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Job practice:
an evaluation and a comparison with vocational labour market training programmes

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Job practice: an evaluation and a comparison with vocational labour market training programmes[§]

by

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Abstract

We have estimated effects of job practice for participants entering the programme between 1999 and 2006. On average the programme had a moderately sized positive effect for the participants – the expected time to work for the unemployed participants was reduced by around six per cent over a 700 days long follow-up horizon counted from the programme start date. Participation also gave rise to higher future labour income and a reduction in social assistance take-up. When we compare job practice with labour market training, we get the somewhat paradoxical result that job practice participants would have gained more from training, while the training participants would have gained equally much from both programmes. A re-shuffling of participants between the programmes, hence, would have been associated with higher average effects.

Keywords: Labour market policies, Evaluation
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1 Introduction

Job practice is an active labour market programme used to improve the labour market prospects of unemployed job seekers through work experience and employer contacts. Presently, the programme is one of the three major programme types for unemployed Swedish workers.[1] The participants perform tasks in regular firms, but are not employed. Participants instead receive income support equivalent to what they would receive from the unemployment insurance. Programme duration does not normally exceed 6 months. The direct costs, i.e., costs in excess of income support, of the programme are low. However, because participants spend less effort on job search than do openly unemployed job seekers, the programme can be expected to decrease the flow from unemployment to work during the participation period (a so called lock-in effect). Hence, in order for the programme to speed up the transition to work, there must be a post-programme treatment effect that offsets the lock-in effect.

Job practice may help the unemployed in a number of ways. First, practice may be a way to acquire skills through learning by doing and, hence, to increase the employability of the participant. Second, practice may be a way to improve the social network of the unemployed. Given that a large fraction of all vacancies are filled using networks/informal channels, this is a potentially valuable characteristic of the programme, especially for participants with poor social networks. Third, practice may give the participant an opportunity to show a prospective employer that (s)he is sufficiently productive despite being unemployed. Given these potential benefits of participation, it is reasonable to predict that a variety of types of participants may gain from the programme.

Job practice programmes of different designs and with different target groups have been components of Swedish active labour market policies since the 1980s, but the number of evaluations is small (Calmfors et al., 2004; Forslund & Vikström, 2011). The present incarnation of the programme has only been evaluated in a small number of studies. Results in evaluations by the programme provider, the Swedish National Labour Market Board (Arbetsförmedlingen, 2012; Nilsson, 2008), suggest that other programmes considered in the evaluations generally had more positive effects on the

[1] The other two are vocational training programmes and subsidised employment. One might also include job search assistance in the list of programme types used.

transition to work, but that the effect of job practice compared to continued job search in open unemployment has been positive for a majority of the years analysed. According to Arbetsförmedlingen (2012), employing a similar method as we do in the present paper, the effects of job practice on the expected time to work were virtually equal to zero between 1999 and 2001, but then slowly improved between 2002 and 2009. The results in Riksrevisionen (2010) suggest that other labour market programmes outperformed job practice as a way back to work. Finally, Forslund et al. (2011) found that vocational training programmes had a larger effect than job practice on the expected time to work, and that this pattern was more pronounced in boom than in recession.

The main purpose of this paper is to provide an evaluation of the average performance of the work practice programme, i.e., to estimate the average effect for the programme entrants over a longer time period (1999–2006). In addition, we provide estimates for different years of entry. If these estimates indicate that the treatment effect varied a lot over this period, this would call for caution when using existing estimates as a guide to the effects of changes in the scale of the programme.

We also estimate effects for different sub-groups, as indicated by different observed characteristics of the participants, such as age, sex, educational level, region of birth and unemployment history. In the same spirit, we present estimates by type and sector of practice. Estimated effects for different sub groups and different types of programmes should be policy relevant when deciding who should get what and when.

From a policy perspective it is often as relevant to ask “which programme?” as to ask “any programme at all?” for a given unemployed person. In a final set of estimates we address this by a direct comparison of the treatment effects for job practice and vocational training programmes.

2 Identification and estimation

In this section we present our approach to identification and estimation of the effects of job practice. Our maintained assumption is that we can derive reasonable estimates assuming selection on observables. In general, this may not be a credible assumption, but in our case we argue that the combination of rich data at our disposal and an appropriate estimator actually means that the assumption, although untestable, makes good sense.

2.1 Design

2.1.1 Conditional independence assumption and matching

To estimate the effect of participating in job practice we use register data from the National Labour Market Board and Statistics Sweden. The unconfoundedness or conditional independence assumption (CIA) is essential for our design. CIA amounts to assuming that we observe all variables (X) that influence both participation in the program (treatment), T, and the outcome of programme participation, Y, (e.g., of duration to work, income or social benefit take-up).

Denote the potential outcome of an individual to participate in the program with $Y\ (T = 1)$ and the outcome of non participation with $Y\ (T = 0)$. Formally, CIA states that T is independent of Y given X, where X denotes the observed covariates[2]. On the basis of this assumption it is possible to identify the *average causal effect* of the programme on the outcome of interest using a matching procedure. The matching procedure will for a programme participant find a matched partner (control) that is similar in all observed respects, except that the partner did not participate in the program. Under CIA, treatment and potential outcomes are independent, given the observed covariates. Hence, under CIA the matching procedure mimics a randomised experiment – the observed average of $Y\ (T = 0)$ for the controls will be an unbiased estimator of the average of the unobserved $Y\ (T = 0)$ for the participants because treatment is independent of the potential outcome given the observed covariates under the assumption. Evidently, the average of $Y\ (T = 1)$ for the participants is an unbiased estimator of the average outcome of participation for the participants.

The result of the matching procedure is a selection of matched pairs with similar observed characteristics. In a successful match the covariates are balanced. Since we have several observed covariates, we employ so called propensity score matching. In propensity score matching we do not match on the covariates, but instead on a scalar valued function of the covariates, the propensity score (Rosenbaum & Rubin, 1983).

[2] See de Luna & Johansson (2007) for a more detailed explanation.

The propensity score is simply the probability to participate in a program given certain characteristics (the covariates). Hence, it is a conditional probability measure given by $p_i(x) = Pr(T = 1|X = x)$

The propensity score is a balancing score: at each value of the propensity score the distribution of X is the same among treated and controls. The CIA is non-testable, but the balancing property can be checked using data. If CIA holds given X, it also holds given the propensity score. Hence, under CIA, matching on the propensity score mimics a randomised experiment, as all relevant covariates will balance when conditioning on the propensity score.

In our application we employ a set-up from Fredriksson & Johansson (2008) where exact matching and propensity score matching are combined. We match exactly on the length of an ongoing unemployment spell before programme start, and at what point of time during a year the programme starts.[3]

Exact matching on the duration to programme start means that potential controls for any programme participant are all registered unemployed who have not yet (at the unemployment duration when the treated individual for which we want to find a control enters the job practice programme) entered any labour market programme. The exact matching on duration to programme start should catch effects of otherwise unobserved covariates (such as unobserved individual characteristics affecting the likelihood of exiting unemployment). Hence, the set-up of Fredriksson & Johansson (2008) is one way to treat selection on unobservables in a matching framework.[4]

The propensity score is unknown and must be estimated from the data. We use probit regressions to estimate the propensity score and then match on the estimated propensity score comparing each participant to the nearest neighbour non-participants (in terms of the propensity score).

2.2 Causal effects: the average effect of treatment on the treated

The parameter of interest in our study is the average causal effect of program participation of the treated on the outcomes of interest.

[3] To be more precise we divided each year (1999-2006) into four strata (January-March, April-June, July-September, October-December) and match on programme start within each of these strata.

[4] Another possible estimator would have been the "timing of events" estimator of Abbring & van den Berg (2003), which relies on another set of (untestable) assumptions to take care of the effects of selection on unobservables.

To fix ideas, assume that each potential participant has an outcome Y_i associated with participation and non-participation, respectively. The outcome of participation is

$Y_i(T_i = 1);\ i = 1, \ldots, N$

where $T_i = 1$ denotes the treatment (participation in job practice) and N denotes the total number of potential participants.

In order to estimate the treatment effect we want to compare the counter factual outcome of non-participation with the actual outcome of participation for those who actually participated in the programme. Let $T_i = 0$ denote non-participation. Then the outcome for non-participation is

$Y_i(T_i = 0)$ where $i = 1, \ldots, N$

The difference in outcomes between $T = 1$ and $T = 0$ describes the causal treatment effect for the ith participant, denoted as

$t_i = Y_i(T_i = 1) - Y_i(T_i = 0)$

Since we only can observe the actual outcome (either $T_i = 1$ or $T_i = 0$) for any individual, we can never estimate t_i. However, consider instead the average effect of treatment for the treated,

$$E\big((t_i|T_i = 1)\big) = E[Y_i(T_i = 1))] - E[Y_i(T_i = 0)]$$

The first term on the right hand side can be estimated directly from data on the outcomes of the participants. The second term must be estimated in some other way. In a randomised experiment all characteristics are on average equal for participants and non-participants. Hence, in a randomised experiment the average outcome of the non-participants will be a good estimator of the outcomes of the participants, had they not participated. The matching procedure in combination with the CIA acts as a substitute for a randomised experiment. Given CIA, the average outcome for matched non-participants is an unbiased estimate of the average outcome for the participants, had they not participated in the programme.

2.3 Censoring of controls

An issue in our study design is the fact that individuals in the control group (individuals which are registered as unemployed, but do not participate in any program at the given point in time) may enter job practice or another labour market programme later in the unemployment spell. To handle this we censor controls when they enter a programme.[5] Notice that it is not an option to condition on future non-participation in programmes. This kind of conditioning on future outcomes will under reasonable assumptions imply an over sampling of individuals with characteristics making them more likely to get a job (the best way not to end up in a programme is to get a job). Hence, conditioning on future non-participation for controls will induce a downward bias on the estimated treatment effect.

2.4 Duration to work

The duration to work is our main outcome of interest. The time to work is analysed with survival analysis. Survival functions for both participants and non-participants are calculated with a Kaplan-Meier estimator. The cumulative difference between the estimated survival functions for the participants and the non-participants gives us an estimate of the effect on the expected duration to finding a job.

Our survival analysis is based on censoring individuals in the control group when they enter any kind of program or when they enter job practice. In the first case, we estimate the effect of participating in job practice where the counterfactual is job search, using the standard services of the PES, without any programme participation. In the second case, the counterfactual instead is an average of all treatments offered at the PES.

As work we define any kind of unsubsidised full-time, part-time and hourly employment that lasts for at least 30 days.[6]

Notice that we measure time to work from programme start, not from programme exit. First, to the extent that participants' outcomes during ongoing programme participation are affected, our design will capture such effects of the programme. For example, we would expect the flow to jobs to be slower during programme participation because participants will spend less effort and time on job search while in the programme. Second, we have no information on the planned programme duration; our data only gives us information on when participants leave the programme. If we would measure time to work from programme exit, we would get biased estimates of treatment effects

[5] If (s)he enters a programme we apply right censoring on the exact day for programme start. This set-up was suggested by Fredriksson & Johansson (2008) and de Luna & Johansson (2007).

[6] More precisely, we define exit to work using the register data of the PES to identify the destinations of exits where work is defined along the lines given in the description.

to the extent that some programme leavers exit early from the programme because they have found a job.

2.5 Effects for sub groups

The aim of the matching procedure is to estimate causal effects of job practice for all participants. It is, however, also of policy relevance to find out if any broadly defined group seems to benefit more (or less) than the average participant. Patterns in effects may also reveal interesting information about through which mechanism the programme works. Hence, we also estimate effects for different sub groups of participants. When we estimate effects for different sub groups, for example non-Nordic immigrants, we only compare treated non-Nordic immigrants with non-Nordic controls. The matching procedure for the subgroups is similar to the matching procedure of the total group. However, in order to increase the sample sizes, we do not use exact matching on time registered before programme start and calendar time of programme start. In these cases we include previous length of the unemployment spell, as well as dummy variables for year and month of programme start as covariates in the probit estimation of the propensity score. All other covariates are used in the same way as when we do exact matching for all individuals in our sample. The only restriction is that we only match individuals in the given sub group.

2.6 Effects for programme starts at different unemployment durations

Our matching approach, where we match exactly on the duration to programme start, enables us to estimate treatment effects for different durations of the ongoing unemployment spell before programme start. Such estimates indicate whether job practice participation has had the best effects for those getting early treatment or if the effects have been better for those getting treatment later. Notice that if treatment effects are heterogeneous over individuals, any differences in treatment effects will reflect both characteristics of the treated group and differences in effects depending on the timing of treatment. Hence, different mechanisms for selecting participants may well affect estimated treatment effects at different durations.

2.7 A comparison with labour market training programmes

Throughout our analysis, the CIA is a maintained assumption. The credibility of this assumption hinges on our ability to observe everything that determines both participation and outcomes. In the sense that all programme participants may share some unobserved characteristic that distinguishes them from non-participants (e.g., something that is observed by case workers but not present in any data, such as a (lack of) firm handshake, CIA is more likely to hold when comparing participation in different programmes. The question of whether participation in one programme gives better effects than participation in another is also an interesting policy question. An important alternative programme is labour market training. Therefore, we also estimate the effect of job practice as compared to labour market training.

3 Data

The data we use come from the IFAU database. The main source of information derives from the Swedish national employment office (Arbetsförmedlingen) and encompasses detailed unemployment and programme participation histories of all individuals that are searching for work trough the Swedish Public Employment Service (PES).[7] Our data on unemployment history extends to 7th of April 2011, which thus is the point of time for right censoring. We also use annual information on labour income, unemployment benefits, social assistance benefits as well as marital status, educational level, country of birth and number of children per household from the database LOUISE administered by Statistics Sweden (SCB).

3.1 Selection of programme participants and control group

We restrict our analysis to individuals between ages 20 and 60, and consider entrants in the period January 1999–December 2006.[8] Based on our data we construct two groups, a treatment group and a control group. In order to be selected into our treatment group an individual must participate in job practice as the first program in the ongoing unemployment spell and must not have been registered as unemployed for a period of at least 180 days before the start of the current unemployment spell. This selection criterion ensures that we measure the effect of job practice and not another labour market program. An additional effect of this selection criterion is that we tend not to

[7] Swedish national employment office collects information from all local offices of the PES into a database "Datalagret". Datalagret contains information on an all individuals register at the PES since august 1991.
[8] The present job practice programme dates back to 1999, and the end year 2006 is chosen because of difficulties to trace participants in the data after the start of the job and development guarantee in 2007.

include individuals with very long unemployment spells. However, both participants and controls may have unemployment histories before the spell under consideration. We also exclude individuals who do not participate at least 30 days in the programme. The reason for doing this is that an individual leaving for work within 30 days from programme start may be aware of the job even before the programme start.[9]

The number of job practice participants varies substantially over the years 1999 to 2006, but there is a pronounced downward trend; the number of participants in 2006 is roughly half the number in 1999; see Figure 1. This downward trend in participation is also present when we consider the fraction of all registered at the PES in job practice, as is evident from Figure 2.

Vocational training programmes, with which we later compare job practice, show a similar declining pattern in participation, also displayed in Figure 2.

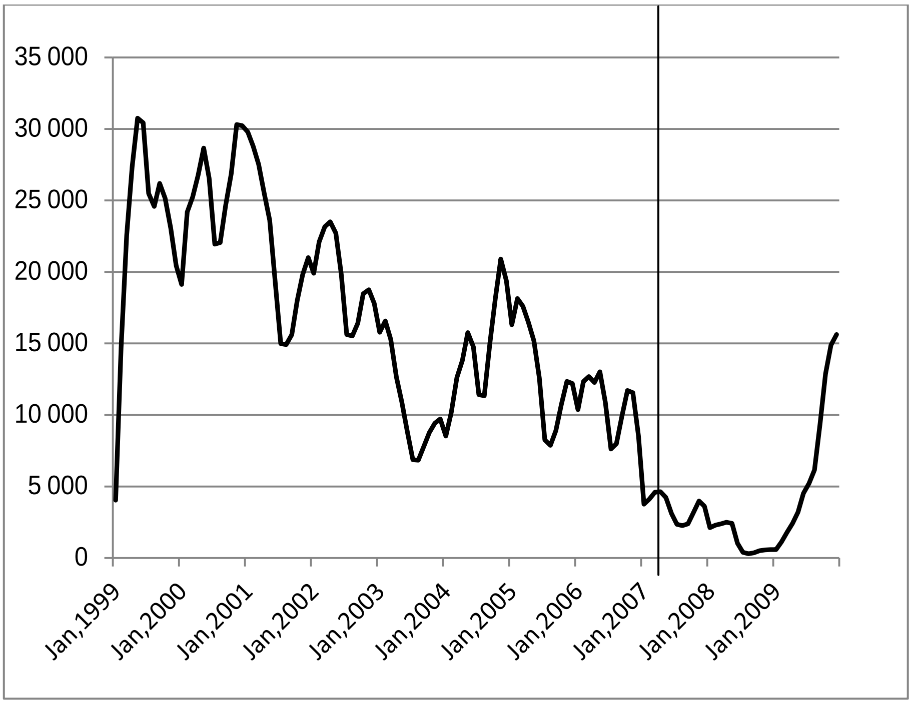

Figure 1: Number of participants in job practice 1999–2009

Note: Figure 1 presents the number of programme participants between 1999 and 2009. The vertical line marks the start of job and development guarantee. From that point in time data are not comparable because job practice participants within the job and development guarantee are not counted.

[9] In the case at hand this means that an individual has been registered in the category 54, for at least 30 days. The control group is treated in the same manner.

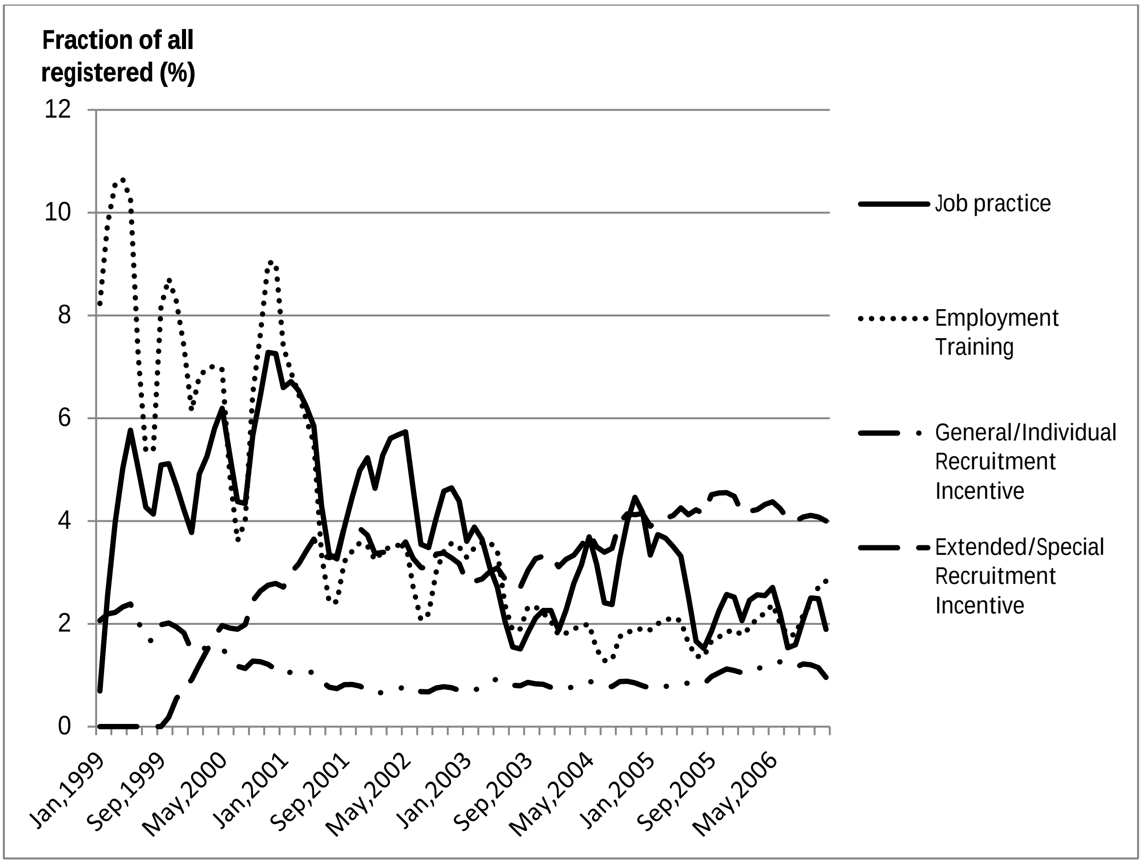

Figure 2: Fraction in different ALMPs of all registered at the PES, stock sampling, 1999–2006

To construct the group of potential controls, we select individuals registered as openly unemployed at the PES on the 15th of each month who have not participated in any labour market programme in their ongoing unemployment spell. For these individuals the 15th is used as the "constructed" programme[10] start date. Furthermore, the same restrictions are imposed on the control group as on the treatment group. We exclude individuals who get a job within the first month after the programme start date. The reason for imposing this criterion is the same as for the treatment group (i.e., that the individual may have information about the job already before the programme start).

Using these criteria we get a sample of 109,202 observations (spells) among the treatment group (distributed over 105,815 individuals) and 9,599,293 observations (spells) among the potential controls (1,367,650 individuals). Since we investigate a seven-year time period individuals can appear more than once. The matched controls are selected among the potential controls in the way described in Section 2.

[10] Programme here actually means just the opposite!

Table 1 shows descriptive statistics (averages) for all unemployed, all job practice participants and participants and controls after matching. The variables shown are those used in the matching procedure.[11]

Comparing job practice participants and all unemployed, there is a weak tendency that participants have characteristics that predict worse labour market prospects: they are less educated, have more previous register days and spells and lower previous incomes.

Imposing restrictions as indicated above, we find matches for 83 000 of the 477 000 participants.[12] Comparing the matched participants with the whole group of participants, we see only moderate differences. The most conspicuous differences are that the matched participants are younger, have higher average previous incomes, higher education and (by construction) shorter ongoing unemployment spells as well as more days since the previous unemployment spell. All in all, there is no strong indication that the estimated effects for the matched participants should be a bad estimate for the whole group of participants.

[11] The following interactions (and nonlinear functions of the variables) are included in the matching procedure: age times registration days, age times income one and two years before programme start, age times social benefits one and two years before programme start, age times social benefits, income squared, income times social benefits. We also control for county of residence which is not included in Table 1.

[12] This large difference primarily reflects the restrictions imposed on the sample of treated and not a large difficulty in finding matches for the selected treated group; imposing the restrictions on the sample we go from 477 000 to 109 000 spells among which we find matches for 83 000.

Table 1: Descriptive statistics (means) for registered unemployed, all job practice participants, matched participants and controls 1999–2006

Variable	All registered unemployed at PES, stock sampling March & October	All spells with job practice	Matched spells with job practice	Matched controls
# of days in PES register before current spell	823.7	919.8	964.1	966.5
# days in ongoing spell at programme start	461.4	514.1	158.9	157.7
# days since last spell	503.0	385.4	745.5	749.5
# previous programme spells	1.9	2.4	2.4	2.4
# previous spells of subsidised employment	0.1	0.1	0.1	0.1
Age	39.1	38.8	34.9	34.9
Income (100s of SEK) year t-2	744.3	578.2	742.3	736.4
Income (100s of SEK) year t-1	671.1	448.2	682.5	679.0
Share, per cent				
Males	45.6	46.2	51.1	51.3
High School	49.0	53.2	54.7	54.5
University, at most 2 years	4.8	4.4	5.4	5.5
University, at least 3 years	15.0	12.9	16.7	16.7
PhD	0.4	0.3	0.3	0.4
Disabled	14.7	14.3	9.5	9.6
Interlocal job seeker	18.4	23.0	20.4	20.6
Entitled to UI	80.3	87.3	86.6	86.3
Applying for full-time job	93.8	95.4	96.0	95.9
Married/partnership	33.2	32.4	30.0	29.9
Kids < 18 years	35.3	35.9	43.8	43.9
Married/partner/kids	39.6	39.7	40.2	40.2
Nordic (except Sweden)	3.5	3.4	2.6	2.6
Non-Nordic	21.8	21.0	21.0	21.2
Social assistance take-up year t-2	20.5	20.6	20.9	21.1
UI benefits year t-2	42.9	55.5	43.6	43.5
Social assistance take-up year t-1	20.8	20.6	19.5	19.7
UI benefits year t-1	50.6	66.3	55.0	54.9
# observations	5 766 182	476 727	82 619	82 619

In Table 2 we show some measures of the duration of spells in job practice and vocational training programmes. The table reveals that job practice spells on average last for about three months, whereas vocational training spells are significantly longer; on average spells in training are around four and a half months. Hence, we would expect smaller locking-in effects of job practice than of training.

Table 2: Duration of programme spells

	Job practice	Employment training
# All program spells, 1999-2006	524 341	354 570
Average program duration, all spells, 1999-2006	96	139
Median program duration, all spells, 1999-2006	62	108
# Matched particpants in job practice, 1999-2006	82 584	
Average program duration, matched participants, 1999-2006	87	
Median program duration, matched participants, 1999-2006	59	
Matched participants, job practice vs. Employment training	94 703	94 703
Average program duration, matched participants job practice vs. Employment training , 1999-2006	88	144
Median program duration, matched participants job practice vs. Employment training , 1999-2006	60	113
Matched participants, Employment training vs. job practice	72 634	72 634
Average program duration, matched participants Employment training vs. job practice , 1999-2006	97	144
Median program duration, matched participants Employment training vs. job practice , 1999-2006	69	112

3.2 Matching

In the matching procedure used in the main analysis we use a combination of exact matching and nearest neighbour propensity score matching with one neighbour.[13] When we analyse different sub groups of participants, we only do propensity score matching.[14]

CIA is not testable in the sense that there is no test to confirm the choice of covariates. However, we can test post hoc if the (propensity score) matching balanced the covariates in the control and treatment groups.

The exact matching follows the setup of Fredriksson and Johansson (2008), where we match exactly on the durations of the ongoing unemployment spell as well as on

[13] We first match on duration to programme and entry date (year, month) exactly, then we enter the other variables into the propensity score.
[14] The cells defined by duration and entry date become small when we analyse sub groups. This means that we get too few observations to estimate the propensity score on within the cells.

year and month of programme start. Since it is not possible to match exactly on the number of registration days before program start, we build strata, with 7 categories[15].

By employing this kind of set-up we take care of several issues, since the procedure balances for fluctuations in the business cycle (matching on year of entrance), seasonal effects (matching on month of entrance) and the waiting time till the programme starts. Matching on the waiting time till programme start will under reasonable assumptions balance unobserved characteristics that are important for the transition from unemployment to work as well as for programme participation. However, once again we stress that the identifying assumptions are untestable and that at the end of the day it is up to the reader to take a stance on the plausibility of the assumptions and the reliability of the results derived.

4 Results

In this section we present our results. The presentation is structured in the following way. *First*, we show results for all participants. *Second*, we present results for various sub groups (year of programme participation, gender, age, educational level, non Scandinavian birth region, disability and waiting time to program). Notice that when we present survival analysis for the sub groups, we match within the relevant subgroup. This means that the effect for the total group is generally not the average of the effects of various sub groups. *Third*, we present estimated effects by sector and occupation of practice. *Finally*, we compare the effects of participation in job practice and labour market training.

4.1 Standard errors

First, however, a brief note on standard errors. Our main interest is focussed on the estimated effect on the expected duration to work. This effect is derived by first estimating survivor functions for participants and controls. Given estimated survivor functions, the difference between the survival for participants and controls at any given duration measures the effect on survival at this duration. These differences can be

[15] 1) less than one month registered, 2) more than one month registered, 3) more than three month registered, 4) more than five month registered, 5) more than eight month registered, 6) more than twelve month registered and 7) more than eighteen month registered.

cumulated and the resulting sum is a measure of the effect of participation on the expected time to work.

It is rather straight forward to derive estimates of the standard errors of the survivor functions and, hence, of the difference between them. However, because of extremely involved dependencies over time, estimating standard errors of the cumulated sum is not feasible. We could display estimated standard errors of the survivor functions in the graphs we use to present our results. However, even for the analysis of effects for sub groups, the situation can be described like this: if there is a clear visual difference between survival rates (the effect is economically significant), then the difference between survival rates is also statistically significant.[16] Hence, in order to avoid making the figures too messy, we have chosen to display survival functions without estimated standard errors.

4.2 Results for the total group of participants

4.2.1 Time to work

We first present an analysis of how programme participation influences the time to find a job. As stated above 'the time to work' is calculated with two different censoring schemes. First, we right censor when individuals of the control group enter job practice. The parameter estimated will in this case capture the effect of job practice where the alternative is to keep on looking for a job as openly unemployed or to find a job through entering other labour market programmes. Second, we right censor individuals in the control group who enter any kind of active labour market program. The parameter estimated in this way will be capture the effect of participation in job practice where the alternative is to keep on looking for a job as openly unemployed. We first present results for right censoring when entering job practice.

Figure 3 presents Kaplan-Meier survival curves for participants and non-participants. We follow the participants and non-participants for 700 days after programme start. Hence, all effect estimates refer to the effect with roughly a 2-year follow-up horizon.

[16] This would not hold for estimated annual effects for even smaller sub groups, e.g., for small groups of participants in single years.

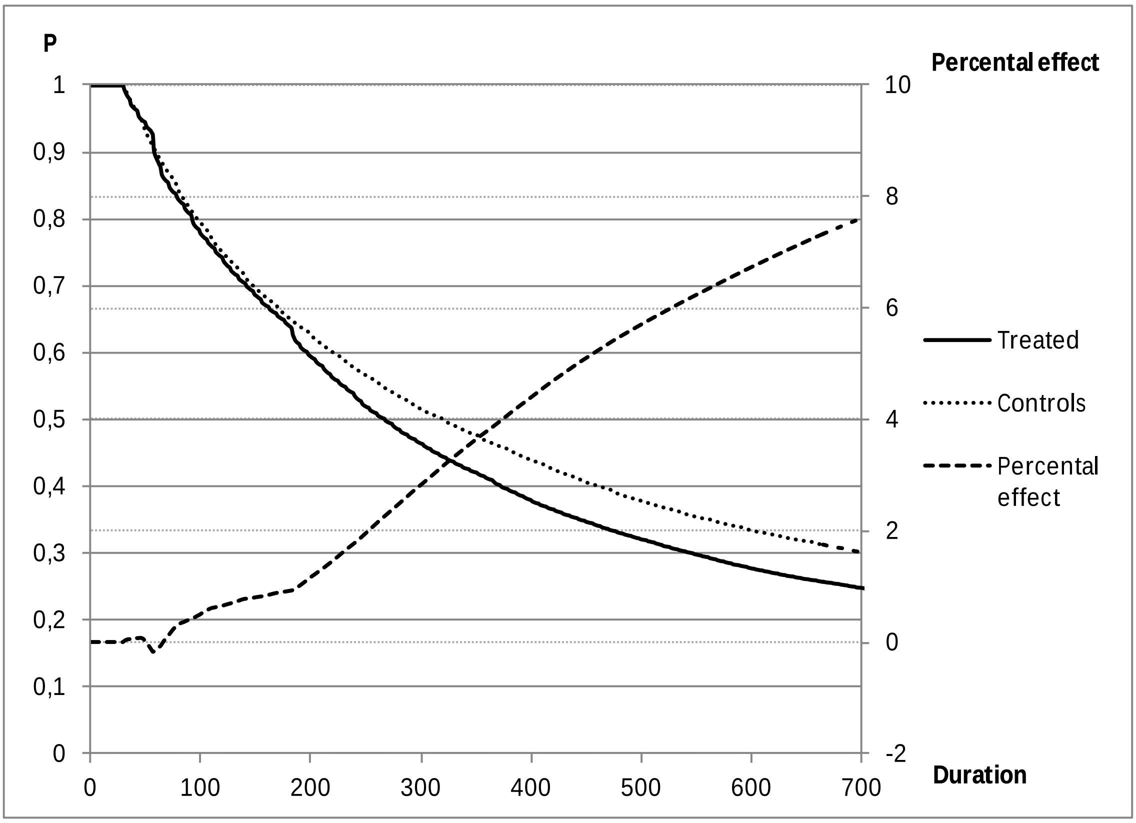

Figure 3: Estimated survival curves for participants and non-participants and estimated treatment effect. All matched participants, 20–60 years old, 1999–2006, censoring when controls enter job practice

At any point in time the vertical difference between the two survival curves measures the (cumulated) difference in job-finding between the two groups. The participants remain jobless to a larger extent in the beginning (for roughly 50 days). This is expected – the participants will search for jobs less intensively while in the programme, and hence will leave unemployment at a slower rate. This is often referred to as the *locking-in effect* of labour market programmes. If anything, the locking-in effect seems small and short. After the first period of locking in, participants start leaving unemployment more rapidly than the controls. The natural interpretation of this is that the programme has a positive *treatment effect*. This treatment effect is present over the rest of our follow-up horizon.

To get a summary measure of the effects for the participants, taking account of both locking-in and treatment effects, we simply sum the differences between the survival curves. This measure, which is a measure of the effect on the expected time to work, is measured along the right-hand-side axis of Figure 3. Measured in this way, the treatment effect for the total sample population is around 7 percent, meaning that programme participation gives rise to a 7 percent shorter expected duration to work given our 700 days follow-up horizon.

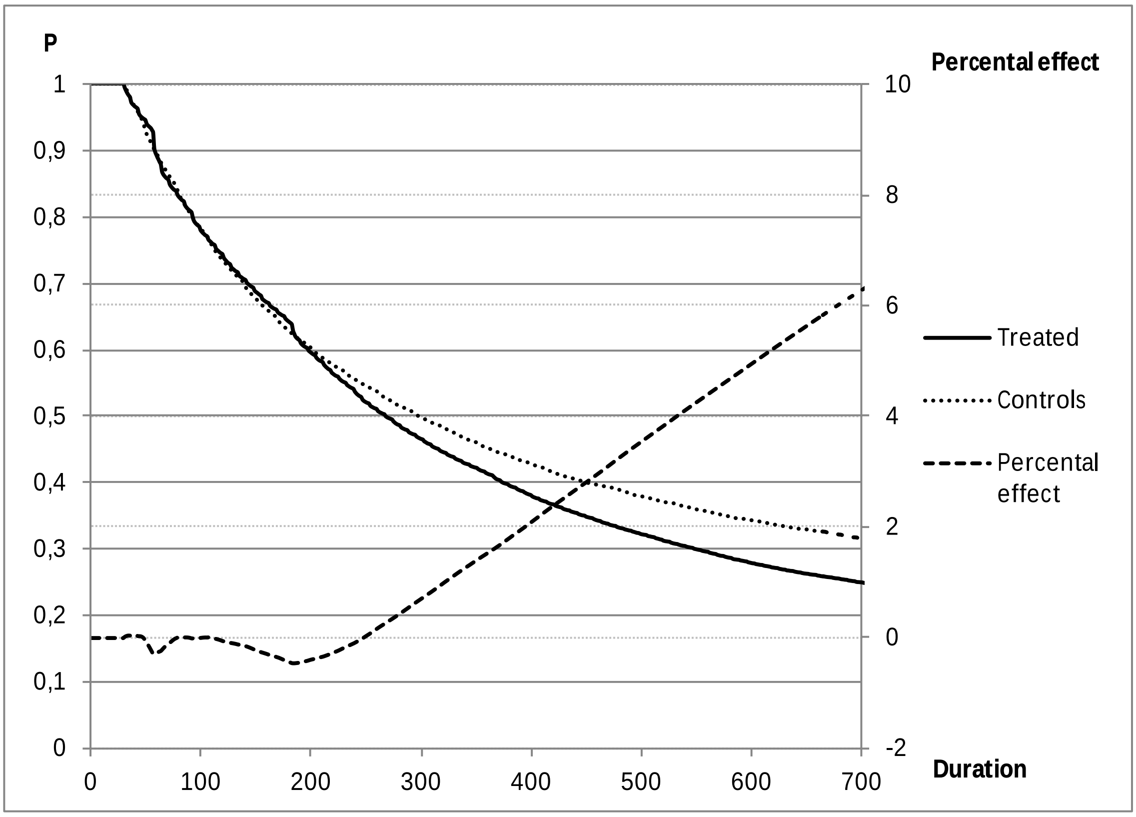

Figure 4: Estimated survival curves for participants and non-participants and estimated treatment effect. All matched participants, 20–60 years old, 1999–2006. Censoring of controls at any labour market programme entry

In Figure 4 we show the estimates with censoring at any programme entry for the controls. Both the time pattern and the size of the estimated effect at the 700 days follow-up horizon are very similar to the results for the alternative censoring scheme.

4.2.1.1 *(How) Do effects on time to work vary over time?*

Figure 5 and Figure 6 show the same estimates with both censoring schemes for the years 1999–2002 and 2003–06.[17] Again, we see that the two censoring schemes produce similar results. The general pattern with an initial locking-in effect is present mainly in the first period. For the years 1999–2002 the treatment effect is large enough to compensate for the locking-in effect; but the effect is much smaller than for the period

[17] We have estimated annual treatment effects. The pattern found in the annual estimates is similar to the patterns in figure 5 and figure 6 in the sense that the estimates for each of the years 1999–2002 are similar to the average over this period and that the same holds for each of the years 2003–06 comparing to the average over these years.

2003–06. We can only speculate about the reason for this pattern. One possibility is that the sharp decline in the number of participants, if treatment effects are heterogeneous, went hand in hand with a selection of participants who gained more from the programme. An explanation in the same spirit would be that case workers could achieve better matches between participants and job practice organisers with fewer participants.

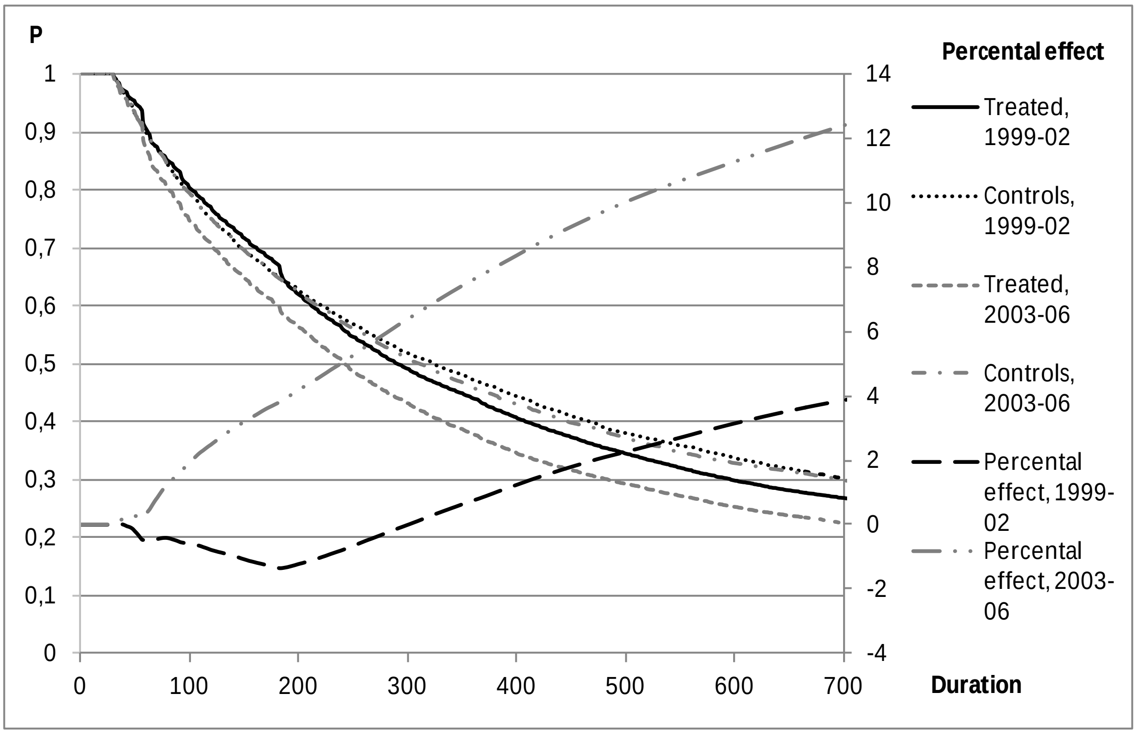

Figure 5: Estimated survival curves for participants and non-participants and estimated treatment effect. All matched participants, 20–60 years old, 1999–2002 and 2003–06. Censoring of controls at entry of job practice

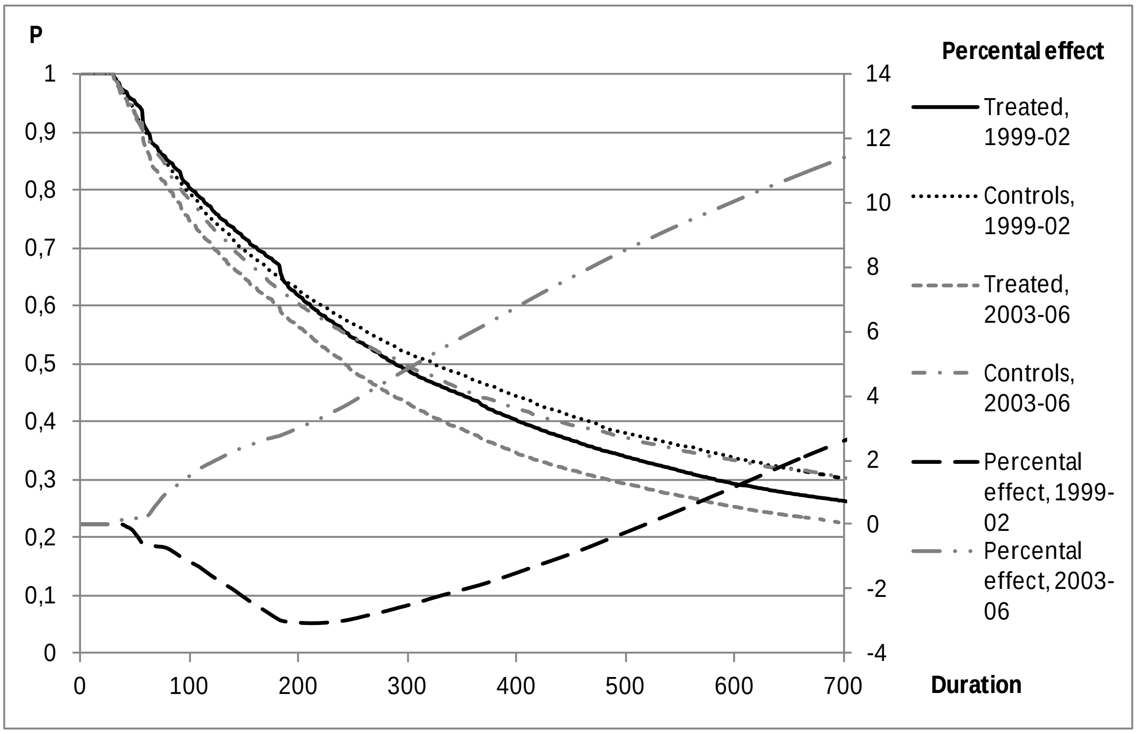

Figure 6: Estimated survival curves for participants and non-participants and estimated treatment effect. All matched participants, 20–60 years old, 1999–2002 and 2003–06. Censoring of controls at entry of any labour market programme

4.2.1.2 Censoring due to lost contact

Both participants and controls are censored if they leave the PES due to "contact lost". In our sample this happens to 9 % of the treated and 14 % of the controls. A number of studies have found that around 50 % leaving the PES in this way have actually found a job (Bring & Carling, 2000; Forslund et al, 2004; Nilsson, 2010; Gartell et al., 2012). The result of randomly assigning exit to work to 50 per cent of both treated and controls who are censored due to lost contact we get the results displayed in Figure 7. As was expected, this reduces the estimated effect significantly, from 7.6 % to 3.9 %.

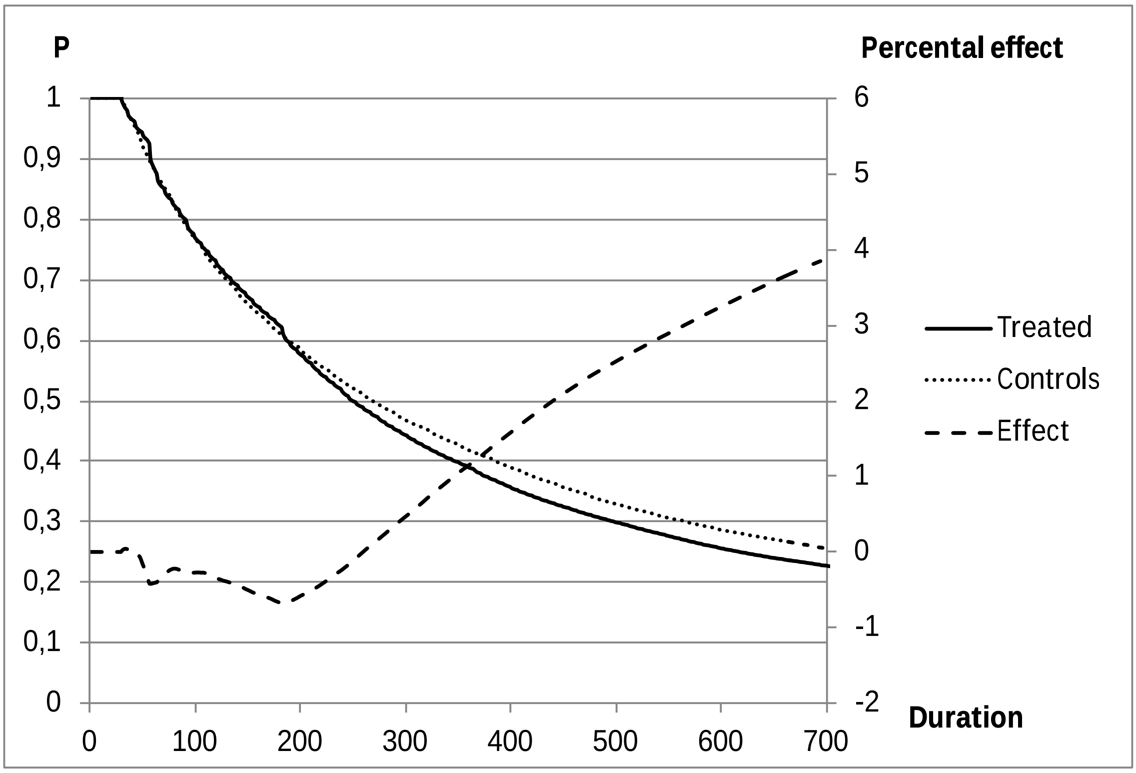

Figure 7: Estimated survival curves for participants and non-participants and estimated treatment effect. All matched participants, 20–60 years old, 1999–2006. Censoring of controls at entry of job practice, 50 % of exits due to lost contact randomly assigned to work

4.2.2 Sensitivity analysis

Although there is evidence that on average around 50 % of those with whom the PES has lost contact (LCs) actually have exited to work, there may be systematic differences, unknown to us, between participants and non-participants in this respect. As this destination is of a non-trivial size, the exact destination of LCs is important for the estimated programme effects – we saw that the estimated effect is reduced substantially if we randomly assign work as the destination for 50 % of both the treated and the controls leaving the PES for unknown reasons.

A somewhat extreme assumption, namely that among the LCs, 50 % of the controls and none of the participants go to work, could serve as a case giving us a lower bound for the treatment effect.[18] The results of this exercise are reproduced in Figure 8.

[18] This assumption admittedly is extreme. The only available direct evidence (Gartell et al., 2012) actually suggests that around 50 % of both participants and controls among th LCs go to work.

Evaluating the effect at a 700-day follow-up horizon, we find that the average effect for the whole period would be 0.4 %; for the period 1999–2002 the number is -4.1 % and for the period 2003–06 we get 6.2 %.

Once again the evidence shows that the estimated effect is indeed sensitive to the assumptions we make about the LCs. However, even under the extreme assumption that none of the treated and 50 % of the controls among the LCs exit to work, we find a positive treatment effect during the latter years of our sample period.

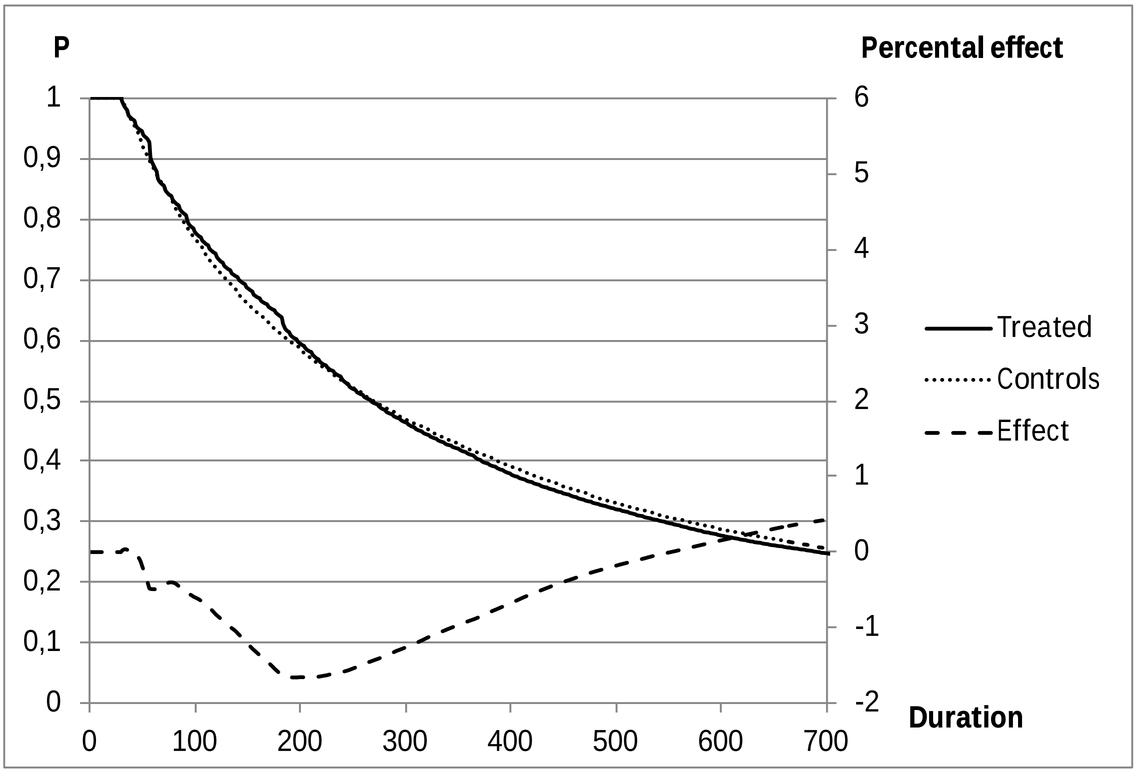

Figure 8: Estimated survival curves for participants and non-participants and estimated treatment effect. All matched participants, 20–60 years old, 1999–2006. Censoring of controls at entry of job practice, 50 % of exits due to lost contact randomly assigned to work, for the controls

4.2.3 Effects on outcomes other than the time to work

Time to work is a natural outcome to study if one is interested in the effect of a labour market programme. It is also a preferred outcome because the use of duration analysis enables us to censor controls that enter any programme. However, other outcomes also merit interest. One such outcome is the fraction of job seekers registered at the PES at different points in time after programme start. This outcome will capture flows to other destinations than work as well as flows back to unemployment.

In Figure 9 we show the proportions of participants and controls remaining registered at the PES at different points in time after programme starts. Over the 24 month period shown, the participants are registered at the PES to a larger extent than the controls. Compared to the results for exit to work, this implies that the participants either return to the PES more rapidly than the controls or that controls go to other exits than work at a more rapid rate than do the participants.

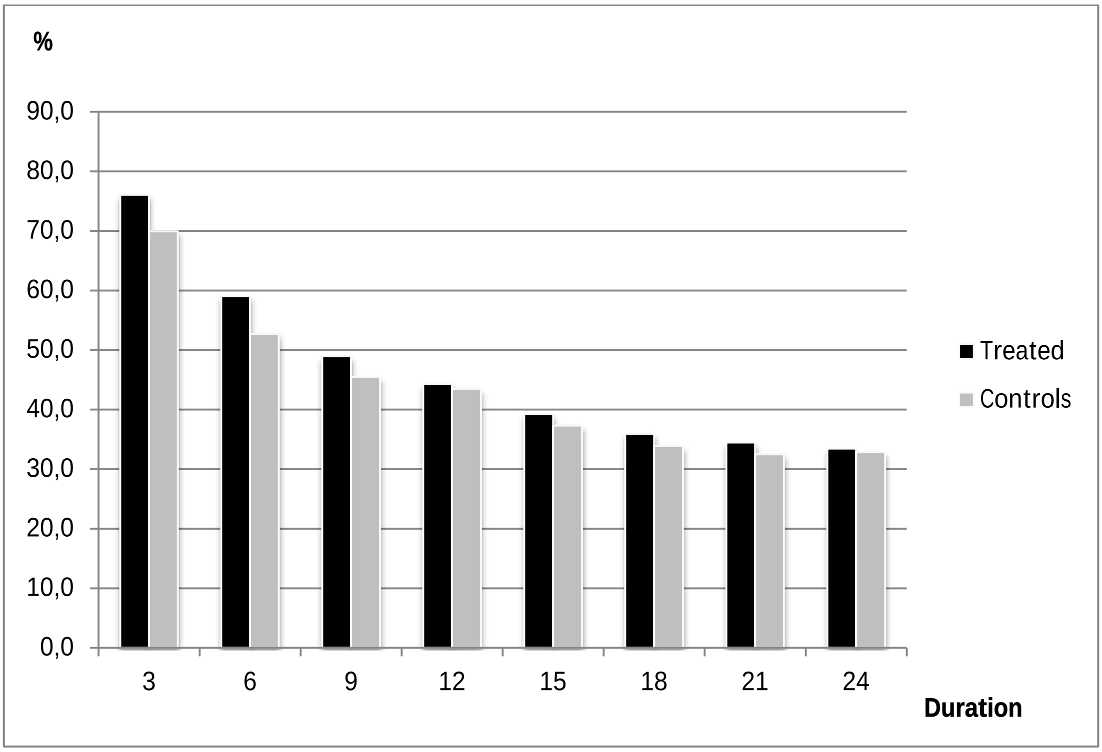

Figure 9: Proportions of participants and controls remaining registered at the PES at different points in time after programme starts, 1999–2006

We have already noticed that controls disappear from the PES for unknown reasons ("contact lost") to a larger extent than participants. This may account for some of the difference. Looking at other exits, we also see that controls exit to education to a larger extent than do participants. Hence, some of difference may be accounted for by controls exiting to other destinations than employment to a larger extent than the participants. Looking further at the data, we can also notice that a larger fraction of the exits are to temporary jobs or to employment by the hour among participants than among the controls. A higher fraction of temporary jobs may mean that participants flow back to the PES at a higher rate for this reason.

There are other outcomes of interest. One such is labour income. In Figure 10 we show annual labour income for participants and matched controls two years before and two years after programme start. There are no significant income differences the years just before programme start, while annual earnings of the participants are significantly higher during the two years after programme start.

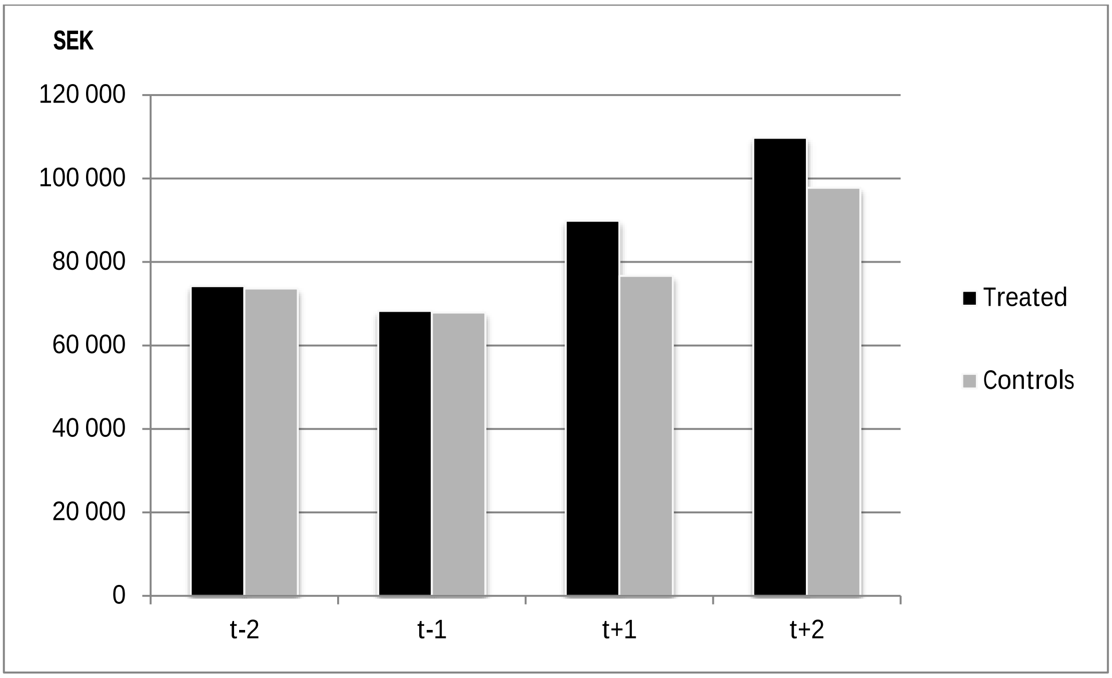

Figure 10: Annual labour income before and after programme start for participants and controls, 1999–2006

In Figure 11 the take-up of UI benefits and social assistance before and after programme participation among participants and controls are shown. Here results are less clear-cut, especially for UI benefits. For social benefit take-up, a simple difference-in-differences for participants and matched controls would indicate a positive treatment effect (reduction in take-up) amounting to .6 percentage points or 4 %. For UI benefit take-up, the sign of the effect depends on the follow-up horizon.

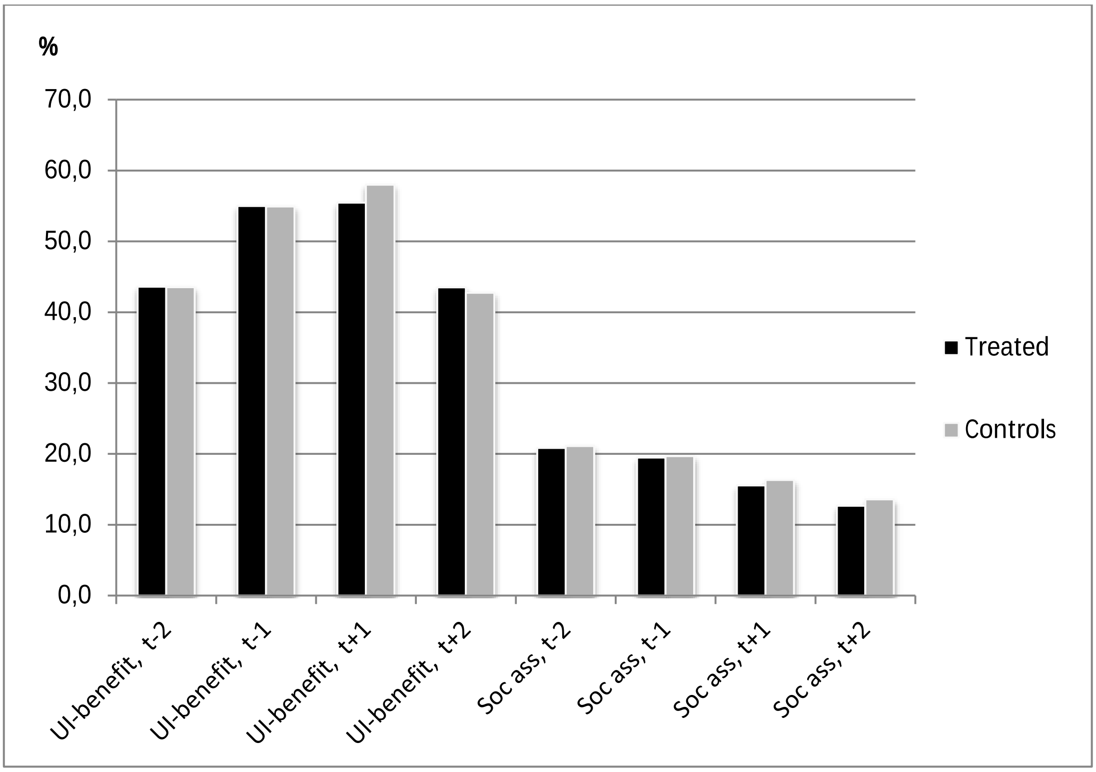

Figure 11: Take-up of UI benefits and social assistance before and after programme start among participants and controls, 1999–2006

Summing up the evidence for all matched participants, we find that participants find jobs faster, but are unemployed registered at the PES at different points in time after programme start to a larger extent than the controls. These findings do not primarily seem to reflect that participants find "bad jobs" and return to unemployment–participation is associated with higher future earnings and a lower take-up of social assistance. The most likely explanation rather is that controls exit for education to a larger extent than do the participants.

Looking at the time pattern of the estimated effects, participants seem to gain more in the later years of the period (2003–06) than the first years in our sample (1999–2002).[19]

4.3 Heterogeneous effects over observed characteristics and different types of practice?

We have seen that effects vary substantially between different years. Hence, one should use estimated treatment effects cautiously as guidance to what can be expected if, for example, work practice is given to larger numbers of unemployed–effects in all likelihood depend on a large number of factors in ways that are largely unknown.

[19] Only results for exit to jobs are shown, but a similar pattern is present also in terms of other outcomes.

Effects may also vary by types of participants or by types of practice. We have estimated treatment effects for a number of broad groups of participants as well as for practice in different occupations (SSYK 1-digit codes) and in different sectors (SNI 1-digit codes).

In Table 3 we present estimated treatment effects for groups of participants that are different in terms of some observed characteristics.

Table 3: Estimated treatment effects for different groups of participants

	1999-2006	1999-2002	2003-2006
Women	9,4	6,1	14,0
n	42 126	24 183	17 943
Men	3,8	-1,6	10,2
n	40 381	22 380	18 001
Non-Nordic	7,1	0,7	14,6
n	17 328	9 854	7 474
Disabled	5,5	2,4	10,0
n	7 886	4 741	3 145
Age 20-24	7,4	4,1	10,6
n	11 954	6 228	5 726
Age 25-49	6,9	0,6	14,6
n	57 444	32 963	24 481
Age 50-60	0,7	-6,3	9,3
n	8 957	5 172	3 785
No high school	7,0	0,7	14,3
N	18 931	11 532	7 399
High school	5,5	-1,3	14,1
N	45 211	27 027	18 184
n	*64 148*	*38 564*	*25 584*
More than high school	8,1	-2,1	15,2
n	18 508	8 104	10 404

First, we can notice that the general pattern that effects tend to improve towards the latter years of our period also holds for each of the sub groups considered in the table. For some of the groups, the estimated effect is actually negative in the years 1999–2002. Second, we see that there are some broad groups of participants that seem to have benefited significantly more than average from the programme (women and participants with more than high-school education) and some for which the estimated effects are significantly lower than average (men and, especially, participants 50 years of age and older). This pattern of effects does not readily lend itself to any interpretation of by which mechanisms the effects arise. Also, differences between groups are generally smaller than differences over time in estimated effects.

In Table 4 we present estimated effects for practice in different sectors (SNI 1–9) and occupations (SSYK 1–9). The period covered in these data is shorter (2001–06) and the number of participants is rather small in some of the cells, so there may be some doubts as to how well the matching actually works. In addition, the assumption of selection on observables makes less sense for these estimations – it is very restrictive to assume that we actually observe everything that is relevant for both participation and outcomes at this level. Hence, results should probably be interpreted more carefully than the other results presented.

Table 4: Estimated treatment effects by industry and occupation; entrants 2001–06

Industry (SNI)/Occupation (SSYK)	Number of matched participants (SNI)	Effects by industry (SNI)	Number of matched participants (SSYK)	Effects by occupation (SSYK)
1	787	22.9	604	-4.4
2	3 650	19.9	6 617	7.0
3	1 250	15.5	8 577	5.7
4	2 503	9.5	4 663	13.2
5	10 506	13.1	12 331	15.3
6	2 049	31.1	1 428	1.5
7	14 423	6.9	5 846	5.4
8	9 864	10.7	4 288	24.1
9	8 580	-3.4	10 667	1.8

Taken at face value, the estimated effects imply a lot of variation across industries (-3.4–31.1 per cent) as well as across occupations (-4.4–24.1 per cent). Examples of industries with high estimated effects are located in manufacturing, mining and transport, whereas exceptionally low returns seem to have occurred in societal and personal services and household activities. Looking instead at occupations, high estimated effects are found for process and machine operators, transport work, office work, customers' services, other services, care and sales. Particularly bleak results are found for occupations in managerial occupations. Once again, we find no easy way to rationalise these results.

4.4 (How) do effects vary over the duration of unemployment before programme start?

It is not evident what one should expect about how treatment effects vary (if at all) over the duration of the ongoing unemployment spell before programme start. On the one hand, early entry means lock-in with a higher probability–unemployed job seekers with high returns to job search will be over represented among those with short unemployment spells. On the other hand, it is a reasonable view that early treatment is better than "waiting" in unemployment for a treatment later in an unemployment spell, given that a treatment should occur at all. In Figure 12, we show estimated treatment effects for treatments occurring at different durations of ongoing unemployment spells.

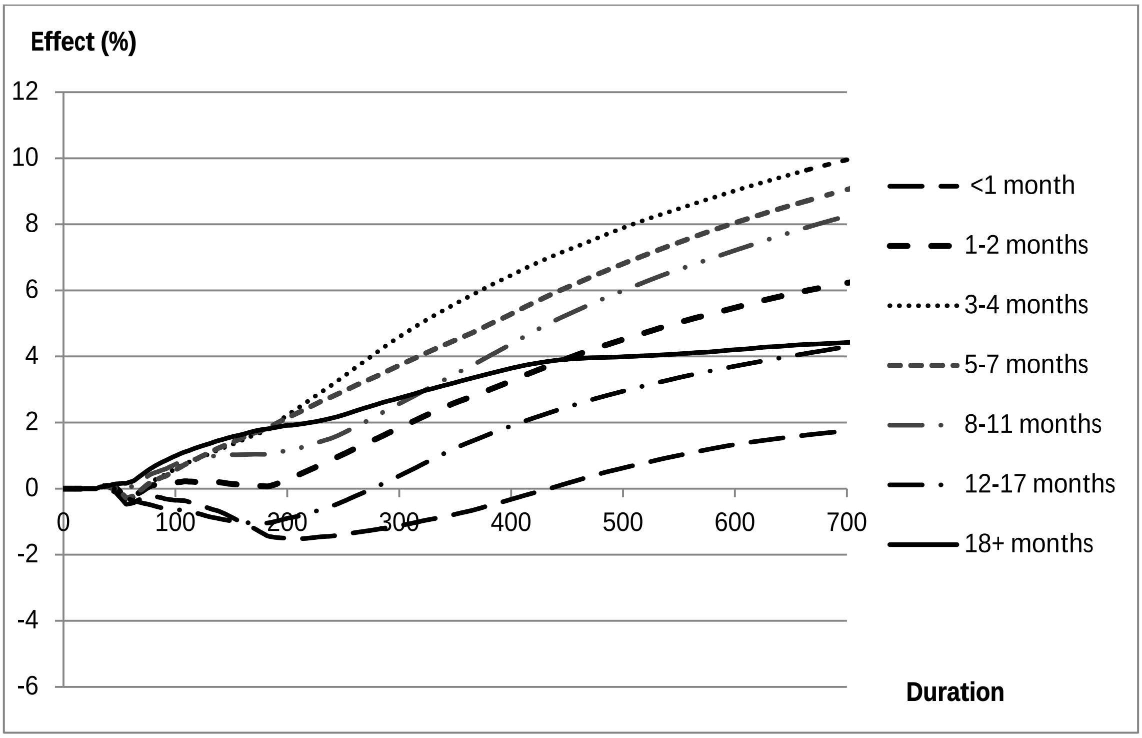

Figure 12: Estimated treatment effects for treatments occurring at different durations of ongoing unemployment spells, 1999–2006

First we can observe that there is no monotonous relationship between estimated treatment effects and the timing of treatment: the highest estimated treatment effect is for participants entering after between three and four months of unemployment; the lowest estimated effects occur for those entering the programme after at most one month of unemployment. The low estimated effect for these early entrants to a large extent reflects significantly more of locking-in, consistent with the idea that many early programme entrants actually would have found a job had they not entered the programme. Hence, care should be taken when selecting unemployed for early programme entry. However, other policy lessons regarding the timing of programme start are not obvious. Notice, for example, that the effects are good for those who entered the programme after 8–11 months of unemployment.

5 A comparison between job practice and labour market vocational training programmes

From a policy perspective, the question of “which programme?” is not obviously less interesting than the question “job practice or no programme?” An important alternative to job practice is vocational labour market training. We now turn to an analysis where we compare the treatment effects of job practice and training.[20] The analysis provides answers to two related but not identical questions. First, what was the effect for job practice participants of going to job practice rather than to vocational training? Second, what was the effect for vocational training participants of going to training rather than to job practice? With heterogeneous treatment effects, these two will generally not coincide, because they are effects for the treated, and the treated will possibly have different characteristics in the two programmes.

In Table 5 we show descriptive statistics for participants in the two programmes alongside with descriptives for all registered unemployed as well as for matched job practice participants (with vocational training participants as controls) and matched vocational training participants (with job practice participants as controls). There are differences between the programme participants (both programmes) and the stock of registered unemployed as well as differences between the matched participants and all participants. The most notable differences between the matched participants in the two programmes suggest that, on average, the training participants were closer to the labour market than the practice participants.

[20] Forslund et al. (2012) also compared job practice to training. Their focus was on how the relative efficiency of the two programmes varies over the business cycle, and they employed a parametric approach to estimate this. The main differences to the present analysis is, first, that we do not estimate effects in different phases of the cycle and, second, that we estimate the treatment effects non-parametrically.

Table 5: Descriptive statistics for registered unemployed, job practice participants, vocational training participants and matched participants 1999–2006

Variable	All registered unemployed at PES, stock sampling March & October	All spells with job practice	All spells with Employ-ment Traning	Matched spells with job practice (vs. Employ-ment training)	Matched spells with Employ-ment training (vs. job practice)
# of days in PES register before current spell	823,7	919,8	855,8	974,1	882,9
# days in ongoing spell at programme start	461,4	514,1	338,9	138,8	92,4
# days since last spell	503,0	385,4	411,0	731,2	720,2
# previous programme spells	1,9	2,4	2,3	2,5	2,2
# previous spells of subsidised employment	0,1	0,1	0,1	0,1	0,1
Age	39,1	38,8	35,8	35,2	35,1
Income (100s of SEK) year t-2	744,3	578,2	721,6	743,0	848,8
Income (100s of SEK) year t-1	671,1	448,2	678,5	693,7	931,7
Fraction (%)					
Males	45,6	46,2	43,4	52,9	52,0
High School	49,0	53,2	60,3	55,2	62,1
University, at most 2 years	4,8	4,4	4,1	5,3	3,9
University, at least 3 years	15,0	12,9	12,9	16,6	14,4
PhD	0,4	0,3	0,2	0,3	0,2
Disabled	14,7	14,3	11,7	9,4	8,6
Interlocal job seeker	18,4	23,0	21,4	20,5	18,7
Entitled to UI	80,3	87,3	84,8	87,5	87,2
Applying for full-time job	93,8	95,4	97,1	95,8	96,7
Married/partnership	33,2	32,4	30,3	30,2	31,0
Kids < 18 years	35,3	35,9	41,4	43,8	45,8
Married/partner/kids	39,6	39,7	34,6	40,5	41,8
Nordic (except Sweden)	3,5	3,4	3,4	2,6	3,2
Non-Nordic	21,8	21,0	22,1	20,1	19,0
Social assistance take-up year t-2	20,5	20,6	23,5	20,1	20,7
UI benefits year t-2	42,9	55,5	48,2	46,0	46,0
Social assistance take-up year t-1	20,8	20,6	22,7	18,7	17,9
UI benefits year t-1	50,6	66,3	60,4	57,7	54,9
# Observations	5 766 182	476 727	329 311	94 703	72 634

In Figure 13, we show estimated survival functions and the treatment effect of taking job practice for those who actually took job practice. For this group we find that after an initial period where vocational training participants are more "locked in", the treatment effect of training more than compensates for this, so that over a 700 days follow-up horizon the expected duration to a job is almost 6 percent shorter for the training participants. Given that training programmes last longer, and given the results in Forslund et al. (2012), these results are expected.

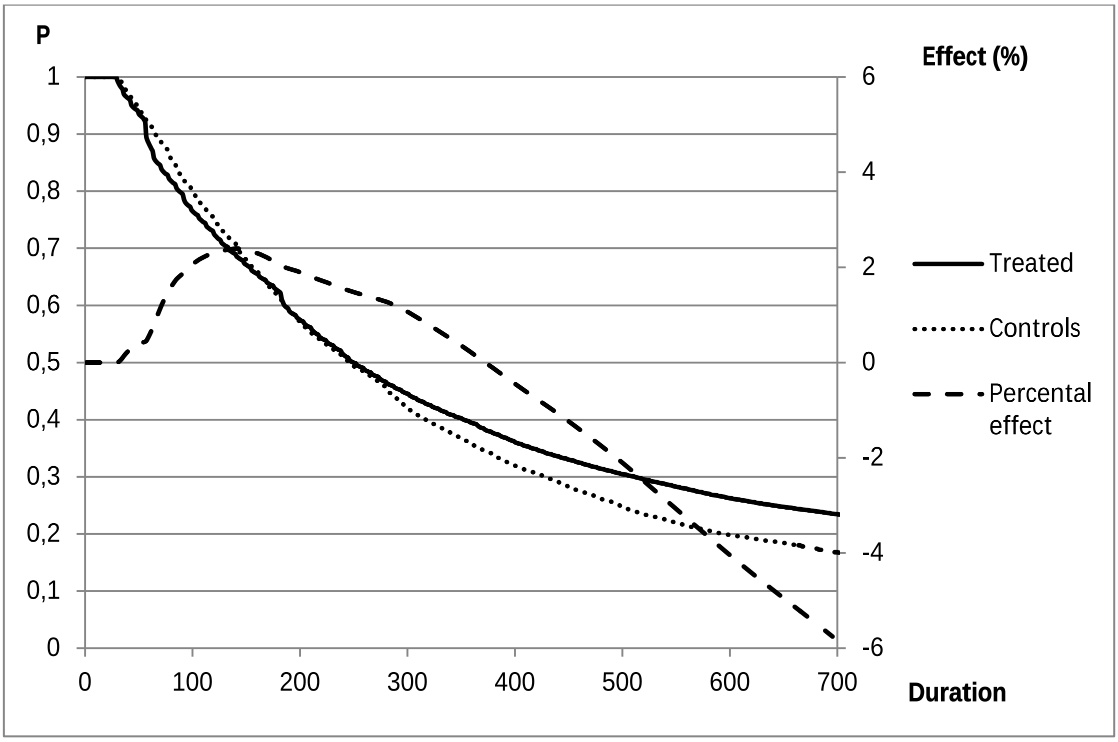

Figure 13: Estimated survival curves and treatment effect. Treated are job practice participants 1999–2006 and controls are matched participants in labour market training

In Figure 14 we instead consider the comparison for those who took vocational training programmes. For this group of participants the relative locking-in effect of training is larger and lasts much longer, so over a 700 days follow-up horizon, there is virtually no difference between the two programmes despite a larger treatment effect of the training programme.[21]

Interestingly enough, the results indicate that, relatively, those participating in practice would have been better off participating in training and that training participants would have been equally well off in practice. Hence, a reshuffling of participants between the programmes would have yielded better results according to the estimates. In a mechanical sense this reflects that participants in the two programmes have different observed characteristics. One possible explanation is that, according to the results in the present study, there were no large differences in effects for different groups. Forslund et al. (2008), on the other hand, found rather substantial differences in effects of training for different groups of participants, where the general pattern suggested that groups with "weak" observed characteristics gained the most from

[21] The estimated point effect indicates that training outperforms practice by less than 0.1 %.

training. In terms of observed characteristics participants in training seem to have at least as "strong" characteristics as the practice participants. Hence, a reshuffling of participants with "weak" characteristics to training could have improved the overall efficiency in programme place allocation.

Should we conclude from this analysis that it would also have been worthwhile to reallocate unemployed workers from practice to training by scaling down the first and scaling up the latter? Not necessarily. A complete analysis must also take costs into consideration. Forslund et al. (2011) report that in 2008, the direct cost of training was SEK 72 000 per participant. The excess benefits of training would have to exceed this amount in order for training to outperform practice in a cost-benefit analysis. By looking at effects on earnings, we can get a more direct answer.

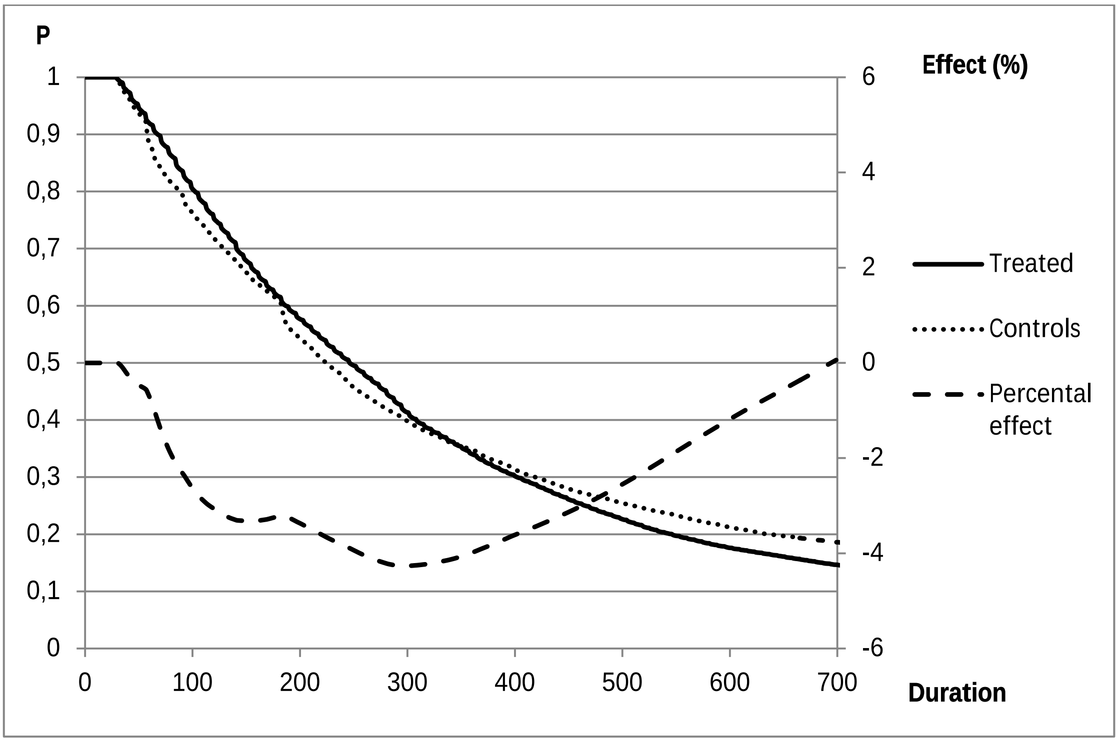

Figure 14: Estimated survival curves and treatment effect. Treated are labour market training participants 1999–2006 and controls are matched participants in job practice

So, how do the programmes compare in terms of earnings and other outcomes? In Figure 15 we show fractions of participants in the registers of the PES at different points in time after programme start for the two programmes, in Figure 16 we compare pre- and post-treatment labour incomes and in Figure 17 we show the fractions of participants receiving UI benefits and social assistance. All figures show the effects for job practice participants compared to matched vocational labour market training participants. The comparisons all indicate that job practice participants would have done better by instead going to vocational training.

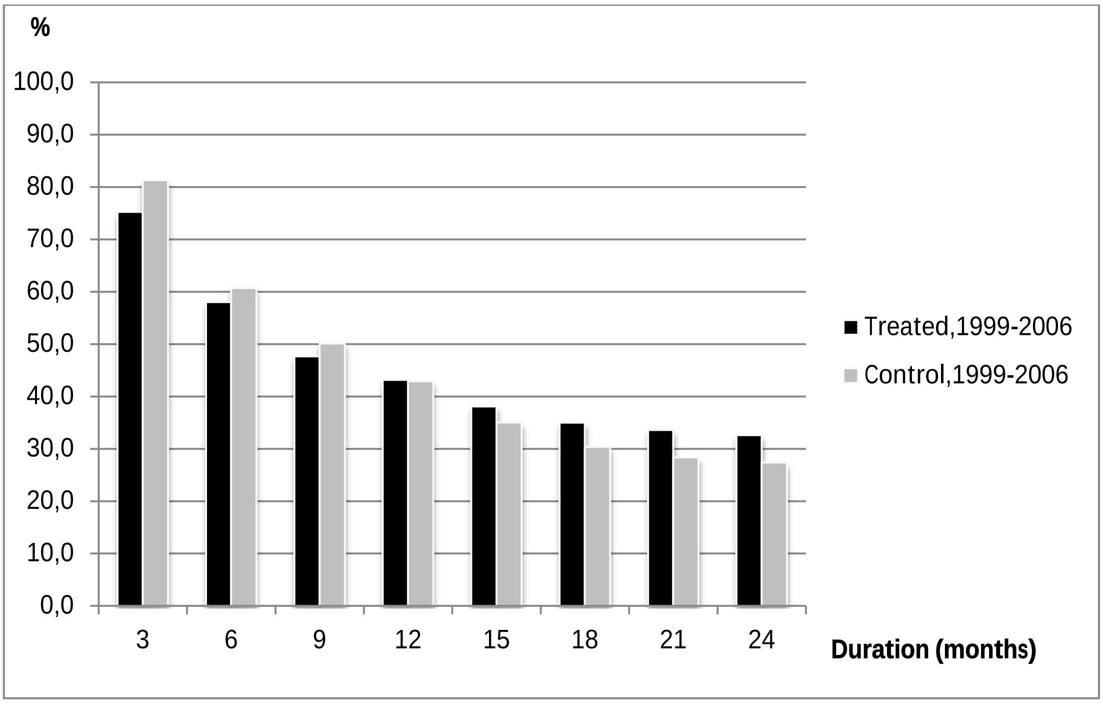

Figure 15: Proportions of treated (job practice) and controls (vocational training) registered at the PES at different points in time after programme starts, 1999–2006

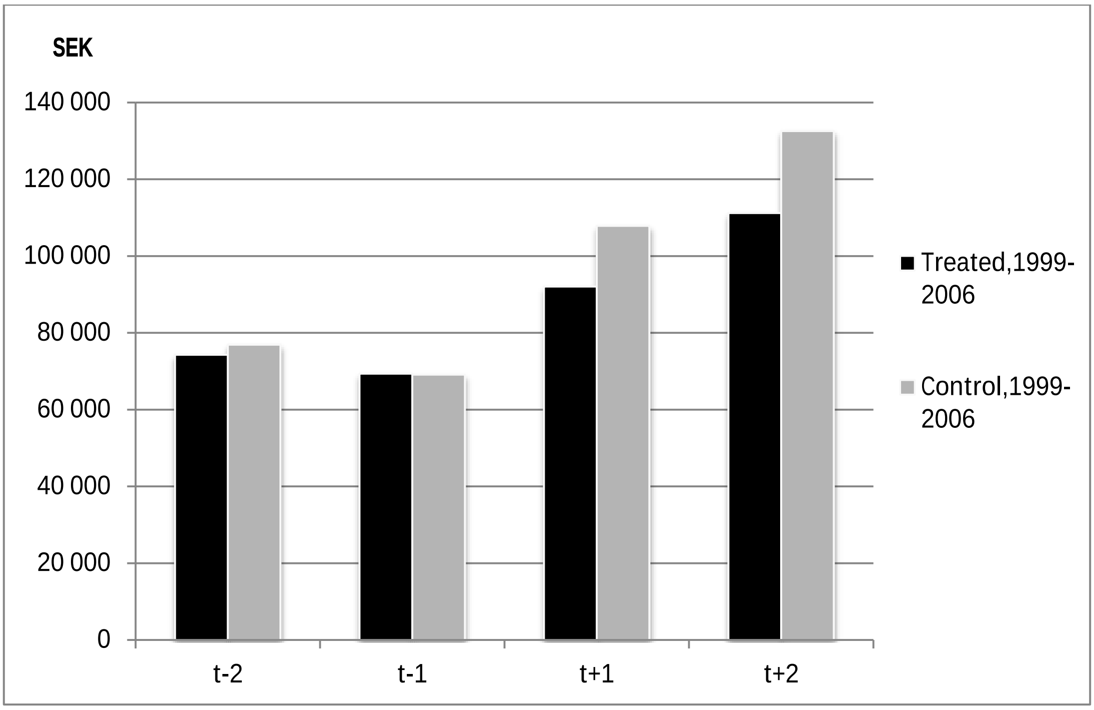

Figure 16: Annual labour income before and after programme start for treated (job practice) and controls (vocational training), 1999–2006

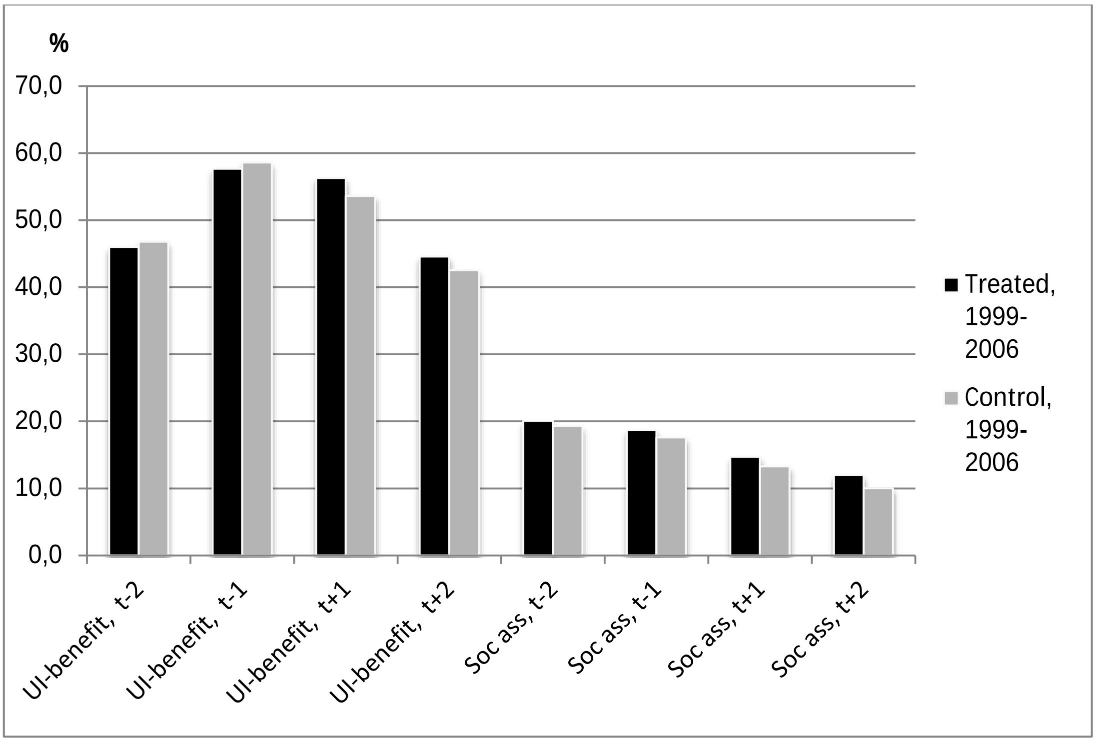

Figure 17: Take-up of UI benefits and social assistance before and after programme start among treated (job practice) and controls (vocational training), 1999–2006

In Figure 18, Figure 19 and Figure 20 we instead show how participants in training programmes would have done had they instead participated in job practice. Once again, in terms of all these outcomes training participants did better than practice participants.

We can now return to the question of whether training was so much better for the participants that it would actually pass a cost-benefit test. We have no definite answer, but we can notice that for practice participants earnings would have been about SEK 16 000 higher the year after programme start and about SEK 21 000 two years after programme start had they instead taken training programmes; for training participants earnings would have been SEK 10 000 and 14 000 lower one and two years after programme start, had they instead gone into practice programmes. Differences in these orders of magnitude would have to prevail for some years in order for training to outperform practice in a cost-benefit sense since the extra programme cost was just above SEK 70 000 in 2008.

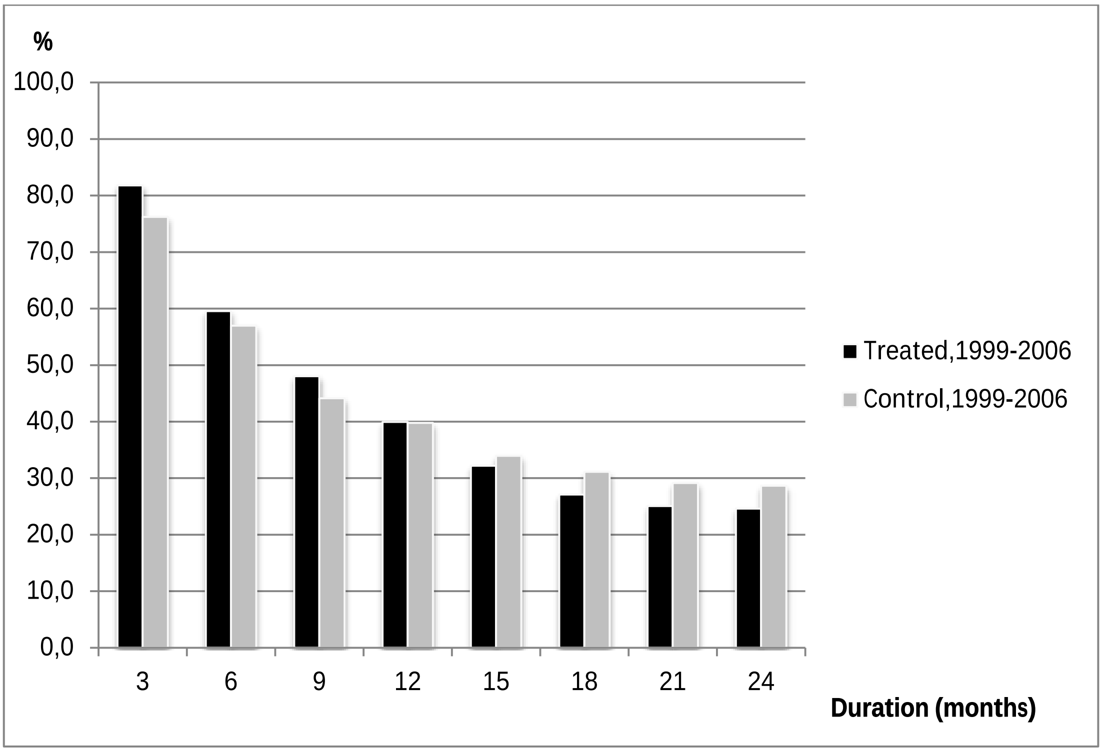

Figure 18: Proportions of treated (vocational training) and controls (job practice) registered at the PES at different points in time after programme starts, 1999–2006

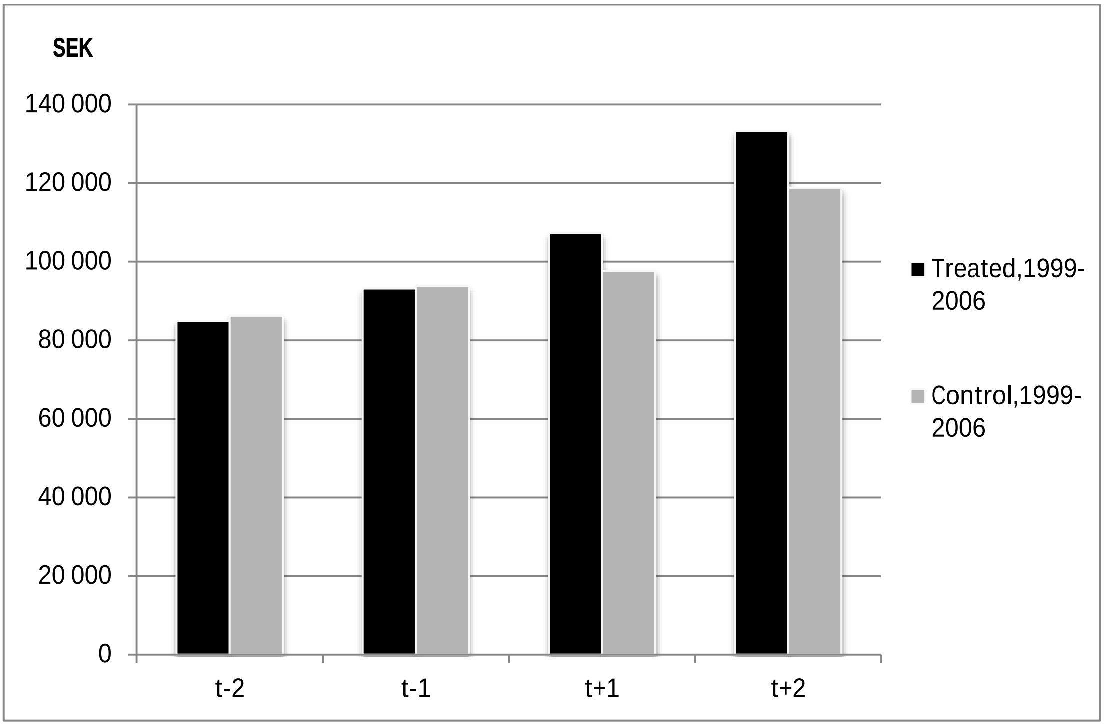

Figure 19: Annual labour income before and after programme start for treated (vocational training) and controls (job practice), 1999–2006

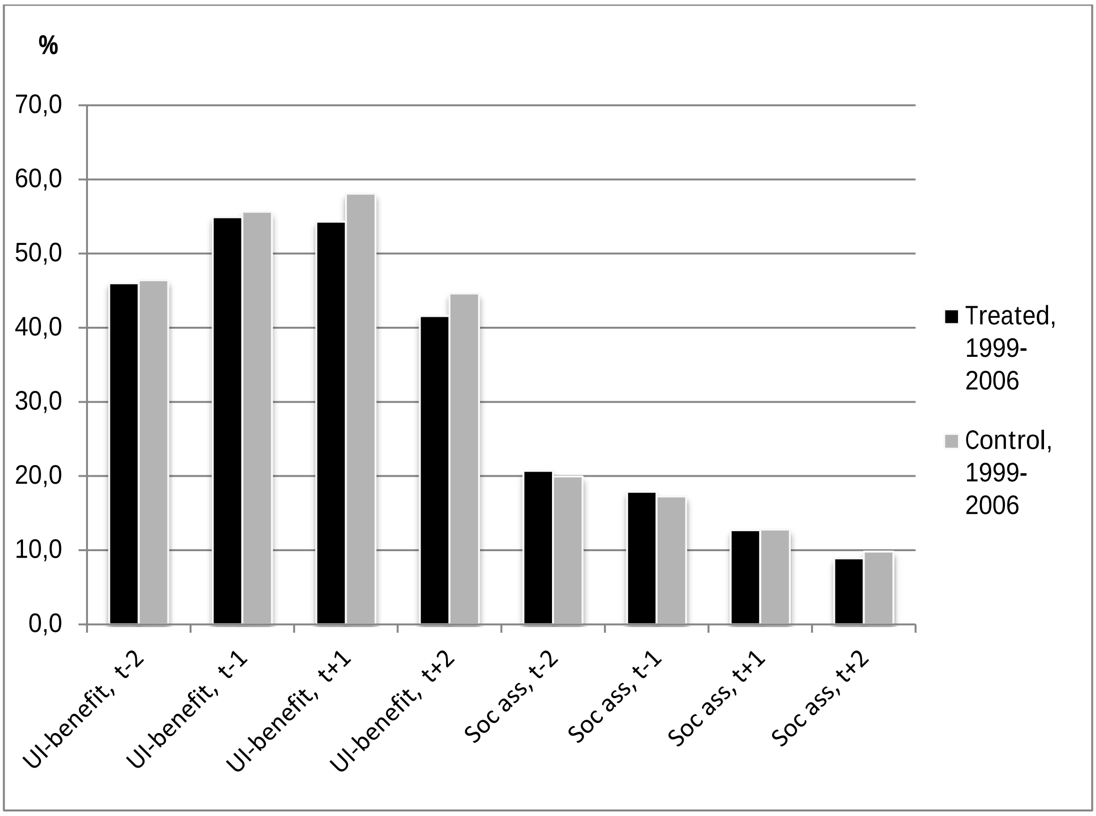

Figure 20: Take-up of UI benefits and social assistance before and after programme start among participants (vocational training) and controls (job practice), 1999–2006

6 Concluding comments

We have estimated the effects of job practice for those who entered the programme between 1999 and 2006. On average, the programme had a positive but not huge effect for the participants – the average expected duration to work for the unemployed was reduced by around 7 % over a two years follow-up horizon from programme start. Furthermore, participation also raised future labour income and reduced social assistance take-up. However, participants remained registered job seekers to a larger extent than non-participants. Most likely, this reflects that non-participants went to education at a higher rate. Finally, effects on unemployment benefit take-up were ambiguous.

The positive average effect hides heterogeneity in a number of dimensions (start date of programme, participant characteristics, sector of participation, occupation in practice, unemployment duration before participation) that we have investigated. The main message of this is that estimated average effects may not be very informative about the effects of expanding the programme – the effects will generally depend on target groups, timing, occupation and sector of practice and possibly interactions between all these, giving rise to treatment effects that will be largely unpredictable given the present state of knowledge.

When we compare job practice to vocational training programmes, we get the paradoxical result that those receiving job practice would have been better off instead going to vocational training, whereas those going to training would have benefited as much from job practice as from training. suggests that a reallocation of job practice participants to vocational training would have been beneficial. The finding does not, however, suggest that resources should have been transferred from job practice to vocational training programmes. The reason is that vocational training programmes are considerably more expensive that job practice. A simple cost-benefit analysis suggests that income effects of the sizes that we find would have to prevail for a number of years in order to make vocational training outperform job practice when also the cost side is accounted for.

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