Are there pre-programme effects of Swedish active labour market policies?
Evidence from three randomised experiments

Pathric Hägglund

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by

Pathric Hägglund[‡]

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Abstract

In this paper experimental data from three Swedish demonstration programmes in 2004 are used to study pre-programme effects of active placement efforts. 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 46 per cent increase in the escape rate between referral to and start of the programme services. This translates into a 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-codes: 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 of unemployment is also studied among those leaving unemployment in the pre-programme period.

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 46 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.

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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 remainder of this paper is structured in the following manner: Section 2 offers a theoretical framework for considering pre-programme effects while Section 3 briefly presents the background, the programme services, and the enrolment procedure of each experiment. Section 4 presents data and outlines the empirical strategy, and Section 5 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 6 sums up and offers some interpretations of the results.

2   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.

3 The experiments[3]

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.[4]

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.[5] The evaluator's job was to design the experiment and to protect the integrity of the experimental design throughout the evaluation period. The evaluator was also in charge of continuously

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[3] For a more extensive presentation of the experiments, the reader is referred to Hägglund (2006b).

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

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

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 the enrolment of job seekers. This is a pre-requisite 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.[6]

The enrolment routines and the interval lengths, briefly presented below, differed both between, and within, the demonstrations. However, all demonstrations performed repeated enrolments of new participants throughout the active phase of the experiments. 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. 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.[7]

3.1 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

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[6] 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.

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

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. Enrolment of participants was carried out in February, April, and August/September. The enrolment routine consisted of two steps. In the first step, those selected to participate were referred to an individual meeting after two weeks 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 three or five 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.[8]

Table 1, columns 1-3, describes some characteristics of the experiment and control groups using data from the unemployment register (*Händel*), and the UI payment register (*A-stat*), both presented in more detail in the next section.[9] 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.[10,11]

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[8] In the first enrolment in February, there was a five-week interval between notification and start of the programme. The following enrolments applied a seven-week interval. After the first meeting the experiment group members received "extra surveillance" in the public employment service internal information system, which, in practice, meant they on average received more job suggestions and job referrals. In the result section, however, I argue that this is not likely to have had any influential impact on the outcome.

[9] More experiment and control group statistics are reported in Hägglund (2006a).

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

[11] Compared to the control group, the JSA-experiment group has a significantly more extensive history of employment office registration, especially as openly unemployed. Note that the experiment and control groups are expected to differ significantly in some aspects (0.05 • the no. of covariates).

3.2   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. Besides openly unemployed, 17 per cent of the sample contained currently part-time employed. 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.

Enrolments 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 enrolment. On average, the length of the pre-programme period was 4.4 weeks. According to *Table 1* (columns 4 and 5), the vast majority of the targeted population was between age 25 and 44. Also, in contrast to the other demonstrations, the majority of the job seekers were women.

3.3   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 public employment service web-applications were emphasised.

The experiment and control groups were gradually filled up with two enrolments 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 scheme	
	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	0.19	0.24	0.20	-	-	0.09	0.13
Upper secondary	0.54	0.49	0.54	-	-	0.83	0.78
University	0.27	0.25	0.25	1.0	1.0	0.08	0.08
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
Citizenship (Swedish)	0.98	0.98	0.99	0.95	0.93	0.96	0.97

	Jämtland scheme			Uppsala scheme		Östergötland scheme	
	Exp. group (JSA)	Exp. group (No JSA)	Control group	Exp. group	Control group	Exp. group	Control group
Unemployment experience							
No. of programmes	4.25	3.54	3.68	1.72	1.62	1.46	1.41
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
UI compensation							
Income-based daily salary (SEK)	864	879	875	844	804	501	484
Daily compensation (SEK)	624	616	626	589	569	486	481
Number of observations	246	250	507	275	242	357	379

Notes: Data are based on information from the week of notification.

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

4　Data and empirical strategy

4.1　Data

I use UI-payment register data (*A-stat*) to follow the spells of unemployment. *A-stat*, administered by the UI Funds, contains weekly data on the number of UI-compensated days, type of UI benefit and benefit level for all unemployed who are entitled for UI benefits since January 1, 1999. Data also include information on payment decisions, previous income and remaining days of benefits. Using *A-stat*, the analysis is narrowed to those qualified for UI benefits. Since *A-stat* lacks information on disruption cause, it is linked to the unemployment register (*Händel*) and the event-specific information at the

disruption date. From *Händel* I also have access to individual-specific information on gender, age, educational level, citizenship, working disability, desired profession, education for and experience in desired profession, and unemployment history.[12]

4.2   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.[13] Matching *A-stat* and *Händel* data to obtain a disruption cause, 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.[14,15]

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 tested services compared with the regular services.[16] To account for the flow into

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[12] Most research on unemployment duration in Sweden has utilised the longitudinal data in *Händel*. However, 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. Data therefore risk to overestimate the length of the unemployment spells. Similar to *Händel*, *A-stat* relies on self-reported information. 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 than *Händel*. For a more detailed discussion about the flaws in the unemployment register, and its importance on the results of this study, see Hägglund (2006a).

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

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

[15] Note that by accounting for very short interruptions, disruptions of the UI spell is *not* necessarily equivalent to not participating in the demonstration services.

[16] 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

regular programmes and the subsequent expected reduced possibility of active job search, I estimate a Cox proportional hazard model.[17] The hazard is defined as the conditional probability of leaving unemployment at time t, given that the person is still unemployed at t. Unemployment spells ongoing at the end of the evaluation period, or ended before due to start of a labour market programme, are treated as right censored observations. This 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 job seeker i is:

$$\theta_i(t \mid x_i) = exp(x_i{}'\beta)\theta_0(t) \tag{1}$$

where $\theta_0(t)$ is the unrestricted baseline hazard, x_i is a covariate vector, and β is the corresponding parameter vector. 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.

5 Results

5.1 Mean differences

Table 3 reports pre-programme effects as mean differences in exits to both known and unknown destinations. UI disruptions without a corresponding disruption cause are referred to as exits to "unknown destinations". The exact exit deviations to the various disruption causes should be interpreted with some

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tested services compared to a "normal" dose of referrals to the regular services. However, with the very short evaluation periods, the vast majority of the control group members are not referred to any services at all.

[17] See Lancaster (1990) for a theoretical presentation.

care since they are most likely sensitive to the higher case-worker/job-seeker intensity in the demonstration services, which reduces the risk of incorrect coding. Of main focus is instead the sum of all disruptions.

In the Jämtland demonstration, a large positive difference in total exits is reported. The 8.7 percentage point's deviation is statistically significant at the one per cent significance level, and corresponds to an effect of 23 per cent in disruption intensity. The experiment group has a somewhat higher exit rate to both known and unknown exit causes. The significant difference in exits to "other known destinations" is the result of relatively more people reporting temporary job-search interruptions. The groups display almost similar portions of exits to regular programmes, which imply that the positive effect is not the result of more frequent exits to regular programmes in the control group.

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 deviation suggests a large and highly significant positive pre-programme effect of 14.2 percentage points, or 37.5 per cent. 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.

Similar to Jämtland, the Uppsala demonstration reports a similar portion of job seekers leaving open unemployment for programme participation in the pre-programme period. Also, the experiment and control groups significantly deviate in the reported job-exit frequency. However, since there is a negative significant deviation in the exit frequency to "unknown destinations", there is a considerable risk that the positive effect is due to information asymmetries and the scheme case workers´ better possibility of keeping track of job seekers. In total, a small and insignificant negative effect is found on the disruption intensity.

In Östergötland, the proportion of control group members initiating a regular programme during the study period significantly outweighs the corresponding portion in the experiment group (-6.1 percentage points). 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 pro-grammes, were an alternative to other active measures. Work-practice schemes,

the Youth Guarantee and preparatory training courses dominated these measures.[18] There are, ex ante, no reasons to 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. In column 5, only a very small positive pre-programme effect on the disruption intensity is reported.

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[18] 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.

Table 3 Pre-programme effects, mean differences (standard errors are within parentheses).

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

5.2 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 4*.

First of all, in correspondence with the results from *Table 3*, referrals to the services 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 31.5 per cent increased exit rate

as a result of being referred to the services. The reported adjusted impact (29.6 %) is somewhat smaller but still significant.[19] The non-adjusted estimations report a large (55.1 %) positive and significant effect from referrals to the combination of services (JSA), and a smaller (10.7 %) positive but insignificant effect from referrals to only the increased monitoring, or the no-JSA, services. Adjustment generates a somewhat smaller JSA point estimate, 46.1 per cent, and a slightly larger no-JSA impact estimate of 12.7 per cent.[20,21]

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 %) but is still insignificant.

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. However, as it turned out, many control group members faced a considerable chance/risk of being referred to regular activities in the near future, which diminished the "treatment dose" between the groups.

Robustness of the results has 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.[22] However, performing

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

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

[21] Analysing the weekly hazards in the pre-programme period shows that the positive pre-programme effect in the JSA-group is rather constant throughout the pre-programme period (Hägglund 2006a). This suggests that pre-programme effects can occur also in situations where the pre-programme interval is relatively short. It also indicates that the extra surveillance in terms of more frequent job suggestions and job referrals among the experiment group members in the last weeks of the pre-programme period (footnote 7), are not likely to have affected the outcome. Had that been the case we would have expected an increasing pre-programme effect. Another strong argument in favour of the reported results is the significant outcome deviation between the experiment groups. Since both these groups received the extra surveillance, the difference must derive from the different services referred to.

[22] 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

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.[23]

Table 4 Pre-programme effects on the hazard rate, non-adjusted and adjusted (standard errors are within parentheses)

	Jämtland			Uppsala	Östergöt-land
	All	*JSA*	*No JSA*		
Pre-programme effect (non-adjusted)	0.315***	0.551***	0.107	-0.005	0.024
	(0.103)	(0.121)	(0.130)	(0.165)	(0.157)
Pre-programme effect (adjusted)	0.296**	0.461***	0.127	0.030	-0.010
	(0.106)	(0.127)	(0.137)	(0.172)	(0.165)

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.

5.3   The flow back to unemployment

To appreciate the importance of pre-programme effects on UI savings, the persistence of the pre-programme outflow must be analysed. 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 inter-pretation. This is because the groups compared in each experiment need not be

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the number of exits accounted for in the analyses. This would thus have a negative effect on the impact estimate.

[23] 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 coded as programme participants.

comparable.[24] The analysis thus only describes the reoccurrence of unemployment among the subsets of experiment and control group members with disrupted UI spells.

Table 5 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.[25]

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 (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 subsequent unemployment is higher among those offered only the increased monitoring (+1.6 weeks) compared to those offered also 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 short-term 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 4) 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,

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[24] 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.

[25] Compensation during regular programmes (activity support) and open unemployment is the same for UI eligible job seekers.

the average number of weeks in regular programmes after 26 weeks is almost similar. Apparently, taking part in the experiment services only seems to have postponed participation in a regular programme.

5.4   The pre-programme effects into perspective

To put the impact estimates of the Jämtland demonstration into some perspective, the average (adjusted) 30 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 6.5 UI-day drop (10.1 and 2.8 UI days for the JSA and the no-JSA services respectively) in the pre-programme period. With an average daily compensation of SEK 626, the reduction of the UI spell in the pre-programme period saves SEK 2 million in UI benefits. Since the demonstration's total expenditures were SEK 2.5 million, the savings covered more than 80 per cent of the expenses.

Table 5 Number of weeks as openly unemployed or as participant in a programme in the 26-week period that follows after exit from unemployment.

	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 unemployment	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 experiment 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 observations	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.

6   Conclusions

This paper has investigated the significance of pre-programme effects of being referred to active placement measures among UI receivers in Sweden. Using experimental data from three separate demonstration programmes in different regions in 2004, disruptions of the UI spell in the interval between notification of the programme and programme start were 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 46 per cent increase of the hazard rate preceding a combination of treatment including job-search assistance activities and increased job-search monitoring, translates into a 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 evidence 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. One possible explanation for the deviating results is that while the Jämtland demonstration invited a broad group of UI receivers to participate, the other two targeted on locally specific difficult groups. 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.

An alternative explanation is the on average low job-search activity level among the unemployed in Jämtland. In the Job-seekers survey, a monthly survey among currently unemployed and programme participants performed by the Swedish Labour Market Board, Jämtland has reported the largest proportion of non-active job seekers in two of the last three years. A low search effort indicates inadequate control of the requirements as UI receivers, which, in turn, could imply great scope for motivation effects.

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