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Experimental evidence from intensified placement efforts among unemployed in Sweden

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Experimental evidence from intensified placement efforts among unemployed in Sweden

Pathric Hägglund

WORKING PAPER 2009:16

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Experimental evidence from intensified placement efforts among unemployed in Sweden[†]

by

Pathric Hägglund[‡]

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Abstract

This paper uses experimental data to study the effects of participation in intensified placement efforts on subsequent job chances and earnings. Five small-scale experiments were performed in four different regions of Sweden in 2004 and the control groups were offered the PES regular services. Due to small samples, many of the impact estimates were imprecise and insignificant. However, the services generally reduced unemployment among the treated. I find significantly enhanced exits to either jobs or other activities (or both) in four of the experiments. Three of the experiments also report positive effects on employment probability and earnings in the years following the programme. Finally, combining job-search assistance and monitoring of job search generated significantly better results than monitoring alone in one of the experiment locations.

Keywords: Active labour market policy evaluation, randomised social experiment, placement efforts

JEL-codes: C93, J64

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1 Introduction

The 21st century has involved a somewhat new role for active placement efforts in Sweden. After the 90s crisis and the drastic increase of people with extensive periods of joblessness, these services have become more targeted towards subgroups of job seekers with a particular troublesome labour market situation, for example youth, immigrants, and long-term unemployed. The activities have also typically been integrated in different “guarantee” programmes, for example the “Youth guarantee” or the Job and Development programme, programmes in which coaching assistance and counseling constitutes the first among several steps attempting to get the unemployed employed.

Earlier research has found active placement efforts to be both effective and inexpensive in comparison to regular labour market programmes.¹ And in contrast to the evaluations of the programmes, the findings are in many cases based on social experiments. In the United States, (Meyer, 1995, Ashenfelter et al., 2005, Klepinger et al., 2002, Black et al., 2003), and in Europe (Delander, 1978, Gorter & Kalb, 1996, Dolton & O’Neill, 1996, Van den Berg & Van der Klaauw, 2006, Graversen & Van Ours, 2008), job-search assistance, counselling and monitoring of job search, either separately or in combinations, are generally found to increase unemployment exits and enhance job chances. Since the positive effects - generally reported for both unemployed in general and for particularly exposed group - recurrently have been found early in the follow-up period, and sometimes even before programme start (see for instance Black et al., Dolton & O’Neill, and Graversen & Van Ours), the services seem to have a motivational effect thus reducing the moral hazard problem embedded in the unemployment insurance system.

This paper reports the results from five experiments in four different regions of Sweden in 2004 where intensified placement activities were tested on exposed groups of unemployed. Participants and non-participants were selected through randomisation and the controls were assigned the PES regular services. I study the programme impact

¹ See for example Martin & Grubb (2001) and Kluve (2006) for overviews.

by comparing mean outcome differences in yearly gross earnings from work, employment status, and number of unemployed weeks, between 2004 and 2006. I also study unemployment duration and use hazard analysis to study the effect of the notification of the services, and the services themselves, separately. Finally I perform cost and benefit analyses to assess whether or not the tested services should be permanently adopted.

The remainder of the paper is structured as follows. Section 2 and 3 describes the experiments and the data used to analyse them. Section 4-9 analyse the effects of the demonstrations, starting in section 4 with the mean differences in earnings, employment status and number of unemployment weeks between 2004 and 2006. Section 5 studies unemployment duration and the outflow from unemployment, section 6 the effect for different subgroups of unemployed, and section 7 performs sensitivity analyses. Section 8 summarise the experiments in terms of cost and benefit, and section 9 looks more closely at the Jämtland results. Section 10, finally, sums up the findings.

2 The experiments

In 2004, the Swedish Labour Market Board (SLMB) funded several pilot schemes with the purpose of testing intensified placement efforts on exposed groups of unemployed in certain regions. The type of services pursued and the targeted subgroups to work with was decided on the region level. Participants and non-participants were selected through randomisation. The demonstrations were carried out alongside the employment offices' regular services which means that non-participants received the same amount of services that they otherwise would have. The demonstrations allowed unemployed both eligible and non-eligible for UI benefits. The experiment analyses, however, only include those who were UI eligible. The main reason is that the risk of sanction is expected to increase the job seekers willingness to participate. This paper reports the results from the experiments in the city of Skellefteå, and the counties of Jämtland, Uppsala, and Östergötland.

2.1 The experimental set-up

The design of the experiments was simple. From a defined target group, job ready job seekers registered at the employment offices were assigned either the demonstration

services or the employment office's regular services. Participation was mandatory and refusal to participate or to obey instructions from the case workers could cause reduction or withdrawal of the UI benefits.² Enrolments of new participants were performed between March and October of 2004 which creates variation in treatment among both treated and controls. Another source of treatment variation is that the services (except in Jämtland) were time-unlimited; the unemployed stayed in the programme until their situation was solved or until the programme ended in December of 2004. If participants left unemployment and then later returned, they were usually readmitted into the services. Finally, since the design of the experiments was determined regionally, the experiments lack a coherent evaluation strategy which makes the results difficult to generalise. On the other hand, more could potentially be learned from five small-scale experiments testing similar services than from one large-scale experiment. Also, with social experiments still being very rare in this field in Sweden, they are still important contributions to the empirical literature.

2.2 The target groups

The services were typically restricted to currently registered as openly unemployed at the employment offices.³ The randomisations thus involved stock based sampling.⁴ With the exception of Jämtland, the demonstrations were targeted towards regional-specific difficult groups. Uppsala, largely characterised by its two universities and 40 000 student, in 2003 held the largest proportion of high (post-upper secondary) educated among the unemployed (approximately 30 %). Accordingly, the demonstration services were targeted towards highly educated in social science, a particular exposed group.

Nationwide the labour market situation in 2003 and 2004 was difficult for youth. With the Swedish lay-off regulation - "last in first out" - youth was a particularly exposed group in lay-off situations. Both the Skellefteå and the Östergötland demon-

² Rejecting a programme referral violates the basic condition for UI compensation and leads to a 25 percent reduction in UI benefits for eight weeks, further refusals first generate a 50 percent cutback for an additional eight weeks and then a full withdrawal of benefits.

³ An exception was made in Uppsala where a small number of part-time unemployed were admitted.

⁴ The randomisation procedure is described in Appendix A.

stration targeted towards unemployed in the age group 18-24. Östergötland, however, almost exclusively focused on long-term unemployed youth entitled to UI benefits. Skellefteå made no such restrictions.

Finally, the Jämtland demonstration was the least targeted one, only conditioning participation on UI entitlement. The motivation for the programme was the notably low search effort among the job seekers. For instance, according to the *Job-seeker survey*, a recurrent survey among unemployed at the employment offices, 31 per cent had not applied for a single job during June of 2003. The services thus involved all currently registered as openly unemployed and eligible for UI benefits.

2.3 The regular services

The control group, or the counterfactual, services consisted of the employment offices' regular services. The controls could thus be offered both matching activities, similar to those offered the treated, and regular labour market programmes, like for instance a training programme. The services differed between the demonstrations both due to the targeted population and the local labour market situation. In Uppsala, the personnel situation was strained and the contact frequency after registering was usually set to 4-6 months (youth excepted). In Jämtland, the follow-ups were set to six months. In the youth experiments in Östergötland and Skellefteå, and also among youth in Uppsala and Jämtland, the regular services involved more frequent contacts and meetings with the case workers. After 100 days, the youth were usually referred to the *Youth Guarantee*, a municipality full-time activation programme including various types of placement efforts, work practice and training schemes for a maximum of 12 months. Also, at the time, there was a large focus on unemployed youth since the government had set a goal to half the number of long-term unemployed youth between 2003 and 2004. This should have further reduced the average treatment dose difference between treated and non-treated youth in the experiments.

Whereas available register data unfortunately lack information on participation in placement and matching activities, participation in regular labour market programmes is continuously reported. It is not obvious that the treated would be less likely to participate in programmes. The more frequent job seeker/case worker contacts could bring forward earlier unknown information about the unemployed thereby increasing the

demand for, for instance, a training programme. *Figure 1 a-d* illustrates the usage of regular programmes by reporting the share of treated and controls currently in a regular programme from the week of notification and the following two years.

Overall, the share of regular programme participants increased during the first three months to either stabilise or decrease thereafter. Note that the shares do not adjust for unemployed leaving unemployment. Only small differences between treated and controls are reported. Also, analysing unemployment-to-programme hazards, all experiments but Uppsala report similar probabilities of being assigned a programme for treated and controls. In Uppsala the hazard is significantly higher in the treatment group. Hence, conditioned on still being unemployed after different durations, the probability of being offered a regular programme is higher among the treated.

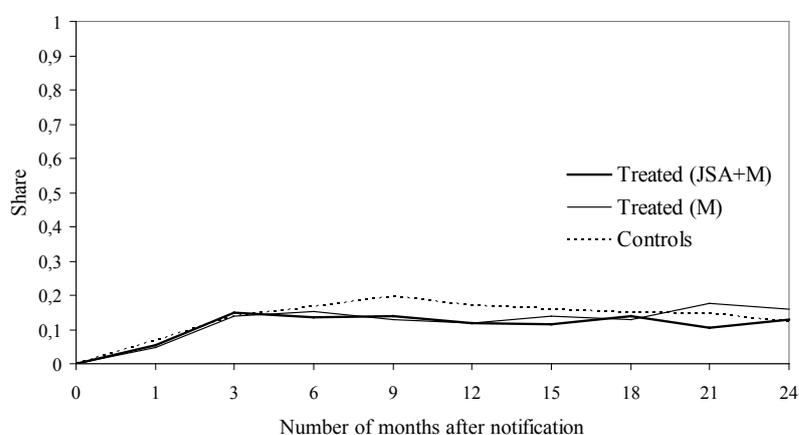


Figure 1 a Share of treated and controls in regular programmes, Jämtland

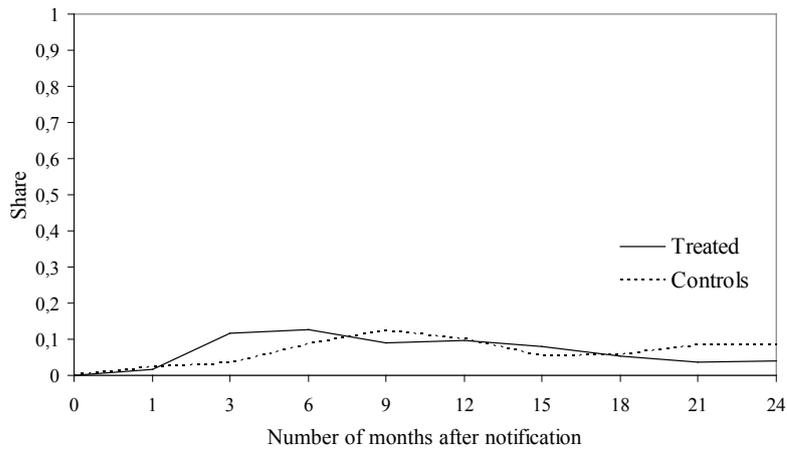


Figure 1 b Share of treated and controls in regular programmes, Uppsala

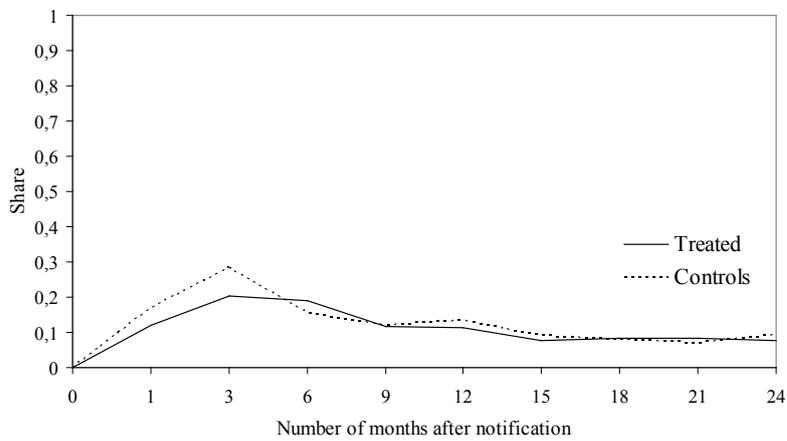


Figure 1 c Share of treated and controls in regular programmes, Östergötland

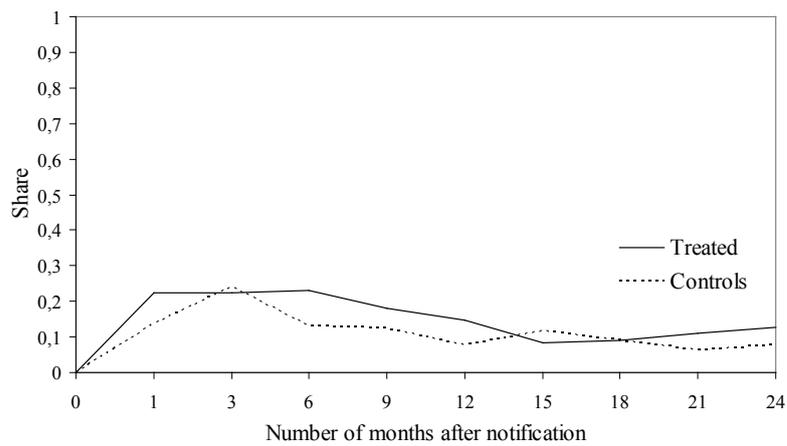


Figure 1 d Share of treated and controls in regular programmes, Skellefteå

2.4 The demonstration services

Although the initial idea was to test and find new alternative modes of placement services, the demonstration activities should rather be described as higher-quality delivery of already existing ones. Compared to the regular activities, the services were intensified and involved more frequent contacts with the employment office case workers. The services were, however, not full-time activities. The activities consisted of four distinguishable activities:

- 1 The first step was usually the same in all demonstrations; individual register information was checked and updated on issues such as recent educational achievements, job experience and occupation searched for. In some cases, the participants received increased surveillance in the data system, which meant that their qualification profiles were continuously matched against all job openings. Positive matches resulted in *job suggestions* or *job referrals*.⁵
- 2 The placement efforts consisted of counselling and of job-search assistance services which typically involved activities like learning about different job-search strategies, self assessment, preparing resumes, completing job applications, and also learning skills in how to best manage the different self-service instrument available at the PES. The activities were usually pursued in weekly or monthly group meetings.
- 3 Besides skill-enhancing activities, monitoring of job search and stricter enforcement of the UI eligibility rules was also emphasised in the demonstrations. These services consisted of both follow-ups on the job-search efforts, performed as integrated parts of the job-search activity meetings, and employer contacts and follow-ups of the job referrals sent out to the unemployed.
- 4 Finally, all demonstrations had access to all regular labour market programmes if that was thought necessary. This possibility was especially pronounced in Uppsala and Östergötland. The idea was however to rely as much as possible on the active placement efforts.

The services in Uppsala consisted of recurrent non-supervised job-search meetings in groups of 8-10 job seekers. The treated were continuously presented with questions of problem-solving character to discuss at the meetings. They also reviewed each others CVs and practiced on job-interview situations. From initially meeting 2-3 hours two times a week, the groups later met only once a week.

Both the Skellefteå and Östergötland demonstration involved traditional job-search assistance activities. Östergötland emphasised skills in managing the self-service Internet applications available at the PES. The activities were arranged as group meetings once a week and the participants were assigned homework between every meeting. In Skellefteå, well documented job-search methods were applied. The participants initially met once a week in groups and then later once a month to follow up the job-search efforts.

The treatment group in Jämtland was randomly divided into two groups. The first group, the *JSA+M group*, received both assisted job search and monitoring of the job-search efforts. The activities were arranged in monthly group meetings. The other group, the *M-group*, was only subject to the monitoring treatment which was arranged as monthly in-person meetings. The design enables the effect from being referred to the combined activities and monitoring alone to be derived separately. The activities in Jämtland were time limited. After three months the participants were referred to the regular activities at the employment office if their situation had not yet been resolved. *Figure 2* and *Table 1* (below) sums up the experiments.

⁵ As opposed to job suggestions, job referrals are legally binding. If the unemployed omits to apply for a job referred to by the case worker, unemployment compensation could be reduced.

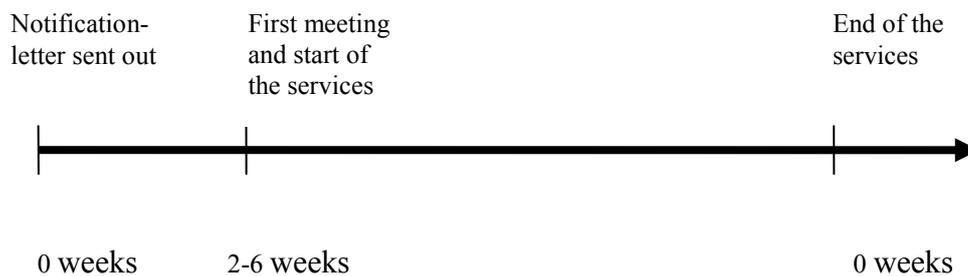


Figure 2 Duration timeline for the demonstration services

2.5 Compliance and pre-programme exits

The ideal experiment keeps a 100 percent difference in treatment between experiment and control group. This way the average treatment effect on the treated (ATET) corresponds to the simple mean difference in outcome between the groups. This ideal situation is however very rare in field experiments and the occurrence of no-shows, drop-outs, and cross-overs often dilute the experiment. The conceptual advance of the social experiment is then reduced. As shown in for example Heckman et al. (1999), the share of participants in the treatment group is often less than 0.7, and sometimes less than 0.5. Also, in some experiments as much as 40 percent of the controls receive substitute services.

All experiments presented in this paper suffer from no-shows, i.e. experiment group members not showing up for the experimental treatment. These no-shows stems from unemployed registered as job ready but in fact awaiting participation in a labour market programme. In these situations, the labour market programme was generally prioritised over the demonstration activities. To reduce the risk of multiple referrals, all demonstrations except Skellefteå agreed upon reviewing all individual acts before randomisation. This way, unemployed with other active measures about to start could be eliminated from the sample. This elimination procedure substantially reduced the number of inactive experiment group members and thus increased the average treatment dose.

Besides no-shows, several experiment group members found other employment in the period between referral to, and start of, the demonstration services. Since the referral itself could alter the job-search behaviour and cause systematic differences in the

experiment and control group composition, we need to keep these non-active treatment group members in the analyses in order to maintain the advantage of the randomisation. The share of pre-programme exits in each experiment to a large extent depends on the length of the pre-programme period.⁶ In Östergötland and Uppsala, using a three and a 4.4-week interval between notification and programme start, 40 and 44 percent respectively left open unemployment before start. In Jämtland, using an average 6.3-week pre-programme period, the share was 57 percent. In Skellefteå where we lack information on the length of the pre-programme period, 12 percent left within a week. On the other hand, between 15 (18) and 25 (34) percent among those who left returned to unemployment within 1 (2) week(s), and the vast majority was reassigned to the demonstration services. A crude estimation would suggest that between 60 and 75 percent of the treatment groups visited the programmes at least once.

In addition, since the controls were offered the regular services, some of them received similar services as the treated. In sum, the difference in the fraction of treated and controls is well below 1. The mean-difference estimator does thus not represent the ATET but rather the “intent-to-treat” impact of intensified services.

⁶ The share also depends on the mobility of the targeted population. Also, in Jämtland, the share was correlated with randomisation outcome (treated/control).

Table 1 The demonstration programmes: an overview

	Jämtland	Uppsala	Östergötland	Skellefteå
Target group	Openly unemployed	Openly unemployed & post secondary educated in social science	Openly long-term unemployed youth	Openly unemployed youth
Type of services	1. Job-search activities & monitoring of job search (JSA+M). 2. Monitoring of job search (M)	Non-supervised job-search workshops	Traditional job-search activities	Traditional job-search activities
Number of observations	1003	518	735	288
<i>Whereof:</i>				
Treated	496	275	357	143
Controls	507	243	378	145

3 Data and empirical strategy

3.1 Data

The unemployment periods were followed in *A-stat*, a register data base containing information on UI payments, benefit type and benefit levels administered by the UI funds. Since *A-stat* lacks information about exit cause, data are combined with the unemployment register data at the time of exit. Since these two registers do not fully match, exits lacking a corresponding exit cause within two weeks are categorised as exits to “cause unknown”. I have access to *A-stat* data until December 31 2006 and unemployment register data until August 2008.

I use the *LISA*-register, administered by Statistics Sweden, to find information about employment status in November each year and also yearly gross earnings from work. *LISA* also contains vast individual information.

Table 2 describes the experiment and control groups. The randomisations were successful in such that the groups are similar in general. In Jämtland, however, the

group receiving only the monitoring services has a significantly less extensive unemployment history than the control group.⁷ The overall extensive periods of unemployment, both in ongoing and previous spells, at least partly reflects the poor labour market in the region. Being the least targeted experiment, Jämtland has representation in all age and educational categories. The Uppsala participants also have experience from extensive periods of unemployment on average. The share of women is also the highest among the experiments. The experimental groups in the youth experiments (Östergötland and Skellefteå) are very similar in most aspects. By definition, both the age average and the share of highly educated are low. However, since Östergötland almost exclusively focused on long-term unemployed, the participants have longer spells of unemployment, both in the current and past periods.

⁷ Note, however, that the experiment and control groups are expected to differ significantly in some aspects (0.05 • the no. of covariates).

Table 2 summary statistics for treated and controls, proportions if not otherwise stated (standard deviations within parentheses)

	Jämtland			Uppsala		Östergötland		Skellefteå	
	Treated (JSA+M)	Treated (M)	Controls	Treated	Controls	Treated	Controls	Treated	Controls
Female	0.32 (0.47)	0.35 (0.48)	0.28 (0.45)	0.53 (0.50)	0.50 (0.50)	0.44 (0.50)	0.38 (0.49)	0.34 (0.47)	0.32 (0.47)
Age									
18-24	0.07 (0.26)	0.08 (0.28)	0.07 (0.25)	0.01 (0.12)	0.01 (0.11)	0.91 (0.29)	0.92 (0.27)	0.92 (0.27)	0.92 (0.28)
25-44	0.57 (0.50)	0.50 (0.50)	0.56 (0.50)	0.73 (0.44)	0.70 (0.46)	0.09 (0.29)	0.08 (0.27)	0.08 (0.27)	0.08 (0.28)
45-	0.35 (0.48)	0.41 (0.49)	0.37 (0.48)	0.25 (0.43)	0.29 (0.46)	-	-	-	-
Educational level									
<=Compuls. school	0.19 (0.39)	0.25 (0.43)	0.21 (0.41)	-	-	0.09 (0.29)	0.13 (0.34)	0.08 (0.28)	0.10 (0.30)
Upper secondary	0.54 (0.50)	0.49 (0.50)	0.54 (0.50)	-	-	0.83 (0.37)	0.78 (0.41)	0.87 (0.33)	0.83 (0.38)
University	0.27 (0.44)	0.26 (0.44)	0.25 (0.43)	1.0 (0.06)	1.0 (0.00)	0.08 (0.26)	0.09 (0.28)	0.04 (0.20)	0.08 (0.27)
Unempl. history*									
Years in ong. spell	0.92 (1,57)	0.88 (1.62)	0.88 (1.54)	0.60 (0.89)	0.66 (0.76)	0.56 (0.66)	0.55 (0.61)	0.13 (0.16)	0.15 (0.24)
Years in all spells	3.51 (4,62)	2.65 (4.10)	3.09 (4.02)	2.16 (2.34)	2.06 (2.09)	1.05 (0.78)	1.07 (0.83)	0.78 (1.22)	0.67 (1.10)
Daily compensation	624 (116)	618 (126)	626 (117)	589 (166)	569 (171)	486 (161)	481 (160)	464 (157)	479 (154)
Number of obs.	246	250	507	275	243	357	378	143	145

Notes: Data are based on information from the week of notification.*Includes periods of open unemployment and programme participation. Bold type indicates deviation from the control group at the significance level 5%

3.2 Empirical strategy

Treated and controls are followed between notification of the demonstration services in 2004 until December 31 2006 in three of dimensions analysed; yearly gross earnings, employment probability, and number of unemployment weeks. Analysing unemployment duration, spells could be followed beyond 2006 had the unemployed entered a regular labour market programme before December 31 2006.⁸

First I use a linear regression model and a logistic model to adjust for random differences in observables comparing the outcomes in yearly gross earnings from work, employment probability and number of unemployment weeks. Analysing earnings and number of unemployment weeks, I use the following model:

$$Y_i = \mathbf{X}_i' \boldsymbol{\gamma} + T_i \lambda + \mathbf{e}_i, \quad (1)$$

where Y_i is gross earnings or number of unemployment weeks each year between 2004 and 2006. \mathbf{X} is a covariate vector with individual information collected from the available registers, and $\boldsymbol{\gamma}$ it's corresponding parameter vector.⁹ T_i is a dummy variable taking the value one if the person is treated and zero otherwise, and λ is the coefficient.

Analysing the binary outcome of employment status, Y_i , in November each year I use the logit specification:

$$Y_i^* = \mathbf{X}_i' \boldsymbol{\gamma} + T_i \lambda + \mathbf{e}_i, \quad (2)$$

where Y_i for the i th individual is 1 if $Y_i^* > 0$, and 0 otherwise.

Second, to discriminate between exits during the pre-programme and programme period I use a Cox proportional-hazard model with time-dependent covariates. Analysing the effect from being assigned the services and the services themselves separately, I use the following model:

⁸ Did the unemployed enter another period of open unemployment after participation in a regular labour market programme, the unemployed was followed in the unemployment register and not in the unemployment payment register (*A-stat*).

⁹ Analysing each experiment, I control for gender, age, educational level, citizenship, working disability, measures of unemployment history, occupation searched for, education and experience in the occupation searched for, UI benefit type, UI benefit level, local labour market office, and demonstration start date.

$$\log \theta_i(t) = \alpha(t) + \mathbf{X}_i' \boldsymbol{\gamma} + \mathbf{Z}_i'(t) \boldsymbol{\lambda} \quad (3)$$

where $\theta(t)$ is the off-unemployment hazard at t . $\alpha(t)$ is the log baseline hazard function and $\boldsymbol{\gamma}$ is a coefficient vector corresponding to the variable vector \mathbf{x} , that includes personal characteristics. Investigating time-specific effects, the vector $\mathbf{Z}(t)$ includes a dummy separating the pre-programme and programme period, and interaction terms capturing the impact of being a treatment group member in each of these periods respectively. $\boldsymbol{\lambda}$ is the corresponding parameter vector.

Analysing the impact of the services on job and “other” exits separately, I use the same specification as in equation 3 but without $\mathbf{Z}(t)$. Also, $\theta_i(t)$ is replaced by $\mathbf{q}_{ij}(t)$, which is the conditional density of leaving unemployment to destination j at time t , given that unemployment was still in progress at $t-1$. Conditioned on the specified destinations (jobs and other activities) being mutually exclusive and jointly exhausting of all possible destinations, the marginal hazard function is the sum of all the state-specific hazard functions:

$$\mathbf{q}(t) = \sum_{j=1}^n \mathbf{q}_j(t). \quad (4)$$

The estimation of each state-specific hazard rate is performed separately by right censoring all other exits.

4 Mean differences

Table 3 reports the effect from the demonstration services on subsequent yearly gross earnings from work and job status at November 1, and also the number of unemployment weeks, between 2004 and 2006.¹⁰

The effects in Jämtland are somewhat mixed in the sense that the results of the *JSA+M* and the *M*-services differ. With one exception, members of both experiment

¹⁰ A few observations are lost in each experiment analysing gross earnings and job chances. This is due to people either having deceased or moved abroad. We assume that either of the occurrences is uncorrelated with being a

groups are unemployed 0.6-2 weeks less on average each year. However, while the *JSA+M* activities throughout report positive effects on both job probability and gross earnings, the effects of the *M*-services are often close to zero and sometimes negative. Comparing the outcome of the two treatment groups, the difference in earnings in 2006, and in job chances in 2005 and 2006, is significant positive in favour of the combined services.¹¹

The results of the Uppsala demonstration are overall positive and in 2005, all outcome measures are significantly positive. The probability of being employed was 53 per cent higher and yearly earnings on average SEK 19,200 (approximately €1,920) higher. In 2006, the earnings difference is somewhat higher while the job-status difference has dropped to 18 percent.

In Östergötland, the outcome differences are throughout small and insignificant. All impact estimators are more positive in 2006 than in 2004. Finally, in the other youth experiment in Skellefteå, the results are quite positive with large, positive and significant effects on either job chances or earnings (or both) in all of the years. The very positive effects are somewhat surprising considering the overall large focus on youth during this period.

In conclusion, the services generally seem to have reduced unemployment among the treated although the effects in most cases are insignificant. Despite imprecise impact estimates, I also find significantly positive effects on either job chances or yearly gross earnings (or both), in three of the experiments. Finally, Jämtland reports better job chances and higher earnings from combining *JSA* activities and increased monitoring than from increased monitoring alone.

treatment or a control group member. The follow-up period in 2004 (1 November) was on average 128 days (Skellefteå), 147 (Östergötland), 180 (Jämtland), and 182 (Uppsala).

¹¹ Using log income does not change any of the conclusions in this section.

Table 3 Adjusted impact estimates on yearly gross earnings from work (SEK 1000), employment status (odds ratios), and number of unemployment weeks 2004-2006

Jämtland						
	<i>JSA+M</i>			<i>M</i>		
	Gross earnings	Job status	# of u.e weeks	Gross earnings	Job status	# of u.e weeks
2004	1.5 [-11.0-14.0]	1.34* [0.96-1.89]	-0.6 [-2.3-1.0]	-3.1 [-15.3-9.0]	1.21 [-0.86-1.70]	-1.1 [-2.7-0.6]
2005	7.2 [-7.5-21.9]	1.38* [0.97-1.98]	-1.7 [-4.3-0.9]	4.7 [-9.5-18.9]	1.08 [-0.76-1.53]	-2.0 [-4.6-0.6]
2006	11.9 [-4.2-28.0]	1.17 [0.79-1.73]	-0.9 [-3.5-1.6]	-0.1 [-16.1-15.8]	0.95 [0.65-1.38]	1.7 [-0.9-4.3]
Uppsala			Östergötland			
	Gross earnings	Job status	# of u.e weeks	Gross earnings	Job status	# of u.e weeks
2004	2.0 [-12.7-16.7]	1.25 [0.84-1.85]	-1.6 [-3.8-0.7]	-2.4 [-9.6-4.8]	1.02 [0.72-1.43]	-0.7 [-2.1-0.7]
2005	19.2* [-2.6-41.0]	1.53** [1.02-2.29]	-2.7* [-5.7-0.3]	-1.6 [-13.1-9.9]	0.91 [0.67-1.25]	0.1 [-2.4-2.6]
2006	20.0 [-4.7-44.7]	1.18 [0.74-1.88]	-2.2* [-4.8-0.3]	4.6 [-8.9-18.1]	1.11 [-0.79-1.55]	0.5 [-1.8-2.9]
Skellefteå						
	Gross earnings	Job status	# of u.e weeks			
2004	18.4*** [5.6-31.1]	1.44 [0.85-2.45]	-2.2* [-4.5-0.1]			
2005	28.7*** [11.1-46.4]	1.93** [1.09-3.41]	-2.8 [-6.2-0.7]			
2006	23.1** [2.8-43.4]	1.64* [0.91-2.95]	-0.5 [-3.8-2.8]			

Notes: No. of obs. analysing gross earnings and job status, Jämtland (JSA+M), 2004: 753, 2005: 749, 2006: 745. Jämtland (M), 2004: 753, 2005: 753, 2006: 750. Uppsala, 2004: 511, 2005: 504, 2006: 500. Östergötland, 2004: 735, 2005: 732, 2006: 729. Skellefteå, 2004: 288, 2005: 288, 2006: 286. No. of obs. analysing # of unemployment weeks, see *Table 1*. I control for gender, age, educational level, citizenship, working disability, measures of unemployment history, occupation searched for, education and experience in the occupation searched for, UI benefit type, UI benefit level, local labour market office, and start date. 95%-confidence intervals are within square brackets. *, **, *** refer to significance at 10, 5, and 1 percent levels respectively.

5 Unemployment duration analysis

The reduced risk of unemployment found in the previous section could either be due to the services speeding up the exits, or due to a reduced risk of recurrent unemployment. This section presents programme impacts both as mean differences in unemployment duration, and as differences in hazard rates. Unemployment includes both periods of open unemployment and participation in regular labour market programmes.¹² I follow the unemployed between the week of notification until either interruption of the UI (or the regular labour market programme) spell or until December 31 2006.¹³ A 0.5-day interruption of the UI spell and a 1-day interruption of the programme spell is enough to end the unemployment spell. In section 5.4, I also report the effect from using a one-week interruption rule instead. In the hazard analyses, transitions are split between exits before (pre-programme) and after (participation) entry into the programme (5.2). Also, exits to different states are analysed separately (5.3).

5.1 Mean difference in unemployment duration

Table 4 reports programme effects as adjusted mean-differences in unemployment duration. Note that since all spells but two (!) had ended before December 31 1996, almost no observations are censored.¹⁴

Participation in the demonstrations usually speeds up the exits from unemployment. All experiments report shorter unemployment spells among the treated. However, only the results from Jämtland are statistically significant. Both services shortened unemployment duration by almost seven weeks, or 32 percent. Considerably smaller but still positive effects were also found in Uppsala (-0.4 weeks), Östergötland (-1.1 weeks), and Skellefteå (-2.2 weeks).¹⁵

¹² As openly unemployed the job seekers receive UI benefits, as regular labour market participants the unemployed instead receive activity support. The compensation level is the same in both states.

¹³ If the unemployed were currently in a regular labour market programme at December 31 2006, I could follow the unemployment spell until August 2008.

¹⁴ One observation each in Uppsala and Skellefteå was still in progress at December 31 2006. They are included analysing both the impact on mean differences and the hazard rates. They are censored in the hazard analyses.

¹⁵ The average unemployment spells among the controls were 22.0 weeks (Jämtland), 22.4 weeks (Uppsala), 20.4 weeks (Östergötland), and 12.0 weeks (Skellefteå).

Table 4 Adjusted impact estimates on unemployment durations (weeks)

Demonstration	Impact estimate
Jämtland	
<i>JSA+M</i>	-6.9 [-11.7- -2.1]***
<i>M</i>	-6.8 [-11.5- -2.2]***
Uppsala	-0.4 [-5.6-4.8]
Östergötland	-1.1 [-5.6-3.3]
Skellefteå	-2.2 [-5.9-1.5]

Notes: Adjustments are made in a linear regression model including the covariates specified in *Table 3*. 95%-confidence intervals are within square brackets. *, **, *** refer to significance at 10, 5, and 1 percent levels respectively.

5.2 Time-varying effects

In *Table 5*, the dynamics of the treatment effects are studied separating the effect from receiving notice of the services, and the services themselves. The first row reports the average effect over the follow-up period.

In Jämtland, both service packages expectedly report positive and significant effects. The *JSA+M* services increased the hazard by 37 percent and the monitoring services by 30 percent. However, the dynamics of the effects differ somewhat. Whereas the *JSA+M* services generate large off-unemployment exits before start, and smaller but still positive effects during the programme, the impact from the monitoring services is the opposite. Remember that the *JSA+M* services were performed in groups, whereas the monitoring services were performed in person. The group meetings may have appeared stigmatizing for some unemployed, which could have generated the large pre-programme exits. Also, with a relatively worse group of unemployed remaining at programme start, the programme effect could be downward biased due to compositional effects not captured by the model. However, the estimated average impacts do not significantly differ between the two treatment packages.

Both the Uppsala and Östergötland services report positive but insignificant effects on the average hazard ratio. Both demonstrations also show larger programme than pre-programme impacts. In Skellefteå, where no separation between pre-programme and programme effect is possible, the effect on the average hazard ratio is large (27 %), positive and significant.

An interesting fact is that the demonstrations (Jämtland) generating the most positive programme effects also generated the most positive pre-programme effects. This could be interpreted as the programmes having a deterrent impact on some unemployed and a job-seeking skill-enhancing impact on others. This scenario would be expected to be especially common in situations where the assigned programme was both ambitious and time-consuming. The Jämtland demonstrations, however, were the least intense programmes of the five tested with meetings only every 3-4 week instead of every one week or 1-2 weeks. With the motivation for the Jämtland demonstration being the low job-search effort level among the job seekers, a possible explanation for the results is that the services interfered with other, perhaps income bringing, activities. The services had then a deterrent impact both during the pre-programme and the programme period.

Table 5 Adjusted programme, pre-programme, and participation impacts on the hazard ratio

	Jämtland	
	<i>JSA+M</i>	<i>M</i>
Average programme effect	1.372 (0.084)***	1.299 (0.084)***
Pre-programme effect	1.515 (0.115)***	1.203 (0.121)
Participation effect	1.238 (0.118)*	1.386 (0.111)***
	Uppsala	Östergötland
Average programme effect	1.056 (0.093)	1.074 (0.078)
Pre-programme effect	1.004 (0.191)	1.034 (0.145)
Participation effect	1.072 (0.105)	1.090 (0.091)
	Skellefteå	
Average programme effect	1.271 (0.123)**	
Pre-programme effect	-	
Participation effect	-	

Notes: Estimations include the covariates specified in *Table 3*. Standard errors are within parentheses. *, **, *** refer to significance at 10, 5, and 1 percent levels respectively.

5.3 Exit to different states

Higher exits from unemployment are not equivalent to societal benefits. Making unemployment more inconvenient could deter some job seekers to other related transfer systems and thus postpone employment transitions.

Table 6 reports the impact on the hazard ratio for transitions to jobs and “other” activities separately. The small sample sizes make it necessary to jointly examine the exits to cause unknown, sickness absence, early retirement and regular training. Studying state-specific exits, the analyses are sensitive to the quality of the local register routines. Worse register routines generate higher shares of exits to cause unknown.

In Jämtland, both treatment packages generated significantly higher exits both to jobs and “other” activities. The exits rose between 30 and 44 percent. Among the “other” exits, transitions to the sickness insurance system due to sickness absence were particularly pronounced. Behind the small positive effect on the average hazard ratio reported in Table 5, the Uppsala services significantly enhanced the job exit rate while the “other” exits were significantly reduced. This result corresponds with the positive effects found on both employment status and yearly earnings in the years following the demonstration (Table 3). Finally, in the youth experiments, Östergötland reports positive but insignificant hazard ratio estimates both to jobs and other exits, while the Skellefteå experiment reports a large, positive and significant effect on the job exit rate. Both results are in agreement with the above findings analysing job status and earnings.

Table 6 Adjusted impact estimates on the hazard ratio to jobs and other activities

	Job	Other
Jämtland		
<i>JSA+M</i>	1.322 (0.121)**	1.440 (0.117)**
<i>M</i>	1.300 (0.122)**	1.306 (0.116)**
Uppsala	1.925 (0.153)***	0.724 (0.121)***
Östergötland	1.101 (0.112)	1.037 (0.111)
Skellefteå	1.848 (0.197)***	1.005 (0.165)

Notes: Estimations control for the covariates specified in Table 3. “Other” activities refer to exits to cause unknown, sickness absence, early retirement and regular training. Standard errors are within parentheses. *, **, *** refer to significance at 10, 5, and 1 percent levels respectively.

5.4 Using a one-week interruption rule

Alternating the required length of the unemployment interruption spell could shed further light as to the nature of the unemployment exits. A consequence of instead using a one-week interruption rule is that the share of unknown exits is significantly reduced.

This since many of the very temporary interruptions is not registered in the unemployment register.

In Appendix B (*Table B 1-Table B 3*), the results from the duration analyses in *Table 4-Table 6* are presented using a one-week interruption rule instead. In *Table B 1*, analysing adjusted mean differences in unemployment duration, the Uppsala and the youth experiments perform similarly as in the main analysis where more temporary interruptions were allowed to end the UI spells. However, the very significant reductions of the UI spells in Jämtland are reduced from almost seven to 4.2 and 4.6 weeks. These are still substantial impacts but no longer significant.

Continuing with the hazard analyses, Uppsala and the youth experiments report almost exactly the same average hazard ratio impact estimates as in the main analysis (*Table B 2*). However, both Östergötland and Uppsala report substantially larger pre-programme estimates and somewhat smaller programme effects. Also, both Uppsala and Skellefteå present smaller, although still significant, impact estimates on the job transitions (*Table B 3*).

Not allowing within-week interruptions alters the hazards in Jämtland quite a bit. Similar to the mean-difference outcomes the average hazard ratios are negatively affected. In fact, only the combined-service impact estimate (reduced from 37 to 22 percent) is still significant. While the *JSA+M* services report considerably lower pre-programme effects (no longer significant), and a similar participation effect, the monitoring services now have a negative impact during the pre-programme period and a somewhat smaller (19 %) but still significant participation effect. Interesting as well is that while the large, positive and significant positive effect on job exits still remains, the transitions to other destinations, and particularly to sickness absence, has now disappeared. One can thus conclude that the significantly enhanced exit rates before programme start found in the main analyses were due to unemployed reporting temporary sick.

6 Group-specific impacts

Average effects are likely to conceal heterogeneity between individuals and groups of unemployed. This section analyses differences in pay-off between subgroups of unemployed by pooling data from all experiments and all years. I adjust for any compositional differences in background factors and also include dummies for each demonstration.

Summing the results from the experiments and over the entire follow-up period, three out of four outcome measures report positive and significant impact estimates. On average, the demonstration services increased gross earnings with SEK 15 500 (approximately € 155), reduced unemployment duration with three weeks and the total number of unemployment weeks with 2.3 weeks, during the 2-3 year long follow-up period.

Men significantly enhance their exit rate from unemployment, whereas women increase their gross earnings more than women. Interesting to note is that youth do not seem to gain from the services despite the very positive effects from Skellefteå. The oldest age category, 45-, report the most positive effects with the highest increase in gross earnings and with large and significant reductions in both unemployment duration (-7.4 weeks), and in total unemployment (-7.2 weeks). Analysing the effects on educational level, the results are somewhat mixed with significantly positive effects among both the lowest, -8.0 weeks in unemployment duration, and the highest, SEK 36 000 in gross earnings and -5.2 weeks in total unemployment, educated. Finally, the services seem to be equally favourable for both short-term and long-term unemployed. If anything, long-term unemployed seem to benefit more, at least in terms of reducing the risk of current and future unemployment.

Table 7 Adjusted group-specific impacts on gross earnings (SEK 1000) and number of unemployment weeks (both aggregated 2004–2006), employment probability 2006 (odds ratio) and unemployment duration (weeks). Pooled data.

	Gross earnings	Employment status, 2006	# of u.e weeks	Unemployment duration (weeks)
Average effect	15.5* (9.1)	1.10 (0.09)	-2.3* (1.3)	-3.0*** (1.1)
Gender:				
Men	14.3 (12.3)	1.06 (0.12)	-2.5 (1.7)	-3.5** (1.5)
Women	20.3 (12.8)	1.15 (0.15)	-1.3 (2.0)	-1.6 (1.8)
Age:				
18-24	5.1 (11.4)	1.05 (0.14)	0.8 (2.1)	0.2 (1.7)
25-44	20.7 (15.7)	1.21 (0.17)	-3.3* (2.0)	-3.7** (1.5)
45-64	34.8 (24.3)	0.94 (0.20)	-7.2** (3.4)	-7.4** (3.5)
Education:				
Compulsory	9.8 (23.2)	1.18 (0.27)	-4.2 (4.3)	-8.0** (3.5)
Upper secondary	1.2 (10.7)	1.03 (0.12)	0.5 (1.8)	-2.4 (1.5)
University	36.7** (19.1)	1.25 (0.18)	-5.2** (2.3)	-1.6 (2.0)
Previous u.e (total).				
<=3 months	24.5* (14.9)	1.11 (0.17)	-1.2 (2.1)	-1.4 (1.7)
>3<=12 months	13.3 (14.0)	1.05 (0.14)	-1.9 (2.0)	-2.4 (1.7)
>12 months	8.9 (20.3)	1.23 (0.22)	-5.2 (3.4)	-6.5** (3.2)

Notes: No. of obs, gross earnings: 2,504, employment status: 2,507, # of unemployment weeks and unemployment duration: 2,544. Estimations control for the covariates specified in *Table 3*. Standard errors are within parentheses. *, **, *** refer to significance at 10, 5, and 1 percent levels respectively.

7 Sensitivity analysis

With the treated and controls being treated side by side, there are several ways in which their respective performance could have affected each other. For example, the treated could have enhanced their job chances at the expense of reduced job chances among the controls. Also, the employment offices could have recruited their most experienced administrators for the demonstration services and replaced them with less experienced personnel to perform the regular services. Under both these circumstances, the benefits of the demonstration services would be exaggerated. On the other hand, if the demonstration activities to some extent were copied by the administrators of the control groups, or inspired them to compete with the treatment groups, this would underestimate the true impact estimates.

A common way of testing whether or not the treatment effects are biased is to use non-experimental methods to compare the performance of the control group to some relevant comparison. Since identifying the experimental groups, with the exception of Skellefteå, involved a thorough review of the individual acts – and a similar procedure is not possible here – relevant comparison groups cannot be attained using available register data. Therefore, I use data to make excess selections of control group members who, in the absence of the individual-act review, would also have been included in the experiments. By doing this, selection biases due to comparing non-comparables can be avoided.¹⁶

Table 7 reports the difference-in-difference estimates comparing unemployment duration in 2003 and 2004 between the constructed control groups and, i) unemployed in the region (county) closest comparing durations in 2003, and ii) among unemployed in all regions.¹⁷ None of the impact estimates is significant I thus find no evidence of the treatment effects being either upward or downward biased. The point estimates are more precise and closer to zero making comparisons with the outcome of all regions, compared to the closest region. Also, whereas the estimates of the non-youth experi-

¹⁶ The share of “true controls”, i.e. unemployed part of the actual control group, was 26.1 percent in Uppsala, 13.9 percent in Östergötland, and 10.6 percent in Jämtland. In Skellefteå, no excess population was necessary.

¹⁷ The region performing most similar to Jämtland in 2003 was the county of Västerbotten. Stockholm, Västmanland, and Norrbotten constitute counterfactuals to Uppsala, Östergötland and Skellefteå respectively.

ments indicates negative spill-over effects and thus overestimated treatment impacts, the estimates of the youth experiments signals the opposite.

Table 8 difference-in-difference regression estimates comparing unemployment duration between "controls" and their counterparts in the region performing closest in 2003, and all regions, weeks

	Region closest in 2003 ^a	All regions ^b
Jämtland	-0.86 (0.62)	-0.26 (0.57)
Uppsala	-0.32 (1.51)	-0.15 (1.32)
Östergötland	1.30 (1.06)	0.37 (0.58)
Skellefteå	2.15 (2.15)	0.57 (1.94)

Notes: No. of observations, Jämtland: ^a: 24,737, ^b: 507,033. Uppsala: ^a: 8,480, ^b: 39,521. Östergötland: ^a: 7,791, ^b: 88,467. Skellefteå: ^a: 3,385, ^b: 58,112. Standard errors are within parentheses. *, **, *** refer to significance at 10, 5, and 1 percent levels respectively.

8 Cost-and-benefit analysis

This section analyses the cost and benefits of the programmes on different levels of society to assess whether or not the tested services should be permanently adopted. Following Meyer (1995), I study the costs and benefits using the perspectives, i) the UI system, ii) government as a whole, and iii) society. Analysing the impact on the UI system, the administrative costs of performing the demonstrations are contrasted against the benefits adding the changes in UI payments over the three year follow-up period.¹⁸ Adding the tax revenues of any earnings increases, I get the perspective of the government.¹⁹ The societal perspective compares the earnings changes with the administrative costs of the experiments. Obviously, with the experiments being small-scale, I say nothing about the general equilibrium effects the services potentially would produce fully implemented.

Although the administrative costs were low (SEK 2-3,000/person on average), the Östergötland demonstration generated small but negative savings in the UI system. Östergötland also generated negative effects on the society level where the effect on earnings also are taken into account. Interesting to note is the Jämtland (M) results

¹⁸ The outcome differences have been adjusted for in regression models including observables. Comparing earnings, a few observations are lost due to missing observations in the LISA register.

where the positive change in the UI benefit system is greater than the change in earnings producing a larger surplus in the UI system than for society. The results correspond with the mixed effects found on the unemployment exit rate (positive) and subsequent earnings (negative).

Finally, Jämtland (JSA+M), Uppsala, and Skellefteå all report large benefits on the UI and on the society level. However, only the effects of the Uppsala (UI benefits) and Skellefteå (earnings) services are significant adding all years.

¹⁹ The tax revenues correspond to tax on earnings (30%), and employer taxes (30%).

Table 9 Analyses of cost and benefits of the demonstrations (SEK 1000, approximately € 100)

	Benefits minus costs					
	Change in	Administrative	Change in	UI system	All government*	Society
	benefits	costs	earnings	(1)+(2)	(4)+.6*(3)	(3)-(2)
	(1)	(2)	(3)	(4)	(5)	(6)
Jämtland						
<i>JSA+M</i>	10.0	-2.0	22.5	8.1	22.1	20.5
<i>M</i>	4.3	-2.0	2.1	2.3	3.6	0.1
Uppsala	18.6	-2.2	40.2	16.4	41.4	38.0
Östergötland	-0.0	-1.9	-0.0	-2.0	-2.0	-2.0
Skellefteå	12.9	-2.6	52.4	10.4	53.0	65.8

Note: *: 0.6 refers to adding earnings taxes (30 %) and employer fee (30 %).

9 JSA and monitoring versus monitoring alone

The results from Jämtland are particularly interesting since they are the only ones generated from one experiment testing two different service packages in one macro-economic environment. So far we have found that both combining JSA and monitoring services, and monitoring alone, increase off-unemployment exits both to jobs and other activities. More interestingly, however, is that while the combined services generate positive effects on employment probability and earnings in the years following the programme, increased monitoring alone does not.

A more detailed analysis shows that 87 (88) percent of those reportedly working in the *JSA+M* group in 2004 were also employed in 2005 (2006). The corresponding shares in the *M*-group and the control group were 83 (84) and 82 (84) respectively. Focusing on those *not* employed in 2004, 47 (62) percent in *JSA+M* group was employed in 2005 (2006) and 46 (64) percent in the control group. In the *M*-group the share was only 41 (55) percent. Both shorter employment spells among those employed in 2004, and lower job transitions among those not employed in 2004, thus seem to explain the worse results for the *M*-group. The results also provide some indications of the *M*-group performing worse than the control group where the unemployed received the regular services. The absence of active measures, besides increased surveillance, thus seems to have delayed unemployment exit for some unemployed.

In the spring of 2005, surveys were performed in all of the demonstration but Uppsala. Experiment and control group members were asked questions about their job-search behaviour (if unemployed) and employments (if employed). *Table 10* reports some of the responses from the group reportedly working in Jämtland. Once again, the samples are small and the results should be carefully interpreted.

I find that approximately 50 percent of the employed returned to a previous employer, a majority (60-70 %) got a job in the same profession as before, and approximately 70 percent got a temporary or hourly employment.

The results indicate that the higher job exits in the treatment groups were the result of the participants more often returning to a previous employer and to their previous

profession. A difference between the treatment groups is that a larger share (68 % compared to 48 %) got a temporary employment in the *M* group. This corresponds to the lower share of employed in 2005 among those employed in 2004. Increased exits to temporary employments are positive if they increase the chances of receiving a permanent job. They could, however, also postpone the transitions to permanent employment.

Table 10 Survey answers for unemployed leaving unemployment for jobs, percent

	<i>JSA+M</i>	<i>M</i>	<i>Control</i>
Employer			
Same as before ^a	48	52	41
Different/no answer	52	48	59
Profession (compared to before)			
Same/similar	70	66	62
Different	29	32	35
Other	1	2	3
Type of employment			
Permanent	36	27	33
Temporary	48	68	57
Hourly	16	4	9

Note: ^a: Also includes earlier subsidised employment with the employer. No. of obs, pre-programme, JSA+M: 94, M: 95, control: 181.

10 Conclusions

This paper reports the results from five pilot schemes performing intensified placement efforts on exposed groups of unemployed. The results are generally encouraging confirming the results from earlier research reporting positive effects from active placement efforts. The small samples however create imprecise and often insignificant impact estimates.

All services reduced unemployment duration. Separating exits, I find significantly increased job exits in four of the experiments. In two of the experiments, both performed in Jämtland, both job-search assistance and monitoring combined, and monitoring alone, shortened the unemployment spells with almost seven weeks. The

effects on employment probabilities and gross earnings in the years following the programme are also positive in general with significant impacts in three of the experiments.

Pooling data from all experiments and from the entire follow-up period (2-3 years), the services seem promising for different subgroups of unemployed except for youth. Interesting to note is that while the positive effects for typically exposed group (low educated and long-term unemployed) were found in terms of reduced unemployment duration, the positive impact for high educated and short-term unemployed was found in higher earnings. This suggests that the exposed groups to a higher extent transitioned to other activities than jobs.

Interesting findings were reported comparing the effect of combining job-search assistance and monitoring, with monitoring alone, in Jämtland. Although both service packages significantly increased off-unemployment exits, only the combined services generated positive effects on employment probability and gross earnings in subsequent years. This implies that monitoring alone is not sufficient to produce positive effects in the long run. Survey data indicate that the increased job exits in the increased monitoring group were the result of more temporary employments. Also, both treatment packages increased short-term sickness absence.

These results highlight the potential risk of active measures. By increasing the cost of remaining unemployed, the temporary exits from unemployment may increase. As a consequence, transitions to permanent employment may be postponed. The found increase in exits to the sickness insurance system also put focus on the interdependence between public social insurance systems and the importance of them jointly creating incentives for the unemployed to work.

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Appendix A

Randomisation procedure

As currently employed at the Swedish Labour Market Board (SLMB), the author of this paper was the responsible evaluator for the presented experiments. The randomisation process involved the following steps:

- 1 The evaluator at the SLMB identifies and marks the specified target group in the public employment services (PES) internal information system.
- 2 The project team workers in all experiments but Skellefteå review the personal acts of all marked candidates and eliminate those likely to have exited from unemployment in the near past, and those with an active labour market programme about to start. This procedure could take a few days.
- 3 The evaluator confirms current status in the unemployment register and performs randomisation using the “ranuni” function in SAS. Those randomised into the treatment group are marked in the PES internal information system. The initial marking is eliminated.
- 4 Treatment group members are sent a letter or an e-mail saying that he/she is selected to participate in a mandatory activation programme at the PES. They are also reminded of the UI requirements. The control group members are not informed of the demonstration services. Neither of the groups is informed of the services being evaluated.

Table A1 a Yearly gross earnings from work (SEK 1000), 2004–2006

	Jämtland			Uppsala	
	<i>JSA+M</i>	<i>M</i>	<i>Control</i>	<i>Treated</i>	<i>Control</i>
2004	81.3	72.8	79.6	94.3	89.0
2005	119.9	109.2	113.0	141.9	122.5
2006	150.9	130.8	140.9	189.2	168.0
	Östergötland		Skellefteå		
	<i>Treated</i>	<i>Control</i>	<i>Treated</i>	<i>Control</i>	
2004	50.8	52.9	79.8	64.4	
2005	77.8	80.4	116.0	96.4	
2006	117.0	114.2	143.0	131.5	

Notes: No. of obs., Jämtland 2004, JSA+M: 246, M: 250, controls: 507. 2005, JSA+M: 246, M: 250, controls: 503. 2006, JSA+M: 242, M: 247, controls: 503. Uppsala 2004, treated: 272, control: 239. 2005, treated: 267, controls: 237. 2006, treated: 264, controls: 236. Östergötland 2004, treated: 357, controls: 378. 2005, treated: 355, controls: 377. 2006, treated: 353, controls: 376. Skellefteå 2004 and 2005, treated: 143, controls: 145. 2006, treated: 143, controls: 143.

Table A1 b Employed (share), 2004–2006

	Jämtland			Uppsala	
	<i>JSA+M</i>	<i>M</i>	<i>Control</i>	<i>Treated</i>	<i>Control</i>
2004	0.52	0.46	0.44	0.57	0.52
2005	0.67	0.60	0.62	0.71	0.62
2006	0.76	0.68	0.73	0.80	0.76
	Östergötland		Skellefteå		
	<i>Treated</i>	<i>Control</i>	<i>Treated</i>	<i>Control</i>	
2004	0.35	0.37	0.58	0.54	
2005	0.50	0.52	0.65	0.58	
2006	0.68	0.65	0.71	0.66	

Notes: No. of obs., Jämtland 2004, JSA+M: 246, M: 250, controls: 507. 2005, JSA+M: 246, M: 250, controls: 503. 2006, JSA+M: 242, M: 247, controls: 503. Uppsala 2004, treated: 272, control: 239. 2005, treated: 267, controls: 237. 2006, treated: 264, controls: 236. Östergötland 2004, treated: 357, controls: 378. 2005, treated: 355, controls: 377. 2006, treated: 353, controls: 376. Skellefteå 2004 and 2005, treated: 143, controls: 145. 2006, treated: 143, controls: 143.

Table A1 c Number of unemployment weeks, 2004–2006

	Jämtland			Uppsala	
	<i>JSA+M</i>	<i>M</i>	<i>Control</i>	<i>Treated</i>	<i>Control</i>
2004	20.3	20.8	20.9	19.9	22.0
2005	19.5	19.4	21.4	16.5	19.0
2006	14.5	16.8	15.2	8.5	10.8

	Östergötland		Skellefteå	
	<i>Treated</i>	<i>Control</i>	<i>Treated</i>	<i>Control</i>
2004	18.6	19.5	17.6	18.5
2005	22.2	21.8	16.3	18.2
2006	14.5	13.8	11.8	11.0

Notes: No. of obs., see *Table 1*.

Appendix B

Table B 1 Adjusted impact estimates on unemployment duration (weeks), using a one-week interruption rule

Demonstration	Impact estimate
Jämtland	
<i>JSA+M</i>	-4.2 [-9.9 - 1.5]
<i>M</i>	-4.6 [-10.3 - 1.1]
Uppsala	-0.7 [-6.6 - 5.2]
Östergötland	-0.9 [-5.8 - 4.0]
Skellefteå	-2.8 [-7.1 - 1.5]

Notes: Adjustments are made in a linear regression model including the covariates specified in *Table 3*. 95%-confidence intervals are within square brackets. *, **, *** refer to significance at 10, 5, and 1 percent levels respectively.

Table B 2 Adjusted programme, pre-programme, and participation impacts on the hazard ratio, using a one-week interruption rule

	Jämtland	
	<i>JSA+M</i>	<i>M</i>
Average programme effect	1.217 (0.083)**	1.133 (0.083)
Pre-programme effect	1.190 (0.141)	0.881 (0.153)
Participation effect	1.227 (0.101)**	1.257 (0.098)**
	Uppsala	Östergötland
Average programme effect	1.064 (0.093)	1.079 (0.078)
Pre-programme effect	1.196 (0.227)	1.140 (0.189)
Participation effect	1.041 (0.101)	1.068 (0.085)
	Skellefteå	
Average programme effect	1.241 (0.123)*	
Pre-programme effect	-	
Participation effect	-	

Notes: Estimations include the covariates specified in *Table 3*. Standard errors are within square brackets. *, **, *** refer to significance at 10, 5, and 1 percent levels respectively.

Table B 3 adjusted impact estimates on the hazard ratio to jobs and other activities, using a one-week interruption rule

	Job	Other
Jämtland		
<i>JSA+M</i>	1.374 (0.103)***	0.950 (0.144)
<i>M</i>	1.291 (0.105)**	0.907 (0.140)
Uppsala	1.598 (0.129)***	0.679 (0.139)***
Östergötland	1.113 (0.104)	1.027 (0.120)
Skellefteå	1.460 (0.162)**	0.990 (0.189)

Notes: Estimations control for the covariates specified in *Table 3*. “Other” activities refer to exits to cause unknown, sickness absence, early retirement and regular training. Standard errors are within parentheses. *, **, *** refer to significance at 10, 5, and 1 percent levels respectively.

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