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The Impact of Government-Sponsored Training Programs on Labour Market Transitions

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1 Introduction

The impact of government-sponsored training programs has been extensively studied in the past couple of decades. Huge amounts have been poured into various skill-enhancing programs or job search assistance by the federal and provincial governments alike with an aim at facilitating self-sufficiency. Yet, many have expressed concern about the skill level of the canadian labour force [OECD, 1998] and have questioned the ability of traditional programs to address the problem.

The discussion surrounding the efficiency or desirability of government-sponsored training programs rests on complex methodological issues. Indeed, there exists no consensus in the evaluation literature concerning proper means of measuring the programs' likely impact. The central issue concerns behavioural adjustments to the mere existence of the programs. Simply put, if those who participate into a training program are "different" on average from those who do not participate, then it becomes very difficult for the analyst to assess the true program impact. For example, if participants are on average more motivated to work than non-participants, than they are likely to have higher earnings or employment rates following participation. Yet the difference between the two groups may have more to do with motivation than program participation *per se*. Naturally, the converse may also hold.

Two approaches have been proposed in the evaluation literature to address the so-called issue of "self-selection". The first is the "experimental approach", based on random assignment of applicants into treatment or control groups. The second is the "non-experimental", or "econometric approach", that relies on micro-data and complex statistical models. Each approach tackles the self-selection issue from a different angle, but the relative merit of each is still the subject of a heated debate [see Heckman and Smith (1995),Burtless (1995), Ham and LaLonde (1996)].

Most would argue that the "experimental" approach is best suited to eliminate self-selection biases and provide adequate mean program impacts, however measured. Yet, recently this view has been challenged by Ham and LaLonde (1996) in their important paper. In essence they argue that random assignment between control and experimental groups provides an adequate *short-term* mean program impact. On the other hand, the treatment and controls experiencing subsequent spells of employment and unemployment are most likely not random subsets of the initial groups because the sorting process is very different for the two. In other words, random assignment does not guarantee that long-term mean program impacts are void of any systematic biases.

The dichotomy between the "experimental" and "econometric" approaches usually arises in a cross-sectional framework. When longitudinal data is available, unbiased short-term and long-term program impacts can easily be computed even if assignment is not random. Different estimators have been proposed in recent years to accomplish this and are usually referred to as "fixed-effects" or "first-difference" methods [see Ashenfelter and Card (1985), Heckman and Hotz (1989), Heckman and Robb (1985), Moffitt (1991)].

In this paper we investigate the impact of federal and provincial training programs aimed at welfare and unemployment insurance recipients. We use a unique longitudinal dataset that contains information on the employment, unemployment and welfare spells that were experienced by a large number of individuals between 1987 and 1994. We rely on first-difference estimators to study the impact of training programs on the duration of employment, unemployment and welfare spells. Our results show that some programs have substantial impacts on these durations and that there is considerable selectivity into the programs.

Section 2 describes the data we use. Section 3 discusses the first-difference estimators. The results are presented in section 4. We conclude the paper in section 5.

2 Sampling Procedure and Data Description

The basic data used for this study are drawn from the records of Quebec's Ministère de la Solidarité sociale. The files contain information on all individuals having received welfare benefits at some time between January 1979 and December 1993. Given the size of the files, a sample of 95 514 individuals was chosen at random. It should be noted that for certain individuals the stay on welfare can be considered, for all intents and purposes, permanent. These are individuals whose physical or mental state is such that, for an indeterminate length of time or even for life, they are indisposed to work. For obvious reasons, these individuals are excluded from the sample. Thus the final sample is comprised of individuals having no handicap or only a minor, intermediate, or temporary physical handicap. Furthermore, they are fit to work.

The welfare administrative files contain no information on employment or unemployment spells. Our sample was thus linked to the Status Vector files (SV) and the Record of Employment files (ROE). Both these files contain very detailed weekly information on insured unemployment spells and employment spells, respectively. Unfortunately, the SV file could only be matched one-to-one with our sample for the period 1987–1994. As a result our final sample is composed of 54 324 cases.

Table 1 presents descriptive statistics for the entire sample. The distinction between men and women relates to the gender of the household's applicant as recorded in the welfare administrative files. It does not relate to the type of household *per se* (*i.e.*, single, two-adult, single parent, two-parent, *etc.*). On average male applicants are slightly older than female applicants, are proportionately less likely to be born in Québec, and are less concentrated in the Montreal area. The individuals in our sample are much younger than the general population Whereas roughly 21% of our sample is between 18 and 24 years of age, census data for 1991 indicates this proportion is only 12% in the population. Similarly, while approximately 13% of our sample is aged 44 years and over, this proportion is close to 42% in the 1991 census.

Both male and female applicants are poorly educated by provincial standards: the average years of schooling is 10.06 and 10.09, respectively. In both cases, as many as 83% have at most 12 years of schooling, the equivalent of a high-school degree (but not necessarily completed). In the 1991 census, these figures were 58.5% and 55.8% for women and men, respectively.

The individuals in our sample are younger and much less educated than the population at large. They are nevertheless representative of those who at one time or another become welfare participants. In assessing the impact of training programs it is important to keep in mind that the results pertain to this particular group.

2.1 Transitions on the labour market

The information available in the various administrative files allows us to identify seven different states on the labour market: welfare, unemployment, employment, welfare training, U.I. training, Job Re-entry program and inactivity.[1] For the purpose of this study a welfare spell

[1] Job Re-entry is a training program available to welfare claimants that is treated separately from other programs for reasons to be explained below. This program is also better known as PAIE (Programme d'Aide à l'Intégration en Emploi).

is defined as an uninterrupted sequence of months during which a household receives welfare benefits. Similarly, an unemployment spell is defined as an uninterrupted sequence of weeks during which an individual receives U.I. benefits. As such, it does not correspond to the usual definition according to which work must be sought to qualify as unemployed. Rather, it is a state during which an individual receives benefits.

Welfare training and U.I. training are states in which individuals actively participate in one of the many schemes available under both programs. As many as 67 032 spells of welfare training and 4 853 spells of U.I. training are recorded in the data. There are many repeat spells both in welfare and U.I. in the data. This feature of the data will be exploited in the empirical strategy. Naturally, there are many more welfare training spells than U.I. training spells since our sample is drawn from the welfare files.

Prior to 1989 there were essentially three programs available to welfare participants: (1) Remedial Education; (2) Labour Force Retraining and Upgrading; (3) Employment Experience. In 1989, three additional programs were implemented: (4) Recognition of Employability and Development Activities; (5) External Manpower Services; (6) Job Re-entry program (JR-P). Each program has its own target population and aims either at enhancing employability or at improving basic skill levels. These 6 programs, which together account for 76% of all training spells, are detailed below.

1. *Remedial Education (34 569 spells)*
   This program provides intensive classroom training to help beneficiaries obtain a high-school diploma. Only those who have left school for at least nine months are eligible. Beneficiaries are entitled to regular benefits and to special allocations to cover registration fees and day-care services.

2. *Labour Force Retraining and Upgrading (2 909 spells)*

This program aims at allowing beneficiaries to acquire basic skills through workplace training that will ease access to low and medium-skill jobs. Only those who have left school for at least 9 months and do not have a college or a university degree are eligible. Participants must work at least twenty hours per week, for a maximum of 52 weeks. Employers must give participants a monthly allowance of at least 100$ to cover related expenses (transit, commuting, *etc.*).

3. *Employment Experience (12 715) spells*
   This program allows beneficiaries to participate in various community projects that promote or enhance abilities, attitudes and behaviours that facilitate integration of the labour market. All beneficiaries are eligible to this program, although priority is given to those who have been on welfare for over a year. Participation usually lasts 12 months and must involve a minimum of 80 hours of work per month. Allowances for day-care services can be granted.

4. *Recognition of Employability and Development Activities (1 136 spells)*
   This program allows beneficiaries to take part in activities offered by external agencies that enhance employability (classroom training *etc.*). Participants are entitled to allowances to cover related expenses and day-care services.

5. *External Manpower Services (835 spells)*
   This program helps beneficiaries seek services from non-profit organisations mainly involved in job search activities or activities that are complementary to those above. Allowances are available to cover participation costs. This program is open to all beneficiaries.

6. *Job Re-entry Program (JRP) (5 328 spells)*
   The Job Re-entry program is an on-the-job training program available to those who have been on welfare for at least 6 months.[2] It is treated separately from other welfare training programs since participants usually are eligible to U.I. benefits upon completion of this program, unlike others. The Ministère de la solidarité sociale directly subsidises firms that hire welfare claimants under this program. Firms pay a (subsidised) salary and the worker pays U.I. contributions. The subsidy lasts for a maximum of 6 months. The program favours full-time jobs that may eventually become permanent.

Training for U.I. claimants is provided under a wide variety of programs. The following 5

[2] Single parents and those aged 45 and over are exempted from this condition.

programs account for 93% of all training spells observed in the data.

1. *Fee-payer (841 spells)*
   Under this program, U.I. claimants are exempted from having to search for work while on training. The claimants or a third party must pay for the training. Fee-payers must qualify for U.I. and have been out of school for more than two years. Until 1991 there were strict rules governing the type of courses a Fee-payer could take.

2. *DIR Clients (1 135 spells)*
   This program applies to all claimants who take part-time training without the sanction of the U.I. authorities. DIR clients may be disentitled for not being available for work, or may be permitted to remain eligible for U.I. if the training does not interfere with job search and if it is agreed they will accept reasonable employment offers while on training.

3. *Course costs (689 spells)*
   Under this program, claimants are eligible to have specific course costs reimbursed (Effective January 1991).

4. *Job Entry (322 spells)*
   This program focuses on women re-entering the labour force after an absence of at least 3 years, or for youths no longer required to attend school and with little labour market experience. To qualify for Job Entry, youths had to have been unemployed for at least 26 of the last 52 weeks. Priority is given to high-school dropouts.

5. *Skill shortages (1 535 spells)*
   This program provides training in designated areas of current or anticipated skill shortages. Skill shortage occupations are designated at the national level with variations across regions according to economic conditions. Training for designated occupations could last up to three years, but only clients with a minimum of 5 years in the labour force could train for longer than 1 year.

Employment spells are determined from the Record of Employment files. These files contain longitudinal information on individuals' job separation over the period from 1974 to 1996. Our analysis focuses on the period from 1987 to 1993. Thus any job that started during that period and which has ended during or prior to 1996 will have a termination date. Jobs that were ongoing at the end of 1996 will not be recorded at all. It is likely that some employment spells will not be measured in our data. Given the average length of employment spells, tough, it is unlikely that many will be missed.

Finally, inactivity is a compound state that is the complement of the other 6 states. It includes inactivity *per se*, and may include states such as full-time school attendance that simply can not be measured given the available information.

The start date and end date of each spell is used to create individual histories on the labour market. Overlaps between states are frequent and are not necessarily the result of measurement errors. It may well be, for example, that a welfare spell and a work spell overlap. Program designs do not forbid this. Given the number of possible states, it is simply not reasonable to allow these overlaps in the analysis. It was decided that, as a rule, starting dates would have precedence over ongoing spells. Thus an ongoing spell with known end date is truncated whenever a new state starts prior to the end date.[3]

The transitions matrices between all seven states are presented in Tables 2–8 for various demographic groups. All seven tables have the same setup: Cells in the top panel correspond to the total number of transitions between different states. Cells in the middle and bottom panels are row and column percentage of the top panel, respectively.

Table 2 shows the transitions for the whole sample. The cells of the first row of the top panel give the number of welfare spells that ended in any of the seven possible states. Thus 25 241 individuals left "welfare" and entered "welfare training", 1 852 entered JRP, 1 530 entered

[3]Preliminary analysis was also conducted giving the end date precedence over the start date of a new spell. The resulting transitions matrices and average durations are very robust to this strategy.

U.I., *etc.* In all, 85 851 transitions out of welfare are observed in the data. Each row has a similar meaning and represent an "origin" state. The first column of the table gives the number of spells in each state that ended in favour of welfare. Thus 5 184 welfare training spells, 241 JRP spells, 8 383 U.I. spells, *etc.* ended in welfare. Each column represent a "destination" state.

The sample comprises 54 324 individuals. The cells of Table 2 indicate that these individuals experienced as many as 452 411 transitions between 1987 and 1993, or 8.33 spells on average. The intensities of the transitions between the various states are better understood when we focus on the middle and bottom panels. Thus the middle panel shows that 29.4% of welfare spells end in welfare training spells. Nearly a third (32.7%) end in employment and a similar number end in inactivity (33.9%). Welfare training, on the other hand, ends either in welfare (37.2%), in employment (25.8%) or in inactivity (31.4%). A small portion end in JRP (4.6%). Only 11.9% of unemployment spells end in welfare, 3% end in U.I. training, 37% end in employment and 37.5% end in inactivity. Employment spells end for the most part in welfare (8.7%), in unemployment (38.9%), in another job (16.7%) or in inactivity (33.7%).

Tables 3 and 4 present transitions matrices for men and women separately. The middle panel of both tables show that the transitions between different states is very different for the two groups. The main differences relate to transitions out of welfare and welfare training. For instance, proportionately more women enter employment upon exiting welfare, and proportionately less enter welfare training. Similarly, upon completing welfare training women tend to return less to welfare and more enter employment than men. The transitions out of the other states are relatively similar across gender.

Transitions matrices computed for individuals below and above 30 years of age are presented in Tables 5 and 6. In general, younger individuals tend to transit more into employment and training, and less into welfare and unemployment. Transitions into inactivity vary considerably according to the state of origin and across the two age groups. This is not very surprising given that the state of inactivity may well represent different things for the two groups.

Finally, the transition matrices are broken down by the level of schooling in Tables 7 and 8. The two categories that we consider are split along grade 12. Surprisingly, there are little differences between the two tables. *A priori* one might have expected poorly educated individuals to transit more into welfare or unemployment, and less into employment.

The transitions on the labour market have three essential dimensions: the state of origin, the state of destination and the duration in any a given state. Tables 2–8 provide useful information on the first two dimensions. One way to represent all three dimensions simultaneously is to look at the distribution of the sample across all seven states on a weekly basis. This distribution synthesises both the transitions across states and the mean duration in each.

Figure 1 plots the proportion of individuals in each of the seven states on a weekly basis. The top portion of the figure traces out the proportion of individuals in non-training states (welfare, unemployment, employment, inactivity), and the bottom portion traces out the proportions in training states (U.I. training, welfare training and JRP). There are two distinct features that arise in January 1987 in the top portion of the figure. First, the proportion of individuals that are inactive is relatively high and second, the proportion of unemployed individuals is zero. These two features are closely linked together. As mentioned earlier, the information on unemployment spells is only available as of January 1987. Consequently, only new spells are identifiable in the data. Spells that were ongoing in January 1987 are classified as "inactivity" in the figures. The gradual decrease in the proportion of inactive individuals is thus partly related to the increase in fresh unemployment spells.

The bottom portion also indicates that the proportion of individuals in JRP is zero up until approximately January-February 1990. This program was implemented in August 1989 and had too few participants in the beginning months to show up in the figure. Similarly, participation in U.I. training programs is essentially zero up until February-March 1987. U.I. training usually occurs after a number of weeks has been spent unemployed. Not surprisingly, then, a certain laps of time is needed before the proportion of U.I. trainees is large enough to show up in the figure. Training spells that were ongoing in January 1987 are classified as "inactivity" and thus help explaining the large proportion of inactive individuals in the top portion of the figure.

Unemployment spells as we have defined them usually last 52 weeks at most. It is thus probably safe to consider the proportions within each state as more reliable roughly as of January 1988. A close look at Figure 1 reveals very interesting patterns. First, the proportion of welfare participants remains relatively constant between 1987 and mid-1990. The economic downturn of 1990 results in an steady increase in the proportion of welfare claimants until the end of 1993. In fact, the proportion increased from 17.1% in January 1988 to 37.9% in December 1993. Such an increase results from both a more important inflow into welfare and longer spell duration [see Duclos, Fortin, Lacroix and Roberge (Forthcoming) for details].

The proportion of employed individuals follows a very distinct seasonal pattern with peaks occurring around June-July and troughs around February-March of each year. Despite these seasonal fluctuations, the proportion of employed individuals increased from 20.6% in January 1988 to 24.4% in January 1990, and then gradually declined to 17.3% in January 1993. The proportion of unemployed individuals is highly negatively correlated with the proportion of employed individuals. The seasonal fluctuations almost perfectly mirror those of employment.

The bottom portion of the figure shows that the proportion of individuals engaged in government-sponsored training programs fluctuates considerably over time. Recall that a number of new welfare training programs have been implemented in 1989. Most of these programs are aimed at enhancing job search skills and usually last a few weeks. The important increase shown in the figure coincide with the implementation of these programs. A dramatic fall occurs towards the end of 1989 presumably linked to budgetary constraints associated with the economic downturn of 1990. The proportion of participants steadily increases thereafter and reaches it highest level at the end of 1993. The proportion of U.I. trainees is relatively constant throughout the whole period, with the exception of 1992. Both the U.I. training programs and JRP have relatively few participants at any point in time. The proportions of participants in these programs vary between 0.2% and 0.8% over the whole period.

The fact that few individuals are engaged in formal training at any point in time is no indication that training programs are inefficient or unattractive. Access to programs is often limited because of insufficient resources. This lack of resources raises a fundamental question: who gets selected into training ? To the econometrician, participation in a training program is the result of two separate unidentifiable processes. First, the participant has undertaken the necessary steps to take part in the program. Second, the individual responsible for the management of the program deemed the participant has eligible. These two processes are likely to be such that participants have unobservable (to the econometrician) characteristics that are systematically different from those of the non-participants. These systematics differences are at the heart of what is usually referred to as *selectivity bias* in the evaluation literature. Fortunately, given the information at our disposal it is possible to devise estimators that, under very general assumptions, will yield unbiased estimates of the programs' impacts. These estimators are presented in the next section.

3 First-Difference Estimators

As mentioned earlier, the identification of program impacts using non-experimental data is feasible under certain conditions. In what follows we briefly sketch our estimation strategy and indicate the conditions that are required to obtain unbiased estimators.

Assume that the training programs under investigation impact an outcome variable denoted Y. For notational purposes, let

Y_{it}^{*} = the level of the outcome variable Y for individual i at time t if he/she has not participated in the program at $k < t$.

Y_{it}^{**} = the level of the outcome Y for the same individual i at time t if he/she has participated in the program at $k < t$.

Then the program impact for individual i can be written as:

$$Y_{it}^{**} = Y_{it}^{*} + \alpha. \tag{1}$$

The difference between the two levels gives the program (treatment) effect and can be written as:

$$\alpha = Y_{it}^{**} - Y_{it}^{*}. \tag{2}$$

Naturally, we cannot observe both Y_{it}^{**} and Y_{it}^{*} for the same individual i. What we do observe is the outcome variable for program participants and non-participants. Let

$$\begin{aligned} d_i &= 1 \text{ if individual } i \text{ has participated in a training program,} \\ &= 0 \text{ otherwise.} \end{aligned}$$

Then an estimate of α could be obtained by differentiating the expected, or average, value of the outcome variable:

$$\tilde{\alpha} = E(Y_{it}^{**}|\, d_i = 1) - E(Y_{it}^{*}|\, d_i = 0), \tag{3}$$

where $E(Y_{it}^{**}|\, d_i = 1)$ is the expected, or average, value of Y_{it} of the participants and $E(Y_{it}^{*}|\, d_i = 0)$ is is the expected, or average, value of Y_{it} of the non-participants. Unfortunately, equation (3) does not provide an adequate measure of the program impact as shown below. What is really needed is:

$$\hat{\alpha} = E(Y_{it}^{**}|\, d_i = 1) - E(Y_{it}^{*}|\, d_i = 1), \tag{4}$$

i.e. the difference between the expected value of Y_{it}^{**} for the participants and the expected value of Y_{it}^* had they not participated in the program. The quantity in (4) is the impact that would be measured by a random assignment of individuals between control and treatment groups. On the other hand, it is straightforward to show that equations (3) and (4) will yield the same estimate if the following condition holds:

$$E(Y_{it}^* | \, d_i = 1) = E(Y_{it}^* | \, d_i = 0). \tag{5}$$

Condition (5) states that $\tilde{\alpha}$ and $\hat{\alpha}$ would be the same if the average value of Y for those who did not participate in the training program was equal to the average value of Y that those who participated would have had, had they not participated in the program.

The above condition is very likely not to hold in many circumstances. Violation of this condition if often referred to as *selectivity bias* in the econometric literature. Many solutions have been proposed to circumvent the difficulties related to the selectivity issue. Of particular interest are the solutions proposed in the context of longitudinal data. These solutions require the availability of pre-treatment and post-treatment information on the outcome variables of both treatment and control groups. To better illustrate the benefits of using a longitudinal framework to control for selectivity into the training programs we need to expand the above model somewhat. Assume training occurs at k, with $t^{'} < k < t$. Let

$$Y_{it'}^* = X_{it'}\beta + u_{it'}, \tag{6}$$

where $X_{it'}$ is a vector of observable characteristics, β is an appropriately dimensioned vector of parameters, and $u_{it'}$ is an random variable capturing unobservable characteristics. We assume for convenience that $E(u_{it'}|X_{it'}) = 0$. Furthermore, let

$$Y_{it} = X_{it}\beta + d_i\alpha + u_{it}, \tag{7}$$

where as before d_i is a dummy variable that equals 1 if individual i has participated in a training program. As before, $Y_{it} = Y_{it}^{**}$ for those who have participated in the program and $Y_{it} = Y_{it}^{*}$ for those who have not.

When assignment into training is not random, selection bias in the estimation of α can arise because of dependence between d_i and u_{it}, *i.e.*

$$E(u_{it}|\ d_i, X_{it}) \neq 0. \tag{8}$$

This implies that $E(Y_{it}|\ d_i, X_{it}) \neq X_{it}\beta + d_i\alpha$. Consequently, an ordinary least squares regression of Y_{it} on X_{it} and d_i will not yield a consistent estimate of α.

Assume that the unobserved characteristics can be decomposed as follows:

$$u_{it} = \phi_i + \nu_{it}, \tag{9}$$

where ϕ_i is a zero-mean individual-specific component, or fixed effect, and ν_{it} is a zero-mean random component that is independent of $\nu_{it'}$ and ϕ_i. In this specification, selection into the program is assumed to depend on ϕ_i only. Define

$$
\begin{aligned}
Y_{it}^{*} - Y_{it'}^{*} &= \text{the change in } Y \text{ from } t' \text{ to } t \text{ for those who have not participated in} \\
&\quad\ \text{the program at } k\ (t' < k < t). \\
Y_{it}^{**} - Y_{it'}^{*} &= \text{the change in } Y \text{ from } t' \text{ to } t \text{ for those who have participated in the} \\
&\quad\ \text{program at } k\ (t' < k < t).
\end{aligned}
$$

The following will yield a consistent estimate of α:

$$\tilde{\alpha} = E(Y_{it'}^{**} - Y_{it}^{*} \,|\, d_i = 1) - E(Y_{it'}^{*} - Y_{it}^{*} \,|\, d_i = 0). \tag{10}$$

This estimator is called a "first-difference" estimator since it contrasts the changes in the outcome variable before and after treatment for both participants and non-participants. The differentiation across time removes the individual-specific component, ϕ_i, upon which selection into the program is assumed to depend. The consistency of the estimator follows from the assumption in (9) since

$$E(\nu_{it} - \nu_{it'} \,|\, d_i, X_{it}) = 0. \tag{11}$$

The added benefit of using longitudinal data lies mainly in the fact that the consistency of the estimator requires less stringent assumptions on the outcome variables. To see this, note that the estimator that would arise under random assignment would be:

$$\hat{\alpha} = E(Y_{it}^{**} - Y_{it'}^{*} \mid d_i = 1) - E(Y_{it}^{*} - Y_{it'}^{*} \mid d_i = 1). \tag{12}$$

This estimator would be equivalent to the first-difference estimator if

$$E(Y_{it}^{**} - Y_{it'}^{*} \mid d_i = 1) = E(Y_{it}^{*} - Y_{it'}^{*} \mid d_i = 0). \tag{13}$$

This condition requires that the *changes* in the outcome variable be the same for both the participants and the non-participants in the absence of treatment. With only post-treatment data, the condition stated in (5) requires that the *levels* of the outcome variable be the same. It is thus clear that condition (13) may hold even though condition (5) does not.

3.1 First-Difference Estimators and Duration Data

The discussion of the previous section has highlighted the benefits of using a first-difference estimator in the context of non-experimental longitudinal data. In this paper, the outcome variable we are interested in consists of time spent in a given state (employment, unemployment, *etc.*). The conditions under which duration data can be treated within a regression framework are well established in the literature [see Kiefer (1988)]. For the sake of completeness we will briefly discuss this issue in what follows.

It is customary in economics to model duration data using so-called proportional hazard models [*e.g.*, Lancaster (1990)]. This class of models states that:

$$\lambda(\tau_i) = \lambda_0(\tau_i) \exp(\delta + X_i'\beta + \phi_i), \tag{14}$$

where $\lambda(\tau_i)$ represents the instantaneous rate at which individual i will leave a given state, conditional on survival up to "τ_i" and on a random variable, ϕ_i, reflecting individual unobserved heterogeneity. This hazard rate is factored into a "baseline hazard", $\lambda_0(\tau_i)$, a regressors component that is assumed independent of time, $\exp(\delta + X_i'\beta)$, and ϕ_i. Assume that X_i only contains a dummy indicator for program participation at $t' < k < t$ and that δ is the intercept term. In what follows we will rewrite $X_i'\beta$ as αd_i, where d_i is a dummy indicator for program participation, to underline the fact that there are no other exogenous variables in the regression.

It can be shown that, conditional on ϕ_i, the probability of surviving at least until "τ_i" is given by:

$$S(\tau_i) = \exp[-\Lambda_0(\tau_i) \exp(\delta + \alpha d_i) \exp(\phi_i)], \tag{15}$$

where $\Lambda_0(\tau_i) = \int_0^{\tau_i} \lambda_0(u)du$ is the integrated baseline hazard. Equation (15) indicates that the conditional survival function is equal to the exponential of minus the integrated hazard. Equation (15) can be written as:

$$\ln S(\tau_i) = -\Lambda_0(\tau_i)\exp(\delta + \alpha d_i + \phi_i), \tag{16}$$

or equivalently,

$$-\ln[-\ln S(\tau_i)] = -\ln \Lambda_0(\tau_i) - \delta - \alpha d_i - \phi_i. \tag{17}$$

Now let

$$\varepsilon_i = -\ln \Lambda_0(\tau_i) - \delta - \alpha d_i - \phi_i. \tag{18}$$

It can be shown that ε_i follows a type I extreme value distribution [*e.g.*, Lancaster (1990), p.20]. Thus we can write:

$$-\ln \Lambda_0(\tau_i) = \tau_i^* = \delta + \alpha d_i + \theta_i + \varepsilon_i, \tag{19}$$

a linear model for τ_i^* in which the error term has a fully specified distribution, albeit not the normal distribution [see Kiefer (1988) for details].[4]

The usefulness of this model is best illustrated if we assume that the baseline hazard is constant and normalised to unity. In this case, it is easy to show that the integrated baseline hazard is equal to the duration of the spell. Therefore, equation (19) becomes:

$$-\ln \tau_i = \tau_i^* = \delta + \alpha d_i + \phi_i + \varepsilon_i, \tag{20}$$

that is, the log of the spell duration is linear in d_i and in the random terms.[5]

In terms of the previous section, we will define $Y_{it'}$ as $(-)$ the log-duration prior to training and Y_{it} as $(-)$ the log-duration following training. As before, we will assume selection into the program depends only on ϕ_i. Under these assumptions, we have:

$$\begin{aligned} Y_{it'} &= \delta_{t'} + \phi_i + \varepsilon_{it'} \\ Y_{it} &= \delta_t + \phi_i + \alpha d_i + \varepsilon_{it}. \end{aligned}$$

[4]Because of the non-normality of the error terms, the test statistics must be viewed with caution.

[5]The exponential specification used here is not quite realistic since it assumes that the hazard rate is not time dependent, which is clearly not the case for employment or unemployment spells. However, it is easy to show that one also obtains a linear model for $\ln \tau_i$ when assuming a Weibull model. This specification, which is a generalisation of the exponential model, assumes that $\lambda_0 = \gamma\tau_i^{\gamma-1}$ and thus allows the hazard rate to be increasing ($\gamma < 0$), constant ($\gamma = 0$) or decreasing ($\gamma > 0$) in duration [see Kiefer (1988)].

Consequently, the first-difference estimator will yield:

$$Y_{it} - Y_{it'} = \delta_t - \delta_{t'} + \alpha d_i + \varepsilon_{it} - \varepsilon_{it'}. \tag{21}$$

This can be rewritten as:

$$Y_{it} - Y_{it'} = a + \alpha d_i + u_{it}. \tag{22}$$

Equation (22) suggests that the least squares method is a feasible estimator of α, the programs' impact. Following the discussion above, the intercept of the regression, a, must be interpreted as the change in the hazard rate of non-participants and $a + \alpha$ as the change in the hazard rate of participants. When only post-treatment information is available, an unbiased estimator of α can not be obtained under the current statistical assumptions. Under the null assumption of no individual-specific effects and random selection into the programs, an unbiased estimator can be obtained from the following regression:

$$Y_{it} = \delta_t + \alpha d_i + u_{it}. \tag{23}$$

In this model, $\exp(\delta_t)$ is an estimate of the baseline hazard rate of participants and non-participants alike, and α is the additional impact of participation on the hazard rate. Under the null assumption, then, estimators of α in (22) and (23) should be asymptotically equivalent.

3.2 First-Difference Estimators and Survival Rates

The discussion of the previous section has shown how a first-difference estimator can be applied to duration data. The main caveat of this approach is its inability to properly handle censored durations. A natural strategy to circumvent the censoring problem is to focus on survival rates. Assume as in (14) that the hazard rate is proportional:

$$\lambda(\tau_i) = \lambda_0(\tau_i)\exp(\delta + \alpha d_i)\exp(\phi_i). \tag{24}$$

Assume as before that the hazard rate is constant through time and the baseline hazard normalised to unity. The conditional hazard function is therefore given by:

$$\lambda(\tau_i) = \exp(\delta + \alpha d_i)\exp(\phi_i). \tag{25}$$

The (conditional) survival rate is given by:

$$S(\tau_i) = \exp\left(-\int_0^{\tau_i} \exp\left(\delta + \alpha d_i\right)\exp(\phi_i)du\right) = \exp\left[-\tau_i\exp(\delta + \alpha d_i)\exp(\phi_i)\right]. \tag{26}$$

It follows that

$$\ln\left[-\ln(S(\tau_i))\right] = \ln\tau_i + \delta + \alpha d_i + \phi_i \tag{27}$$

Hence,

$$\ln\left[-\ln(S(\tau_i))\right] - \ln\tau_i = \delta + \alpha d_i + \phi_i. \tag{28}$$

Finally, if we index "before" and "after" training as previously by t' and t, we have:

$$\begin{aligned}\{\ln\left[-\ln(S(\tau_i)\right] - \ln\tau_i\}_t - \{\ln\left[-\ln(S(\tau_i)\right] - \ln\tau_i)\}_{t'} &= (\delta_t - \delta_{t'}) + \alpha d_i \\ &= a + \alpha d_i,\end{aligned} \tag{29}$$

where a, as before, represents the change in the baseline hazard between t and t' and α measures the impact of training on the baseline hazard. Note that the individual-specific effects are removed by the differentiation.

The implementation of the above estimator rests on the assumption that $S(\tau_i)$ is known. It is not possible to compute an estimate of $S(\tau_i)$ at the empirical level. On the other hand the sample survival rates, $\hat{S}(\tau)$, say, can easily be computed.[6] In a sense, the $\hat{S}(\tau)$ are "average"

[6]In the empirical section we use the Kaplan-Meier estimator of the survival rates. This estimator has many desirable properties, one being that it is a consistent estimator of $S(\tau)$ under quite general conditions, another

survival rates. Consequently, it is no longer appropriate to talk about individual-specific effects. Instead we must assume that all the participants share a common unobserved component and that all non-participants share another common unobserved component. Consequently, we can write (28) as:

$$\ln\left[-\ln(\hat{S}(\tau))\right] - \ln\tau = \delta + \alpha d + \phi, \tag{30}$$

where d is a dummy variable indicating whether $\hat{S}(\tau)$ is computed for the participants or not, and ϕ is the "group-specific" unobserved heterogeneity. The estimator in (29) becomes:

$$\begin{aligned}
\left\{\ln\left[-\ln(\hat{S}(\tau)\right] - \ln\tau\right\}_t - \left\{\ln\left[-\ln(\hat{S}(\tau)\right] - \ln\tau\right\}_{t'} &= (\delta_t - \delta_{t'}) + \alpha d, &(31)\\
\left\{\ln\left[-\ln(\hat{S}(\tau)\right]\right\}_t - \left\{\ln\left[-\ln(\hat{S}(\tau)\right]\right\}_{t'} &= (\delta_t - \delta_{t'}) + \alpha d, &(32)\\
&= a + \alpha d. &(33)
\end{aligned}$$

The estimated survival rates, $\hat{S}(\tau)$, are computed over identical discrete intervals before and after the training window. Consequently, both terms in $\ln\tau$ in (31) cancel out in (32).

being that it can also be considered a maximum likelihood estimator of $S(\tau)$ [see Lawless (1982)].

4 Econometric Results

The estimators of the previous section require pre-training and post-training information on the outcome variables of participants and non-participants alike. In this section we will study the impact of JRP, U.I. and welfare training programs on the duration of employment, unemployment and welfare spells.

4.1 Sample Selection Scheme

The first-difference estimators require information on individual durations (participants and non-participants) in different states before and after training. The spells in our data run from the beginning of 1987 to the end of 1993. We have chosen to consider only training spells that occur between January 1990 and December 1991. This choice was made for two reasons. First, many welfare training programs were implemented in 1989. Our two-year training window is large enough to allow many recipients to enter and complete a program. Second, the training window must be narrow enough so that few employment, U.I. and welfare spells that occur afterward will be censored.

To understand how the samples are selected, it is perhaps best to refer to Figure 2. The figure illustrates how the sample used to estimate the impact of U.I. training on employment duration is selected.[7] In terms of the previous section, the two-year window is equivalent to k, the period after December 1991 corresponds to t, and the period before the training window

[7] To illustrate how we select observations to measure the impact of U.I. training programs on welfare or unemployment duration, one simply replaces "Employment" by "Unemployment" or "Welfare" in the figure. Similarly, to measure the impact of welfare training programs, one simply replaces "UI with training" and "UI no training" by "Welfare with training" and "Welfare no training", respectively.

corresponds to t'.

To qualify as a participant, an individual must have taken part in a U.I. training program during the training window. Participation in other programs during the window is not allowed to avoid confusing the impact of various programs. In our example the training spell is delimited by the light-shadowed box of the top portion of the figure. Non-participants include those individuals that have experienced a U.I. spell during the training window and which have not taken part in any training programs. The unemployment spell is delimited by the light-shadowed box in the bottom portion of the figure. The employment spell of both the participants and the non-participants must have started at most two years following training. Similarly, they must have ended at most two years before the beginning of the training spell. The employment spells are delimited by the dark-shadowed boxes in the figure. Note that the employment spells may have started or ended within the training window.

The sample selection scheme imposes rather severe constraints on the data. For example, to measure the impact of U.I. training on the duration of welfare spells, the participants must have experienced a welfare spell prior to training and following training. Non-participants, on the other hand, must have experienced an unemployment spell during the two-year window and welfare during and after the window. The same applies when studying the impact of JRP or welfare training programs on the three outcome variables under investigation. Hence one must keep in mind that the sample changes according to the training program and the outcome variables. There are thus a total of 27 separate samples used in the econometric analysis.[8]

[8] There are three programs, three outcome variables and three demographic groups (women, men, and total).

4.2 Duration Results

Tables 9–11 present estimates of the impact of training programs on the log-duration of employment (upper panel), unemployment (middle panel) and welfare (bottom panel) spells. The tables also presents results for the complete samples, and for men and women separately.

By construction spells that occur prior to training are not right censored. On the other hand a significant number of spells that follow training are right censored. There is no simple way to satisfactorily treat the censoring problem within a regression framework. The exclusion or inclusion of censored observations can both lead to positive or negative biases on the impact of training programs [see Kiefer (1988)]. We thus present three different regressions to investigate the robustness of our results with respect to censoring. In the first regression we simply include a dummy variable that equals 1 if the observation is right censored. In the second we do not control for censoring. Finally, in the third regression we exclude all censored observations. As it turns out, our results are fairly robust to the treatment of the censored observations.

In each panel we also report a "post-treatment" regression. These regressions only use data on durations that occur after the two-year window. To the extent participation is purely exogenous, differentiating post-treatment durations should yield an unbiased estimator of training impact.[9] If participation is not exogenous, and to the extent our first-difference estimator yields an unbiased estimate of the programs' impacts, any discrepancy between them must be attributed to selection into the programs. Finally, each panel reports the number of observations used in the regressions, the number of censored observations and the number of

[9]For the sake of brevity we only report the results in which we include a dummy variable for censored observations. The results are very robust to the treatment of the censoring problem.

program participants.

4.2.1 U.I. Training Programs

Table 9 reports the results on the U.I. training programs. The top panel concerns the impact of the latter on employment duration. The first-difference estimators using all observations are relatively robust to the treatment of censoring. Recall that the parameters represent the change in the hazards between periods t and t'. According to the parameter estimates, the hazards have increased between 1988–1990 and 1992–1993. Thus irrespective of the training programs the duration of employment spells have decreased between the two periods. The impact of training is negative but not statistically significant at conventional levels.

If the analyst focused on post-training data alone, the conclusions would be reversed. Indeed, the impact of training is now positive and statistically significant. In other words, participation in U.I. training schemes increases the exit rate out of employment and thus have a negative impact on the duration of employment spells.

If participation were truly exogenous, the parameter estimates of training should be very similar across specifications. The discrepancy between the first-difference and the post-treatment estimators indicates that there probably is negative selection into the U.I. training programs. It appears the participants have unobserved characteristics that make their stay in employment shorter than average, and that these characteristics are correlated with the probability of participating or being selected into a U.I. program. In terms of our statistical model, equation (8) is violated.

A comparison between the results for men and women reveals interesting patterns. The first-difference estimators show that the changes in the hazards are almost identical for the two groups. Women appear to be very little affected by training programs. The relevant parameters are small in absolute value and not statistically significant. Men, to the contrary, benefit more from the programs. On the other hand, post-treatment estimators show that both men and women are adversely affected by the program and more so for women.

The middle panel of the table looks at the impact of U.I. training on the duration of unemployment spells. The first-difference estimators are very similar across specifications. The parameter estimates of the intercepts indicate that the hazard rates have increased between 1988–1990 and 1992–1993. So on average unemployment spells have decreased over that period. The training programs have a positive and highly statistically significant impact the exit rates. They thus contribute in shortening the average spell length.

The post-treatment estimates are much higher than those obtained from the first-difference estimators and are statistically highly significant. To the extent the first-difference estimators manage to remove individual-specific effects, it can be concluded that there is positive selection into U.I. training schemes. Indeed, is appears that individuals with unobserved characteristics that contribute to short spells are more likely to be selected than otherwise. On the whole, though, it must be concluded that the programs contribute in shortening average spell lengths.

The last panel of the table is concerned with the impact of U.I. training programs on the duration of welfare spells. Very few participants experienced welfare spells before and after the two-year window. Consequently the parameter estimates of the training programs are relatively imprecise. Nevertheless, the first-difference estimates of the intercept depict a nice pattern. Indeed, they indicate that women's hazard rates have decreased between 1988–

1990 and 1992–1993, whereas men witnessed an increase in their hazards rates over the same period. The parameter estimates of the training programs are positive although not statistically significant. Post-treatment parameter estimates of training programs' impacts are positive and statistically significant, and much larger than the first-difference estimators. This result is similar to that obtained with respect to the duration of unemployment spells and is indicative of positive selection into the programs.

4.2.2 Welfare Training Programs

Table 10 presents the results on the welfare training programs. The parameter estimates of the intercept in the top panel indicate that the employment hazard rates have increased between 1988-1990 and 1992–1993. On average, then, the mean duration of employment spells of welfare recipients have decreased over that period. This is true for both men and women. Note that the results are relatively sensitive to the treatment of censored observations. When we do not control for censoring the intercept is negative. In general, though, controlling for censoring or removing censored observations yield very similar results. Welfare training programs have a positive and statistically significant impact on the hazard rates. It also appears that men are more adversely affected than women. This result is somewhat surprising but has also been found in other studies [see, *e.g.* Gritz (1993) for the U.S., Cockx and Ridder (1996) for Belgium and Bonnal, Fougère and Sérandon (1997) for France]. A possible explanation of this result is that participation in a welfare program could be taken by employers as a signal of unsatisfactory performance in previous employment.

The post-treatment estimates are relatively robust across all specifications. The training programs have a positive and statistically significant impact on the hazard rates. Once again the post-treatment estimates of the programs' impacts are larger that the first-difference estimates. It can thus be concluded that there is negative selection into these programs just as was the case with the U.I. programs.

The middle panel reports the results with respect to unemployment spells. The estimates of the intercept show that the average duration has decreased over the 1988–1990 and 1992–1993 period for both men and women. The training programs have a positive impact on the hazard rates and are statistically significant only for women. The post-treatment estimates of the training programs are relatively similar to those obtained from the first-difference estimators. Given there are very few participants that have had unemployment spells before and after the two-year window, the results are not sufficiently precise to make any firm conclusion regarding selection into training.

The last panel reports the results on the duration of welfare spells. The parameter estimates of the intercept suggest that the duration of the welfare spells have decreased considerably over the sample period. The training programs have a significant and negative impact on men's hazards only. The post-treatment estimates of the training programs have a positive impact on the duration of welfare spells. Consequently, it must be concluded that there is positive selection into training.

4.2.3 Job Re-entry Program

Recall that JRP is a training program for welfare recipients. Under this training scheme individuals are usually hired for six-month periods in a subsidised job and receive workplace training. While on training they contribute to the U.I. system and are thus entitled to benefits.

Table 11 presents the results on JRP. The parameter estimates of the intercept are similar to those of Table 10. This is not surprising since the control group is very similar in the two tables. In general, then, the hazard rates of employment, unemployment and welfare spells have all increased between 1988–1990 and 1992–1993. In other words, the duration of these spells have all decreased over that period.

The first-difference estimates of the impact of training on employment duration are relatively robust across specifications. Participation is associated with longer employment spells (lower hazards) for both men and women. Women's employment duration seems to be more sensitive to participation than men's duration. Post-treatment parameter estimates are slightly smaller in absolute value than first-difference estimates. We are thus lead to conclude that there is negative selection into JRP.

Participation in JRP has literally no impact on the duration of unemployment spells as no parameter estimates if found to be statistically significant.

Finally, participation in JRP has a negative (positive) impact on the duration (hazard rates) of women's welfare spells. A comparison of first-difference and post-treatment parameter estimates once again lead us to conclude that there is negative selection into JRP.

4.3 Survival Rates Results

As mentioned in the previous section, the main caveat associated with first differences in log-duration is the inability to properly handle censored data. Although our empirical results are relatively robust to alternative treatments of censored observations, we have also highlighted the benefits of using survival rates as a possible measure of duration.

The price to pay for using survival rates is a considerable lost of degrees of freedom. Indeed, weekly survival rates must be computed with sufficiently numerous observations to be meaningful. As a consequence, it is not possible to analyse men and women separately.

Figures 3–11 plot the survival rates for each program and each state. The top panel in each figure plots the differences in the survival rates before and after the training window for participants and non-participants separately. The bottom panel plots the differences in the survival rates of participants and non-participants after the training window. The regression results reported in Table 12 summarise these figures. The left-hand side portion of the table concerns first-difference estimators while the right-hand side portion concerns post-treatment estimators.

Not surprisingly the parameter estimates are consistent with the findings of the previous section. The top panel of Table 12 shows that the hazard rates of employment, unemployment and welfare spells have increased between 1988–1990 and 1992–1993. The U.I. training programs have a negative impact on exits out of employment and a positive one on exits out unemployment and welfare. A comparison of post-treatment and first-difference estimates yield similar results as those of the previous section except for unemployment. Indeed, it is found that the post-treatment estimate of the training program is smaller in absolute value. One would thus be lead to conclude that there is negative selection into training. Recall that the heterogeneity must be regarded as group-wise rather than individual-specific. This is a much stronger assumption that is less likely to hold in the data.

The middle panel of Table 12 reports results for welfare training programs. Training is found to have a positive impact on the hazard rates of employment and welfare spells.[10] Post-

[10]We do not report results on unemployment spells since there were too few observations to compute mean-

treatment estimates are larger for employment and smaller for welfare spells. Thus positive selection is found for employment but negative selection seems to hold for welfare, contrary to what was found previously.

Finally, the bottom panel reports results on JRP. The first-difference parameter estimates are all statistically significant. It is found that JRP has a negative impact on exits out of employment and unemployment, but a positive and large impact on on exits out of welfare. None of the post-treatment parameter estimates is statistically significant.

5 Conclusion

In this paper we have investigated the impact of federal and provincial training programs aimed at welfare and unemployment insurance recipients using a unique dataset that contains very detailed informations on transitions between seven different states on the labour market. We have shown how traditional first-difference estimators can be used in the context of duration data.

Our results show that all training programs have substantial impacts on the durations of either employment, unemployment and/or welfare spells. In particular, it was found that participation in a U.I. training program translated into longer employment spells for men, and shorter unemployment spells for both men and women. Participation in welfare training programs, on the other hand, does not benefit men. Indeed, it was found that participants had longer welfare spells and shorter employment spells. Women's participation was found to shorten the duration of unemployment spells. Finally, the Job Re-entry program was found

ingful survival rates.

to benefit both men and women since it was associated with longer employment spells and shorter welfare spells.

The empirical analysis also indicated that there is substantial selectivity in program participation. Consequently, simple post-training comparisons between participants and non-participants will yield biased estimators of the true programs' impacts.

The data at our disposal indicate that the individuals in our sample experience many transitions on the labour market within a very short period. Unfortunately, the estimators we have used in this paper do not fully take advantage of all the information that is currently available in the dataset. A more efficient empirical strategy would model all the individual transitions on the labour market and would treat the training programs as separate states. Similar work has been conducted by many [see Bonnal et al. (1997), Gritz (1993), Ham and LaLonde (1996), *etc.*] using either smaller datasets, less states or a combination of the two. These papers convincingly show that such modelling generates very plausible estimates of the programs' impacts and allow to perform interesting policy simulations. Another benefit of more in-depth modelling is to allow time-varying covariates to affect the transitions between states, something that can not be easily done using our empirical strategy.

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TABLE 1
SAMPLE DESCRIPTIVE STATISTICS

	Men	Women
Age at first welfare spell		
Less than 24	21.06	22.17
24–34	26.75	27.86
34–44	37.40	37.69
44+	14.80	12.29
Birth Place		
Indian	1.03	0.83
Quebec	75.57	80.69
Canada	2.81	2.88
Outside Canada	15.76	11.37
Refugee	4.83	4.23
Region of residence at first welfare spell		
Gaspésie	2.45	2.41
Bas St-Laurent	2.85	2.78
Saguenay–Lac St-Jean	4.85	4.60
Québec - Appalaches	10.90	11.35
Mauricie–Bois francs	6.97	6.57
Estrie	3.96	4.00
Ville de Montréal	22.86	26.99
Montérégie ouest	13.37	12.32
Montréal métro–Laval	12.76	11.48
Laurentides–Lanaudière	9.51	8.57
Outaouais	4.41	4.21
Abitibi-Témiscamingue	2.54	2.72
Côte-Nord	2.07	1.71
Nouveau Québec	0.51	0.29
Years of schooling		
1–4	0.064	0.064
5–8	0.188	0.192
9–11	0.436	0.445
12	0.146	0.131
13–17	0.148	0.142
18+	0.018	0.026
No. observations	25 468	28 856

TABLE 2

TRANSITION MATRIX - TOTAL SAMPLE

Destination / Origin	Welfare	Training Welfare	JRP	Unemp.	Training UI	Employ.	Inac.	TOTAL
Welfare	5	25241	1852	1530	23	28058	29142	85851
Training Welfare	5184	5	645	119	1	3601	4376	13931
JRP	241	79	0	85	0	2126	482	3013
Unemployment	8383	935	38	6514	2115	26137	26474	70596
Training UI	18	6	0	2504	1	254	80	2863
Employment	12042	1807	319	53945	788	23116	46752	138769
Inactive	58429	3479	159	11379	177	63765	0	137388
TOTAL	84302	31552	3013	76076	3105	147057	107306	

TRANSITION MATRIX - TOTAL SAMPLE
(ROW PERCENTAGE)

Destination / Origin	Welfare	Training Welfare	JRP	Unemp.	Training UI	Employ.	Inac.
Welfare	0.0	29.4	2.2	1.8	0.0	32.7	33.9
Training Welfare	37.2	0.0	4.6	0.9	0.0	25.8	31.4
JRP	8.0	2.6	0.0	2.8	0.0	70.6	16.0
Unemployment	11.9	1.3	0.1	9.2	3.0	37.0	37.5
Training UI	0.6	0.2	0.0	87.5	0.0	8.9	2.8
Employment	8.7	1.3	0.2	38.9	0.6	16.7	33.7
Inactive	42.5	2.5	0.1	8.3	0.1	46.4	0.0

TRANSITION MATRIX - TOTAL SAMPLE
(COLUMN PERCENTAGE)

Origin / Destination	Welfare	Training Welfare	JRP	Unemp.	Training UI	Employ.	Inac.
Welfare	0.0	80.0	61.5	2.0	0.7	19.1	27.2
Training Welfare	6.1	0.0	21.4	0.2	0.0	2.4	4.1
JRP	0.3	0.3	0.0	0.1	0.0	1.4	0.4
Unemployment	9.9	3.0	1.3	8.6	68.1	17.8	24.7
Training UI	0.0	0.0	0.0	3.3	0.0	0.2	0.1
Employment	14.3	5.7	10.6	70.9	25.4	15.7	43.6
Inactive	69.3	11.0	5.3	15.0	5.7	43.4	0.0

TABLE 3

TRANSITION MATRIX - WOMEN

Destination / Origin	Welfare	Training Welfare	JRP	Unemp.	Training UI	Employ.	Inac.	TOTAL
Welfare	2	12171	1114	943	16	18079	14571	46896
Training Welfare	2276	4	421	60	0	2382	2250	7393
JRP	148	36	0	54	0	1353	279	1870
Unemployment	4769	459	16	4373	1225	17061	16336	44239
Training UI	12	2	0	1483	0	152	42	1691
Employment	7733	1138	213	33937	478	15398	28847	87744
Inactive	31753	1913	106	6867	109	38138	0	78886
TOTAL	46693	15723	1870	47717	1828	92563	62325	

TRANSITION MATRIX - WOMEN
(ROW PERCENTAGE)

Destination / Origin	Welfare	Training Welfare	JRP	Unemp.	Training UI	Employ.	Inac.
Welfare	0.0	26.0	2.4	2.0	0.0	38.6	31.1
Training Welfare	30.8	0.1	5.7	0.8	0.0	32.2	30.4
JRP	7.9	1.9	0.0	2.9	0.0	72.4	14.9
Unemployment	10.8	1.0	0.0	9.9	2.8	38.6	36.9
Training UI	0.7	0.1	0.0	87.7	0.0	9.0	2.5
Employment	8.8	1.3	0.2	38.7	0.5	17.5	32.9
Inactive	40.3	2.4	0.1	8.7	0.1	48.3	

TRANSITION MATRIX - WOMEN
(COLUMN PERCENTAGE)

Origin / Destination	Welfare	Training Welfare	JRP	Unemp.	Training UI	Employ.	Inac.
Welfare	0.0	77.4	59.6	2.0	0.9	19.5	23.4
Training Welfare	4.9	0.0	22.5	0.1	0.0	2.6	3.6
JRP	0.3	0.2	0.0	0.1	0.0	1.5	0.4
Unemployment	10.2	2.9	0.9	9.2	67.0	18.4	26.2
Training UI	0.0	0.0	0.0	3.1	0.0	0.2	0.1
Employment	16.6	7.2	11.4	71.1	26.1	16.6	46.3
Inactive	68.0	12.2	5.7	14.4	6.0	41.2	

TABLE 4

TRANSITION MATRIX - MEN

Destination / Origin	Welfare	Training Welfare	JRP	Unemp.	Training UI	Employ.	Inac.	TOTAL
Welfare	3	13070	738	587	7	9979	14571	38955
Training Welfare	2908	1	224	59	1	1219	2126	6538
JRP	93	43	0	31	0	773	203	1143
Unemployment	3614	476	22	2141	890	9076	10138	26357
Training UI	6	4	0	1021	1	102	38	1172
Employment	4309	669	106	20008	310	7718	17905	51025
Inactive	26676	1566	53	4512	68	25627	0	58502
TOTAL	37609	15829	1143	28359	1277	54494	44981	

TRANSITION MATRIX - MEN

(ROW PERCENTAGE)

Destination / Origin	Welfare	Training Welfare	JRP	Unemp.	Training UI	Employ.	Inac.
Welfare	0.0	33.6	1.9	1.5	0.0	25.6	37.4
Training Welfare	44.5	0.0	3.4	0.9	0.0	18.6	32.5
JRP	8.1	3.8	0.0	2.7	0.0	67.6	17.8
Unemployment	13.7	1.8	0.1	8.1	3.4	34.4	38.5
Training UI	0.5	0.3	0.0	87.1	0.1	8.7	3.2
Employment	8.4	1.3	0.2	39.2	0.6	15.1	35.1
Inactive	45.6	2.7	0.1	7.7	0.1	43.8	

TRANSITION MATRIX - MEN

(COLUMN PERCENTAGE)

Origin / Destination	Welfare	Training Welfare	JRP	Unemp.	Training UI	Employ.	Inac.
Welfare	0.0	82.6	64.6	2.1	0.5	18.3	32.4
Training Welfare	7.7	0.0	19.6	0.2	0.1	2.2	4.7
JRP	0.2	0.3	0.0	0.1	0.0	1.4	0.5
Unemployment	9.6	3.0	1.9	7.5	69.7	16.7	22.5
Training UI	0.0	0.0	0.0	3.6	0.1	0.2	0.1
Employment	11.5	4.2	9.3	70.6	24.3	14.2	39.8
Inactive	70.9	9.9	4.6	15.9	5.3	47.0	

TABLE 5

TRANSITION MATRIX - INDIVIDUALS ≤ 30 YEARS

Destination / Origin	Welfare	Training Welfare	JRP	Unemp.	Training UI	Employ.	Inac.	TOTAL
Welfare	2	13683	941	823	13	15393	13312	44167
Training Welfare	3268	3	356	74	1	2390	2381	8473
JRP	121	43	0	48	0	1132	258	1602
Unemployment	4264	503	16	3061	1247	13984	13803	36878
Training UI	11	2	0	1467	0	165	53	1698
Employment	7134	1221	191	28524	506	14589	31388	83553
Inactive	29451	1890	98	5826	93	40856	0	78214
TOTAL	44251	17345	1602	39823	1860	88509	61195	

TRANSITION MATRIX - INDIVIDUALS ≤ 30 YEARS
(ROW PERCENTAGE)

Destination / Origin	Welfare	Training Welfare	JRP	Unemp.	Training UI	Employ.	Inac.
Welfare	0.0	31.0	2.1	1.9	0.0	34.9	30.1
Training Welfare	38.6	0.0	4.2	0.9	0.0	28.2	28.1
JRP	7.6	2.7	0.0	3.0	0.0	70.7	16.1
Unemployment	11.6	1.4	0.0	8.3	3.4	37.9	37.4
Training UI	0.6	0.1	0.0	86.4	0.0	9.7	3.1
Employment	8.5	1.5	0.2	34.1	0.6	17.5	37.6
Inactive	37.7	2.4	0.1	7.4	0.1	52.2	

TRANSITION MATRIX - INDIVIDUALS ≤ 30 YEARS
(COLUMN PERCENTAGE)

Origin / Destination	Welfare	Training Welfare	JRP	Unemp.	Training UI	Employ.	Inac.
Welfare	0.0	78.9	58.7	2.1	0.7	17.4	21.8
Training Welfare	7.4	0.0	22.2	0.2	0.1	2.7	3.9
JRP	0.3	0.2	0.0	0.1	0.0	1.3	0.4
Unemployment	9.6	2.9	1.0	7.7	67.0	15.8	22.6
Training UI	0.0	0.0	0.0	3.7	0.0	0.2	0.1
Employment	16.1	7.0	11.9	71.6	27.2	16.5	51.3
Inactive	66.6	10.9	6.1	14.6	5.0	46.2	

TABLE 6

TRANSITION MATRIX - INDIVIDUALS > 30 YEARS

Destination / Origin	Welfare	Training Welfare	JRP	Unemp.	Training UI	Employ.	Inac.	TOTAL
Welfare	3	11558	911	707	10	12665	15830	41684
Training Welfare	1916	2	289	45	0	1211	1995	5458
JRP	120	36	0	37	0	994	224	1411
Unemployment	4119	432	22	3453	868	12153	12671	33718
Training UI	7	4	0	1037	1	89	27	1165
Employment	4908	586	128	25421	282	8527	15364	55216
Inactive	28978	1589	61	5553	84	22909	0	59174
TOTAL	40051	14207	1411	36253	1245	58548	46111	

TRANSITION MATRIX - INDIVIDUALS > 30 YEARS
(ROW PERCENTAGE)

Destination / Origin	Welfare	Training Welfare	JRP	Unemp.	Training UI	Employ.	Inac.
Welfare	0.0	27.7	2.2	1.7	0.0	30.4	38.0
Training Welfare	35.1	0.0	5.3	0.8	0.0	22.2	36.6
JRP	8.5	2.6	0.0	2.6	0.0	70.4	15.9
Unemployment	12.2	1.3	0.1	10.2	2.6	36.0	37.6
Training UI	0.6	0.3	0.0	89.0	0.1	7.6	2.3
Employment	8.9	1.1	0.2	46.0	0.5	15.4	27.8
Inactive	49.0	2.7	0.1	9.4	0.1	38.7	

TRANSITION MATRIX - INDIVIDUALS > 30 YEARS
(COLUMN PERCENTAGE)

Origin / Destination	Welfare	Training Welfare	JRP	Unemp.	Training UI	Employ.	Inac.
Welfare	0.0	81.4	64.6	2.0	0.8	21.6	34.3
Training Welfare	4.8	0.0	20.5	0.1	0.0	2.1	4.3
JRP	0.3	0.3	0.0	0.1	0.0	1.7	0.5
Unemployment	10.3	3.0	1.6	9.5	69.7	20.8	27.5
Training UI	0.0	0.0	0.0	2.9	0.1	0.2	0.1
Employment	12.3	4.1	9.1	70.1	22.7	14.6	33.3
Inactive	72.4	11.2	4.3	15.3	6.7	39.1	

TABLE 7

TRANSITION MATRIX - LESS THAN GRADE 12

Destination / Origin	Welfare	Training Welfare	JRP	Unemp.	Training UI	Employ.	Inac.	TOTAL
Welfare	2	11966	465	486	6	7357	7662	27944
Training Welfare	2296	2	135	34	0	1044	903	4414
JRP	91	19	0	21	0	538	81	750
Unemployment	2854	369	7	1459	270	5475	6125	16559
Training UI	3	2	0	334	1	25	9	374
Employment	3783	723	105	12248	96	5101	10706	32762
Inactive	17564	1486	38	2704	25	14477	0	36294
TOTAL	26593	14567	750	17286	398	34017	25486	

TRANSITION MATRIX - LESS THAN GRADE 12
(ROW PERCENTAGE)

Destination / Origin	Welfare	Training Welfare	JRP	Unemp.	Training UI	Employ.	Inac.
Welfare	0.0	42.8	1.7	1.7	0.0	26.3	27.4
Training Welfare	52.0	0.0	3.1	0.8	0.0	23.7	20.5
JRP	12.1	2.5	0.0	2.8	0.0	71.7	10.8
Unemployment	17.2	2.2	0.0	8.8	1.6	33.1	37.0
Training UI	0.8	0.5	0.0	89.3	0.3	6.7	2.4
Employment	11.5	2.2	0.3	37.4	0.3	15.6	32.7
Inactive	48.4	4.1	0.1	7.5	0.1	39.9	

TRANSITION MATRIX - LESS THAN GRADE 12
(COLUMN PERCENTAGE)

Origin / Destination	Welfare	Training Welfare	JRP	Unemp.	Training UI	Employ.	Inac.
Welfare	0.0	82.1	62.0	2.8	1.5	21.6	30.1
Training Welfare	8.6	0.0	18.0	0.2	0.0	3.1	3.5
JRP	0.3	0.1	0.0	0.1	0.0	1.6	0.3
Unemployment	10.7	2.5	0.9	8.4	67.8	16.1	24.0
Training UI	0.0	0.0	0.0	1.9	0.3	0.1	0.0
Employment	14.2	5.0	14.0	70.9	24.1	15.0	42.0
Inactive	66.0	10.2	5.1	15.6	6.3	42.6	

TABLE 8

TRANSITION MATRIX - GRADE 12 AND ABOVE

Destination / Origin	Welfare	Training Welfare	JRP	Unemp.	Training UI	Employ.	Inac.	TOTAL
Welfare	1	8622	467	342	8	6015	5100	20555
Training Welfare	1867	2	123	41	1	866	634	3534
JRP	69	29	0	18	0	530	71	717
Unemployment	2364	392	10	1157	505	4677	5643	14748
Training UI	2	4	0	640	0	36	14	696
Employment	2798	640	70	10750	170	4234	9031	27693
Inactive	13268	1313	47	2404	39	12492	0	29563
TOTAL	20369	11002	717	15352	723	28850	20493	

TRANSITION MATRIX - GRADE 12 AND ABOVE
(ROW PERCENTAGE)

Destination / Origin	Welfare	Training Welfare	JRP	Unemp.	Training UI	Employ.	Inac.
Welfare	0.0	41.9	2.3	1.7	0.0	29.3	24.8
Training Welfare	52.8	0.1	3.5	1.2	0.0	24.5	17.9
JRP	9.6	4.0	0.0	2.5	0.0	73.9	9.9
Unemployment	16.0	2.7	0.1	7.8	3.4	31.7	38.3
Training UI	0.3	0.6	0.0	92.0	0.0	5.2	2.0
Employment	10.1	2.3	0.3	38.8	0.6	15.3	32.6
Inactive	44.9	4.4	0.2	8.1	0.1	42.3	

TRANSITION MATRIX - GRADE 12 AND ABOVE
(COLUMN PERCENTAGE)

Origin / Destination	Welfare	Training Welfare	JRP	Unemp.	Training UI	Employ.	Inac.
Welfare	0.0	78.4	65.1	2.2	1.1	20.8	24.9
Training Welfare	9.2	0.0	17.2	0.3	0.1	3.0	3.1
JRP	0.3	0.3	0.0	0.1	0.0	1.8	0.3
Unemployment	11.6	3.6	1.4	7.5	69.8	16.2	27.5
Training UI	0.0	0.0	0.0	4.2	0.0	0.1	0.1
Employment	13.7	5.8	9.8	70.0	23.5	14.7	44.1
Inactive	65.1	11.9	6.6	15.7	5.4	43.3	

TABLE 9
THE IMPACT OF UI TRAINING
LOG(DURATION)

	All Observations				Women				Men			
	First-Difference			Post-Treatment	First-Difference			Post-Treatment	First-Difference			Post-Treatment
	Total Sample	Without Censoring	Without Censored Observations		Total Sample	Without Censoring	Without Censored Observations		Total Sample	Without Censoring	Without Censored Observations	
Employment Duration												
Intercept	0.561	0.445	0.559	-2.688	0.547	0.441	0.547	-2.607	0.583	0.453	0.580	-2.829
	(0.013)	(0.013)	(0.013)	(0.010)	(0.016)	(0.016)	(0.016)	(0.012)	(0.021)	(0.021)	(0.022)	(0.016)
Training	-0.058	-0.093	-0.041	0.091	-0.015	-0.028	-0.016	0.117	-0.132	-0.203	-0.086	0.042
	(0.047)	(0.049)	(0.050)	(0.035)	(0.060)	(0.061)	(0.062)	(0.043)	(0.078)	(0.081)	(0.085)	(0.058)
Censored	-1.277			-1.536	-1.280			-1.584	-1.276			-1.437
	(0.041)			(0.030)	(0.054)			(0.039)	(0.064)			(0.048)
No. Observation	13 410	13 410	12 173	13 410	8 448	8 448	7 738	8 848	4 962	4 962	4 435	4 962
No. Censored	1 237	1 237	0	1 237	710	710	0	710	527	527	0	527
No. Participants	918	918	816	918	575	575	527	575	343	343	289	343
Unemployment Duration												
Intercept	0.100	0.126	0.099	-3.455	0.083	0.107	0.082	-3.488	0.137	0.168	0.138	-3.381
	(0.013)	(0.013)	(0.013)	(0.010)	(0.015)	(0.014)	(0.015)	(0.011)	(0.025)	(0.024)	(0.025)	(0.019)
Training	0.390	0.367	0.393	0.984	0.396	0.375	0.402	0.982	0.371	0.343	0.368	0.967
	(0.032)	(0.032)	(0.032)	(0.025)	(0.039)	(0.039)	(0.039)	(0.030)	(0.058)	(0.057)	(0.058)	(0.044)
Censured	0.332			0.318	0.318			0.319	0.355			0.306
	(0.046)			(0.035)	(0.054)			(0.041)	(0.086)			(0.065)
No. Observation	7 987	7 987	7 438	7 987	5 398	5 398	5 035	5 398	2 589	2 589	2 403	2 589
No. Censored	549	549	0	549	363	363	0	363	186	186	0	186
No. Participants	1 207	1 207	1 194	1 207	740	740	730	740	467	467	464	467
Welfare Duration												
Intercept	-0.008	0.001	-0.008	-3.263	-0.129	-0.113	-0.129	-3.238	0.197	0.200	0.198	-3.305
	(0.025)	(0.025)	(0.025)	(0.016)	(0.032)	(0.031)	(0.032)	(0.020)	(0.042)	(0.041)	(0.042)	(0.027)
Training	0.097	0.094	0.098	0.290	0.117	0.112	0.125	0.284	0.044	0.042	0.031	0.305
	(0.105)	(0.105)	(0.107)	(0.067)	(0.134)	(0.134)	(0.136)	(0.085)	(0.169)	(0.169)	(0.170)	(0.109)
Censured	0.269			-0.092	0.365			-0.195	0.132			0.162
	(0.130)			(0.083)	(0.151)			(0.095)	(0.251)			(0.163)
No. Observation	5 178	5 178	4 992	5 178	3 274	3 274	3 138	3 274	1 904	1 904	1 854	1 904
No. Censored	186	186	0	186	136	136	0	136	50	50	0	50
No. Participants	290	290	280	290	174	174	170	174	116	116	114	116

TABLE 10
THE IMPACT OF WELFARE TRAINING
LOG(DURATION)

	All Observations				Women				Men			
	First-Difference			Post-Treatment	First-Difference			Post-Treatment	First-Difference			Post-Treatment
	Total Sample	Without Censoring	Without Censored Observations		Total Sample	Without Censoring	Without Censored Observations		Total Sample	Without Censoring	Without Censored Observations	
Employment Duration												
Intercept	0.059	-0.052	0.060	-2.715	0.069	-0.037	0.068	-2.644	0.040	-0.082	0.044	-2.861
	(0.015)	(0.014)	(0.014)	(0.010)	(0.018)	(0.017)	(0.017)	(0.012)	(0.026)	(0.025)	(0.026)	(0.018)
Training	0.303	0.196	0.269	0.456	0.152	0.038	0.198	0.390	0.499	0.411	0.370	0.582
	(0.080)	(0.082)	(0.089)	(0.054)	(0.104)	(0.107)	(0.115)	(0.071)	(0.125)	(0.128)	(0.141)	(0.084)
Censoring	-1.010			-1.174	-1.086			-1.236	-0.898			-1.042
	(0.043)			(0.029)	(0.055)			(0.037)	(0.070)			(0.047)
No. Observation	10 506	10 506	9 315	10 506	6 954	6 954	6 257	6 954	3 552	3 552	3 058	3 552
No. Censored	1 191	1 191	0	1 191	697	697	0	697	494	494	0	494
No. Participants	320	320	248	320	181	181	144	181	139	139	104	139
Unemployment Duration												
Intercept	0.164	0.255	0.166	-3.496	0.149	0.252	0.150	-3.525	0.200	0.262	0.201	-3.425
	(0.014)	(0.013)	(0.013)	(0.011)	(0.016)	(0.015)	(0.015)	(0.013)	(0.028)	(0.026)	(0.027)	(0.022)
Training	0.245	0.339	0.116	0.174	0.386	0.513	0.225	0.285	0.036	0.081	-0.047	-0.001
	(0.111)	(0.113)	(0.128)	(0.090)	(0.138)	(0.143)	(0.160)	(0.112)	(0.187)	(0.189)	(0.219)	(0.150)
Censured	0.581			0.569	0.672			0.669	0.379			0.344
	(0.035)			(0.028)	(0.041)			(0.033)	(0.067)			(0.054)
No. Observation	5 060	5 060	4 256	5 060	3 538	3 538	2 988	3 538	1 522	1 522	1 268	1 522
No. Censored	804	804	0	804	550	550	0	550	254	254	0	254
No. Participants	71	71	48	71	43	43	27	43	28	28	21	28
Welfare Duration												
Intercept	0.579	0.596	0.578	-3.048	0.463	0.478	0.462	-3.021	0.764	0.786	0.762	-3.091
	(0.029)	(0.028)	(0.029)	(0.020)	(0.037)	(0.036)	(0.038)	(0.026)	(0.045)	(0.044)	(0.045)	(0.030)
Training	-0.016	-0.024	-0.010	0.234	0.028	0.022	0.033	0.284	-0.143	-0.154	-0.136	0.213
	(0.061)	(0.061)	(0.062)	(0.042)	(0.089)	(0.089)	(0.091)	(0.061)	(0.084)	(0.083)	(0.085)	(0.057)
Censured	0.276			0.064	0.231			0.052	0.396			0.072
	(0.110)			(0.075)	(0.140)			(0.096)	(0.179)			(0.121)
No. Observation	4 702	4 702	4 450	4 702	2 746	2 746	2 581	2 746	1 956	1 956	1 869	1 956
No. Censored	255	255	0	255	165	165	0	165	87	87	0	87
No. Participants	1 001	1 001	968	1 001	456	456	437	456	545	545	531	545

TABLE 11

THE IMPACT OF JRP

LOG(DURATION)

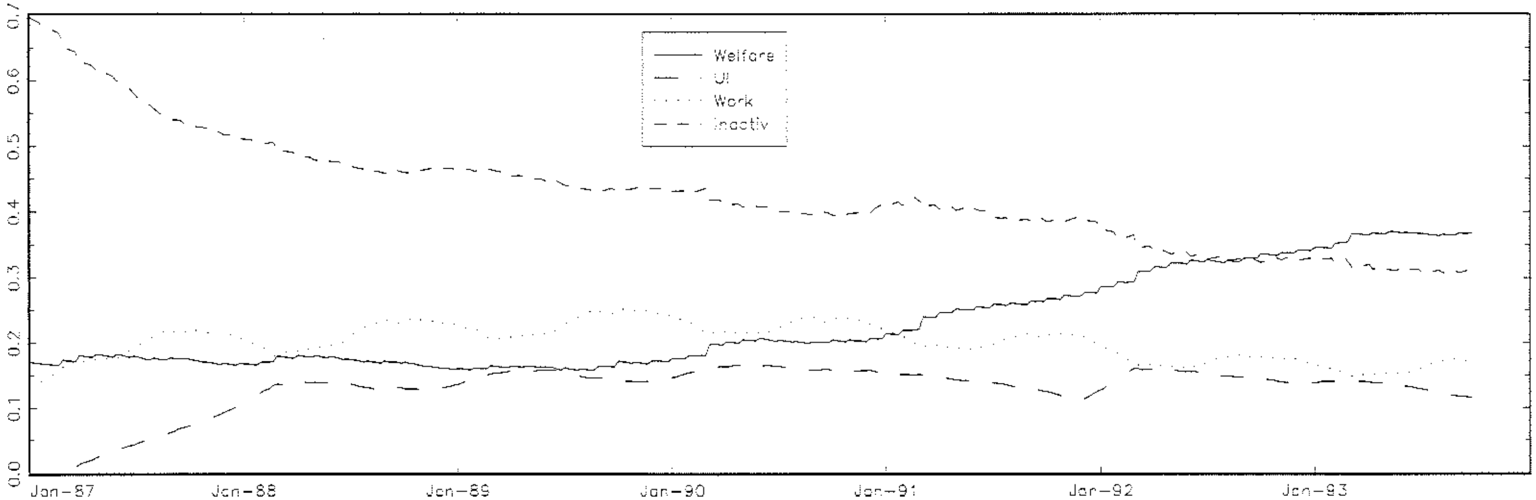
	All Observations				Women				Men			
	First-Difference			Post-Treatment	First-Difference			Post-Treatment	First-Difference			Post-Treatment
	Total Sample	Without Censoring	Without Censored Observations		Total Sample	Without Censoring	Without Censored Observations		Total Sample	Without Censoring	Without Censored Observations	
Employment Duration												
Intercept	0.022	-0.092	0.022	-2.757	0.031	-0.074	0.032	-2.682	0.004	-0.128	0.001	-2.909
	(0.014)	(0.014)	(0.014)	(0.010)	(0.017)	(0.017)	(0.017)	(0.012)	(0.026)	(0.025)	(0.026)	(0.017)
Training	-0.360	-0.303	-0.350	-0.251	-0.392	-0.349	-0.398	-0.274	-0.300	-0.219	-0.263	-0.189
	(0.053)	(0.055)	(0.054)	(0.035)	(0.065)	(0.067)	(0.066)	(0.044)	(0.091)	(0.094)	(0.092)	(0.059)
Censored	-1.396			-1.587	-1.433			-1.647	-1.335			-1.455
	(0.049)			(0.033)	(0.062)			(0.042)	(0.081)			(0.053)
No. Observation	10 679	10 679	9 836	10 679	7 089	7 089	6 584	7 089	3 590	3 590	3 252	3 590
No. Censored	843	843	0	843	505	505	0	505	338	338	0	338
No. Participants	724	724	696	724	461	461	442	461	263	263	254	263
Unemployment Duration												
Intercept	0.168	0.206	0.167	-3.502	0.155	0.201	0.156	-3.528	0.198	0.218	0.195	-3.440
	(0.013)	(0.012)	(0.013)	(0.010)	(0.015)	(0.014)	(0.014)	(0.012)	(0.026)	(0.024)	(0.026)	(0.020)
Training	0.007	-0.007	0.017	-0.056	0.062	0.044	0.054	-0.008	-0.097	-0.103	-0.060	-0.154
	(0.045)	(0.045)	(0.046)	(0.036)	(0.053)	(0.054)	(0.053)	(0.043)	(0.083)	(0.083)	(0.088)	(0.066)
Censored	0.290			0.337	0.367			0.414	0.137			0.179
	(0.035)			(0.028)	(0.041)			(0.033)	(0.066)			(0.052)
No. Observation	5 692	5 692	4 968	5 692	3 950	3 950	3 472	3 950	1 742	1 742	1 496	1 742
No. Censored	724	724	0	724	478	478	0	478	246	246	0	246
No. Participants	414	414	381	414	269	269	250	269	145	145	131	145
Welfare Duration												
Intercept	0.537	0.536	0.537	-3.090	0.435	0.433	0.432	-3.054	0.720	0.722	0.724	-3.153
	(0.031)	(0.030)	(0.031)	(0.021)	(0.038)	(0.038)	(0.039)	(0.027)	(0.050)	(0.049)	(0.050)	(0.035)
Training	0.157	0.157	0.156	-0.032	0.206	0.205	0.220	-0.090	0.063	0.066	0.040	0.073
	(0.072)	(0.072)	(0.074)	(0.051)	(0.091)	(0.091)	(0.094)	(0.064)	(0.118)	(0.117)	(0.122)	(0.082)
Censored	-0.016			-0.203	-0.044			-0.172	0.073			-0.284
	(0.139)			(0.097)	(0.171)			(0.121)	(0.236)			(0.165)
No. Observation	4 070	4 070	3 905	4 070	2 605	2 605	2 495	2 605	1 465	1 465	1 410	1 465
No. Censored	165	165	0	165	110	110	0	110	55	55	0	55
No. Participants	705	705	665	705	446	446	422	446	259	259	243	259

TABLE 12
THE IMPACT OF TRAINING PROGRAMS
DIFFERENCES IN $\ln(-\ln[S(t)])$

	Differences-in-Differences						Post-Treatment					
UI Training												
	Employment		Unemployment		Welfare		Employment		Unemployment		Welfare	
Intercept	1.503	(0.020)	0.220	(0.061)	0.019	(0.036)	0.105	(0.098)	-1.557	(0.168)	-0.487	(0.125)
Training	-0.959	(0.028)	1.954	(0.086)	0.129	(0.051)	0.035	(0.139)	1.558	(0.238)	0.477	(0.177)
No. Obs.	172		100		148		172		100		148	
Welfare Training												
	Employment		Unemployment		Welfare		Employment		Unemployment		Welfare	
Intercept	-0.060	(0.094)	–		0.435	(0.108)	-0.972	(0.237)	–		-1.342	(0.253)
Training	0.459	(0.134)	–		2.023	(0.153)	0.754	(0.336)	–		0.069	(0.357)
No. Obs.	64		–		44		64		–		44	
JRP												
	Employment		Unemployment		Welfare		Employment		Unemployment		Welfare	
Intercept	0.029	(0.051)	0.568	(0.044)	0.415	(0.015)	-0.243	(0.176)	-2.097	(0.171)	-0.987	(0.141)
Training	-0.189	(0.072)	-0.296	(0.063)	1.679	(0.021)	0.015	(0.249)	0.002	(0.242)	-0.066	(0.199)
No. Obs.	126		80		68		126		80		68	

Figure 1

Distribution Across States: Complete Sample



Distribution Across Training Programs: Complete Sample

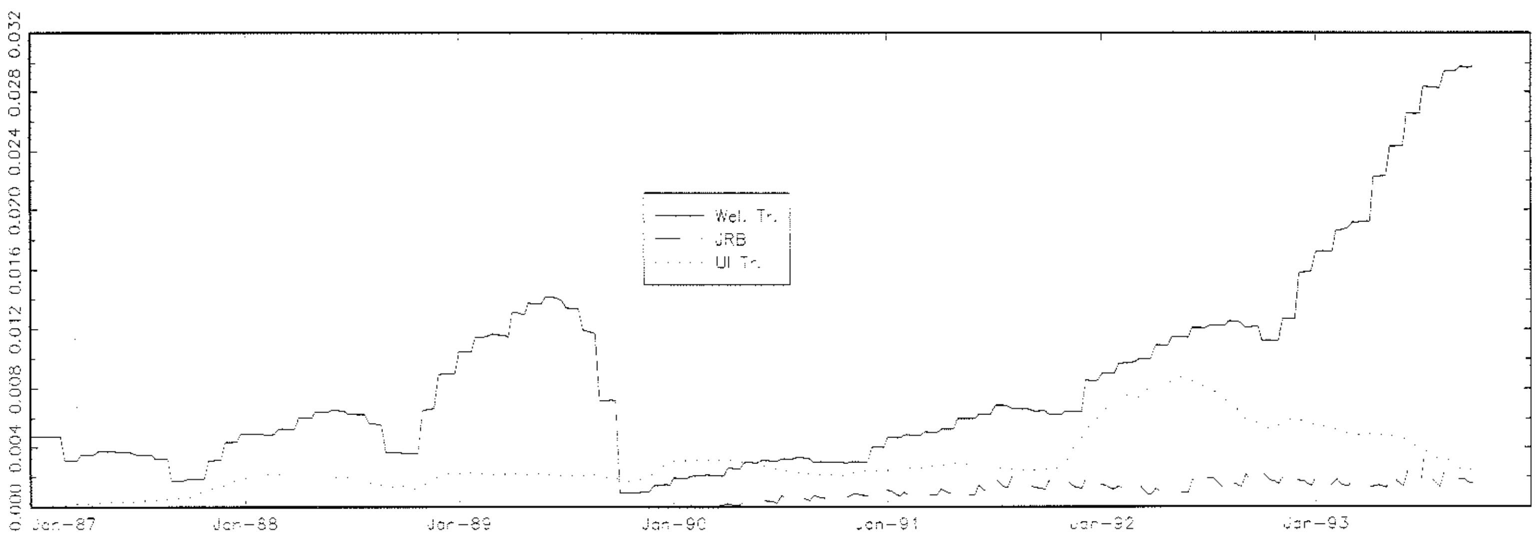


Figure 2
Sample Selection Scheme
Example: The Impact of UI Training on Employment Duration

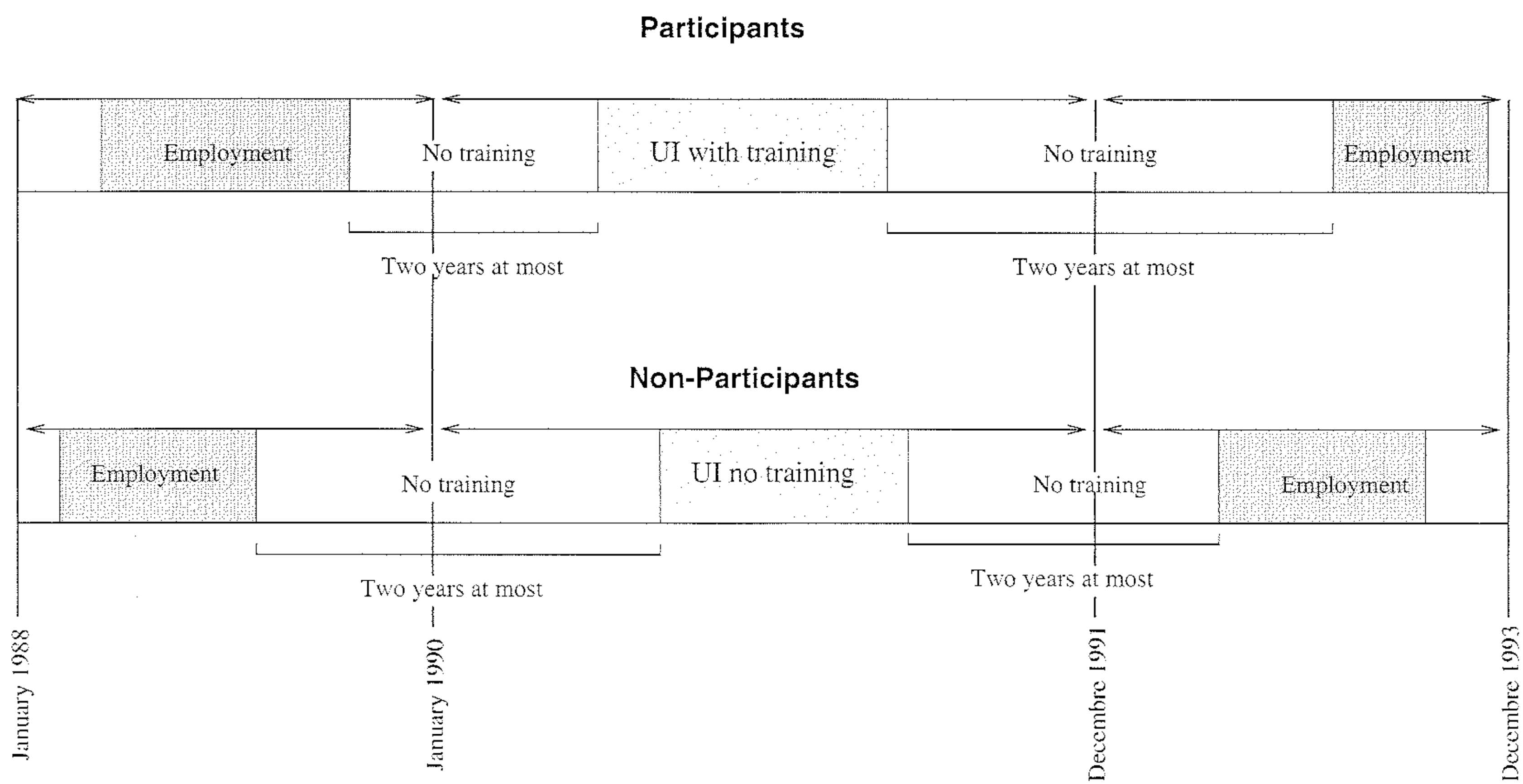


Figure 3

Impact of UI Training on Employment Duration

$S(\tau)^t - S(\tau^{t'})$

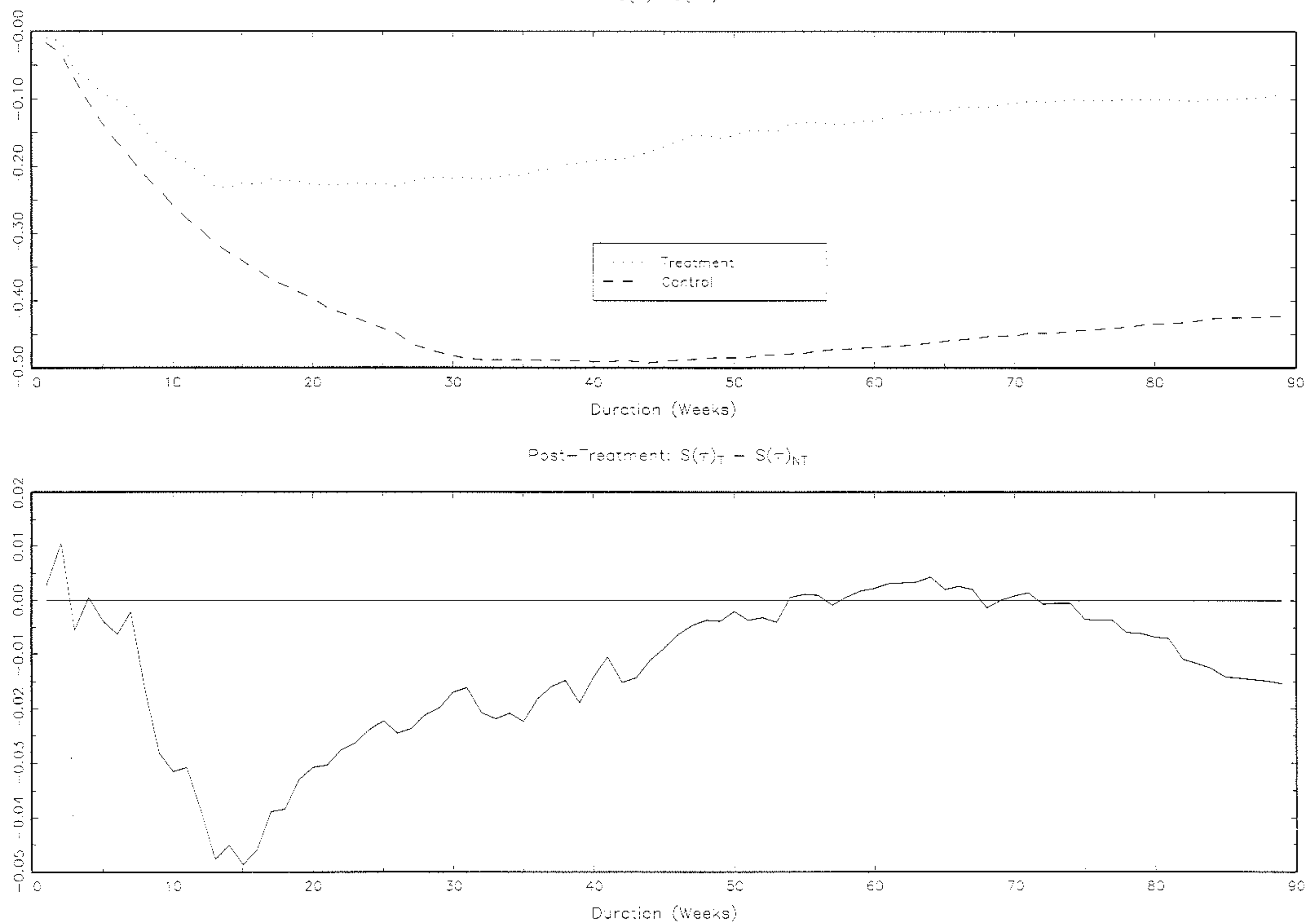
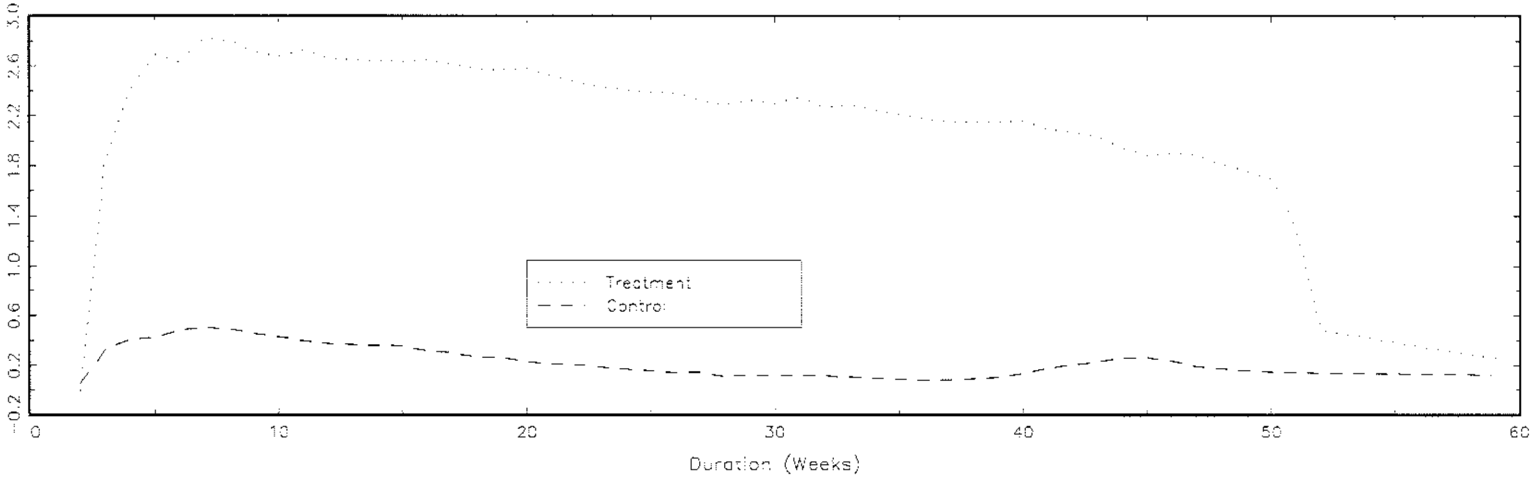


Figure 4
Impact of UI Training on Unemployment Duration
$S(\tau)^t - S(\tau^t)$



Post-Treatment: $S(\tau)_T - S(\tau)_{NT}$

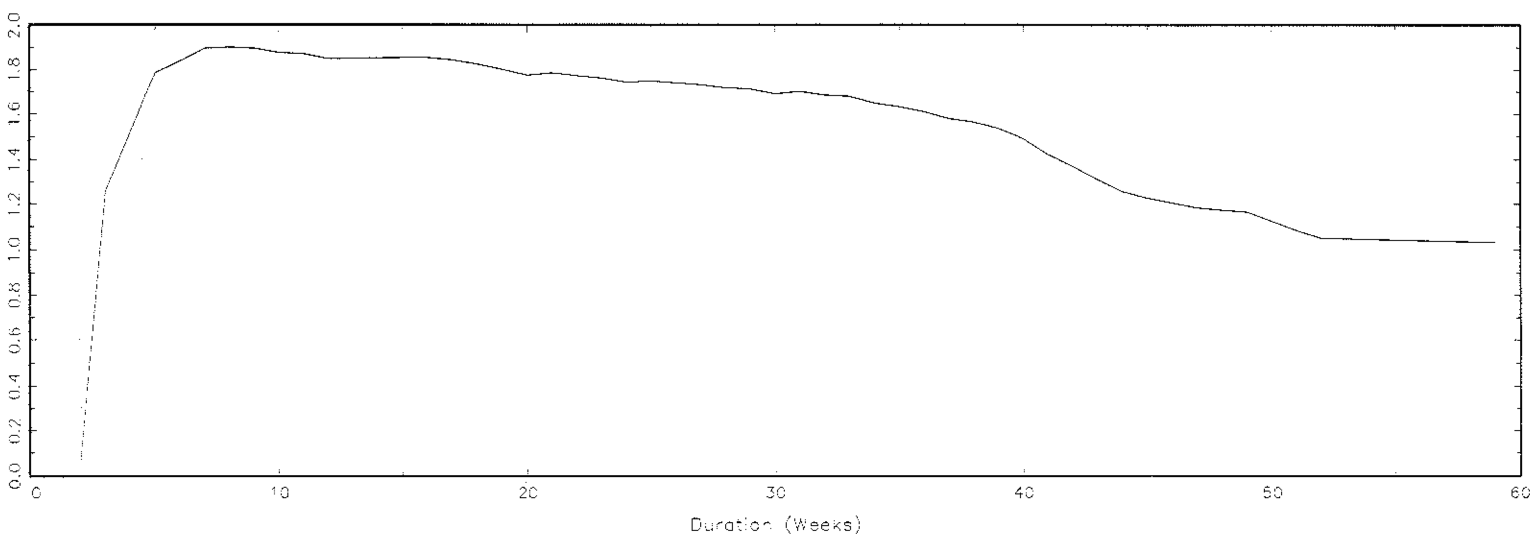
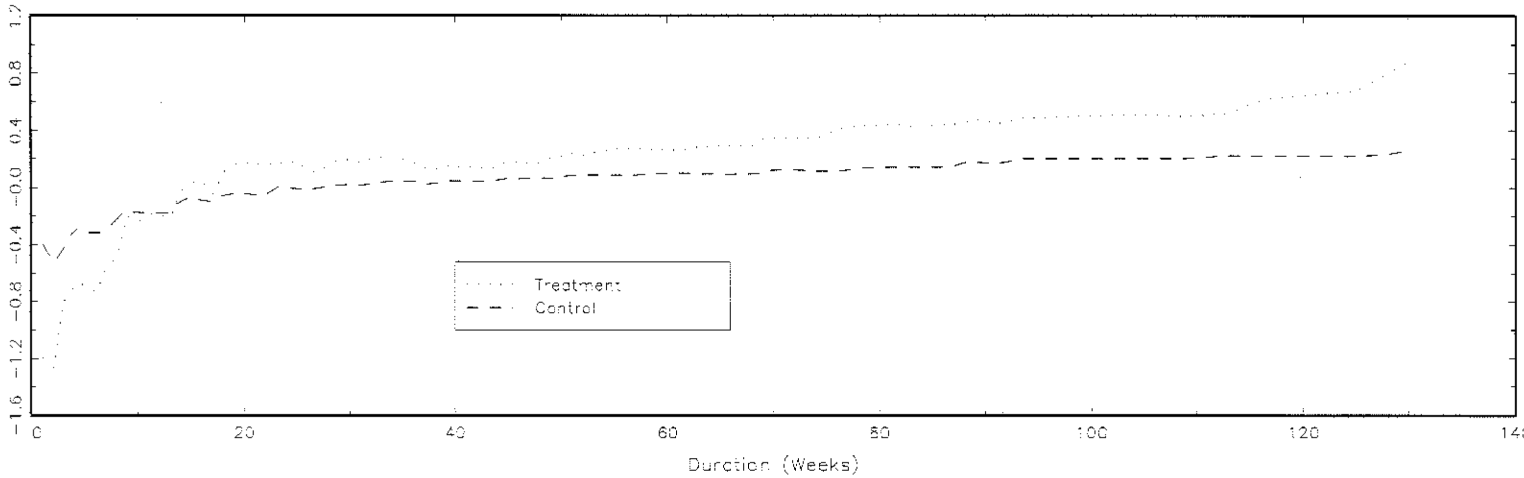


Figure 5

Impact of JI Training on Welfare Duration

$S(\tau)^{t} - S(\tau^{t'})$



Post-Treatment: $S(\tau)_{T} - S(\tau)_{NT}$

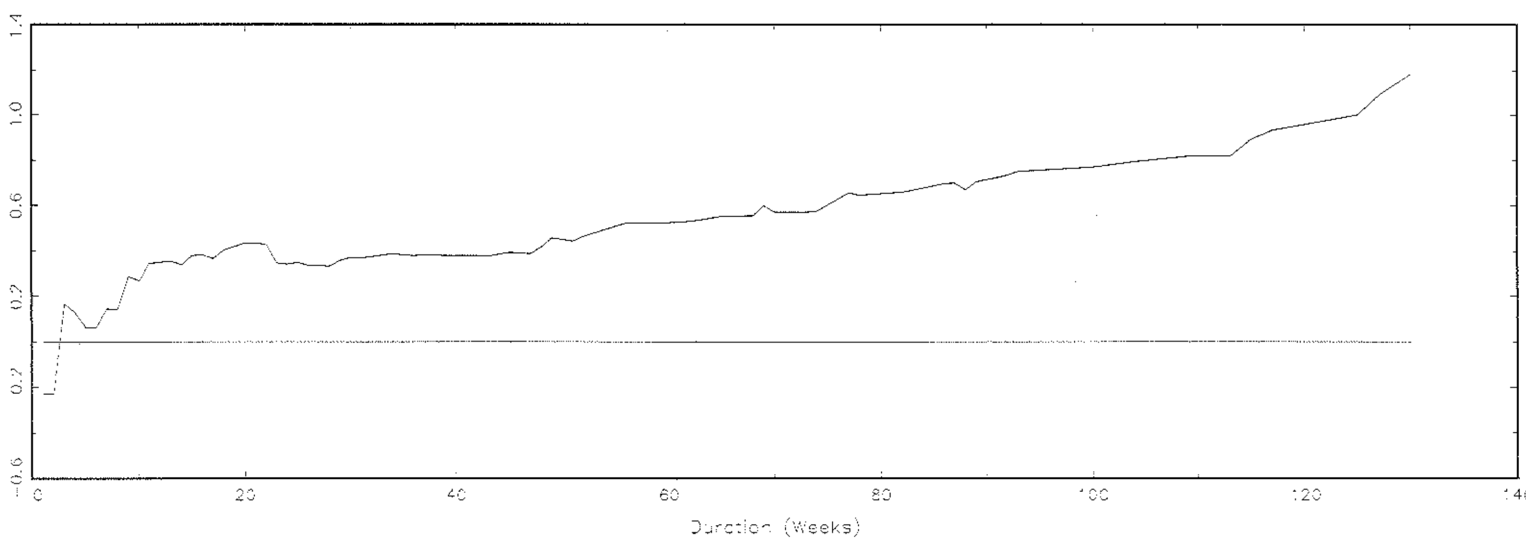
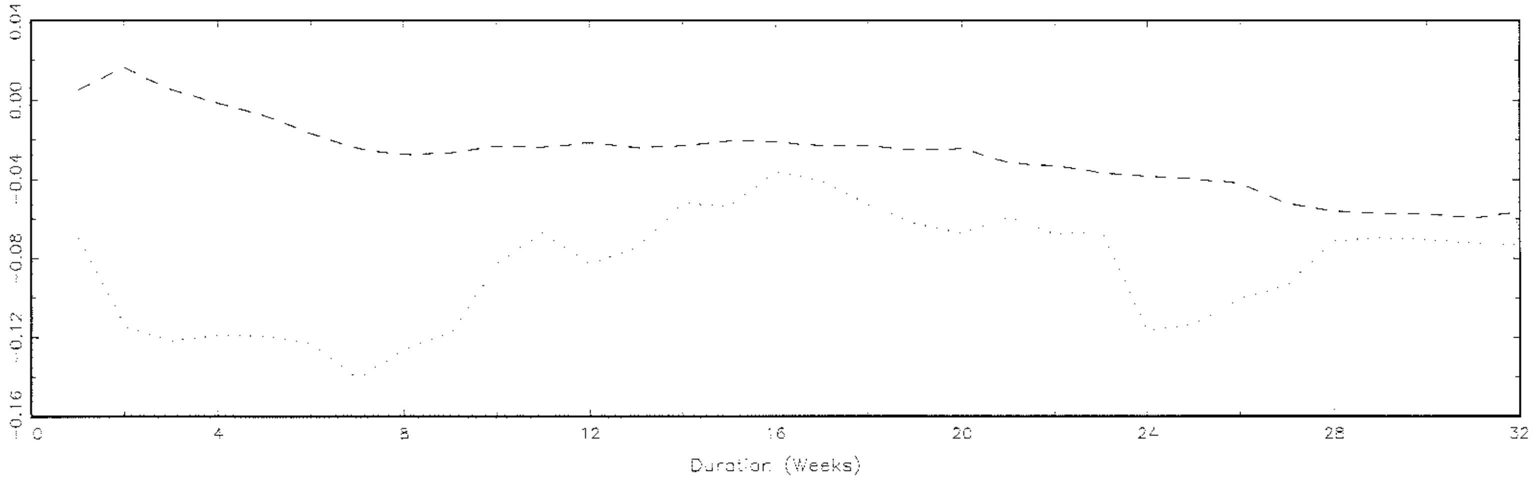


Figure 6
Impact of Welfare Training on Employment Duration
$S(\tau)^{t}-S(\tau^{t'})$



Post-Treatment: $S(\tau)_{T} - S(\tau)_{NT}$

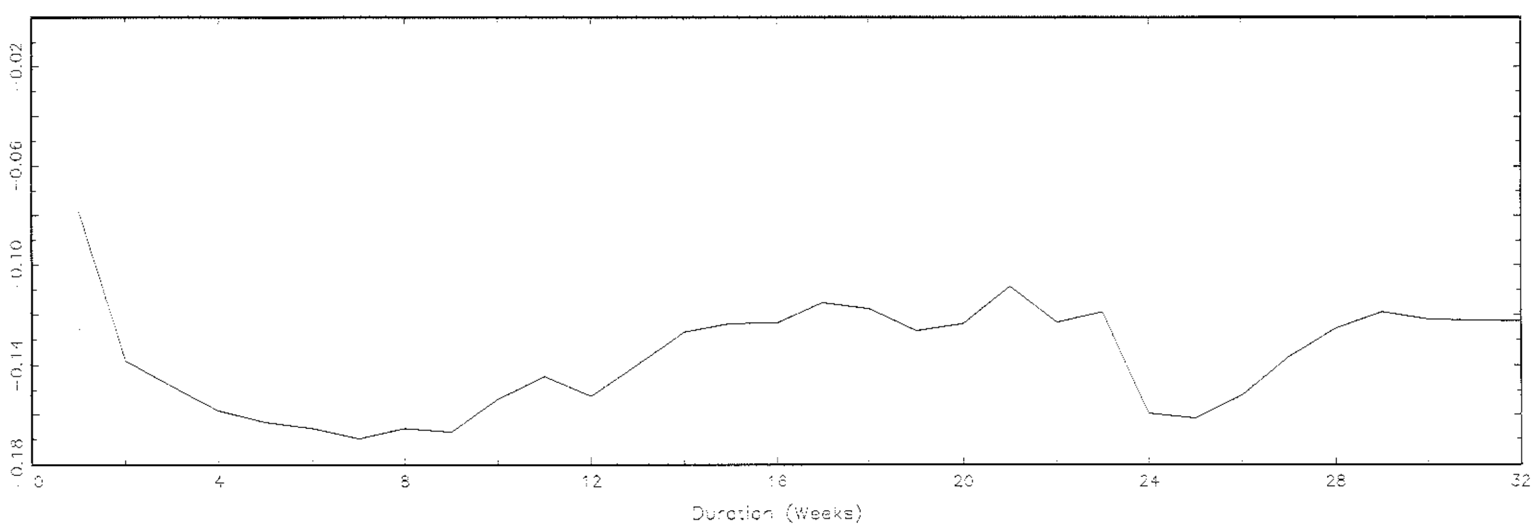


Figure 7
Impact of Welfare Training on Welfare Duration
$S(\tau)^t - S(\tau^{t'})$

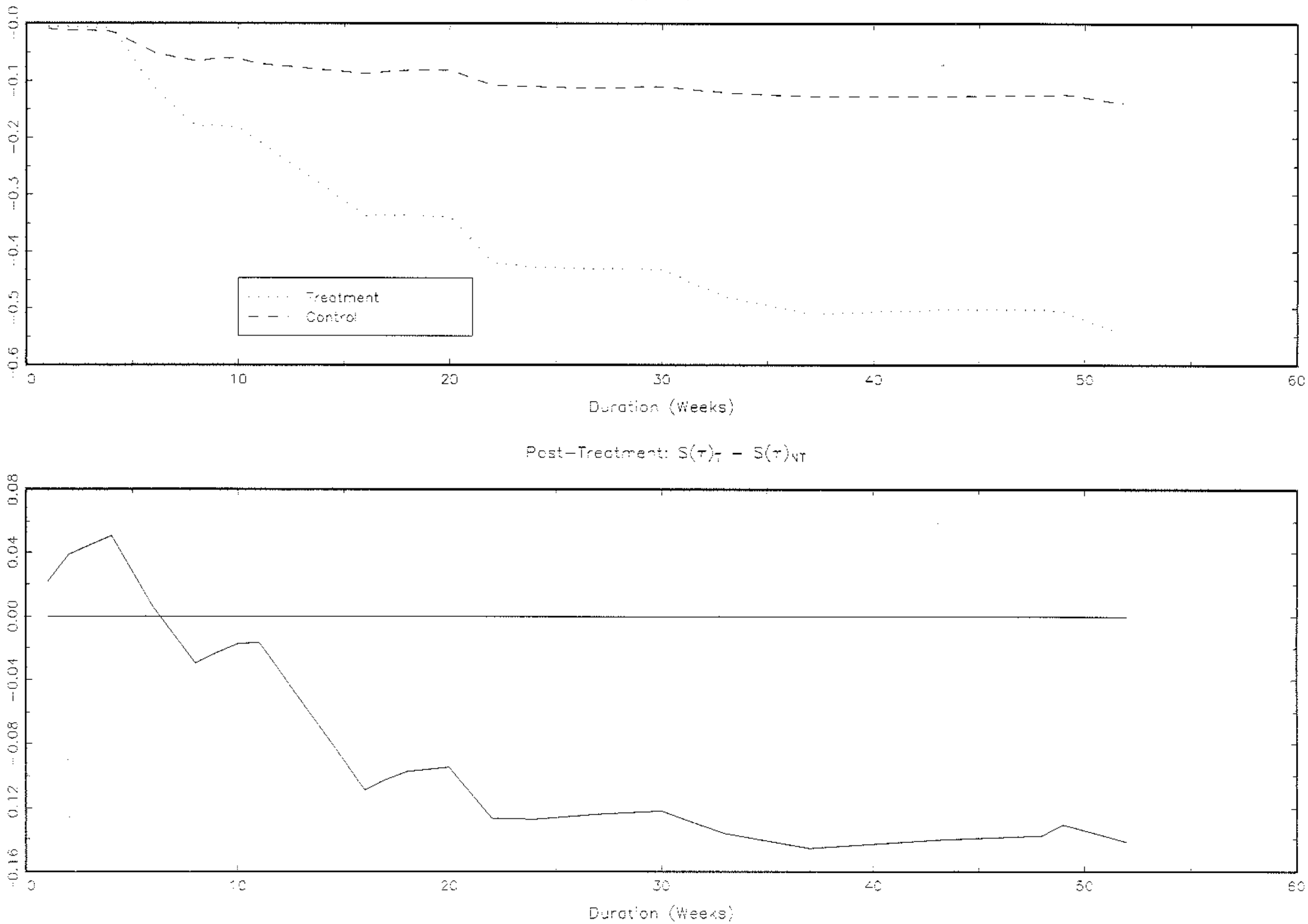
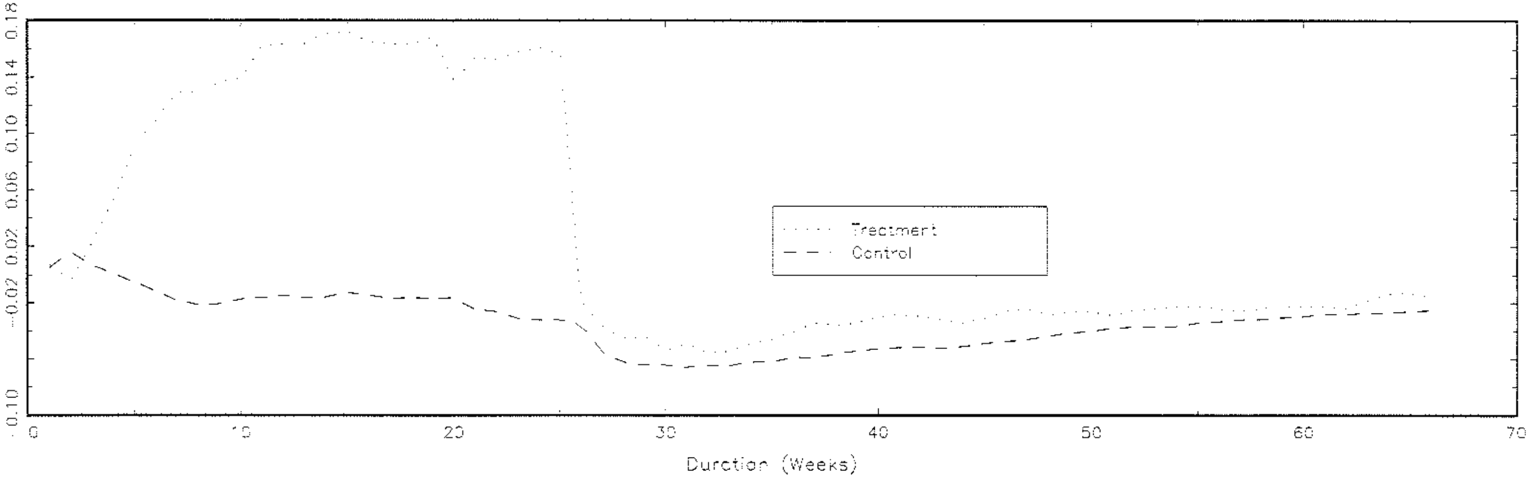


Figure 8

Impact of JRB on Employment Duration

$S(\tau)^t - S(\tau^{t'})$



Post-Treatment: $S(\tau)_T - S(\tau)_{NT}$

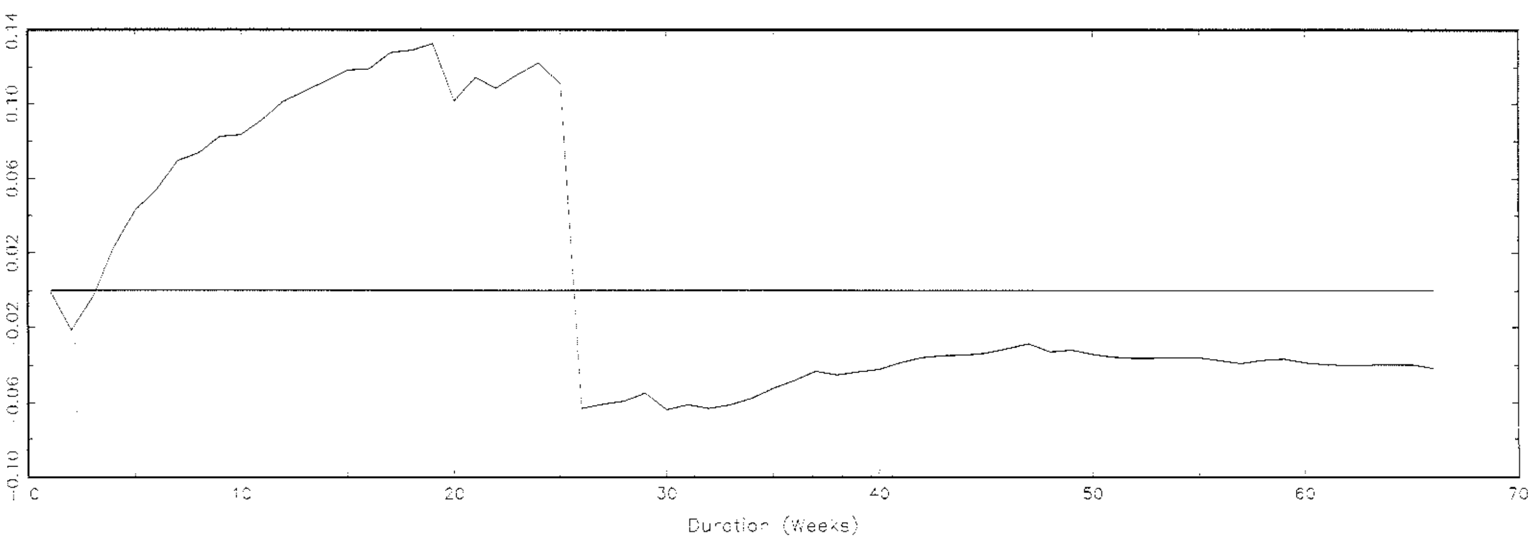
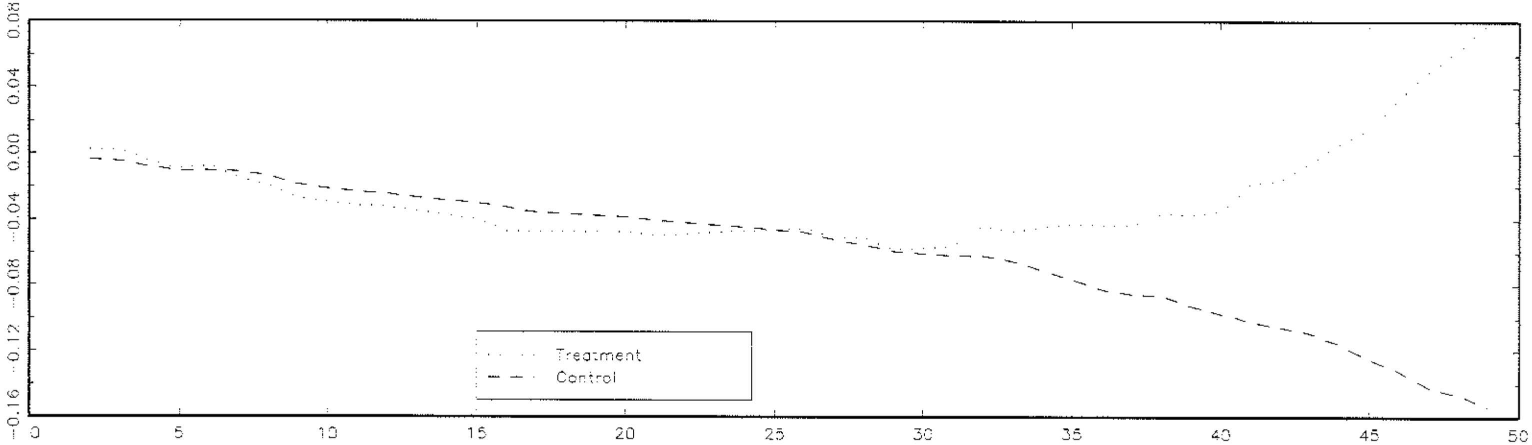


Figure 9

Impact of JRP on Unemployment Duration

$S(\tau)^t - S(\tau^{t'})$



Post-Treatment: $S(\tau)_T - S(\tau)_{NT}$

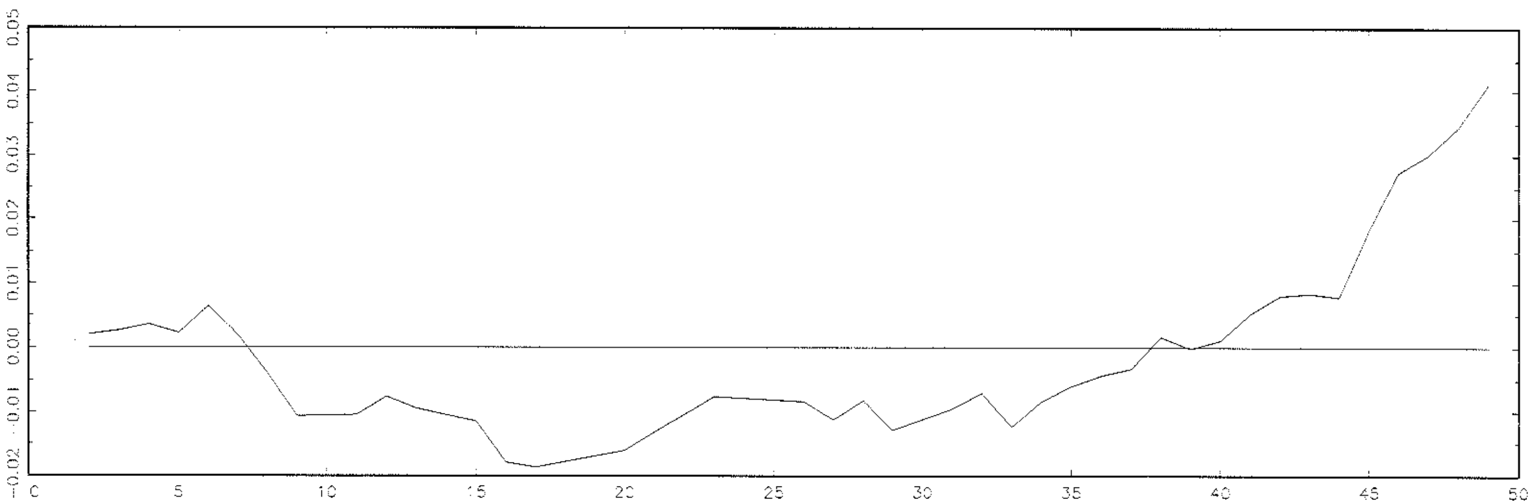
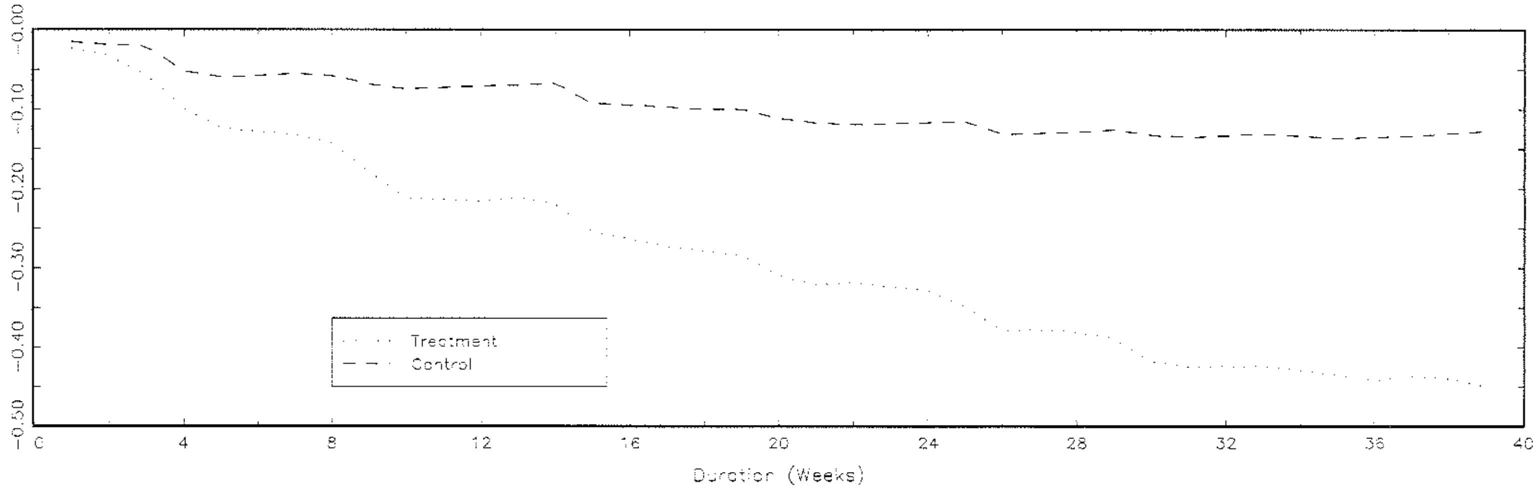


Figure 10
Impact of JRP on Welfare Duration
$S(\tau)^{t} - S(\tau^{t'})$



Post-Treatment: $S(\tau)_T - S(\tau)_{NT}$