteristics of his/her previous job according to whether it was in formal or informal sector, whether it was part or full time and if the person was self-employed or wage earner; type of occupation; and reasons why the previous job was left¹²

4 Results

Hazards out of unemployment and of employment were estimated separately for males and females. The results are presented in tables 3 - 5. We estimated two cases. One in which unobserved heterogeneity is controlled for and the other in which it is not. As stated in the previous section, we assume that¹³:

Unobserved heterogeneity is independent of the covariates, independent across spells and is drawn from a distribution with two points of support, v_1 and v_2 , with probability p_1 and p_2 respectively.

As the last four rows of tables 3 and 4 indicate, the statistically significant values of the two point distribution v_1 and v_2 imply that controlling for unobserved heterogeneity was required in the case of exits from unemployment, both for female and male participants. By contrast, we rejected the hypothesis of biases due to unobserved differences across individuals in the case of hazards out of employment for male and female participants. Two explanations for this result differing with that of unemployment are the following ones. First, only individuals that found a job are included in estimations in table 5, after a corresponding matching procedure was conducted when required. That is, we might have been left with a group without unobserved heterogeneity across individuals; those that presented it remained unemployed. Second, unobserved heterogeneity, but dependent across spells, might prevail, but this is not captured by our estimations because we explicitly ruled it out with our statistical assumptions.¹⁴

Results of the coefficients for the treatment variable Z in columns 2 and 5 in these tables indicate that ignoring unobserved heterogeneity leads to erroneous interpretations about the effectiveness of the program in helping both females and males out of unemployment. The effect of the program on hazard rates out of unemployment and of employment is given by the estimated coefficient for this dummy variable which indicates program participation. A positive impact on re-employment dynamics is implied when the exponential value of this coefficient is above (below) unity in hazards out of unemployment (employment).

¹² These are presented and detailed in the appendix.

¹³ Our procedure maximized a likelihood function with respect to both the location and amount of probability mass at each of the point of support.

¹⁴ A multiple spell estimation that relaxes the assumption of independence between the covariates and the heterogeneity term is left for future research, as a way to have a more general specification.

It is also interesting to point out two counter-intuitive results implied by the last columns of table 3. According to the 10th, 11th and 4th rows, hazards out of unemployment have the following implications, when biases due to unobserved heterogeneity are not eliminated: a) female head of households do not leave unemployment faster than daughters living with their parents and b) women who left their previous job because the plant closed or because their contract ended do not leave unemployment faster than the rest (except those that left their previous job due to marriage or care of their children or relatives). Once unobserved heterogeneity is controlled for, these results no longer hold: female head of households find jobs twice as fast as daughters do and women that leave unemployment faster are those that were in that status because their contract finished or because the plant they were working at closed.¹⁵

Our nonparametric estimation of the time dependence of the hazard function indicates that no flexibility of duration dependence was required for the case of exits out of unemployment (first and second rows). By contrast, the first five rows in tables with hazards rates out of employment indicate that these first increase and then start declining -as suggested in theoretical economic models of job-matching and turnover (Jovanovic (1979)).

The main conclusions of the impacts of the program on re-employment dynamics of participants are:

1. We reject the hypothesis that the impact on female participants' transitions out of unemployment is statistically insignificant, once biases due to unobserved heterogeneity are eliminated. This implies that the program is effective in helping them find a job faster. The opposite is the case for male participants: once we control for unobserved heterogeneity, we conclude that the program does not help them in finding a job faster. Our results indicate that, for females there is a reduction of 35% approximately in the time needed to find a job with respect to the counterfactual (not taking a training program, as captured by the comparison group)¹⁶

2. By increasing the time they hold on to their jobs, relative to what it would be the case if they have not participated, females and males participants benefited from the services provided by the program.

This result for male participants illustrates another case that could lead to erroneous interpretations about the effectiveness of the program: an evaluation might suggest that the program is not effective in improving employability of participants, when there is an exclusive concentration on the program's impact on leaving unemployment.¹⁷

¹⁵ As it is shown in the second column of table 3, they find a job more than twice as fast than those who left it to study and almost fifty percent faster relative to those that left their jobs due to a dissatisfaction with them.

¹⁶ For females' hazard rates out of unemployment, the impact of the program in terms of percentage reduction in time required to find a job is calculated as follows. The exponential of the parameter affecting the variable Z (dummy for participating in the program) is 1.55. With this figure we get an impact of approximately 0.645 (1/1.55) in the expected time to find a job which is attributed to their training. This implies a reduction of 35% in the time to find a job, relative to the case of not participating. In view of the unemployment hazard function specification, the standard normalization for unobserved heterogeneity sets expected value equal to one. Hence in this case we refer to observations whose average value of the unobserved heterogeneity has an unadjusted hazard.

¹⁷ This is the conclusion that Quentin and Minowa (2001) arrived to in evaluating this program. (They only considered the impact of training on hazard out unemployment).

As showed in our analysis, the impact on re-employment dynamics is positive because they retain their jobs for a longer period of time, even if the program is not effective in helping them find a job faster.

(Tables 3, 4, 5)

Incorporating impact on re-employment dynamics in the cost benefit analysis

The analysis conducted in this paper highlights cases in which benefits attributed to improving re-employment dynamics of participants might, on their own, compensate the cost of a program. That is, it indicates that a program's evaluation must go beyond the impact on wages of beneficiaries or on the time out of unemployment. With estimates such as the ones conducted here, an integral cost-benefit analysis can quantify, in addition to a program's impact on wages, its impact on earnings due to additional weeks individuals work in a year, relative to what would have been the case if they have not joined the program.

To illustrate this point, we use the parameter that captures the effect of the program on hazard rates out of employment and calculate the percentage increase in the time participants retained their jobs -relative to what would have been the case if they had not participated in the program. The average impact on working days attributed to program participation is presented in table 6¹⁸: sixty nine on female participants and forty three on male participants. With these results, and an assumption of post-training wages remaining at pre-treatment levels, a first approximation to benefits in yearly income earnings of participants can be obtained for cost-benefit analysis.

There is an alternative procedure that has as additional advantage not only more precision, but also that allows for an extension of the time framework for more than a year. This procedure is the following one: with the hazard ratios, computed in (3), the survival and baseline function can be calculated by:

(Table 6)

$$S(t; x) = S_0(t)\Omega_1 h_0(t) \exp(X\mathbf{a} + Z\mathbf{b}) \tag{4}$$

$$S_0(t) = \exp\left(-\int_{-0}^t h_u du\right) \tag{5}$$

Where $S(t;x)$ is the survivor function represented by $(1-F(t))$ in the denominator of (3). With these functions is possible to simulate the effects of program participation on the duration of employment. This can be computed by taking the difference between the survival-time in employment by participants and their survival time in the hypothetical case of non- participation. This procedure is followed conditioning on individual characteristics and taking selection in unobservables into account. It is worth stressing that difference between survival rates for participants and survival rates of non-participants can be obtain for periods that are beyond the time framework in which the evaluation was conducted. That is, not only for a year, but for a longer period of time.

¹⁸ The inverse of this parameter was multiplied by the average number of days that non-participants were employed during the year.

5 Conclusions

We worked with a data set that registered the duration of unemployment and of employment after training was provided by a program target at the unemployed. This information, a set of variables associated with characteristics of their previous work and with their demographic profile, enabled us estimate the program's impact on re-employment dynamics. Our results indicate that this program, which has been offered by the Mexican government for more than a decade, is more than a safety net providing temporary relief for the unemployed- as has been suggested in studies using the same data set, but ignoring a number of issues raised in this paper.¹⁹

As it might be the case of other training programs in developing countries, in this one, impact on earnings of trainees attributed to additional weeks working in a year --even in the absence of a major improvement in daily wage -- might be a benefit large enough to compensate a large part of the costs of the program.

¹⁹ *E.g. Giugale et. al. (2001).*

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Figure 1
Re-employment dynamics of trainees

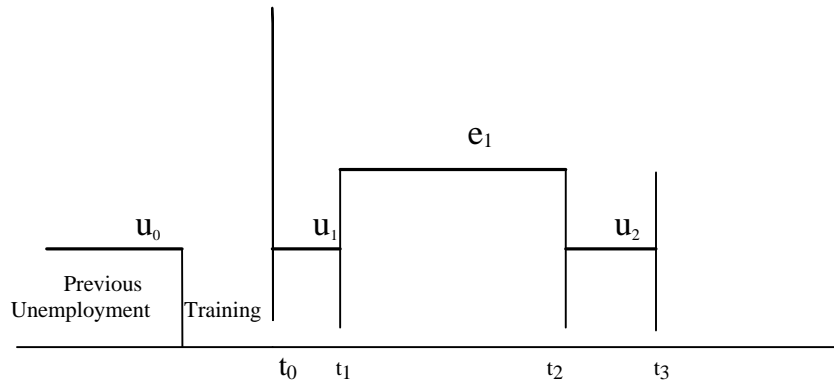


Figure 2

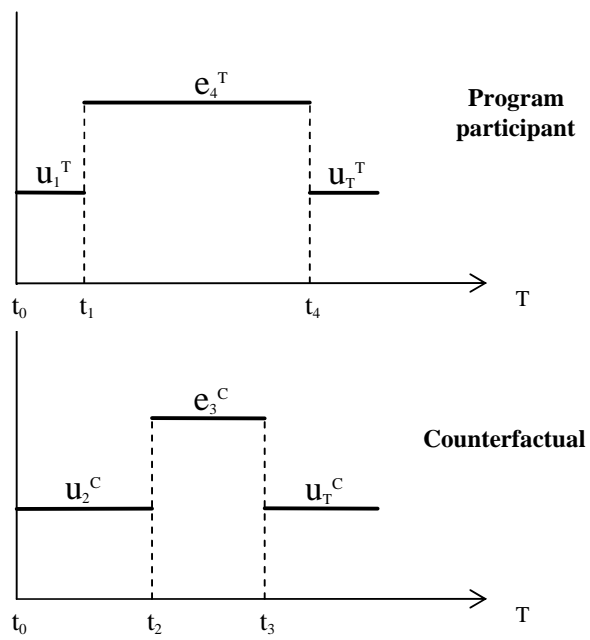


Table 1
Kaplan Meier Survivor Functions
(Proportion remaining in each job status)

Interval in days	Participants				Control group			
	Men		Women		Men		Women	
	Unemploy ment	Employ ment	Unemploy ment	Employ ment	Unemploy ment	Employ ment	Unemploy ment	Employ ment
0 30	0.66	0.95	0.79	0.94	0.8	0.96	0.87	0.98
30 60	0.48	0.9	0.71	0.88	0.63	0.95	0.73	0.97
60 90	0.39	0.85	0.66	0.82	0.42	0.89	0.64	0.93
90 120	0.33	0.76	0.6	0.73	0.35	0.82	0.61	0.86
120 150	0.29	0.71	0.56	0.67	0.31	0.76	0.54	0.75
150 180	0.25	0.68	0.53	0.64	0.27	0.67	0.48	0.71
180 210	0.21	0.65	0.49	0.62	0.23	0.64	0.45	0.63
210 240	0.19	0.62	0.47	0.57	0.21	0.6	0.41	0.57
240 270	0.17	0.59	0.43	0.56	0.18	0.55	0.4	0.56
270 300	0.16	0.57	0.4	0.52	0.17	0.51	0.37	0.54
300 330	0.14	0.54	0.37	0.49	0.14	0.49	0.35	0.44
330 360	0.12	0.51	0.33	0.46	0.13	0.42	0.34	0.44
Number of observations	1432	1369	354	268	273	224	164	99
Censored spells	161	773	114	153	238	96	108	41
Completed spells	1271	596	240	115	35	128	56	58

Table 2
Unemployed Individuals With Prior Working Experience

	Number of Observations	Propensity Score				
		Mean	Median	Min	Max	Std. Dev.
Men						
Control Group	1390	0.889	0.938	0.065	0.986	0.132
Participant Group	1390	0.891	0.94	0.065	0.996	0.133
Women						
Control Group	322	0.848	0.912	0.126	0.977	0.169
Participant Group	322	0.849	0.911	0.123	0.986	0.169

Table 3
Hazard Functions
Female Unemployment Spell

Variable	A		B	
	β	exp(β)	β	exp(β)
h(1)	0.0001 (0.08)		0.0027 (0.02)	
h(2)	0.0003 (0.07)		0.0025 (0.02)	
Left job due to marriage or care of relative	-1.51 (0.00)	0.20	-1.27 (0.00)	0.28
Left job due to contract finishing or plant closure	0.80 (0.00)	2.23	-0.01 (0.48)	0.99
Left job voluntarily due to dissatisfaction or change of address	0.38 (0.04)	1.46	-0.07 (0.37)	0.94
Northern Region	-0.05 (0.42)	0.95	-0.34 (0.02)	0.71
The Coast	0.03 (0.46)	1.03	-0.46 (0.00)	0.63
The South states	-0.96 (0.00)	0.38	-0.43 (0.01)	0.65
Mexico City and Central Area	-0.47 (0.03)	0.63	-0.51 (0.00)	0.60
Head of household	1.06 (0.00)	2.87	0.38 (0.03)	1.46
Daughter	0.35 (0.13)	1.42	0.44 (0.04)	1.55
Age	0.01 (0.24)	1.01	0.02 (0.02)	1.02
Full time wage-earner, formal sector	-0.17 (0.28)	0.84	0.34 (0.06)	1.40
Full time wage-earner, informal sector	-0.86 (0.00)	0.42	-0.19 (0.20)	0.83
Part time wage-earners	-1.36 (0.00)	0.26	-0.62 (0.01)	0.54
Full time self employed	-0.83 (0.02)	0.44	0.15 (0.31)	1.16
Single	0.10 (0.36)	1.10	0.04 (0.42)	1.04
Unempl. between one and two months	1.53 (0.00)	4.63	0.60 (0.00)	1.83
Unempl. between two and three months	0.81 (0.00)	2.24	0.11 (0.29)	1.12
Unempl. between three six months	0.34 (0.10)	1.41	-0.29 (0.07)	0.75
Unempl. more than six months	-0.36 (0.09)	0.70	-0.42 (0.02)	0.66
Complete primary and incomplete secondary school	0.40 (0.15)	1.50	0.36 (0.11)	1.43
Post-primary courses	-0.20 (0.35)	0.82	-0.26 (0.28)	0.77
Incomplete secondary school	1.50 (0.00)	4.50	0.42 (0.10)	1.52
Complete secondary education or incomplete post-secondary school training	0.48 (0.13)	1.62	0.27 (0.18)	1.31
Complete post-secondary school training courses	0.10 (0.42)	1.10	0.25 (0.21)	1.28
Incomplete high school	-0.16 (0.37)	0.85	0.03 (0.47)	1.03
Complete high school	-1.52 (0.00)	0.22	-0.30 (0.22)	0.74
Education above the previous	-0.64 (0.11)	0.53	-0.63 (0.07)	0.53
Z (treatment group=1, 0 otherwise)	0.44 (0.00)	1.55	-0.25 (0.02)	0.78
V1	1.00 (0.00)		1.00 (0.00)	
V2	26.91 (0.00)			
P1	0.34 (0.00)		1.00 (0.00)	
P2	0.66 (0.00)			

P-value in parenthesis A: Controlling for Unobserved Heterogeneity , B: Not Controlling for Unobserved Heterogeneity

Table 4
Hazard Functions
Male Unemployment Spell

Variable	A		B	
	β	exp(β)	β	exp(β)
h(1)	0.0100 (0.00)		0.0100 (0.00)	
h(2)	0.0300 (0.00)		0.0200 (0.00)	
Northern Region	-0.25 (0.00)	0.78	-0.11 (0.04)	0.90
The Coast	-0.28 (0.00)	0.76	-0.10 (0.07)	0.91
In Bond (maquiladora) Northern Region	-0.15 (0.13)	0.86	-0.11 (0.16)	0.90
The South states	-0.29 (0.00)	0.75	-0.23 (0.00)	0.80
Mexico City and Central Area	-0.22 (0.00)	0.80	-0.16 (0.01)	0.85
Left job due to marriage or care of relative	0.54 (0.06)	1.72	0.29 (0.19)	1.34
Left job due to contract finishing or plant closure	0.35 (0.00)	1.42	0.32 (0.00)	1.38
Left job voluntarily due to dissatisfaction or change of address	0.40 (0.00)	1.50	0.45 (0.00)	1.57
Head of household	0.07 (0.21)	1.07	0.20 (0.00)	1.22
Age	-0.02 (0.00)	0.98	-0.02 (0.00)	0.98
Full time wage-earner, formal sector	0.26 (0.03)	1.30	0.20 (0.03)	1.22
Full time wage-earner, informal sector	0.26 (0.03)	1.30	0.31 (0.00)	1.36
Part time wage-earners	-0.73 (0.00)	0.48	-0.41 (0.00)	0.67
Full time self employed	0.12 (0.2)	1.13	0.12 (0.14)	1.13
Single	-0.63 (0.00)	0.53	-0.34 (0.00)	0.71
Unempl. between one and two months	0.01 (0.45)	1.01	0.02 (0.39)	1.02
Unempl. between two and three months	-0.33 (0.00)	0.72	-0.15 (0.03)	0.86
Unempl. between three six months	-0.12 (0.1)	0.89	-0.09 (0.1)	0.92
Unempl. more than six months	-0.91 (0.00)	0.40	-0.65 (0.00)	0.52
Complete primary and incomplete secondary school	-0.39 (0.00)	0.68	-0.15 (0.09)	0.86
Post-primary courses	-0.30 (0.19)	0.74	-0.30 (0.19)	0.74
Incomplete secondary school education	0.13 (0.16)	1.14	0.03 (0.38)	1.04
Complete secondary education or incomplete post-secondary school training	-0.20 (0.05)	0.82	-0.13 (0.11)	0.88
Complete post-secondary school training courses	-0.38 (0.04)	0.69	-0.31 (0.05)	0.74
Incomplete high school education	-0.06 (0.35)	0.94	-0.07 (0.3)	0.93
Complete high school education	-0.29 (0.03)	0.75	-0.26 (0.02)	0.77
Education above the previous	-0.16 (0.16)	0.85	-0.10 (0.24)	0.91
Z (treatment group=1, 0 otherwise)	-0.04 (0.24)	0.96	0.11 (0.00)	1.12
V1	1.00 (0.00)		1.00 (0.00)	
V2	16.44 (0.00)			
P1	0.74 (0.00)		1.00 (0.00)	
P2	0.26 (0.00)			

P-value in parenthesis A: Controlling for Unobserved Heterogeneity , B: Not Controlling for Unobserved Heterogeneity

Table 5
Hazard Functions
Employment Spell

Variable	Male		Female	
	β	$\exp(\beta)$	β	$\exp(\beta)$
h(1)	0.0170 (0.02)		0.0500 (0.11)	
h(2)	0.0250 (0.02)		0.0600 (0.11)	
h(3)	0.03 (0.02)			
h(4)	0.02 (0.02)			
h(5)	0.03 (0.02)			
Northern Region	0.28 (0.01)	1.32	0.10 (0.34)	1.10
The Coast	-0.06 (0.29)	0.94	-0.10 (0.35)	0.90
In Bond (maquiladora) Northern Region	0.14 (0.05)	1.15	-0.31 (0.17)	0.73
The South states	0.17 (0.03)	1.18	-0.16 (0.26)	0.85
Left job due to marriage or care of relative	0.44 (0.00)	1.55	-0.48 (0.12)	0.62
Left job due to contract finishing or plant closure	-0.02 (0.45)	0.98	-1.09 (0.00)	0.34
Left job voluntarily due to dissatisfaction or change of address	-0.16 (0.05)	0.85	-0.61 (0.03)	0.54
Head of household	-0.18 (0.03)	0.84	0.89 (0.00)	2.43
Age	-0.22 (0.00)	0.81	-0.15 (0.15)	0.86
Full time wage-earner, formal sector	0.19 (0.14)	1.21	0.20 (0.26)	1.22
Full time wage-earner, informal sector	-0.23 (0.1)	0.80	0.09 (0.39)	1.09
Part time wage-earners	0.24 (0.14)	1.27	0.55 (0.08)	1.74
Full time self employed	0.25 (0.08)	1.29	-0.13 (0.4)	0.88
Single	-0.03 (0.36)	0.97	-0.12 (0.35)	0.89
Complete primary and incomplete secondary school	0.36 (0.03)	1.44	-0.12 (0.35)	0.89
Post-primary courses	-0.60 (0.28)	0.55	-0.57 (0.1)	0.57
Incomplete secondary school education	0.49 (0.00)	1.64	0.52 (0.26)	1.68
Complete secondary education or incomplete post-secondary school training	0.04 (0.41)	1.05	-0.26 (0.28)	0.77
Complete post-secondary school training courses	0.12 (0.34)	1.12	-0.39 (0.21)	0.68
Incomplete high school education	0.25 (0.12)	1.28	-0.49 (0.22)	0.61
Complete high school education	0.03 (0.45)	1.03	0.12 (0.43)	1.12
Education above the previous	-0.35 (0.08)	0.70	-1.26 (0.13)	0.28
Z (treatment group=1, 0 otherwise)	-0.18 (0.00)	0.84	-0.42 (0.02)	0.66
V1	1.00 (0.00)	0.00	1.00 (0.00)	0.00
V2	1.00 (0.01)	0.00	1.00 (0.00)	0.00
P1	0.14 (0.00)		0.46 (0.00)	
P2	0.86 (0.44)		0.54 (0.28)	

P-value in parenthesis

Table 6

	Female		Male	
	Average Number of Days Spent in each status during the year		Average Number of Days Spent in each status during the year	
	If Treated	If Untreated	If Treated	If Untreated
Unemployment	75.48	144.94	87	130.82
Employment	198.46	129	257.83	214

Appendix

Table 7
LOGIT Regression for the matching procedure

Variables	Coef	Z
Left job due to marriage or care of relative	-1.81	-4.70
Left job due to contract finishing or plant closure	-2.02	-7.87
Left job voluntarily due to non-satisfaction or change of address	-1.21	-4.74
Northern Region	1.73	5.77
The Coast	0.61	2.12
In Bond (maquiladora) Northern Region	0.32	1.10
Southern States	1.91	5.44
Head of household	1.15	3.79
Son/Daughter	0.25	0.87
Single	-0.04	-0.11
Un. between one and two months	-0.58	-2.44
Un. more than two and up to three months	0.18	0.87
Un. more than three and up to six months	0.34	1.44
Un. more than six months	0.75	3.48
Full time wage-earner, formal sector f.f.m35	0.47	2.12
Part time wage-earners	1.80	8.06
Full time self employed	1.65	5.29
Full time wage-earner, informal sector i.f.m35	1.74	6.24
Education (5 categories)	1.61	6.76
Previous job occupation (9 types)		
Sex	-0.40	-2.25
Age	0.41	8.45
Age Squared	-0.005	-7.98
School2	1.33	4.84
School3	0.87	1.44
School4	2.19	6.94
School5	2.66	9.34
School6	2.13	4.02
School7	0.67	2.02
School8	2.42	6.91
School9	2.70	7.56
Ocu1	0.33	1.06
Ocu2	2.90	4.98
Ocu3	1.01	4.38
Ocu4	1.57	4.87
Ocu5	1.50	6.09
Ocu6	0.23	0.64
Ocu7	0.81	3.23
Ocu8	1.35	5.13
Ocu9	1.24	4.33
Ocu10	1.61	3.58
Constant	-8.80	-8.51

