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Natural and classical experiments in Swedish labour market policy

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NATURAL AND CLASSICAL EXPERIMENTS IN SWEDISH LABOUR MARKET POLICY

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ABSTRACT

Effects of Changes in the Unemployment Insurance Eligibility Requirements on Job Duration — Swedish Evidence

This paper investigates the impact of the unemployment insurance (UI) entrance requirement on employment duration among earlier unemployed in Sweden. I exploit changes in the rule taking place in 1994 and 1997 to study behavioural adjustments in the timing of job separation between 1992, 1996, and 1998. Performing across-year analyses, I find evidence of clustering of job exits at the time of UI qualification. By using predicted hazard rates for each week, I calculate an approximate 2.9-week extension in average employment duration between 1996 and 1998 due to the 5-week prolonging of the entrance requirement.

Job-search Assistance Using the Internet – Experiences from a Swedish Randomised Experiment

This paper reports the experience from a randomised experiment offering voluntary job-search assistance on the Internet to job seekers at Swedish public employment offices. The purpose is to, i) investigate to what extent the evaluation design manages to avoid common difficulties in experimental evaluation, ii) assess the effect of the programme on the employment outcome, and iii) use the nonbiased experimental results as a benchmark evaluating the performance of frequent nonexperimental estimators. I find that the evaluation design successfully circumvents inherent difficulties in the experimental approach, such as ethical concerns, bureaucratic behaviour and randomisation bias. However, the voluntariness of the programme caused severe compliance problems in terms of both no-shows and dropouts. This is accounted for by analysing the effect of the “intent-to-treat” (the policy parameter of most interest), which is close to zero. Studying the effects of various doses of actual treatment, using an nonexperimental instrumental variable model, I fail to reject the hypothesis of a zero programme effect. Finally, a methodological comparison suggests that standard nonexperimental techniques succeed in reproducing the nonbiased experimental results.

Are there Pre-programme Effects of Swedish Active Labour Market Policies – Evidence from Three Randomised Experiments

This paper takes advantage of unique experimental data from three demonstration programmes in 2004 to investigate pre-programme incentive effects of active placement efforts at the employment offices in Sweden. The exit rate from unemployment between referral to and start of the programme services is compared between UI eligible experiment and control group members. The results are mixed. In one of the experiments, targeted towards a broad group of UI receivers, arranged job-search activities in groups combined with increased monitoring of job-search efforts generated a 38 per cent increase in the escape rate from unemployment in the weeks leading up to programme start. This translates into an almost two-week reduction of the ongoing UI spell. Referrals to increased monitoring alone did not have the same effect on exit behaviour. In the other two experiments, targeted towards youth and highly educated respectively, referrals to active placement efforts had no effect on the pre-programme outflow.

CONTENTS

Acknowledgements

Reaching the final destination of an almost ten year academic endeavour, surely I must have some profound insight to share with you at this time. Well, of course not. And enough about me, this section is dedicated to those who helped me make this possible. I am especially indebted to three persons.

First of all, Fredrik Jansson, who I met in the first year of the graduate programme. Thanks to him, I ended up at the Swedish Labour Market Board (AMS), which during the 1990s had started up an in-house research unit for labour market analyses. This was the best possible environment for me to learn about the implementation of labour market policy and the analytical tools of labour market policy evaluation. Fredrik took me on and then later became my supervisor at AMS. Besides being both curious and analytical, and therefore very research-oriented, Fredrik is outgoing and has the ability to inspire people around him. I think this is a rare combination and I really hope to work with him again in the future.

When Fredrik in 2001 moved on to bigger tasks, I was left with Anders Harkman… Anders is the kindest, most humble person I know, and the first person I call for analytical advice. Despite no longer being colleagues, I have continued to draw on his wisdom and skills, and I hope to keep doing that for many more years. So don't even think about retiring yet!

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Stockholm, December 2005
Pathric Hägglund

Introduction

This thesis consists of three separate essays in labour market policy evaluation using both natural and classical experimental evaluation designs. In this introduction, I first present the research questions of each essay before describing the implementation and the results in more detail. The last section reports practical experiences from pursuing four randomised experiments presented in essay 2 and 3. Since the previous, and only, Swedish experiment in this field was conducted in 1975, I extract some lessons of perhaps some use in future randomised demonstrations.

The first essay explores an often neglected parameter of the unemployment insurance (UI) system that regulates the required attachment with the labour market in order to qualify for UI: the entrance requirement (ER). The ER specifies the minimum amount of work necessary to be entitled for UI benefits. Most research on UI has focused on the impact of changes in the replacement ratio or the length of the benefit period on unemployment duration. However, significantly less is known about the UI regulation's influence on the employment relationship. To what extent does it affect people's timing of job separations?

Both firms and employees have incentives to account for the UI system in their decisions. The prototypical case is the seasonal industry where workers combine spells of work and unemployment in circular flows. Since the firms know that the UI system will attenuate the workers' separation costs, they employ workers for short periods to meet short-term needs. In this case, benefit receipt primarily acts to redistribute income and leisure for actors "playing the system" – and not as an insurance.

In Sweden during the 1990s, the ER was tightened on two occasions, requiring additional attachment to the labour market in order to qualify for benefits. To what extent do the agents of the labour market adjust to the new requirements and how does the change in the ER affect average employment duration? These issues are dealt with in the first essay of this dissertation.

The second paper considers the new opportunities offered by the Internet for the public employment services (PES). Since 1995, several on-line placement services have been introduced in Sweden. Today, the PES Vacancy bank, where employers advertise their job vacancies, has 600,000 unique visitors every month. At the employment offices, several self-service instruments have been introduced. For instance, in a computerised training programme, the unemployed can learn the basics of effective job search. Also, for a few years now, the newly unemployed who register at the employment office is responsible for formulating the individual "action plan".[1] This is all part of a new labour market policy strategy where resources are reallocated from newly unemployed to long-term unemployed and other disadvantaged groups.

The new policy has been subject to much criticism, not only among the unemployed but also among employment officers and union representatives. They question the proposition that the self-service instruments can be as efficient as personal meetings with the employment officers. With less personal contact early in the unemployment spell, identifying the unemployed in most need of assistance becomes more difficult. Extensive periods without intervention then risks worsening job chances even further. Another objection is that people are not as skilled using computers as many may think. The self-registration has caused trouble where much effort must be spent on correcting and complementing data in the information system. This has to some extent annulled the intended time-saving from implementing the self-service instruments.

Current developments involve a higher degree of interactivity between job seekers and employment officers, which means that further dimensions of traditional employment services are being added to the Internet services. In 2002, the Swedish Labour Market Board (SLMB) carried out a nation-wide demonstration programme for pursuing job-search activities on the Internet. The

[1] The individual "action plan" consists of, among other things, the specified job-search requirements, the current geographical search area (which depends on the length of the unemployment period), and suitable active measures.

idea was to investigate whether or not these types of services were feasible to perform on the Internet, and whether or not the services should be a permanent feature of the employment services. The demonstration was preceded by an application procedure where voluntary job seekers could sign up for participation. What is the clients' interest in this type of services, and what are the job-chance enhancement prospects for the unemployed?

In the third and final essay, I investigate the occurrence of anticipatory effects of active placement efforts at the employment offices in Sweden. Being one of the countries with the highest expenditures on active labour market policies, Swedish research on individual programme effects has explicitly focused on the impact during and after the programme services. The results of these studies have often shown only small and even adverse effects of programme participation (see Calmfors et al. 2001).

Besides upgrading skills and activating long-term unemployed to improve employability, policymakers often motivate the usage of active measures as a tool for testing work motivation. A common perception among employment officers is that referrals to different types of compulsory programme activities help to remove those having little problem finding employment, thus reducing the extent of moral hazard behaviour. This is because participation is expected to be time consuming and thus reduce time for both leisure and job search. If the activities are anticipated, this would lower the value of being unemployed before start and the exit rate from unemployment is expected to increase. If, on the other hand, the expected returns of participation – in terms of improved job chances and/or the distribution of wage offers during and after the services – overshadow the negative aspects, the value of unemployment increases and the escape rate drops before start.

Empirically, the former version is supported. A few non-experimental Swedish studies find evidence of increased exit rates from unemployment in the weeks prior to the start of labour market programmes (see for instance Carling et al., 1996). None of these studies, however, explicitly set out to study the "motivational" aspects of the active labour market policies. Outside Sweden, experimental studies from the U.S (see for instance Black et al. 2003) and the U.K (Dolton & O'Neill, 1996), find significantly increased off-unemployment hazard rates prior to attending different job-search and re-employment services.

The present study explicitly investigates the behavioural adjustment of being referred to active placement efforts. Using experimental data from three different regions of Sweden, I try to answer the questions; how common are "pre-programme" effects, under what circumstances do they occur, and can they in turn motivate the large spending on active measures?

The three essays

Essay I. Effects of Changes in the Unemployment Insurance Eligibility Requirements on Job Duration – Swedish Evidence

This paper contributes to the very sparse empirical literature on the relationship between the entrance requirement (henceforth: ER) and the timing of job separations. Exploiting changes in the ER taking place in 1994 and 1997, I study behavioural responses in the timing of job exits in 1992, 1996 and 1998. I also study the effect of the last ER change on average employment duration. To my knowledge only four Canadian studies, between 1994 and 1998, have investigated the effect of the ER on employment duration. All find evidence of significant increases of the employment-unemployment hazard when the ER was satisfied.

My analyses are restricted to employment spells of persons who prior to employment were unemployed. To establish the length of employment spells, I use unemployment register data and information on job duration until returning to unemployment. A flexible piece-wise constant hazard model captures weekly job exits corresponding to the time of fulfilling the ER.

Studying each year separately, I find in contrast to the Canadian studies no clear evidence of adjustments to the ER in terms of distinct mass termination of job spells at, or around, the week of fulfilment. I propose several possible explanations for this finding, for instance the lack of data on exact employment duration and the possibility of fulfilling the ER through several, instead of just one, composite periods of employment. Instead performing across-year analyses with years with different ER, I find evidence of adjustments in job turnover to the entrance requirement in all three years. Analysing the effect in one industry (farmers) and one region (Norrbotten), both characterised by relatively high seasonality in the production process, suggests that changes in the ER primarily affects sectors with relatively large recurrent flows between jobs and unemployment (indicating a high degree of awareness of the UI system). Using the estimated UI parameters, I estimate an approximate 3-week prolonging of the average employment duration due to the 5-week extension of the ER between 1996 and 1998. On Canadian data, Green & Riddell (1997) estimated a 1.5-week increase in the average duration of employment in regions with high unemployment as a result of a temporary extension of the ER from 10 to 14 weeks.

Essay II. Job-search Assistance Using the Internet – Experiences from a Randomised Experiment in Sweden

The contribution of this paper is threefold. First, I investigate the employment outcome from a demonstration programme offering voluntary job-search assistance on the Internet. Second, by randomly assigning job seekers to different services, I explore to what extent the experimental evaluation design succeeds in circumventing common difficulties in experimental evaluation. Third, taking advantage of the experimental design and the non-biased experimental impact estimate, I assess to what extent standard non-experimental evaluation methods succeed in replicating consistent impact estimates.

In North America, practical and analytical experiences from conducting social experiments have been accumulated for almost 40 years. In Europe, however, with a few exceptions in Sweden, Britain, Norway (2), Denmark and the Netherlands, experiments have not been an alternative evaluating social programmes. Therefore, documenting the experiences and learning the most from every opportunity is important.

I find that the experimental design successfully avoids many typical problems inherent in social experiments, for instance randomisation bias, bureaucratic behaviour and Hawthorne effects. However, the sample size is small (636). Furthermore, a large proportion of the experiment group members either never entered the programme (47%), or dropped out early. Both "no-shows" and dropouts are common in experimental evaluation. However, they pose a problem only in situations where the effect of actual participation is the parameter of most interest. In this case, with no activity-level restriction, I estimate the effect of the "intent-to-treat", which explicitly considers the choice of not participating at all.

With the low activity level, the intent-to-treat impact estimate fails to reject the hypothesis of a zero programme impact. Furthermore, using the random assignment as an instrument in an IV model, studying the effect of various levels of actual treatment, no significant effects are found.

Finally, the comparison of methods shows that standard non-experimental evaluation techniques successfully reproduce the experimental intent-to-treat impact estimate. The results may, however, in part be due to the non-adjusted outcome similarity between the control and the (constructed) comparison group members. Also, with an experimental impact estimate of low precision only large deviations would have generated another conclusion. Further analyses of the ability of these methods to account for selection into other types of programmes are necessary in future research.

Essay III. Are there Pre-Programme Effects of Swedish Active Labour Market Policies - Evidence from Three Randomised Experiments

The third and final essay investigates the incentive effects of being referred to compulsory active placement efforts at the employment office. Using unique experimental data from three separate demonstration programmes in 2004, the exit rate from unemployment between referral to and start of the programme services is compared between UI eligible experiment and control group members.

The results from the three studies are mixed. In two of the demonstrations, in Uppsala and Östergötland, no effect of the referrals was found. In the third experiment, in Jämtland, I find evidence of a 38 per cent increase in the off-unemployment hazard rate preceding services involving a combination of compulsory job-search assistance activities and increased monitoring of job-search efforts. By offering two different treatment packages with random assignment to each treatment, I find that the positive effect derives from the job-search assistance activities. The effect of referrals to recurrent interviews for monitoring the job search is significantly lower and non-significantly different from the exits of the control group. This finding is possibly the result of the job-search assistance activities being arranged in groups, which for some unemployed may be stigmatising, as opposed to the in-person interviews. The estimated effect of being referred to the job-search assistance activities corresponds to a two-week reduction of the ongoing UI spells. The positive effect is not the result of more temporary interruptions of the unemployment spells among the experiment group members due to, for instance, less attractive job matches.

I propose two possible explanations for the diverse results. First, whereas the Jämtland demonstration invited a broad group of unemployed to participate, the other two targeted on specific groups of unemployed. These groups could on average have relatively less scope for finding a way out of unemployment. Second, Lindbeck et al. (2004) argue that the large local variations in sick leave in Sweden are related in major part to a "sick leave culture" based on local-specific attitudes towards sick leave. An interesting fact is that the county of Jämtland reported the highest sick leave in the country in 2003. If there exists a sick leave culture in Jämtland, it would be easy to imagine a similar tradition within the UI system, which to some extent might explain the results of the Jämtland demonstration.

A final result of this paper suggests that using the unemployment register as the sole informant when analysing programme effects on unemployment duration could cause substantially biased results. Comparing outcomes when using the unemployment register and those when instead using the more reliable UI payment register data, the programme effect of the activities in two of the experiments are severely upward biased.

Some lessons from conducting the experiments

Since the experiments presented in this thesis are the first in this policy field in Sweden in 30 years, I take the opportunity to report some experiences from conducting them.

Planning the experiment

In the experiments, especially those reported in the last essay, the planning period was somewhat short. Additional time spent with the case workers in the scheme groups would have helped me, as an evaluator, to learn more about the administrative work at the employment offices, for instance regarding the enrolment routines. Extra preparation time would also have allowed the scheme group members to more fully grasp the intention behind the evaluation requirements. The significance of the fact that this "learning process" (on both sides) to some extent reached into the active period of the experiments should, however, not be overrated. Working routines as well as service elements are inevitably fine-tuned over time as skills increases among the scheme workers.

With only limited preparation time before evaluation, however, a recommendation is to first test the outlined strategies in a brief trial period before start. This way, obvious flaws can immediately be corrected and the services offered at the start of the evaluation period and the ones offered in the end can be made as similar as possible.

Organisation

The organisation behind the demonstrations reported in the last essay consisted of three levels. At the top, supervising the programmes, was a *project group* at the

SLMB. This project group consisted of four persons, one assigned to each scheme and as such appointed chairman of the reference group attached to each scheme.

The *reference group*, which represented the middle level of the scheme organisation, also contained the assigned SLMB evaluator, representatives from the county labour boards and the operative team manager of the particular project (the *project team manager*). In some cases, the local industry and/or the local trade unions were also represented. The role of the reference group was to continuously follow the demonstration activities and to make strategic decisions on comprehensive matters concerning the course of the project.[2]

Finally, at the operative level, a *project team* of 3-5 case workers including the project team manager carried out the services at the local employment offices. Members of the project team were selected by the county labour board.

The reference groups were thus responsible for operating the demonstrations and making decisions along the way. However, the reference group met only every 4-6 weeks and transitional decisions were sometimes necessary to settle immediate and unforeseen issues. For instance, when the extent of the register deficiencies (described in more detail in the third paper) became apparent shortly into the evaluation period, the evaluator's recommendation to "clean" the register before randomisation met some resistance.[3] There was confusion about "who is making the decisions here". Clearly, a more well-defined, as well as a more flexible, decision-making structure was necessary to deal with these situations, particularly in view of the lack of experience in this field of evaluation among those involved.

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[2] These matters could for instance concern the amount of time and resources allocated to contacts with the unemployed and the employers respectively, or the rate at which new job seekers should be added to the experiment (and control) group.

[3] The register deficiencies primarily consist of late registration of events. For instance, job seekers who find jobs or leave the work force sometimes omit to inform the employment office. Using register data to identify the target populations thus involves the risk of wrongly identifying employed persons as unemployed.

Bureaucratic resistance

Bureaucratic resistance, i.e., a lack of cooperativeness among those administrating the experiments, is probably the greatest threat to a successful experiment. Experience shows that routines implemented without the necessary approval risk being poorly executed (see Björklund & Regnér, 1996). Due to the risk of resistance, or sometimes for reasons of cost, designing the best possible experiment could involve specifying arrangements not optimal from a theoretical point of view. Finding compromises, balancing the expected returns in terms of a higher quality evaluation against the risk of lower acceptance among those carrying it out is an important task for an evaluator. The following are two examples of concessions made in the experiments conducted.

First, as regards the register deficiencies, an outreach procedure where target group members were contacted before randomisation would have been the most efficient way to reduce the occurrence of no-shows. This way, current status could have been checked and the register information corrected before start. However, such a routine would have involved extensive administrative procedures before each new enrolment and would have been costly, not only through a lower acceptance for the experiment, but also in terms of a lower flexibility in the enrolment procedure with fewer observations in consequence.

Second, the control group members being directed towards the employment office's regular services make interpretation of the experimental estimator more difficult since the effect from treatment relative to non-treatment is no longer identifiable. When controls receive good substitutes for a programme, the outcome difference could be zero although the effect from treatment relative to no treatment is positive. The problem of substitution is endemic in both experimental and non-experimental evaluation. However, obstructing the controls from receiving alternative training, in this case their normal services, would undoubtedly have raised ethical objections, not only among the employment officers.

Compliance

In contrast to the substitution problem, compliance is specifically related to experimental evaluation. The problem of compliance occurs when people assigned to participate either do not show up ("no-shows") or drop out of the programme evaluated. This dilutes the experimental estimator because the difference in treatment between the experiment and control group is reduced.

Certainly, unlike researchers in fields such as medicine and biology, a researcher conducting a social experiment cannot fully control the level of treatment among either experiment or control group members. If the choice of participation is related to the entitlement of compensation from the social insurance system, for instance UI, at least there are strong incentives to comply with the programme assigned. Still, compliance problems can arise for several reasons. For example, if register data are used to identify the population to which the programme is targeted, observations can be lost due to register deficiencies. Second, if there is a waiting period between randomisation and programme start (or rather between informing the experiment group members of the randomisation outcome and programme start), some observations are almost inevitably lost due to people moving or changing status (for instance from unemployment to employment). In the more unusual case where the programme is voluntary, i.e. when rejecting treatment does not involve UI sanctions, non-compliance should be regarded as an outcome of the programme. Here, the policy parameter of most interest is the effect of the "intent-to-treat" rather than the effect of participation. From an evaluation perspective, the lack of compliance in this case constitutes no "compliance problem".

In the second essay – evaluating the effects of offering job seekers Internet services – the experiment and control group members were not informed of the randomisation outcome until the day of programme start. With no waiting period, those leaving job-seeker status between randomisation and programme start could be eliminated because this outflow can be assumed to be unrelated to the randomisation outcome. However, with the programme being voluntary, a large amount of the experiment group members chose either to not show up (47%) or to drop out after receiving only a small treatment dose. Although the policy-relevant intent-to-treat parameter was still retrieved, this estimator was not very informative to the group of prospective participants as to whether or not to apply for participation. For this group, the effect of actual treatment, or the "treatment-on-the-treated" parameter, had been of more interest.

The last paper explicitly deals with the compliance problem generated by the waiting period. More specifically, the incentive effects of being referred to active placement efforts were investigated. In order to isolate the effect of this particular type of compliance problem, I must account for other types of compliance problems in the analysis. Since the programmes of interest were mandatory, this was not an issue here. However, using flawed data when attempting to identify target populations caused some problems. In particular, employed persons were wrongly identified as unemployed. However, by using alternative data (UI-payment data) in performing the analyses, the impact of register deficiencies on the results was neutralised.

Concluding remarks

Reporting some experiences from conducting the experiments presented in two of the essays, I have especially pointed to some flaws in the implementations such as too short preparation period, ambiguous decision-making policies, and various kinds of compliance issues. In this respect, the lesson from these experiments is similar to the experiences from earlier experiments, showing that the design and the interpretation of experiments, in practice, are more difficult than it may first seem.

However, the benefits from experiments as opposed to non-experimental methods are undeniable. First of all, correctly performed, a simple comparison of mean outcomes generates a consistent estimate of the programme impact on those participating. Second, the experimental design with a randomly assigned experiment and control group is easy for policymakers to understand. Finally, experiments provide valuable opportunities to assess the performance of various standard non-experimental methods.

With this in mind, the perhaps most useful finding is that despite the flaws, parameters providing a basis for policy decisions have been retrieved. In that respect, these experiments serve as good examples for future experiments.

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ESSAY 1

Effects of Changes in the Unemployment Insurance Eligibility Requirements on Job Duration - Swedish Evidence[†]

Pathric Hägglund[‡]

Abstract

This paper investigates the impact of the unemployment insurance (UI) entrance requirement on employment duration among earlier unemployed in Sweden. I exploit changes in the rule taking place in 1994 and 1997 to study behavioural adjustments in the timing of job separation between 1992, 1996, and 1998. Performing across-year analyses, I find evidence of clustering of job exits at the time of UI qualification. By using predicted hazard rates for each week, I calculate an approximate 2.9-week extension in average employment duration between 1996 and 1998 due to the 5-week prolonging of the entrance requirement.

Keywords: Unemployment insurance, eligibility requirement, duration analysis

JEL classification: J22, J65, J68

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1. Introduction

Most research about the impact of the unemployment insurance (UI) system has focused on the replacement ratio or the length of the entitlement period. These parameters have been embodied in job-search models to explain labour supply. But the UI system also consists of eligibility requirements that could also affect labour market behaviour. The entrance requirement (ER) is the number of weeks a person must work to become eligible for UI benefits. To what extent does the ER influence employment duration, that is, the time period in which the person is employed?

Several studies, among them Cousineau (1985) and Kesselman (1985), note that such a connection may exist on the employee side.[1] Kesselman notes that "there are... some workers in all industries and regions who prefer a lifestyle of intermittent work combined with regular unemployment spells subsidised by UI benefits." Such a work pattern fits the description of seasonal jobs. The variation in the extent of activity could be demand-driven (tourist industry) but more likely due to within-year fluctuations in production costs (construction work, farming, forestry, fishery).[2] Also, firms that are aware of the UI regulations know that the UI system will attenuate the workers' separation costs and can therefore employ workers for short periods to meet short-term needs. So the behaviour of rational agents on both sides of the labour market could account for the UI system. In these cases, benefit receipt primarily acts to redistribute income and leisure for actors "playing the system" — and not as an insurance.

Internationally, only a few studies have focused on the ER and its impact on employment duration. Baker & Rea (1994), Christofides & McKenna (1996), Green & Riddell (1997), and Green & Sargent (1998) used employment hazards to study UI incentives in spell duration. They all used data from the Canadian *Longitudinal Labour Market Activity Survey*

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[1] Both studies refer to the Canadian labour market.

[2] Edebalk & Wadensjö (1978).

to construct large samples of job duration. Christofides & McKenna found evidence of that a significant number of jobs were terminated when the ER was satisfied in 1986/87. Green & Riddell and Baker & Rea made use of a temporary extension in the ER from 10 to 14 weeks in 1990. Both found evidence of increased hazard rates the week of fulfilling the eligibility requirement. Green & Riddell estimated a 1.5-week increase in the average duration of employment in regions with unemployment rates over 11.5%. Baker & Rea conclude that the effect that they observe may in part be due to the awareness of the UI system in Canada and Canadians' high degree of familiarity with the programme. So similar results should extend to countries in which the work force has knowledge about UI. They also argue that UI-programme awareness will be highest in industries or regions with employment instability. The reason is that frequent unemployment spells distribute information about the UI system among the work force. Finally, Green & Sargent found substantial UI-related impacts on the job-spell hazard rate in seasonal but not in non-seasonal industries. The effect of the ER extension on average job duration is positive for seasonal jobs. Apart from high unemployment regions, however, the effect is very small.

In 1996, the Swedish UI system required that to qualify for benefits, applicants must have worked 5 calendar months within a 12-month period. [3] In July 1997, this rule was changed to 6 calendar months. The reason for extending the ER was that the Swedish government wanted applicants to have a closer affiliation with the labour market in order to receive UI compensation. So the change is primarily directed toward people outside the UI system — those who have not yet satisfied the work requirement a first time. But the extension also affects job duration in general because all of those, who initiate job spells, have the incentives to fulfil the minimum requirement. The main object of this paper is to investigate the ER's influence

[3] In Sweden, a first-time applicant must work to qualify for benefits. A second-time (or more) applicant could, up until 2001, qualify through participating in labour market programmes.

on the timing of job exits among those inside the UI system, i.e., those who have fulfilled the ER at least once. I exploit changes in the rules taking place in 1994 and 1997 to study behavioural adjustments in the timing of job separation in 1992, 1996, and 1998 respectively. To establish the length of employment spells, I use unemployment register data and information on the duration between the end of one unemployment spell, and start of another.

Using a flexible piece-wise constant exponential hazard model to pick up weekly job exits, I find evidence of an increased job turnover rate at the time of fulfilling the ER in all three years. The approximate 5-week extension of the ER in 1997 generated an estimated 3-week prolonging of the average employment spells between 1996 and 1998. Restricting the analysis to one sector and one region, each characterised by relatively large circular flows between jobs and unemployment (and likely high awareness of the UI system), indicates that changes in the ER is particularly important in industries and regions with seasonal pattern in the production process.

The next section describes Sweden's UI system and explains the ER for all years studied. The following section presents a simple, static, labour supply model. This serves as theoretical motivation in which the laid-out UI incentives predict job-termination clustering at the minimum number of required weeks of work. Section 4 contains some descriptive statistics concerning the degree of circular flow on the labour market and its importance in this context. Section 5 presents the data. Section 6 outlines the empirical framework, and Section 7 presents the results. The last section sums up and makes some concluding remarks.

2. Unemployment benefit in Sweden

The Swedish unemployment benefit system has two parts: i) *Basic insurance*, whereby compensation is available for those who are *not* members of a UI fund and are age 20, and ii) *Income-loss insurance*, whereby a person must have paid membership dues into a UI fund during a period of at least 12 months - the *membership condition* rule.[4]

From July 1, 1989 to July 1, 1994, applicants had to be employed 75 days (at least 3 hours a day) in 4 calendar months during the last 12 months in order to qualify for UI benefits. The 12 months are called the reference period. Between January 1, 1995 and July 1, 1997, the ER was a minimum of 80 days of employment (at least three hours a day) occurring during 5 calendar months in the 12-month reference period. In practice, the two rules were rather similar; the difference was that work (or equivalent) had to occur in one more month. In 1997, the requirement was changed to include work in at least 6 calendar months during a 12-month period and at least 70 hours each month. Or, a person had to work at least 450 hours during a composite period of 6 calendar months and at least 45 hours each month. The restriction implies that working in the interval January 15 – June 15 is enough to receive the UI provision from July 1, 1997. In practice, this is only a 5-month period, but because work has occurred during 6 calendar months, the ER is fulfilled. In the same way, 4 months was sufficient between 1996 and 1997, and 3 months was enough 1989-1994.[5]

In 1992, qualifying applicants received 90 per cent of their daily earnings; in 1996, 75 per cent; and in 1998, 80 per cent. The benefit period is 300 days (5 days per week, i.e., 60 weeks). An applicant, age 55, (age 57 from January 1998) is entitled to 450 days of benefits. To receive any compensation, the entrance requirement (ER) must also be fulfilled. From January 1, 1996, working is the only way to become eligible as a first-time ap-

[4] I do not describe the contents of the basic insurance in any detail because basic insurance recipients are excluded from the analysis later. The reason is the differing ERs for basic insurance receivers and for those who received income-loss insurance in 1996.

[5] For information in this section part, see SFS 1988:645, SFS 1994:1673, and SFS 1997:238. Details about the temporary rule between July 1 1994 and January 1 1995 are not given here.

plicant. For a second-time applicant, the re-qualifying condition applies. Then participation in labour market training, vocational rehabilitation, education financed by training allowance and military service, also enable an applicant to qualify. UI entitled who quit employment without a legitimate reason are disqualified from benefits for nine weeks.[6]

3. Theoretical motivation

In the following, I present a static labour supply model earlier used by Moffitt & Nicholson (1982) and Green & Riddell (1997) that outlines the effects of UI for workers.[7] The model assumes that unemployment is voluntary and that agents have limited time horizons in which they consider their budget opportunities and choose the number of weeks to be employed and unemployed, respectively. The individual maximises utility, which is a function of total net income (consumption) and leisure over the period. The model assumes a continuous distribution of preferences generating a corresponding distribution of employment spells.

Figure 1 shows the budget constraint for an unemployed person with UI benefits, depicted as (*CDE*), and without UI benefits (*CB*). I use a one-year time horizon to focus on seasonal unemployment where work is concentrated to a limited period each year. Prior to benefit exhaustion, an additional unemployed week reduces income by W-B, where $B=Wb$ is the weekly UI benefit. After exhaustion, at D, an additional week of unemployment implies the loss of income corresponding to the net potential weekly wage (W). *HMIN* denotes the minimum number of weeks a person

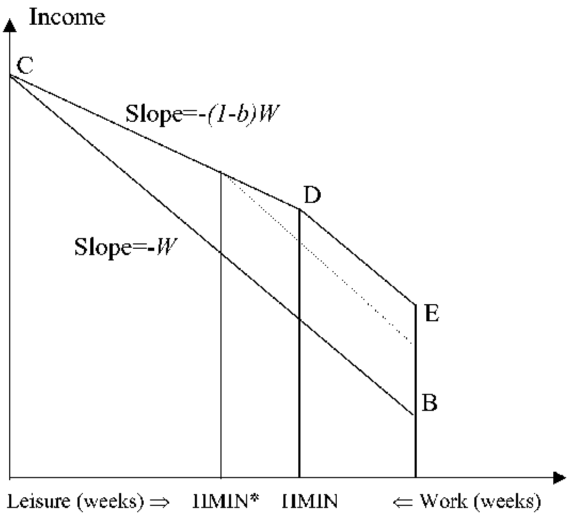
[6] For information in this section part, see SFS 1989:331, SOU 1996:150, SFS 1997:238, SFS 1987:226, and SFS 1995:1636.

[7] Christofides & McKenna (1996) present a model that includes the potential influence of the ER on both worker's and firm's behaviour. Their model presumes that quits and layoffs are behaviourally distinct. However, both in Canada and Sweden, individuals who quit risk a waiting period before receiving benefits. It is therefore reasonable to expect implicit contracts between workers and firms as the main source of ER effects.

must work to become entitled for UI benefits. Two particular responses suggest job-termination clustering at *HMIN*:

1. Many of those who in the absence of UI wish to work less than *HMIN*, would in the presence of UI want to work additional weeks to qualify for benefits. This primarily concerns those with spells a few weeks short of the ER and not people ending jobs well before.
2. Many of those who, in the presence of UI, would work beyond *HMIN*, would in the presence of UI face both income and substitution effects that imply a reduction in work to *HMIN*.

Figure 1: Budget constraint faced by an unemployed, 52-week horizon



A change in the required weeks of work is expected to shift the mass point in the figure from *HMIN* to $HMIN^*$. Given an extension of four weeks, the return for a person at the initial kink that adjusts to the new ER is $4W+(x\bullet 0.8\bullet W)$, where x is the number of unemployment weeks. Depending on the distribution of individual preferences, some people will also reduce their labour supply or choose to withdraw from the labour force. The height of the new mass point and the effect on average employment duration is hard to predict.

4. The circular flow on the labour market

This section focuses on the unemployment dynamics in Sweden between 1994 and 1997. In Table 1, the first row reports the total number of unemployment weeks in each year. The second row gives the number of weeks attributable to first-time unemployed — either receiving benefits or not. The latter group is a target group in the government's requirement for more work in order to receive compensation. The contribution from this particular group to the stock of unemployment weeks is halved over the studied years (from 15.4 to 7.6%). Dividing the first-time registered into UI receivers and non-UI receivers respectively (rows 3-6), the reduction is derived from the former group. Note also the average unemployment spell in this group is 6-8 weeks more extensive than the average spell in the group of non-receivers, and that the average spell length for both groups is gradually diminishing.

The table also provides an estimate of the degree in which unemployment is attributable to persons who were employed for a relative short period (at least twice between 1994 and 1997) and were also openly unemployed the remaining days of a 360-day period (rows 7).[8] This is the work pattern that we would expect among seasonally unemployed, where jobs are concentrated to a certain period each year. The share is stable around one per cent which indicates that this could be a relatively small problem on aggregate. Including repeated participation in labour market pro-

[8] Only start of the employment period is restricted to the particular calendar year. So the number of unemployment weeks in these rows only roughly refers to the particular calendar year.

grammes (LMPs), seasonal unemployment amounts to only 3.6-4.1 per cent of aggregate unemployment (row 9). The relative strict definition of circular flow keeps the measures down.

Rows 10 and 12 report the number of unemployment weeks derived from people entering jobs and LMP, respectively. The decreasing number preceding LMPs in row 12 is mainly due to a 10-week drop in the average unemployment period foregoing the programme starts between 1994 and 1997 (row 13). In turn, this affects the corresponding spells of unemployment before entering a job (row 11), because the time for job search is reduced. The shorter unemployment periods need not influence the magnitude of circular flow. But if an unemployed person is encouraged to take more temporary jobs (or to take jobs of short duration) to avoid programme participation, the circular flow between jobs and unemployment could increase. Figure 2 shows the elapsed time before a person who left unemployment for a job returns to unemployment. The job duration in 1997 is significantly shorter than in 1994.[9,10]

[9] The test performed is a log-rank test (Allison, 1995); the test statistic is distributed as $\chi^2_{(1)}$ and takes a value of 70.8.

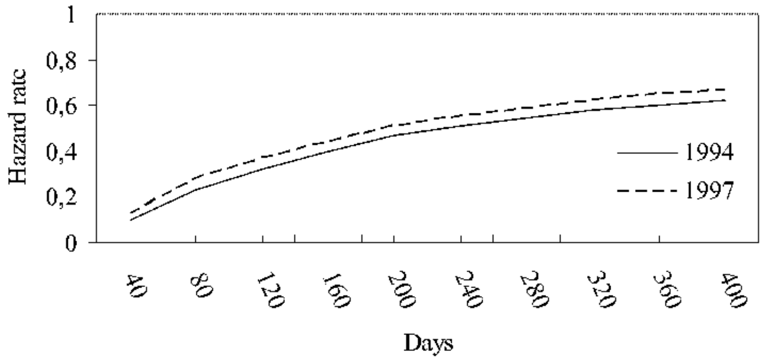
[10] Figures A1 and A2 in Appendix A illustrate that recurrent unemployment is more frequent in certain industries and regions. Comparing regions, local labour markets tend to have a higher circular-flow level. With the seasonal aspect in mind, this is no surprise because these markets are located in the northern part of the country where the winter season affects the job pattern. Among job categories, manufacturing and mining is above average while administrative work is well below the same. Farming, forestry, and fishery have a high share of circular-flow behaviour (9-12%) due to extreme working conditions. Figure A2 does not depict theses industries.

Table 1: Total unemployment weeks 1994-97 allocated on different types of unemployment, 1000s weeks. Numbers in parentheses show the share of total number of unemployment weeks in each year (row 1)

	1994	1995	1996	1997
1) Total number of unemployment weeks in a calendar year.	22,478	22,038	20,715	18,723
2) Total number of unemployment weeks for people registered as unemployed for the first time since 1991.*	3,417 (15.4%)	2,462 (11.2%)	1,756 (8.5%)	1,421 (7.6%)
3) receiving UI compensation	2,081 (9.4%)	1,385 (6.3%)	952 (4.6%)	571 (3.0%)
4) mean duration of unemployment spells (weeks).	21.7	20.6	18.5	17.1
5) not receiving UI compensation	1,082 (4.9%)	934 (4.2%)	707 (3.4%)	826 (4.4 %)
6) mean duration of unemployment spells (weeks).	14.8	14.1	10.3	9.0
7) Total number of unemployed weeks for people who, at least twice in the years 1994-97, worked for 3-9 months (composite time) and were unemployed the remaining days of a 360-day period.	234 (1.1%)	272 (1.2%)	272 (1.3%)	195 (1.0%)
8) Total number of unemployed weeks for people who, at least twice in the years 1994-97, participated in a LMP 3-9 months (composite time) and were unemployed the remaining days of a 360-day period.	600 (2.7%)	628 (2.8%)	555 (2.7%)	470 (2.5%)
9) Total number of weeks of circular flow (7+8).	833 (3.8%)	900 (4.1%)	827 (4.0%)	665 (3.6%)
10) Total number of unemployment weeks in which a person with UI compensation enters a job.	5,226 (23.6%)	4,538 (20.6%)	4,120 (19.9%)	3,842 (20.5%)
11) mean duration of unemployment spells (weeks).	14.3	13.6	12.5	10.9
12) Total number of unemployed weeks where a person with UI compensation enters a LMP.	6,051 (27.3%)	5,906 (26.8%)	4,896 (23.6%)	3,701 (19.8%)
13) mean duration of unemployment spells (weeks).	21.5	18.6	15.1	11.8

Notes: (1) The sample size is 5% of the population, so all measures are multiplied by 20 to get estimates at the level of the population. (2) The sample includes individuals between ages 18-65. Source: Own computations from the Swedish Labour Market Board's longitudinal data set. *: This was the first year of the longitudinal data base.

Figure 2: Duration of employment before returning to unemployment in 1994 and 1997



Note: (1) The sample size is 5% of the population, so all measures are multiplied by 20 to get estimates at the level of the population. (2) The sample includes individuals between 18 and 65. Source: Own computations from the Swedish Labour Market Board's longitudinal data set.

5. The data

Studying employment spells I use unemployment data and the duration between the end of one unemployment period, and the start of another. The database *Händel*, administered by the Swedish Labour Market Board (SLMB), consists of continuous information about every unemployment and programme spell of all people registered at the employment offices from August 1991 and onward. *Händel* also includes individual characteristics such as gender, age, education, desired profession, experience in desired profession, citizenship, county, and disability. The unemployment exit cause is also available. The database contains no employer-specific information. I use three separate samples of individuals who left unemployment for jobs in the years 1992, 1996, and 1998. From the database *AK-stat*, administered by the UI funds, I match on data on UI compensation type and previous income. These data are, however, only available for the years 1996 and 1998. For 1992, I use *Händel* data to establish whether a person is eligible for UI benefits or not.[11]

Lacking exact data on employment spells could cause bias calculating the days of work. For instance, if a person initiates an education spell after a few weeks of work, employment duration is upward biased. For that matter, I exclude the youngest age group (18-24), where students are overrepresented. Another reason for excluding this group is that students tend to have short jobs in the summer, which are of less interest in this study.

Another potential source of bias is the fact that this study is limited to single composite periods of work. As stated earlier, the ER can be satisfied through one, single, work period and several shorter periods. A person starting an employment spell with 10 insured weeks only needs 5 more weeks to satisfy the ER in 1992. Because I only include one observation (employment spell) per person in each sample, I assume that individuals enter employment with no accumulated insured weeks. This leads to underestimation of the true time in employment.[12] There are two arguments for this restriction. First, besides working, LMP-participation also entitles to benefits. Combining programmes and work to fulfil the ER is common behaviour among the repeatedly unemployed. This study focuses on the relationship between employer and employee, and restricting to composite periods seems justified because I can then distinguish between workers and programme participants. Second, Green & Sargent (1998) find that ER effects mainly occur in seasonal industries. This suggests that workers, who take advantage of the system, work the exact number of weeks in one, sin-

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[11] Thoursie (1998) found inconsistencies comparing those registered as UI receivers in *Händel*, and those actually collecting benefits in *AK-stat*. See also study 3 in this dissertation.

[12] If a person satisfies the sample criterion more than once in a year and thus has multiple employment spells, the included observation is randomly selected. I do so to avoid systematic differences in job duration within a particular year. It is plausible that employment spells initiated in the summer are shorter than jobs starting in other months.

gle spell — rather than in several shorter periods across the entire reference period. [13]

The analysis only includes those entitled to income-loss insurance in the unemployment period preceding the job spell. This further accentuates the focus on people who have earlier working experiences and the habit of "playing the system". I also restrict to persons with Swedish citizenship. Finally, in contrast to Green & Sargent (1998), I have no explicit information about seasonal and non-seasonal jobs, only on regular and temporary jobs. However, since jobs of various spells occur under both definitions, I choose to not distinguish between different types of registered jobs. Spells that did not end before May 31 1999 are censored.

5.1 Identifying the initial week of eligibility

We must find out whether or not a person has worked long enough to fulfil the UI requirement. The working requirement in 1992 involved 75 days of work in 4 calendar months. Because 75 days (15 weeks) always includes 4 calendar months, all job spells of 75 days meet the ER. In 1996 and 1998, the required number of calendar months in which work must occur implies a variation in the ER. Initialising a spell early in the month calls for additional weeks of work when trying to reach the fifth (1996) or sixth (1998) month. Table 2 illustrates this. Register information about the job-start dates is available, so this variation is considered in the analysis. Note that in 1996, one day (three hours) of work in one month was enough to take that particular month into account when fulfilling the ER. The 45 hours/month requirement in 1998 creates a 4-week spread assuming that people work ordinary weeks (5 days, 40 hours). Depending on job type and

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[13] When the reference period is determined, time when the applicant has been prevented from working due to: 1) certified illness, 2) military service, 3) labour market training, 4) vocational rehabilitation, or 4) training for which training allowance can be received, are excluded. So the reference period is generally longer than 12 calendar months.

industry, a person could fulfil the hours/month requirement in two days. Because the data lack exact information of the job spell, the hours/month specification in 1998 makes the identification of the first week of eligibility less reliable.

Table 2: Initial week of UI eligibility in 1992, 1996 and 1998, by day of start of employment

1992		**1996**		**1998**	
Start date (day)	HMIN92 *(Weeks)*	*Start date (day)*	*HMIN96 (Weeks)*	*Start date (day)*	*HMIN98 (Weeks)*
1-31	16	1-9	18	25-28	25
-	-	10-31	17	29-1	24
-	-	-	-	2-11	23
-	-	-	-	12-18	22
-	-	-	-	19-24	21

5.2 Sample characteristics

The original samples represent 40 per cent of the unemployment spells ending with the individual leaving for jobs in each year. All spells longer than 30 weeks and/or in progress as of May 31, 1999 are censored. Employment spells ending in ways other than unemployment are also censored. A favourable labour market situation thus implies a larger amount of censored spells. This is reflected in Table 3 where 1996, the year with highest aggregate unemployment, reports the lowest proportion of censored spells.[14]

The distribution of employment duration is clearly affected by the distance to the stop date in 1999. Disregarding the third quintile, the 1996 spells are generally shorter compared to the other years. This corresponds

[14] Aggregate unemployment was 4.8%, 8.1%, and 6.5% in 1992, 1996, and 1998, respectively.

to the lower share of censored spells in 1996. The proportions of females, people living in big cities, individuals with university experience, and spells initiated in the summer months are all rather constant among the years.

Table 3: Sample characteristics 1992, 1996 and 1998

	1992	1996	1998
Number of spells	51,632	49,102	46,281
% Censored	52.5	43.9	53.3
% Female	44.9	46.5	46.1
Duration of employment spell (days):			
25% Quintile 1	84	63	71
50% Median	201	154	183
75% Quintile 3	602	370	293
Age (average)	36.5	37.6	38.0
% living in big cities	41.1	39.6	40.3
% experience of university	19.0	16.8	17.1
% spells initiated in June-August	30.7	35.2	37.5

Source: Own computations from the Swedish Labour Market Board's longitudinal data set.

6. Empirical framework

To study the job spells, I use the piece-wise constant exponential hazard model for each of the three samples (see Lancaster 1990). The baseline hazard of this model is flexible and does not follow a specific distribution. Employment duration can enter through weekly dummies picking up the theoretically predicted spikes in the employment hazard. Assuming that several background factors have a multiplicative effect on the hazard rate, the general specification is:

$$\log \theta(t) = x'\gamma + \sum_{m=1}^{M} \beta_m d_m(t) \qquad (1)$$

where $\theta(t)$ is the employment hazard, x is a vector of explanatory variables with corresponding coefficient vector γ, d_m are indicators of the time interval (week) into which t falls, i.e., $d_m = 1$ if and only if t is in the m^{th} interval. β_m is a coefficient vector.

As x-variables, I use gender, age, educational level, desired profession, experience in desired profession, county type, month in which the spell begins, previous unemployment, regional unemployment, and past earnings (from job previous to this).[15] Duration is entered through a step function with separate dummy variables for each of the first 30 weeks.

In 1992, there was no variation in the ER due to when in the month the job started. So a potential ER effect is captured by the dummy variable corresponding to the 16th week in the step function (β_{16}), which is the initial week of eligibility that year.

In 1996 and 1998, the variation in the first entitlement week (see Table 2) makes it necessary to distinguish between the general flow back to unemployment, represented by the step function, and the specific consequence of the UI fulfilment. I do this by including (besides the step function) a separate time-varying variable that accounts for information about start date. In 1996, $HMIN_{y=96}^{r=96}$ (y refers to the particular year studied, and r denotes the year of the UI rule) then equals 1 in week 17 or 18, and zero otherwise. In the same way, $HMIN_{y=98}^{r=98}$ takes the value 1 a particular week between 21-25 and zero in all the others.

Extending equation 1 with this time-varying variable gives:

$$log\,\theta(t) = x'\gamma + z(t)\lambda + \sum_{m\ 1}^{M} \beta_m d_m(t) \tag{2}$$

[15] Information on regional unemployment consists of yearly averages of unemployment at the county level. Previous unemployment refers to the time as openly unemployed until job start.

where the middle term is the time-dependent dummy and the corresponding coefficient. Note that the time-invariant x covariates determine the hazard level for a given set of characteristics. The baseline hazard together with $z(t)$, in which the UI variables capture the ER effects, deals with the variation over time. The individual variation in the UI requirement in 1996 and 1998 thus helps to separate these two types of duration dependence.

6.1 Within-year testing of ER effects

Some factors suggest that finding an ER effect is more complex than restricted to a spike at $HMIN$. Due to the single-spell restriction in this study, those who initiate spells with insured weeks become eligible for benefits before $HMIN$. This makes the exit rate pattern in the weeks leading up to $HMIN$ hard to predict. Also, timing job exit to a certain week is difficult. Some people may even prefer timing their separation a few weeks above the $HMIN$ to insure against involuntary absence from work — illness, for example. Depending on the degree of risk aversion in the population, the hazard rate after $HMIN$ could exceed the exit rate at $HMIN$. Finally, a drop in the hazard immediately after UI fulfilment also indicates a behavioural effect. So an ER effect could show as an increase, or a drop, in the hazard rate at $HMIN$ or in the weeks surrounding $HMIN$.

To study the exit rate in the weeks around $HMIN$, I construct variables that correspond to the average of exit rates 3-5 and 1-2 weeks before the ER, and 1-2 and 3-5 weeks after the ER. In 1992, with no variation in the ER, this implies reconstructing the step function in equation 1 instead using aggregate dummies for the weeks 11-13 (β_{11}), 14-15 (β_{14}), 17-18

(β_{17}), and 19-21 (β_{19}). For the remaining weeks (1-10, 16, and 22-30), I use single dummy variables.[16]

In 1996 and 1998, when variation in the ER is present, I specify separate time-varying variables that correspond to an average of $HMIN-(3-5)$, $HMIN-(1-2)$, $HMIN+(1-2)$ and $HMIN+(3-5)$ for each year. The step functions in 1996 and 1998 are specified as single dummy variables up to 30 weeks.[17]

To summarise, the equation estimated for 1992 involves no *z(t)* variables. Instead, β_{16} captures the flow back to unemployment the first week of eligibility. To evaluate the differences in exit intensity the weeks around the week of eligibility, I test the hypotheses in Table 5.[18] An ER effect suggests that for a specific year at least one of these hypotheses is rejected. This corresponds to the above discussion concerning increasing and decreasing hazard rates around $HMIN$.

[16] For 1992 I thus estimate:
$$\log\theta(t) = x'\gamma + \sum_{m=1}^{10}\beta_m d_m(t) + \beta_{11}d_{11} + \beta_{14}d_{14} - \beta_{16}d_{16} + \beta_{17}d_{17} + \beta_{19}d_{19} + \sum_{m=22}^{30}\beta_m d_m(t).$$

[17] For 1996 and 1998 I thus estimate:
$$\log\theta(t) = x'\gamma + \lambda_1(HMIN_{-(3-5)}) + \lambda_2(HMIN_{-(1-2)}) + \lambda_3(HMIN) + \lambda_4(HMIN_{(1-2)}) + \lambda_5(HMIN_{(3-5)}) + \sum_{m=1}^{M}\beta_m d_m(t)$$, for 1996 and 1998 respectively.

[18] A 1-degree-of-freedom Wald chi-square statistic is calculated by the following formula: $(b_1-b_2)^2/[s.e.(b_1)]^2+[s.e.(b_2)]^2-2*(cov[b_1,b_2])$, where b_1 and b_2 are the β-estimates.

Table 4: Tests of the transition rates from job to unemployment between weeks in 1992, 1996 and 1998

Test	1992	1996	1998
$coeff(HMIN-(3-5))-$ $coeff(HMIN-(1-2))$	$(\beta_{11})-(\beta_{14})$	$coeff(HMIN_{y-96}^{r=96}-(3-5))-$ $coeff(HMIN_{y-96}^{r-96}-(1-2))$	$coeff(HMIN_{y-98}^{r=98}-(3-5))-$ $coeff(HMIN_{y-98}^{r-98}-(1-2))$
$coeff(HMIN\ \ (1\ \ 2))=$ $coeff(HMIN)$	$(\beta_{14})=(\beta_{16})$	$coeff(HMIN_{y-96}^{r-96}-(1-2))-$ $coeff(HMIN_{y-96}^{r-96})$	$coeff(HMIN_{y-98}^{r-98}-(1-2))=$ $coeff(HMIN_{y-98}^{r-98})$
$coeff(HMIN)=$ $coeff(HMIN+(1\ \ 2))$	$(\beta_{16})=(\beta_{17})$	$coeff(HMIN_{y-96}^{r-96})=$ $coeff(HMIN_{y-96}^{r-96}+(1-2))$	$coeff(HMIN_{y-98}^{r-98})=$ $coeff(HMIN_{y-98}^{r-98}+(1-2))$
$coeff(HMIN+(1-2))=$ $coeff(HMIN+(3-5))$	$(\beta_{17})=(\beta_{19})$	$coeff(HMIN_{y-96}^{r-96}+(1-2))=$ $coeff(HMIN_{y=96}^{r-96}+(3-5))$	$coeff(HMIN_{y-98}^{r-98}+(1-2))=$ $coeff(HMIN_{y=98}^{r-98}+(3-5))$

6.2 *Across-year testing of ER effects*

An ER effect dispersed over the weeks surrounding the week identified as the initial week of ER fulfilment could be difficult to identify since no drastic spike or drop in the hazard rate need to be present. Performing across-year estimations make possible to identify deviations in the hazard *levels* in the weeks of interest.

Pooling data from different years involves the assumption of a common step function for both years and equal impact of covariates. Besides the parameter specifying the ER for the particular year investigated, a year dummy captures the differences in the labour market situations between the years. To identify the ER effect, an interaction term taking the value one when returning to unemployment at the time of the ER the particular year of the ER, is specified.

To give an example, when analysing the job exits at the ER in 1992 using 1992 and 1996 data, i.e., testing $coeff(HMIN_{y=92}^{r=92})\leq coeff(HMIN_{y=96}^{r=92})$, I use the following information:

- $\beta_{16}\bullet d_{16}$, where d_{16} is a dummy variable that takes the value 1 the 16th week.

- $\gamma \bullet Year92$, where $Year92$ is a dummy variable that takes the value 1(0) if a person initiates a job in 1992 (1996).
- $\delta \bullet (d_{16} \bullet Year92)$ is an interaction term that takes the value 1 in week 16 in 1992, or zero otherwise.

If $\delta > 0$ the 1992 hazard is above the 1996 hazard at the 16th week. If $\delta < 0$ the opposite holds. Similarly, comparing the exit rates at the time of the ER in 1996 correspondingly implies involvement of the 1996 ER. We then test $coeff(HMIN_{y\cdot 92}^{r=96}) \geq coeff(HMIN_{y\cdot 96}^{r=96})$.

7. Results

I proceed with the empirical analysis as follows. I begin by presenting the baseline rate of job-to-unemployment transitions and also the baseline rate of LMP-to-unemployment transitions for each year in Section 7.1. The LMP-to-unemployment analysis is interesting because LMPs re-qualified for benefits in the years analysed. With different ERs, we would expect mass point adjustments in the timing of programme exits. Section 7.2 reports the covariate effects on survival in employment. In section 7.3 the UI-related parameters are introduced and within-year analyses of job turnover in the weeks surrounding UI fulfilment are performed. Section 7.4 reports between-year analyses in job turnover with different UI rules, and Section 7.5 restricts the same analysis to one sector (farmers), and one region (Norrbotten). Finally, Section 7.6 put the ER extension in 1997 into some perspective estimating the effect on average employment duration.

7.1 Baseline job-to-unemployment and LMP-to-unemployment hazards

The flexible specification of the baseline hazard allows for many spikes for different reasons. Spikes can occur due to seasonality in the labour market and local employment initiatives that provide many jobs of fixed duration.

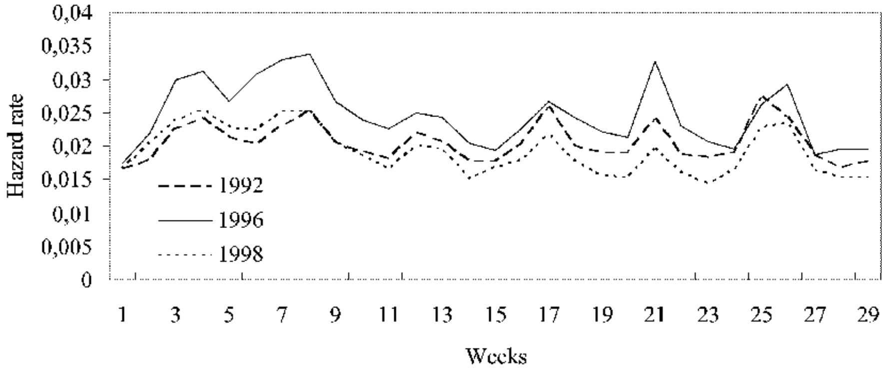
The simple baseline hazard does not distinguish between any of the potential sources of the spikes. Generally, an adjustment is apparent if the potential mass point corresponding to the UI condition moves from the old to the new minimum requirement.

The baseline employment-unemployment transitions in Figure 3 are generally higher in the earlier weeks, probably corresponding to the large number of temporary jobs in the summer. Both the 1992 and the 1996 hazards show higher frequencies of job separation at 17 weeks, which are possible ER effects in those years. The same holds for the increase at the 21st and the 25th week in 1998. The time pattern is quite similar for all years. The ratio between the 17-week hazard and the 21-week hazard is 0.82 for 1996 and 1.1 for 1998. An adjustment due to the latest change in the ER suggests a higher ratio for 1996 than for 1998. This creates doubts about how the increased exit rates at these particular weeks should be interpreted. It is quite possible that they are the result of something other than the ER. The spikes at 25-26 weeks could be caused by non-extended trial employments.[19,20]

[19] A trial employment is an employment where the firm after six months must decide whether to offer the employed a regular employment or not.

[20] Overall, the 1996 hazard is clearly above that of 1992 and 1998. This probably reflects the less favourable labour market situation. The null hypothesis that the survivor functions are the same for 1996 and 1998, for all *t*, is rejected at the 1% significance level. The test performed is a log-rank test (Allison, 1995), the test statistic is distributed as $\chi^2(1)$ and takes a value of 605.

Figure 3: Baseline transition rates from job to unemployment 1992, 1996 and 1998



Next, I explore UI fulfilment by participating in LMPs. In the years analysed, participation in a programme entitled to a fresh period of benefits. As a consequence, the programmes were often targeted towards unemployed near benefit expiration.[21] With the 1994 and 1997 changes in the ER, we would expect adjustments in programme duration between the 1992, 1996, and 1998 samples. The samples consist of 9,149, 5,953 and 3,993 programme spells initiated in 1992, 1996, and 1998, respectively.[22] Labour market training was the dominating programme in 1992. In 1996 and 1998, work-experience and workplace-introduction programmes replaced the proportion in labour market training, which diminished.

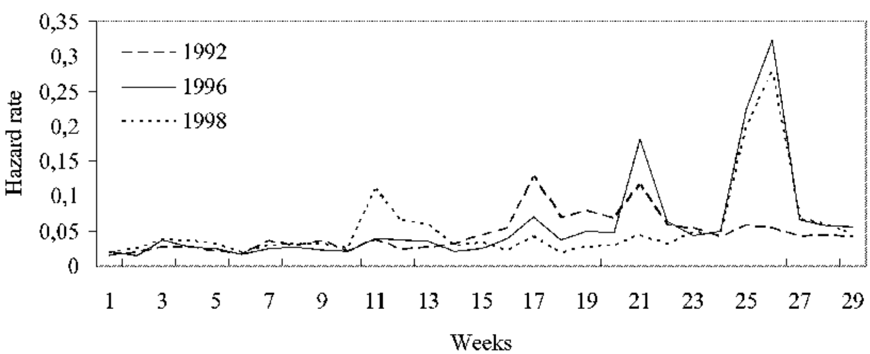
Figure 4 plots the baseline LMP-unemployment transitions. Compared with the job hazards, the patterns for 1992 and 1996 are rather similar from the 15th week and onward with spikes at 17, 21, and 25-26 weeks.

[21] See for instance Carling et al. (1996).

[22] Only one observation per individual is included in each sample. If a person has several different programme spells in the same year, the included observation is randomly selected. Multiple programme spells following each other are treated as one single observation. People not returning to unemployment after the spell are censored. See Table B1 in Appendix B for more details.

The lower transition rates at earlier weeks correspond to reduced individual opportunity of variation in the duration in LMPs. The 1992 hazard grows slightly toward the 16th week and peaks at the 17th week due to exits from public temporary jobs and labour market training. The depicted 1996 hazard shows a similar pattern up to 17 weeks, but the largest departures occur at 21 and 26 weeks as a result of ended work-experience programmes. The large exit rates at 21 weeks in 1992 and 1996 show that LMPs in some cases are shorter than the regular 26 weeks, but that they still, with a few weeks margin, satisfy the ER. In 1998, when this no longer holds, the hazard is flat, which suggests an effect of the new UI rules. Apart from the large exits in computer/activity centre at 11-13 weeks, the 1998 hazard stays at a low level up to 26 weeks, which is around the latest ER.[23]

Figure 4: Baseline transition rates from LMPs to unemployment 1992, 1996 and 1998



[23] Note that by repeating participation in a computer/activity centre, a person could become eligible for benefits.

7.2 Job-to-unemployment hazards using a model with covariates

Table 5 presents the estimates from the duration model for each year. The estimates give the effects on survival in employment. The results are generally rather intuitive. High education, big cities, previously well-paid jobs, on average, lead to longer working spells. Starting employment in January also increases the probability of relatively long spells. In contrast, these factors in general have a negative effect on job duration: age compared to the base group (25-34), certain job categories (manufacturing and mining, transport and communication, services, forestry, fishery and farming) and high local unemployment.

Table 5: Covariate effects on employment duration using a piece-wise constant exponential specification. Standard errors are within parentheses

	1992		1996		1998	
Constant	6.613	***	7.069	***	7.091	***
	(0.070)		(0.073)		(0.078)	
Man	-0.136	***	-0.110	***	-0.088	***
	(0.017)		(0.018)		(0.020)	
Age						
25-34	-		-		-	
35-44	0.032	*	-0.080	***	-0.074	***
	(0.016)		(0.016)		(0.019)	
45-54	0.064	**	-0.116	***	-0.178	***
	(0.020)		(0.019)		(0.021)	
55-64	-0.041		-0.222	***	-0.277	***
	(0.031)		(0.027)		(0.029)	
County						
Big city[a]	-		-		-	
Local labour markets[b]	-0.146	***	-0.161	***	-0.138	***
	(0.023)		(0.019)		(0.021)	
Other	-0.003		-0.055	***	-0.073	***
	(0.017)		(0.017)		(0.018)	
Education						
<Upper secondary, 2 years	-		-		-	
Upper secondary, 2 years	-0.124	***	-0.089	***	0.003	
	(0.017)		(0.018)		(0.019)	
Upper secondary, 3-4 years	0.009		-0.003		0.099	***
	(0.025)		(0.025)		(0.027)	
University	0.172	***	0.189	***	0.306	***
	(0.026)		(0.028)		(0.032)	
Desired profession						
Technical, scientific, liberal	-		-		-	
Health and social work	0.123	***	0.026		0.213	***
	(0.030)		(0.030)		(0.034)	
Administrative work	-0.003		-0.015		0.163	***
	(0.031)		(0.032)		(0.035)	
Commercial work	0.007		-0.034		0.066	
	(0.034)		(0.036)		(0.040)	
Farming, forestry and fishery	-0.424	***	-0.389	***	-0.354	***
	(0.038)		(0.037)		(0.041)	
Manufacturing and mining	-0.412	***	-0.461	***	-0.389	***
	(0.027)		(0.029)		(0.032)	
Transport and communication	-0.237	***	-0.278	***	-0.214	***
	(0.034)		(0.037)		(0.042)	
Services	-0.216	***	-0.262	***	-0.103	**

	(0.032)		(0.034)		(0.038)	
Regional unemployment[c]	-0.002		-0.031	***	-0.043	***
	(0.009)		(0.005)		(0.006)	
Month in which spell began						
January	-		-		-	
February	-0.005		0.097	***	0.142	***
	(0.040)		(0.037)		(0.042)	
March	0.021		0.226	***	0.257	***
	(0.038)		(0.036)		(0.039)	
April	-0.078	*	0.175	***	0.112	**
	(0.036)		(0.032)		(0.035)	
May	-0.417	***	-0.279	***	-0.245	***
	(0.033)		(0.030)		(0.033)	
June	-0.716	***	-0.794	***	-0.722	***
	(0.032)		(0.028)		(0.031)	
July	-0.599	***	-0.714	***	-0.649	***
	(0.038)		(0.031)		(0.036)	
August	-0.168	***	0.074	*	0.177	***
	(0.034)		(0.031)		(0.032)	
September	-0.295	***	-0.206	***	-0.175	***
	(0.035)		(0.033)		(0.036)	
October	-0.318	***	-0.336	***	-0.283	***
	(0.037)		(0.036)		(0.040)	
November	-0.457	***	-0.430	***	-0.421	***
	(0.037)		(0.038)		(0.041)	
December	-0.366	***	-0.377	***	-0.313	***
	(0.043)		(0.046)		(0.055)	
Unemployment duration[d]	-0.085	***	0.028		0.062	*
	(0.021)		(0.020)		(0.027)	
Income-based daily salary (SEK)	~		0.010	**	0.006	
			(0.004)		(0.004)	
Experience						
No experience	-		-		-	
Some experience	-0.036		-0.030		-0.067	*
	(0.026)		(0.028)		(0.031)	
Long experience	0.092	***	0.055	*	0.029	
	(0.025)		(0.026)		(0.029)	
Log likelihood value	-121,748		-119,804		-99,082	
Number of observations	51,632		49,102		46,281	

Significance levels: *<0.05, **<0.01, ***<0.001. Notes, (~): No available information. [a]: Refers to the counties of Stockholm, Göteborg and Bohus (later Västra Götaland), and Malmöhus (later Skåne). [b]: Refers to the counties of Värmland, Kopparberg, Gävleborg, Västernorrland, Jämtland, Västerbotten, and Norrbotten. [c]: County-specific yearly averages of the unemployment rate. [d]: Refers to periods of open unemployment before job start.

7.3 *Within-year estimations of ER effects*

Tables 6 presents the weekly hazard estimates at, and around, the weeks of fulfilling the ER in 1992, 1996 and 1998.[24] Note that the estimates surrounding the 1992 ER captures the general transition from employment to unemployment represented by the baseline hazard.[25] Again, due to the variation, the estimates around the 1996 and 1998 ER extracts from other forms of duration dependence and thus more explicitly focuses on ER effects.[26]

To illustrate the estimated ER effects, I plot the hazards suggested by applying the estimates to a flat baseline of 0.020 for 1992, 0.024 for 1996, and 0.019 for 1998 (Figures 5a-c). These are the calculated hazard averages for the first 30 weeks in each year. In Figure 5a, studying the hazard around the ER in 1992, the hazard decreases toward the 16th week and increases significantly the following weeks. This suggests a late ER effect due to difficulties in timing job separation to a certain week.

Turning to the ER in 1996, Figure 5b depicts a small upward trend toward the weeks of UI fulfilment in 1996 — based on the UI-related effects from Table 6. Although the $HMIN_{y=96}^{r=96}$ estimate is significant, the rise is not significant compared to the preceding period. The significantly positive exit rates in the weeks leading up to the ER could have been caused by individuals entering the employment spell with insured weeks. However, the lack of spikes creates doubts as to whether or not an ER effect exists.

The hazard surrounding the weeks of the 1998 ER in 1998 drastically increases in the two-week period before satisfying the ER, and drops 3-5

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[24] I only report the estimates of the UI-related parameters in the following. Please contact the author for the results of the full model estimations.

[25] The employment-unemployment flow during the first week of employment constitutes comparison in this analysis.

[26] However, the model specification opens for a possible multicollinearity problem between the time-varying and the step function variables. Through larger standard errors, this could affect inferences of tests including these estimates.

weeks after. This suggests some adjustments in the timing of job exits due to the ER.

Overall, the within-year estimations are somewhat ambiguous as to the effect of the ER. A clear job-exit pattern is difficult to find. This could be due to the result of the ER effects being dispersed over several weeks due to problems identifying the initial week of benefit entitlement, and/or difficulties in timing job exits to a certain week.

Table 6: Estimates of UI-related effects around the ER in 1992, 1996 and 1998, in each year. Standard errors are within parentheses

Variable	**1992**	**Wald´s test**	**1996**	**Wald´s test**	**1998**	**Wald´s test**
HMIN –(3-5) weeks	0.147*** (0.039)	0.24	0.353*** (0.068)	0.26	-0.376*** (0.065)	51.71***
HMIN –(1-2) weeks	0.130*** (0.043)	2.72	0.390*** (0.090)	0.78	0.141 (0.088)	1.72
$HMIN^{r=92}$ (β_{16})	0.050 (0.052)	32.38***	0.453*** (0.098)	1.43	0.025 (0.110)	0.21
HMIN +(1-2) weeks	0.321*** (0.042)	23.76***	0.370*** (0.096)	1.92	0.066 (0.096)	43.44***
HMIN +(3-5) weeks	0.149*** (0.041)		0.264*** (0.079)		-0.429 (0.085)	

Notes: No. of observations, 1992: 51,635; 1996: 49,102; 1998: 46,281. (1) Base controls include the covariates in Table 5 and a step function in duration. (2) Significant levels: *<0.10, **<0.05, ***<0.01. (3): Wald´s test is specified in note 18.

Figure 5a: Fitted hazard around the 1992 ER, in 1992

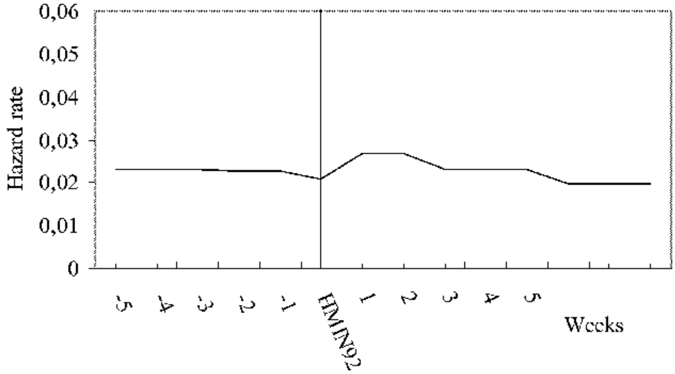


Figure 5b: Fitted hazard around the 1996 ER, in 1996

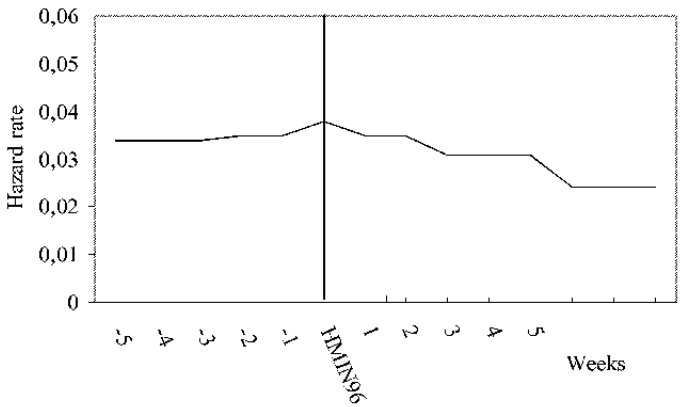
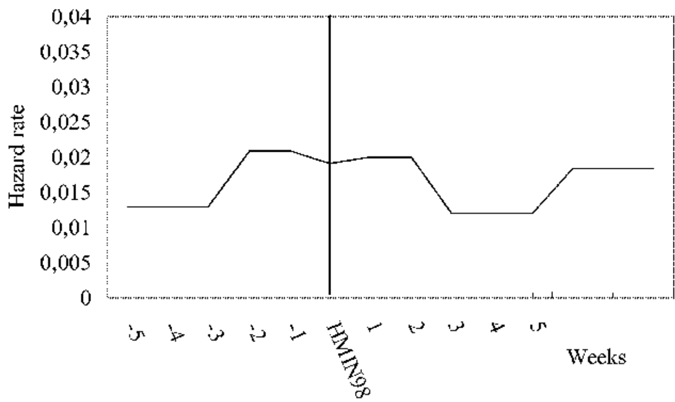


Figure 5c: Fitted hazard around the 1998 ER, in 1998



7.4 *Across-year estimations of ER effects*

The ambiguous results in the within-year analyses motivate further investigation of ER effects instead focusing on across-year comparisons of job exits for more reliable inference. I perform across-year analyses for all possible pair combinations of ERs (rows) and comparison years (columns), for both job turnovers at the exact week of satisfying the ER, and the following two weeks. The ER-effect estimates are reported in Table 7. A positive impact estimate suggests a positive effect of the ER in the particular combination of years investigated. Note however that effects dispersed over the weeks surrounding the ER could have some implications comparing two years where the ER has been extended. For example, if the ER affects the job exits in the subsequent weeks, the effect of the most recent change among the two years compared is downward biased. With late adjustments, effects corresponding to the oldest ER should be easier to detect.

The results generally support the delayed effect mechanism. Studying the 1992 ER, both the 1996 and 1998 comparisons suggest late significant shifts away from the old rules. Examining the 1996 ER, no evidence of an ER effect is found comparing with the job exits in 1992. The point estimates even suggest a lower (!) job turnover rate in 1996. Note however that the ER in 1992 and 1996 only differ by 1-2 weeks. Instead comparing with the exits in 1998, a significant positive impact estimate is reported, suggesting a 16 per cent higher exit rate in the weeks of the 1996 ER.

Table 7: Across-year estimations of ER effects at the week of ER fulfilment, and in the two-week period following the week of ER fulfilment

	1992		1996		1998	
	$HMIN_{y=92}$	$HMIN_{y=92}$ (1-2)	$HMIN_{y=96}$	$HMIN_{y=96}$ (1-2)	$HMIN_{y=98}$	$HMIN_{y=98}$ (1-2)
ER rule:						
1992	-	-	0.082 (0.059)	0.114*** (0.038)	0.003 (0.061)	0.104** (0.041)
1996	-0.086 (0.052)	-0.030 (0.040)	-	-	0.161*** (0.059)	0.040 (0.043)
1998	0.081 (0.069)	0.187*** (0.048)	-0.155** (0.067)	0.074 (0.050)	-	-

Notes: No. of observations, 1992/96: 100,734; 1992/98: 97,913; 1996/98: 95,383. (1) Base controls include the covariates in Table 5 and a step function in duration. (2) Standard errors are within parentheses. (3) Significant levels: *<0.10, **<0.05, ***<0.01. (4): Wald´s test is specified in note 18.

Finally, the 1998 ER seems to have caused an adjustment of the timing of job exits compared to 1992, at least studying the two weeks following ER fulfilment. The results from the 1996 comparison are more ambiguous. The significantly negative effect at the first week of UI eligibility suggests that the impact estimate captures a delayed effect of the 1996 UI rule. The recovery in the subsequent two-week period provides some indications of a late ER adjustment corresponding to the new rules. The lack of a more pronounced effect is possibly the result of the design of the 1998 rules, earlier discussed in Section 5.1. The rule involves a higher degree of uncertainty establishing the first week of UI entitlement.

Summing up the across-year comparisons, we find evidence of an enhanced unemployment risk at the time of meeting the working requirement for all of the three UI regimes investigated. While the 1996 ER generates an increased hazard in the first week of UI fulfilment, the 1992 and 1998 rules involve a higher job exit rate in the subsequent two-week period.

7.5 Analysing the ER effects in one sector and in one region

Green & Sargent (1998) discovered substantial UI-related impacts on the job hazard for seasonal, but not non-seasonal, jobs. In the following, I narrow the analysis to one occupational group (farmers) and one local labour market (Norrbotten), both characterised by a relatively high degree of recurrent unemployment in the labour force according to Section 4.[27] I restrict the analysis to a comparison between 1996 and 1998. The small samples inevitably produce effect estimates with large standard errors.

The results in Table 8 further underline the results of the main analysis. Distinct mass point shifts away from the 1996 ER in 1998 are identified for both farmers and UI receivers in Norrbotten. Adjustments in accordance to the new working requirement in 1998 are, similar to when analysing the full samples, difficult to establish. Although the small samples and large confidence intervals suggest a careful interpretation of the point estimates, it is interesting to note that the 1996 ER effects in the sub samples indicate impacts 7-8 times as large as the average impact. The results thus support the proposition that changes in the ER primarily affect sectors where repeated unemployment is relatively common and where the awareness of the UI system is relatively high.

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[27] Farmers are defined as individuals belonging to the farmers UI fund.

Table 8: Across-year estimations of ER effects at the week of ER fulfilment, and in the two-week period following the week of ER fulfilment, in one sector (farmers), and one region (Norrbotten)

	Farmers				**Norrbotten**			
	1996		1998		1996		1998	
	$HMIN_{y=96}$	$HMIN_{y=96}$ (1-2)	$HMIN_{y=98}$	$HMIN_{y=98}$ (1-2)	$HMIN_{y=96}$	$HMIN_{y=96}$ (1-2)	$HMIN_{y=98}$	$HMIN_{y=98}$ (1-2)
ER rule:								
1996	-	-	0.234	0.730*	-	-	0.855***	0.214
			(0.431)	(0.420)			(0.294)	(0.177)
1998	0.105	0.200	-	-	-0.043	0.053	-	-
	(0.552)	(0.413)			(0.241)	(0.205)		

Notes: No. of observations, Farmers: 1,411; Norrbotten: 4,500. (1) Base controls include a step function in duration, gender, age, education, desired profession (only in the Norrbotten analysis), experience in desired profession, previous unemployment, county-specific UR (only in the farmer analysis), county type (only in the farmer analysis), and month of employment. (2) Standard errors are in parentheses. (3) Significant levels: *<0.10, **<0.05, ***<0.01. (4): Wald´s test is specified in note 18.

7.6 Effects on average employment duration

To provide a measure of the size of the observed effects of the 1997 extension of the ER, I use a formula from Green & Riddell (1997) to estimate average employment duration using baseline and covariate estimates from the duration model where all covariates are set to their average values in each year. Average employment duration is calculated as,

$$E(emp) = \sum_{H=1}^{29} Hf(H) + \left[\prod_{H=1}^{29} (1 - h(H) \right] \left(29 + \frac{1}{h_{30}} \right), \tag{3}$$

where, $f(H)$ is the density function for employment duration based on the fitted hazard estimates, H is week and h_{30} is the hazard rate for the 30th week in 1998. For weeks beyond 30, I assume a constant hazard equal to the hazard rate for this particular week. Assuming a decreasing hazard, this may underestimate the actual average employment duration.

To predict hazard values for each week, I also include the estimates of the UI-related variables. We already know that employment spells in general were longer in 1998 compared to 1996 from Figure 3. Using this specification, the average duration increased from 60.0 to 63.8 weeks. In evaluating the effects from the extension, we wish to control for across-year differences in baseline hazards and individual characteristics. I could then restrict to the immediate effects of the change in the ER. To accomplish this, I replace the 1998 UI parameters, i.e., the parameters capturing fulfilment of the ER and the weeks surrounding ER fulfilment, by the 1996 UI parameters in the fitted hazard of 1998. The expected duration then drops from 63.8 to 60.9 weeks, creating a 2.9-week extension as a result of the altered ER. In the calculated extension, I make a reservation for the difficulty in confirming the initial week of eligibility, especially for the weeks surrounding the 1998 ER.

8. Conclusions

I investigate the effect of the UI entrance requirement (ER) on employment duration on the Swedish labour market in 1992, 1996, and 1998. I do so by exploiting changes in the ER in 1994 and 1997. The study is restricted to UI receivers, i.e., unemployed who have satisfied the ER at least once, and thus focuses on people with some working experience. It is important to be aware that an extension of the ER also has consequences on people who have not yet fulfilled the requirement a first time. By making the entry into the UI system more difficult, it is quite possible that social assistance increase expenditures.

Studying each year separately, I find no clear evidence of adjustments due to the ER in terms of distinct mass points of job terminations at, or around, the week of fulfilment. Several possible explanations have been introduced here; the lack of exact data on employment duration, the concentration on single spells in the analysis, and the problems in timing the job exit to one particular week.

However, instead analysing across-year differences, I find evidence of adjustment to the ER in all three years. By using predicted hazard rates for each week, I calculate an approximate 2.9-week extension in average employment duration between 1996 and 1998 due to the 5-week prolonging of the ER in 1997. Analysing the effects in one industry (farmers), and one region (Norrbotten), suggests that the ER extension primarily affected sectors where repeated unemployment, indicating seasonality in the production process, was relatively common.

In comparison with the Canadian studies, Green & Riddell (1997) concluded a 1.5-week extension between 1989 and 1990 due to a 4-week prolonging of the ER in high unemployment regions. Using individual and regional variation in the UI parameters, Green & Sargent (1998) find substantial adjustment of job durations to the UI requirements primarily in seasonal jobs. The effect on average job duration is marginal in general but increasing with regional unemployment rate. According to theory, the ER has little effect on the choices to end jobs well before the minimum requirement. Because an extension implies more weeks unaffected by the ER, the increase of jobs of short duration may offset the potential mass-point extension at higher weeks. Finally, similar to Green & Riddell, I examine only a short-term reaction. When people have fully adjusted to the new ER, the result may be different.

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Appendix A

Figure A1: Share of unemployment weeks for people who, at least twice in the years 1994-97, worked for 3-9 months (composite time) and were unemployed the remaining days of a 360-day period, by county

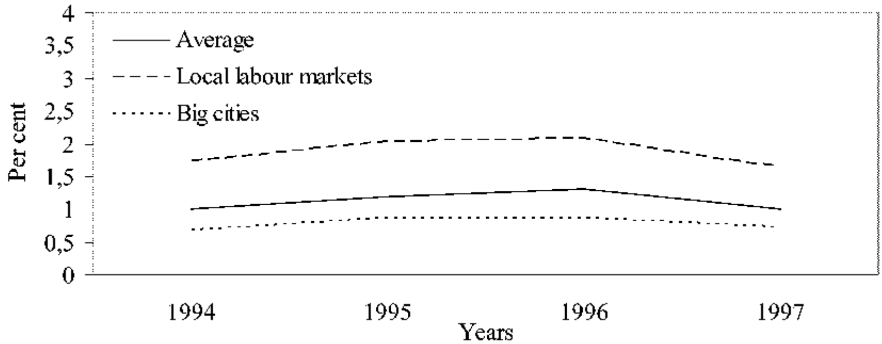
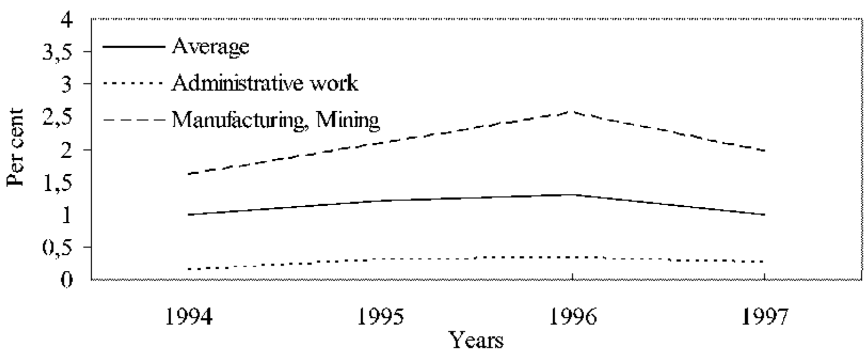


Figure A2: Share of unemployment weeks for people who, at least twice in the years 1994-97, worked for 3-9 months (composite time) and were unemployed the remaining days of a 360-day period, by job category



Appendix B

Table B1: Types of LMPs and their share 1992, 1996 and 1998

LMP (%)	1992	1996	1998
Recruitment subsidy	*	5.2	0.0
Youth traineeship	*	0.0	7.5
Start your own business	*	4.7	1.0
Public temporary work	27.8	4.8	0.0
Work experience programme	1.1	40.3	39.5
Trainee in temporary replacement pro-	6.2	4.1	0.0
Immigrant programme	#	#	1.6
Workplace introduction	*	13.1	11.6
Computer/activity centre	*	3.5	12.1
Labour market training	64.8	24.3	26.7

Source: 1992, 1996, and 1998 longitudinal data from the Swedish Labour Market Board. The samples include individuals registered as Swedish citizens that are between ages 25-65. The samples represent about 30% of the programme spells in 1992, 1996, and 1998. Notes: (*) From 1995, (#) The Workplace Introduction programme replaced the Immigrant programme in 1995.

ESSAY 2

Job-search Assistance Using the Internet - Experiences from a Swedish Randomised Experiment[†]

Pathric Hägglund[‡]

Abstract

This paper reports the experience from a randomised experiment offering voluntary job-search assistance on the Internet to job seekers at Swedish public employment offices. The purpose is to, i) investigate to what extent the evaluation design manages to avoid common difficulties in experimental evaluation, ii) assess the effect of the programme on the employment outcome, and iii) use the nonbiased experimental results as a benchmark evaluating the performance of frequent nonexperimental estimators. I find that the evaluation design successfully circumvents inherent difficulties in the experimental approach, such as ethical concerns, bureaucratic behaviour and randomisation bias. However, the voluntariness of the programme caused severe compliance problems in terms of both no-shows and drop-outs. This is accounted for by analysing the effect of the "intent-to-treat" (the policy parameter of most interest), which is close to zero. Studying the effects of various doses of actual treatment, using an nonexperimental instrumental variable model, I fail to reject the hypothesis of a zero programme effect. Finally, a methodological comparison suggests that standard nonexperimental techniques succeed in reproducing the nonbiased experimental results.

Keywords: Internet job search, policy evaluation, social experiment

JEL classification: C93, J64

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1. Introduction

This paper reports the experience of a demonstration programme offering voluntary job-search assistance on the Internet to job seekers at Swedish public employment offices. By using random assignment to these programme services, the study contributes to the sparse literature on experimental evaluation of labour market topics in Europe in general, and in Sweden in particular. The current case in fact represents the first Swedish experiment in this field since 1975.[1]

The non-random selection of programme participants constitutes a serious threat in estimating nonbiased policy effects. Random assignment is the statistical solution to this problem, since it balances the properties affecting the subsequent outcome between members of the experimental and the control group. However, experiments can create problems of their own. A careful evaluation design is necessary to avoid biases inherent in the experimental approach.

This paper investigates: i) how the experimental evaluation design succeeds in circumventing common difficulties in experimental assessment,[2] ii) the employment outcome from being offered, and receiving, the job-search club services respectively, and iii) which nonexperimental evaluation methods are likely to produce consistent results in evaluation situations similar to this one, that is, in the absence of an experimental design. The last analysis is performed through a comparison between the presumed nonbiased "intent-to-treat" impact estimate and those derived from using ex-post constructed comparison groups.

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[1] To my knowledge, apart from Delander (1978) on Swedish data, the only other experiments conducted in Europe are reported in White & Lakey (1992) in the UK, Torp, Raaum, Hernaes & Goldstein (1993) in Norway; Raaum, Torp & Goldstein (1994) in Norway; Van den Berg & Van der Klaauw (2001) in the Netherlands; Bratberg, Grasdal & Risa (2002) in Norway; and Rosholm & Skipper (2003) in Denmark.

[2] See for instance Heckman & Smith (1995) or Björklund & Regnér (1996) for a general presentation of these difficulties.

Several findings can be extracted from this study. First of all, the experimental design successfully avoids many experiment-related problems such as ethical concerns, bureaucratic behaviour and randomisation bias. The voluntariness of the services generated a large fraction of no-shows and early dropouts, which made the experimental estimator almost uninformative about the effect of actual participation. However, with the services being voluntary, the policy parameter of interest is the "intent-to-treat" parameter, which explicitly takes into account the possibility of not participating at all. The estimated intent-to-treat impact estimate is not significant and close to zero. Similarly, estimating the effect of different doses of actual treatment, using a nonexperimental IV technique, no significant programme effects are found. Finally, testing the performance of various nonexperimental estimators, these generate impact estimates close to the experimental estimate. This would suggest that the available data efficiently capture the mechanisms underlying the self-selection process.

The paper is divided into two parts. The first focuses on the programme contents and the experimental design, describing in detail the virtual job-search club services and the implementation phase. Data and descriptive statistics are presented as well as experimental and nonexperimental impact estimations on the transition to employment over a six-month period. The second part introduces frequently applied nonexperimental estimators and their identifying assumptions. I also report the performance of these estimators in reproducing the experimental results.

I The Experiment

2. The job-search club services

The Internet offers new opportunities for the public employment service (PES). Since 1995, several on-line placement services have been introduced in Sweden. The Vacancy bank, where employers advertise their job vacancies, had 450 000 visitors in April 2001. In 2001, the PES Internet services in Sweden were used every month by more than 550 000 individuals, which corresponds to approximately 15 per cent of the workforce. Current developments involve a higher degree of interactivity between job seekers and employment officers, which means that further dimensions in the field of traditional employment services are being added to the Internet services.

In the spring of 2002, a small committee at the Swedish Labour Market Board (SLMB), including myself as administrator of the experiment, was assigned to carry out a nation-wide demonstration programme investigating the possibility of pursuing job-search club activities on the Internet.[3] The results were supposed to provide the basis for a policy decision as to whether or not the services should be a permanent feature of the employment services. Of particular interest was the service's ability to improve effectiveness of the matching. The programme was tested on a group of voluntary job seekers. Anyone currently registered as a job seeker was welcome to apply for participation. This included openly unemployed, programme participants as well as employed persons looking for a new job. Since the services were offered on the Internet, no geographical restrictions were introduced.

No specific requirements were specified as regards the activity among the participants. Although recommended to visit the programme

[3] The local labour market board of Västra Götaland first introduced the services in October 2000.

every day, they had the opportunity to quit at any time without risking reduced UI compensation. The only prerequisite as a participant, besides being registered at the employment office, was to have access to a computer with email and Internet facilities away from the local employment office. This was crucial since the participants could not access the job-search club services at their local employment offices.

In contrast to many of the more expensive labour market programmes, evaluations of traditional job-search assistance generally show positive outcomes.[4] In Sweden, most evaluations conclude enhanced job chances, at least for measures targeted to subgroups of unemployed.[5] Traditional job-search assistance activities are expected to positively affect job transitions through an increased job-offer arrival rate. Under weak restrictions on the wage offer distribution, the effect from receiving more job offers is expected to dominate the negative effect from a higher degree of selectivity in choosing which job offer to accept.[6]

Similar to traditional job-search clubs, the concept of the virtual version was to teach job-seeking skills. The programme, which was executed by three full-time employed case workers (coaches) situated in Stockholm, provided guidance as to where and how to make contact with suitable employers. An important part of this was to help the participants to discover their own good qualities and to strengthen their self-confidence. Participants learned how to write job applications and CVs and how to behave during job interviews from an Internet based working material. The theoretical elements were combined with practical exercises whereby the participants received feedback from the coaches. The programme allowed interactivity among programme members and the benefits of group dynam-

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[4] See Martin & Grubb (2001) for an international review.

[5] See Calmfors et al. (2001) for a review of Swedish experiences.

[6] Van den Berg (1994).

ics. Organised group discussions and on-line chats were permanent features of the services.

A comparison between the traditional and virtual version of the job-search club services also reveals some important differences that could be of importance to the outcome. First of all, participants in the former receive services according to a predetermined schedule supervised by caseworkers. The activities in the latter involve working in an Internet environment, where the participants choose for themselves when, where from, and for how long they wish to be active. Secondly, instead of participants working in the presence of caseworkers and other participants, they are expected to work individually and away from the employment office itself.

3. Experimental design

The virtual job-search club services were carried out in the summer of 2002. The voluntary job seekers were randomly selected into two groups. One of the groups was offered the job-search club activities *in addition* to their regular services, whereas the other group was directed to the regular services at the employment offices. The access to this extra assistance was expected to have a positive impact on the participants′ job chances. Since participants were subject to the same basic treatment as nonparticipants, the tested services needed to provide positive programme effects in order to be economically motivated.

Applications were submitted in two enrolment periods with two corresponding start dates, May 15 and June 5. The first enrolment period, in which information about the services was available at all local employment offices in Sweden, took place between April 29 and May 10. The second enrolment period, in which applications could be submitted either at the employment offices or on the PES homepage, took place between May 21 and May 31. The programme ended on September 6 2002. For those offered the services, an early entry date allowed approximately three months' services.

The success indicator used is "exit to employment" during the six-month follow-up period (between May 15/June 5 and December 1 2002). In constructing this indicator, special attention is paid to those already employed at the start of the experiment. Only employment resulting in a move "upwards" in the ranking system counts as a successful outcome.[7]

An alternative choice of outcome measure is employment status December 1 2002. Compared to the exit indicator, this measure adds a quality dimension to the employments by taking into account the potential flow back to unemployment. However, since the unemployment register data (Händel) does not include information on employment status, we have to presuppose that the current employment status among those who exited from unemployment, and did not return, is equal to the cause of separation. Considering that the sample includes students looking for a temporary occupation, this indicator overestimates the true employment rate. Furthermore, using employment status at a certain date as a success indicator makes the results sensitive to the particular date chosen. Although I consider my chosen indicator to be superior, I report the results of this indicator in a footnote.[8]

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[7] The following employment-type ranking, based on unemployment register information, is applied to those already employed: 1) Regular employment, 2) Job-changer, 3) Temporary employed, 4) Part-time employed or employed by the hour. If a person exits to employment involving a higher employment-type ranking, the person counts as employed. If a person remains in the same employment-type category, or exits to an employment with a lower ranking, the opposite holds. Note that people in the highest ranked category are not included constructing the sample.

[8] A third possible measure comparing the performance of experiment and control group members is unemployment duration. This approach, however, involves relatively sophisticated analytical methods which would interfere with the otherwise simple and intuitive feature of the experimental approach.

Even though application for participation was voluntary, most of those offered the programme services did not immediately take action. To encourage participation, applicants were contacted by email and/or telephone, and were reminded of the service offer. They were also told that their password would expire on a certain date. Although this increased the participation rate, a significant fraction of the experiment group never took part in the activities at all. This is discussed further in the next section.

Table 1. Sampling scheme

	First enrolment (April 29-May 10 2002)[a]	**Second enrolment** (May 21-May 31 2002)[b]	**Total**
No. of applicants	346	497	843
of whom:			
Registered at the employment office	**265**	**371**	**636**
of whom:			
Experiment group	**140**	**203**	**343**
of whom:			
Participants	68	113	181
No-shows	72	90	162
Control group	**125**	**168**	**293**

Note: Corresponding start dates, [a]: May 15, and [b]: June 5.

A total of 843 valid applications were received in the two enrolment periods (Table 1). Forty-three applications were eliminated either because an invalid email address was reported or because the applicant had found a job before the start date. Another 164, not currently registered at an employment office, were also excluded. Of the remaining 636 job seekers, 343 were randomised into the experimental group that was offered the demonstration services. Of these, 181 (53%) visited the job-search programme home page at least once (henceforth, "participants"), while 162 (47%)

never visited at all (henceforth, "no-shows"). The control group, 293 persons, did not receive any service offer during the follow-up period, but were directed instead to the regular services of their local employment offices. The applicants were informed by email whether or not they were to be admitted to the experimental programme at the start dates, i.e., May 15 or June 5 2002.

4. Common difficulties in experimental evaluation

New ways of organising active measures are particularly suitable for experimental evaluation. Björklund & Regnér (1996) conclude: "Indeed, we are convinced that this (*read: alternative ways of organising job-search activities*) is the field where the benefits of classical experiments are the greatest and where the traditional problems can be handled most easily".

Demonstration programmes relating to new services, rather than established ones, offer a fairly straightforward example of social experimentation. In the present case, for instance, no-one was being denied services they would otherwise have been entitled to. Thus there was no need for ethical concern that services were being denied to part of the eligible population. To oppose random assignment in such a situation implies that the relevant services should be implemented immediately without being tested first. Because nobody knows for sure that the experimental group members actually gain anything from their participation, there is no ethical reason for preferring this alternative. As is typical of small-scale demonstration programmes, there were more eligible applicants than available programme slots. Thus randomisation is not an unfair selection instrument.

Co-operation from the administrators at different levels is crucial in conducting a successful experiment. The administrators should behave as if the services were in normal operation. This requirement was most likely fulfilled although we cannot completely rule out the possibility that the coaches were overly enthusiastic about the programme, and therefore more effective than under normal conditions. However, avoiding ethical concerns most certainly had a positive impact on the willingness among programme administrators to cooperate and to follow the outlined evaluation strategy. The demonstration programme thus avoided the risk of bureaucratic resistance.

Also, evaluating new types of work organisation and using new technology, almost by definition eliminates the risk of *substitution bias* that occurs when control-group members receive services similar to those being offered to the experimental group. In the case of these job-search club services, there were no obvious substitutes.[9] Also, since the local administrators could not control the assignment process and possibly distort the experiment group, and since the applicants were not told that the programme was being evaluated, bias due to a nonrepresentative pool of participants (*randomisation bias)*, and/or to participants altering their behaviour during the programme (*Hawthorne effect*), could be ruled out. Finally, by not imposing geographical constraints, it was possible for even a small-scale programme to be carried out nationwide. Hence, the risk of *displacement effects* due to experiment group members acquiring employment at the expense of control-group members, was significantly reduced.

While the design manages to avoid several typical problems inherent in social experiments, some important issues still remain. First of all, as shown in Table 1, the experiment involves relatively few observations

[9] As to the extent that the control group members to a higher degree received traditional job-search club activities, this information is not available since these activities are not recorded in the official registers. However, the *Job seekers survey* (a monthly survey performed by the Swedish Labour Market Board among the currently unemployed and programme participants) of December 2002, reveals that only 13 per cent answered that they had participated in any job-search activities arranged by their local employment office within the last six months. This share was somewhat higher for youth, and somewhat lower for highly educated. This suggests that substitution in terms of *virtual* vs *traditional* job-search activities, if any, is small.

(636). This suggests that the programme impact estimators will produce estimates with low precision, which implies that only very large outcome differences will have a chance of becoming statistically significant. The small-scale dilemma is present in most of the European experiments. For example, the Van den Berg & Van der Klaauw experiment (2001) included 394 UI receivers, the Bratberg, Grasdal & Risa study in 2002 was based on a sample of 560 workers on sick leave, and the Rosholm & Skipper paper from 2003 contained 812 unemployed applying to participate in labour market training programmes. Also, the 1975 Swedish Delander study consisted of 410 currently unemployed.

A second problem is compliance. Table 2 presents three different measures of the experiment group members' level of activity in the job-search club. The measures reveal both large proportions of no-shows, i.e., people who never entered the programme, and dropouts, i.e., people who dropped out of the programme prior to receiving all of the treatment. The first column shows that almost 50 per cent of the experiment group never visited the job-search programme home page. Of those who did, 40 per cent did it on one occasion only. The second column tells us that only about 30 per cent actively used the services in more than one hour. According to the third column, between 70 and 80 per cent of the experiment group members failed to complete any of the practical exercises in the programme. The presence of no-shows and dropouts reduces the difference in treatment between experiment and control group members and makes the simple outcome comparison, or the "intent-to-treat" estimator, less informative about the effects of actually receiving the tested programme services, i.e., the effect of "treatment-on-the-treated". Compliance problems are common in experimental evaluations. Heckman et al. (1999) show that in experiments conducted in the U.S between 1975 and 1992, the portion of experiment group members receiving treatment was often less than 0.7, in some cases even below 0.5. In Europe, the Rosholm & Skipper experiment suffers from both no-shows (48%) and cross-overs (22%), i.e. control group members receiving treatment.

Table 2. Distribution of three measures of activity in the job-search club among experiment group members

Percentile	**No. of accessions**	**No. of operative minutes**	**Per cent of exercises completed**
0	0	0	0
10	0	0	0
20	0	0	0
30	0	0	0
40	0	0	0
50	1	27	0
60	1	36	0
70	1	65	0
80	4	183	6
90	11	520	35
100	327	4539	100
Mean	*6*	*200*	*8*

Note: Number of observations: 343.

There could be several reasons for the large amount of no-shows in this experiment.[10] The services being nonmandatory is probably the most important explanation. Just as submitting an application was voluntary, so was participation. No penalty was imposed on those who ignored the possibility

[10] It is important to distinguish between no-shows and "attriters". No-shows do not receive the services but remain in the follow-up sample, while attriters are usually eliminated. In our sample, 10 per cent in the experiment and 10 per cent in the control group were deregistered and coded "cause unknown" (attriters). This indicates that the employment officer lost contact with the unemployed. Since the attrition is not systematically related to either of the groups, the attriters are *not* excluded from the sample.

of joining the programme. The lack of computer availability could be another explanation. Although specified as a prerequisite, not all applicants would necessarily have had access to an outside computer. Finally, some of the absence could be due to deficiencies in data. For instance, when registered job seekers find employment they could omit to report to the employment office. As a result, the register would overestimate the true number of unemployed at any given moment. Hence, although the experiment and control groups at the programme start date consisted of currently registered job seekers, they possibly include persons no longer unemployed.

Similar to the case of no-shows, there are potentially multiple reasons for the presence of dropouts. Clearly, the voluntariness allowed participants not fully satisfied with the services to quit. However, the services encouraged practising the skills during treatment, which means that getting a job is one likely reason for not pursuing the programme. Furthermore, as displayed in Table 3 below, the services particularly appealed to the group of young job seekers. More than 25 per cent of the applicants were below the age of 25. This is generally a mobile group of unemployed with, on average, short spells of unemployment. The group is also highly prioritised by the authorities which means that their unemployment spells are more frequently interrupted by labour market programmes. Finally, the chosen period of performing the services, including the summer months June, July and August, is probably an additional explanation for the small-dose problem. In these months, the search activity is generally lower on average.

In sum, several common pitfalls associated with experimental and even nonexperimental evaluation have been avoided here. But the voluntariness most likely contributed to low activity in the job-search programme where the majority of those offered the services either denied the offer, or dropped out early.[11] To the extent that some of the no-shows were

[11] This would probably have been avoided had the services instead been a compulsory full-time activity. On the other hand, then other problems, for instance ethical objec-

caused by the applicants failing to fulfil the requirements as participants, these could have been minimised had the randomisation and programme start been preceded by an outreach procedure. Then the computer availability criterion, as well as the job seeker status prerequisite, could have been confirmed.

5. The evaluation problem

The fundamental evaluation problem arises because a person cannot be observed in two labour market states at the same time. Consequently, the evaluation problem is typically formulated at the population level and focuses on mean impacts of participation. Using similar notation as Heckman et al. (1999), let $D=1$ indicate the offer to participate in the programme, $D=0$ otherwise, and Y_1 and Y_0 the respective outcomes. The average treatment effect on the treated is then:

$$E(\Delta \mid D=1) = E(Y_1 - Y_0 \mid D=1) = E(Y_1 \mid D=1) - E(Y_0 \mid D=1). \tag{1}$$

In reality, we observe Y_1 for those treated and Y_0 for the nontreated. Comparing means between the observables we get:

$$\begin{aligned} &E(Y_1 \mid D=1) - E(Y_0 \mid D=0) = \\ &E(Y_1 \mid D=1) - E(Y_0 \mid D=1) + \{E(Y_0 \mid D=1) - E(Y_0 \mid D=0)\}, \end{aligned} \tag{2}$$

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tions, bureaucratic behaviour and randomisaton bias, would potentially have been issues of more concern. An agreement of some sort would perhaps have increased the compliance intensity. But then a sanction system would have been necessary to maintain these agreements.

which equals the average treatment effect on the treated plus a bias term. The last part of equation 2 is attributable to the fact that the outcomes of those not offered treatment are not necessarily representative of the nonobservable outcomes of those offered treatment had they not been offered it. Given that treatment is randomly assigned, the selection-bias problem is solved because D is independent of the potential outcomes. As a consequence, the bias term within braces in equation 2 equals zero and

$$E(Y_1 \mid D=1) - E(Y_0 \mid D=0) = E(\Lambda \mid D=1). \tag{3}$$

Random assignment thus ensures that all those offered treatment and all those not offered treatment are comparable as groups, and that differences in the subsequent outcomes are attributable to programme participation. If, for some reason, members of the experiment group fail to participate in the services (no-shows), equation 3 reflects the effect the *availability* of the services, or the intent-to-treat, rather than the effect of treatment.[12]

[12] In order to recover the treatment effect on the treated, we must adjust equation 3 for the presence of no-shows. If T is introduced as an indicator of programme services actually being received, where $T=1$ represents participation and $T=0$ otherwise, then $\frac{E(Y_1 \mid X, D=1) - E(Y_0 \;\; X, D=0)}{P(T=1 \mid X, D=1)}$ resolves the treatment effect on those treated (Bloom, 1984). The equation simply scales up the mean-difference estimate by the fraction of participants in the experimental group. To estimate the treatment impact correctly, one assumption is that the mean outcome of no-shows in the experimental group is the same as their analogs in the control group, that is; $E(Y \;\; X, D=1, T=0) = E(Y \;\; X, D=0, T=0)$. Note that in presence of dropouts, the treatment-on-the-treated estimator more accurately represents various levels of partial treatment, rather than the effect of full treatment.

6. Data and descriptive statistics

The experiment and control group members have been followed in *Händel*, an event database administered by the SLMB. *Händel* records all unemployment and labour market programme (LMP) periods, as well as the causes of separation, since August 1991. The register contains information about *personal characteristics* (gender, age, educational level, citizenship, working disability, community etc), and *profession* (desired profession, experience and education in desired profession). The longitudinal character of Händel also makes it possible to define variables reflecting an individual's *unemployment history* (for instance; duration of the ongoing unemployment spell, total duration of all unemployment spells, number of times openly unemployed and number of LMPs embarked upon).

Studying the descriptive data of the various groups of job seekers in Table 3, three different comparisons are especially interesting. First of all, with this experiment being the first test of the clients′ interest in the services, it is interesting to examine the characteristics of those who applied for participation. A comparison between all job seekers (column 1) and the full experiment sample (column 6), shows men to be significantly overrepresented among the latter (t-tests in column 7). This could be due to the relatively large proportion of women registered at the employment offices as part-time employed. Since employed persons, as well as programme participants, are less attracted by the services, this is a natural consequence. Rather expectedly, the applicants are somewhat younger and more highly educated. The differences are especially noticeable for those in the 18-24 and 25-34 age range, and among those with experience from university studies. This last corresponds to the results presented in Kuhn & Skuterud (2002). Accordingly, people seeking jobs demanding special theoretical competence or shorter university education are overrepresented among the applicants. Compared to the average job seeker, the applicant group also contains a larger proportion of people living in big cities, and people experienced and educated in their desired professions. Finally, the applicants have more frequent unemployment spells and have more often participated in programmes. They are also currently experiencing unemployment spells that are only half as long as those of the non-applicants.

A second comparison, answering to the question of how successful the randomisation was, is between the characteristics of the members of the experiment and control group. Except for random differences, the groups should be similar regarding both observables and nonobservables. The random differences diminish with the number of observations. In small samples, however, the discrepancies can be quite substantial. Comparing the groups in columns 4 and 5 reveals that the mean deviations are almost exclusively small. However, 31 per cent in the experiment group had received at least two years of higher education (university), compared to only 24 per cent in the control group. The difference is statistically significant at the 5%-level (t-tests in column 8). Since educational level is usually positively correlated with employment probability, the mean-difference estimator could overestimate the true programme effect. The experiment group also comprises a significantly larger proportion of job seekers looking for craftsman´s work. Finally, members of the experiment group had started on significantly fewer LMPs than the control-group members at experiment start. Note that with 49 variables, the groups would be expected to significantly differ in 2-3 of those (0.05*54=2.45).

Comparing participants and no-shows among those offered services (columns 2 and 3), reveals a non-random selection into participation. The no-show rate is higher in the youngest age category and among those with a low educational level, as opposed to the age category 25-34 and job seekers with more than two years of university studies. Also, people who are currently employed or taking part in a programme more often reject the offer of participating. An interesting result is that the no-show rate is higher among those who submitted their applications at the employment offices

(54%), rather than through the PES homepage (28%). Since the choice of application channel may signal whether or not an individual has access to an outside computer, this is a useful finding for further testing of the services.

Table 3. Summary statistics for the experiment group (participants and no-shows), the control group and all registered job seekers. Bold type indicates statistical significance at the 5%-level

	All job seekers (1)	**Experiment group**			**Control group (5)**	**Full expe-riment sample (6)**	**t-test (1)-(6) (7)**	**t-test (4)-(5) (8)**
		Participants (2)	*No-shows (3)*	*Mean (4)*				
Male	0.48	0.58	0.60	0.59	0.55	0.57	***-4.40***	*0.84*
Age								
18-24	0.18	0.22	0.34	0.27	0.26	0.27	***-4.98***	*0.42*
25-34	0.25	0.35	0.29	0.32	0.33	0.32	***-3.64***	*-0.19*
35-44	0.24	0.22	0.24	0.23	0.22	0.22	*1.19*	*0.27*
45-54	0.19	0.16	0.11	0.14	0.14	0.14	***3.11***	*-0.23*
55-	0.14	0.06	0.02	0.04	0.05	0.05	***10.21***	*-0.62*
Mean	*38.2*	*34.5*	*31.6*	*33.1*	*33.5*	*33.3*	***10.82***	*-0.38*
Educational level								
<Compulsory school	0.11	0.03	0.07	0.05	0.07	0.06	***4.82***	*-0.99*
Compulsory school	0.19	0.12	0.17	0.14	0.17	0.16	***2.24***	*-1.17*
Upper secondary	0.5	0.36	0.45	0.4	0.43	0.42	***4.09***	*-0.79*
University <2 years	0.06	0.1	0.09	0.09	0.08	0.08	***-2.32***	*0.83*
University >=2 years	0.14	0.39	0.23	0.31	0.24	0.28	***-7.50***	***2.07***
Graduate level	0.00	0.01	0.00	0.00	0.01	0.01	*-0.69*	*-1.11*
Home county								
Big city[a]	0.42	0.59	0.60	0.60	0.56	0.58	***-7.82***	*0.80*
Local labour markets[b]	0.26	0.16	0.11	0.14	0.13	0.13	***8.51***	*0.27*

Other	0.32	0.25	0.29	0.27	0.31	0.29	*1.95*	*-1.08*
Experience in desired profession (yes)	0.65	0.72	0.72	0.72	0.68	0.70	***-2.87***	*1.12*
Education in desired profession (yes)	0.47	0.64	0.55	0.6	0.61	0.60	***-6.46***	*-0.50*
Desired employment type								
Full-time	0.51	0.55	0.51	0.53	0.5	0.52	*-0.53*	*0.73*
Part-time	0.08	0.02	0.02	0.02	0.03	0.02	***7.53***	*-1.07*
Full-time/part-time	0.42	0.44	0.47	0.45	0.47	0.46	***-2.00***	*-0.40*
Desired profession								
No classified profession	0.16	0.05	0.07	0.06	0.08	0.07	***7.75***	*-1.00*
Management work	0.01	0.03	0.01	0.02	0.01	0.01	*-0.59*	*1.50*
Special theoretical competence	0.07	0.27	0.09	0.19	0.16	0.18	***-7.10***	*0.75*
Short university education	0.08	0.18	0.08	0.13	0.15	0.14	***-3.86***	*-0.56*
Administrative work	0.12	0.09	0.14	0.11	0.17	0.14	*-1.27*	*-1.93*
Service, health care & commercial	0.23	0.13	0.25	0.19	0.22	0.20	*1.32*	*-0.90*
Farming, forestry & fishing	0.02	0.01	0.01	0.01	0.01	0.01	***4.59***	*-0.16*
Craftsman's work	0.11	0.08	0.14	0.11	0.06	0.09	*1.79*	***2.42***
Machine, transport & communication	0.11	0.09	0.09	0.09	0.08	0.08	***2.07***	*0.41*
No vocational training required	0.10	0.07	0.12	0.09	0.07	0.08	*1.32*	*0.99*
UI-compensation								
Non	0.17	0.17	0.29	0.22	0.26	0.52	***-3.91***	*-1.12*
Base premium	0.08	0.16	0.17	0.16	0.19	0.02	***-6.17***	*-0.91*
Income-related	0.75	0.68	0.54	0.61	0.55	0.46	***8.10***	*1.68*
Working disability (yes)	0.20	0.08	0.12	0.10	0.12	0.11	***6.97***	*-0.94*
Citizenship								
Swedish	0.90	0.83	0.82	0.83	0.86	0.84	***4.12***	*-1.09*
Other Nordic countries	0.02	0.03	0.03	0.03	0.02	0.03	*-1.33*	*0.71*
Other	0.08	0.14	0.15	0.15	0.12	0.14	***-3.86***	*0.85*
Expanded search area[c] (yes)	0.12	0.25	0.17	0.22	0.21	0.21	***-5.26***	*0.23*
Unemployment experience								

No. of LMPs	3.96	4.30	5.02	4.64	5.41	4.99	***-5.47***	***-2.10***
No. of unemployment periods	6.61	8.12	9.02	8.54	9.49	8.98	***-9.35***	*-1.92*
Ongoing unemployment period, years	2.06	1.06	0.91	0.99	1.10	1.04	***12.95***	*-0.80*
All unemployment periods, years	4.93	4.13	4.09	4.11	4.38	4.23	***4.04***	*-1.37*
Status at the experiment start								
Openly unemployed	0.28	0.63	0.45	0.55	0.5	0.52	***-11.88***	*1.18*
In job	0.33	0.09	0.13	0.11	0.12	0.12	***15.29***	*-0.47*
In LMP	0.39	0.28	0.42	0.34	0.38	0.36	*1.27*	*-0.91*
Start date								
15/5-02	-	0.38	0.44	0.41	0.43	0.42	*0.48*	*-0.47*
5/6-02	-	0.62	0.56	0.59	0.57	0.58	*-0.48*	*0.47*
Application channel								
ams.se	-	0.36	0.15	0.26	0.31	0.28	-	*-1.24*
Employment Office	-	0.64	0.85	0.74	0.69	0.72	-	*1.24*
Cumulative exit to employment up to 1/12-02	**0.29**	**0.37**	**0.29**	**0.33**	**0.31**	**0.32**	-	-
Number of observations	6899	181	162	343	293	636		

Notes: 1. "All job seekers" in col. 1 refers to a cross-section of all those registered at the employment offices on May 15 2002. The number of observations (6,899) corresponds to approximately 1 per cent of the population. [a]: Refers to the counties of Stockholm, Västra Götaland and Skåne. [b]: Refers to the counties of Värmland, Dalarna, Gävleborg, Jämtland, Västernorrland, Västerbotten and Norrbotten. [C]: During the first 100 days of unemployment, a job seeker is allowed to restrict the search area geographically.

7. Empirical strategy

I use two statistical techniques to evaluate the job-search club services. First, the intent-to-treat experimental estimator compares the outcome between the experiment and control group members. This estimator estimates the effect of offering the possibility to participate and does not take into account that a large part of the experiment group did not actually participate, or only participated a little. While the intent-to-treat impact estimator represents the effect we would observe from implementing the services, and therefore is the policy parameter of most relevance, it is not likely to be informative about the gains of actually participating. Therefore I employ an instrument variable (IV) model in which I use the dummy variable of being randomly assigned to participate as an instrument of participation.[13] The random assignment indicator is highly correlated with actual participation and also uncorrelated with the outcome. The outcome equation is specified as follows:

$$Y_i^* = \gamma D_i + \varepsilon_i, \tag{4}$$

where Y_i^* is an unobserved variable related to the binear observable variable Y_i in the following way: $Y_i = 1$ if $Y_i^* > 0$, 0 otherwise. Y_i indicates whether or not an individual was employed during the follow-up period. D_i represents three levels of activity in the programme using the number of accessions and operative minutes in the programme as indicators of treatment dose. I define "treatment" as visited the job-search programme home page at least 1, 5, and 10 times respectively, and actively used the services

[13] See for instance Rouse & Krueger (2004).

in more than 1, 3, and 6 hours respectively. γ indicates the dose-specific effect.

The regression relationship describing the endogenous decision to participate is specified as:

$$D_i = \delta Z_i + u_i , \qquad (5)$$

where D_i denotes the various levels of participation. Z_i is the assignment indicator (1 if member of the experiment group and 0 otherwise), and δ_i is the corresponding coefficient. Assuming joint normally distributed error terms ε_i and u_i, equations (4) and (5) are estimated by maximum likelihood.

8. Results

8.1 Effects of intent-to-treat

Table 4 presents mean-difference estimates comparing the cumulative transitions to employment for experiment and control group members in the six months follow-up period (June-December). Comparing the full sample of experiment and control group members, the assessment is performed on the basis of the intent-to-treat principle. Both adjusted and unadjusted impact estimates are reported. The adjusted estimator is estimated by first identifying a common range in which both experiment and control group members have an actual chance of receiving an offer to participate (see Bratberg et al., 2002).[14] After eliminating those lacking support, i.e. those with no

[14] The offer probability was estimated using a Probit model including the explanatory variables in Table 3, except for "application channel". This variable is excluded to make the link to the analyses of the second part of the paper (the evaluation of the nonexperi-

counterpart in the opposite group, a Probit model adjusts the programme effect for random differences in observed characteristics.

According to the unadjusted impact estimates in the first column, the experiment group members get jobs at a somewhat higher rate, especially during the first two months. The July estimate is weakly positive significant. An explanation could be that those who were motivated to invest in the programme, were active from the start. As they gradually dropped off, for instance to employment, the average activity level in the programme diminished. This is further accentuated by the stated last possible programme start date which made late entries impossible. Hence, a programme effect, if any, would be expected to appear early in the follow-up period.

The (unadjusted) six-month result in Table 4 is slightly positive (1.9 percentage points) but insignificant. The standard error indicates a large 95% confidence interval of the six-month (December) impact estimate, from –5.4 to 9.2 percentage points. Compared to the unadjusted impact estimates, the adjusted estimates are throughout somewhat lower. The six-month effect is negative, -1.3 percentage points.[15,16]

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mental evaluation methods) more transparent. Five members of the experiment group lacked upper-tail support in the control group. In the same way, four control-group members fell below the experiment group range of support.

[15] Controlling for observables normally reduces the confidence interval surrounding the impact estimate, thus allowing smaller deviations in outcomes between experimental and control group members to become statistically significant. However, due to the loss of statistical degrees of freedom, the standard error could also become somewhat larger.

[16] Instead using *employment status at December 1 2002* as the dependent variable, the mean-difference estimator generates a negative (-2.3 percentage points) effect. Probit adjustment further emphasises the negative effect, generating a point estimate significant at the 10% level (-6.1 percentage points). Combined with the results of the main analysis, this implies that the duration of the employment was on average somewhat shorter among the experiment-group members.

Table 4. Intent-to-treat estimates (unadjusted and adjusted) of the effect on the cumulative exit to jobs, percentage points

Month	**Differences in means (unadjusted)**	**95% conf. interval**	**Probit estimates (adjusted)**	**95% conf. interval**
July	0.040* (0.024)	-0.008 – 0.088	0.033 (0.021)	-0.008 – 0.073
August	0.031 (0.028)	-0.023 – 0.086	0.026 (0.026)	-0.026 – 0.078
September	0.007 (0.032)	-0.055 – 0.069	-0.006 (0.031)	-0.067 – 0.054
October	0.032 (0.035)	-0.037 – 0.100	0.010 (0.036)	-0.061 – 0.080
November	0.023 (0.036)	-0.048 – 0.094	-0.007 (0.038)	-0.082 – 0.068
December	0.019 (0.037)	-0.054 – 0.092	-0.013 (0.039)	-0.090 – 0.064

Notes: Standard errors, calculated as the root of $(var_1/n_1 + var_2/n_2)$, are within parentheses. (1) * refers to significance at the 10 per cent level. (2) No. of observations (unadjusted): 636 (343 experiment group members and 293 control group members). No. of observations (adjusted): 627 (338/289). (3) Adjustment based on a Probit regression estimating employment probability. The estimation includes the regressors in Table 3, except for information on "education in desired profession" and "application channel".

8.2 Effects of treatment-on-the-treated

The large compliance problems make the intent-to-treat estimator a poor indicator of the effects of actually taking part of programme services. To perform such analyses, I estimate the IV model presented in equations (4) and (5), using the random assignment as an instrument, and the information on number of accessions and number of operative minutes from Table 2 as indicators of treatment intensity. Table 5 reports the treatment-on-the-treated effects of different levels of activity in the job-search programme.

The results are consistent across the two measures of received treatment: larger treatment doses are associated with larger job chances.[17] However, no significant effects are found. Note that by defining "treatment" as activity at increasingly higher levels, I assume that the effect of increasingly levels of treatment is equal to the effect of no treatment at all. With positive effects of small doses of treatment, the effects of large doses are underestimated.[18]

Table 5. Instrumental variables (IV) estimates of the effect of treatment-on-the-treated, using various definitions of treatment

No. of accessions	Coefficient (std.err)	**No. of operative hours**	Coefficient (std.err)
>=1	0.095	**>=1**	0.165
	(0.196)		(0.330)
>=5	0.282	**>=3**	0.257
	(0.552)		(0.504)
>=10	0.472	**>=6**	0.391
	(0.928)		(0.767)

Note: 636 observations.

[17] The Wald test of exogeneity estimating the treatment effects in Table 5 throughout reports insignificant test statistics, indicating no endogeneity in the participation choice. This suggests that a regular probit regression may be appropriate for estimations.

[18] Including covariates in the estimations generates a negative relationship between treatment dose and treatment effect. Similar to the estimations without covariates (reported in Table 5), however, none of the impact estimates are statistically significant.

8.3 Summing up the results

Neither the intent-to-treat impact estimator, estimating the effect of being assigned the services, nor the effect of various levels of actual participation, provide evidence of any effects of the evaluated job-search programme. However, one can not exclude the possibility that the services have some favourable impact on certain subgroups of unemployed. Unfortunately, with the small samples separate subgroup analyses are not meaningful. According to Table 3, openly unemployed job seekers between the age of 25 and 34, with experience from university studies, and with some work experience in the desired profession, are among those most willing to test the services. Targeting to this group could thus be an idea in future investigations of the services.

Finally, if the services not being enforced made it more difficult to analyse the effects of actual treatment, it clearly made the demonstration programme a relevant test of the clients′ interest in this type of services, and their expectations of the effects of the programme contents. The small-sized sample and the small-dose problem is therefore useful and policy relevant information in itself.

II Evaluating Nonexperimental Evaluation Methods

Next, I use the six-month intent-to-treat impact estimate as a benchmark to assess commonly used nonexperimental evaluation techniques. The purpose is to examine the extent to which available data and standard econometric methods succeed in replicating the assumed nonbiased experimental results. This method of utilising experimental data to evaluate the performance of various nonexperimental evaluation techniques has been applied in several studies, almost all of them using U.S. data. For instance, both LaLonde (1986) and Fraker & Maynard (1987) showed that traditional econometric methods often fail to repeat experimental results. Recent nonexperimental evaluation literature places greater emphasis on matching procedures. Applied to high-quality data, Heckman, Ichimura & Todd (1997) conclude that, compared to standard regression methods, these estimators generate results more consistent with those produced from experimental evaluation.[19] In Europe, a recent Norwegian study (Bratberg, Grasdal & Risa 2002), shows good correspondence in the outcomes when comparing experimental and nonexperimental estimators.

In the present analysis, the randomly drawn control group is replaced by a comparison group drawn from the population of those currently registered at the employment offices on May 15, and June 5, 2002. The outcomes for 6,899 individuals represent the counterfactual events in the absence of randomised controls. Previous research has demonstrated the importance of using comparable data in evaluating the results of different studies.[20] Here, information is acquired from the same database for members of the experiment, control and comparison groups. Hence, outcome and explanatory variables are defined and measured in the same way.

9. Nonexperimental estimators

The *first* model to be tested is the above presented *Probit specification* in Equation (4). However, D now represents the offer to participate (1 if member of the experiment group and 0 otherwise), and not various doses of participation. Also, a covariate vector, X, captures observed differences between experiment and comparison group members. The model is specified as,

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[19] Other similar studies include: Friedlander & Robins (1995), Dehejia & Wahba (1999, 2002), Heckman, Ichimura, Smith & Todd (1998), Smith & Todd (2000).

[20] See Heckman et al. (1997).

$$Y_i^* = \beta' X_i + \gamma D_i + \varepsilon_i \tag{6}$$

where $Y_i = 1$ if $Y_i^* > 0,\ 0$ otherwise. β is a coefficient vector.

This model is based on the identifying assumption that X_i fully captures the mechanisms affecting both the probability of receiving an offer to participate, i.e., the decision to apply, and the outcome in the absence of the programme. This is a strong assumption. If some selection is on unobserved variables, sample selection bias arises because

$$E(\varepsilon_i \mid X_i, D_i = 1) - E(\varepsilon_i \mid X_i, D_i = 0) \neq 0\,. \tag{7}$$

The *second* model to be evaluated is an *IV model* similar to the one presented in equations (4) and (5). However, equation (4) is now replaced with equation (6). Also, instead of linear regression relationship describing the endogenous decision to apply for participation, I specify a bivariate probit model where selection is governed by:

$$D_i^* = \delta' Z_i + u_i; \qquad D_i = 1 \text{ if } D_i^* > 0,\ 0 \text{ otherwise.} \tag{8}$$

In the equation, Z_i is a vector of explanatory variables affecting the choice of submitting an application, and δ is a vector of coefficients. As an exclusion restriction, i.e., a variable that is not part of X, I use the binary variable “education in desired profession”.[21]

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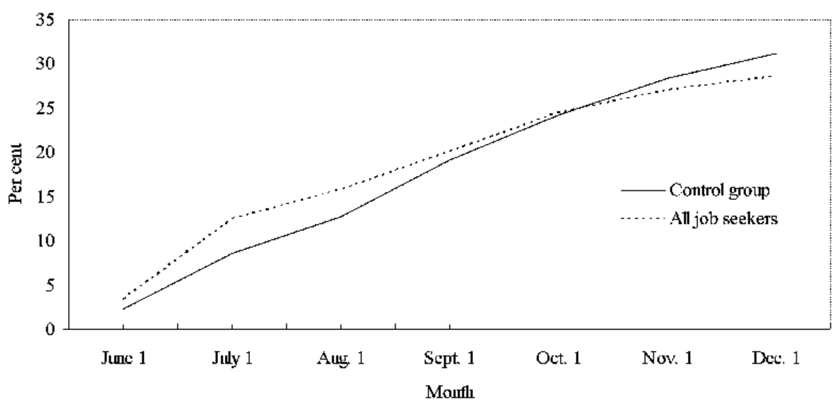
[21] Exclusion restrictions improve identification, although they are not formally required in parametric sample selection models.

In recent years, matching procedures have become more popular alongside the traditional parametric methods. Matching methods pair participants (in our case people receiving an offer to participate) with nonparticipants who are similar as regards observed attributes and estimate programme effects by comparing mean outcomes. However, rather than matching on a set of covariates X, Rosenbaum & Rubin (1983) showed that matching on the probability of participation, or the propensity score, also generates consistent estimates. Since finding a comparison group member becomes increasingly difficult for every covariate added in X, this is a major advantage, because the propensity score $P(X)$ is a (one-dimensional) scalar. For matching methods to properly estimate the programme impact, it is necessary that the outcome in the absence of the programme service, conditional on a set of explanatory variables, is independent of treatment T. For this *conditional independence assumption* to hold, all variables affecting both participation and nonparticipation outcomes must be observed and accounted for. Needless to say, the credibility of the matching estimator hinges on the richness of the available data. The major benefit of matching compared to regression, which also conditions on a set of observed variables, is that matching precludes any assumptions of functional form. According to Dehejia & Wahba (2002) and Smith & Todd (2000), its nonparametric character could considerably reduce bias in the impact estimate. Another advantage is that matching methods only match programme and comparison-group members in the range of $P(X)$ that is common to both groups. Matching thus avoids comparing the incomparable.

The propensity score can be implemented in different ways. The most common is the *nearest-neighbour estimator*, whereby participants and nonparticipants who are closest in terms of $P(X)$ are matched. This is the *third* estimator to be evaluated. An alternative to one-to-one matching models is to include several nearest neighbours, whereby the participant outcome is contrasted with a weighted average of outcomes. Therefore, the *fourth* model to be tested is a *kernel-based matching* model where the weight allotted to each non-treated unit is in proportion to its closeness to its matched treated counterpart. Heckman et al. (1997) conclude that in small samples, the choice of matching estimator can make a difference.

By comparing the cumulative exits to employment for the control and the comparison groups, we get an idea of to what extent the nonexperimental estimators need to adjust for differences in the outcomes in order to recover the experimental results. According to Figure 1, the exit rates are surprisingly similar considering the above described differences between applicants and nonapplicants (see Table 3). Within six months, 31.1 and 28.6 per cent of the control group and comparison group respectively had achieved employment status. Hence, the tested methods need only adjust for minor deviations in the outcome.

Figure 1. Cumulative exit to employment for members of the control group, and all job seekers



10. Comparing Experimental and Nonexperimental Results

The first column of Table 6 repeats the regression-adjusted six-month intent-to-treat programme effect presented in Table 4. The three following columns (2-5) report the different nonexperimental estimates. The nonexperimental Probit estimate in column 2, -0.026, deviates by only 1.3 percentage points from the experimental Probit.[22] This is clearly within the sampling error interval.[23] Note also the higher precision in the nonexperimental estimate due to the larger number of observations.

Due to the small discrepancy, it is not likely that the sample selection model (column 3) corrects the programme effect for any selection on unobservables. Using the exclusion restriction, "education in desired profession", estimation identifies a positive correction term (Rho).[24] The programme effect is thus adjusted downwards. Since Rho is insignificant, however, this suggests identification without a selection equation.

[22] The common-support procedure excludes 626 members of the comparison group, and three members of the experiment group. Since only three experiment group members failed to find a comparable, violating the common-support condition would only have a negligible impact on the estimated programme effect.

[23] A more direct approach to evaluate nonexperimental estimators, which is applied by Heckman et al. (1997), Heckman et al. (1998) and Smith & Todd (2000), uses data on comparison group members and randomised-out controls. Although similar to the experiment group in observed and unobserved characteristics, the latter group did not receive any programme services. Hence, a correctly specified nonexperimental estimator should identify a zero programme impact. Performing a probit analysis replacing experiment-group members by randomised-out controls produces a programme effect equal to –0.027 (0.026). The result confirms that the nonexperimental method is effective in estimating the true programme impact.

[24] The p-values are 0.012 and 0.487 for the variable "education in desired profession" in the selection and outcome equation respectively.

Columns 4 and 5 report the results for matching estimators.[25] The nearest-neighbour estimator produces a point estimate somewhat further from the experimental result. The impact estimate, -0.056, is slightly downward-biased.[26] However, the result is somewhat sensitive to the set of conditioning variables in $P(X)$.[27] Also, performing separate matching on the length of the ongoing registration spell in months, the estimator yields a point estimate (-0.023) considerably closer to the adjusted experimental result.[28]

Note that despite the fact that the nonexperimental Probit and the matching estimates are both based on the same set of covariates, the matching estimates report lower programme impacts. This is due to the different weighting schemes of the underlying programme effects. The matching estimator, like the experimental mean-difference estimator, places weight in proportion to the probability of being in the experiment group. Hence, the people most likely to apply for participation are those who get most weight in the programme impact. The Probit estimate, on the other hand, puts most weight in the middle of the probability distribution.[29]

Finally, the result from the kernel-based matching estimator, -0.040, differs by 2.7 percentage points.[30] The benefit from using several compari-

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[25] Both matching estimators perform separate matchings on the relevant start date. Hence, each matched pair started either on May 15 or June 5. In this way, we ensure similar length of exposure.

[26] Combining data on the randomised-out control group and the nonexperimental comparison group, the estimator generates an insignificant programme effect of -0.059 (0.043).

[27] In contrast, the result is not sensitive to the defined caliper distance applied in the matching.

[28] Heckman et al. (1998) emphasise the benefits of access to information about recent labour force history in the performance of nonexperimental evaluation.

[29] Angrist & Krueger (1999) discuss the weighting issue in depth.

[30] Modifying the applied default bandwidth (0.6) does not dramatically alter the results.

son group members (instead of just one) is very likely due to the small number of experimental observations. Placing less weight on each particular comparison outcome helps to reduce the uncertainty in the estimated programme effect.

To summarise, the experimental programme results are robust when testing various nonexperimental estimators. The predicted programme effects estimated in a simple Probit model, and two matching procedures, are all fairly close to, and within sampling variance from, the experimental impact estimate. The findings suggest that the available data successfully identifies and adjusts for non-random selection. However, the test of the nonexperimental methods is somewhat weakened by the similar outcomes of the control and comparison group members. The tested methods thus only needed to adjust for small minor differences in the outcome. Also, with such poor precision in the experimental impact estimate, only very large deviations would have generated another conclusion.

Table 6. Intent-to-treat estimates of the effect on cumulative exit to jobs within 6 months after enrolment, using various estimators (estimated standard errors are within parentheses)

	Experimental, Probit (adjusted) (1)	Nonexperimental, Probit (2)	Nonexperimental, bivariate Probit (3)	Nearest neighbour matching[a] (4)	Kernel matching[a] (5)
Programme effect[b]	**-0.013 (0.039)**	**-0.026 (0.024)**	**-0.144 (0.183)**	**-0.056 (0.039)**	**-0.040 (0.029)**
Rho			0.222 (0.245)		
Male	0.039 (0.121)	0.057 (0.039)	0.060 (0.039)		
Age					
18-24	-	-	-		
25-34	0.064 (0.168)	-0.134 (0.055)**	-0.151 (0.058)***		
35-44	-0.088 (0.200)	-0.184 (0.060)***	-0.206 (0.065)***		
45-55	-0.398 (0.234)*	-0.217 (0.066)***	-0.243 (0.072)***		
55-	-0.296 (0.336)	-0.545 (0.080)***	-0.583 (0.090)***		
Educational level					
<Compulsory school	-	-	-		
Compulsory school	-0.340 (0.275)	0.016 (0.076)	0.018 (0.076)		
Upper secondary	0.056 (0.246)	0.156 (0.069)**	0.154 (0.069)**		
University	-0.097 (0.266)	0.175 (0.076)**	0.188 (0.077)**		
Home county					
Big city[c]	-	-	-		
Local labour markets[d]	0.132 (0.177)	0.124 (0.045)***	0.104 (0.051)*		
Other	0.030 (0.131)	0.075 (0.041)*	0.063 (0.043)		
Experience in desired profession					
No	-	-	-		

Yes	-0.061 (0.135)	-0.023 (0.043)	-0.009 (0.046)
Desired employment type			
Part-time, part-time/fulltime	-	-	-
Fulltime	-0.065 (0.112)	- (0.035)	-0.021 (0.035)
Desired profession			
No classified profession	-	-	-
Management work	1.035 (0.577)*	0.011 (0.165)	0.048 (0.170)
Special theoretical competence	-0.050 (0.276)	0.086 (0.084)	0.129 (0.097)
Short university education	-0.210 (0.276)	0.133 (0.078)*	0.161 (0.084)*
Administrative work	-0.102 (0.277)	0.065 (0.073)	0.085 (0.077)
Service, health care & commerc.	-0.137 (0.257)	0.101 (0.063)	0.116 (0.066)*
Craftsman's work	0.051 (0.294)	0.224 (0.075)***	0.235 (0.076)***
Machine work, transport & com.	0.043 (0.291)	0.120 (0.075)	0.131 (0.076)*
No vocational training required	-0.159 (0.291)	0.101 (0.075)	0.112 (0.076)
UI-compensation			
No	-	-	-
Base premium	0.185 (0.189)	0.288 (0.071)***	0.290 (0.071)***
Income related	0.662 (0.168)***	0.340 (0.055)***	0.330 (0.057)***
Working disability			
No	-	-	-
Yes	-0.294 (0.207)	-0.660 (0.062)***	-0.665 (0.062)**
Citizenship			
Non-Swedish	-	-	-
Swedish	0.078 (0.164)	0.168 (0.061)***	0.151 (0.064)***
Expanded search area[e]			
No	-	-	-
Yes	0.259 (0.142)	0.148 (0.050)***	0.158 (0.051)***
Unemployment experience			
No. of LMPs	-0.004 (0.024)	-0.011 (0.008)	-0.009 (0.008)
No. of unemploy. periods	0.021 (0.016)	0.018 (0.005)***	0.020 (0.006)***

Ongoing unemploy. period, years	-0.029 (0.042)	-0.069 (0.011)***	-0.071 (0.011)***		
All unemploy. periods, years	-0.080 (0.027)***	-0.027 (0.007)***	-0.028 (0.007)***		
Status at the experiment start					
Openly unemployed	-	-	-		
In job	-0.216 (0.187)	-0.166 (0.044)***	-0.192 (0.053)***		
In LMP	-0.151 (0.132)	-0.181 (0.046)***	-0.192 (0.047)***		
Start date					
15/5-02	-	-	-		
5/6-02	-0.146 (0.115)	-0.218 (0.034)***	-0.217 (0.034)***		
Constant	-0.399 (0.350)	-0.620 (0.104)***	-0.577 (0.115)***		
Log-likelihood	-	-	-		
Pseudo R^2	0.094	0.097			
Number of observations	627	6613	6613	646	7242

Notes: col. (1) refers to regression-adjusted experimental estimates. col. (2) Probit estimates, nonexperimental. col. (3) bivariate Probit-estimates, nonexperimental. *, **, *** refers to significance at 10, 5 and 1 per cent level respectively. [a]: Standard errors are bootstrapped. Caliper distance used in the nearest-neighbour matching: 0.01. Applied bandwidth in the kernel-based matching: 0.6. [b]: Estimated treatment effects and standard errors are in percentage points, covariate effects reflect the estimation coefficients. [c]: Refers to the counties of Stockholm, Västra Götaland and Skåne. [d]: Refers to the counties of Värmland, Dalarna, Gävleborg, Jämtland, Västernorrland, Västerbotten and Norrbotten. [e]: During the first 100 days of unemployment, a job seeker is allowed to restrict the search area geographically.

11. Conclusions

This paper has used data from a randomised experiment to investigate the effect of offering job-search activities on the Internet. Experiments on labour market topics are very rare in Europe in general, and in Sweden in particular. Thus, in conducting the first such experiment in Sweden since 1975, the first pertinent question concerns whether or not the evaluation design succeeded in deriving interesting and relevant policy parameters, i.e., did the experiment work or not?

Many experiment-related problems such as ethical concerns, bureaucratic resistance and randomisation bias were circumvented. With the services being voluntary, however, a considerable fraction of the applicants either failed to show up or dropped out early in the process. For instance, as much as 47 per cent of those offered the services never entered the programme, despite the fact that they submitted applications only within a few weeks before. This made the experimental estimator almost uninformative about the effects of actually receiving the services. However, with no activity restrictions, the policy parameter of most interest is the effects of the intent-to-treat, which explicitly takes into account the possible choice of not participating at all, or dropping out early.

What did we then learn about the future of conducting job-search activities on the Internet? Well, with less than a thousand job seekers showing interest in the programme, and only 636 relevant applications submitted, it is likely that the Swedish Labour Market Board misjudged the clients′ interest in these services. Since one of the purposes of the demonstration was to explore the demand for this type of services, this was useful information. However, the small-sized sample produced impact estimates with low precision. The intent-to-treat estimate shows no significant effect on subsequent job transitions of being offered the possibility to take part of the programme services. Furthermore, applying an instrumental variable approach on the experimental data, using the random assignment as an exclusion restriction, no significant effects were found of various doses of actual treatment. The results suggest that the functioning of the virtual job-search club services need to be further improved before full implementation. Also, further investigations would perhaps benefit from a more precise targeting of the services. Among those showing interest in the services, highly educated openly unemployed in the age of 25-34, with some work experience in the desired profession, were relatively active in the programme.

The final consideration concerns the methodological findings of this paper and is connected with the opportunity for assessing nonexperimental estimators. In evaluating the experiment group outcome against the outcome from a constructed comparison group, using various techniques to offset systematic differences, we found standard econometric methods to be successful in reproducing the experimental impact estimate. Since this is the first study of its kind on Swedish data, and the results may not be relevant to selection into other programmes, further analyses of the abilility of these methods to account for various selection processes are important in future research.

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ESSAY 3

Are There Pre-programme Effects of Swedish Active Labour Market Policies? - Evidence from Three Randomised Experiments[†]

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Abstract

This paper takes advantage of unique experimental data from three demonstration programmes in 2004 to investigate pre-programme incentive effects of active placement efforts at the employment offices in Sweden. The exit rate from unemployment between referral to and start of the programme services is compared between UI eligible experiment and control group members. The results are mixed. In one of the experiments, targeted towards a broad group of UI receivers, arranged job-search activities in groups combined with increased monitoring of job-search efforts generated a 38 per cent increase in the escape rate from unemployment in the weeks leading up to programme start. This translates into an almost two-week reduction of the ongoing UI spell. Referrals to increased monitoring alone did not have the same effect on exit behaviour. In the other two experiments, targeted towards youth and highly educated respectively, referrals to active placement efforts had no effect on the pre-programme outflow.

Keywords: Pre-programme effect, policy evaluation, social experiment

JEL classification: C93, J64

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1. Introduction

Typically, the impact on labour market participation is the parameter of interest in studying the effects of various active measures on the unemployed. However, by acknowledging behavioural adjustments before commencement, further aspects of the potential benefits of labour market policies are captured. Pre-programme effects are the result of an increased job-search effort or a lower reservation wage between notification of the programme and planned programme start. They are therefore also referred to as "motivation effects".

Besides activating the unemployed and upgrading their skills, active labour market policies decrease the utility of unemployment by reducing the amount of "leisure time". A common perception among employment officers is that referrals to different types of compulsory programme activities help to remove those having little problem finding employment, thus reducing the extent of moral hazard behaviour. This is confirmed in empirical studies where positive effects before actual treatment have been found in both typical placement efforts, i.e. job-search assistance activities and recurrent follow-up meetings, and in labour market programmes.

Evaluating experimental data from the Worker Profiling and Reemployment Services in Kentucky, Black et al. (2003) ascribe a large part of the 2.2 weeks reduction in benefit receipt in the treatment group to exits coinciding with notification of re-employment services (job-search training and preparation courses). In the Maryland UI Work Search Demonstration experiments (1997), a short job-search training course reduced the average duration of UI payments by five per cent. The effect was largely generated by an increased hazard rate in the period immediately preceding scheme start. In the U.K., Dolton & O'Neill (1996) assessed the "negative threat component" from compulsory interviews after six months of unemployment in the Restart programme. Using experimental data, they found a significant increase in the off-unemployment hazard rate prior to attending a Restart interview.

On non-experimental data, a study from Australia (DEWRSB, 2001) found large “compliance effects” (around 10 percentage points) from referrals to a three-week job-search training programme. Rosholm & Svarer (2004), on Danish data, include a measure of the risk of programme participation as an explanatory variable in estimations of the off-unemployment hazard rates. They conclude that the perceived risk of future programmes decreases the average unemployment duration by three weeks. Carling & Larsson (2005), find a slightly increased exit rate to employment before the start of a Swedish youth measure introduced in the late 1990s. Finally, Jensen et al. (2003) failed to establish a similar enhanced exit rate preceding a Danish youth unemployment reform implemented in 1996.[1]

This study uses experimental data from three demonstration programmes in Sweden in 2004, where alternative placement activities at the public employment offices were tested against the regular services.[2] The design of the experiments explicitly allows for the examination of exit behaviour between the time of being notified of the programme and programme start. The pre-programme effects are identified as the difference in the escape rate from unemployment and the receipt of UI benefits between the experiment group offered the scheme services and the control group offered the regular services. The subsequent propensity to unemployment is also studied among those leaving unemployment in the pre-programme period.

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[1] Motivation effects are also confirmed in studies of the exit rate from unemployment close to UI benefit exhaustion in UI systems applying “soft” UI-duration constraints where participation in programmes qualifies for additional days of compensation (see Carling et al., 1996, Thoursie, 1998, and Roed et al., 2002, on Swedish data, and Lalive et al., 2000, on Swiss data). The increased exit pattern on approaching benefit exhaustion found in these studies should, with some reservation in the Swedish studies, be ascribed to the prospect of having to participate in a programme.

[2] In Sweden, only two social experiments have been reported in this field (Delander, 1978, and Hägglund 2005).

The results are mixed. I find evidence of increased off-unemployment hazard rates in the pre-programme period in one of the three demonstration programmes offering a combination of compulsory job-search assistance activities and increased monitoring of the job-search efforts. By randomly assigning two groups to different services, I conclude that the positive effect derives from the referrals to job-search assistance activities. Referrals to merely increased job-search monitoring do not have the same positive effect on the exit rate. A possible explanation is that the job-search assistance activities were arranged in groups whereas the monitoring consisted of in-person interviews. The estimated 38 per cent increase in exit rate corresponds to a two-week reduction of the ongoing UI spells. The analysis shows that the positive effect is not the result of more temporary interruptions of the unemployment spells due to, for instance, less attractive job matches.

A final purpose of this paper deals with the fact that the analysis – in contrast to many previous studies on unemployment duration in Sweden – uses UI payment data instead of unemployment register data to estimate unemployment duration. The drawback with unemployment register data is the heavy reliance on self-reported information, which probably tends to exaggerate true unemployment. The analysis in Appendix A shows that the deficiencies in the unemployment register causes substantially biased impact estimates.

The remainder of this paper is structured in the following manner: Section 2 briefly presents the background, the programme services and the admission procedure of each experiment. Section 3 offers a theoretical framework for considering pre-programme effects while Section 4 presents data and motivates the use of UI payment data on unemployment spells. Section 5 discusses the interpretation of the outcome comparison in a situation where the treatment gap between experiment and control group is less than the theoretically desired 100 per cent and relates the estimated parameter to the "intent-to-treat" parameter. Section 6 specifies a Cox-proportional hazard model to explicitly takes into account the flow into regular programmes. Section 7 reports mean-difference comparisons as well as hazard rate estimations of the pre-programme outflow. The number of weeks of unemployment in the 26 weeks subsequent to UI spell interruption is also presented. Section 9 sums up and offers some interpretations of the results.

2. The Experiments

In 2004, the Swedish Labour Market Board (SLMB) granted funds for several demonstration programmes to be conducted at local employment offices with the purpose of testing alternative modes of carrying out placement activities. Placement activities here comprise job-search assistance, interviews, in-depth counselling, monitoring of job-search efforts, and employer contacts. The county labour boards were invited to apply for funding if they could present a strategy for improved matching. Rather than new innovative methods of matching, these strategies typically involved higher quality delivery of already existing services. The activities could either be tested on broad groups of unemployed or be targeted towards some particularly difficult group.[3]

The official document commissioning the county labour board to execute the activities agreed upon, in some cases specified criteria for the selection process. In accordance with the experimental design, participants and non-participants would be selected through randomisation. Also, the control group was supposed to be assigned the employment offices' regular services.

The SLMB appointed an evaluator from within its own ranks, responsible for conducting the experiments.[4] The evaluator's job was to design the experiment and to protect the integrity of the experimental design

[3] The programme activities were carried out by project teams at the employment offices. Each team consisted of 3-5 case workers.

[4] The author of this paper was, as currently employed at the SLMB, responsible for conducting the three experiments presented in this paper.

throughout the evaluation period. The evaluator was also in charge of continuously performing randomisation on new experiment and control group members to replace those leaving unemployment for jobs etc. In common for the experiments presented in this paper is that a pre-specified routine with fixed intervals between notification of the programme and programme start preceded every admission of new job seekers. This is a prerequisite in order to be able to discriminate between behavioural responses to being notified of the programme and participation itself. The scheme services were compulsory, which means that rejecting a referral caused a reduction in UI compensation.[5]

The admission routines and the interval lengths, briefly presented below, differed both between, and within, the demonstrations. The referrals briefly introduced the job seekers to the objectives and the general working methods of the demonstrations. Those receiving UI benefits were also reminded of their obligations as UI receivers and the penalties involved in violating them. UI eligibility was a pre-requisite only in one of the three schemes (Jämtland). For reasons presented in Section 4, however, the analyses are only performed on those currently receiving UI compensation. Finally, the sample sizes were restricted due to capacity constraints, i.e., as a result of the number of coaches and the intensity of the services in each scheme.[6]

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[5] UI-recipients are obligated to pursue the referrals suggested by the case workers both to jobs and to programmes. The employment offices are responsible for following up on referrals and must report UI eligible job seekers who violate the basic conditions for compensation to the UI Funds. The UI Funds then make the decisions about whether or not the negligence should render withdrawal of benefits. Rejecting a referral leads to a gradual reduction of compensation. The first time the claimants refuse an offer they risk a 25 per cent cutback for eight benefit weeks. A second refusal in the same benefit period, reduces compensation by 50 per cent for an additional eight weeks. A third refusal, finally, leads to full withdrawal.

[6] The policy documents specified a requirement of at least 300 experiment group members.

2.1 *The Jämtland demonstration*

The purpose of the Jämtland demonstration was to test new methods of increasing search activity among the unemployed. The programme activities were targeted towards the openly unemployed who were "match ready" and eligible for UI compensation. The experiment group was randomly divided into two separate groups. The first group (*the JSA group*) received both assisted job-search and increased job-search monitoring in monthly group meetings. The other group (*the increased job-search monitoring group, or the no-JSA group*) was only subject to increased job-search monitoring treatment which involved monthly in-person interviews. This design enables the effects of being referred to JSA and those of increased job-search monitoring to be identified separately. The programme was time-limited (3 months), and participation in practice involved 4-5 meetings at the employment office.

The scheme services were carried out between February and November, 2004, and involved 611 experiment group members (311 receiving both treatments and 300 subject only to increased monitoring), along with another 642 control group members. Of these, 496 (246 +250) and 507 respectively collected UI benefits the week of the referrals and are therefore included in the main sample. Admission of participants was carried out in two steps. In the first step, those selected to participate were referred to an individual meeting where an initial assessment was performed. The participants were also informed that their next meeting, which either was a JSA-group meeting or an individual job-search monitoring meeting, would take place 4-5 weeks later. A second referral confirmed this. An on average 6.3-week interval was applied between the job seeker first being notified and programme start.[7]

Table 1, columns 1-3, describes the characteristics of the experiment and control groups using data from the unemployment register

[7] In the first admission in February, there was a five-week interval between notification and start of the programme. The following admissions applied a seven-week interval.

(*Händel*), and the UI payment register (*A-stat*), both presented in more detail in the next section. Of the three demonstrations, the Jämtland demonstration is the least targeted one, with representation in all age groups and educational level categories. The average job seeker had been registered at the employment office for approximately five years, whereof one year in the ongoing unemployment period.[8,9]

2.2 The Uppsala demonstration

The motivation of this programme was a growing number of long-term unemployed among persons with post upper secondary education. Since the situation was particularly alarming among those specialised in social science, the demonstration programme was targeted towards this group. Activities primarily consisted of frequent non-supervised workshops in groups of 8-10 persons. The experiment comprised a total of 1092 (517) job seekers (UI eligible), where 549 (275) were offered the demonstration services and 543 (242) were directed to the regular services.

Admissions took place in February, May, September and November of 2004. First being informed of and introduced to the services in a letter or by e-mail, the job seekers were asked to update the coaches on any recent educational achievements and new work experience. A second notification was sent out as a reminder of the start date. The interval between first being notified and programme start was gradually reduced from initially six to two weeks in the last admission. On average, the length of the pre-programme period was 4.4 weeks.

[8] Periods of registered job seeking as employed are included.

[9] Compared to the control group, the JSA-experiment group has a significantly more extensive history of employment office registration, especially as openly unemployed. The JSA-experiment group is also significantly more likely to be searching for part-time employment. Note that the experiment and control groups are expected to differ significantly in some aspects (0.05 • the no. of covariates).

According to Table 1 (columns 4 and 5), the vast majority of the targeted population was between age 25 and 44. Note also that a portion of the sample (17 per cent) was part-time employed.

2.3 The Östergötland demonstration

The labour market in 2003 was troublesome for youth. In the fall, the Swedish government proclaimed that measures were to be taken at employment offices to cut long-term unemployment in this group by 50 per cent within one year. The situation was particularly difficult in Östergötland, a region where youth were especially exposed to major lay offs, and where they also had the most difficult time finding new jobs. The programme in Östergötland proceeded in parallel with the nationwide goal of halving the number of young long-term unemployed. This is important when interpreting the results, as the service level in the control group would be expected to exceed the “normal” service level for youth in that region. The idea of the demonstration, which among unemployed youth focused primarily on the UI eligible, was to intensify the case worker/job seeker contacts through weekly meetings in job-search clubs. Skills in managing the PES web-applications were emphasised.

The experiment and control groups were gradually filled up with two admissions every month between March and October (July and August excepted). A total of 487 (357) job seekers registered as openly unemployed (UI eligible) were singled out for participation, whereas another 504 (379) were controls. The referrals were sent out three weeks before the week of the first group meeting.

In Table 1, the young target population is reflected in a low age average, a relatively low educational level, a brief unemployment history and low UI compensation per day.

Table 1. Summary statistics for the experimental and control groups. Bold type indicates statistical significance at the 5%-level

	Jämtland scheme			Uppsala scheme		Östergötland	
	Exp. group (JSA)	Exp. group (No JSA)	Control group	Exp. group	Control group	Exp. group	Control group
Female	0.32	0.35	0.28	0.53	0.50	0.44	0.38
Age							
18-24	0.07	0.08	0.07	0.01	0.01	0.91	0.92
25-34	0.30	0.28	0.32	0.44	0.46	0.09	0.08
35-44	0.27	0.23	0.24	0.29	0.24	-	-
45-54	0.18	0.19	0.21	0.19	0.19	-	-
55-	0.17	0.22	0.16	0.06	0.11	-	-
Mean	40.33	41.46	40.25	37.56	38.57	22.71	22.55
Educational level							
<Compulsory school	0.04	0.10	0.07	-	-	0.04	0.04
Compulsory school	0.15	0.14	0.13	-	-	***0.05***	0.09
Upper secondary	0.54	0.49	0.54	-	-	0.83	0.78
University <2 years	0.07	***0.09***	0.05	0.09	0.11	0.04	0.03
University >=2 years	0.19	0.16	0.20	0.83	0.81	0.04	0.05
Graduate level	0.01	0.00	0.00	0.08	0.08	0.00	0.00
Experience in desired profession (yes)	0.85	0.80	0.84	0.72	0.76	0.81	0.77
Education in desired profession (yes)	0.75	0.69	0.74	0.91	0.90	0.58	0.54

Desired employment type							
Full-time	0.56	0.55	0.60	0.62	0.61	0.55	0.56
Part-time	***0.02***	0.01	0.00	0.01	0.05	0.01	0.01
Full-time/part-time	0.42	0.44	0.40	0.36	0.34	0.45	0.42
Desired profession							
No classified profession	0.01	0.01	0.01	0.00	0.01	0.02	0.02
Management work	0.02	0.02	0.04	0.08	0.10	0.01	0.01
Special theoretical competence	0.14	0.16	0.15	0.63	0.63	0.04	0.03
Short university education	0.14	0.15	0.16	0.17	0.15	0.07	0.09
Administrative work	0.10	0.11	0.13	0.08	0.07	***0.17***	0.11
Service, health care & commercial work	0.15	0.14	0.10	0.03	0.02	0.31	0.29
Farming, forestry & fishing	0.02	0.02	0.02	0.00	0.00	0.02	0.03
Craftsman's work	0.20	0.20	0.21	0.00	0.00	0.18	0.20
Machine/transport & communic. work	0.17	0.14	0.13	0.01	0.00	0.11	0.13
No vocational training required	0.07	0.06	0.04	0.00	0.01	0.07	0.09
Working disability (no)	0.97	0.97	0.96	100.00	100.00	0.99	0.99
Citizenship							
Swedish	0.98	0.98	0.99	0.95	0.93	0.96	0.97
Other Nordic countries	0.00	0.01	0.00	0.01	0.02	0.01	0.01
Other	0.01	0.01	0.01	0.04	0.05	0.03	0.02
Expanded search area[a] (no)	0.83	0.88	0.86	0.77	0.72	0.80	0.79
Unemployment experience							
No. of programmes	4.25	3.54	3.68	1.72	1.62	1.46	1.41
No. of periods as openly unemployed	***9.42***	7.90	8.12	5.19	5.05	4.25	4.04

Time registered at the employm. office							
In ongoing unemployment period, years	1.07	0.99	0.95	0.60	0.66	0.56	0.55
In all unemployment periods, years	***5.86***	4.71	4.84	3.07	3.08	1.73	1.82
In programme, years	1.26	0.97	1.06	0.56	0.48	0.32	0.34
In open unemployment, years	***2.53***	2.05	2.22	1.60	1.59	0.73	0.73
UI-compensation type							
Income related	0.93	0.94	0.94	0.79	0.75	0.66	0.64
Base premium	0.07	0.06	0.06	0.21	0.25	0.34	0.36
UI compensation							
Income-based daily salary (SEK)	864	879	875	844	804	501	484
Daily compensation (SEK)	624	616	626	589	569	486	481
Weekly compensation (SEK)	3121	3080	3127	2678	2580	2422	2404
No. of remaining days of compensation	165	181	176	201	193	202	196
Local employment office[b]							
J-land Uppsala Ö-götland							
30 10,11 34,35,36	0.91	0.89	0.90	0.05	0.03	0.37	0.43
31 20,21,24,25,26 61,62,63	0.04	0.06	0.04	0.91	0.93	0.39	0.37
32 30,31,32 71,72,73,74	0.03	0.02	0.03	0.04	0.03	0.24	0.20
35	0.02	0.03	0.04				
Enrolment period[c]							
1	0.36	0.35	0.35	0.50	0.58	0.18	0.17
2	0.37	0.34	0.34	0.17	0.16	0.20	0.21
3	0.27	0.31	0.31	0.21	0.15	0.21	0.26
4	-	-	-	0.12	0.11	0.18	0.18

5	-	-	-			0.22	0.18
Unemployment status at randomisation							
Part-time employed	-	-	-	0.17	0.16	-	-
Unemployed	100.00	100.00	100.00	0.83	0.84	100.00	100.00
Number of observations	246	250	507	275	242	357	379

Notes: Data are based on information from the week of notification. [a] During the first 100 days of unemployment, a job seeker is allowed to restrict the search area geographically. [b] Local employment office (Af) numbers refer to: In Jämtland 30: Af Östersund, 31: Af Bräcke, 32: Af Hammarstrand, 35: Af Svenstavik. In Uppsala, 10: Af Enköping, 11: Af Bålsta, 20: Af Utbildning/Uppdrag, 21: Af Gottsunda, 24: Af Resurs, 25: Af Knivsta, 26: Af Kompetens, 30: Af Tierp, 31: Af Gimo, 32: Af Skutskär. In Östergötland, 34-36: Af Norrköping, 61-63: Af Linköping, 71: Af Motala/Vadstena, 72: Af Af Mjölby/Boxholm/Ödeshög, 73: Af Söderköping/Valdemarsvik. [c] The enrolment periods differ between the demonstration schemes. , “1” represents admissions in 2005. For Jämtland, 4/2, 10/2, 17/2. 2: 14/4, 21/4, 28/4, 6/5. 3: 19/8, 26/8, 2/9, 8/9. For Uppsala, 8/2. 2: 22/4, 27/4, 6/5. 3: 10/9. 4: 5/11. For Östergötland, 3/3, 10/3. 2: 1/4, 10/4. 3: 21/4, 6/5. 4: 26/8, 8/9. 5: 23/9, 7/10.

Table 2: The demonstration programmes: An overview

	Jämtland	**Uppsala**	**Östergötland**
Target group	Openly unemployed, eligible for UI	Openly unemployed/part-time workers & post secondary educated in social science	Openly unemployed youth
Type of services	1. Arranged job-search activities in groups & increased monitoring. 2. Increased job-search monitoring	Non-supervised job-search workshops & job acquisition	Arranged job-search activities in groups
Average # of weeks between notification and programme start	6.3	4.4	3.0
Number of observations (All/UI eligible)	1253/1003	1092/517	991/736
- Experiment group (All/UI eligible)	611/496	549/275	487/357
- Control group (All/UI eligible)	642/507	543/242	504/379

3. Theoretical Framework

In a standard job-search framework individuals choose between income and leisure so as to maximise the present value of expected utility. The present value of unemployment increases with the number of insured days remaining in the benefit period. As benefit approaches exhaustion, the declined value of unemployment is reflected in a lower reservation wage causing a rise in the escape rate out of unemployment (see Mortensen, 1977).

Introducing the possibility of being referred to active placement efforts, similar to those offered in the demonstrations, the expected utility from programme participation must also be considered (for example, see Carling et al. (1996), or Black et al. (2003)). First of all, participation is to various degrees expected to be time consuming and to reduce both leisure time and time for job search. This has a negative effect on the value of unemployment. For some unemployed, other aspects of participation, for instance activities in groups, also might reduce the utility of unemployment during the services. If the claimants anticipate those activities, the value of unemployment falls prior to programme start.

Second, job seekers might also anticipate benefits from participating in active placement efforts. If the services are effective, they would be expected to improve future job chances and/or the distribution of wage offers during and after receiving the services. This would increase the utility of being unemployed before start. If, on the other hand, the services are expected to have a negative impact on future job chances and wages, the opposite holds.

The expected effect of programme referrals on the escape rate from unemployment prior to start thus largely depends on the expected effectiveness of the programme. If expectations are positive and the effect overshadows the negative aspects of participation, the net effect on the value of unemployment is positive and the exit rate slows down. If expectations are negative, at least for some of those referred to the programme, we would expect an increased exit rate prior to start.

Finally, the lower value of unemployment could also affect job turnover and future risk of unemployment if the programme referral caused the job seekers to accept less qualitative job matches.

4. The data

Most recent research on unemployment duration in Sweden has utilised unemployment register data from the SLMB (*Händel*). *Händel* continuously follows the events of each unemployed job seeker between periods of open unemployment and enrolment in programmes until deregistration.[10] A drawback with *Händel* is the heavy reliance on self-reported information. For instance, job seekers who find jobs or leave the work force sometimes omit to inform the employment office. If the employment office is notified after some time, the code for exit cause would be correct, but the deregistration date could be wrong. If the employment office is *not* notified, this subsequently causes deregistration with the code "reason unknown", in which case both the deregistration code and the registered date for leaving unemployment are wrong. Furthermore, shorter periods of inactive job seeking sometimes pass without inducing an event. One such example is sickness, where the recipients remain registered as unemployed but instead of UI-compensation collect sickness allowances. As a consequence of these register deficiencies, the unemployment register most likely exaggerates the true number of unemployed persons at any given time.

The improved quality in recent years of the UI-payment register data (*A-stat*), administered by the UI Funds, offers an alternative to *Händel* in following unemployment spells.[11] Again, information is based on reports from the claimants. However, rather than notifying the employment office, UI-eligible job seekers leaving unemployment simply quit sending in their applications for UI compensation. More importantly, falsely reporting to the UI fund could lead to prosecution. *A-stat* should thus be more reliable then *Händel*. *A-stat* does not, however, contain information about disruption cause.

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[10] *Händel* also contains individual information on gender, age, educational level, citizenship, working disability, occupation sought, education for and experience in occupation sought, etc.

[11] *A-stat* contains weekly data on the number of UI-compensated days, type of UI benefit and benefit level for all unemployed who are entitled to either Basic insurance or Income-related UI benefits since January 1, 1999. Data also include information on payment decisions, previous income and remaining days of benefits.

Were it not for the register deficiencies in *Händel*, *Händel* and *A-stat* would match when it comes to the timing of exits. A disruption of benefit periods is usually due to the start of a job or a programme or some other activity outside the employment office.[12] Comparing the date for a registered event in *Händel* with the disruption date of the UI spell in *A-stat* should provide an estimate of the measurement error involved in using *Händel* for approximating unemployment duration. The sample used in Table 3 contains UI-eligible job seekers ending their compensation period in the first six months of 2004.[13] The table shows the deviation distribution of four different exit-types; programme, job, other known cause, and unknown cause. The sample is restricted to UI eligible job seekers ending their spell with at least 20 remaining days of UI benefits. This restriction is necessary to avoid those (very few) remaining unemployed after exhausting their benefit period.

The positive values of Table 3 imply lags in *Händel*, i.e., the transition dates in *Händel* are set at later dates than the disruption dates in *A-stat*. The few negative values are most likely due to the difficulties of establishing the exact date of disruption in *A-stat*.[14] Not surprisingly, exits to programmes (for instance vocational training, work experience and recruitment incentive schemes) exhibit the highest degree of correspondence between the databases. Among the various types of exits, this is the one in most control of the case workers. More than 80 per cent of the UI disruptions due to start of a programme have a corresponding transition date within 7 days.

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[12] Although very rare, job seekers run a slight chance of remaining unemployed after benefit exhaustion without any intervention by the employment office. Less than 0.3 per cent (601 individuals) of the average number of unemployed persons during the fourth quarter of 2004 remained unemployed two weeks after benefit exhaustion (IAF, 2005).

[13] The sample is unrelated to the samples used in the experiment analyses.

[14] Recall that *A-stat* contains weekly data. The negative values could also be due to some case workers, as a matter of routine, setting the transition date to certain days in the week, for instance Mondays.

Table 3: No. of days between the reported UI-benefit spell disruption date (A-stat), and the reported transition date from unemployment (Händel), by disruption cause

Exit	to programme	to job[a]	to other, cause known[b]	to other, cause unknown
Quintile				
0	-2.0	-2.0	-2.0	0.0
10	0.0	0.0	0.0	8.0
20	0.0	0.0	0.0	14.0
30	0.0	0.0	3.0	20.5
40	0.0	2.0	7.0	29.0
50	0.0	5.5	9.0	38.0
60	0.0	7.5	16.0	50.0
70	4.0	14.0	27.0	65.5
80	7.0	26.5	50.0	86.0
90	34.0	56.0	79.0	120.0
95	73.0	87.0	105.0	148.0
99	133.0	154.0	179.0	206.5
100	244.0	238.5	240.0	263.5

Note: a) Part-time jobs and employment by the hour are excluded since these jobs in general do not terminate a UI spell. b) Includes exits to retirement and training (other than labour market training). Sample sizes: Programmes: 4,893, Jobs: 14,671, Cause known: 3,072, Cause unknown: 1,840.

According to the table, unemployment spells ending in jobs run a considerable risk of being overestimated, thus confirming the above misgivings. Approximately 40 per cent of the exits are registered at least one week after termination of the UI spells, and roughly 30 per cent are registered at least two weeks late. The correspondence is even worse regarding exits to retirement and regular training (cause known), and, in particular, exits with an unknown cause.

There are at least two strong reasons for using *A-stat* to study unemployment duration in these experiments. First of all, *Händel* was used to identify the target groups. With the deficiencies in *Händel*, this suggests that randomisation could, in fact, involve people no longer unemployed. Reaching out to the experiment group members by sending out notifications of the scheme services, we would expect those wrongly coded in the experiment group to be systematically exposed and corrected as opposed to the randomised out controls. This would cause an upward bias in the difference in exit rates between the groups in favour of the experiment group.

Second, with an expected higher coach/job-seeker contact frequency in the experiment group, the risk of wrongly defined, and/or late dated, events in *Händel* is expected to be reduced. Put differently, the events in the experiment group are expected to be more accurately coded, which once again is likely to work in favour of the demonstration services' results. To conclude, using *Händel* to analyse pre-programme effects most likely involves overestimating the difference in exit rates in favour of the experiment group.

Relying instead on *A-stat*, the analysis must be narrowed to those qualified for UI benefits. In Jämtland, and to some extent also in Östergötland, where the services were targeted towards the UI eligible, the loss of observations is relatively small (20 and 26 per cent respectively).[15] In the Uppsala demonstration, however, 53 per cent of the sample was lost. Also, since *A-stat* lacks information on disruption cause, it is linked to *Händel* and the event-specific information at the disruption date. If, however, a corresponding disruption cause is not found within two weeks, a constructed disruption cause is used.

[15] Claims for UI benefits are sent in to the UI Funds in arrears, usually between two and four weeks after the week of unemployment. Hence, fully updated information on current UI claimants is not available.

5. How to interpret the treatment parameter

An important feature of the design of these experiments is the possibility for control group members to receive regular services. This is a consequence of the experiments originally being designed to assess the effectiveness of the new services compared with the regular services. Thus, rather than the mean impact of referral compared with no referral ("the mean-effect-of-referral"), the outcome difference provides an estimate of the *marginal* effect of referrals to the tested services compared to a "normal" dose of referrals to the regular services. With the very short evaluation periods, however, the vast majority of the control group members are not referred to any services at all.

Evaluation situations where the difference in "treatment" between experiment and control group are less than the desired 100 per cent are very common.[16] In order to retrieve the mean-effect-of-referral parameter when controls receive similar services requires the use of non-experimental methods, or at least non-experimental assumptions. By weighting up the outcome difference between experiment and control group by the difference in fraction treated in each group, the effect of treatment (referral) is identified under the assumption that the impact of referral is the same in the experiment group offered the demonstration services and the control group offered the substitute services.[17] In the present context, however, this would be a strong assumption since the experiment and control group referrals are expected to involve different types of programme activities. For instance, the regular services could involve employment training and work experience programmes that in contrast to the demonstration services are relatively expensive meas-

[16] Heckman et al. (1999) show that the fraction of controls receiving services in a variety of U.S. experiments in the 80's and 90's in many cases amounts to 20-50 per cent, and that the difference in the receipt of treatment often falls short of 50 per cent.

[17] This assumption is a generalised common effect assumption. The generalised version, allowing for heterogeneous effects, is that the mean impact of referral in the experiment group is the same as the mean impact of referral among those referred in the control group (Heckman et al. (1999).

ures normally preceded by implicit agreements between case workers and job seekers. In turn, this could be reflected in the pre-programme outflow where the control group members are more motivated to participate and perhaps less motivated to leave unemployment before programme start. Thus, opposite to assessments of various labour market measures where substitute measures are expected to reduce the outcome difference between treatment and comparison group, referrals in the control group could, in fact, *increase* outcome differences.

In addition to control-group substitution, all experiment and control groups contain persons wrongly identified as members of the targeted populations. This primarily refers to job seekers who already had a job or a regular programme about to start, and whose referrals correspondingly were withdrawn.[18] Since these were equally distributed between the experiment and control groups, their presence should not affect the outcome differences. Inactive observations do, however, reduce the scope for identifying them.[19]

The exact extent of no-shows and control group members being referred to substitute measures in the samples is unknown since we have no information on referrals, only on regular programme starts.[20] However, the fraction of experiment group members starting a regular programme in the pre-programme period is a good approximation of the occurrence of no-shows. Further, the difference in the amount of regular programme starts between experiment and control group in the pre-programme period should in-

[18] In evaluating the programme, these would be referred to as "no-shows".

[19] The project teams agreed to review the *daily notes* for the target population before randomisation, and remove those with wrong or inconsistent information and/or programmes about to start. The daily notes is an instrument in the public office internal information system with which the case workers keep track of the dates for, and contents of, meetings and agreements with the unemployed. This "cleansing"-procedure was repeated before every new admission and contributed to reduce the amount of no-shows dramatically, although not completely eliminating them.

[20] Note, however, that these starts could lie outside the range of the evaluation period.

dicate the potential importance of substitute measures. A small difference suggests an impact estimate with large correspondence with the mean-effect-of-referral parameter.

6. Empirical strategy

Each demonstration programme is studied separately. UI claimants receiving a full week of compensation in the week of notification are followed until the week of the programme start, unless the UI spell is disrupted before start.[21,22] The cause of disruption is obtained from *Händel*. Matching *A-stat* and *Händel* data, a two-week lag is applied in which the disruption date from *A-stat* and the date of the registered event in *Händel* is allowed to deviate. Where the deviation is larger than two weeks, a constructed event ("cause unknown") is assigned to the job seeker.

I report mean differences in UI-disruption incidence in the pre-programme period due to various exit causes. The analysis thus not considers jobs acquired during the pre-programme period but with start dates outside the range of the evaluation period. A disruption is defined as anything between very temporary intermissions of 0.5 days to permanent exit.[23][24,25]

To utilise the longitudinal data, and to account for the flow into regular programmes, I also perform survival (hazard) analyses. The hazard is de-

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[21] The start week is included in the evaluation period.

[22] In Uppsala, those part-time employed at randomisation received less than a full week of compensation the week of the referral.

[23] An exception is made for the part-time employed in Uppsala, for whom a one-week disruption limit applies.

[24] Note that by accounting for very short disruptions is shown that disrupting the UI spell is *not* necessarily equivalent to not participating in the demonstration services.

[25] Other outcome indicators, for example "collecting benefits (yes/no) the week of demonstration start", were also considered. However, studying disruption incidence offers the opportunity to explore incentive effects throughout the entire pre-programme period and not just at the end.

fined as the conditional probability of leaving unemployment at time t, given that the person is still unemployed at t.[26] Since we are only interested in exits to jobs or out of the workforce in a short interval, many observations will have ongoing unemployment spells at the end of the evaluation period. Some will also have exited unemployment to participate in regular programmes. In both cases, these observations are treated as right censored observations, which means that their time T is set to the time until the end of the evaluation period or until starting a regular programme respectively. In the remaining cases, T refers to a completed (non-censored) unemployment period where transitions to jobs or out of the workforce are jointly examined. The hazard $\theta_i(t)$ at time t, for person i, is written as

$$\theta_i(t) = \lim_{\Delta t \to 0} \frac{Pr\{t \leq T_i < t + \Delta t \mid T_i \geq t\}}{\Delta t}. \quad (1)$$

Following Meyer (1990), I specify the baseline hazard for T without making any functional form assumptions and estimate the model semi-parametrically. The hazard function of T for job seeker i is:

$$\theta_i(t \mid x_i) = exp(x_i{}'\beta)\theta_0(t) \quad (2)$$

where $\theta_0(t)$ is the unrestricted baseline hazard, x_i is a covariate vector, and β is the corresponding parameter vector. Equation 3 is of the proportional hazard model variety in which the explanatory variables have a constant proportional effect on the hazard. Note that the explanatory variables adjust for random heterogeneity in observables. I report the effects both including and excluding the explanatory variables.

[26] See Lancaster (1990).

7. Results

7.1 Mean differences

Table 4a reports the proportion of disrupted and non-disrupted UI spells during the pre-programme period for experiment and control group members. It also reports disruption causes. Disruptions without a corresponding disruption cause are referred to as exits to "unknown destinations". The exact proportion of exits to the respective disruption cause should be interpreted with some care since they are most likely sensitive to local register routines and the case workers′ possibility of keeping track of job seekers.

In the Jämtland demonstration, a large proportion of the sample had pauses in the UI spells in the on average 6.3 week evaluation period. Less than 50 per cent had a non-interrupted UI spell. Among those with non-censored UI spells, only about 50 per cent had a reported exit cause within two weeks in the unemployment register. The experiment and control groups display almost similar portions of programme participants. This indicates that besides those who already had a programme waiting at the time of randomisation, very few additional control group members initiated a programme, at least not in the pre-programme period.[27] The experiment group had a somewhat higher exit rate to all known and unknown exit causes. The significant difference in exits to "other known destinations", shown in Table 4b, is the result of relatively more people reporting temporary job-search interruptions.

In total, 46.6 and 37.9 per cent had pauses in their UI spells at some point during the pre-programme period in the experiment and control group respectively. This 8.7 percentage points positive difference is statistically significant at the 1% significance level.

[27] As commented on earlier, no information is available as to what extent control group members were notified of a programme *during* the pre-programme period that started outside the range of the evaluation period.

Table 4a: Disrupted (and non-disrupted) UI spells, on disruption causes

	Jämtland				Uppsala		Östergötland	
	Exp. group (All)	*Exp. group (JSA)*	*Exp. group (no JSA)*	Con-trol group	Exp. group	Con-trol group	Exp. group	Con-trol group
Non-censored:								
Job[a]	0.188	0.211	0.164	0.158	0.109	0.058	0.090	0.098
Other known des-tinations[b]	0.050	0.049	0.052	0.030	0.029	0.017	0.053	0.026
Unknown destinations[c]	0.228	0.260	0.196	0.191	0.189	0.260	0.118	0.127
Sum non-censored	0.466	0.520	0.412	0.379	0.327	0.335	0.261	0.251
Censored:								
Reg. pro-grammes[d]	0.101	0.110	0.092	0.132	0.022	0.021	0.132	0.193
Ongoing spells	0.433	0.370	0.496	0.489	0.651	0.645	0.608	0.557
Sum cen-sored	0.534	0.480	0.588	0.621	0.673	0.665	0.740	0.750
No. of ob-servations	496	246	250	507	275	242	357	379

Notes: [a)] Also includes part-time jobs. [b)] Includes exits due to retirement, regular studies and temporary job-search interruptions. [c)] Includes exits with a registered event "reason unknown" in Händel, and exits lacking an event within 14 days from the UI disruption date. [d)] Includes exits to for instance labour market training and subsidised employment.

An interesting result is that practically the entire positive effect in the Jämtland demonstration stems from the subgroup that, besides increased monitoring, was referred to job-search assistance activities. The exit rate of 52.0 per cent, programme exits held constant, suggests a large and highly significant positive pre-programme effect of 14.2 percentage points, or 37.5

per cent (Table 4b). The differences in exits to jobs and to "unknown destinations" are both significant. The effect of being assigned only the increased monitoring is small but slightly positive (3.3 percentage points). It thus seems that being referred to job-search activities in groups is considered a far less attractive alternative than being referred to continuous individual follow-up meetings.

Table 4b: Pre-programme effects, mean differences (standard errors are within parentheses)

	Jämtland			Uppsala	Östergötland
	All	*JSA*	*No JSA*		
Non-censored:					
Job[a]	0.030 (0.024)	0.054* (0.031)	0.006 (0.029)	0.051** (0.024)	-0.008 (0.022)
Other known destinations[b]	0.021* (0.012)	0.019 (0.016)	0.022 (0.016)	0.013 (0.013)	0.027* (0.014)
Unknown destinations[c]	0.037 (0.026)	0.069** (0.033)	0.005 (0.031)	-0.071* (0.037)	-0.009 (0.024)
Sum non-censored	0.087*** (0.031)	0.142*** (0.039)	0.033 (0.038)	-0.007 (0.042)	0.010 (0.032)
Censored:					
Reg. programmes[d]	-0.031 (0.020)	-0.022 (0.025)	-0.040 (0.024)	0.001 (0.013)	-0.061** (0.027)
Ongoing spells	-0.056* (0.031)	-0.119*** (0.038)	0.007 (0.039)	0.006 (0.042)	0.051 (0.036)

Note: No. of observations, Jämtland (All): 1003, Jämtland (JSA): 753, Jämtland (No JSA): 757, Uppsala: 517, Östergötland: 736. : a) Also includes part-time jobs. b) Includes exits due to retirement, regular studies and temporary job-search interruptions. c) Includes exits with a registered event "reason unknown" in *Händel*, and exits lacking an event within 14 days from the UI disruption date. d) Includes exits to for instance labour market training and subsidised employment. *, **, *** refer to significance at 10, 5 and 1 per cent levels respectively.

Compared to Jämtland, the overall disruption intensity in the pre-programme period in Uppsala is lower (Table 4a, columns 5 and 6). This should, at least partly, be due to the on average shorter pre-programme period (4.4 weeks). The relatively few exits to regular labour market programmes could be the result of an unusually effective cleansing-procedure where those with programmes about to begin almost completely were eliminated from the sample before randomisation. More likely, however, this reflects the low priority of this group at the employment offices. The experiment and control groups deviate in both the reported job-exit frequency (higher in the experiment group), and in the exits to "unknown destinations" (higher in the control group). According to Table 4b, both differences are significant. This could be a real effect of the referrals to the demonstration services. There is a considerable risk, however, that the deviations are due to information asymmetries where the higher case-worker/job-seeker intensity in the demonstration services reduces the amount of "lost" observations. In total, a small and insignificant negative effect is found on the disruption intensity.

Östergötland applied the shortest pre-programme period of the three compared schemes; three weeks. Noticeable, therefore, is the high exit rate to regular programmes (Table 4a, columns 7 and 8). As much as 19 per cent of the control group initiated a regular programme during the study period, which significantly outweighs the corresponding portion in the experiment group (13%). This reflects the focus at this time on the subgroup of unemployed youth and that the demonstration services here, more than in the other demonstration programmes, were an alternative to other active measures. These measures were dominated by work-practice schemes, the Youth Guarantee and preparatory training courses.[28] There are, ex ante, no reasons to

[28] The Youth Guarantee is a programme where the municipalities sign agreements to offer full-time activities to long-term unemployed youth. Carling & Larsson (2005) conclude that these activities involve very much the same distribution of labour market programmes offered youth at the regular employment offices, i.e., mainly work-practice schemes and training programmes.

assume that these measures, would have a systematically different impact on pre-programme exits compared to the experiment group activities. However, due to the difference in programme intensity, a direct comparison of the transitions to jobs and other exits would not be as relevant. Despite the possible effect on exits to "other known destinations", the total share of disrupted UI spells is almost identical between the groups. Table 4b reveals a very small positive pre-programme effect on the disruption intensity.

7.2 Proportional hazard model estimation

This section reports the pre-programme effects on the off-UI receipt hazard rate where exits to regular programmes are censored. Both the non-adjusted results and the results adjusted for randomly arisen heterogeneity in observables are presented in Table 5. Figures 1a-3c (below) depict the weekly hazards to jobs and "other exits" both aggregated and separated.[29]

First of all, in correspondence with the descriptive results from Table 4a and 4b, admissions in the Jämtland demonstration generate a positive significant effect on the outflow from unemployment before programme start. The non-adjusted point estimate reports a statistically significant 27.4 per cent increased exit rate as a result of being referred to the services. The reported adjusted impact (25.9%) is somewhat smaller but still significant.[30] The non-adjusted estimations report a large (43.9%) positive and significant effect from referrals to the combination of services (JSA), and a small (10.2%) positive but insignificant effect from referrals to only the increased monitoring, or the no-JSA, services. Adjustment generates a somewhat

[29] The hazard estimates are based on the Lifetable method (Allison, 1995).

[30] The larger standard errors are due to the loss of statistical degrees of freedom.

smaller JSA point estimate, 37.9 per cent, and a slightly larger no-JSA impact estimate of 12.0 per cent.[31,32]

The non-adjusted impact estimate from being referred to the Uppsala demonstration services is practically zero (-0.5%). Controlling for random differences in observable characteristics, the impact estimate is adjusted upwards (3.0%).[33]

The results from the pre-programme effect estimations in the Östergötland demonstration provide no evidence of any behavioural adjustments. Both the adjusted and non-adjusted impact estimates are close to zero. The results could possibly be explained by the relatively short (3 weeks) pre-programme interval. Also, the large focus on unemployed youth reduced the "treatment dose" between the groups. As it turned out, many control group members faced a considerable chance/risk of being referred to regular activities in the near future.[34]

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[31] Comparing the two treatments, the difference in outcomes corresponds to p-values of 0.13 (non-adjusted), and 0.097 (adjusted).

[32] Figure 1a shows that while the no-JSA and the control group hazards follow each other with only a small degree of deviation, the JSA hazard is higher throughout the period. According to theory, we would predict a gradually increasing hazard as time approaches programme start (here at 5 and 7 weeks). However, the Jämtland demonstration applied a two-step enrolment procedure where the job seekers first had to attend an individual meeting, before actually starting the programme services. The result thus suggests that pre-programme effects could appear also in very short pre-programme intervals. The two types of services show somewhat different exit patterns to the different destinations (Figures 1b and 1c). While the JSA job hazard peaks in the week leading up to programme start and the "other exit" dips (primarily the fifth week), the no-JSA hazards show the opposite pattern.

[33] The plotted hazards in Figure 2b and 2c correspond with the results from Table 4a and 4b where the registered job exits were higher among the experiment group members, and where the exits to other destinations were relatively more common in the control group.

[34] The destination-specific hazards in Figures 3b and 3c depict a stable job hazard (3b) and a sharply increasing hazard to other exits (3c) among the experiment group members.

Table 5: Pre-programme effects on the hazard rate, non-adjusted and adjusted (standard errors are within parentheses)

	Jämtland			Uppsala	Östergötland
	All	*JSA*	*No JSA*		
Pre-programme effect (non-adjusted)	0.274*** (0.098)	0.439*** (0.114)	0.102 (0.122)	-0.005 (0.153)	0.024 (0.146)
Pre-programme effect (adjusted)	0.259** (0.101)	0.379*** (0.120)	0.120 (0.128)	0.030 (0.159)	-0.010 (0.153)

Note: No. of observations, Jämtland (All): 1003, Jämtland (JSA): 753, Jämtland (No JSA): 757, Uppsala: 517, Östergötland: 736. *, **, *** refer to significance at 10, 5 and 1 per cent levels respectively.

Robustness of the results have been tested in two respects. First of all, by modifying the two-week requirement in finding an exit cause from the unemployment register, the relative exits to programmes and other destinations could be altered, which in turn could affect the results.[35] However, performing analyses on 1, 3 and 4-week requirements only have negligible effects on the impact estimates. Second, by limiting the analyses to UI receivers with at least 20 days remaining in the current benefit period, only small changes of the impact estimates are found.[36]

7.3 The flow back to unemployment

To appreciate the importance of pre-programme effects on unemployment duration and UI savings, the persistence of the pre-programme outflow is

[35] If, for some reason, programme participation were systematically registered with a larger delay in the experiment group, a three- or four-week (instead of a two-week) requirement would reduce the number of exits accounted for in the analyses. This would thus have a negative effect on the impact estimate.

[36] Job seekers close to benefit exhaustion either received 300 fresh days of UI compensation, in which case they remained in the demonstration programme, or a referral to the *Activity Guarantee*, in which case they were registered as programme participants.

crucial. For instance, if a positive effect on the exits before start is the result of very temporary interruptions, the programme would only have generated minor total savings in the UI system.

Analysing the flow back to unemployment, outcome differences between the experiment and control groups do not necessarily provide a causal interpretation of the effect of programme referral on subsequent risk of unemployment. This is because the groups compared in each experiment need not be comparable.[37] The analysis thus only describes the reoccurrence of unemployment spells among the subsets of experiment and control group members with disrupted UI spells.

Table 6 reports the number of unemployed weeks in the 26 weeks after interruption of the UI spell among experiment and control group members leaving unemployment in the pre-programme period. Unemployed weeks here include both periods of open unemployment, where UI benefits are collected, and spells of regular programme participation, where the job seeker instead receives *activity support*.[38] I thus combine information from Händel and A-stat.[39]

Overall, the risk of returning to unemployment is high. Only 25-30 per cent has no reported days of unemployment in the 26-week period. In the different experiments, on average 10-12 weeks were spent either as openly unemployed or as regular programme participants. Generally, the experiment groups report somewhat more unemployed weeks, although the differences

[37] For instance, if the experiment group members are found on average to be more likely to return to unemployment, this could either be due to the referrals having a negative effect on the job matches, or due to the relatively worse job matches being realised earlier because of the programme referrals.

[38] Compensation during regular programmes (activity support) and open unemployment is the same for UI eligible job seekers. For those not entitled to UI benefits, the activity support amounts to SEK 223 each day (Faktablad, Arbetsmarknadsutbildning, AMS).

[39] A-stat is used for following periods of open unemployment while Händel data are used for the regular programme spells.

(0.1-1.6 weeks) are not significant. The differences are also small studying openly unemployed spells and regular programme spells separately.

Among those with disrupted UI spells in Jämtland, the difference in unemployment is larger among those offered only the increased monitoring (1.6 weeks) and smaller among those offered the JSA-services (0.5 weeks). The positive effect on the pre-programme outflow among the latter thus not seems to be the result of a larger amount of very short interruptions of the UI spell, for instance due to reporting sick the first day of the programme or accepting more temporary jobs. The same does not necessarily hold for the no-JSA group combining an insignificant 12 per cent increased exit rate before programme start (Table 5) with a similarly insignificant 1.6-week increase in number of unemployed weeks in the following 26 weeks.

Both Uppsala and Östergötland report very small deviations in unemployment between experiment and control group members. Interesting to note is that although starting a regular programme was a significantly more common pre-programme exit cause among the control group members in Östergötland, the average number of weeks in regular programmes after 26 weeks are almost similar. Apparently, taking part in the experiment services only seems to have postponed the decision of participation in a regular programme.

7.4 The pre-programme effects into perspective

To put the impact estimates of the Jämtland demonstration into some perspective, the average (adjusted) 26 per cent enhanced hazard rate corresponds to a similar drop in the average unemployment duration in the pre-programme period. With an average pre-programme UI spell of 22 days in the control group, this translates into a 5.7 UI-day drop (8.3 and 2.6 UI

Table 6: Number of weeks as openly unemployed or programme participant in the 26-week period that follows after exit from unemployment in the pre-programme period

	Jämtland				Uppsala		Östergötland	
	Exp. group (All)	*Exp. group (JSA)*	*Exp. group (no JSA)*	Con-trol group	Exp. group	Con-trol group	Exp. group	Con-trol group
Quantile (%):								
90	25.4	25.4	25.6	24.3	25.2	25.0	23.6	25.0
75	20.9	20.6	21.1	18.1	19.8	17.8	18.2	18.7
50	9.8	8.1	10.8	9.0	8.5	8.0	12.0	10.2
25	0.0	0.0	1.0	0.0	0.0	0.0	0.6	2.0
10	0.0	0.0	0.0	0.0	0.0	0.0	0.0	0.0
Average no. of unemployed weeks	11.3	10.8	11.9	10.3	10.5	10.2	11.2	11.1
Whereof as in:								
-open unem-ployment	9.6	9.2	10.1	9.0	9.4	9.3	8.9	9.0
-programme	1.7	1.6	1.8	1.3	1.1	0.8	2.3	2.1
Difference between ex-periment and control group in average no. of unemployed weeks[a]	1.0 (0.9)	0.4 (1.1)	1.6 (1.2)	-	0.4 (1.5)	-	0.1 (1.3)	-
No. of obser-vations	231	128	103	192	90	81	93	94

Note: [a] Standard errors are within parentheses. *, **, *** refer to significance at 10, 5 and 1 per cent levels respectively.

days for the JSA and the no-JSA services respectively) in the pre-programme period. With an average daily compensation of SEK 626 (in the control group), the reduction of the UI spell in the pre-programme period saves SEK 1,8 million in UI benefits. Since the demonstration's total expenditures were SEK 2,5 million, the savings covered more than 70 per cent of the expenses.

8. Conclusions

This paper has investigated the significance of pre-programme effects of being referred to active placement measures at public employment offices in Sweden. Using experimental data from three separate demonstration programmes in three different regions in 2004, disruptions of the UI spell in the interval between notification of the programme and programme start was compared between the experiment and control groups. My findings support previous research suggesting that the response to the "disutility" involved in complying with activation requirements could be substantial. In the Jämtland demonstration, the 38 per cent increase of the hazard rate preceding a combination of treatment including job-search assistance activities and increased job-search monitoring, translates into an almost two week reduction of the ongoing unemployment spell. By offering two different treatment packages, with random assignment to each treatment, I conclude that the positive effect derives from the referrals to the job-search assistance activities. The effect of referrals to recurrent interviews monitoring the job search is significantly lower and non-significantly different from the exits of the control group. This finding is possibly the result of the job-search assistance activities being arranged in groups, which for some unemployed persons may be experienced as stigmatising, as opposed to the in-person interviews. Comparison of the subsequent unemployment spells gives no indication of the enhanced exit rate being the result of less attractive job matches, or other temporary UI disruptions.

Two of the demonstrations, in Uppsala and Östergötland, show no evidence of any effect on the pre-programme exit rate. From three in various respects different experiments, it is difficult to formulate any general conclusions, for instance as to when a "motivation effect" can be expected to occur. However, at the individual level, one can think of a positive pre-programme effect as a product of two mechanisms, both needed for a positive outcome to arise. First, the programme must decrease the utility of unemployment relative to other alternatives in order to create incentives to speed exits. Second, the actions, in terms of increased job-search efforts and/or the lower reservation wage, must have good prospects of paying immediate dividends in terms of unemployment exit. In order to distinguish between pre-programme and programme effects, the exits must take place in the pre-programme period.

With this in mind, I propose two possible explanations for the diverse results. First, whereas the Jämtland demonstration invited a broad group of unemployed to participate, the other two targeted locally specific difficult groups of the unemployed. The latter groups could on average have relatively less scope for finding a way out of unemployment. Also, the offered activities might appear relatively more attractive for these groups. Second, in an article examining sick leave in Sweden as a social phenomenon, Lindbeck et al. (2004) argue that the large local variations in sick leave, in major part, are related to what they refer to as a "sick leave culture" with origins in local-specific attitudes towards sick leave. An interesting fact presented in that study is that the county of Jämtland reported the highest sick leave in the country in 2003 with almost twice as many reported days of sick leave as the county with the lowest sick leave. If there exists a sick leave culture in Jämtland, it would be easy to imagine a similar tradition within the UI system. In support of this notion is the outcome of the "Job-seekers survey", a monthly survey among the currently unemployed or programme participants performed by the Swedish Labour Market Board (SLMB), in which Jämtland has reported the largest proportion of non-active job seekers in two of the last three years. A low job-search effort level indicates inadequate control of the requirements as UI receivers, which, in turn, could imply great scope for motivation effects. The analysis performed in this paper is not clear as to the destinations of the unemployment exits (approximately 50 per cent of the non-censored UI spells lacked a corresponding disruption cause in the register data). The results, however, indicate that the positive effect is equally due to job exits and other "unknown" exits, including sick leave.

Unlike the expensive and often inefficient training programmes and special youth measures, active placement efforts with the purpose of providing necessary skills for effective job search have, in the evaluation literature, been shown to be both effective and less costly (see Martin & Grubb, 2001, and Calmfors, Forslund & Hemström, 2002, for overviews). The results from the Jämtland demonstration highlight the role of these activities as instruments to test work motivation and to reduce the excess use of UI benefits. The course of Swedish labour market policy in recent years has been to downsize and target these activities towards difficult groups of unemployed, particularly youth and long-term unemployed. The SLMB has on several occasions received much criticism for abandoning large groups of unemployed and for replacing the personal services at the employment offices with the enhanced use of self-service Internet services and a recently implemented telephone service (Arbetsmarknaden, 2003, Dokument inifrån, 2004, Lo-tidningen, 2005). Based on the results from this and earlier programme evaluation studies, as well as recommendations to rely as much as possible on combinations of active placement efforts and monitoring of the job-search activity (Martin & Grubb, 2001), the Swedish government and the SLMB are well advised to reconsider their current strategy.

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The Jämtland-scheme hazards

Figure 1a: Pre-programme disruption of UI spell, all exits

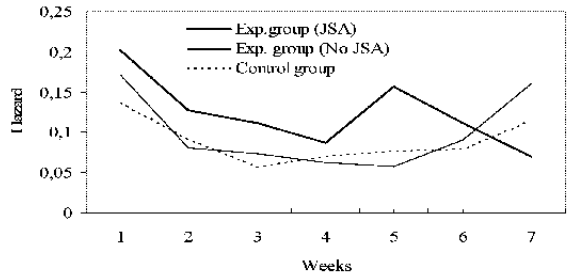


Figure 1b: Pre-programme disruption of UI spells, job exits

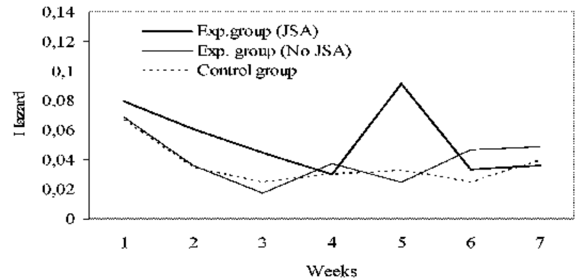
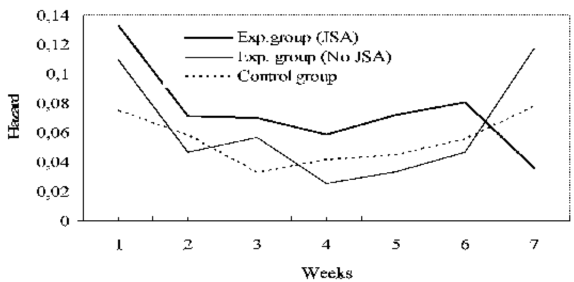


Figure 1c: Pre-programme disruption of UI spell, other exits



The Uppsala-scheme hazards

Figure 2a: Pre-programme disruption of UI spell, all exits

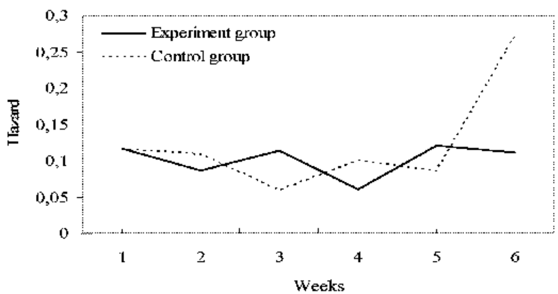


Figure 2b: Pre-programme disruption of UI spell, job exits

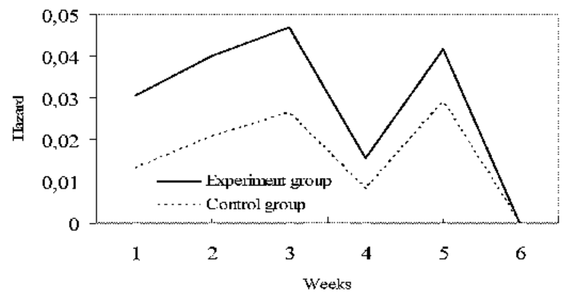
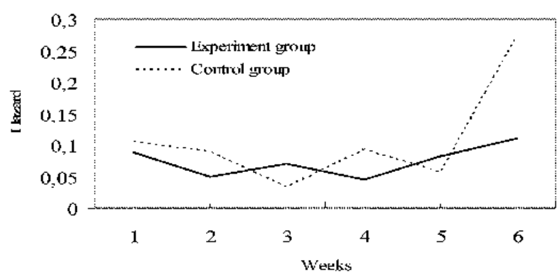


Figure 2c: Pre-programme disruption of UI spell, other exits



The Östergötland-scheme hazards

Figure 3a: Pre-programme disruption of UI spell, all exits

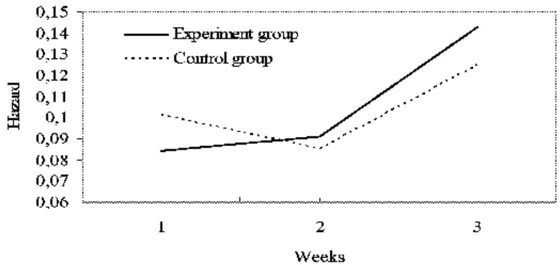


Figure 3b: Pre-programme disruption of UI spell, job exits

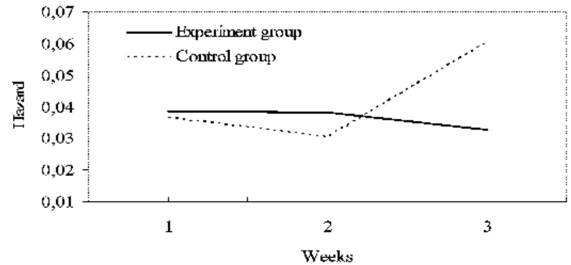
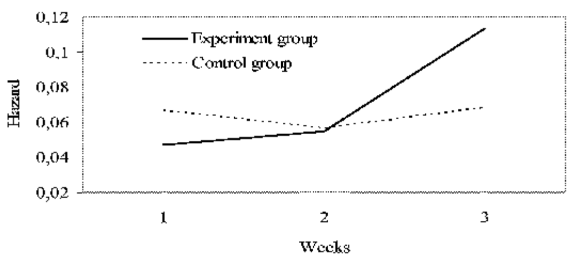


Figure 3c: Pre-programme disruption of UI spell, other exits



Appendix A

The last exercise of this paper deals with the alleged register effects arising from deficiencies in *Händel*. The purpose is to explore the bias involved in using poor data when analysing unemployment duration and pre-programme effects.

As described in Section 4, the flaws in the unemployment register affect the prospect of accurately portraying pre-programme incentive behaviour in two possible ways. First, the experiment and control groups may include persons no longer unemployed. Second, due to the higher personnel intensity, experiment group members have a higher chance of being correctly coded. Throughout, the first type of error is referred to as the *sample error component*, whereas the latter denotes the *measurement error component*.

Exploring the *sample error component* involves identifying individuals who, although they were registered as unemployed at the time of randomisation, we have strong reasons to believe were, in fact, not unemployed. Two restrictions are imposed on those individuals; 1) They must have ended an UI-payment spell at some point 16 weeks prior to the week of the referral. The 16 weeks is arbitrarily chosen. However, it is important that the applied period is not too short.[40] 2) They must have at least 30 UI days remaining in the last week of receiving compensation. This requirement eliminates those (very few) who have used up all their UI days and still remain unemployed. The underlying assumption is that UI recipients do not stop collecting benefits with remaining UI days unless they leave unemployment status.

The first row of Table A repeats the non-adjusted pre-programme effects reported in Table 5 that will constitute the benchmark values here.

[40] Table 3 showed that events registered 16 weeks late were unusual, but not unique.

There is one exception however. As a matter of convenience the results from the Uppsala demonstration include only those full-time unemployed. Hence, instead of -0.005, -0.029 is used as the comparison estimate. The second row reports the pre-programme effect using the same sample as in the benchmark estimations. This way, the register effect produced using the "wrong" sample is held constant and the importance of the *measurement error component* is explicated. The third row reports the pre-programme effect taking into account both error components. As mentioned earlier, each component separately suggests a distortion of the pre-programme effect in favour of the experiment group. We thus anticipate the total register effect, reported in the last row, to be positive.

Row 2 throughout reports the *measurement error* to positively bias the effect parameter. While the bias is relatively modest in the Jämtland experiment, both Uppsala and Östergötland report substantially inflated effects of the referrals. Introducing additional observations in row 3, we would expect the already upward biased estimates to increase even further. This prediction fails, however, in analysing the results from the Jämtland and the Östergötland demonstration, where the bias instead becomes smaller. In Uppsala, accounting for the *sample error component* contributes to double the upward bias of the pre-programme effect. These results could be due to the Uppsala demonstration being the only experiment not originally targeted towards the UI eligible. This reduced the scope for the *sample component* to affect the outcome. There are, however, no obvious explanations for why the information of a larger number of control group members was corrected in the pre-programme period.

In aggregate, the bias associated with using the unemployment register is slightly negative in the Jämtland demonstration, and very much positive in the Uppsala and Östergötland demonstrations. In the latter two, the almost zero impact estimates become positive, 0.578 and 0.343 respectively, and highly significant. The size of the upward bias is expected to be negatively correlated with the evaluation period length, since the number of events per time interval is expected to be gradually diminishing. The results reported here should therefore represent upper bounds for the register effects. Finally, note that the larger samples in row 3 do not (!) reduce the standard errors surrounding the impact estimates. The reason is that the lags in reported events in the unemployment register reduce the number of events accounted for in the analyses.

Table A: Pre-programme effects on the hazard rate (unadjusted), using *Händel* data (standard errors are within parentheses)

	Jämtland			Uppsala	Östergötland
	All	*JSA*	*No JSA*		
Pre-programme effect (benchmark) (A)	0.274*** (0.098)	0.439*** (0.114)	0.102 (0.122)	0.008 (0.175)	0.024 (0.146)
Pre-programme effect (unemployment register data, benchmark sample) (B)	0.333*** (0.127)	0.502*** (0.146)	0.142 (0.159)	0.296 (0.291)	0.478** (0.204)
Pre-programme effect (unemployment register data, new sample) (C)	0.240** (0.102)	0.377** (0.118)	0.088 (0.128)	0.578*** (0.193)	0.343** (0.149)
Total register effect (C)-(A)	-0.034	-0.062	-0.014	0.570	0.319

Note: No. of observations; Jämtland: B: 1,003 and C: 1,221, Uppsala: B: 431 and C: 593, Östergötland: B: 736 and C: 877. *, **, *** refer to significance at 10, 5 and 1 per cent levels respectively.

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