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An evaluation of the active labour market programmes in Sweden

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by

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Abstract

The low unemployment rates traditionally enjoyed by Sweden have often been attributed to the country's extensive system of active labour market programmes, which have thus often been regarded as a model for other countries to emulate. The paper investigates the presence of short- and long-term treatment effects on a number of outcomes, including employment and unemployment benefit collection. Special attention is devoted to subsequent outcomes experienced by former participants, in particular to their job attachment and their performance should they fall back into unemployment. Finally, the distinctive feature of the Swedish labour market policy, whereby participation in programmes renews eligibility to generous unemployment compensation, is investigated in relation to the incentives it is likely to create to keep cycling between compensated unemployment spells and programme participation. The approach used is propensity score matching, with some additional analyses trying to account for a partially unobserved outcome variable due to misclassification problems in the data. Joining a programme as opposed to waiting longer in open unemployment appears on average to have a positive dynamic effect on participants' employment rates. The overall findings indicate however that the human capital-enhancing component of the programmes may not always be strong enough to outcompete the work disincentives provided by the system. Furthermore, even when cycling has been ruled out by focusing on individuals observed to exit their unemployment spell, programmes are found to have no effect on any of the outcomes considered.

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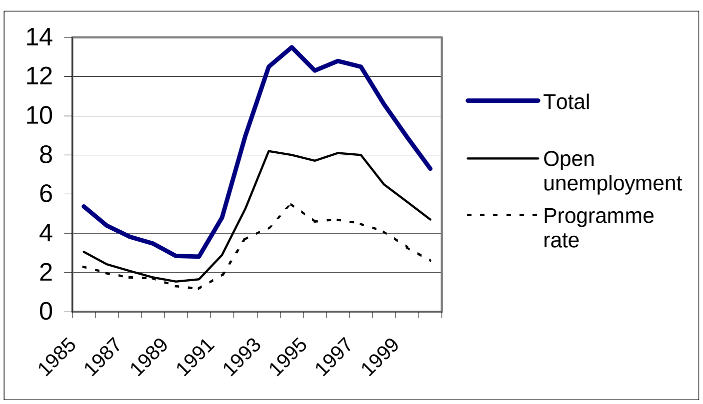
1. Introduction

By European standards, Sweden has traditionally enjoyed low unemployment rates. The 'Swedish model' with its extensive offer of various labour market programmes has been credited with this outcome by many observers (e.g. Layard, Nickell and Jackman, 1991). Quite interestingly, in the UK a programme (the 'New Deal') sharing some of the features of the Swedish set-up has recently been introduced (April 1998).

The beginning of the 1990s has however witnessed a dramatic change in the labour market situation in Sweden, whose economy was brought to its deepest economic slump in more than 50 years. Unemployment swiftly reached unprecedented levels, more than quadrupling between 1990 and 1993 (cf. Figure 1.1), and this despite a further expansion in the offer of labour market programmes. Concomitantly, the budget has come under increasing pressure; in 1993 Sweden's budget deficit at 13% of GDP is among the highest in the Western world, while the national debt has reached 80% of GDP.

Figure 1.1 Swedish total unemployment, broken into open unemployment and programme participation rates, 1985-2000 (%)



Sources: Open unemployment rate from the Labour Force Survey (LFS); programme rate as the number of participants from the National Labour Market Board (AMS) register over the labour force from LFS.

Labour market programmes have represented a large investment for the Swedish government, which spends over 3% of GNP on such measures, compared to 2.1% in Germany and 0.4% in the US (Forslund and Krueger, 1995, Table 1). Programmes for the unemployed are a large-scale undertaking; in 1997, for instance, 191,000 individuals – representing 4.5% of the labour force – participated on average in such programmes (excluding measures for the disabled). In each month of the same year, 37,000 individuals, or 10% of the total yearly stock of unemployed, were enrolled in the most expensive programme, labour market training.

In the presence of both rising unemployment and expanding budget deficit, the justifiability of the expense of the Swedish labour market programmes hinges on the assessment of their actual effectiveness.[3] Secondly, it may well be that the bundle of measures that was effective in tackling a certain kind of unemployment is no longer so successful in a different environment. Since the underlying causes of unemployment are diverse and most likely to vary among target groups, geographical regions as well as over time (see Schmidt, 2000), the developments in the last decade seem to cast doubts as to the universal cure-all nature of the 'Swedish model'.

Considering the importance of these issues, several authors have in the past lamented the absence of an adequate microeconometric literature assessing the impacts of the Swedish system.[4] The recently established Swedish Office of Labour Market Policy Evaluation (IFAU), however, has increasingly carried out research in this as well as wider directions – examples include Carling and Gustafson (1999) for self-employment subsidies *versus* subsidised jobs, Melkersson (1999a, b) for programmes targeted at the disabled, Larsson (2000) for youth programmes, Johansson and Martinson (2000) for two types of labour market training programmes, and Carling and Richardson (2001) for the relative efficiency of eight of the Swedish programmes; Dahlberg and Forslund (1999)

[3] Ideally, even if positive net (social) benefits from the programmes were found, one would have to show that this was the best outcome achievable from the resources invested.

[4] For a macroeconomic evaluation of the Swedish programmes, see Forslund and Krueger (1995); Forslund and Kolm (2000) investigate the effects of the Swedish programmes on wage pressure.

look at displacement effects of the programmes, while Lundin and Skedinger (2000) investigate the effects of the degree of decentralisation on programme administration.

Further general interest in the Swedish case arises from a few features of the institutional set-up which raise some methodological issues not previously addressed in the typical US programme evaluation literature.[5]

As it will be more fully described in the next section, in Sweden the active and passive components of the labour market policy are closely intertwined: unemployed workers who participate in a programme effectively renew their eligibility to generous unemployment compensation. Since individuals may thus have an incentive to select into the programmes just to renew their benefits, programmes may end up reinforcing the work disincentive associated with the unemployment insurance system.

In a context where individuals can potentially participate in a dozen of different programmes, the object of the evaluation – the 'treatment' itself – is not an immediate choice. Furthermore, *any* unemployed who registers at an employment office can potentially become a participant. In fact, it may be argued that those who are not observed to go on a programme before finding a job have not been treated *because* they have waited long enough to find a job. In addition, since our data record individual histories, an individual may enter a programme in a later registration spell, all of which makes the selection of the 'group of non-participants' not as straightforward as in the typical evaluation of a given programme.

These difficulties are compounded by the fact that all the programmes under scrutiny are ongoing programmes, which take place continuously over time. Individually differing starting dates make the time before and after the programme well defined only for a given participant.

[5] Some Western European countries' labour market programmes share a few of the Swedish features (in particular, a variety of ongoing measures); see in particular the recent work by Lechner (e.g. 1996, 1999a, 1999b) and Gerfin and Lechner (2000).

The extremely rich dataset being used in the evaluation of the Swedish programmes may finally give rise to additional general interest in the results. Often in the literature programme effects are evaluated at a given – and arbitrary – point in time (e.g. on the last observation day, or after a year). By contrast, being able to follow up individuals for a relatively long time (up to six years) allows us to capture dynamic effects, including both short and long-term ones. In addition, the available data allows us to identify a larger number of destination states and outcomes than generally available, so that different questions concerning programme effectiveness may be asked:

- Are programmes effective in helping participants find a job faster?
- Are programmes suited to endow them with skills and good working habits to keep a job longer?
- Do programmes foster the further acquisition of human capital in the regular education system?
- Do programmes lead to further (repeated) participation in subsequent programmes?
- Have programmes taught former participants how to deal more effectively with a new unemployment experience?
- Do programmes induce participants to start cycling between compensated unemployment and eligibility-renewing programme spells?

Before turning in Section 5 to the answers to these and other questions, the next section outlines the Swedish labour market policy in some detail, while Section 3 describes how the data we use captures such an institutional framework. Section 4 expounds the evaluation problem, while the relevant parts of Section 5 explain how the statistical matching techniques have been adapted to the Swedish context. Subsection 5.2.4 additionally tries to address the problem of a partly unobserved outcome variable arising from attrition/misclassification problems in the available database. Section 6 looks at unemployment-programme cycling behaviour, while the concluding section draws everything together.

2. The Swedish labour market policy

The Swedish labour market policy has two components: a benefit system that supports individuals while unemployed and various active labour market programmes offered in order to improve the opportunities of unemployed workers.

Unemployment compensation is provided in two forms, the most important one being unemployment insurance (UI). UI benefits are very generous – the income-related daily compensation is 80% of the previous wage[6] – and are available for a long duration – 60 calendar weeks, more than twice the maximum duration of unemployment benefits in the US.

An (even part-time) unemployed person registered at a public employment office and actively searching for a job is eligible for unemployment benefits if the following conditions are satisfied:

1. membership condition: the claimant must have paid the (almost negligible) membership fees to the UI fund for at least 12 months prior to the claim;
2. work condition: the claimant must have been working for at least five months during the twelve months preceding the current unemployment spell. Until 1996, a 5-month participation in practically any labour market programme would count as employment in allowing participants to become eligible for their first time.
3. an offer of 'suitable' work – or of a labour market programme – must be accepted; refusal to accept a job/programme might lead to expulsion from compensation.

[6] This maximum level of compensation has changed a few times during the 1990s; from 90% of the previous wage, it was reduced to 80% in July 1993, then further to 75% in January 1996, before being restored to 80% in September 1997. Note however that the system has a ceiling in terms of the amount of daily compensation, so that formerly high-wage unemployed individuals benefit from a lower compensation rate.

The second form of unemployment compensation is cash labour market assistance (KAS). This supplementary compensation system has been mainly designed for new entrants in the labour market who usually are not members of any UI fund. Daily taxable benefits are significantly lower than UI (around half) and are paid out for half the UI period (30 calendar weeks)[7] and claimants are subject to a work condition similar to the one for UI, which can however be replaced by the education condition of having finished at least one year of school in excess of the nine compulsory ones.

Turning now to the labour market programmes, their stated overall purpose is to prevent long periods out of regular employment and to integrate unemployed and economically disadvantaged individuals into the labour force. There are various kinds of programmes available, ranging from labour market retraining to public sector employment such as relief work, to subsidised jobs, trainee replacement schemes, work experience schemes and job introduction projects, to programmes targeted at specific groups, such as a variety of special 'youth measures', special measures targeted at the disabled, or self-employment and relocation grants. The different types of programmes may thus variously benefit participants by either facilitating their job search, augmenting their human capital with formal teaching or by providing them with job experience, improving their working habits or offering a cheap way for employers to screen their productivity.

Most programmes have a maximum duration of six months, though participants stay on average for around four months.

The two components of the Swedish labour market policy just outlined – passive unemployment benefit policy and active labour market programmes – are closely linked, in that participation in a labour market programme for five months counts as employment and thus qualifies for a renewed spell of unemployment compensation. Thus despite the fact that the period during which an

[7] The maximum durations reported for both UI and KAS benefits refer to individuals below 55 years. Claimants aged over 55 are entitled to 90 weeks of UI and 60 weeks of KAS; if aged

unemployed job-seeker can receive unemployment benefits is fixed, it is in fact possible to extend it indefinitely by using programme participation as a passport to renewed eligibility.

A peculiar type of 'programme' should be mentioned before concluding. This 'programme' does not allow to renew eligibility; in fact, 'participation' in it is a pre-requisite for collecting unemployment benefits. Such a 'treatment' is simply being registered at an employment office. Registered job-seekers take advantage of the various employment services offered by the offices, not only in terms of the increasingly computerised job information and matching of vacancies to applicants, but also in terms of the so-called job-seeker activities, which include search-skill-enhancing activities such as training courses on how to apply for a job and motivation-raising activities.

In Sweden nobody is really left 'untreated'. The treatment status to which programme participants will be compared to in the following analyses is thus not one of being completely abandoned to fend for oneself, but the baseline treatment offered by the employment offices. In conclusion, it is important to bear in mind that the effectiveness of the properly defined programmes will be assessed against the benchmark of the employment offices' services. Since job services are expected to enhance job search intensity and efficiency rather than human capital or working habits, it might for instance be that this 'no-treatment' is more effective than standard programmes in reducing unemployment duration, but less in prolonging job retention. Finally, if such job services turn out to be more effective than standard programmes, they will most likely be more cost-effective as well (cf. Figure 2.1).

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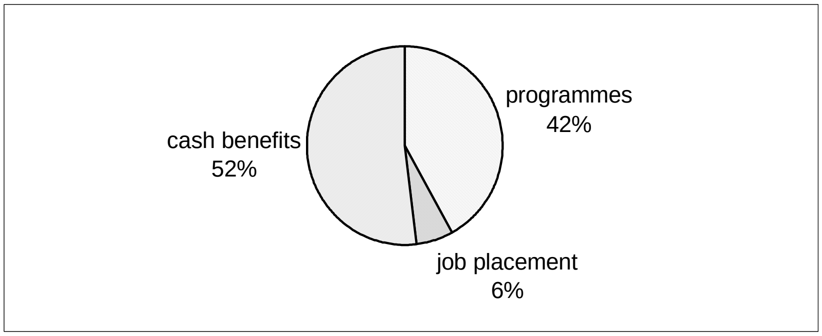
over 60, to 90 weeks of KAS.

Figure 2.1 Breakdown of labour market policy expenditures (1995/96)



Note: The cost of the programmes and that for job placement are not directly comparable, since some of the programmes pay participants an allowance equivalent to the unemployment cash benefits the individual would have received as openly unemployed.

3. Data and sample selection

The dataset we have constructed to capture the institutional framework described in the previous section is the result of combining two main sources, which reflect the programme component (Händel) and the benefit component (Akstat) of the labour market policy.

Händel is the unemployment register, of which the various databases contain information on all unemployed individuals registered at the public employment offices. This register-based longitudinal event history dataset, available from 1991, provides for each individual the start and end date of registration spells, and within these, of 'open' unemployment spells and programme participation spells. In addition to individual labour market status information over time, the data provide important personal characteristics of the job-seekers, as well as of the occupation they are looking for. We have used the information regarding the reason for ending the registration spell (e.g. obtained employment, gone on regular education or left the workforce) to impute the labour market status of the individual between registration periods.[8]

[8] Following Carling, Holmlund and Vejsiu (1999) unemployment spells durations have been slightly adjusted in order to disregard short interruptions of the spells. Two adjacent unem-

Akstat originates from the unemployment insurance funds and provides information on individuals entitled to UI benefits or KAS. This dataset, available from 1994, includes information on the amount and type of compensation paid out to the individual, as well as on previous earnings and working hours.

The end result is a very large and comprehensive dataset of 150,000 individuals who first[9] register in 1994 and are then followed until the end of November 1999.

Unemployment individuals entitled to compensation are required to be registered at a public employment office in order to collect their benefits. Furthermore, the employment offices have a monopoly over the active labour market programmes, so that any individual intending to participate needs to register. In fact, over 90% of the unemployed do register at an employment office[10], making our employment register-based dataset quite representative of the population of interest.

The lives of these individuals have been divided into adjacent sub-periods in which they are characterised by a different labour market category (e.g. employed, unemployed, part-time unemployed, temporary employed, on relief work, on a labour market training programme, etc.). In addition to being extremely rich in providing information (to the day) about the duration of stay in a labour market category, the final dataset includes the necessary array of demographic and human capital variables together with information on unemployment benefit recipiency and type of entitlement.

From the original data set, a subset of 116,130 individuals has been selected who, in addition of first entering the employment offices in the same calendar year, 1994 (when unemployment was still at its highest – cf. Figure 1.1), regis-

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ployment spells separated by a short (no longer than 7 days) break have been merged into one long spell. A similar adjustment has been made when an individual's first period of registration at the employment office is a short non-unemployment spell immediately followed by an unemployment spell.

[9] Strictly speaking, one cannot exclude that our individuals have had contact with the unemployment office before August 1991, date when Händel starts.

ter as openly unemployed. In particular, given that the main purpose of the programmes is to enhance the re-employability of the unemployed, we exclude individuals registering as employed or directly entering as programme participants.

All our individuals are in the 18-55 age group and have no occupational disabilities.[11]

4. The evaluation problem and statistical matching

The prototypical evaluation problem can be framed within the potential-outcome approach[12], a framework which fruitfully highlights the definition of treatment effects and clarifies the conditions needed to identify them.[13]

We are interested in evaluating the causal effect of a treatment of interest (treatment 1), relative to another treatment (treatment 0), on the outcome or response variable Y, which is experienced by units in the population of interest. It is thus essential that each unit be *potentially* exposable to any of the two treatments. For example, an evaluator may want to assess the causal effect of a policy variable such as participation in a programme, relative to no participation,

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[10] From a validation study by Statistics Sweden, quoted in Carling, Edin, Harkman and Holmlund (1996, Footnote 7).

[11] In the full dataset, over 18,000 individuals were observed to experience at least one negative duration (i.e. where the exit date from the spell comes before the entry date), resulting in 21,200 negative spells. After correcting what clearly appeared to be mistakes (in particular, when the negative spell has exactly the same number of days as the preceding spell as well as having a preceding category equal to the following), 8,800 negative spells are left. Following a suggestion by Katarina Richardson, the remaining 8,000 individuals involved have been kept in the sample after having deleted their spells from one spell before the negative one onwards.

[12] Though the concepts behind the framework have been around since Fisher (1935) and Neyman (1935), Rubin (1974) formally applied them to the study of causation; see however also the work by Roy (1951) and Quandt (1972).

[13] Standard references in the evaluation literature include the comprehensive work by Heckman, LaLonde, and Smith (1998), as well as Heckman and Robb (1985), Heckman, Ichimura, and Todd (1997, 1998), Heckman, Ichimura, Smith and Todd (1998) and Rosenbaum and Rubin (1983, 1985).

on the employment rate experienced at a given time by individuals in the unemployed population.

Let Y_1 be the outcome that would result if the individual were exposed to the treatment of interest (treatment 1) and Y_0 the outcome that would result if the same individual received treatment 0. Let us denote the binary indicator of the treatment actually received as $D \in \{0, 1\}$. For a given individual i, the actually observed outcome is then $Y_i = Y_{0i} + D_i (Y_{1i} - Y_{0i})$. To complete the notation, let X be the set of attributes (i.e. characteristics that are not affected by the treatment, such as all time-invariant characteristics of the individuals, as well as pre-exposure variables).

It is useful to highlight that already at this very general stage an assumption has to be made. The stable unit-treatment value assumption (SUTVA)[14] requires that an individual's potential outcomes depend only on his own participation, not on the treatment status of other individuals in the population (thus ruling out cross-effects or general equilibrium effects) and that whether an individual participates or not does not depend on the participation decisions of others (e.g. thus excluding peer effects in the participation decision).[15]

The effect of treatment 1 on unit i as measured by Y and relative to treatment 0 (in the following the treatment effect for a single unit i) is $Y_{1i}–Y_{0i}$, the difference between the unit's potential outcome were it exposed to treatment 1 and the unit's potential outcome were it to receive treatment 0.

Since no unit can be in two different states at the same time, so that either Y_{1i} or Y_{0i} is missing for each i, at the core of the evaluation problem is the attempt to estimate missing data: the 'Fundamental Problem of Causal Inference' makes

[14] SUTVA was first expressed by Rubin (1980) and further discussed in Rubin (1986) and Holland (1986b). SUTVA in causal effects studies is a more general version of the assumption of no interference between units in experimental studies (Cox, 1958) or of stable response in surveys (Rubin, 1987).

[15] SUTVA is in fact the assumption that the model's representation of outcomes is adequate, that is that Y_{ti} – the observed outcome of unit i if unit i is exposed to treatment t – only de-

it impossible to *observe* the individual treatment effect and thus to make causal inference without making generally untestable assumptions (Holland, 1986a).

Whether inferences are drawn from experimental or observational data, extremely strong assumptions are needed to identify the individual treatment effect[16] or other parameters depending on the joint distribution of Y_{1i} and Y_{0i}. By contrast, the *average* treatment effect at the population, or at a sub-population, level can be identified under generally less stringent assumptions (some of which will be highlighted shortly). Parameters that only depend on the marginal distributions of Y_{1i} and Y_{0i} are the 'average treatment effect' $E(Y_1-Y_0)$, the 'average treatment effect on the non-treated' $E(Y_1-Y_0|D=0)$ and the 'average treatment effect on the treated' $E(Y_1-Y_0|D=1)$. The last parameter is the one receiving most attention in the evaluation literature and will be the focus of the following discussion[17] as well as the object of evaluation in the empirical part (as explained in Section 5.1, in Sweden all (registered) unemployed individuals potentially belong to the treatment group). In our case, assessing the expected treatment effect for those individuals actually observed to receive the treatment thus amounts to addressing the question of how the post-programme outcome of unemployed workers participating in a Swedish programme compares to how they would have fared had they not taken part in the programme, on average.

For participants we do observe Y_1, the outcome after the treatment, so that the average observed outcome for participants is an unbiased estimate of the

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pends on i and t (in particular, not on what treatments other units receive nor on the mechanism assigning treatment t to unit i) and that D_i only depends on i.

[16] Examples include the unit homogeneity assumption ($Y_{1i}=Y_{1j}$ and $Y_{0i}= Y_{0j}$ for two units i and j) or the assumption (implied by the former) of a homogeneous treatment effect across the population (additivity assumption). Another assumption, mainly relevant to physical experiments, consists of constancy of response to treatment 0 over time together with the effect of treatment 0 and the measurement process not affecting the value of Y_{1i}, so that unit i may be sequentially exposed to treatment 0 and then to treatment 1, each time measuring the resulting Y_i (Holland, 1986a).

[17] For the average effect on the non-treated, a symmetric procedure applies; while the average treatment effect is simply a weighted average of the other two parameters. In the special case where the effect of the treatment is homogeneous among individuals, the three effects would coincide.

first component of the effect of treatment on the treated, $E(Y_1|D=1)$. We do not however observe participants' Y_0, the outcome they would have experienced had they not participated. To overcome the fundamental problem of causal inference, the counterfactual $E(Y_0|D=1)$ needs to be somehow constructed on the basis of some usually untestable identifying assumptions that justify the use of the observed pairs (Y_1, D=1), (Y_0, D=0).

A general issue is that treated individuals may not be a random sample of the population, but they may receive treatment on the basis of characteristics which also influence their outcomes. For example, if there is cream-skimming by programme officers – trying to pick out the best candidates for training – , or if the most able, determined and motivated individuals manage to get into the programmes, participants are of better quality than the rest, and would have done well anyway, resulting in an over-estimate the impact of the programme. Since one wants to estimate returns from participating in a programme which are not attributed to the composition of the unemployed workers taking these programmes, $E(Y_1|D=1) - E(Y_0|D=0)$ would in general be biased for the effect of treatment on the treated.[18]

An exception is when the independence assumption $Y_0 \perp D$ can credibly be invoked. This is in particular the case of a randomised experiment, where random assignment of individuals to treatment ensures that potential outcomes are independent of treatment status. Since now $E(Y_0|D=1) = E(Y_0|D=0) = E(Y|D=0)$, the treatment effect can consistently be estimated by the difference between the observed mean of the outcome variable in the treatment group and the observed mean in the no-treatment group.

When randomised experiments are not available, as in the present evaluation of the Swedish programmes, other estimators have to be devised, relying on appropriate identifying assumptions.

[18] Holland (1986a) refers to it as the *prima facie* causal effect – in general an associational parameter for the joint distribution of the observable pair (Y, D).

One approach to construct a suitable comparison group is based on statistical matching. Matching estimators try to mimic an experiment by choosing a comparison group from all non-participants such that the selected group is as similar as possible to the treatment group in terms of their observable characteristics. Simple matching estimators, for instance, pair each treated unit to an observably similar non-treated unit; smoothed versions[19] associate to the outcome Y_i of treated unit i a 'matched' outcome given by a weighted average of the outcome of all non-treated units (within the common support), where the weight given to non-treated unit j is in proportion to the closeness of the observables of i and j.

When the relevant differences between any two individuals are captured in their observable attributes (where 'relevance' is in terms of their potential outcomes), matching methods can yield an unbiased estimate of the treatment impact. This underlying identifying assumption, henceforth CIA ('conditional independence assumption', also called 'ignorable treatment assignment' or 'selection on observables') requires the evaluator to have access to a set of conditioning observed attributes X such that, for a given value of the X vector, the distribution of the (counterfactual) outcome Y_0 in the treated group is the same as the (observed) distribution of Y_0 in the non-treated group, in symbols:

$$Y_0 \perp D \mid X$$ [20]

In other words, what the CIA requires is that the finally chosen group of matched controls – i.e. conditional on the X's used to match and select them – does not differ from the group of treated by any variable which is systematically linked to the non-participation outcome Y_0. This allows the use of matched non-participants to measure what participants would have fared, on average, had they not participated.

[19] See in particular the kernel-based matching estimator proposed by Heckman, Ichimura and Todd (1997, 1998) and Heckman, Ichimura, Smith and Todd (1998).

The plausibility of CIA needs to be argued on a case-by-case basis, with account being taken of the informational richness of the available dataset (i.e. X) and the institutional set-up where selection into the treatment takes place. In particular, the set of the X's should contain all the variables that are thought to simultaneously influence *both* participation *and* outcomes in the absence of participation. A discussion of how likely the basic matching assumption is to be fulfilled in our case is postponed to the first part of Section 5.2.2.

Formally, the effect of treatment on the treated

$$\begin{aligned}
&E(Y_1|D=1)-E(Y_0|D=1)\\
&=E_X\left[\left(E(Y_1|X,D=1)-E(Y_0|X,D=1)\right)\mid D=1\right]\\
&\overset{CIA}{=}E_X\left[\left(E(Y_1|X,D=1)-E(Y_0|X,D=0)\right)\mid D=1\right]\\
&=E_X\left[\left(E(Y|X,D=1)-E(Y|X,D=0)\right)\mid D=1\right]
\end{aligned}$$

The conditional treatment effect is estimated by taking the difference of the average observed outcome in the two groups conditioning on X; averaging over X in the treated (D=1) group then yields the estimated treatment effect.

For the matching procedure to have empirical content, it is also required that $P\{D=1|X\}<1$ over the set of X values where we seek to make a comparison, which guarantees that all treated individuals have a counterpart in the non-treated population for each X for which we seek to make a comparison.[21] In particular, if there are regions where the support of X does not overlap for the treated and non-treated groups, matching has to be performed over the common support region; the estimated treatment effect has then to be redefined as the mean treatment effect for those treated falling within the common support. Note

[20] For the average effect of treatment on the treated, the weaker version of the CIA in terms of conditional mean independence suffices: $E(Y_0 \mid X, D=1) = E(Y_0 \mid X, D=0)$. For the average effect of the non-treated, either version of the CIA would be in terms of Y_1.

[21] When object of the evaluation is the effect of treatment on the non-treated, we need to assume that $0<P\{D=1|X\}$, so that a match can be found for all D=0 units.

that if the treatment effect varies among individuals, restricting to the common subset may actually change the parameter being estimated.[22]

A practical problem in implementation arises if the X's are highly dimensional, and especially if they contain continuous variables (which would have to be discretised). In this case, when performing matching the number of observations in each cell can get very small, and there may be empty cells left.[23] A very useful variable in this respect is the so-called propensity score $e(x) \equiv Pr\{D=1|X=x\}$ – the non-trivial (due to the aforementioned condition) conditional probability of participation given a vector of observed characteristics x.

As Rosenbaum and Rubin (1983) show, by definition treatment and non-treatment observations with the same value of the propensity score have the same distribution of the full vector of regressors X (the balancing property of the propensity score: $X \perp D \mid e(X)$). It is thus sufficient to match exactly on the propensity score – a single variable on the unit interval – to obtain the same probability distribution of X for treated and non-treated individuals in matched samples.

Under CIA and the condition that $0<e(X)<1$ over $\tilde{\mathrm{X}}$:

$$Y_0 \perp D \mid e(X) \text{ for } X \text{ in } \tilde{\mathrm{X}}$$

Under the two matching assumptions, a matched sample at each propensity score $e(X)$ is thus equivalent to a random sample: individuals with the same value of $e(X)$ but a different treatment status can act as controls for each other, so that at any value of $e(X)$, the difference between the treatment and the non-treatment averages is an unbiased estimate of the average treatment effect at

[22] An advantage of social experiments is that since randomisation generates a comparison group for each X in the population of the treated, the average effect of the programme can be estimated over the entire support. By contrast, under the CIA matching generates a comparison group, but only for those X values that satisfy $0<\text{Prob}\{D=1|X\}<1$.
[23] With a binary treatment and K binary covariates (i.e. dummy variables), the minimum sample size for each cell having at least one treated and one control is $2 \bullet 2^k = 2^{k+1}$.

that value of $e(X)$.[24] The estimate of matching can thus be thought of as a weighted average of the estimates from many mini random experiments at different values of $e(X)$.

Possibly the main attraction of the matching method is its non-parametric nature, which avoids the need to define a specific form for the outcome equation, selection process or unobservables in either equation. In practice, though, the propensity score needs to be estimated and this is generally done parametrically. Even so, the semi-parametric method does not rely on distributional assumptions nor does it impose functional forms restrictions in the outcome equation (while e.g. least squares adjustments heavily rely on linearity, which is usually justified neither by economic theory nor by the data[25]). In addition, heterogeneous individual treatment effects are allowed for, so that no assumption of a constant additive treatment effect for different individuals is required. Finally, in contrast to standard parametric methods, matching estimators highlight the problem of common support.[26] In fact, the absence of good overlap may in general cast doubts as to the robustness of traditional methods relying on functional form to extrapolate outside the common support.

In a model-free environment, matching is thus able to eliminate two of the three selection-bias sources identified by Heckman, Ichimura, Smith and Todd (1998): the bias due to the difference in the supports of X in the treated and control groups (failure of the common support condition) and the bias due to the

[24] This result holds for a more general 'balancing score' $b(X)$, a function of X such that $X \perp D | b(X)$. The finest and most trivial balancing score is X itself, while the propensity score is the coarsest one.

[25] One could however allow for more flexible specifications by including higher-order and interaction terms.

[26] Nearest-neighbour matching estimators – provided not too 'tolerant' – automatically use the observations within the common support of X (or of $e(X)$), while good practice (see Heckman, Ichimura and Todd (1997, 1998) and Heckman, Ichimura, Smith and Todd (1998)) requires the analyst to perform kernel-based matching only over the region of common support.

difference between the two groups of the distribution of X over its common support. [27]

5. Matching in the Swedish institutional set-up

5.1 Treated and non-treated in Sweden and other key choices

As mentioned in the Introduction, the Swedish institutional set-up poses a few interesting methodological issues which have to be resolved before applying matching techniques.

Object of our evaluation is a system with a wide array of different ongoing programmes, which take place continuously over time and are open to all registered job-seekers; unemployed individuals in turn can – and in fact often do – register repeatedly, and they can be treated at different times during their observed unemployment history. In such a context, several choices have to be taken, including how to actually define the treatments, as well as the treated and the non-treated individuals. Only then will one be able to formulate meaningful causal questions.[28]

As to the choice of **units**, we will look at individuals registering as unemployed for their first time.

As to the definition of the **treatments**, the focus is accordingly on the *first* treatment individuals may receive within their first registration as unemployed, so that any subsequent programme participation is viewed as an outcome of that first treatment. Furthermore, the Swedish active labour market policy is considered in its totality: the aim of the present study is to shed some light on the *overall* effectiveness of the Swedish unemployment system, a system compris-

[27] The remaining source of bias is the one due to selection on unobservables. Arguing its importance amounts to arguing the inadequacy of CIA for a particular problem; as noted above, this should be done in relation to the richness of the available observables (i.e. data) in connection to the selection/outcome processes.

[28] It is certainly true what Holland (1986b, p.969) remarks: "it is usually easy to make an identification of U, K and Y with units, treatments and a response variable in a randomised experiment, but complex observational studies can provide cases in which reasonable people might disagree as to the proper identification of the elements of the model."

ing both a collection of different types of programmes and a closely intertwined unemployment benefit component. All the various types of programmes are thus aggregated into one 'programme'.[29] Treatment 1 is thus any programme in which a first-time unemployed can participate. The definition of treatment 0 is however considerably less straightforward.[30]

Generally speaking, in Sweden nobody decides *never* to join any programme. By contrast, everyone will – in principle – be treated at some time, if he is unemployed long enough. In fact, bringing this reasoning to its limit, one could argue that the reason an unemployed individual has not been observed to go on a programme is *because* he has found a job (before).

In such a context, two sets of analyses will be pursued, each one looking at a different subgroup of individuals and asking a different type of question.

We first start by considering individuals *entering* unemployment (for their first time). Once registered, unemployed job-seekers (and programme officers) are likely to take their decisions *sequentially* over time in unemployment, so that the choice open to them is not whether to participate or not to participate *at all*, but whether to participate now or not to participate *for now*. An unemployed individual may decide not to participate for now, knowing that he will be able to join a programme later on. For those de-registering before being treated, the waiting choice has proved successful. When looking at individuals entering unemployment, the effect one can thus try to assess in such an institutional context concerns the possible benefits from joining compared to *waiting* (that is, of actual participation compared to postponing the participation decision). The effect of the joining decision will be evaluated with respect to a series of **outcomes**, ranging from the duration of the first unemployment experience (Section 5.2.6), to various labour market states of individuals over time (the differential probabilities of unemployment, of employment, of subsequent programme participation, of non-participation including in particular further studies, of benefit

[29] Looking at the impact of different programmes is left to future work.

collection and of cycling between unemployment compensation and programmes for up to five years from the treatment – Section 5.2.3 – 5.2.5).

The focus of the second set of analyses will be on subsequent outcomes experienced by individuals *who have been observed to follow a given path*. Conditioning on past outcomes, in particular on exit from the unemployment system, allows one to look at the impact of actual programme participation compared to *actual* non-participation. In particular, we will be able to address the effectiveness of previous programme participation in helping *employed* individuals keep their jobs longer (5.3.1), or in helping *newly unemployed* individuals exit their new unemployment spell faster (5.3.2).

A final difficulty arises from the fact that we are considering a system of ongoing programmes, taking place continuously over time and with individually differing starting dates (both in calendar terms and in relative terms, i.e. in terms of time since registration). This implies that potentially important variables related to the distance in time to the beginning of the programme are not defined for the non-treated group and thus cannot be included in the estimation of the propensity score. Probably the most crucial of these variables is the unemployment duration prior to entering a programme.[31]

First of all, there is a question of 'common support': one needs to be unemployed to be enrolled into a programme, so all potential comparisons to a treated individuals should have at least reached a stage where they could have joined the programme.

Secondly, elapsed unemployment duration is likely to capture some important unobservables; since average unobserved 'ability' (standing for any unobserved skill linked to an individual's ability to exit an unemployment spell) declines over unemployment duration, a participant's observed time spent in unemploy-

[30] The discussion of an absent 'non-treatment' group was initiated by Carling and Larsson (2000a, b).

[31] The importance of recent unemployment histories (even more than earnings dynamics) as a variable to be controlled for has been highlighted in particular by Heckman, Ichimura and Todd (1997).

ment before entering a programme reveals a bound on his expected unobserved ability. One has thus to make sure that he is paired to a non-treated who has spent in unemployment at least the time it took the participant to join the programme.

Thirdly, there are some (albeit loose) regulations, as well as some particular incentives which link elapsed unemployment duration to programme participation; some programmes for instance formally require 4 months of open unemployment prior to enrolment, while approaching unemployment benefit exhaustion may make individuals more likely to enter a programme. Unemployment duration is thus an important X variable for explaining not only subsequent outcomes as discussed above, but also the participation decision (D=1).

Finally, in the presence of individually differing starting dates one needs to set a relative time scale to begin measuring the effect of the programme. Matched non-participants need to be assigned a hypothetical unemployment duration prior to the programme, which splits their observed unemployment duration into a pre-programme attribute and an outcome sub-spell. The interrupted unemployment duration of their respective matched participants (i.e. their time to the programme) would seem an obvious candidate.

Ideally, we would then match on T^1, the duration of the first unemployment spell interrupted by the programme. The obvious problem is that T^1 for the non-treated is an unobserved counterfactual (i.e. their waiting time before the start of a programme had they entered one) and it thus cannot be included in the estimation of the propensity score.

Still, for the reasons expounded above, it is essential to impose that all comparison individuals eligible to be matched to a given treated unemployed for T^1 before entering the programme have reached an unemployment duration of at least T^1: this ensures that both treated and control have a 'complete'[32] unemployment duration T^0 at least as long as T^1.

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[32] 'Complete' refers to the unemployment duration an individual would experience were he not to join a programme. T^0 is thus observed for individuals whose unemployment spell ends

5.2 The effect of joining versus waiting

5.2.1 Methodology

The requirement in terms of unemployment duration can be fulfilled in quite a natural way when implementing our version of the average effect of treatment on the treated, i.e. the average effect of 'joining compared to waiting for those who join'.

The causal inference problem we want to address can be formalised as follows. Let $D \varepsilon \{J,W\}$ denote the treatment indicator, where J stands for joining and W for waiting. The response variable is $Y(i, D, t)$, the labour market status of individual i at time t if individual i were exposed to treatment D. To simplify the notation, in the following the fact that the response is a function of time rather than simply a scalar is ignored and the short-hand notation Y_J and Y_W is used to denote the potential outcomes corresponding to joining and waiting, respectively.

The mean effect of treatment on the treated we aim to estimate is the average effect, for those observed to join a programme, of joining when they did compared to waiting longer than they have:

$$\tau \equiv \mathrm{E}(Y_J - Y_W \mid D{=}J)$$

We proceed by first subdividing this complex problem into a sequence of M simpler problems. By discretising unemployment duration, τ can be written as:

$$\tau = \sum_{m=1}^{M} E(Y_J - Y_W | D = J, T^1 = m)\ Pr(T^1 = m | D = J)$$

In concrete terms, we stratify our sample by (discretised) unemployment duration. In particular, for each m = 1, …, M we calculate τ^m, the effect of treating an unemployed individual in his m^{th} month of unemployment compared to

'naturally', i.e. in employment or non-participation (nobody is censored for the full 6-year observation period), while T^1 is observed for individuals joining a programme; for the latter, we know that $T^0 \geq T^1$.

not treating him at least until the end of his m^{th} month (in other words, compared to waiting at least m months):

$$\tau^m \equiv \mathrm{E}(Y_{J_m} - Y_{W_m} \mid D^m{=}1)$$

where $\{Y_{J_m}, Y_{W_m}\}$ is subsequent labour market status (respectively conditional on J_m – joining a programme in one's m^{th} month – on W_m – waiting longer than m months) and $D^m = 1$ if $D{=}J$ and $T^1{=}m$, i.e. for those joining a programme in their m^{th} month of unemployment. (Note that $\{D{=}J\} = \bigcup_{m=1}^{M} \{D^m{=}1\}$.)

The form of CIA we require to identify τ^m is:

$$Y_{W_m} \perp D^m \mid X{=}x,\ T^0{\geq}m \qquad\qquad (m{=}1, \ldots, M)$$

In words, given $X{=}x$ and having reached the same duration in unemployment[33], the distribution of Y_{W_m} for individuals joining a programme now is the same as the one for individuals deciding to wait. Say we observe two individuals with the same characteristics and who have reached the same unemployment duration, one of which joining a programme now, while the other deciding to wait longer. For the control, the waiting decision may prove successful, in the sense that he will subsequently find a job or de-register for other reasons. Alternatively, he may later decide – or be forced – to join a programme. What the CIA requires in such a context is that the probability distribution of such outcomes is the same for the observably-similar treated individual had he then decided to wait longer as well.

The parameter of interest τ can finally be calculated as a weighted average of the τ^m's (weighted according to the observed month of placement (T^1) distribution of the treated):

$$\tau = \sum_{m=1}^{M} E(Y_{J_m} - Y_{W_m} | D^m = 1)\ Pr(D^m = 1 | D = J) = \sum_{m=1}^{M} Pr(D^m = 1 | D = J)\ \tau^m\ .$$

[33] Note that we have in fact succeeded in ensuring that for *both* a given treated and his matched non-treated, the complete unemployment duration T^0 is larger than $T^1(=m)$.

In implementation, we set M=18, so that what we will be looking at is the impact of entering a programme within one and a half year of first registration; 94% of all treated are however observed to enter a programme within such a time span. (See the Appendix for the sample sizes of the two sub-groups by unemployment duration and for a description of the protocol used.)

This methodology has the rather peculiar feature that in the calculation of the overall effect a given treated individual (joining a programme in his m^{th} month) may also act as control (being a potential control for (m-1) groups of treated). In fact, the *relative* time scale, which is implicit in the set-up of the problem and central in the way it has been addressed, makes it quite intuitive to think of him as different persons, whose contributions start at different origins (i.e. at different T^1's).[34]

Following this procedure also allows us to assess whether there is a differential programme impact according to the time the individual has spent in unemployment before joining the programme (T^1). A very interesting group is the one observed to enter a programme exactly at benefit exhaustion: does joining a programme long before benefit exhaustion have a different effect from programme participation 'just' to renew eligibility?

To this respect it is important to underline that we are not looking at the multiple-treatment version of the problem, so that the M single effects cannot be compared between themselves in any causal way. This is because each single effect of treatment on the treated pertains to a *different* group of treated (the J_m's are effectively mutually exclusive) and relates to a different population of units (i.e. those still unemployed after m months, of which only smaller and smaller subsets are potentially exposable to treatments J_k and W_k with k>m). When comparing programme effects by month of entry, then, we shall be not

[34] In particular, a given treated individual counts as one treated person, whose contribution starts being evaluated at the moment he enters the programme (i.e. from *his* T^1) and may count as control person for 'otherwise similar' treated individuals who have joined a programme before him. In this latter case, his outcome represents the waiting counterfactual out-

trying to assess $E(Y^m - Y^k \mid D^m=1)$, i.e. the issue of whether joining a programme after m months in unemployment leads these participants to experience better or worse outcomes than if they had joined after k months; what we shall assess is if the programme effect for those individuals who join a programme after m months is better or worse than the programme effect for the k^{th}-month joiners.

Final methodological considerations

The type of matching used to select those non-treated to act as controls for the treated is nearest-neighbour matching with replacement (and within caliper): each participant is matched with replacement to that non-participant with the closest propensity score[35]; treated individuals for whom a match close 'enough' has not been found and unmatched non-treated individuals are discarded.

There is a trade-off between matching quality and variance involved in the choice of how many non-treated individuals to match to a single treated individual and in the connected issue of whether or not to allow a non-treated to be used more than once. The type of matching chosen here focuses on minimising bias alone (the large samples available and the size of the effects uncovered have essentially dictated this focus). One-to-one matching typically involves an efficiency loss, since only the participant and its closest neighbour are used, instead of a larger number of close neighbours. By contrast, when more than one comparison unit is assigned to any treated (e.g. in kernel-based matching), the variance is reduced but at the possible cost of an increased bias. Similarly, using the same non-treated individual more than once (matching with replacement) can improve matching quality[36], but it increases the variance, which has to be adjusted accordingly: the more times a non-treated observation is used,

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come for his matched treated individuals, where evaluation begins when the matched treated start their respective programme (i.e. from his matched treated individuals' T^1's).

[35] Given that any order-preserving transformation of the propensity score is sufficient to matching purposes, the predicted linear index rather than the predicted probability is used, as the former allows one to be more discriminating on individuals with predicted probabilities in the tails of the distribution.

the larger the related standard error of the estimated effect.[37] Ideally, the standard errors should also adjust for the estimation error in the score. For kernel-based matching, Heckman, Ichimura and Todd (1998) have analytically derived the asymptotic distribution for the corresponding estimator of the effect of treatment on the treated; for one-to-one matching, though, the common solution is to resort to bootstrapped confidence intervals. This is however not pursued here, mainly due to the extreme amount of computer time that would be needed.[38] The reported confidence bands will thus be too narrow; it is hoped though that the considerable sample sizes and the very flexible specifications (including all interaction terms) used in estimating the propensity score may lessen the understatement of variability due to using the estimated rather than the true propensity score.[39]

A final methodological consideration concerns the choice of the origin on the relative time scale. Having matched on the precondition that the comparison individuals have to have been unemployed for at least the number of months it took their unemployed treated counterpart to get into the programme, it is quite

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[36] In their application, Dehejia and Wahba (1998, 1999) do in fact find the performance of simple matching with replacement very satisfactory compared to more complex extensions or methods without replacement.

[37] Following Lechner (2000), the variance of the treatment effect at time t is calculated by assuming independent observations, fixed weights, homoskedasticity of the outcome variable within the treated and within the control groups and that the variance of the outcome does not depend on the propensity score:

$$\frac{1}{N_t^1} Var(Y|D_t^M = 1) + \frac{\sum_{i \in \{D_t^M = 0\}} \omega_i^2}{(N_t^1)^2} Var(Y|D_t^M = 0)$$

where N_t^1 is the number of matched treated present at time t, D^M=1 denotes the matched treated, D^M=0 the matched controls and ω_i is the number of times control i has been used, where $\sum_{i \in \{D_t^M = 0\}} \omega_i = N_t^1$.

[38] It takes almost 8 hours to match our 32,000 treated just *once*.

[39] Again, the effects uncovered are certainly not likely to give rise to debates as to their significance – cf. especially Section 5.3.

natural (cf. the discussion above) to set the origin t=0 for matched comparisons when their respective treated begin their programme.[40]

For the treated, however, the (not obvious) choice affects what treatment is and when its effect begins. Should one start to evaluate the performance of the treated when they enter or when they exit the programme? The answer largely depends on how the time spent on the programme is considered. Participation in some programmes requires that the individuals continue job-searching activities while participating. The offices too continue to search for them, since participants are still registered and requested to be 'at the labour market disposal'. Individuals are also required to drop out of a programme if a 'suitable' job is found for them. What this implies is that the observed programme duration is endogenous. In addition, in the light of the series of timing sub-questions asked (what is the effect of entering a programme *now*), it seems natural to set t=0 for the treated too at the time when they *begin* their programme.

In the following analyses of Section 5.2 then, treatment 1 is 'starting/being assigned to a programme', also commonly referred to in the literature as the 'intention to treat'. The causal effect of a programme starts to work with the beginning of the programme, so that any lock-in effect of the programme is viewed as a constituent part of the effect.

5.2.2 Determinants of programme participation

Selection of unemployed individuals into programmes and available information: Can the CIA be justified in our case?

Ideally, all the variables that influence the participation ('joining now') decision as well as potential 'waiting longer' outcomes – that is the outcomes that would occur where such decision to be postponed – should be included in the estimation of the propensity score. One thus needs to first consider the mechanisms through which individuals join a programme and to then assess whether

[40] If a comparison is used more than once, his contribution is repeatedly evaluated using the different starting dates of his matched treated counterparts.

the available regressors capture all the relevant factors affecting the participation decision and future potential outcomes.

Selection mechanism

In Sweden, the unemployment offices are characterised by decentralisation, which gives job officers quite a large degree of freedom, making it quite hard to see any clear patterns in the selection into the available programmes. Job-seekers themselves are however likely to have a considerable degree of influence; between 45-50 per cent of the participants in work experience schemes, work place introduction and labour market training were initiators to the programmes according to an unpublished Swedish study. Certain features of the programmes themselves are also likely to affect individuals' decisions to join one. In particular, entitled individuals who are running out of benefits may try to participate in programmes allowing them to renew eligibility. It would thus appear that information regarding the entitlement status of an individual (UI, KAS or not entitled), as well as the time he has already spent in unemployment is crucial, in that it affects both incentives to participate at a given time and future labour market outcomes. Similarly, factors relating to potential returns from participation – e.g. age, previous stock of human capital in terms of both specific and general education and specific experience, occupation being sought, citizenship – or affecting the opportunity cost or psychological cost of participation – such as gender, part-time unemployment status or previous education – should also be included in the conditioning set.

Still, officer selection is also likely to heavily bear on the final decision. The Swedish employment service is not limited to just a brokerage function, but it also administers both unemployment insurance and selection into programmes. For the programmes mentioned above, the job co-ordinators had in fact been initiators in about 30-40% of the cases. Also, although in general an unemployed job-seeker must be willing to participate, this may not always apply to individuals receiving unemployment compensation; for them, the proposal of a programme can be used as a work test, the turning down of which may entail suspension from benefits. Programme administrators often have considerable discretion over whom they admit into programmes. For some programmes, in particular labour market training, for instance, it may be felt that some officers may look at qualifications to cream-skim the most promising candidates.[41] To capture this selection by job officers, educational qualifications and entitlement status are again critical, with other useful information relating to an overall evaluation by the officer of the situation and character of the unemployed job-seeker – if already part-time employed, if looking for a part-time job, if willing to move to another locality, if the required qualification information is missing, if judged to be able to take a job immediately, or to be in need of guidance, or to be difficult to place. Such individual traits are in fact quite likely to affect the joining decision as well as the counterfactual outcomes in terms of subsequent participation or employment probability.

In the light of the selection mechanism just reviewed, the basic matching assumption in our context requires that conditional on all the information observed, the fact that an unemployed individual goes into a programme this month while another waits longer is not correlated with the outcome the individual joining now would have experienced had he not entered the programme for now (in particular in terms of later programme participation and job finding probabilities).

Two issues thus need to be considered: the possibility of anticipatory[42] effects in terms of future programme participation and in terms of future employment.

1. 'Waiting' outcome as subsequent programme participation

The institutional nature of the programme system (a seemingly continuous flows of different programmes often on an individual, *ad hoc* basis) should

[41] This is particularly true from 1999 onwards, when a new goal was introduced specifically for labour market training aiming at 70% of participants to be in a job six months after training. Before then, the incentives for cream-skimming may in general have not been very strong, since despite the fact that all programmes were followed up in terms of work placement rates, goals in terms of these rates were left rather vague (e.g. the placement rate "should increase").

[42] I thank Bernd Fitzenberger for having pointed this out to me.

make it less likely for an unemployed job-seeker to have to turn down a programme offer perceived as second-best in order to wait for a free slot on his first-choice programme (this would also reduce the likelihood of an 'Ashenfelter dip' problem in terms of reduced job search prior to participation). Even if he did wait, though, he would not enter his first-best programme with probability one, but would still be exposed to the possibility of finding a job or deciding (or be forced) to join another programme in the meantime.

An interesting piece of information in the Swedish dataset is an unemployment spell characterised by having been offered a labour market programme. Having gone through the selection process and having been offered a place is positively but far from perfectly correlated with subsequent participation.

In addition, the possibility of a supply constraint is directly controlled for by including variables that reflect local conditions and capture the possibility of a limited supply of programmes for which registered job-seekers may be competing. Thus besides the local variable denoting the county, we have constructed two such indicators at the level of the municipality (basically most "kommun" have one employment office, though the biggest cities might have several). The 'programme-rate' is given by the number of *participants* in all programmes as a proportion of all individuals registered as openly unemployed or as programme participants at the individual's municipality the month he enters the programme. This information relates to the local programme capacity, e.g. in terms of slots available. By contrast the 'offer-rate', representing the proportion of unemployed workers who have been *offered* a labour market programme out of all openly unemployed who are registered at that municipality in that month, gives an idea of the degree of utilisation of the programme capacity, e.g. in terms of a waiting list of unemployed individuals having already been offered to participate and against whom a potential candidate is competing.

Simultaneously conditioning on individual programme offer, local programme rate and local offer rate prevailing at the time of the joining-waiting decision should go a long way towards controlling for anticipatory effects in terms of subsequent programme participation. Finally, to account for seasonal patterns in programme entry[43], the month of first registration is also included.[44]

2. 'Waiting' outcome as subsequent employment

Several pieces of information are used to capture and characterise the recent unemployment history of the individuals under examination, the variable identified to be the most important one (even more than earnings dynamics) by Heckman, Ichimura and Todd (1997). All our individuals register at the unemployment office for their first time[45], so their only (recent) unemployment experience relates to the present unemployment spell. As outlined in Section 5.1, unemployment duration both captures observables and provides an upper bound to average unobserved ability. Part-time unemployment spells denote individuals who are still maintaining contact with the regular labour market and are probably both subject to less human capital depreciation and in a better position to look for a (full-time) job, by exploiting their bargaining position, additional contacts and references. Similarly, individuals entitled to unemployment benefits are also characterised by a good degree of labour market attachment due to the work requirement they have to fulfil. A subjective indicator of experience for the profession sought (none, some, good) is another interesting piece of information provided in the present dataset. Looking at the incidence of the three levels by age reveals that it is in fact quite a reliable indicator. Difference in prior work experience is important since it results from both observed and unobserved differences between characteristics of the treated and non treated (cf. Ham and LaLonde, 1996). This indicator can be viewed as a summary statistics of the amount – as well as effectiveness, transferability and obsolescence – of previous human capital accumulation, on-the-job training and learning-by-doing.

[43] Participation in the various types of programmes exhibits strong seasonal fluctuations, with significant drops in summer (August) and to a lesser extent in January.
[44] It should be kept in mind that we are always conditioning exactly on elapsed unemployment duration.
[45] At least since the beginning of the Händel dataset, in August 1991.

An issue that needs to be considered is the possibility for unemployed individuals to be re-hired by their former employer. If they *know* that their employer is going to call them back (e.g. they are seasonal workers, or have a credible agreement with their employer allowing the temporarily dismissed employee to collect unemployment benefits), they are likely to have no (or less) incentives to participate in programmes at any given month in unemployment; in addition, they *do* find employment. In such a scenario, the CIA would be violated: programme participation would be (negatively) correlated with a subsequent outcome (job accession), which would bias our estimates of programme effects (downwardly). Other observables included to control for potential anticipatory effects of this kind include the occupation/skill type of the job-seeker, as well as the month of registration, which should help capture seasonal unemployment. There is in fact explicit information in our data recording if an unemployed individual de-registers because he has obtained further employment by his former employer. Although qualitative analyses based on this information would indicate that recalls are not likely to be too serious a problem, the quality of this code has been found to be low: interviews with unemployed individuals who had found a job show that recalls are much more common than recorded in the data (Jansson, 1999).

More generally, the CIA would be violated in the presence of hidden job offers, that is if an individual waiting longer has decided to do so *because* he *knows* that he will be hired shortly. It has to be said, though, that our by-month version of CIA is less likely to be violated due to this kind of anticipatory effects than the traditional 'overall' CIA defining non-treated as those individuals whose unemployment spell is not interrupted by any programme, but directly ends in de-registration. An individual who in month m is offered to start a job in month k (k>m) represents a violation of the 'traditional' CIA, but of only (m-k) of our CIA's. How serious this issue is going to be in our case thus largely depends on the typical time span between job offer and job commencement (and whether or not an individual who is going to start a job typically remains/is allowed to remain registered at the unemployment office in the meantime).

Results from the estimation of the propensity score for each pair of subgroups

A series of 18 probits has been estimated, each one modelling the probability of joining a programme after $m \in \{1, 2, \ldots 18\}$ months spent in unemployment.

The two subgroups used to estimate the probability of participating in a programme, *conditional* on having reached an unemployment duration of m months, are the subgroup of treated entering a programme in their m^{th} month and the subgroup of non treated still unemployed after m months.

All variables are measured at first entry into unemployment, except those relating to the present unemployment spell, which are calculated at 'entry' into the programme (where for the non-treated, 'entry' refers to the month of unemployment by which they have been stratified). A rather flexible specification has been allowed for by including all relevant interaction terms and a quadratic for age.

It is interesting to look at the factors which significantly affect the treatment probability by unemployment duration. In fact, the direction of their impact as well as their statistical significance often change depending on how long the individuals have already been unemployed.

- Demographic factors

Gender – or its interactions –, as well as citizenship never seem to play a role in the participation participation. Age has by contrast a more complex influence: for individuals remaining unemployed up to 10 months, age has a strong negative effect, which increases, in absolute size, with age, while for individuals joining programmes later, age plays no statistically significant role up to the 14^{th} month, after which age has a positive effect, increasing with age.

- Human capital

The only educational category which has a consistently significant and positive effect on programme participation is vocational secondary education. Individuals with secondary (non-vocational) education are more likely to participate than the lowest educational group (compulsory education) only if in the first half of their first year of unemployment, while degree holders are slightly more likely to join than the base group in the second half of their first year of unemployment.

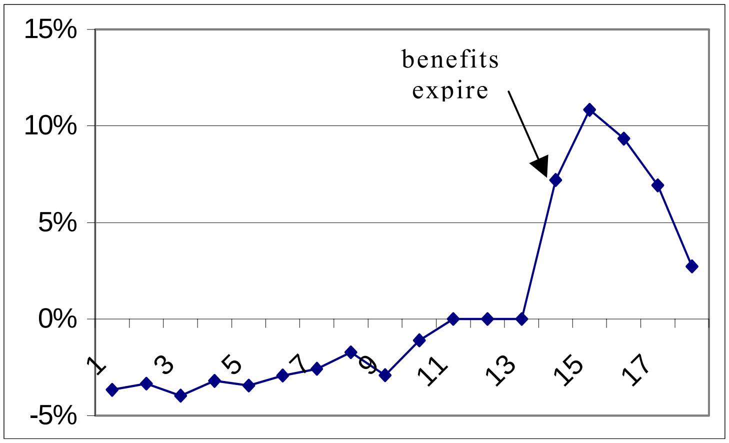
Indicators of the level of experience unemployed individuals regard themselves as having for the job they have applied for, as well as an indicator denoting the individual's judgement concerning the suitability of his education for the profession sought have no impact on the probability of receiving treatment.

On the other hand, missing the above information is a consistently extremely significant deterrent to programme participation. Information is generally missing when it is difficult to establish what occupation is suitable for the job-seeker, generally someone without experience and/or occupation-specific training or someone who for some reason cannot return to his previous occupation. Recently self-registration is becoming more common, so that an alternative explanation is that individuals who do not take the trouble of filling in the relevant form may send a signal to placement officers of not being particularly keen or 'co-operative';

- Entitlement status

The changing impact of entitlement status over the unemployment duration prior to joining the programme is undoubtedly the most interesting finding among the determinants of programme participation. Figure 5.1 shows the evolution of the marginal effect of UI status over unemployment duration prior to joining the programme. The plotted marginal effects represent the percentage points difference in the probability of entering a programme in that month of individuals entitled to UI vis-à-vis non-entitled individuals, who however have the same observed characteristics of UI-individuals.

Figure 5.1 Marginal effect of UI-status on the probability of joining a programme (percentage points difference in the treatment probability with respect to non-entitled with the same characteristics of UI individuals), by time unemployed prior to programme



For almost the entire first year in unemployment, roughly up to the 10th month, receiving benefits discourages programme participation. The effect then becomes insignificant, while just at benefit exhaustion, i.e. after 14 months, the effect becomes significant and its sign reversed: being entitled to UI increases the likelihood of now joining a programme, with the impact reaching its peak just after benefit exhaustion – in the 15th month of unemployment. Being entitled to UI confers to individuals reaching this unemployment duration an 11 percentage points higher likelihood of being accepted into a programme than observably identical non-entitled individuals. Carling, Edin, Harkman and Holmlund (1996) as well had found that UI-entitled individuals close to benefit exhaustion are significantly more likely to join a programme than those without unemployment compensation. Note that unemployed individuals seem to have quite a tight control on their entry into programmes[46], which lends further support to our assumption of no anticipatory effects.

[46] During most of the 1990s, unemployed individuals approaching benefit exhaustion were guaranteed the right to participate in programme in case they were unable to find a job.

- Profession sought

While the occupational sector does not seem to ever influence the probability of being treated, looking for a part-time job has a negative impact on the programme participation decision throughout. Specifically looking for a part-time job penalises individuals wishing to participate vis-à-vis those looking for a full-time occupation or being willing to accept either. By contrast, being willing to accept a job somewhere else has a positive, albeit small effect on the likelihood of entering a programme in a given month.

- Present unemployment spell

Having experienced at least one part-time unemployment sub-spell has a consistently negative impact on the probability of joining a programme. Individuals who have been employed part-time but, deeming this unsatisfactory, remained registered are significantly less likely to be offered a programme, irrespective of their unemployment duration. Within this group, entitled individuals (and especially UI – part-time unemployed can – and do! – claim benefits) are further penalised.

Looking now at the overall assessment by placement officers of the unemployed individual's situation, having already gone through the selection process and having been offered a labour market programme is the single most important determinant of the joining 'now' decision, with a consistently highly significant and positive impact of around 15 percent (for the average individual in our sample). Similarly, unemployed individuals having experienced sub-spells where it was felt they needed guidance are consistently more likely to participate than those unemployed who have never been considered in such a need. Maybe less intuitive is the positive effect of having experienced at least one unemployment sub-spell with the placement services (could take a job immediately), which has a positive impact throughout. By contrast, unemployed individuals having been considered difficult to place, or belonging to the special category (workers temporarily dismissed for less than 10 days, holiday workers not receiving unemployment benefits or job-seekers looking for a job with a duration of less than 10 days) are slightly less likely to join a programme at any month than those who have never belonged to any of these categories.

Finally, the significance and sign of month of entry into unemployment vary by unemployment duration.

- Local conditions

While the significance and sign of the county of residence vary by minimum duration of unemployment, the local participation rate displays a significantly positive impact on the participation probability throughout: individuals in a municipality with a programme capacity one percentage point higher than in another municipality are 0.1 to 0.2 percentage points more likely to participate in that month. By contrast, the local offer rate has a significantly negative impact; unemployed individuals in a municipality where the proportion of job-seekers on the programme waiting list is one percentage point higher than in another municipality are 0.2 to 0.9 percentage points less likely to join a programme.

Quality of matching

This paragraph presents some basic information concerning the ability of the propensity score of summarising all the pre-treatment variables, and in particular the extent to which matching on these estimated probabilities balances the relevant variables between the treated and the selected matched non-treated to be used as comparisons.[47]

Table 5.1 presents comparisons of the means in key characteristics, including the propensity score, between the two groups, as well as an indicator suitable to assess the ‘distance’ of the marginal distributions of the relevant characteristics in both groups (cf. Rosenbaum and Rubin, 1985). For a given covariate X, the standardised difference after matching is defined as the difference of the sample means in the treated and matched non-treated subsamples as a percentage of the

[47] For further details see the Appendix.

Table 5.1 Imbalance of the most important covariates:
Means in the two groups and standardised differences (%)

	Treated	Matched Controls	% bias
Age at entry	29.3	29.6	-2.86
Gender female)	49.3	48.5	1.48
Foreign	24.8	24.7	0.38
Education			
compulsory	17.8	17.5	0.78
upper secondary	17.3	17.4	-0.30
vocational upper secondary	49.7	49.6	0.06
university	15.2	15.5	-0.60
Education for job sought (yes)	58.2	58.6	-0.79
Experience for job sought			
some	24.0	24.3	-0.65
good	38.5	38.9	-0.91
Missing information	5.1	5.0	0.63
Entitlement status			
UI	30.3	31.3	-2.09
KAS	5.0	5.5	-2.30
County			
Stockholm	17.5	17.4	0.16
Göteborg and Bohus	14.8	14.8	0.06
Malmöhus	12.7	12.9	-0.55
Sector			
professional and technical work	13.9	13.9	0.24
health, nursing and social work	11.3	11.4	-0.03
administrative, managerial and clerical work	13.2	13.5	-0.92
sales	11.9	12.0	-0.13
production	20.9	21.5	-1.55
services	9.7	9.7	-0.08
Part-time	3.0	3.1	-0.71
Interlocal	17.4	17.5	-0.17
Month of registration	6.3	6.4	-2.82
January	8.8	8.6	0.54
June	18.2	17.9	0.74
August	12.2	12.4	-0.36
First register as part-time unemployed	3.2	3.0	1.16
Part-time unemployment	6.8	6.6	1.00
Type of unemployed			
able to take a job immediately	77.4	77.9	-1.16
offered a labour market programme	5.4	4.9	2.23
need guidance	21.2	21.3	-0.21
Propensity score	-1.262	-1.267	0.95
Average absolute standardised difference			0.86
Median absolute standardised difference			0.71
Number of observations	31,975	31,975	

square root of the average of the sample variances in the treated and non-treated groups.[48]

Overall, the quality of the selected comparison group seems high; the remaining bias in the score index is barely less than 1%, but what really matters is to look at the biases for the various covariates one is trying to balance. Important characteristics capturing human capital (such as experience, education and the ‘motivation’ indicator) present a remaining bias of less than 1%, while entitlement status is balanced with a 2% remaining bias. In fact, the bias for any covariate is well below 3%. A summary statistics for the overall balance of the regressors between the two groups is offered by the median and mean absolute standardised bias (median and mean taken with respect to all the variables), which are 0.7 and 0.9% respectively.

5.2.3 Outcomes over time

This section looks at various measures of outcomes over a five-year period to investigate how unemployed individuals who join a programme perform, on average, compared to a situation where they would have waited further. We correspondingly start by assessing the dynamic effect of joining a programme on the programme participation probability, summarising both the (endogenous) duration of the programme joined as well as possible repeated participation in subsequent programmes. Had they postponed their waiting decision, participants would have remained openly unemployed; an interesting outcome to assess is thus the unemployment probability, and in particular in terms of compensated-unemployment probability. We also look at the intermediate outcome given by the probability of being part-time unemployed (employed), before focussing on the employment probability, which after the initial lock-in effects comprises the both the treatment impact on the job finding probability and on job attachment once employed. We finally consider the treatment effect on other routes out of

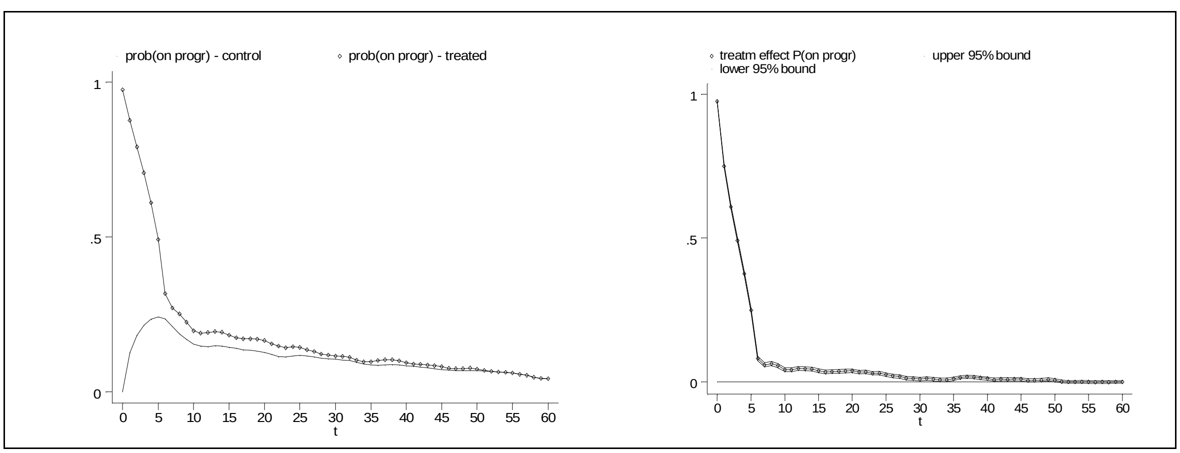
[48] In symbols: $100 \cdot \dfrac{\overline{X}_1 - \overline{X}_{0M}}{\sqrt{\left(V_1(X) + V_{0M}(X)\right)/2}}$

the unemployment system, in particular on the probability of regular education and of non-participation.

To fix ideas, it may be useful to start with the interpretation of the graphs in Figure 5.2, depicting the probability of programme participation for treated and matched controls over time, as well as the corresponding treatment effect. By definition, at time 0 the probability of the treated to be on a programme is one, while for the controls it is zero. One month later, at t=1, a participant has, on average, an 89% chance of still being on the programme. Had he instead not gone on a programme when he did but had he waited longer, he would be expected, on average, to be on a programme one month later with probability 0.12.

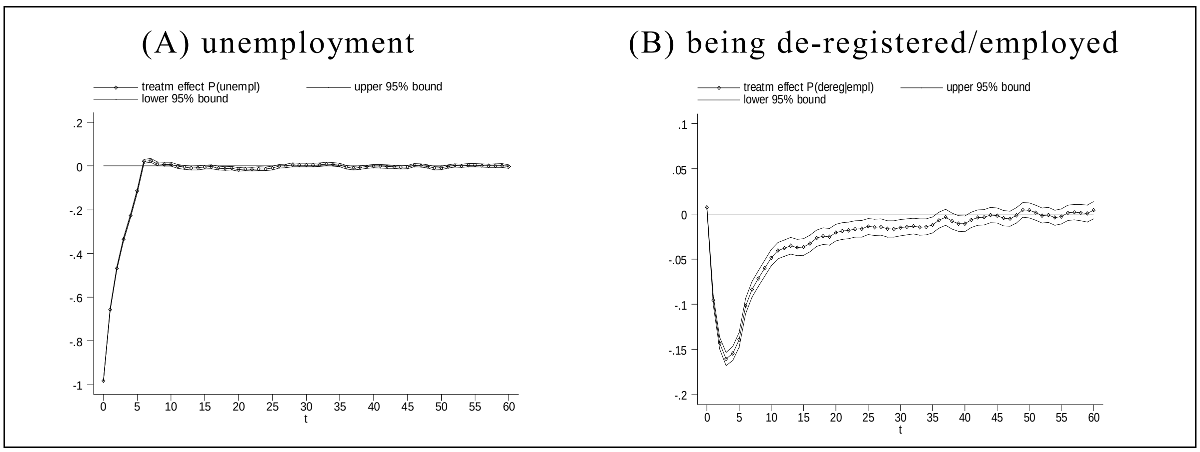
The treatment effect starts at entry into the programme, so that the treated-controls differential probability of subsequently being on a programme is viewed as the causal effect of joining 'now' *versus* waiting 'longer', averaged over the participants' distribution of months of placement. The graph shows a relatively large and persistent treatment effect: for up to two and a half years from joining, participants are significantly more likely to be on a programme than if they had further postponed their initial participation decision.

Figure 5.2 Programme participation probability for treated and controls over time and corresponding treatment effect



A serious indication about the influence of programmes on subsequent labour market status is given by the unemployment probability, and in particular by probability of being on unemployment benefits over time. While Figure 5.3A shows absolutely no treatment effect on the probability of being unemployed after the typical programme duration, Figure 5.4 indicates that *as soon as* the programme typically ends (i.e. after about 4 months), the negative effect (by construction, compensation while on programmes is not counted as unemployment benefits) abruptly turns into a large positive one. Over our horizon, participants remain significantly more likely to be drawing benefits up to four years after having joined the programme.

Figure 5.3 Treatment effect on the probability of

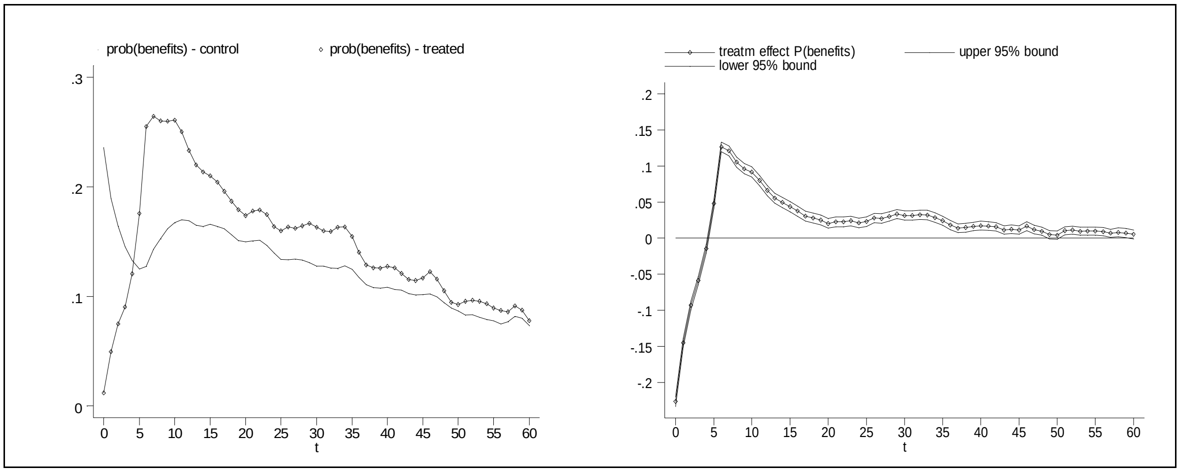


An intermediate outcome between employment and unemployment is part-time unemployment. On the one hand, if one looks at it as part-time *employment*, one may claim that such individuals are in fact in the regular labour market, are earning a wage, and are possibly accumulating job-specific human capital. On the other hand, these individuals view themselves as *unemployed* (albeit part-time), are considered by the offices as job-seekers (looking for a full-time job), and, most importantly, can claim benefits[49]. In fact over two thirds of all part-time unemployment spells in our data have positive compensation. Thus it is with mixed feelings that one may regard the outcome in terms of the prob-

[49] Individuals who look for full-time work but are only able to get a part-time job can receive UI to compensate for their loss of income; since 1993 there is no limit on the duration of such complementary payments.

ability of being part-time (un)employed. It turns out that programmes have an initial negative (lock-in) effect, which then becomes insignificant over the remaining horizon.

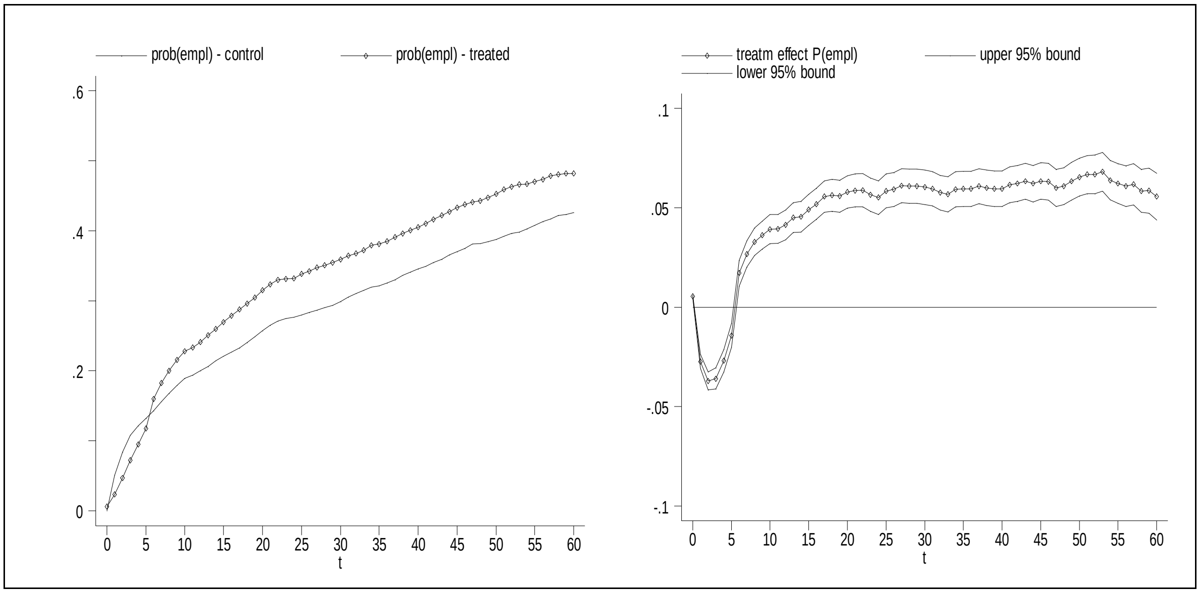
Figure 5.4 Benefit collection probability for treated and controls over time and corresponding treatment effect



A particularly important outcome is the probability of being employed over time. Do individuals who have joined a programme in a given month spend more time in regular employment, overall, than if they had further postponed their participation decision? How does programme participation affect participants' short and long-term employment probabilities?

The graphs in Figure 5.5 show that while on average joining programmes initially reduces the chance of finding employment (lock-in effect), when they typically end it appears that participants perform significantly better than their (at-least-up-to-now)-non-treated counterparts, displaying statistically significantly higher and increasing employment shares over time. Over the first 5 years since the start of the programme, the treated seem to enjoy an average of 5% higher employment probability. Joining a programme 'now' seems to actually reduce the expected overall time out of regular employment, on average.

Figure 5.5 Employment probability for treated and controls over time and corresponding treatment effect



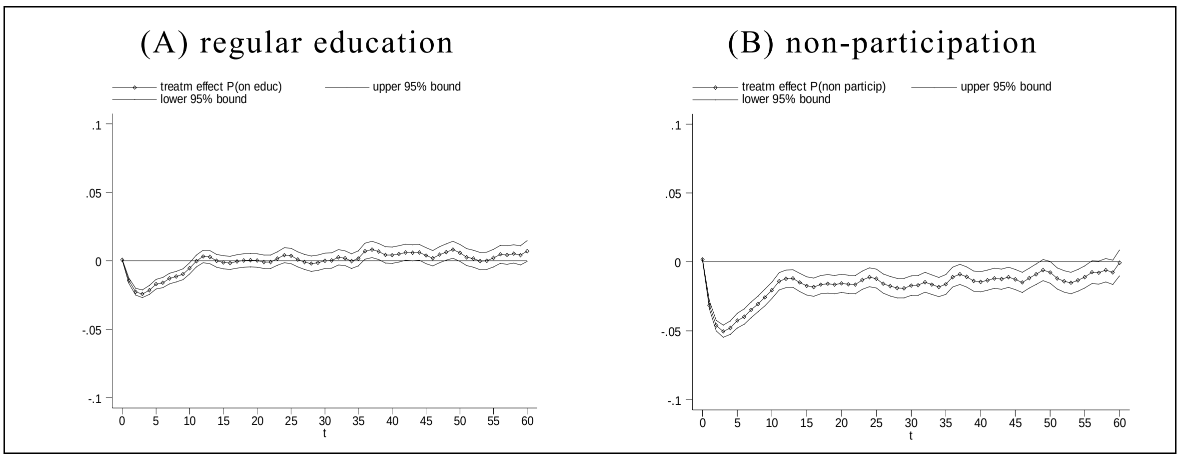
This conclusion is not however supported when assessing a different type of outcome: the probability of being out of the unemployment system. This outcome encompasses the previous one – the probability of being employed – but it also considers as a 'success' other reasons for being de-registered, given by having gone on regular education, having left the labour force or having been de-registered because of 'contact ended'. What we know about people being de-registered is that they are somewhere 'out there', in the labour market, out of the labour force, on other education – in any case out of the official unemployment system and certainly not claiming benefits.

When considering this type of outcome, programmes do not seem to be beneficial. Much to the contrary, the initial sizeable negative lock-in effect is gradually reduced in size, still the negative programme effect persists up to the end of the 3rd year since joining the programme (see Figure 5.3B).

In order to shed more light on these two contradictory results, we next look at the programme effect on the various labour market states that make up the 'out of the unemployment system' one.

Let us begin by looking at the probability of studies in the regular education system. If programmes enhance participants' human capital, they may find it easier to accumulate further human capital and may decide to deepen or specialise the acquired knowledge. So do programmes lead to further educational investments? The answer from the Figure 5.6 is that joining programmes does not seem to have any statistically significant effect on education rates beyond the initial negative lock-in impact. Participants are no more likely to invest in further education than comparable individuals who have postponed their participation decision. By contrast, joining seems to have a significantly negative effect on non-participation rates[50], which persists up to 5 years after the joining decision.

Figure 5.6 Treatment effect on the probability of being on

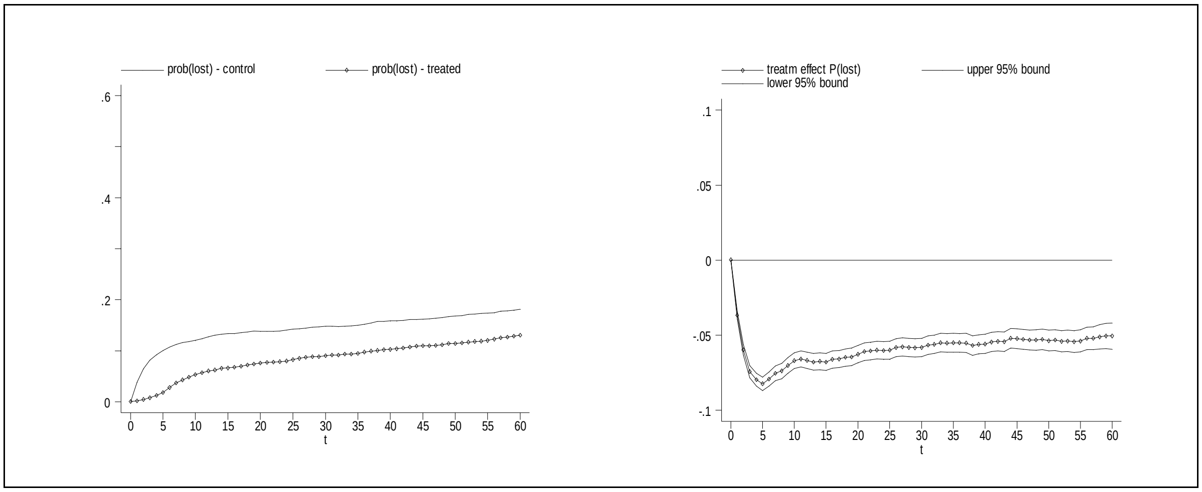


This is however a small treatment effect, so that the suspicion arises that the divergent impact on employment rates and on de-registration/employment rates may in fact be due to a negative programme impact on the last type of registration, the 'lost' status. In the following, 'lost' refers to an individual spell following de-registration, the reason of which has been recorded as 'contact ended'. This happens when a registered unemployed individual, having first missed an appointment at the official employment office, subsequently fails to

[50] Note that non-participation includes education in the regular system.

contact the agency within a week. In fact, the negative programme effect on 'lost' rates is decidedly large (Figure 5.7).

Figure 5.7 Lost probability for treated and controls over time and corresponding treatment effect



The problem of the 'lost' individuals is a serious ones; in fact, it prevents us from fully observing the outcome of interest, that is the true labour market status these individuals find themselves in. We do not know which of these spells is in reality an employment spell the former unemployed did not bother to report to the agency, and which is by contrast still part of the preceding unemployment spell. It has to be said that many of these lost spells are followed by unemployment spells, but even when over half of the lost spells observed in our data are preceded as well as followed by unemployment spells, one should not be entitled to infer that the individual had in reality remain unemployed all the time, since he might have found a job without reporting it and then lost it again.

Bring and Carling (2000), who have tried to trace back a sample of 'lost' individuals, have found that around half of them had in fact found a job, which highlights how severely under-reported employment status is in the available data. The large negative programme effect on 'lost' rates would thus turn out to be in part a large negative effect on employment rates.

In conclusion, the above evidence for a positive programme impact on employment probability needs to be carefully checked against these lost spells. We have started some further investigation using various sensitivity analysis, bounds and imputation techniques, the results of which are sketched in the next section.

5.2.4 Trying to account for a partially unobserved outcome variable

The analyses of the preceding section have clearly shown that the uncovered evidence for a positive programme impact on employment probability may not necessarily be robust to the presence of the lost spells. This section presents some selected results of the investigations performed in this direction, leaving it to future work to thoroughly address the issue of the 'lost' individuals for all the types of analysis performed in the whole of Section 5.

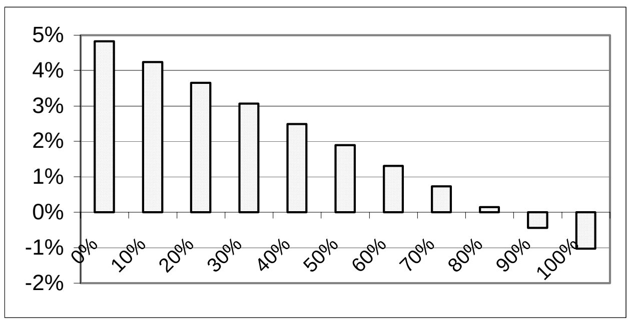
In the following, define Y_i to be a dummy variable equal to 1 if the individual is in employment (at a given time) and 0 otherwise. For simplicity of exposition, let us abstract from time and from the two groups (the calculations have obviously been performed separately for each group and for each point in time). Let L be a dummy variable indicating the 'lost' state, and D the usual treatment indicator.

A very simple sensitivity analysis without any additional external information looks at the estimated effects on employment rates under various assumptions about the percentage of 'lost' individuals who have in reality found a job. A 'misclassification' rate of 0% would thus mean that the observed employment rates (thus the effect on employment probability in Figure 5.5) are the true ones, while at the other extreme a 100% misclassification rate would imply that it is the sum of the observed employment rates and lost rates that represents the true employment rate. Note that this analysis assumes that the probability of being misclassified is the same for lost treated and lost controls, that is that outcome data Y are missing completely at random:

$$P(Y{=}1 \mid L{=}1, D{=}1) = P(Y{=}1 \mid L{=}1, D{=}0).$$

Figure 5.8 confirms that the observed average employment effect (4.8%) would in fact decline with more lost individuals having in reality found a job. With the almost 50% misclassification rate found in the survey by Bring and Carling (2000), it would be more than halved. Still, to have the effect disappear or change sign, one would need to assume that 80% or more of the lost individuals had in reality found a job.

Figure 5.8 Average treatment effects on employment probability (averaged over the 5-year horizon since start of the programme) by misclassification rate



A second step makes use of external information provided by the follow-up survey by Bring and Carling (2000) to impute to each 'lost' individual spell the probability of it in reality being an employment spell. Unfortunately, the *X*'s used by these authors do not include previous programme participation.[51] We thus need to assume that the misclassification probability is independent of treatment status, this time however given observables *X*, in other words, that the outcome data *Y* is missing at random:

$$P(Y{=}1 \mid X{=}x, L{=}1, D{=}1) = \mathrm{P(Y{=}1 \mid X{=}x, L{=}1, D{=}0)}$$

Using Bring and Carling (2000, Table 4) $\hat{\beta}$ coefficient estimates, the conditional probability of misclassification of a given lost individual with observed characteristics *X* is estimated by:

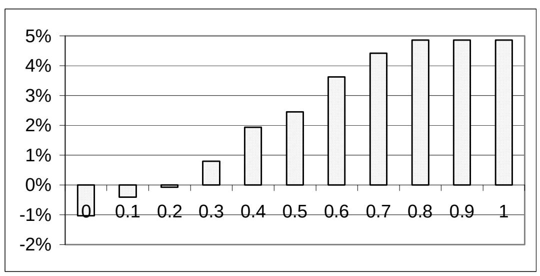
[51] Regressors include age group, gender, foreign status, human capital indicators (work experience, education), city region, and a few age-human capital interaction terms. Implicitly, we are also conditioning on non-entitlement: being registered is a prerequisite for drawing benefits, and in fact none of the lost spells in our data is characterised by unexpired eligibility.

$$\hat{p}_i^Y \equiv \hat{P}(Y_i = 1 | L_i = 1, X = x_i) = \left(1 + e^{-\hat{\beta}' x_i}\right)^{-1}$$

Two alternative strategies are then pursued.

We decide that a given lost individual has in reality found a job if his misclassification probability is larger than a given cutoff τ, that is if $\hat{p}_i^Y > \tau$ we consider that lost spell as an employment spell. The analysis of the treatment effect on employment probability is then performed as in Section 5.2.3 for various cutoffs τ.[52] Figure 5.9 – strikingly similar to Figure 5.8 – summarises the corresponding average employment effects; in fact if can be seen that a positive effect persists up to a cutoff as low as 30%.

Figure 5.9 Average Employment Effect by Cut-Off Probability (averaged over the 5-year horizon since start of the programme)



An alternative possibility is to count a lost individual with an (estimated) misclassification probability $\hat{p}_i^Y$ as a ($\hat{p}_i^Y$)th of an employed individual. In fact, simple calculations show that we can estimate the employment rate (separately for the treated group and the control group and at a given time period) as[53]:

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[52] Note that a cutoff of 0 corresponds to a 100% misclassification rate, while a cutoff of 1 to a 0% misclassification rate.

[53] The object of interest is the employment probability (or employment rate) for a given group at a given time, $P(Y=1)$, which can be written as: $P(Y=1) = \sum_x P(Y=1 | X=x) P(X=x)$.

$P(X=x)$ can be estimated by $\#\{X=x\}/N$, where $\#\{A\}$ denotes the number of elements in set A and N is the total number of individuals in the group being considered. We thus focus on $P(Y=1|X=x)$, which can be decomposed as:

$$P(Y=1|X=x)=P(Y=1|X=x,L=0)P(L=0|X=x)+ P(Y=1|X=x,L=1)P(L=1|X=x).$$

In our data we observe all terms except $P(Y=1|X=x,L=1)$, for which we use the estimated probability that a 'lost' individual with characteristics X has in reality found a job. That is: $P(Y=1|X=x,L=1)$ is estimated by $\hat{p}_i^Y$; $P(L=l|X=x)$ by $\#\{X=x,L=l\}/\#\{X=x\}$ for $l=0,1$; and

$$\hat{P}(Y=1)=\frac{1}{N}\left(\sum_{i\in\{L=0\}}Y_i+\sum_{i\in\{L=1\}}\hat{p}_i^Y\right)$$

where N is the total number of individuals (in the group and time period under consideration). The resulting dynamic treatment effect on employment is plotted in Figure 5.10 below. Even though visibly reduced from the observed one, joining a programme seems to still have a long-lasting positive impact on employment rates over time, compared to similar individuals who have decided to wait longer.

In these last two types of analyses, we have used the imputed misclassification probability to estimate the employment probability of a lost individual irrespective of his treatment status – a regressor not included in the estimation by Bring and Carling (2000).[54] This amounts to assuming that for a given set of the X's, the distribution of the probability that a lost individual has in reality found a job is the same in the treated and non-treated groups. In our case, treated individuals are those observed to enter a programme, while all we know about non-treated individuals is that they not necessarily do so, making it not easy to argue if such an assumption is likely to be systematically violated, and if yes, in which direction. Still, since we are looking at outcome measures (probabilities or rates) which are bounded, we can apply the core idea of the literature on non-parametric bounds in the presence of missing data (see e.g. Manski, 1990) and exploit the additional information from the survey to derive worst- and best-case bounds for the treatment effect of employment rates.

We start by writing the conditional misclassification probability of lost individuals with characteristics X as:

$$P(Y{=}1 \mid X{=}x, L{=}1) = P(Y{=}1 \mid X{=}x, L{=}1, D{=}1)\, P(D{=}1 \mid X{=}x, L{=}1) + \\ + P(Y{=}1 \mid X{=}x, L{=}1, D{=}0)\, [1-P(D{=}1 \mid X{=}x, L{=}1)]$$

$P(Y{=}1|X{=}x,L{=}0)$ by $\sum_{i\in\{X=x,L=0\}}Y_i \,/\, \#\{X{=}x,L{=}0\}$. Simplifying and integrating out the X's finally yields the formula in the main text.

For each lost individual, we know:

- his treatment status $D \in \{0,1\}$
- his treatment probability given the lost status, $P(D=1 \mid X_i, L=1) \equiv e_i$ [55]
- his misclassification probability $P(Y_i=1 \mid X_i, L_i=1) \equiv \hat{p}_i^Y$

The procedure to derive worst- and best-case bounds (where worst or best are from the point of view of programme effectiveness) consists in assigning $\pi_i \equiv P(Y_i=1 \mid X_i, L_i=1, D=d_i)$ by setting $\overline{\pi}_i \equiv P(Y_i=1 \mid X_i, L_i=1, D=1-d_i)$ to its maximum or minimum, compatible with the given $\hat{p}_i^Y$ and e_i, as well as with all probabilities $P(\bullet) \in [0,1]$. Table 5.2 displays the setting of $\overline{\pi}_i$ and the corresponding computation of π_i for the various cases, while the resulting bounds on the treatment effect on employment rates over time are shown in Figure 5.10.

As expected, the treatment effect under the best-case scenario far surmounts the observed one, with the joining decision paying off in terms of a sizeable and increasing extra chance of being in employment over time. In fact, while the observed treatment effect soon stabilises at around 6%, the favourable bound keeps rising, reaching double a level (12%) five years after programme start. Quite interestingly, the upper bound on the employment effect is in fact always larger than the observed one in *absolute* size, entailing a larger lock-in effect

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[54] Other important information regarding the unemployment spell prior to attrition is similarly missing, in particular the duration of the unemployment spell prior to attrition or the number of times the individual has registered as unemployed before attrition.

[55] Due to the absence of a 'standard' $D=0$ control group, the probability that a lost spell with characteristics X belongs to a treated as opposed to a 'non-treated' individual has been estimated separately by month of entry. In particular, for a given treated i, e_i is the estimated probability that a lost spell with characteristics X_i belongs to a treated individual as opposed to an individual who was still unemployed when treated i joined the programme. An individual j who is used (possibly repeatedly) as control for a treated entering in month m_1 starts being evaluated from m_1 and if he has lost spells, the corresponding employment probability bounds are calculated using the probability that a lost spell with his characteristics X_j belongs to an individual treated in month m_1 as opposed to an individual who was still unemployed after m_1 months. If this same individuals j also acts as control for another treated entering in month m_2, he counts as another person whose outcome starts being evaluated from m_2 and whose lost spells are evaluated using bounds on the probability that a lost spell with his characteristics X_j belongs to an individual treated in month m_2 as opposed to an individual who was still unemployed after m_2 months.

during the first five months. Similarly, the figure confirms the expectation of a dynamic worst-case-bound treatment effect considerably lower than the observed one, with the former ranging between between -3 and 0 percentage points after the lock-in phase. Overall, the impression from the graph is that one may need to invoke assumptions particularly unfavourable to the treatment in order to have the treatment effect disappear or reverse sign.

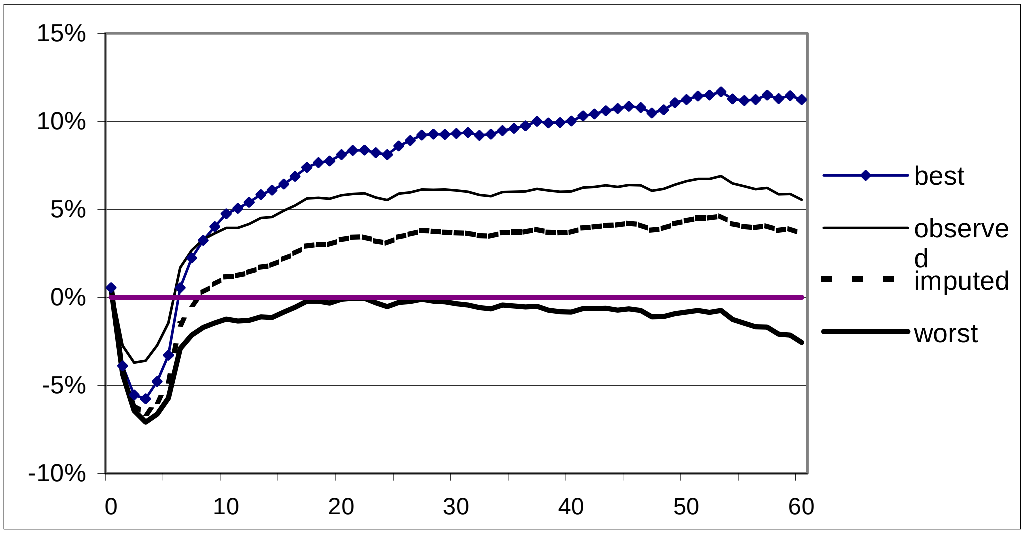
Table 5.2 Computation of π_i to derive worst- and best-case bounds

Worst-Case Scenario			Best-Case Scenario		
Treated assign the highest possible $\bar{\pi}_i$ compatible with $\hat{p}_i^Y$, e_i and $\pi_i \geq 0$			**Treated** assign the lowest possible $\bar{\pi}_i$ compatible with $\hat{p}_i^Y$, e_i and $\pi_i \leq 1$		
If	Set $\bar{\pi}_i$ =	Thus π_i =	If	Set $\bar{\pi}_i$ =	Thus π_i =
$\hat{p}_i^Y \leq 1-e_i$	$\hat{p}_i^Y/(1-e_i)$	0	$\hat{p}_i^Y \geq e_i$	$(\hat{p}_i^Y-e_i)/(1-e_i)$	1
$\hat{p}_i^Y > 1-e_i$	1	$(\hat{p}_i^Y+e_i-1)/e_i$	$\hat{p}_i^Y < e_i$	0	$\hat{p}_i^Y/e_i$
Controls assign the lowest possible $\bar{\pi}_i$ compatible with $\hat{p}_i^Y$, e_i and $\pi_i \leq 1$			**Controls** assign the highest possible $\bar{\pi}_i$ compatible with $\hat{p}_i^Y$, e_i and $\pi_i \geq 0$		
If	Set $\bar{\pi}_i$ =	Thus π_i =	If	Set $\bar{\pi}_i$ =	Thus π_i =
$\hat{p}_i^Y \geq 1-e_i$	$(\hat{p}_i^Y+e_i-1)/e_i$	1	$\hat{p}_i^Y \leq e_i$	$\hat{p}_i^Y/e_i$	0
$\hat{p}_i^Y < 1-e_i$	0	$\hat{p}_i^Y/(1-e_i)$	$\hat{p}_i^Y > e_i$	1	$(\hat{p}_i^Y-e_i)/(1-e_i)$

All the analyses in this section were meant to offer some qualitative[56] evidence as to the robustness of the uncovered positive employment effect with respect to the problem of the lost individuals. Overall, these findings would seem to indicate that the effect of participating in a programme compared to postponing such a decision may remain positive under a variety of assumptions.

[56] No standard errors have yet been derived to inform about the significance of the effects.

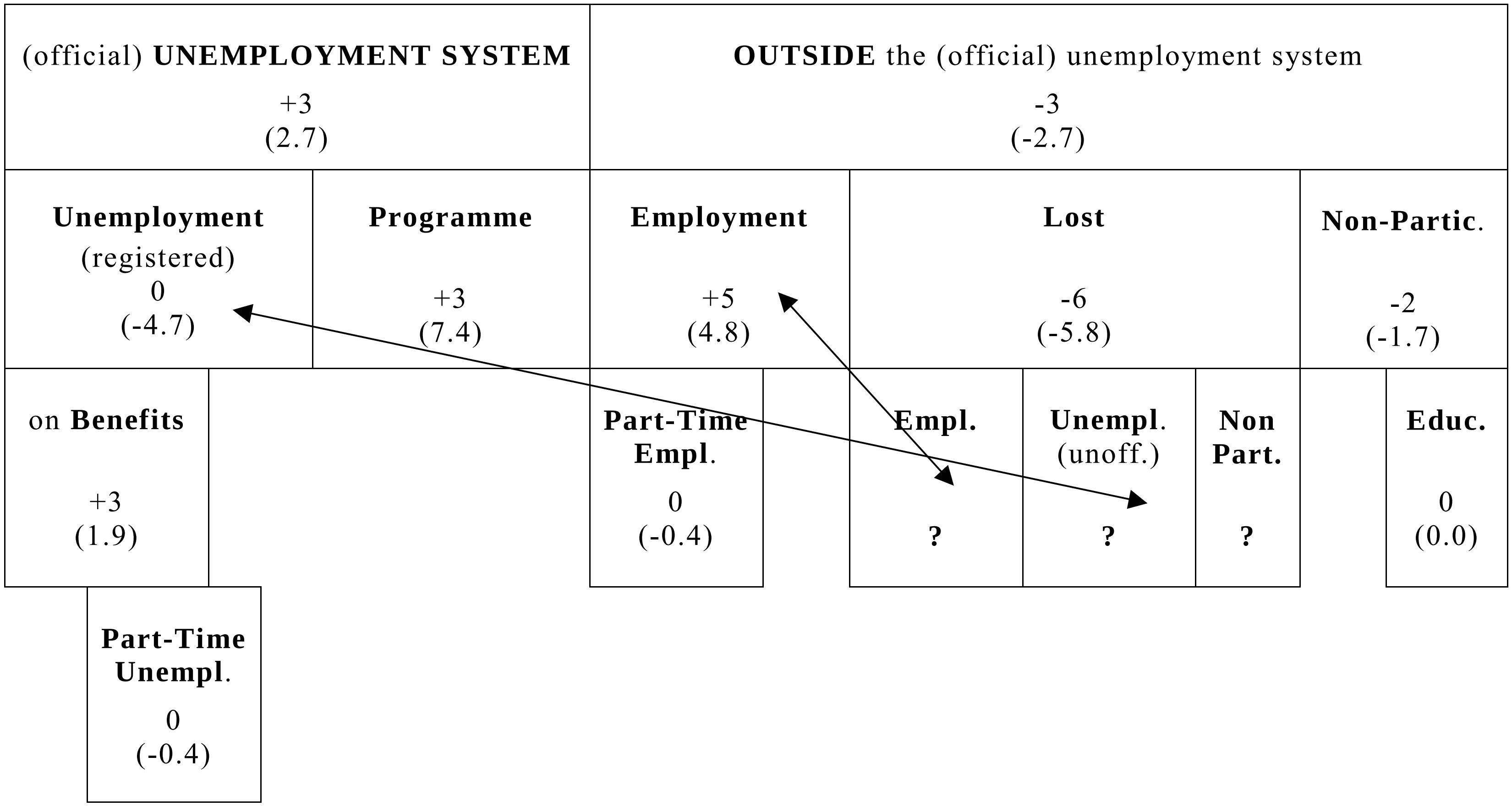
Figure 5.10 Treatment effect on employment probability, using observed employment rates, imputed employment rates, worst-case and best-case bounds



5.2.5 A summary so far

Figure 5.11 organises and summarises the findings obtained so far concerning the treatment effects on the various labour market states considered. It reports both the monthly treatment effects averaged over the 5-year period from entry into the programme and an indication of the more permanent, long-term effects informally gauged from the graphs after 4-5 months from programme entry. The figure clearly highlights the importance of the true nature of the 'lost' state to truthfully reflect the treatment impact on *actual* (as opposed to as observed by the employment offices) employment. Some conclusions can however be drawn as to the treatment effect on unemployment. A large positive effect on compensated unemployment, together with the absence of an effect on (registered) unemployment, would entail a negative effect on uncompensated unemployment, which is likely to be further reinforced by the observed negative effect on 'lost' rates (note that 'lost' individuals who are in reality unemployed cannot be drawing compensation).

Figure 5.11 A summary so far: Average treatment effects on various labour market states (%)



Note:

- Bracketed figures are the treatment effect averaged over the 5-year horizon since the start of the programme (cf. second column of Table 5.6);
- Non-bracketed figures refer to around 4-5 months after programme start and informally gauge more long-term stable effects.

Thus, focussing on registered unemployment, what programmes seem to affect is merely the *type* of the unemployment spell experienced, in particular the treatment 'swaps' uncompensated unemployment for compensated one.

Broadening the scope to consider the effect on the 'true' unemployment rate (i.e. both official and un-registered, the latter as experienced by the 'lost' individuals), the treatment is likely to reduce it overall, achieving this by reducing the probability of uncompensated unemployment only (in fact, by more that it increases the probability of compensated unemployment).

Finally, though, one should bear in mind that the treated also have a sizeably larger long-term probability of being on programmes than comparable individuals who have postponed their participation decision, so that the treated remain significantly more likely to be registered at an unemployment office over time.

To roughly fix ideas, let us assume that, conditional on the X's, the employment status of the lost individuals is missing at random for those who join and those who wait longer, so that we can imputed their employment probability. Let us further abstract from the non-participation state (a rather strong assumption for the lost individuals). These two assumptions allow us to completely 'attribute' the observed treatment effect on lost rates partly to the observed effect on employment and partly on unemployment rates. We further focus on that treatment effect which after 4-5 months (thus ignoring the initial lock-in effect and the endogenous duration of the treatment) seems more stable and permanent in nature.

The figures resulting from this series of simplifying assumptions are presented in Table 5.3, where '*de facto*' (as opposed to 'observed') highlights that we are trying to account for the lost spells. Joining a programme would thus make participants on average 3 percentage points more likely to be in employment over time, compared to similar individuals who have postponed their participation decision. However, participants would also be 3 percentage points more likely to be in compensated unemployment and another 3 percentage points more likely to be on programmes over time. Overall, participants would thus have on average a 9% lower probability of being in an unemployment spell not supported by benefits and not registered either. Under the various assumptions which allow such calculations, joining a programme compared to waiting 'longer' would seem to 'swap' uncompensated, unregistered unemployment not only for compensated unemployment as well as for further programme participation, but also for employment in the regular labour market.

Table 5.3 A crude summary of the effect of participation *versus* waiting: The more permanent treatment effect on the probability of being in the various states, 6 months after joining a programme

de facto unemployed		on programmes [a]	*de facto* employed [b]
compensated [a]	non-compensated [c]		
+ 3%	– 9%	+ 3%	+ 3%

Notes: Non-participation state is ignored
[a] The more long-term effect is informally gauged from Figures 5.2 and 5.4.
[b] Employment probability is imputed for the lost individuals (i.e. assuming missing at random); the more long-term effect is then informally gauged from Figure 5.10.
[c] As implied by the other calculations.

Treatment effects by month of placement

Further interesting insights are gleaned when looking at the various outcomes for different sub-groups of the treated based on the time they have spent in unemployment before being placed on a programme.

Looking at the set of graphs by placement month (not shown) reveals that the treatment effects for individuals joining a programme earlier on – around the third month in unemployment – are considerably better, both of those for 1st-month joiners and especially of those for individuals placed on a programme in their 14th month in unemployment – i.e. just around the time benefits expire. This can easily be appreciated from Table 5.4, which summarises the treatment effect over time in a single indicator, by taking the average over the 5-year horizon.

Table 5.4 Average treatment effects (over the 5-year horizon since the start of the programme) by month of placement into the programme (%)

Rates/Probabilities (% points)	Placement in x^{th} month: 1 to 18	1	3	6	14	18
Employment (observed)	4.8	1.3	4.8	4.7	2.3	3.1
Deregistered | employment	-2.7	-2.7	-2.7	-3.5	-6.4	-7.3
Lost	-5.8	-4.7	-7.0	-4.8	-4.4	-6.1
Non-Participation	-1.7	0.8	-0.5	-3.3	-4.4	-4.3
Education	0.0	0.9	0.7	-0.8	-0.3	0.5
On Programmes	7.4	7.2	6.9	8.3	8.2	12.2
Unemployment	-4.7	-4.5	-4.2	-4.8	-1.8	-4.9
Part-Time Unemployment	-0.4	-0.0	-0.2	-0.2	-1.5	-4.8
Benefit receipt	1.9	1.5	1.3	1.9	5.2	3.3

5.2.6 Job accession

When focusing on employment, we have thus far been considering employment rates over time. The probability of being employed at a given point in time summarises both potential effects on job finding rates (including the lock-in effect) and on job attachment once an occupation has been found.

The next two types of analysis try to assess how these rates originate by looking at the two components of the employment effect in more detail.

As it should be clear from the methodological discussion in Section 5.1, the three types of analysis are not directly comparable. The analysis expounded in Section 5.2.3 as well as the one presented in this section address more of a timing question concerning the impact of joining a programme *versus* waiting longer in open unemployment for individuals registering for their first time as unemployed, while the analysis of job attachment in Section 5.3.1 not only focuses on a different question, i.e. the effectiveness of programme participation compared to *actual* non-participation, but does so for a special *sub-sample* of the initially unemployed individuals. Finally, the following two analyses consider a *continuous* spell (of unemployment and of employment, respectively) only, while employment rates pertain to the status of individuals over the entire observation period and irrespective of any interruption.

Remaining unemployed for a long time can potentially be harmful in several ways; in addition to the sizeable social outlay for benefits needed to finance prolonged unemployment spells, the unemployed individuals' human capital may quickly deteriorate, so that the jobs they may remain suited for will become less and less skilled and qualified; they may gradually lose hope and motivation, reducing their search intensity and possibly suffering increasing psychological costs. Finally, there may be a stigma effect associated to unemployment – as well as, possibly, to lengthy programme participation –, so that it may be important to find a job rather quickly. Such a stigma effect from both unemployment and programme participation has in fact been documented by Agell and Lundborg (1999). Their 1998 survey of employers shows that 27% and 21% of the interviewed managers regard prolonged unemployment duration and programme participation, respectively, as a *strong* signal of low productivity.[57] Even participation in the most sought-after programme and the one most likely to raise human capital and thus productivity, labour market training, is viewed by 14% of employers as a strong indicator of low productivity.

A very relevant question in such a context is thus whether individuals who have joined a programme find a job easier than if they had waited longer.

In order to assess whether joining helps participants find a job sooner, we consider their survivor function in unemployment (possibly including post-treatment subsequent programme participation) compared to what it would have been had they not entered the programme when they did.

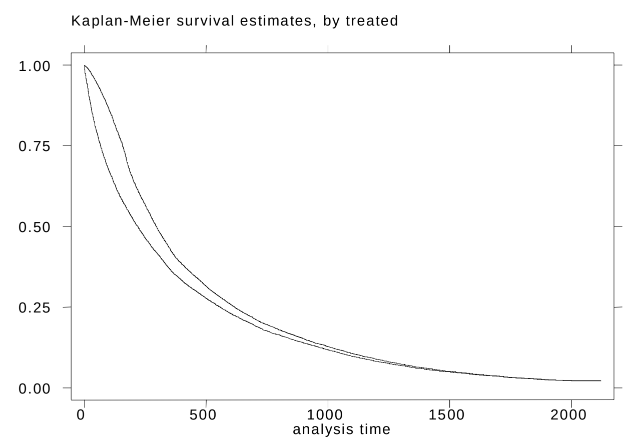
As in the previous analysis, unemployment duration for the treated is measured from their entry into the programme, for controls from the moment their respective matched participants joined the programme.

[57] On a 1 to 9 scale, strong means a score of a least 7.

In the following, when considering unemployment duration we mean an uninterrupted spell, possibly made up of various sub-spells where the individuals are either registered as unemployed, as part-time unemployed or taking part in a programme. Such a wider 'unemployment' spell thus ends either in employment, or in de-registration for other reasons (in this paper, this includes 'lost', an issue that will be addressed in future work), or else is censored on the last observation day. Again note that this spell may include further programme participation; subsequent programme participation – including the possibility of starting a cycle between unemployment and programmes – is regarded as an outcome of the treatment, defined as the *first* programme joining decision.

The survival functions for the two groups plotted in Figure 5.12 are significantly different, with participants expected to remain unemployed for 2 months or 15% longer than if they had postponed their participation decision.

Figure 5.12 Survival function in unemployment (days) for treated and controls

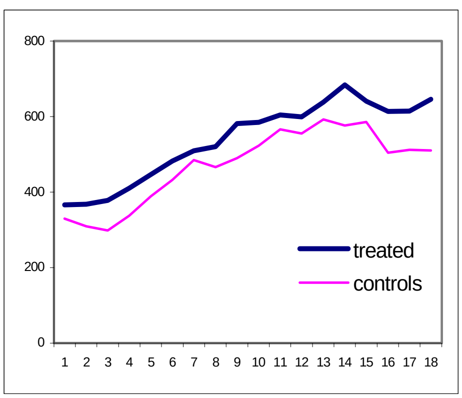


Notes:Test for equality of survivor functions: Pr>chi2=0.0000.
Expected unemployment duration: treated – controls = 468 – 406 = 62 days.

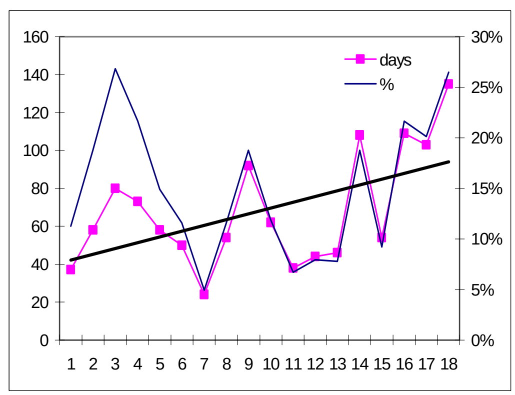
Looking at heterogeneous treatment effects on subsequent unemployment duration by month of placement into the programme, reveals some interesting patterns. First of all, the Figure 5.13A shows that the longer an individual has already been unemployed, the longer his remaining expected unemployment duration is going to be, and this irrespective of treatment status. Treated individuals, though, seem to remain unemployed for longer than comparable non-treated. What's more, the magnitude of the impact varies greatly according to how long the participants have been unemployed before joining the programme (Figure 5.13B). Roughly speaking, for individuals joining programmes earlier on the unfavourable treatment effect tends to be smaller in magnitude. For instance, those participants joining a programme in their first month subsequently remain unemployed for roughly one month longer than if they had waited longer, on average; for those joining the programme in their 14^{th} month in unemployment the extra unemployment duration is over 3 months.

Figure 5.13

(A) Average expected unemployment duration (days) for treated and controls, by month of placement



(B) Absolute (days) and relative (%) average treatment effect on unemployment duration, by month of placement



5.3 The effect of participation versus actual non-participation

5.3.1 Job attachment

We have thus seen that joining programmes does not seem to be more effective, on average, in getting participants into employment, compared to a situation where they would have waited further in open unemployment and availed themselves 'full-time' of the benchmark treatment represented by job placement.

Quite to the contrary, joining a programme appears to considerably prolong the overall time spent in unemployment (including further programmes) by the treated.

This might however be due to a reduced job search while taking part in the programme (the lock-in effect that has always been part of the treatment effect), as well as to the possibility that programmes increase participants' productivity and thus reservation wages. After completion of the programme participants may feel that they have enhanced their human capital and thus feel allowed to be more 'choosy' as to the level of jobs they would consider accepting. Programmes may in fact prevent individuals from taking the first low-skilled, dead-end or temporary job they come across, which they might soon lose or quit anyway (and thus be possibly back crowding the employment offices).

These considerations suggest the possibility that programmes may not necessarily be very effective in getting participants into regular employment *quickly*, to the contrary they may indeed prolong the overall time spent as registered job-seekers. In the light of the discussion in Section 2, there may be in fact the presumption that it is by contrast the benchmark treatment in terms of job *search* assistance and enhancement offered by the employment offices (and to which the controls have been exposed for longer) that may be more effective in getting unemployed individuals back into work faster. However, programmes may well endow unemployed workers with new working skills, enhanced productivity, additional work experience and improved working habits, which, once having the chance of being revealed on a job, may help former participants to remain employed longer. It could thus be that programmes may take more longer than full-time job placement but get participants into good (i.e. lasting) jobs rather than getting them quickly into short-lived ones.

Since the stated objective of the Swedish active labour market policy is to reduce the registration periods at the unemployment offices, focussing on the impact of the programmes on the exit rate from unemployment ignoring what happens once in employment would provide only a limited assessment of their effectiveness.

This section tries to address the question of whether individuals who have actually participated in a programme *and* found a job stick to it longer than if they had not participated (and had found a job).

Methodology

In order to analyse the effect of the programmes on how stable and long-lasting the jobs found by participants turn out to be, we look at the survival function of the *first* employment spell for those *who have found employment.*

The first step is thus to select the relevant sub-groups from the initial unemployed population. The non-treated sub-pool is made up of individuals experiencing an unemployment spell followed by an employment spell: we keep as potential controls all the UE-individuals (39,750 of them). The treated sub-group is composed of UP(U)E-individuals, that is of those treated who directly go from the programme to employment (UPE; 5,180 of them) and of those treated who resume their unemployment spell after the programme, after which they however find employment (UPUE; another 5,300 of them).

We then estimate the propensity score on these two groups, giving the probability of belonging to the UP(U)E group. A crucially important regressor to include in estimation is the unemployment duration (of the complete first spell for non-treated UE-individuals and for the UPE-individuals and of the sum of the two U spells for the UPUE-individuals).[58]

Another set of crucial new variables to condition on is the type of employment found on exit from unemployment: employment by the former employer[59],

[58] This variable has been included together with its main interaction terms and a quadratic.

[59] If some of our UE individuals have not gone on a programme because they *knew* they would be recalled *and* if recalled employment is a systematically different type of employment from non-recalled employment, say because it typically consists of seasonal employment, then UE recalled individuals' employment spell may be bound from the start to be shorter. This would lead us to overestimate programme effects on survival in employment for those who have found a job. Conditioning on recalled employment, though not reliably recorded in the data (see the discussion in Section 5.2.2), should at least partly address this issue.

temporary employment (registered or not) and employment while still registered at the offices (i.e. temporary employment and employment while looking for a new job).

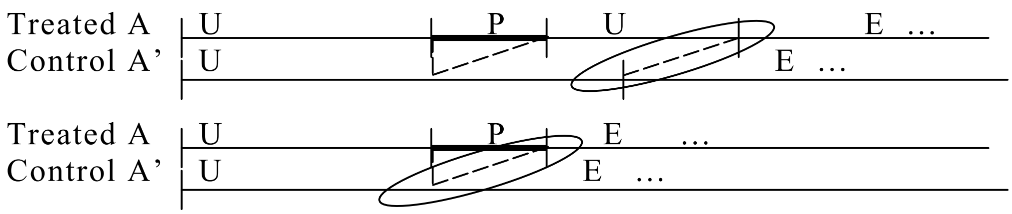
Matching on the score can balance unobserved characteristics only to the extent that they are correlated with the observed ones used in estimation. Observed unemployment duration (and to a lesser extent type of employment found) is therefore critical to this end, since matching on it should also eliminate any observable and unobservable heterogeneity in exit from unemployment.

The outlined procedure should also be robust to the possible existence of heterogeneous (according to observables) programme effects on job *finding* probabilities. It is important to note that, as typical with the matching approach, we are not able to match away a programme effect on job accession rates that is heterogeneous with respect to still uncaptured unobserved variables. If such unaccounted for variables also affect subsequent employment duration, the estimated programme effects on job retention would be biased. To illustrate this point, the impact of the programme on employment duration would be downward biased if the programme affected participants job finding rates, having a stronger impact, say, for low-ability individuals and if low-ability individuals remained on average employed for a shorter period.

Note that we are matching also on *post*-treatment variables; this is because we aim at selecting two observably similar groups at the onset of the employment spell, in particular we want to match away possibly heterogeneous programme effects on subsequent unemployment duration (i.e. job finding probability) as well as on the type of employment found, and we want these individuals to be as similar as possible with respect to all those variables which may influence employment duration, irrespective of whether such variables have themselves been affected by programme participation.

Our resulting two groups should thus be made up of individuals who, at the start of their employment spell, are as similar as possible in terms of their observed characteristics and, having experienced the same complete unemployment duration, possibly very similar in terms of unobservable characteristics which are thought to be relevant in terms of the outcome being evaluated – i.e. survival in employment. The sole remaining difference should ideally be that one group only has received the 'pill' of the programme (see Figure 5.13).[60]

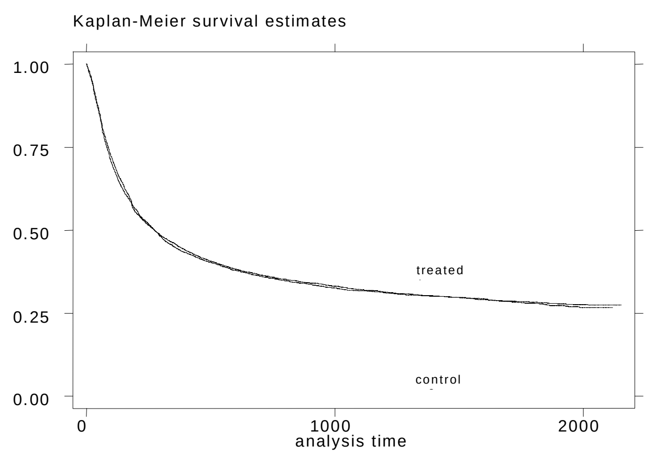
Figure 5.14 Matching for job attachment



Survival in Employment

We can now turn to the assessment of the programme effect on the outcome of interest here – the duration of employment for those who have found it. Figure 5.15 reveals that the programme has had absolutely no effect whatsoever.[61]

Figure 5.15 Survival function in employment (days) for treated and matched controls who find employment



[60] Matching quality information is presented in the Appendix, which shows that the critical variables total unemployment duration and type of employment have indeed been satisfactorily matched.

[61] Test for equality of survivor functions: chi2(1) = 0.30, Pr>chi2 = 0.5816;
Expected employment duration: treated – controls = 796 – 806 = -10 days.

In order to assess the importance of conditioning on unemployment duration, the same procedure has been carried out without including this variable. The results do change in fact quite dramatically, the two survivor functions being now statistically different (Pr>chi2=0.0001) and displaying a positive programme effect of 23 days (+3%). Assuming that the first specification is the 'correct' one, this finding confirms the crucial role of recent unemployment history found by Heckman, Ichimura and Todd (1997).

Heterogeneous Effects

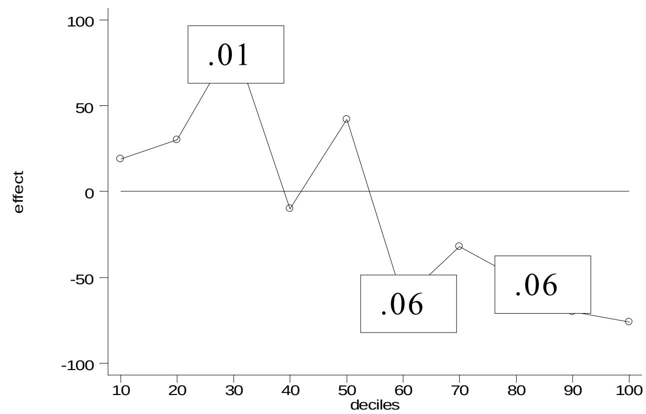
The whole analysis has been performed separately for various subgroups of the population of interest in order to investigate the possibility of heterogeneous programme effects on employment duration and to address the question of whether certain types of individuals stand a better chance than others to benefit from programme participation.

In fact, no significant differential impact according to the time the individual has spent in unemployment before entering the programme, entitlement status, human capital (i.e. education and experience), sex, citizenship or age could be detected. The only, albeit mild evidence of heterogeneous programme effects is with respect to the propensity score (and for the very very young; for details, see the Appendix). In particular, programmes appear to benefit most those with a low probability of participating, while they seem to harm those with a higher participation probability (see Figure 5.16).

Outcomes over Time

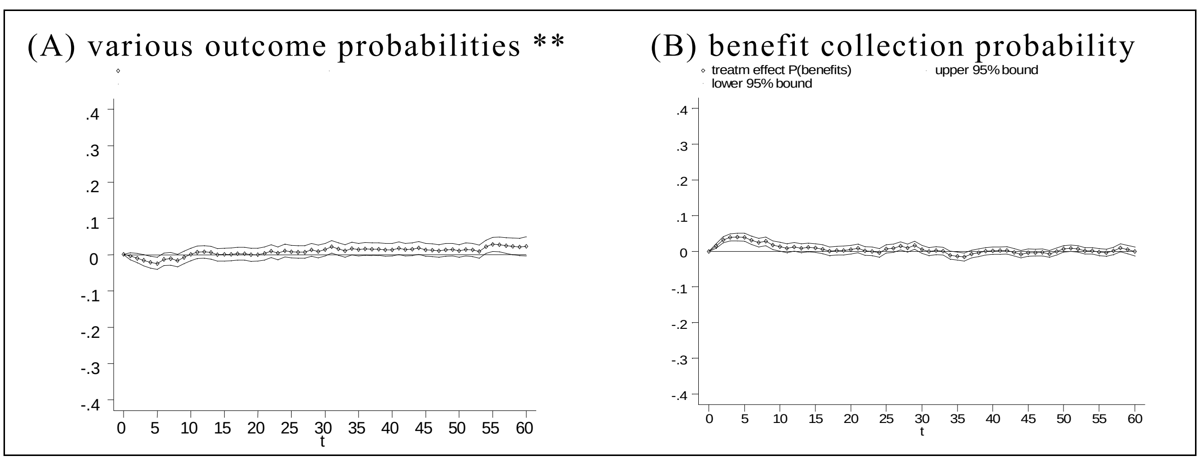
The analysis is finally extended by moving beyond survival in an uninterrupted employment spell and following these individuals for up to 60 months from the start of their employment spell (t=0 thus marks the start of employment). Figure 5.17A is representative of the treatment effects on the probability of being in various labour market states over time, showing a constant zero effect on employment probability[62], de-registration/employment probability, unemployment probability, programme participation probability, non-participation probability, education probability and part-time unemployment probability. Figure 5.17B shows that the only 'advantage' former participants have is in terms of a positive effect (of around 5%) for up to 10 months on benefit collection probability.

Figure 5.16 Treatment effect, in days, by score deciles



Note: for significant effects, the figures in the boxes denote the significance level.

Figure 5.17 Treatment effect over time on
(*t* months after entry into employment)



** Graphs virtually identical for probability of employment, of de-registration/employment, of unemployment, of programme participation, of non-participation, of regular education and of part-time unemployment.

[62] In contrast to survival in continuous employment, this outcome indicator would capture transitions to other jobs which are interrupted by (however short) unemployment spells.

5.3.2 Falling back into unemployment

So far we have seen that programmes, far from helping participants to find jobs sooner, keep them longer unemployed. And once participants do find a job, they do not seem to be able to retain it any longer than if they had not participated. Still, should they lose it (again), it could be that programmes have equipped them with skills – maybe just in terms of knowledge and taking advantage of the functioning of the unemployment system – which allow them to deal with subsequent unemployment more effectively.

This section tries to investigate whether those former participants who are observed to fall back into unemployment can deal with this condition more efficiently than if they had not been treated in their first unemployment experience. In particular, we examine whether the programme has taught them how to exit from unemployment faster, including availing themselves of programmes (the duration of which however counts in terms of the unemployment spell under examination.

Methodology

Matching is performed in the same spirit as it has been done in the previous type of analysis.

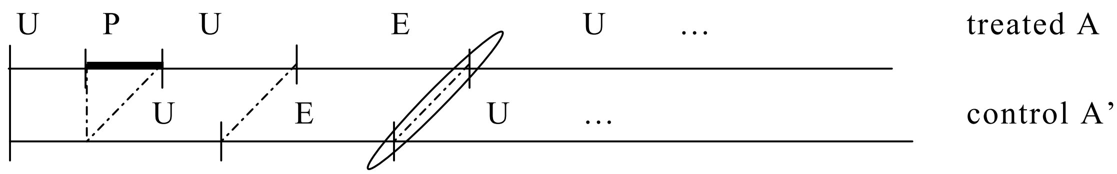
First, the two sub-groups of interest are selected: the non-treated are of the type UEU (22,900 of them), while the treated are the UP(U)EU individuals (over 6,000 of them). Secondly, the propensity score is estimated, this time including among the regressors the total unemployment duration before finding a job and the type of employment found (and lost), as well as the time spent in employment; and finally, matching is performed.[63]

Ideally, the procedure should ensure that at the beginning of their second unemployment occurrence, the group of treated and the group of matched controls have the same relevant observed as well as unobserved characteristics (the latter captured by conditioning on their labour market history, including the duration

[63] For information on matching quality, see the Appendix.

of their unemployment and employment spells). The two groups should differ only in that the treated alone have experienced a programme in their previous open unemployment spell (Figure 5.18).

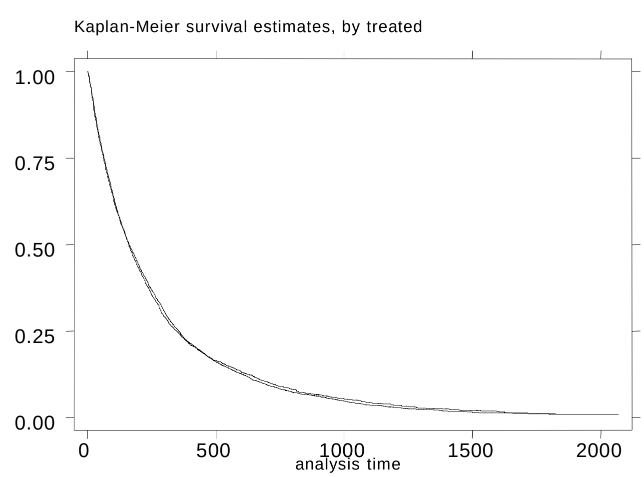
Figure 5.18 Matching for repeated unemployment



Survival in Unemployment

We are now in a position to look at the programme impact on continuous unemployment duration once fallen back into unemployment, where unemployment includes time possibly spent participating in programmes. As for job attachment, the survivor functions are indistinguishable (see Figure 5.19).[64]

Figure 5.19 Survival function in continuous unemployment (days) for treated and matched controls who fall back into unemployment

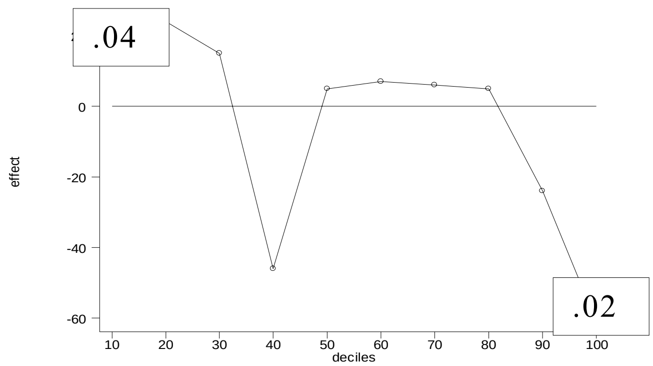


[64] Test for equality of survivor functions: chi2(1) = 0.00, Pr>chi2 = 0.9486;
Expected unemployment duration: treated – controls = 279 – 288 = -9 days.
Again, if one were to ignore unemployment and employment duration, the results would be strikingly different (chi2(1) = 19.81, Pr>chi2 = 0.0000).

Heterogeneous Effects

As for survival in employment, no significant differential programme impact has been found depending on the individuals' characteristics, with some heterogeneous effect being again confined to the participation probability (this time, however, it is individuals with a very low propensity score who suffer a harmful programme effect, while the reverse is true for individuals with a very high participation probability – see Figure 5.20, and, for more details, the Appendix).

Figure 5.20 Treatment effect, in days, by score deciles



Note: for significant effects, the figures in the boxes denote the significance level.

Outcomes over time

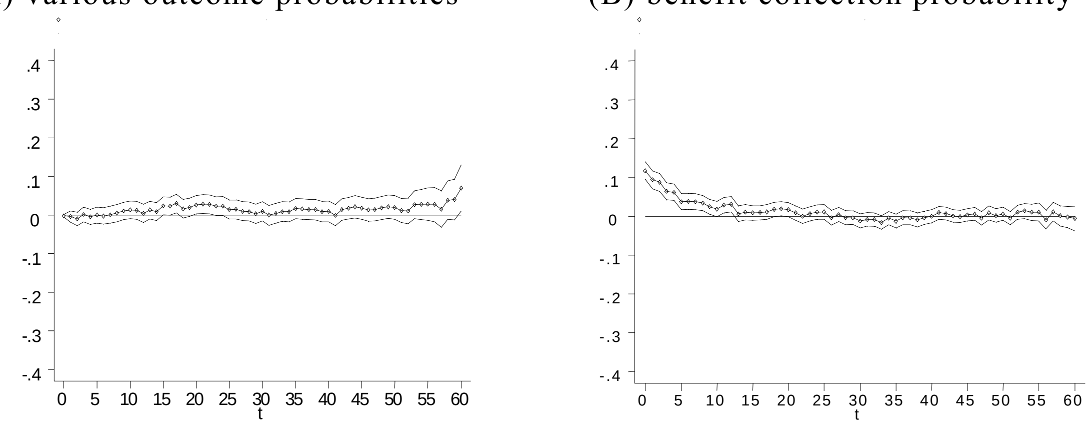
We now move beyond the survival in the new unemployment spell and look at the programme impact on the probability of being in various states over time. Time is as usually measured in months, and t=0 for when the two groups become (register as) unemployed again.

Figure 5.21A is representative of the treatment effects in terms of various outcomes over time: having participated in a programme in their first unemployment experience has given former participants no edge in exiting unemployment, nor are they significantly more likely to experience (repeated) programme participation than if they had never been on a programme before. There is no evidence whatsoever of any programme effect on employment rates, with the same applying to the other routes out of unemployment – being de-registered for other reasons, leaving the workforce or going on regular education.

While registered as unemployed, individuals can collect unemployment benefits. Figure 5.21B shows that at entry into the new unemployment spell, former participants are over 10 percentage points more likely to be actually receiving benefits. This positive programme effect on benefit collection rates gradually dwindles, remaining significant up to the 10^{th} month in the new unemployment spell. Since we have matched on unemployment duration (thus on benefit exhaustion), as well as on employment duration (thus on benefit renewability through the work condition), the initial higher likelihood of collecting benefits by the treated can be explained by their former participation in the programme, which has given them an edge towards renewing their benefits.

Figure 5.21 Treatment effect over time on
(*t* months after entry into new unemployment spell)

(A) various outcome probabilities ** (B) benefit collection probability



** Graphs virtually identical for probability of employment, of de-registration/employment, of unemployment, of programme participation, of non-participation, of regular education and of part-time unemployment.

In conclusion, having participated in a programme gives no particular advantage – nor disadvantage, it has to be said – in leaving unemployment should one fall back in it, the only really noticeable boon for former participants (from their point of view, probably not from the evaluator's one) being a short-lasting higher chance of collecting benefits at entry into the new unemployment spell.

6. Unemployment-programme cycling behaviour

As described in Section 2, a feature which is quite distinctive of the Swedish institutional system is that labour market programmes are intricately tied in with the unemployment insurance system, since participation in such programmes qualifies for new periods of unemployment compensation.

Thus while programmes may offer the possibility of enhancing the human capital of participants *in principle*, programmes *as a fact* serve as a vehicle to renew unemployment benefits, and could thus reinforce the work disincentive associated with the unemployment insurance system.

After a brief review of the relevant Swedish literature, this section both draws all the previous evidence together and looks at some further clues, all of which point to cycling behaviour as the most likely 'culprit' for most of the disappointing programme effects uncovered.

Previous Swedish evidence on the importance of issues relating to unemployment benefits, work disincentive effects and cycling behaviour would seem to favour such an interpretation. Agell and Lundborg (1999) describe a microsimulation based on the benefit and tax regulations (including housing allowances, social assistance and child care fees) in 1998, according to which 4% of the unemployed would actually gain nothing from finding employment, 38% would have a disposable income between 90 and 99% of their income if employed, and 36% would have a disposable income between 80 to 89% of their income in employment. Regnér (1997) provides some evidence that job-seekers may often have entered labour market training just to renew benefits; results by Carling, Edin, Harkman and Holmlund (1996) based on a competing risks model show that UI-entitled individuals close to benefit exhaustion are significantly more likely to exit their unemployment spell to a programme than those without unemployment compensation (cf. their Figure 3). They also uncover a UI work disincentive effect, though small in size. The small effect could partly be accounted for by their type of data, which do not record actual compensation received by the unemployed. In a subsequent study, Carling, Holmlund and Vejsin (1999) do in fact find a significant and large negative UI effect on job finding rates. Ackum Agell, Björklund and Harkman (1995) find that prolonged spells of benefit-programme periods are indeed common in Sweden, while Hägglund (2000) detects a very interesting sensitivity of employment duration as well as time spent on a programme to changes in the UI work requirement.

To investigate cycling behaviour in our data explicitly, let us first propose a working definition of a *cycle* as a chain of at least four alternating unemployment (U) – programme (P) spells, in symbols UPU**P**(..UP..). The programme spell in bold denotes the first spell in the cycle. In concrete terms, we allow an individual to be unemployed, to interrupt this spell by joining a programme and to then resume it. However, if he then enters a new programme, then we consider this his first spell in a cycle. Therefore a cycling programme spell is defined as P preceded by UPU, and a cycling unemployment spell as U preceded by PUP.

A *compensated cycle* is defined as a chain of the type described above, but where in *each* unemployment spell, including the one preceding the start of the cycle, the individual draws unemployment compensation (UI or KAS): UPU^c**P**$(..U^cP..)$.

Let us first of all give a rough idea of the quantitative importance of the phenomenon, both in terms of individuals involved and in terms of cycle length, for the individuals in our sample. Almost one in two (42%) of our 32,000 treated start cycling (from the treatment), and for more than half of them this is a compensated cycle. Over 1 in 3 of the compensated cycles has four or more switches, i.e. individuals registering as unemployed have an event history of the type $UPU^cPU^cPU^c$ or longer (with one quarter of them being censored).

Let us move beyond this descriptive analysis to look at the causal treatment effect of joining a programme (*versus* waiting longer) on the probability to be on a compensated cycle. Figure 6.1 and Table 6.1 show that programme participants have a rather high compensated cycling probability over time (8% on average over the 5-year horizon considered), which peaks around 15% and which still remains at 5% after 5 years from the start of the programme. What is however more interesting is to look at are the differential probabilities between participants and those who waited longer; the figure displays quite a sizeable treatment effect on the probability of being in an unemployment-benefit-compensated cycle over time (over the observation period, participants are on average 3.5 percentage points more likely to be in the midst of a compensated cyle), an effect which persists well up to 50 months after entry into the programme. The figures for the treatment effect on general cycling behaviour are obviously even larger.

Figure 6.1 Compensated cycle probability over time and corresponding treatment effect
Full group of treated (*t* months after entry into programme)

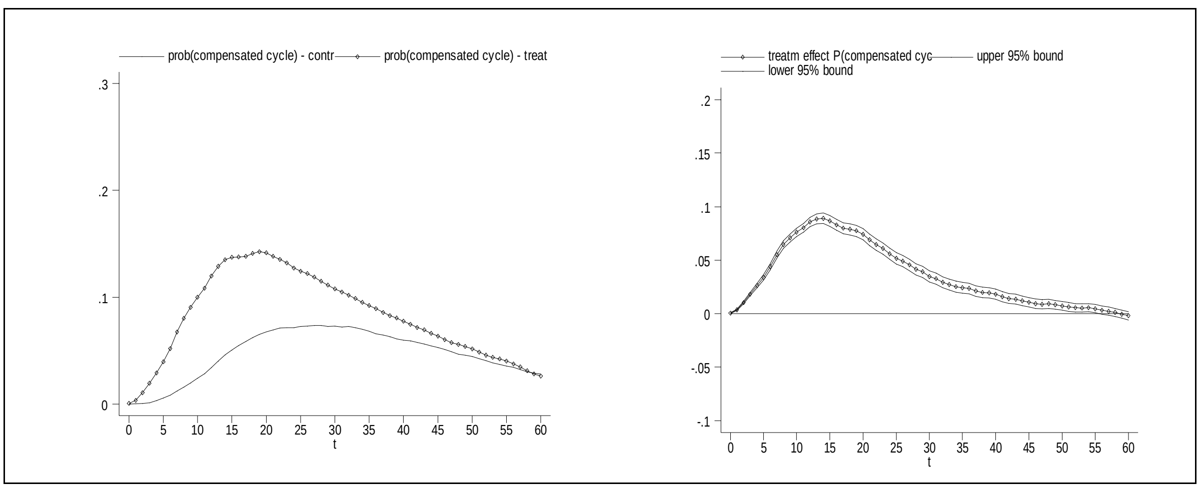


Table 6.1 Cycling and compensated cycling:
Average probabilities and average treatment effects
(over the 5-year horizon since the start of the programme)

	Treated	Controls	Effect
Cycle	12.8	7.6	5.2
Compensated Cycle	8.1	4.7	3.5

Cycling itself may be considered a worrying phenomenon for a number of reasons; the fact that unemployed individuals keep going on various programmes withouth exiting unemployment is clear evidence of a failure of the programme system itself, while the importance of compensated cycling behaviour points to a likely failure in the way incentives are taken into account by the intertwined UI-programme policy system. Entitlement to unemployment compensation, with the related issues of benefit exhaustion and eligibility renewal through programme participation, may in fact be heavily involved in the above findings. We thus turn to analysing the linkages between entitlement and cycling behaviour.

When we have looked at the determinants of programme participation by prior unemployment duration in Section 5.2.2, it was all too clear that entitled individuals have preferential access to programmes at and just after their benefit exhaustion, being over 10 percentage points more likely to gain access to a programme than if they had not been entitled. Although this may be taken as an indication that joining a programme may often by done purely in order to escape benefit exhaustion (in fact, during the 1990s those at risk of benefit exhaustion were guaranteed a place in a programme), on its own this would not account for the absence of positive programme effects. It may be that programmes do manage to teach individuals some new skills and good working habits, even though participants may initially have joined them out of the need to renew their eligibility to unemployment compensation.

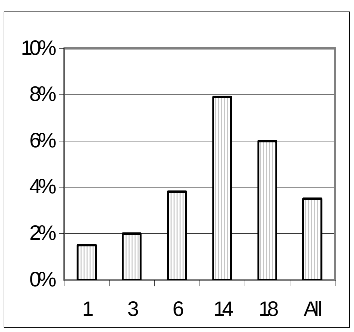
This possibility does not however seem to be supported by further evidence presented in previous sections: individuals entering a programme in their 14th

month in unemployment (the month coinciding with benefit exhaustion) have been found to be that sub-group of the treated for whom the various programme effects have consistently been among the worst. The disappointing results in terms of the probability of employment, de-registration, studies or especially benefit receipt over time, as well as in terms of survival in unemployment could partly be explained by cycling behaviour.

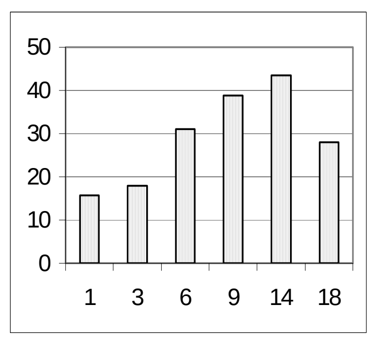
In particular, by far the worst treatment effect in terms of compensated cycling probability is again displayed by those joining a programme in their 14th month of unemployment (henceforth the 14-group): Figure 6.2A shows that for this group the average effect is an 8 percentage point higher probability of being in a compensated cycle over time, against an 1.8 higher probability for individuals joining a programme in their first month of unemployment and an average of 3.5% for the whole group of treated.

Figure 6.2

(A)
Average treatment effect on compensated cycling probability, by month of placement



(B)
Percentage of individuals becoming compensated cyclers, by month of placement



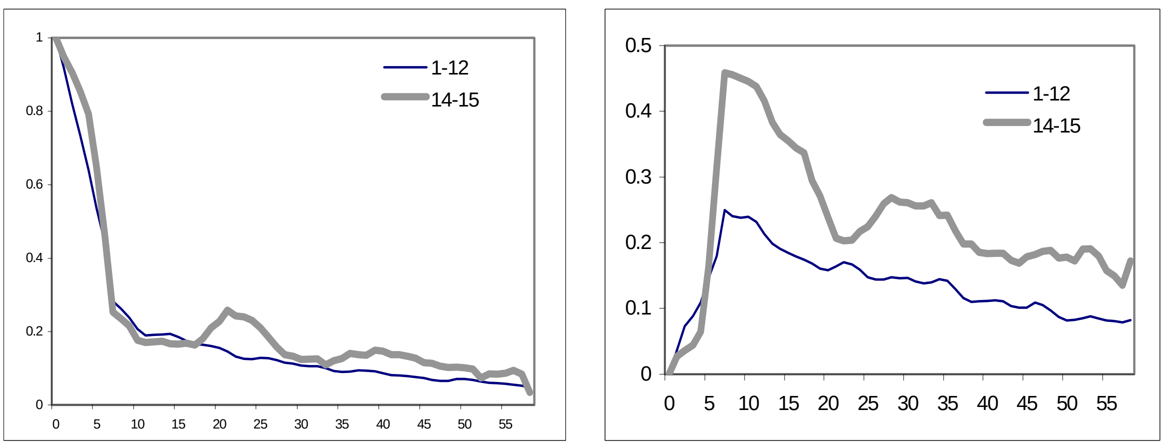
When looking at sub-groups of participants based on their month of placement (cf. Figure 6.2B), the 14-group again stands out as the one with the highest probability of becoming a cycler: 44% of the individuals joining a programme in their 14th month of unemployment are observed in our data to start a compensated cycle (60% a general cycle), compared to around 15% (32%

for a general cycle) of those entering a programme early (within 3 months of open unemployment).

Figure 6.3 contrasts the behaviour of the 14-group with that of individuals who are treated within a year of registration. The graphs plot the two groups' probabilities of being on a programme over time, as well as the probability of being actually collecting unemployment benefits over time. Measurement starts at the beginning of the first programme (i.e. the treatment).

Figure 6.3 Programme rates over time and benefit collection probability over time: 14-group versus 1-12 group
(*t* months after the beginning of the first treatment)

Programme rates | Benefit collection probability



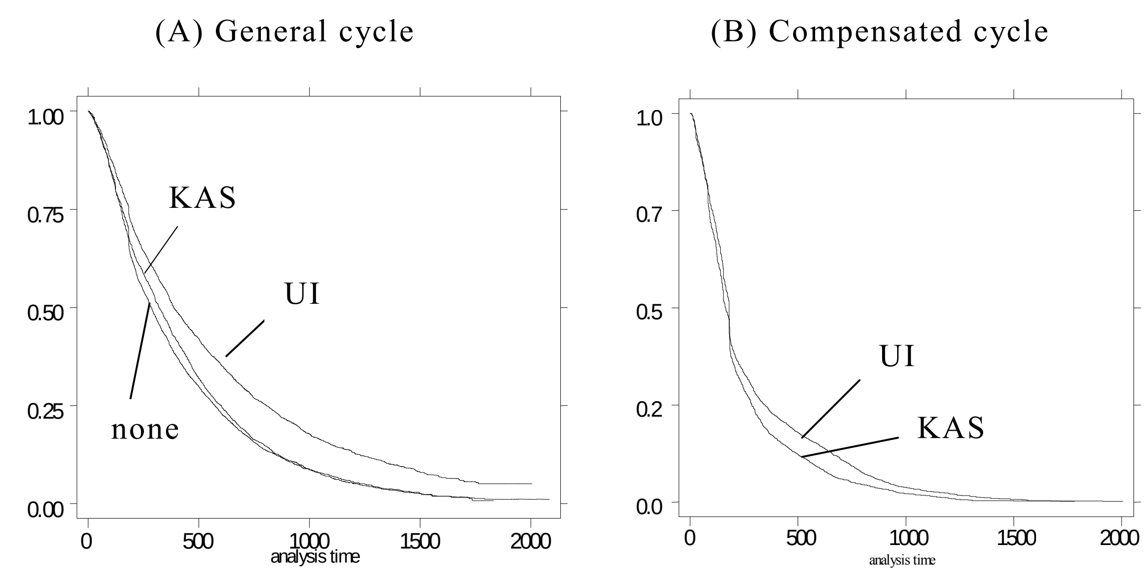
As expected, programme rates steadily decline over time. However, while for the 1-12 group they keep declining up to 5 years after first receiving treatment, they visibly peak again after 20 months for the 14-group. This is very revealing: to renew benefits, participation needs to last at least 5 months, to this add another 14 months as the maximum period of compensated unemployment, after which we are witnessing these individuals going back into programmes. More than one in four (26%) of the individuals in the 14-group is again on a programme in their 21st month, compared to 15% of the 1-12 group, and 13% for its 1-3 sub-group. The story is however not over yet; just add another 5 months spent on the programme starting from their 19th month, another 14 months of compensated unemployment, and it should come to no surprise that the 14-group exhibits a further peak in participation probability at around the 38th month (15% of them are on a programme, compared to 9% of the 1-12 group).

That the spells of unemployment are likely to be supported by benefits is shown by the right-hand side graph of Figure 6.3. After the initial rise due to the exit from the programme back to unemployment, the probability of benefit collection steadily declines for the 1-12 group. For the 14-group the pattern of this indicator is quite different. After the end of the programme, that is after around 5 months, it skyrockets reaching almost 50% (compared to barely 25% for the earlier placement group). This means that after the treatment, half of the 14-group individuals not only fall back into unemployment, but they receive unemployment compensation. Their probability of benefit collection then begins to fall, though not only does it remain significantly higher than the one for the other group over the whole observation period, but it also rises again in correspondence of 25 months. If we allow 5 months on the treatment programme, another 14 in compensated unemployment, another 5 in a further programme to renew eligibility, we arrive at the start of their 25th month, from which time almost one third of the original 14-group participants is again claiming benefits. Once again, the story continues, with another hump in correspondence of 14 months of compensated unemployment, after which the benefit receipt probability drops, only to start rising again after 5 months.

Thus far we have seen that starting a cycle from the first treatment is far from uncommon, and that this applies in particular to those who enrol into programmes in their 14th month in unemployment, a period suspiciously coinciding with unemployment compensation exhaustion. The supposition that these individuals may just view the programme as a passport to renew benefit entitlement is supported by the finding that the main treatment effect is a stronger incentive to keep switching between compensated unemployment spells and subsequent programme participation.

Further evidence that entitlement (renewability) considerations may be a weighty driving force behind incentives to participate and thus behind the observed programme effects can be gleaned by examining the survival in a cycle by entitlement status. In Figure 6.4, entitlement status is determined by receiving actual compensation in their 2nd unemployment spell, the one just before entering a new programme and thus beginning a cycle (i.e. UP**U**P…).

Figure 6.4 Survival function in a cycle (days), by entitlement status



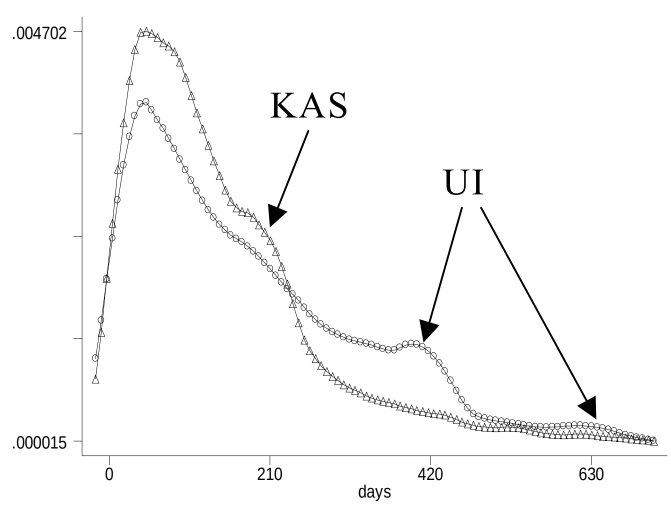
Test for equality of survivor functions:
Pr>chi2 = 0.0000.

T
est for equality of survivor functions:
Pr>chi2 = 0.0000.

UI-entitled participants survive significantly (both statistically and literally) longer in a cycle than KAS-entitled or non entitled treated. After having been treated and having resumed their interrupted unemployment spell, individuals who still have (some) unemployment insurance benefit left and enter a new programme are expected to alternate between unemployment and programme participation for 654 days, 20% longer than KAS-entitled cyclers (512 days) and 35% longer than non-entitled cyclers (483 days). Focusing on the subset of fully compensated cycles, UI-entitled individuals have an expected survival of 287 days against 241 days for KAS recipients, again confirming the ranking of the durations on the basis of the strength of the incentives provided by UI and by KAS (as mentioned in Section 2, the latter pays out on average around half of the cash benefits provided by the former, and for only half of the period).

Another way of looking at entitlement and cycles is to consider the duration of compensated unemployment spells within cycles by entitlement status. Figure 6.5 shows that not only UI-compensated unemployment spells in cycles last on average significantly longer than KAS-compensated ones, but that there is an unmistakable clustering in correspondence of the maximum duration of benefits for both types of entitlement: at 210 days for KAS and at 420 days (and 630 days; for over 55 claimants) for UI.

Figure 6.5 Density distribution of the duration of compensated unemployment spells within cycles (days), by entitlement status



Up to now we have examined the cycling behaviour of individuals who have started a cycle from the treatment itself. A final piece of evidence relates to the outcomes experienced by individuals who have *not* started a cycle from their first programme.

In fact, while we have seen that the various treatment effects have been among the worst for those joining a programme at the time of benefit exhaustion, when in Section 5.3 we have looked at the performance of individuals who have been observed *not* to start cycling from the treatment, our results have shown that participation in a programme does not seem to have harmed the non-first-time cyclers, though it has been impossible to detect any benefit they may have derived from it.

Let us thus focus on those treated individuals who have not started cycling after having participated in a programme, but have instead exited their interrupted unemployment spell for a job, and consider the potential programme effect on cycling for the specific sub-samples of individuals of Sections 5.3.1 and 5.3.2, that is for

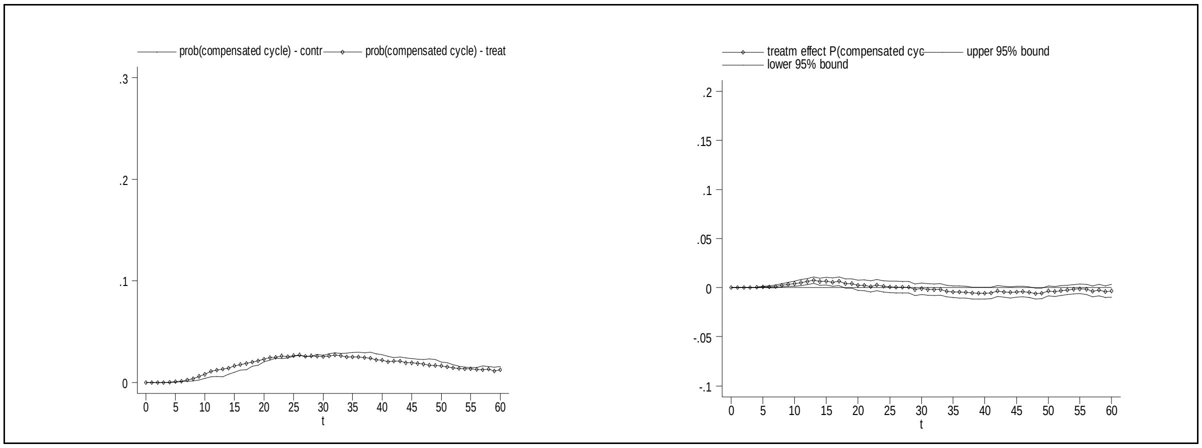
(A) individuals who have found a job – some on their own (UE individuals), some after having experienced a programme (UP(U)E individuals) – without starting a cycle (i.e. without going on a second programme);

(B) the sub-set of them who have fallen back into unemployment after having found a job (i.e. UEU and UP(U)EU individuals).

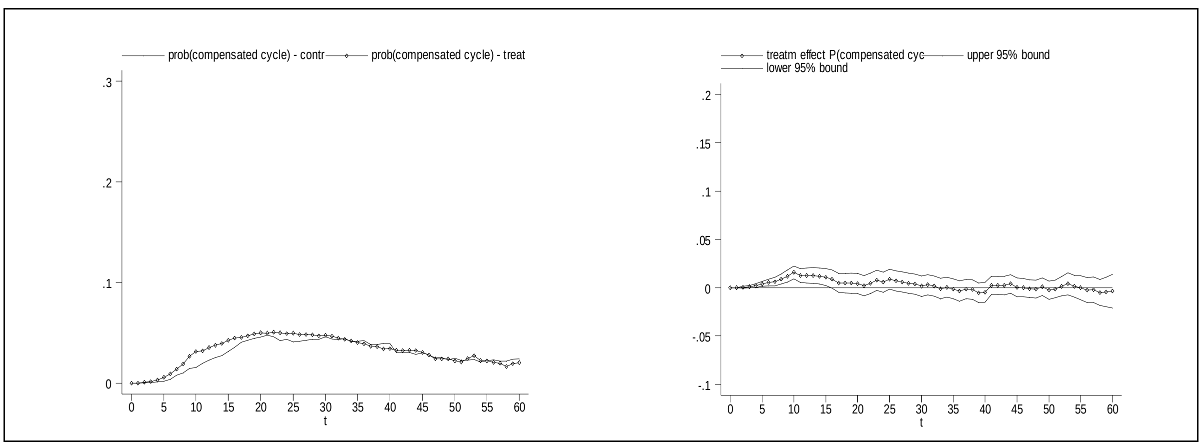
Figure 6.6 – which should be contrasted to Figure 6.1 – shows that programmes keep having no effect on these individuals: former participants are hardly ever (both statistically and in terms of the effect size) more likely to be on a cycle (after having registered again) than comparable non treated. How to view such a result is largely a matter of interpretation. A favourable stance towards the programmes may tend towards viewing former participants as no more likely to be on a cycle than if they had not participated. It has however to be kept in mind that the participants we are now looking at have been selected among those who did not start a cycle from their first treatment. In addition, what can safely be stated is that the programme effect on the cycling probability is certainly not a negative one, i.e. programmes do *not* discourage former participants to start cycling in the future.

Figure 6.6 Compensated cycle probability over time and corresponding treatment effect, non-first-time cyclers

(A) UP(U)E *versus* UE
(*t* months after entry into employment)



(B) UP(U)EU *versus* UEU
(*t* months after entry into new unemployment)



7. Conclusions

It is felt that the most important issue in evaluating the Swedish programmes concerns the co-ordination and interaction between the two components of the labour market policy in Sweden – labour market programmes and the unemployment insurance system.

On the one hand, labour market programmes do offer the possibility of remaining out of work for a possibly indefinite period of time under very generous terms, thus reinforcing the work disincentive associated with the generous unemployment insurance system. Several mechanisms may contribute to weaken the incentives to escape unemployment, such as a higher reservation wage, a lower search intensity or a lower geographical mobility. On the other hand, labour market programmes may offer the possibility of investment in several dimensions of human capital; if this second component is sufficiently strong, the possibilities on the labour market, including the earnings on the job, may become high enough to outweigh the work disincentives created by the system.

What has to be noticed, however, is that while the first component is a fact – programmes do allow to renew benefits, and until 1996 they even allowed to become eligible for the first time –, the mere existence of the second one is a supposition, though a widely held one. Object of the present analysis is in fact to evaluate the presence and strength of this human capital-enhancing component of the programmes. In particular, in the Swedish institutional set-up any potential productivity-enhancing components of the programmes would have to fiercely compete with the cycling incentives to be able display any positive effects.

The findings from our analyses can be broadly grouped into two main sets.

The first set of results (Section 5.2) relates to how programme participants perform from the treatment compared to a hypothetical state where they would have waited longer in open unemployment. We had to conclude that, on average, joining a programme keeps participants longer in the current unemployment spell than if they had waited, and that these individuals are expected to experience a lower probability of being out of the *official* unemployment system over time, as well as a higher probability of benefit collection and of being in a compensated unemployment-programme cycle for at least up to five years from the start of the treatment, than do comparable individuals who have not joined a programme at least up to when participants did. A possible explanation for all these effects is the reinforced work disincentive associated with the entitlement renewability that participation allows. On the other hand we also found evidence of a positive treatment effect on employment rates, which would seem to be quite robust to the misclassification problem of the 'lost' individuals.[65] Such an impact seems to derive from a greatly reduced likelihood of *unregistered* unemployment over time. Under some simplifying assumptions, joining a programme compared to waiting longer would thus 'swap' uncompensated, unregistered unemployment not only for unemployment-benefit compensated (and thus registered) unemployment as well as for further programme participation, but also for employment in the regular labour market.

Nevertheless, various pieces of evidence have consistently hinted at a considerable role of entitlement and its renewability behind the incentives to participate. Entitled individuals have an (over 10 percentage points) preferential access to programmes at and just after their benefit exhaustion than if they had not been entitled, so that entering a programme appears to be often done purely in order to escape benefit exhaustion. While this tells us nothing about the possibility of a human capital-enhancing effect of the programmes, the first set of results show that for individuals entering a programme around benefit exhaustion the various programme effects are among the worst than for any other group of treated. From such results it would seem that the human capital-enhancing component of the programmes may not in general be strong enough to always outcompete the work disincentives provided by the system.

The second sets of results – Section 5.3 – looks at outcomes for sub-groups of former participants selected on the basis of their observed performance after the treatment. When looking at the performance of individuals who have been observed *not* to start cycling from the treatment, programme participation appears to be practically irrelevant for their subsequent labour market perform-

[65] A proper classification of the 'lost' individuals is important also when looking at programme effects conditional on past outcomes, since they too should ideally enter in the determination of the sub-populations considered; in the present analyses of Section 5.3 these individuals are ignored.

ance. While participation has not harmed these non-1st-time cyclers, it does not seem to help those participants who have managed to find a job to retain it longer, and nor does it give former participants fallen back into unemployment any kind of particular advantage in dealing with their new unemployment experience. The set of findings that when cycling has been ruled out by construction, programmes have no effect on any of the outcomes considered would actually point towards an absence of human capital-enhancing components in the programmes taken by this subset of individuals.

Before being able to make a more conclusive judgement, though, in addition to the misclassification problem of the 'lost' individuals at least three other important issues would require further investigation.

We have considered a number of interesting outcomes, all represented by labour market states individuals may find themselves in (e.g. unemployment, employment, regular education, non-participation etc.). However we had no information on post-programme earnings or wages – an important measure of potential programme effects on individual productivity, which would allow a more complete picture of the comparative performance of former participants who have found a job.

Secondly, the present analysis has looked at the programme-benefit system in its entirety, lumping all kinds of programmes into an anonymous 'treatment'. Overall, the system may not be seen as fulfilling its aim. A possibility to be explored in further work is to adequately take account of an institutional environment where individuals can potentially participate in a wide array of different types of programmes. Different treatments may in fact have heterogeneous effects. Thus while some programmes may simply lock participants in rather useless and low-qualified tasks, others may indeed endow individuals with marketable transferable skills, whose return on the labour market may turn out to be large enough to outweigh the work disincentives created by the system. A multiple-treatment analysis would thus aim at identifying the best-performing programmes.

Finally, the results obtained in this paper rely on a non-parametric technique which assumes selection on observables. Despite the richness of the available dataset, the discussion in Section 5.2.2 has highlighted some potentially remaining sources of bias. Future work should assess the robustness of such results to the assumptions needed by an alternative approach aimed at identifying a structural econometric model. Such an approach would explicitly model the sequence of choices facing individuals and take into account the endogeneity of the selection of unemployed workers to specific programmes, which are intertwined with unemployment benefits eligibility and renewability.

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Appendix

This appendix provides additional information on matching for the three main types of matching analyses performed.

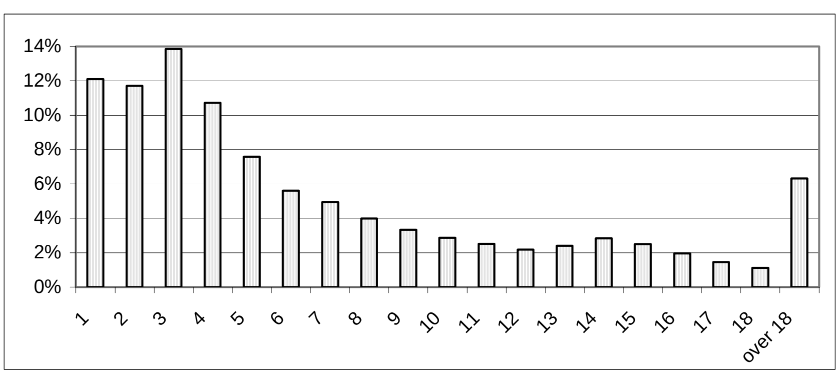
1. MATCHING FOR JOINING *VERSUS* WAITING

Matching Protocol

I) The following procedure is repeated for each month up to one and a half year since registration (i.e. m=1,2, … 18):
   1. The relevant units are selected: all those treated who enter a programme within their m^{th} month in unemployment and all those non-treated who are still unemployed after m months since first registering at the employment office.
   2. The propensity score $e(X;m) \equiv Pr\{D^m=1 \mid X,\ T^0 \geq m\}$ is estimated on the two sub-groups, giving an estimate of the conditional probability of joining a programme in the m^{th} month of unemployment.
   3. This sub-group of participants is matched to the corresponding sub-group of (at least yet) non-participants by nearest-neighbour with replacement within caliper δ; treated unit i is matched to that non-treated unit j such that:
   $$\delta > e(X_i, m) - e(X_j, m) = \min_{k \in \{D^m=0\}} \{|e(X_i, m) - e(X_k, m)|\}.$$
   4. The differential performance of the sub-group of treated and the sub-group of matched comparisons is used to calculate the effect of entering a programme after m months *versus* waiting longer.

II) All the M=18 the results by month of entry are finally aggregated to obtain the average expected effect of joining in a given month of unemployment compared to waiting longer, where the average is taken with respect to the observed joining distribution (i.e. the T^1 distribution) of the treated.

Treated individuals by month of placement (%)



Treated and potential controls by time in unemployment

Month	Treated	Non-Treated	Month	Treated	Non-Treated
1	4,141	99,992	10	982	21,417
2	4,004	85,377	11	861	19,047
3	4,739	68,669	12	747	17,115
4	3,665	53,478	13	823	15,093
5	2,602	43,976	14	968	13,247
6	1,922	37,283	15	852	11,474
7	1,689	32,101	16	672	10,064
8	1,367	27,494	17	501	8,938
9	1,140	24,159	18	384	7,998

treated	=	31,975
matched controls	=	21,999

Total number of times a control is used (%)

1	70.1
2	20.0
3	6.3
≥4	3.6

8,070 treated individuals (25%) are previously used as controls.

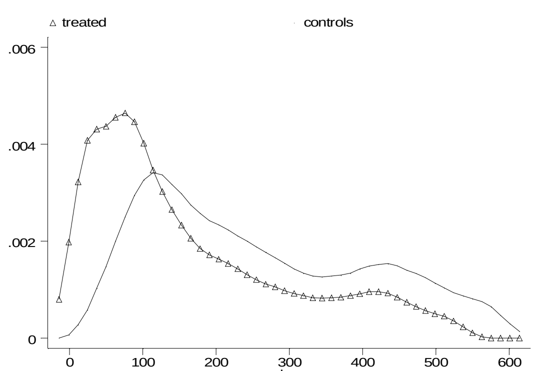
Mean and percentiles of difference in linear prediction between treated and matched controls

Mean	0.00037
25%	0.00001
50%	0.00002
75%	0.00007
95%	0.00056

Support of the linear prediction

Min	-3.54066
Max	0.99597

Duration distribution of the (interrupted) first unemployment spell for treated and matched controls



2. Matching Conditional on Having Found Employment

treated = 10,351
matched controls = 7,049

Imbalance of the most important covariates:
Means in the two groups and standardised differences (%)

	Treated	Matched Controls	% bias
Age at entry	29.47	29.78	-2.91
Gender female)	47.66	47.51	0.29
Foreign	12.88	13.56	-2.03
Education			
compulsory	15.39	16.69	-3.55
upper secondary	13.95	13.15	2.34
vocational upper secondary	54.62	54.50	0.25
university	16.04	15.66	1.03
Education for job sought (yes)	65.43	65.81	-0.79
Experience for job sought			
some	24.13	24.06	0.18
good	43.32	44.17	-1.71
Missing information	2.78	2.68	0.65
Entitlement status			
UI	38.06	40.44	-4.87
KAS	5.69	5.90	-0.91
County			
Stockholm	17.06	16.35	1.92
Göteborg and Bohus	15.99	16.54	-1.49
Malmöhus	12.50	12.42	0.23
Sector			
professional and technical work	14.39	14.92	-1.48
health, nursing and social work	12.62	13.31	-2.07
administrative, managerial and clerical work	14.60	14.08	1.49
sales	12.81	13.31	-1.49
production	23.69	23.42	0.64
services	8.86	8.13	2.60
Part-time	3.41	3.66	-1.36
Interlocal	18.37	17.93	1.13
Month of registration	6.22	6.27	-1.62
January	10.70	11.27	-1.82
June	17.20	16.01	3.19
August	12.92	12.85	0.20
First register as part-time unemployed	5.10	6.11	-4.37
Part-time unemployment	18.24	20.18	-4.93
Type of unemployed			
able to take a job immediately	91.25	90.31	3.24
offered a labour market programme	5.06	3.85	5.85
need guidance	15.37	16.41	-2.85
Unemployment duration	266.19	278.86	-4.53

Type of employment			
recalled	9.74	9.24	1.71
temporary	40.58	39.31	2.58
registered	36.24	34.39	3.86
Propensity score	-0.4199	-0.4209	0.14
Average absolute standardised difference			2.02
Median absolute standardised difference			1.67
Number of observations	10,351	10,351	

Total number of times a control is used (%)

1	72.7
2	20.0
3	5.3
≥4	2.0

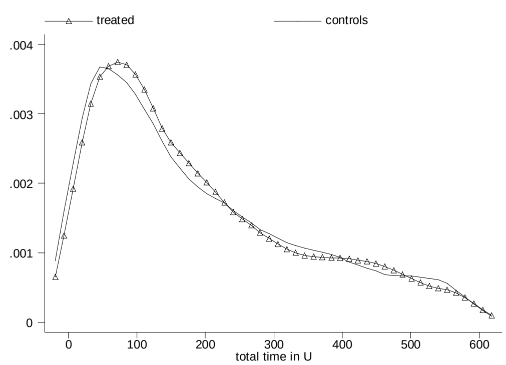
Mean and percentiles of difference in linear prediction between treated and matched controls

Mean	0.00155
25%	0.00000
50%	0.00003
75%	0.00010
95%	0.00138

Support of the linear prediction

Min	-3.06122
Max	3.08517

Total unemployment duration distribution for treated and matched controls



Heterogeneous treatment effects

	Expected employment duration		Effect	
	Treated	Controls	Days	Significance
Gender				
- males	896	900	-4	.91
- females	677	709	-32	.17
Entitlement				
- ui	870	875	-5	.77
- kas	851	805	46	.62
- none	731	738	-7	.77
Age				
- 18-20	564	623	-59	.01
- 20-25	740	726	14	.33
- 25-35	909	899	10	.70
- 35-54	896	915	-19	.74
Citizenship				
- Swedish	789	818	-29	.11
- foreign	812	772	40	.34
Human Capital				
- low	760	741	19	.36
- secondary	739	755	-16	.66
- university	1,031	1,049	-18	.67
- no experience	726	726	0	.85
- a lot exper.	894	897	-3	.999
- univ & lot exper	1,090	1,137	-47	.36
- low & no exper	704	668	36	.94
Time in Unempl.				
- before joining				
≤90	752	792	-40	.11
180-270	760	746	14	.50
400-500	752	701	51	.07
- complete				
≤90	752	804	-52	.87
180-270	724	766	-42	.21
400-500	774	764	10	.52
Participation probability (deciles)				
10	932	913	19	.55
20	919	889	30	.30
30	879	792	87	.01
40	742	752	-10	.86
50	806	764	42	.30
60	702	765	-63	.06
70	715	747	-32	.31
80	677	727	-50	.06
90	676	746	-70	.08
100	621	697	-76	.44

3. Matching Conditional on Having Fallen Back into Unemployment

treated = 6,021
matched controls = 4,052

Imbalance of the most important covariates:
Means in the two groups and standardised differences (%)

	Treated	Matched Controls	% bias
Age at entry	28.93	29.15	-2.03
Gender female)	50.52	50.76	-0.47
Foreign	12.49	13.25	-2.28
Education			
compulsory	16.08	16.81	-1.97
upper secondary	14.65	15.01	-1.03
vocational upper secondary	55.94	54.18	3.54
university	13.34	14.00	-1.93
Education for job sought (yes)	63.58	62.48	2.27
Experience for job sought			
some	25.24	25.63	-0.88
good	41.69	41.37	0.64
Missing information	2.92	2.41	3.20
Entitlement status			
UI	37.44	38.40	-1.99
KAS	5.45	6.58	-4.75
County			
Stockholm	15.38	14.38	2.80
Göteborg and Bohus	15.55	15.60	-0.14
Malmöhus	12.41	12.52	-0.35
Sector			
professional and technical work	12.59	12.34	0.75
health, nursing and social work	14.20	15.50	-3.64
administrative, managerial and clerical work	12.72	12.27	1.36
sales	12.72	12.81	-0.25
production	24.07	24.58	-1.20
services	9.85	9.90	-0.17
Part-time	3.44	4.17	-3.82
Interlocal	17.72	17.16	1.49
Month of registration	6.19	6.27	-2.44
January	10.60	11.24	-2.08
June	18.45	16.72	4.54
August	13.40	13.60	-0.58
First register as part-time unemployed	4.87	5.90	-4.56
Part-time unemployment	18.24	20.46	-5.64
Type of unemployed			
able to take a job immediately	92.06	91.00	3.82
offered a labour market programme	4.98	4.14	4.06

need guidance	14.53	14.83	-0.84
Unemployment duration	256.77	264.94	-3.23
Type of employment			
recalled	7.39	7.17	0.83
temporary	47.82	46.47	2.70
registered	41.55	40.01	3.14
Employment duration	273.24	267.81	1.62
Propensity score	-0.3987	-0.4002	0.21
Average absolute standardised difference			2.16
Median absolute standardised difference			2.03
Number of observations	6,021	6,021	

Total number of times a control is used (%)

1	72.8
2	16.9
3	5.5
≥4	4.8

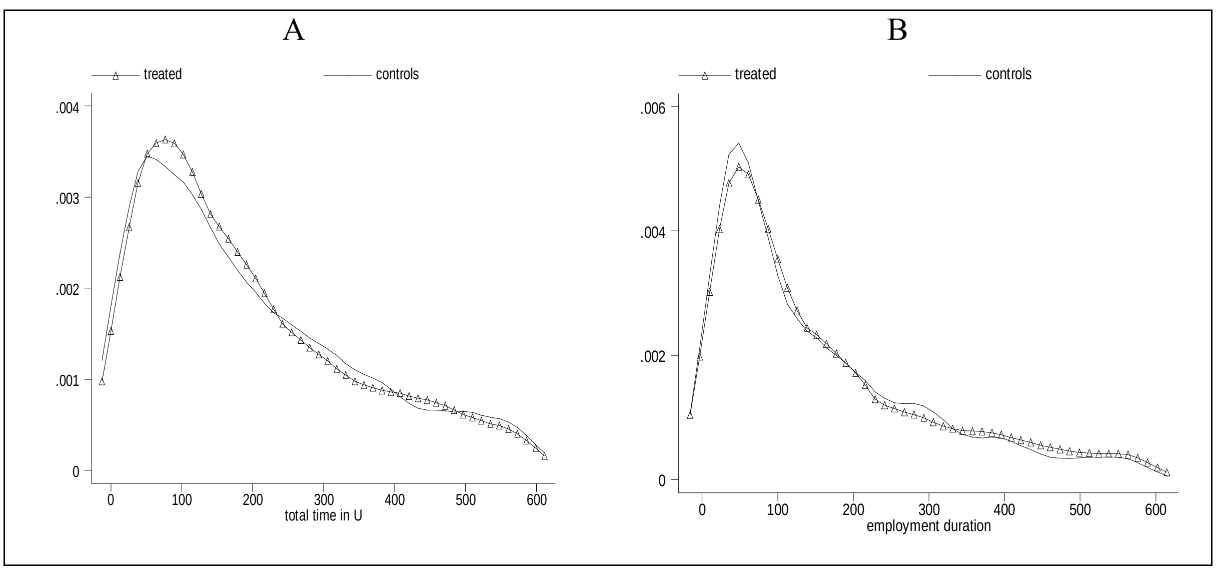
Mean and percentiles of difference in linear prediction between treated and matched controls

Mean	0.00215
25%	0.00002
50%	0.00006
75%	0.00019
95%	0.00222

Support of the linear prediction

Min	-3.0079
Max	3.0121

(A) Total unemployment and (B) employment duration distributions for treated and matched controls



Heterogeneous treatment effects

	Expected employment duration		Effect	
	Treated	Controls	Days	Significance
Gender				
- males	260	257	3	.43
- females	293	304	-11	.35
Age (at re-entry into unempl.)				
- under 25	212	214	-2	.72
- over 25	331	338	-7	.83
Citizenship				
- Swedish	267	268	-1	.54
- foreign	345	376	-31	.14
Human Capital				
- low	341	360	-19	.96
- university	297	291	6	.42
Participation probability (deciles)				
10	294	274	20	.04
20	256	232	24	.23
30	241	226	15	.72
40	211	257	-46	.63
50	260	255	5	.92
60	267	260	7	.00
70	275	269	6	.89
80	326	321	5	.59
90	309	333	-24	.10
100	302	363	-61	.02