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Employment subsidies—A fast lane from unemployment to work?*

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Abstract

The treatment effect of a Swedish employment subsidy is estimated using exact covariate-matching and instrumental variables methods. Our estimates suggest that the programme had a positive treatment effect for the participants.

We also show how non-parametric methods can be used to estimate the time profile of treatment effects as well as how to estimate the effect of entering the programme at different points in time in the unemployment spell.

Our main results are derived using matching methods. However, as a sensitivity test, we apply instrumental variables difference-in-difference methods. These estimates indicate that our matching results are robust.

JEL Classification: C14, C41, J23, J38, J68
Key words: Evaluation, employment subsidies, exact covariate-matching

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1 Introduction

Evaluations of Swedish active labour market programmes (ALMPs) in the 1990s have generally indicated rather disappointing results for the participants. However, a number of studies of Swedish ALMPs also suggest that programmes close to the regular labour market in the sense that they resemble ordinary jobs have fared better than other programmes in this respect.[1] Two recent surveys of labour market policies in a number of countries have reached rather conflicting conclusions about the relative efficiency of different active labour market programmes. Heckman et al. (1999, p. 2079) conclude, after surveying a large number of studies of European ALMPs, that they cannot "...conclude that any one active labour market policy consistently yields greater employment impact than any other." Martin & Grubb (2001, p. 31), on the other hand, conclude that "In several OECD countries, evaluations have found that these programmes [hiring subsidies paid to private employers] have a greater impact than public training programmes or direct job creation measures." The latter conclusion is more in line with the findings in recent Swedish studies. In this paper we study the treatment effects of employment subsidies, targeted at the long-term unemployed, used in Sweden from the beginning of January 1998.

An inherent difficulty in programme evaluations is the issue of selection. In this paper we use two distinct identification strategies. First, because we have rich enough data, we use matching estimators.[2] Second, to check the robustness from the matching estimators, we also identify the treatment effects using instrumental variable (IV) methods. As an instrument we use the fact that the employment subsidy (ES) programmes are financed in a way that does not impose any burden on the budget of the public employment service (PES) offices. When the National Labour Market Board faced budget cutbacks in 1999, this hit the counties differently; in some counties assignment to all programmes was reduced substantially for budget reasons, whereas other counties were affected much less.[3] We argue that this gave rise to variations in programme participation that were unrelated to the programme effects and, hence, can be used as an

[1] See, for example, the survey in Calmfors et al. (2001).

[2] In addition to a number of personal characteristics of eligibles, we can observe their unemployment histories since August 1991.

[3] The events are discussed in more detail in *Section 3.2*.

instrument. We have no good way to determine which (if any) of the identification strategies is the "correct" one. Hence, we use both, and to the extent that the results agree, we feel confident that they are reliable.

A number of previous studies of Swedish ALMPs have investigated the effects of recruitment subsidies. Recruitment subsidies were used between 1981 and 1997 and were similar to the employment subsidies we study. These studies generally indicate good results.[4] Carling & Richardson (2001) compared the effects of eight different programmes on the probability of finding a job and found that recruitment subsidies (possibly challenged by self-employment grants) was the best programme in this respect. Sianesi (2002) compared the effects of participating in six programmes. Recruitment subsidies was the only programme that gave a significantly higher probability to get and keep a job compared to looking for a job in open unemployment.

Carling & Richardson (2001) argue that their results probably do not reflect selection by showing that programme placement depended more on the public employment service (PES) office that the job seeker had visited than on her observed characteristics. If differences between PES offices were uncorrelated with non-observed characteristics of the job seekers, there should be no selection problem. Sianesi (2002) also assumed that she had a sufficient amount of information about the job seekers to be able to observe all factors that affect both programme placement and the result of programme participation for the analysed individuals.[5] Hence, both Carling & Richardson (2001) and Sianesi (2002) rely on that they by conditioning on a large number of observable characteristics of the job seekers or their location can estimate treatment effects without selection problems.

In this paper we primarily rely on the matching estimator suggested by Fredriksson & Johansson (2004). This estimator gives estimates not only of a treatment effect, it gives estimates also of the time profile of a treatment effect. In addition, we are able to estimate treatment effects for programme entry at different points in time (i.e., after unemployment

[4] A possible exception is Carling & Gustafson (1999), who found that persons employed with recruitment subsidies faced twice as high a risk to re-enter unemployment than persons who had received self-employment grants. Calmfors et al. (2001) surveys the studies comprehensively. In the present paper we only discuss the most recent studies.

[5] See also Sianesi (2001).

spells of different lengths). These estimates convey important information on the optimal timing of policies. We use the matching estimator because we argue that we, just like Carling & Richardson (2001) and Sianesi (2002), have rich enough information to make it plausible that we avoid selection on unobserved characteristics that also influence the outcome of the participation. However, unlike previous studies, we also estimate treatment effects with IV methods as a robustness check.

Our results are qualitatively similar to those in previous studies of similar Swedish programmes. Programme participation increased the flows into jobs compared to non-participation for the participants. This effect has a clear and intuitively reasonable time profile: during the first seven months after entering the programme the participants were "locked into" the programme. Thereafter the probability to find a job was larger with than without the employment subsidy. If we sum the effects over all participants we find that the time in unemployment decreased by about 8 months. This approximately corresponds to an effect of about 8 %. These results do not seem to reflect selection on unobservables—our IV estimates are very similar to the matching estimates. Regarding the timing of the programme, it seems to have been of minor importance: qualitatively, the effects for those entering just after programme eligibility (after 12 months of unemployment) were very similar to the effects for those entering three years later in the unemployment spell.

2 The employment subsidy programmes

2.1 The programmes

As of January 1, 1998, an *individual employment subsidy* replaced the former programmes relief work, recruitment subsidies and trainee replacement schemes.[6] The employment subsidy was targeted at the long-term unemployed, i.e., persons at least 20 years old and registered as unemployed at the PES for at least 12 months.[7] The subsidy, 50 % of total

[6]For a more thorough description of the employment subsidy programmes, see Lundin (2000, 2001).

[7]The subsidy could, in exceptional cases, be paid for persons who were not long-term unemployed, provided that they had participated in other programmes or had been temporarily employed. An exception was also made for youth under age 25. For this group 90 days of unemployment was the eligibility criterion.

wage costs, was paid for a maximum period of 6 months.[8]

In October 1999, the individual employment subsidy was abandoned and replaced by two programmes: a *general employment subsidy* and an *extended employment subsidy*. The set of rules and regulations for the general employment subsidy was the same as for the previous individual employment subsidy. The extended employment subsidy was both much more generous and more strictly targeted at the long-term unemployed: the subsidy level was 75 % of total wage costs for 6 months and then 25 % for another 18 months.[9] The eligibility requirement was fixed at being registered at the PES as unemployed or programme participant for at least 36 months. This requirement was changed to 24 months in January 2000.[10]

Another novelty introduced in October 1999 was that the subsidies were no longer paid out as grants to the firms. Instead, the employer received a tax reduction. The most important consequence of this for our IV estimations is that the subsidies no longer were paid out of the budget of the local PES office.

2.2 The programme assignment process

The selection to the ES programme involves at least three sets of agents: caseworkers, the (eligible) unemployed workers and the firms. The matching approach presumes that these agents do not possess *and* use information (related to programme outcomes) that is unobserved by us.

A first question relates to which of the agents has most influence over the programme assignment decision.

In a trivial sense, the employers always have an influence over the programme assignment in the sense that they can veto any suggestion

[8] The subsidy was also capped at SEK 350 (approximately € 35) per day and could be extended to 12 months in some exceptional cases.

[9] The maximum daily amount was set at SEK 525 for the first 6 months and then SEK 175 for the following 18 months.

[10] Since August 2000 there are two other forms of employment subsidies in operation. There is a *special employment subsidy*, targeted at persons above age 56, and another form of the extended employment subsidy targeted at those registered at the PES for at least 48 months. We will look at neither of these in the present study. However, in the empirical analysis we will treat persons entering extended and the two special employment subsidy programmes as censored from the point in time they enter the programmes.

from a caseworker. The caseworkers have the same position *vis a vis* the unemployed and the employers.

Who is the active party? In a survey to caseworkers at the PES, Lundin (2000) asked a question about who normally takes the first initiative to assignment to general ES. According to these (269) caseworkers, the two main agents were the PES and the firms. The former normally took the first initiative according to 55 % of the caseworkers whereas the firms were the prime movers according to 33 % of the respondents. Only 6 % of the caseworkers replied that the unemployed worker normally took the first initiative. Similar results were found by Johansson (1999), also asking caseworkers the same question about another programme (trainee replacement schemes) involving assigning unemployed workers to ordinary jobs. The results in Harkman (2002) indicate that unemployed workers were largely indifferent between different programmes.[11] The results in Harkman (2002) also indicate that the will to participate was driven at least as much by other considerations (such as social reasons and the possibility to renew UI benefit eligibility) as by expectations of an increased employability.[12] Furthermore, some 20 % of the respondents stated that they participated in a given programme because they had been recommended by a caseworker to do so.

Altogether, the evidence on the programme assignment process presented above indicates that we need not worry so much about private information among the potential programme participants. Caseworkers and firms seem to be more important.

The evidence in Harkman (2002) suggests that caseworkers have fairly strong views on the appropriateness of different programmes.[13] However, the results in Eriksson (1997), where several caseworkers assessed the suitability for labour market training of a number of persons, show that the (usually unobserved, but in her study observed) heterogeneity of the caseworkers was more important than the unobserved (in her study captured

[11]The unemployed were asked about their interest to participate in 5 different programmes (not including ES). The pairwise correlations between the different programmes fell between 0.4 and 0.6.

[12]The share of those who entered a programme because of expectations of an increased employability was falling with longer pre-programme unemployment duration. This points to the importance of controlling for previous unemployment experiences among the unemployed.

[13]In contrast to what was the case for the unemployed, the pairwise correlations between the ranking of the different programmes were generally low.

by fixed effects) heterogeneity of the unemployed. Carling & Richardson (2001) analysed the relative influence of the characteristics of the unemployed and the PES office identity on the allocation of unemployed between two programmes, conditional on entering one of them. The main result is that office identity is far more important than the characteristics of the unemployed in determining which programme the unemployed will enter. Hence, even though there is reason to believe that caseworkers observe more than is in our data, they seem to develop decision rules that may be correlated within but not between PES offices. Controlling for PES office identity (which is observed in our data) then would make the study less susceptible to selection bias.[14]

How about the influence of the firms? The answers to a survey question to ES participants in Lundin (2001) indicate that 22 % of the ES participants had previous work experience in the firm where they were employed with the ES. Of these, 18 percentage points performed the same tasks in the firm both with and without the employment subsidy. Furthermore, around 70 %, of these 22 %, had their latest employment spell in the firm less than two years prior to the survey. Moreover, Omarsson (2000) and Jansson (1999) show that almost 50 % of the registered unemployed who found a job in the mid 1990s were rehired by a previous employer.[15] Hence, a substantial fraction of the ES participants were known to the employers, who potentially may have acted on prior information (unobserved by us) that may be related to the participants' future employment prospects. We believe, however, that we are likely to capture this in the previous unemployment history of the eligibles.

Finally, evidence in Lundin (2001) suggests that not all ES participants viewed their subsidised employment as a permanent solution—more than 30 % of them responded that they had applied for at least one job while on the subsidised job. This may also suggest that selection on unobserved characteristics is not that serious a problem. In a study of the competition between employed and unemployed job applicants Eriksson & Lagerström (2004) control for all characteristics of the applicants observed by the employers and find strong evidence that firms view employment status as an important signal for productivity. Hence, it is imperative to condition on past unemployment: not only should one control for unobserved hetero-

[14] In our matching estimates, we control for the local labour market, not the municipality; in the IV estimates, we control for municipality.

[15] This fraction was, however, falling in unemployment duration (Jansson 1999).

geneity per se, past unemployment seems also to be an (observed) signal to the employer.

2.3 Programme participation

Figures 1 and 2 show the monthly development of participation in the employment subsidy schemes between January 1998 and September 2002.

In *Figure 1* total participation, both for those eligible for the general ES and total participation, in all ES schemes is displayed. Total participation includes all persons who, according to the registers of the National Labour Market Board, were registered in any of the programmes. The eligible participants are those who, according to the criteria given in *Section 2.1*, were eligible for the general ES programme and, in addition, were above age 25.[16]

The development in 1999 in *Figure 1* is of special interest for our IV estimations. First, as the budget cutbacks were imposed in April and May, programme participation dropped. In fact, the timing was such that the most affected counties imposed a general "placement stop" (PS) at different points in time between July and September.[17] The most rapid drop in programme participation occurred between June and July. Although there seems to be a seasonal pattern in participation with a drop between June and July in all the years in our data, the drop in 1999 is larger than in the other years. Then, although we can see a slight increase in participation already in September 1999, there is a marked increase in both series late in 1999, consistent with the idea that the combination of budget cutbacks and the financing of the ES programmes would give the PES offices an incentive to increase placement in the subsidy programmes.

Looking instead at *Figure 2*, we see how participation in the individual ES which was financed in the usual way) fades out during the second

[16]The most important criterion imposed to define eligibility is an uninterrupted register spell at the PES lasting for at least 12 months. The difference between the two curves, by definition, reflects that a substantial fraction of the participants either were not eligible according to our strict criteria or were below age 25. The exact procedure that we used to construct our data set is described in *Section 4*.

[17]This affected participation in all programmes, but it is reasonable to assume that expensive programmes would be more affected than cheap programmes, such as the ES programmes. It is also clear from our data that the phrase "placement stop" should not be taken completely at face value and that programme participation started to decrease already before the "formal" placement stop was imposed.

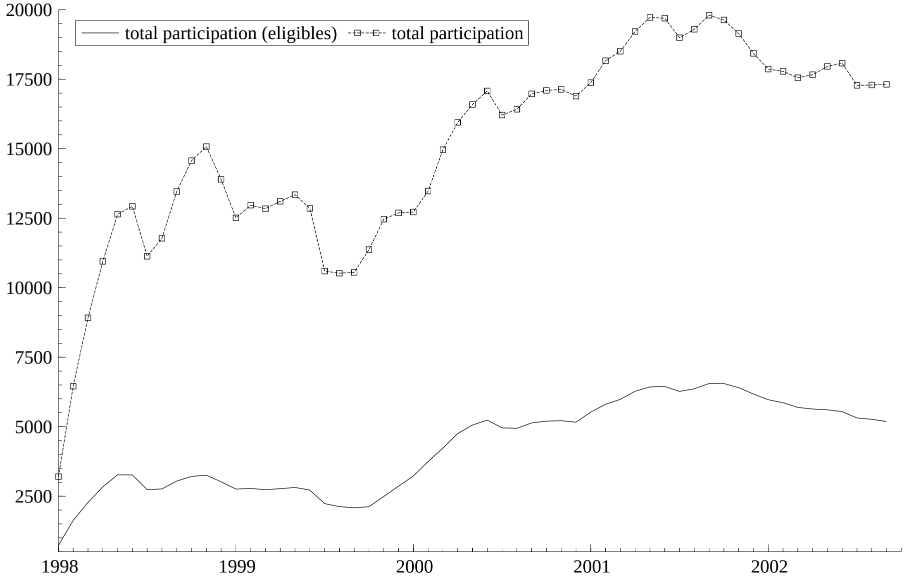

Figure 1: Participation in employment subsidy schemes 1998:1–2002:9 (number of persons)
Source: Own computations using information from the Händel data base

half of 1999. But we also see a sharp increase in participation in the general and (especially) the extended employment subsidies beginning in October 1999, when they were introduced. We use only information on participation in individual and 'general ES in our analysis. However, it is clear that we must take the emergence of the other employment subsidy schemes into account in our analysis, as a sub group of those individuals eligible for the general ES were also eligible for other ES schemes. We do this by treating them as programme eligibles as long as they do not enter any of the ES schemes. If they enter any of the other schemes, we censor them from this point in time. This procedure is justified if anticipations of future participation in any of the other ES schemes do not change their job-seeking behaviour.[18]

Figure 3 plots the survival functions (from eligibility, i.e., after 12

[18] See the discussion in Richardson & van den Berg (2001).

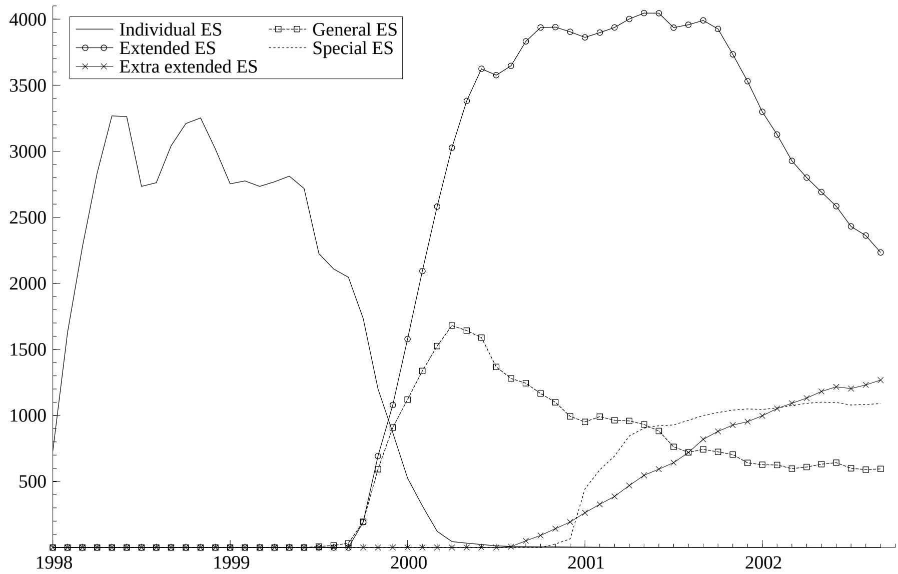

Figure 2: Participation in different employment subsidy schemes 1998:1–2002:9
Source: Own computations using information from the Händel data base

months of open unemployment) for participants and eligible non-participants. Initially, the non-participants leave unemployment faster than the participants. This would be consistent with the idea that participation initially "locks in" the participants. Then, after some initial 20 months, the fraction of participants remaining without a regular job becomes significantly lower than the corresponding fraction of non-participants. However, none of these "effects" should be given a causal interpretation. The "success" of the non-participants in the initial periods may, at least partly, be that non-participants become non-participants precisely because they find jobs.[19] The "success" of the participants later on may partly reflect the fact that they, on average, entered the register at an earlier date (see *Table 2*), and, hence, have had a longer period of time to find a job so that they to a smaller degree tend to become censored (censoring is in

[19]See the discussion in Fredriksson & Johansson (2004).

October 2002) and because the participants also absent the program have better job opportunities. We also see that most eligibles have entered the programme after some 80 months of unemployment and that most enter the programme relatively early.

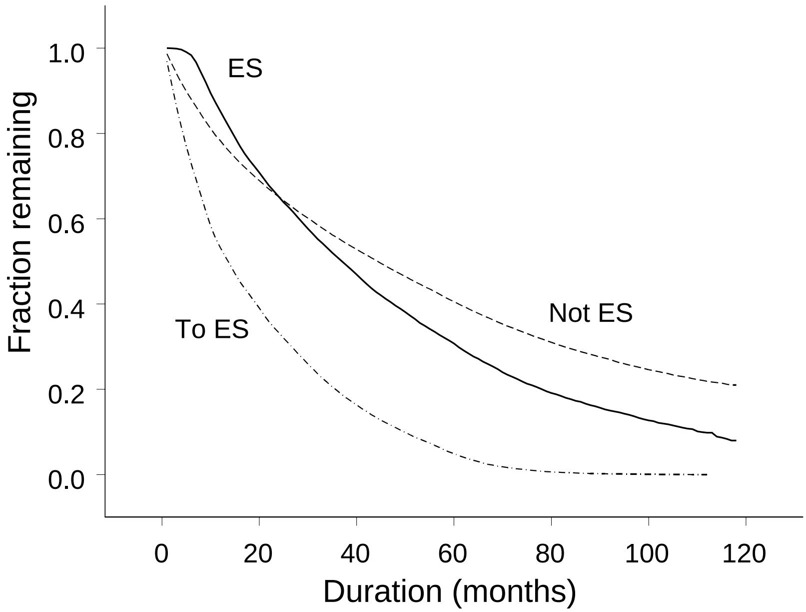

Figure 3: Survival functions from eligibility for participants (ES) and non-participants (Not ES). Risk is employment. Survival function from eligibility for participants (To ES). Risk is ES.

3 Identification

To estimate programme effects in this dynamic setting, we use two different identification strategies. Our main approach is (exact) covariate-matching. Here the idea is that if we can observe (and account for) all factors that jointly determine participation and the outcome of participation, then in practice we have a randomised experiment where we can compare the outcome of participants and matched non-participants to estimate the treatment effect. The critical assumption is that we actually observe everything of importance.

Our other approach is to exploit variations in programme participation caused by the budget cut-backs in April/May 1999 and the change to funding of the ES schemes through tax reductions in October 1999. This variation should be unrelated to the programme effects and gives us yet another way to take care of selection into the ES programme.

The two identification strategies are described more thoroughly in sections 3.1 and 3.2.

3.1 Matching

When determining the treatment effect of programme participation, the fundamental problem can be described as one of missing data. We can never observe a person as both participant and non-participant in a certain programme during a certain time period. Hence, we must always compare participants with non-participants. In controlled experiments programme treatment is randomised across those in the stock of eligibles for the programme. In such a setting there is no reason to believe that there are systematic differences between participants and non-participants. An average treatment effect can therefore easily be estimated through a comparison of the means of the two groups.

In the absence of controlled experiments there are good reasons to believe that participation is related to characteristics of the individuals that are also related to the outcome. Matching aims at "creating" an experimental situation by comparing persons who are so similar that participation actually becomes random. This identifying assumption is not testable, but obviously the identification strategy demands a lot of the data—unless we have rich information about the individuals, the chances are small that we actually observe everything of importance both for participation and the results of participation. However, we have rather rich information about those eligible for the employment subsidy scheme. In particular, we observe their unemployment experiences since August 1991.[20] This variable arguably picks up many factors that otherwise would be hard to

[20] As pointed out on page 8, we also observe the municipality of the unemployed. The data are described in *Section 4*.

observe.

In the matching we follow Fredriksson & Johansson (2004) and estimate the difference in survival rates at duration t from treatment (i.e. when entering into ES) for an "average" treated individual if in ES instead of not being in ES. The details of the matching are presented in Fredriksson & Johansson (2004).

3.2 Instrumental variables identification

There is geographical variation and variation over time in the probability of being assigned to ES. This variation in itself, however, cannot be used directly as it may be related to other factors (e.g., the business cycle, the labour market situation). Therefore we compare employment offices in the same local labour market, where in some ES was stopped and in others not. We could simply use this placement stop within the same local labour market as an instrument. We do not do so, because we worry over the monotonicity condition required for IV identification. Instead we use the introduction of a tax reduction (TR, see below), which should give a bigger increase in treatment probability in the programme-stop (PS) than in the non-PS regions. Essentially, we compare the PS with the non-PS regions, but in a difference-in-difference estimator we exploit not the differences in treatment probability between these two regions but rather the increase in treatment probability after the TR.

Hence, we use regional and time-series variations in programme participation induced by responses to changes programme financing as an alternative way to identify the treatment effects of the ES programmes. Below we describe the events in some detail.

The budget of the National Labour Market Board is laid out annually in the budget bill. The fiscal year coincides with the calendar year. The government decides on the allocation of funds at the turn of the year. The National Labour Market Board allocates funds to the regional (county) labour market authorities, which in turn allocate funds to the local PES offices.[21]

During 1999 the budget for the National Labour Market Board was considered tight by officials at the Board. On top of this, in April, the government withdrew funds. Changes in the allocation of funds are primarily undertaken as a result of changed needs of the number of programme

[21] Those funds should cover the expenses of the PES office for the whole year.

slots. The changes in 1999 were primarily caused by revised business cycle forecasts (Government Bill 98/99:100 1999, p 40). These cutbacks were distributed according to the size of county budgets. The consequences, however, were unevenly distributed depending on how much of the funds had already been used. In some counties programme placement was cut substantially, in some counties they were cut heavily (placement stop (PS), "beslutsstopp") and some counties, finally, were only mildly affected.[22]

We pointed out in *Section 2.1* that a feature of the ES programmes was that, from October 1999, they were given in the form of a tax reduction and, hence, did not impose any cost on the local PES offices. Our point of departure, finding support in the answers to our questionnaire to financial managers at the county labour market authorities, is that this design of the subsidy schemes would give local PES offices different incentives to expand the subsidy programmes depending on whether or not they were located in a county that was severely hit by the budget cutbacks. If so, then the hazard into the subsidy schemes would depend on the location of the unemployed job seeker: we would, *ceteris paribus*, expect a more rapid flow into employment subsidy programmes in counties severely affected by the budget cutbacks (i.e., in the PS counties). However, it may well have been the case that the PS counties were severely affected by the budget cutbacks because of bad conditions in the labour market.[23] Such conditions would also imply a lower job-finding probability, in which case the events would seem not to provide us with a good instrument for evaluating the impact on employment of ES participation.

However, local labour markets, defined by Statistics Sweden based on commuting patterns, extend over county borders.[24] Our questionnaire revealed that, in fact, there were 69 municipalities (with local PES offices) located in different counties, hit differently by the budget cutbacks, but within the same local labour market (totally 8 different local labour markets).[25] Hence, in these 8 local labour markets the new mode of financing

[22]We have distributed a questionnaire to the financial managers of the regional labour market authorities to establish how they reacted to the cutbacks. See also the discussion in *Section 2.3.*

[23]A bad state in the labour market could have induced large expenditures on labour market programmes early in 1999, which in turn created a need for a more restrictive programme placement policy after the budget cutbacks.

[24]In principle, local labour markets are defined so as to minimise commuting over the market borders.

[25]The municipalities are displayed on a map in Appendix C.

the ES programme is a potentially good instrument in the sense that it can be expected to have affected programme participation without at the same time affecting the outcome conditional on whether a municipality was or was not in a PS county.

The events in 1999 would seem to suggest a second natural way to identify treatment effects: we could use the budget cutbacks in April as an instrument for participation in ES. Notice, however, that this does not necessarily work as an instrument because the expected effect of the cutbacks on ES participation is ambiguous. On the one hand, the cutbacks implied generally lower programme volumes. On the other hand, we would expect the most expensive programmes to be the ones most affected. Hence, it is likely that there would have been a substitution from expensive programmes (i.e., training) to cheaper programmes, such as ES. The net effect is *à priori* ambiguous. More importantly for the IV strategy to work: we cannot be sure that the sign of the effect is the same in all 69 municipalities.

We use the placement stop (PS) and tax reduction (TR) to estimate the treatment effect of the employment subsidy (ES) with three different IV estimators. The logic is straight forward: if the programme actually speeded up the flow to employment and if the tax reduction (exogenously) pushed more persons into the programme in the PS municipalities (as compared to municipalities in the same local labour markets), then the flow into employment must have increased after October 1999. Some technicalities of the estimations are described in *Appendix B*.

3.2.1 The difference in difference idea

We use three different IV estimators, which basically differ in the functional form assumptions made. For all three estimators the principle of identification is the following: Let $Y_i(0)$ and $Y_i(1)$ be potential employment if joining the ES program ($D_i = 1$) and not ($D_i = 0$). We observe $Y_i(0)$ for the non participants and $Y_i(1)$ for the participants. The observed employment then equals

$$\begin{aligned} Y_i &= Y_i(0) + D_i(Y_i(1) - Y_i(0)) \\ &= \beta_0 + D_i\beta_i + \varepsilon_i, \end{aligned}$$

where $\varepsilon_i = Y_i(0) - \beta_0$. D_i is determined by the increased placement in ES in the PS municipalities due to the tax reduction, Z_i, as

$$\begin{aligned} D_i &= D_i(0) + Z_i(D_i(1) - D_i(0)) + \eta_i \\ &= D_i(0) + Z_i\alpha_i, \end{aligned}$$

where $D_i(0)$ is treatment taken if $Z_i = 0$ and $D_i(1)$ is the treatment taken when $Z_i = 1$.

Under the assumptions

1. Potential outcomes and treatments are independent of Z_i, i.e.

$$Y_i(0), Y_i(1), D_i(0), D_i(1) \perp\!\!\!\perp Z_i,$$[26]

2. Monotonicity ($\alpha_i > 0$, i.e., there exist no defiers)

the local average treatment effect (LATE) is defined as

$$\delta = \frac{E(Y_i|Z_i = 1) - E(Y_i|Z_i = 0)}{E(D_i|Z_i = 1) - ED_i|Z_i = 0)}$$

Here "|" denotes "conditional on". LATE is the treatment effect of those affected (the compliers) by the instrument, Z_i. If the individuals do not select into ES on relative advantages, δ is the treatment effect for a randomly drawn unemployed.

The above presentation of a treatment effect estimand is quite standard, see Angrist & Krueger (1999). For our application, however, we need to consider both the timing of treatment and the timing of employment. In addition, we also need to condition on the tax reduction (TR) and placement stop (PS) municipality for Z_i to be a valid instrument. Let τ_i^p and τ_i^t be the (fix time) eligibility duration to the placement stop and to the tax reduction, respectively and let $\tau_i = \tau_i^p + w(\tau_i^l - \tau_i^p)$, where $w_i = 1$ if an individual is affected by the tax reduction and $w_i = 0$ otherwise.

Let the potential future outcomes if not treated and treated at eligibility duration τ_i be $Y(\tau_i, 0)$ and $Y(\tau_i, 1)$, respectively, and denote the potential future treatments $D(\tau_i, 0)$ and $D(\tau_i, 1)$. If we condition on the placement stop, tax reduction and duration, τ_i, we assume $Z(\tau_i)$ to be a valid instrument. Thus,

$$(Y(\tau_i, 0), Y(\tau_i, 1), D(\tau_i, 0), D(\tau_i, 1)) \perp\!\!\!\perp Z(\tau_i)|(PS(\tau_i^p), TR(\tau_i), \tau_i) \quad (1)$$

[26]The symbol "$\perp\!\!\!\perp$" denotes "independent of" as defined in Dawid (1979).

Let D_i and Y_i be the, post τ_i, observed assignment and outcome, respectively and assume that

$$E(D_i) = g(\tau_i + \alpha_1 Z(\tau_i) + \alpha_2 TR(\tau_i) + \alpha_3 PS(\tau_i^p)) \tag{2}$$

and

$$E(Y_i) = h(\tau_i + \beta_1 Z(\tau_i) + \beta_2 TR(\tau_i) + \beta_3 PS(\tau_i^p)), \tag{3}$$

are known. Then, given the assumption in (1) it is possible to approximate a treatment effect as

$$\tilde{\delta} = \frac{\beta_1 g'(\cdot)}{\alpha_1 h'(\cdot)}, \tag{4}$$

where $g'(\cdot)$ and $h'(\cdot)$ are first order derivatives of the functions above.

If $g(\cdot)$ and $h(\cdot)$ are identity function this is the "standard" Wald estimator (see, e.g., Angrist & Krueger (1999)). This Wald estimator is equivalent to the two stage least squares estimator (2SLS).[27]

For the 2SLS estimator the assignment to the programme is specified as

$$D_i = \tau_i + \alpha_1 Z(\tau_i) + \alpha_2 TR(\tau_i) + \alpha_3 PS(\tau_i^p) + \eta_i \tag{5}$$

and the outcome is specified as

$$Y_i = \tau_i + \delta_1 D_i + \beta_2 TR(\tau_i) + \beta_3 PS(\tau_i^p) + \varepsilon_i. \tag{6}$$

Here η_i is a regression error, while ε_i is not. The 2SLS estimator uses predictions from the first step as an instrument for D_i in the estimation of the outcome model in the second step.

The three different IV estimators that we use differ regarding the assumptions made to identify treatment effects. The first estimator is non-parametric, the second estimator requires additive separability and constant (over unemployment durations) treatment effects and the third in addition requires a functional form assumption (we estimate proportion hazard regression models to employment subsidy and to job). The advantage with putting on more structure is, of course, increased efficiency. The disadvantage is an increased bias. All three IV estimators are based on ratios of estimates. However, only the second one can bee seen as a

[27]If individuals select into ES based on relative advantages the treatment effect estimates will be biased. For the linear approximations, i.e., if $g(\cdot)$ and $h(\cdot)$ are identity functions, the LATE is estimated.

standard Wald estimator, whereas the other two estimators can be seen as variants thereof.

In the first estimation we suggest a Wald estimator in a duration framework. This enables us to estimate treatment effects for the up to t duration treated unemployed. The second estimator is a traditional 2SLS estimator where first equation (5) is estimated. In a second step we then estimate (6) using the first step predictions as an instrument. To implement this estimator we need to make an assumption about the time it takes for the treatment to have an effect. For this reason we estimate the model under different lengths of the evaluation period.

In the third approach proportional hazard models of equations (2) and (3) is specified (i.e. $E(D)$ is exchanged for hazard to ES and $E(Y)$ is exchanged for the hazard to employment).[28] The effect of the ES is then estimated as the ratio between separate maximum likelihood estimates:

$$\widehat{\Delta}^{PH} = \widehat{\beta}_1 / \widehat{\alpha}_1 \tag{7}$$

The interpretation of Δ^{PH} is the percentage effect on the monthly unemployment rate from increasing the number in ES by one percent. In addition to the functional form assumption, we make an assumption about the timing of the effect: the effect is immediate after entering ES and the same (on the hazard) throughout unemployment spell.

4 The data

Our data derive from the administrative database Händel from the National Labour Market Board. The database contains information on all registered unemployed persons in Sweden since August 1991. The database includes information on, e.g., age, highest level of education and sex, as well as the individuals' registration date, job training activities and starting and ending dates of participation in labour market programmes. It also contains information on what kind of programme the unemployed person participates in.[29] The data used in this study extend to October 2002.

[28] The estimated equations are given in equations (B4) and (B5) in *Appendix B2*.

[29] More precisely: each job seeker is registered under some "job-seeker category" defining his or her labour market status. For each individual, every change in job-seeker category is registered.

For each individual registered at the PES we observe an event history including the number of spells and days of unemployment. All persons who have left the employment offices register before the introduction of the ES programme are dropped from the data. To eliminate uncertainty of at what point in time the individuals first were registered as unemployed at the unemployment office, we exclude all of those for which the first spell of unemployment occurred before January 1, 1992.

One of the main criteria to become eligible for the programme is to be registered as unemployed at the employment office for a continuous period of at least 365 days (see *Section 2.1*). Thus, to become eligible for the programme, the unemployed must have a spell of unemployment of at least 365 days extending to at least the day when the programme started. Individuals who do not fulfil this criterion are dropped from the data we analysed in this study.

Until December 31, 2000, individuals under 25 years of age had the possibility to start the ES programme with a registered spell of only 90 days. Due to this exception, all unemployed persons under 25 years of age on January 1, 1998 (the starting date of the programme) or later are excluded from the data set. We have also excluded those who at the month they registered at the PES were at least 63 years old (15,160 persons).[30] We have also excluded all individuals who had register spells with negative duration before the last spell (324 spells). Finally, because we have discretised time, we have discarded 63 spells that began as ES spells but ended in employment in at most 29 days.

A spell of unemployment is defined as an uninterrupted period of time when an unemployed person is registered at the employment office. The spell is ended if the unemployed person gets a job for a period of at least 30 days, or if he or she, for any reason, leaves the employment offices' register for a period of at least 30 days.

It is possible to have more than one spell of unemployment of at least 365 days without interruption during the time the ES programme has been going on. Thus, an individual can be eligible for the programme more than once. The unit of observation is chosen to be every time a person becomes eligible for the ES programme. In the analysis we use information on each individual's total number of spells and days in unemployment before becoming eligible for the ES. For those who are eligible more than once

[30]This is done because the individuals, to become eligible, in principle must be registered for one year. Furthermore, the programme duration is also at least six months.

the total number of days and spells is aggregated each time they become eligible. Thus, the data include only persons who have been eligible for the ES programme on at least one occasion.

The individuals in the data are separated into two different groups: those who start the ES programme after 365 days or longer, and those who do not start the programme. Each time a person becomes eligible, the total number of months until he or she either leaves the PES office or becomes right censored is calculated. The point in time for right censoring is 1 October 2002 (which is where our data set ends) or the point in time where a person leaves the register for other destinations than work. For those who enter the ES programme, the duration to ES is calculated as well.

The outcome of interest is if, and at what point in time, the individual gets a job. In this study the outcome "job" is defined as either to leave the employment offices' register for employment[31] or being temporary employed[32] for a minimum period of 30 days. A non-trivial number of persons leave the register for unknown reasons (which may well include work, see Bring & Carling, 2000, or Sahin, 2003). To the extent that there are systematic differences between participants and non-participants in the fraction of those who leave the register for unknown reason that actually leave it for work, we would get biased estimates of the treatment effect by using our measure of "job".[33] To check this, we used a matched register with information both on "Avaktualiseringsorsak"[34] according to the Händel data base and on monthly employment status according to firms' reports to the tax authorities (derived from a data base from Statistics Sweden). In this matched data base we can follow individuals for the years 1998 through 2000. In fact, people on ES leaving the PES for unknown reasons to a larger extent than eligible non-participants went to a job during the period according to our data. [35] If anything, then, using our measure of "job" would tend to give the estimated treatment effect of ES a downward bias.

The total number of persons aged 25–63 who were eligible for ES in

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[31] Leaving the register if the variable "Avaktualiseringsorsak" is equal to 1, 2 or 3.

[32] The individual is considered to have a job if he or she is registered under the jobseekers category "Skat 31" for a period of at least 30 days.

[33] This problem has been pointed out by Sianesi (2001, 2002).

[34] When a person leaves the register, each exit has a code, avaktualiseringsorsak.

[35] See *Appendix A* for details.

the whole country and the period between January 1998 and October 2002 was 631,358. This population of eligibles is described in *Table 1.* 3.2 % of the eligible spells ended up entering individual or general ES schemes (Skat 45 or Skat 47). The most salient feature of the eligible persons is that they on average had a long lasting relationship with the PES: in addition to the days spent in the register in order to become eligible, the average number of days in the register was almost 500 and the average number of earlier spells in the register was almost 1.5. Approximately 40 % of the spells ended in regular employment.

4.1 The data used for the matching estimates

Matching is based on 631,358 persons, $19,951$ of which enter ES. In addition to the eligibility duration, we match on all covariates in *Table 2* as well as on the local labour market in which the individual is registered a a job seeker. Some variables have been categorised (age and the number of previous programmes in four categories, the number of previous register days in five categories).[36]

In *Table 2* we compare the participants and non-participants in the eligible population. A significantly higher fraction (64 % as compared to 39 %) of the ES participants ended up in employment. As we have already pointed out, this does not necessarily indicate a positive treatment effect; it more likely reflects the fact that the programme participants on average registered earlier (in calender time; T_0 is smaller) at the PES and, hence, on average had spent a longer time looking for a job. Males and non-Nordic immigrants were over represented and disabled were under represented among the participants. The education level was higher among the participants, the participants were younger and had spent less time at the PES prior to the last period of unemployment. Given reasonable priors (ours) about how these characteristics should influence the exit to employment, most likely the participants should be expected to leave unemployment more rapidly than the non-participants.

In the matching estimation, $12,300$ persons in the ES programme are removed due to lack of common support (no matching individual found in control group). Hence, the matching is based on $7,651$ treated individuals. Descriptive statistics for the matched participants are displayed in column five ("Matched mean") of *Table 2.*

[36]The categories are defined in *Table 1.*

Table 1: Descriptive statistics for all eligibles

Variable	Description	Mean	Std. dev.	Min	Max
ES	=1 if in ES program	0.03	0.17	0	1
Duration	Current spell duration (months)	23.7	23.2	1	118
Employed	=1 if regularly employed	0.40	0.49	0	1
Male	=1 if male	0.41	0.49	0	1
NonNordic	=1 if non-Nordic citizen	0.14	0.35	0	1
NoUI	=1 if no unemployment insurance	0.18	0.38	0	1
Disabled	=1 if disabled	0.10	0.30	0	1
Gymnasium	=1 if upper secondary degree	0.35	0.48	0	1
University	=1 if university degree	0.12	0.33	0	1
Age_1	=1 if age ≤ 30	0.22	0.42	0	1
Age_2	=1 if $30 <$ age ≤ 40	0.31	0.46	0	1
Age_3	=1 if $40 <$ age ≤ 50	0.24	0.43	0	1
TD_1	=1 if days in register during previous spell (TD) = 0	0.38	0.48	0	1
TD_2	=1 if $0 < \text{TD} \leq 100$	0.05	0.21	0	1
TD_3	=1 if $100 < \text{TD} \leq 500$	0.20	0.40	0	1
TD_4	=1 if $500 < \text{TD} \leq 1000$	0.18	0.38	0	1
TP_1	=1 if previous number of programmes (TP) = 0	0.39	0.49	0	1
TP_2	=1 if $0 < \text{TP} \leq 5$	0.39	0.49	0	1
TP_3	=1 if $5 < \text{TP} \leq 15$	0.21	0.41	0	1
T_0	Month turning eligible, January 1998=1 October 2002=118	69.8	27.6	1	118

Inspecting column five in *Table 2* we see that the matched sample on the whole (as expected) resembles the non-participants more than the whole group of participants. This means that the participants in the matched sample (compared to all participants) had somewhat fewer earlier days in unemployment or labour market programmes (see TD_1–TD_4), smaller fractions of males, non-Nordic immigrants, persons without unemployment insurance and disabled. They also are somewhat older and less educated. On balance, given our priors, they may be a positively selected group in terms of labour market prospects, but the differences are not staggering.

4.2 The data used for the IV estimates

The data set used in the IV estimations is limited to the persons who were eligible for ES under the period of placement stop (May 1999–December

Table 2: Mean characteristics of participants (ES), non-participants (No ES), and exactly matched sample (Matched)

Variable	ES Mean	No ES Mean	ES-No ES t-value	Matched Mean
Duration	34.38	23.37	62.90	–
Employed	0.64	0.39	71.54	–
		Covariates		
Male	0.61	0.41	56.38	0.56
NonNordic	0.21	0.14	25.41	0.14
NoUI	0.16	0.18	-5.43	0.11
Disabled	0.06	0.10	-20.41	0.02
Gymnasium	0.43	0.35	24.08	0.42
University	0.12	0.12	-3.43	0.09
Age_1	0.26	0.22	11.34	0.24
Age_2	0.32	0.31	3.39	0.30
Age_3	0.27	0.24	7.04	0.25
TD_1	0.41	0.38	9.44	0.51
TD_2	0.05	0.05	4.28	0.02
TD_3	0.22	0.20	8.37	0.19
TD_4	0.18	0.18	1.22	0.15
TP_1	0.41	0.38	7.15	0.51
TP_2	0.42	0.39	7.82	0.35
TP_3	0.17	0.22	-18.62	0.14
T_0	58.33	70.22	-65.75	60.18

1999) in the 8 local labour markets with municipalities that were and were not affected by the placement stop (PS). This gives us 80,905 individuals living in 69 municipalities in the IV estimations.

Some characteristics of these data are presented in *Table 3*. Compared to the group of all eligibles in *Table 1*, the persons in the sample used in the IV analysis look fairly similar, although they had somewhat fewer previous days in the register, fewer previous spells in programmes, a smaller fraction finding employment, a larger fraction of ES participants, a larger fraction of non-Nordic immigrants, a higher fraction of males, a higher fraction with no UI and larger shares with at least high school education. Thus, the IV sample seems to be a positive selection of the unemployed in comparison to the total sample of eligibles.

Table 4 compares participants in ES schemes with non-participants and persons in PS municipalities with persons in non-PS municipalities.

A few points are worth mentioning. First, males are strongly over-

Table 3: Descriptive statistics for the data set used in the IV estimations[1]

Variable	Mean	Std dev	Min	Max
ES	0.04	0.19	0	1
Duration	33.52	26.25	1	118
Employed	0.44	0.50	0	1
Male	0.44	0.50	0	1
NonNordic	0.18	0.38	0	1
NoUI	0.23	0.42	0	1
Disabled	0.10	0.31	0	1
Gymnasium	0.37	0.48	0	1
University	0.18	0.39	0	1
Age_1	0.22	0.41	0	1
Age_2	0.30	0.46	0	1
Age_3	0.24	0.43	0	1
TD_1	0.42	0.49	0	1
TD_2	0.05	0.22	0	1
TD_3	0.20	0.40	0	1
TD_4	0.18	0.39	0	1
TP_1	0.42	0.49	0	1
TP_2	0.41	0.49	0	1
TP_3	0.17	0.38	0	1
T_0	59.87	21.20	1	84

[1] For descriptions of the variables, see *Table 1*

represented among participants. Second, the fraction of disabled is much lower and the fraction of non-nordic immigrants is substantially higher among the participants. Third, the number of previous days and previous spells in the register is slightly lower for the participants. Finally, the participants on average entered the register at an earlier date than the non-participants, which means that the proportion of censored observations tends to be higher among the non-participants. Thus, also for this sample the participants seems to have better job opportunities also absent the ES.

Comparing instead across PS and non-PS municipalities, a number of differences are worth noticing: first, persons in PS municipalities on average had shorter durations of open unemployment and entered the register later but also had a larger number of days in previous register spells. Second, males, non-nordic immigrants, and persons without UI are overrepresented in the PS municipalities. However, as we apply a difference-in-difference estimator, these differences should not be problematic.

Table 4: Descriptive statistics for participants and non-participants and for eligibles in PS and non-PS municipalities[1]

	Not ES	**ES**		**Not PS**	**PS**	
n	77,941	2,964		9,749	71,156	
Variable	**Mean**	**Mean**	**t-value**	**Mean**	**Mean**	**t-value**
ES	0	1	na	0.04	0.04	4.02
Duration	33.28	40.06	-14.59	38.40	32.86	19.06
Employed	0.43	0.64	-23.54	0.41	0.44	-6.47
Male	0.44	0.59	-16.20	0.37	0.45	-16.57
NonNordic	0.18	0.25	-9.03	0.09	0.19	-29.77
NoUI	0.23	0.19	5.48	0.16	0.24	-17.68
Disabled	0.11	0.06	9.30	0.11	0.10	2.44
Gymnasium	0.37	0.40	-3.85	0.36	0.37	-1.95
University	0.18	0.18	0.87	0.10	0.19	-26.93
Age_1	0.22	0.21	1.34	0.18	0.22	-9.78
Age_2	0.30	0.31	-0.91	0.27	0.31	-8.64
Age_3	0.24	0.28	-4.28	0.26	0.24	4.34
TD_1	0.42	0.43	-1.63	0.46	0.41	10.19
TD_2	0.05	0.05	-1.41	0.05	0.05	-0.34
TD_3	0.20	0.21	-0.60	0.19	0.21	-4.37
TD_4	0.18	0.17	1.10	0.16	0.18	-5.14
TP_1	0.42	0.43	-1.02	0.47	0.41	10.51
TP_2	0.40	0.43	-3.18	0.38	0.41	-4.70
TP_3	0.17	0.14	5.99	0.14	0.18	-8.12
T_0	60.06	54.80	13.39	58.06	60.12	-8.81

[1] For descriptions of the variables, see *Table 1*

For our instruments to be valid, it is required that the municipalities in the local labour markets that we analyse in the IV estimations have a similar labour market development within each local labour market. We also require that the placement stop actually affects the rate of programme placement differently in the PS and non-PS municipalities. These features are explored in *Figure 4*, where we plot the number of openly unemployed, the number of ES participants, the number of participants in the extended ES and the total number of programme participants among the persons who are eligible for the ES between July 1998 and June 2000 in the eight local labour markets we analyse in the IV estimations, distinguishing between municipalities with and without PS.

In the two upper panels of *Figure 4* we see that open unemployment and participation in the extended ES develop extremely similarly in the two groups of municipalities. Although not conclusive, this evidence supports instrument validity.

The two bottom panels show ES participation and total participation in all ALMPs for the eligible sample. Looking first at total programme participation (right panel), there was a pronounced drop in both PS and non-PS municipalities, beginning at about the time when the budget cutbacks were enforced (April or May).[37] The drop was somewhat larger in the PS municipalities, and the programme volume in the PS municipalities did not catch up during the period depicted in the figure. For ES participation (left panel), the picture was a different one. In the PS municipalities, ES participation decreased when the PS was enforced, while the opposite was true for the non-PS municipalities. Then, in October 1999, ES participation increased in the PS municipalities whereas it decreased between October and December 1999 in the non-PS municipalities.

All in all, the developments shown in *Figure 4* indicate that the budget cutbacks and the change in the mode of finance of the ES programme actually gives rise to some independent variation in ES participation between municipalities that we can use to identify a treatment effect.

5 Results

5.1 The matching estimator

We begin by considering the matching estimates of the treatment effect of the employment subsidy programme. In *Figure 5* we show estimated survival functions, $\widehat{S}^1(t)$ and $\widehat{S}^0(t)$ for the individuals who entered ES and not entering ES, respectively. For both samples (not matched and matched) we condition on the eligibility duration and use the Kaplan-Meier estimator (see, e.g., Lancaster, 1990). For the ES sample we estimate the un-

[37] This drop had a significant seasonal component as identified in our empirical analysis.

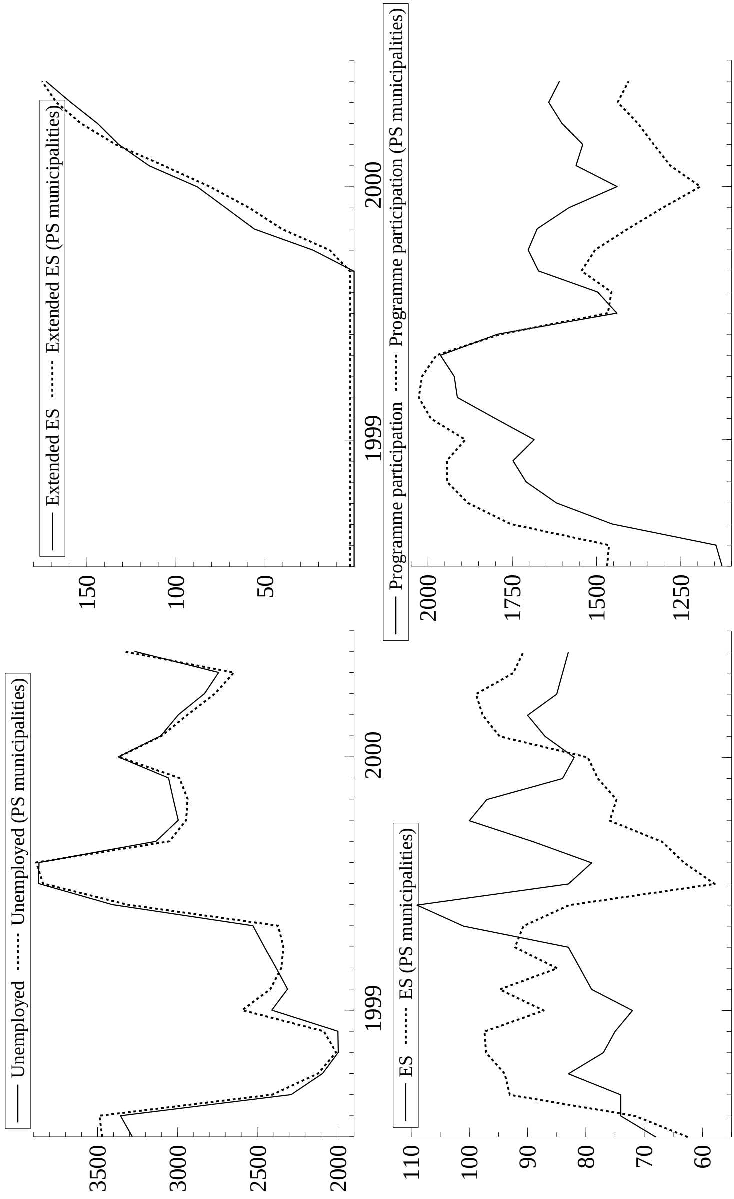

Figure 4: Open unemployment, ES participation, participation in extended ES and total participation in ALMPs by eligibles in PS and non-PS municipalities July 1998–June2000. Means and ranges are matched for comparability.

Source: Own computations using information from the Händel data base conditional (on the eligibility duration) Kaplan-Meier survival function to employment from ES entrance. The estimator for the non ES sample uses all unemployed at risk of entering ES or job (the same eligibility duration as for the ES sample is created by matching on eligibility duration as for the ES sample). An individual entering ES at a later time is censored at that time. For the matching estimator we, in addition to the eligibility duration, also match on the covariates given in *Table 2* as well as on the local labour market in which the individual is registered as job seeker. For details on the matching estimators, see Fredriksson & Johansson (2004).

Figure 6 shows the estimated treatment effects ($\widehat{\Delta}(t) = \widehat{S}^0(t) - \widehat{S}^1(t)$) with 95 % confidence intervals (c.i.). In both figures we show estimates before and after matching on covariates. This means that the differences between the upper and the lower panels in both figures reflect the effects of matching (i.e., the effects of controlling for the observed individual characteristics).

The estimates in *figures 5* and *6* show that after an initial period of about 6 months with a negligible (negative) treatment effect there is an upward jump; from then on the effect gradually becomes smaller, but is positive and significant over the rest of the follow-up horizon (57 months). This scenario is consistent with an initial period of locking in and a subsequent period with a positive treatment effect. The sum of the effects over the whole follow-up horizon is 7.78, which corresponds to a decrease in unemployment duration (from the ES entrance) by almost 8 months.

Further, by comparing the upper and lower panels of *figures 5* and *6*, we infer that observed heterogeneity seems to matter: the "treatment effects" are reduced considerably by matching. It is also noteworthy that the initial treatment effects are negative (implying locking in) only after matching.[38] Hence, it seems that there is a positive selection into ES.

One advantage with the matching estimator is that we can compute treatment effects for different durations in unemployment before entering the programme. In *Figure 7* we plot the treatment effect for those entering during months 0–3 after eligibility, and in *Figure 8* we plot the treatment effect for those entering during months 36–39.

The general message conveyed by figures 7 and 8 is that treatment effects look rather similar irrespective of the timing of programme en-

[38] This finding may seem surprising given that the programme lasts for six months. However, Lundin (2001) found that more than 30 % of the programme participants had applied for at least one job while on the subsidised job.

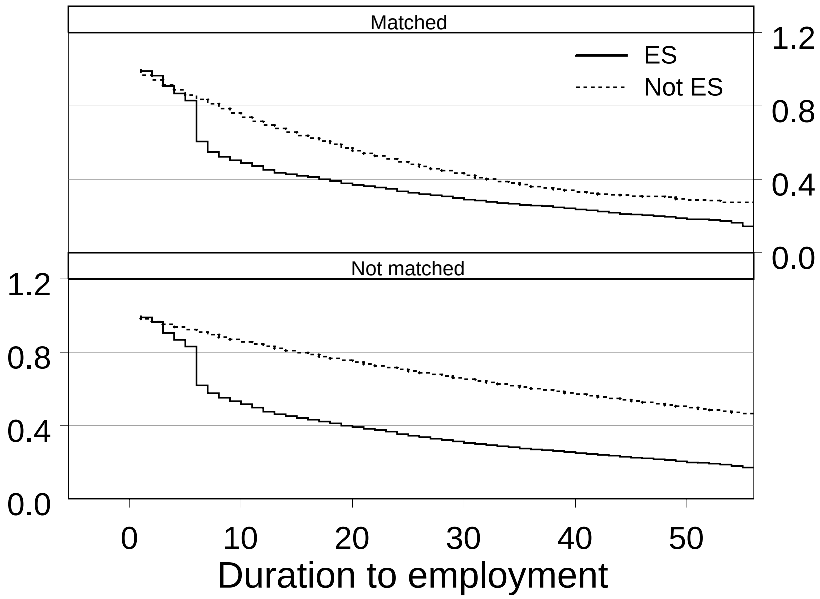

Figure 5: Survival functions to employment for participants and eligible non-participants

trance.[39] Once again, comparing the matched and unmatched estimates, there seems to be a positive selection into ES for both early and late entrants. The pattern is, however, more pronounced for the early entrants.

There is an important caveat concerning the interpretation of the results. In all figures we see that there is an upward jump in the estimated treatment effect after 6 months. A likely explanation for this is that the participants simply tend to stay employed where they had their subsidised employment. On the one hand, this is an intended effect of the programme. On the other hand, this result may be seen as an indication that (consistent with previous evidence) the programme tends to displace

[39]This is true for the point estimates. The confidence interval for those beginning treatment during months 36–39 is, however, wide due to the small number (the number of matched persons is 206; for 489 persons we found no match) of treated persons.

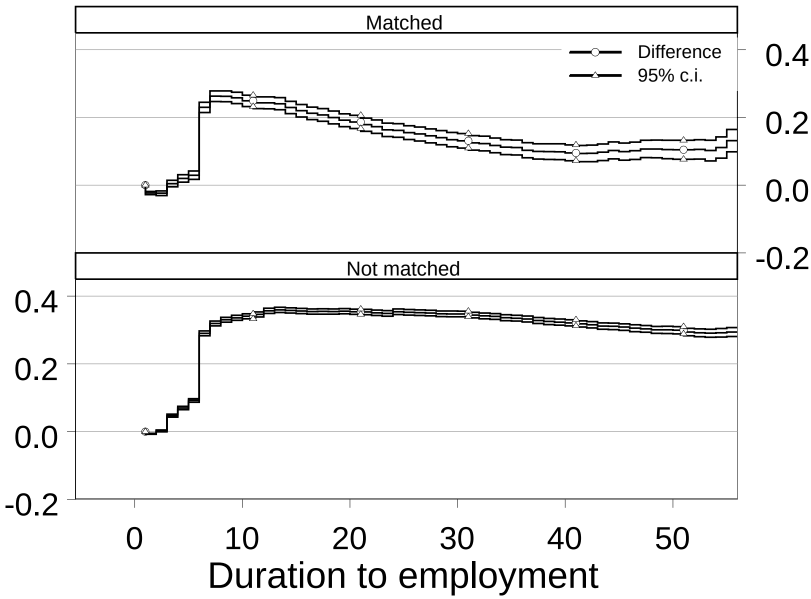

Figure 6: Estimated treatment effect

regular employment.

The matching estimator identifies a true treatment effect under the conditional independence assumption (CIA). We believe that we have rich enough information to make the identifying assumption plausible. However, the assumption is non-testable, so we cannot be sure that the matching results do not, at least to some degree, reflect selection rather than a causal effect.

Another concern is that the results would have a limited external validity because the sample for which we have a common support is small and seems to be positively selected (see *Section 4*). To check the importance of this, we have also estimated the treatment effect using propensity score matching. This gives us, roughly, twice as large a sample in the common support. The results are virtually unchanged as compared to the results derived by the exact covariate-matching procedure.

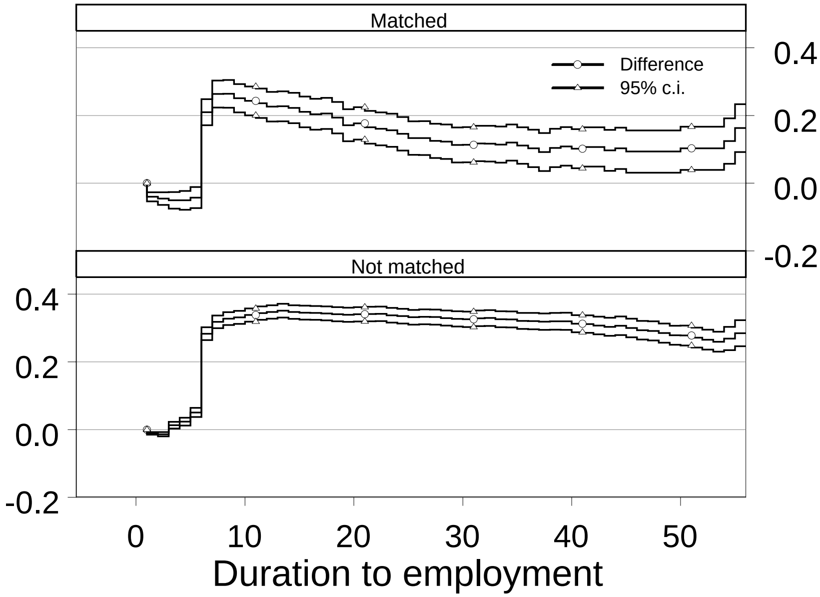

Figure 7: Estimated treatment effect for participants entering during months $t = 0, \ldots, 3$

Hence, we now turn to the results derived by our IV estimators. If the results are similar, this gives increased support to our interpretation of the matching estimates.

5.2 The IV estimation

Details on the estimation methods are given in Appendix B. However, the basic principle, as discussed in *Section B*, is to estimate separate models for the hazard into programmes and into employment.

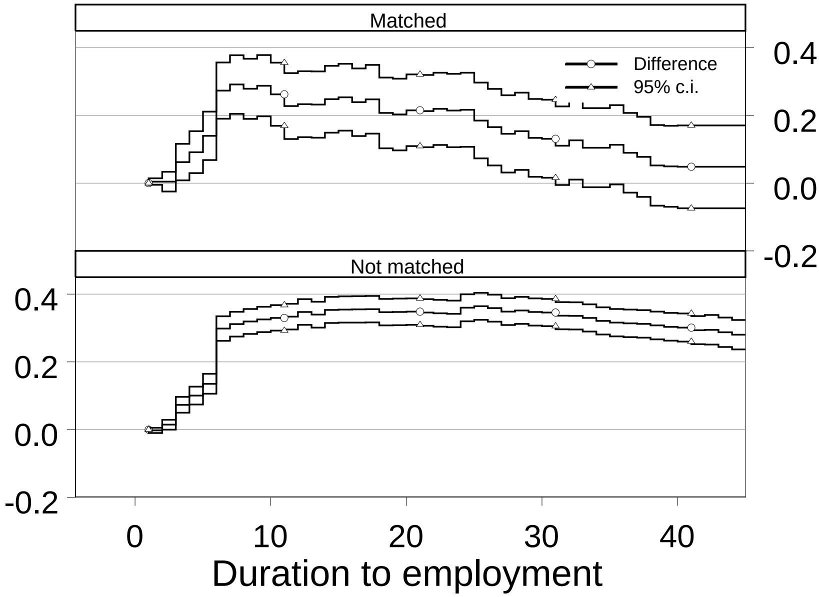

Figure 8: Estimated treatment effect for participants entering during months $t = 36, \ldots, 39$

5.2.1 Non-parametric IV estimation

In *Figure 9* we show the difference-in-difference estimate of the effect of the tax reduction on the hazard to employment. The similarity to the matching estimates of the treatment effect is striking regarding the time profile: after a few initial months of locking in, the flow into employment goes up steadily until approximately three years. It then stays constant for about a year, to finally end up at some 2 % at the end of the follow-up horizon. To us, this resemblance to the matching estimates is reassuring: although the parameters are not the same (especially since the individual treatment effects do not depend on entrance date, see figures 6–8), we see no good reason why the time profile should be different.

For our approach to be valid, we first rely on the tax reduction in

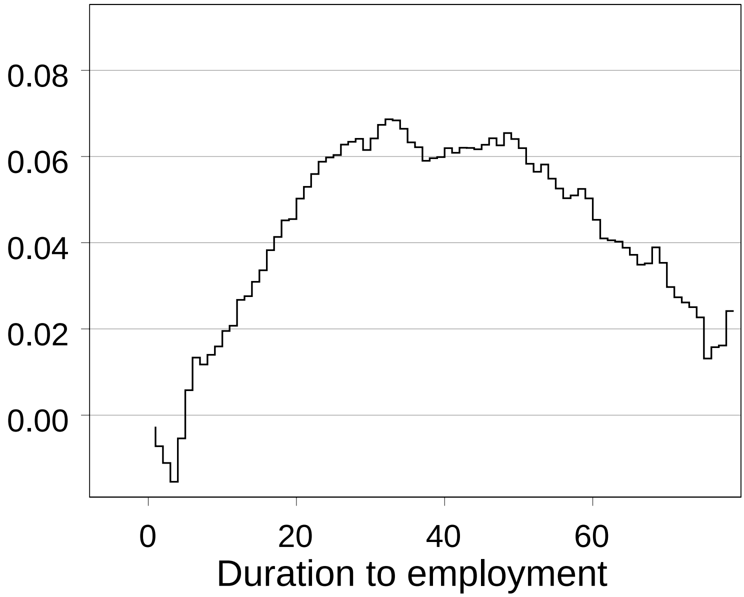

Figure 9: Estimated effect of instrument on the hazard to employment

October 1999 to increase the inflow to ES in the placement-stop municipalities relative to the non-PS municipalities. In *Figure 10* we plot the difference-in-difference estimate of the effect of the tax reduction on the inflow into ES.

First, we see that the effect indeed goes in the expected direction. However, we also see that it is quite small (almost zero during the first year) and takes considerable time to build up. Furthermore, looking at standard errors (not shown in the figure), we also see that the effect is estimated with low precision.

Given all caveats with precision of the estimates, we nevertheless find it worthwhile to show the IV estimates of the programme effect. These are displayed graphically in *Figure 11*.

Once again, the feature of the estimates we like to focus on is the time pattern of the effects. Although the estimated effect is (unreasonably)

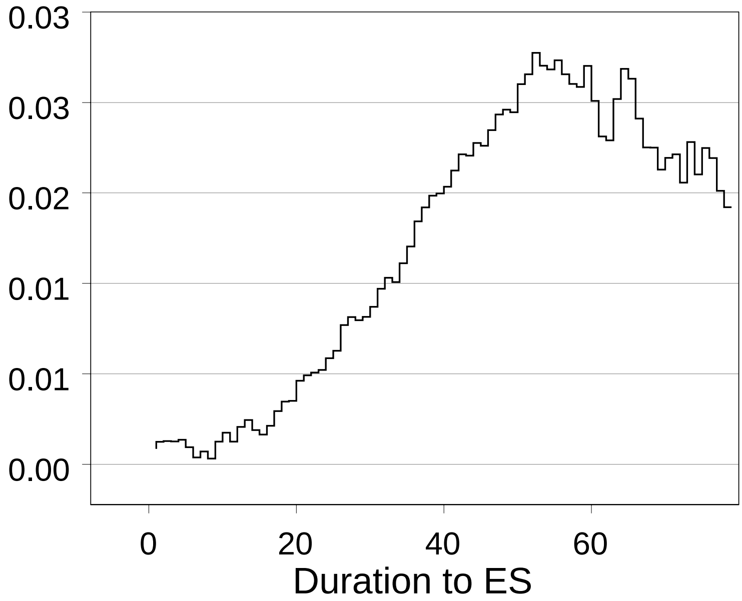

Figure 10: Estimated effect of instrument on hazard into employment subsidy programme

high, much higher than the corresponding matching estimates, the time profile bears close resemblance to the matching estimates.

5.2.2 Two stage least squares estimation

For the second IV estimator (the 2SLS estimator) we include all individuals who have a duration in unemployment that extends into May 1999, thus $PS(\tau_i^p) = PS_i$. We also "assume" that the shortest time period to measure the effect of the ES is one month after entering treatment. Since the effects of the tax reduction take place for only 3 months (October-December) we only use the first three months after the placement stop (May-July) and measure employment during three months (leaded with one month) after either the PS and the TR, i.e., for June-August and

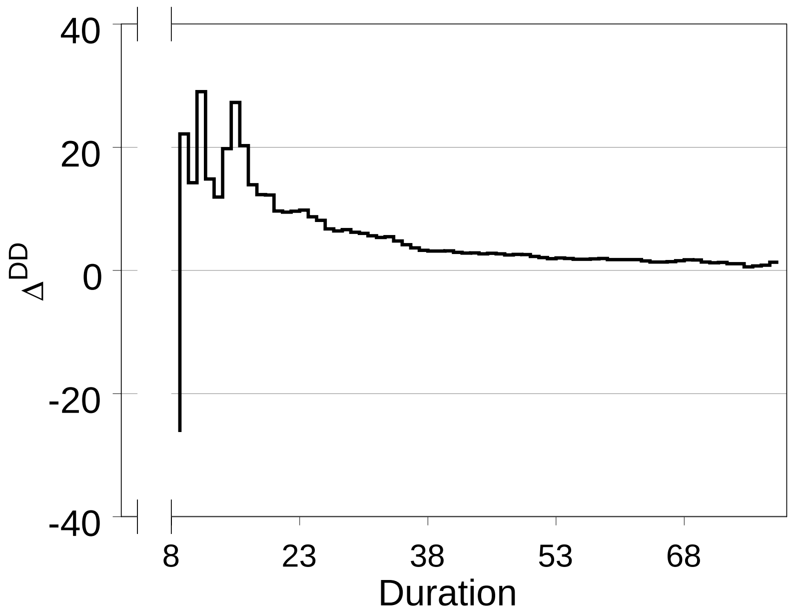

Figure 11: IV point estimates of the treatment effect of employment subsidies

November-January. The evaluation period is a maximum of three months from the point in time when an individual has entered ES. In addition, we extend this three months window to a maximum of a twelve months: June-April and November-July. This implies a maximum of 12 months and a minimum of 7 months past ES entrance. When the window is longer than five months, we assume that the effect is the result of the first treatment (i.e. the placement stop). Thus, the evaluation period is longer for the pre tax-reduction sample. This procedure should be conservative, i.e., it should bias the estimated effect downwards.

The results from the 2SLS estimations are presented in *Table 5.* We can see that the tax reduction (statistically significant) induces more people into ES in the PS municipalities than in the non PS municipalities (the point estimates of α_1 in *equation* (5), displayed in the first column of

Table 5, are positive for all lengths of the evaluation window). The effect on employment of this increase in placements into ES for the PS municipalities (the point estimates of δ in *equation* (6), given in the fourth column of *Table 5*) is however never statistically significantly different from zero. This result stems from bad precision rather than from small point estimates. The pattern of a direct locking in effect (negative point estimates of δ) and an thereafter positive effect is, however, in accordance with both the previously estimated time profiles.

Table 5: Parameter estimates (est.) and estimated standard errors (s.e.) from the 2SLS estimator: first and second step[1]

	First step estimator			**IV estimator**		
Window	**est.**	**s.e.**	**est./s.e.**	**est.**	**s.e.**	**est./s.e.**
3	.010	.002	4.12	-2.662	2.534	-1.05
4	.008	.002	3.76	-4.140	2.865	-1.44
5	.007	.002	3.04	-6.418	3.627	-1.77
6	.007	.002	2.63	-4.848	3.152	-1.54
7	.005	.003	1.73	-3.704	3.967	-.93
8	.008	.003	2.50	-2.520	2.367	-1.06
9	.009	.003	2.61	.510	1.962	.26
10	.008	.003	2.45	.927	2.130	.44
11	.010	.003	3.16	.837	1.722	.49
12	.010	.003	3.52	.425	1.622	.26

[1] In total 86 parameters are estimated: we control for the eligibility duration to either tax reduction or to the placement stop date (a factor at 83 levels), TR, BS and either Z (in the first step) or D.

5.2.3 Cox regression models

Now we turn to the result from the separately estimated Cox regression models. Here, as opposed to in the previous IV estimators, unobserved heterogeneity matters for our estimated treatment effect. It is well known (see, e.g., Lancaster, 1990) that unobserved heterogeneity would bias our estimates downwards. However, if the unobserved heterogeneity is the same for the inflow into ES and into job, our effect estimate would not be biased. This is so since the effect is estimated as ratios between two equally biased estimates. However, to mitigate the consequence of unobserved heterogeneity, we include several control variables in the Cox regressions: we include monthly dummies, municipal dummies and all covariates given in *Table 1*. The results from the regression are displayed in *Table 6*. We, again, find that the tax reduction increased the probability to join ES more in the PS municipalities than in the non-PS municipalities (the point estimate of α_1 in *equation* (2)) is significantly positive. The point estimate of the effect (1.42) implies that the odds to start an ES program in a PS municipality increased by 4 times as compared to the non-PS municipalities. The estimated effect of our instrument (Z) on the flow to jobs (the (significant) point estimate of β_1 in *equation* (3)) implies that the odds of finding a job in a PS municipality after the tax reduction increased by 11 %.

The effects on unemployment from increasing the number in ES by one per cent ($\widehat{\Delta}^{PH} = \widehat{\beta}_1/\widehat{\alpha}_1$) then equals $0.11/1.42 = 0.07$ with an estimated standard error[40] of 0.003. This is a quite large (and statistically significant) effect, however not unreasonable. Remember that only about 3 % of the long term unemployed participate in the ES program. Under the assumption of a constant hazard, λ, we calculate[41] the average monthly job hazard for this long term unemployed to 4.17 %. By increasing the number in ES with one per cent this would increase the hazard to 4.46 % and the corresponding average duration would decrease by two months (from 24 months to 22 months).

5.3 Summary of the results

For the matching estimator we estimate the effects of ES from the ES entrance date. For all IV estimators we estimate the treatment effect for those treated up to t.[42] In addition, the results from the matching and IV estimators are based on different samples. We believe that the main results from the matching estimators are corroborated by our IV analysis. This strengthens our belief that we actually have sufficient information to match on to successfully remove selection problems. The sample selections made

[40] The standard error is estimated by Gauss approximation.

[41] We assume a constant hazard, λ, then $\lambda = 1/m$, where m is the average duration. $m = 24$, see *Table 1*.

[42] This is true also for the proportional hazard model. However, the treatment effects are assumed to be independent of the entrance date.

Table 6: Parameter estimates (est.) and asymptotic standard normal statistics (est./s.e.) for the hazard regression models to ES and to job

	Hazard to ES		Hazard to job	
Parameters	**est.**	**est./s.e.**	**est.**	**est./s.e.**
PS	-0.34	-175.33	0.04	27.77
TR	-2.14	-416.47	-0.18	-100.64
Z	1.42	268.56	0.11	74.55
February	0.08	25.38	-0.02	-8.07
March	0.18	58.74	-0.02	-6.93
April	0.09	31.30	0.06	22.84
May	-0.04	-12.43	-0.10	-37.63
June	0.06	21.64	0.00	-1.06
July	0.11	37.29	0.03	10.35
August	0.11	38.03	0.02	7.01
September	0.12	39.02	0.05	18.20
October	0.06	21.99	-0.02	-7.81
November	0.10	32.19	-0.01	-3.29
December	0.20	67.39	-0.02	-8.77
Males	0.63	507.35	-0.02	-14.41
NonNordic	0.47	252.28	0.05	33.50
NoUI	-0.31	-186.14	-0.43	-279.16
Disabled	-0.57	-311.45	-0.81	-471.50
Gymnasium	0.28	192.14	0.65	507.51
University	0.28	148.13	0.78	475.95
AGE	-0.16	-253.90	-0.26	-477.74
TD	-0.27	-136.36	-0.01	-4.71
TP	-0.93	-131.78	0.05	7.54

[1] In the regression we additionally include a factor for municipality (69 levels) and 105 fixed time effects.

are discussed in *Section 4*. The general result is that both samples (the matched sample and the sample used for the IV estimations) are positive selections from the original sample of eligibles. Wether the positive results of ES on employment also would be valid for the less advantaged groups is an open question. Our guess is that, since the difference in background for both selected samples is not large and the effect is large, the results found here are also externally valid.

6 Concluding comments

Our results suggest that there is a positive treatment effect of participating in the general employment subsidy programme. This is in line with results in previous studies of Swedish ALMPs. It is also consistent with results in programme evaluations in other OECD countries (Martin & Grubb 2001). On the other hand, our results are in contrast with the results for most other Swedish programmes used during the last decade, where the available evidence suggests that these programmes have had negative or insignificant treatment effects. This would seem to suggest that a heavier emphasis on employment subsidies in the policy mix should be beneficial.

However, there is also ample evidence, both Swedish and for other countries, indicating that subsidised jobs in the private sector have larger dead-weight and substitution effects than other programmes (Calmfors et al. 2001, Martin & Grubb 2001). Hence, there is a trade-off that should be taken into consideration when designing labour market policies.

We apply methods suggested by Fredriksson & Johansson (2004) to estimate treatment effects when programme assignment is a random process. This non-parametrical procedure gives us estimates of the time profile of effects as well as differential effects for entry at different points in time. Although our approach is data demanding, it can definitely be applied when evaluating other programmes. This is important, because random programme start is a generic characteristic of most labour market programmes. Finally, we show how to apply instrumental variables difference-in-difference estimators in a duration setting. Also this procedure could be applied to evaluations of other programmes.

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Appendix

A Persons leaving the PES register for unknown reasons

A non-negligible number of persons leave the register of the PES for unknown reasons. One possibility is that some of them, because they have found a job, see no reason to contact the PES and, hence, leave the register for unknown reasons.[43] To find out the extent to which those eligible for employment subsidies who leave the PES register for unknown reasons have found a job, we have matched the Händel data base of the National Labour Market Board with data from Statistics Sweden with monthly information on labour incomes.[44] This procedure enables us to ascertain whether there are systematic differences between those who have entered the ES programme and the other eligibles with respect to the job-finding rate among those who left the register for unknown reasons.

Accordingly, we measure labour income (through monthly employer information to the tax authorities) the month after a person has left the PES register for unknown reasons. To be considered employed in our analysis, a person should have income during at least 32 days, and the first recorded income should occur no later than ten days after leaving the register.

Around 4,500 participants in ES left the PES register for unknown reasons during the period 1998–2000. In *Table A1* we show the fractions of eligibles leaving the PES register with incomes in different intervals according to our definition above.

We see that a majority of those who left the register had a monthly income at least amounting to SEK 5,000. It is not evident which income limit should be used to define "employment"; it is, however, clear that with a reasonable cut-off, the fraction employed is relatively large. This is in accordance with the results in Bring & Carling (2000) and Sahin (2003).[45]

Further, we see that the share in employment is larger for the ES

[43] That this is actually the case is indicated by Bring & Carling (2000) and Sahin (2003).

[44] Our available information covers the period 1998–2000.

[45] Their results apply to register leavers in general, whereas our results apply to those eligible for ES, i.e., persons with a long duration in the register.

Table A1: Labour incomes among eligibles who left the PES register for unknown reasons

Monthly labour income	Participants	Non-participants
≥ 2275	67	55
≥ 5000	62	50
≥ 10000	47	38
≥ 12500	34	28
≥ 15000	19	17

participants, irrespective of the cut-off chosen. This indicates that we, by disregarding this exit to work in our empirical analysis in this paper, if anything underestimate the treatment effect of ES.

B IV estimation: Estimand and estimator

B1 The first IV estimator

Let T be the duration to employment and let $\{Y(\tau)\}_{\tau=0}^{t}$ be the sequence of monthly employment indicators. Furthermore let $T(0)$ be the duration in unemployment if not in ES and let s be the time when entering ES. If an individual is employed at duration $T = t$, $Y(t) = 1$. Treatment assignment can in this framework be denoted $D(t) = I(T(0) > T = t)$. The meaning of this is simply that the duration in unemployment if not treated is interrupted by entering ES. In the same fashion, denote $Z(t) = I(T > t|T \in \text{October 1999})$. Thus $Z(t) = 1$ if an unemployment spell giving rise to ES eligibility extends into October 1999, i.e., when the tax reduction came into effect. The survival function before and after TR can, given this notation, be written

$$S(t|Z(t)) = \Pr(T > t|Z(t)) = \Pr(Y(t) = 0|Z(t), \{Y(\tau) = 0\}_{\tau=0}^{t-1}).$$

This survival function may be decomposed according to

$$\begin{aligned} S(t|Z(t)) = & \Pr(Y(t) = 0|D(t) = 1, Z(t), \{0\}) \Pr(D(t) = 1|Z(t), \{0\}) \\ & + \Pr(Y(t) = 0|D(t) = 0, Z(t), \{0\}) \Pr(D(t) = 0|Z(t), \{0\}), \end{aligned}$$

where, to make the presentation more compact, $\{Y(\tau)=0\}_{\tau=0}^{t-1}=\{0\}$. If $Z(t)$ is a valid instrument, $\Pr(Y(t)=0|D(t)=j,Z(t),\{0\})=\Pr(Y(t)=0|D(t)=j,\{.\})=S^{j}(t)$; $j=0,1$; and, hence,

$$\begin{aligned}
S(t|Z(t)) &= S^{1}(t)\Pr(D(t)=1|Z(t),\{.\})+S^{0}(t)\Pr(D(t)=0|Z(t),\{.\}) \\
&= [S^{0}(t)-S^{1}(t)]\Pr(D(t)=0|Z(t),\{.\})+S^{1}(t) \\
&= [S^{0}(t)-S^{1}(t)]\overline{F}(t|Z(t))+S^{1}(t).
\end{aligned}$$

$S^{1}(t)$ is the survival function in unemployment for the up to t randomly (from the flow of eligibles) assigned ES participants, while $S^{0}(t)$ is the counterfactual survival function if not in ES, i.e., $\Delta(t)=S^{0}(t)-S^{1}(t)$ measures the effect of treatment for the up to duration t randomly assigned individuals. $\overline{F}(t|Z(t))=\Pr(D(t)=0|Z(t),\{0\})$ is the survival function to ES. If we take the difference between before and after TR we get

$$S(t|Z(t)=0)-S(t|Z(t)=1)=[S^{0}(t)-S^{1}(t)]\left[\overline{F}(t|Z(t)=0)-\overline{F}(t|Z(t)=1)\right]$$

Hence, the causal effect of participation in ES before t can be estimated as

$$\Delta^{D}(t)=[S(t|Z(t)=0)-S(t|Z(t)=1)]/(\overline{F}(t|Z(t)=0)-\overline{F}(t|Z(t)=1)). \tag{B1}$$

The difference estimator in (B1) cannot, however, identify a causal parameter since $Z(t)$ is not ignorable in the survival function to employment when conditioning on treatment. *First*, at the same time as the TR was introduced, a number of other events took place. Most importantly, the extended employment subsidy was introduced (and rapidly reached larger volumes than the ES we study). In addition, business cycle changes, which affected the chances to find a job, may have occurred. Hence, a simple before-after comparison may be misleading. *Second*, to the extent that there is an effect of participation on the hazard to employment, participants and non-participants will become right-censored to different extents. Also for this reason the estimator (B1) will not identify a causal parameter. Both of these complications can be taken care of by instead using a difference-in-difference estimator, where we compare the municipalities affected and non-affected by the placement stop (PS), but located in the same local labour markets.

Hence, let $W=0$ denote a non-PS municipality and $W=1$ a PS municipality. Further, denote the survival function for $w=j$, and $Z(t)=$

k by $S(t|Z(t) = k, W = j) = S(t|k,j)$ and use the same notation for the survival function to ES. Then we can write a difference-in-difference estimator as

$$\Delta^{DD}(t) = \frac{S(t|0,1) - S(t|1,1) - (S(t|0,0) - S(t|1,0))}{\overline{F}_{T^s}(t|0,1) - \overline{F}_{T^s}(t|1,1) - \left(\overline{F}_{T^s}(t|1,0) - \overline{F}_{T^s}(t|0,0)\right)}. \quad \text{(B2)}$$

B1.0.1 Estimation Let $\hat{F}(t|0,1)$ and $\hat{F}(t|1,1)$ be the ex post and ex ante Kaplan-Meier survival estimates into ES for PS municipalities; $\hat{S}(t|0,1)$ and $\hat{S}(t|1,1)$ the corresponding estimates to employment. Further, let $\hat{F}(t|1,0), \hat{F}(t|0,0)$ and $\hat{S}(t|0,0)$ and $\hat{S}(t|1,0)$ be the corresponding estimates for the non-PS municipalities. To estimate the ex post survival functions we use all individuals who are at risk (to ES and job) before October 1999. The ex ante estimates are based on the individuals at risk (ES and job) in October, November and December 1999.

To determine the effect we estimate separate models for the hazards to ES and employment. Needless to say, for our approach to be valid, the PS municipalities should put eligibles into ES to a higher extent than the non-ES municipalities when the mode of finance changed in October 1999.

B2 The third IV estimator: proportional hazard regression

Let $\lambda^J(t, TR, PS)$ be the probability (hazard rate) to be employed after t months of unemployment and let $\lambda^{ES}(t, TR, PS)$ be the probability to enter ES after t months of unemployment. These probabilities are functions of the tax reduction and the placement stop. Thus $\lambda^J(t, 0, 1)$ denotes the probability to be employed for an eligible individual in a PS municipality before the tax reduction was introduced.

The "observed" probability to be employed after t periods in unemployment is

$$\lambda^J(t) = \lambda^J(t,0,0)^{(1-TR(t))(1-PS(t))}\lambda^J(t,1,0)^{TR(t)(1-PS(t))}\times$$
$$\lambda^J(t,0,1)^{(1-TR(t))PS(t)}\lambda^J(t,1,1)^{TR(t)PS(t)} \quad \text{(B3)}$$

Assuming time constant "effects" we have $\beta_2 = (\ln\lambda^J(t,1,1) - \ln\lambda^J(t,0,1))$, $\beta_3 = (\ln\lambda^J(t,0,1) - \ln\lambda^J(t,0,0))$ and $\beta_1 = (\ln\lambda^J(t,1,1) - \ln\lambda^J(t,0,1) - (\ln\lambda^J(t,1,0) - \ln\lambda^J(t,0,0)))$. Under this assumption, equation (B3) can be simplified as

$$\lambda^J(t) = g_0(t)\exp(\beta_1 Z(t) + \beta_2 TR(t) + \beta_3 PS(t)) \quad \text{(B4)}$$

where $g_0(t) = \lambda^J(t, 0, 0)$ is the baseline hazard to receive employment after t months in unemployment and $Z(t) = TR(t)PS(t)$, thus $Z(t) = I(T > t|T \in \text{October 1999})$ if living in a PS municipality. Here $PS(t)$ is a step function for an unemployed in a PS municipality (i.e., $PS(t) = 1$ after the placement stop occurred) and $TR(t)$ is also a step function for all unemployed from October 1999.

The probability to enter the ES program can, similarly, be be written as

$$\lambda^{ES}(t) = h_0(t) \exp(\alpha_1 Z(t) + \alpha_2 TR(t) + \alpha_3 PS(t)). \tag{B5}$$

Notice that both $PS(t)$ and $TR(t)$ differ from the specifications for the employment hazard. In the hazard to ES, they are both impulse functions (instead of step functions): $PS(t) = 1$ only for a duration spell in May–December 1999 and $TR(t) = 1$ only for spells in October–December 1999. This reflects the assumption that the instrument affected the assignment to ES only during the placement stop period, whereas the effects on the hazard to employment may occur later.

Our estimator of the treatment effect in the proportional hazard setting simply becomes

$$\Delta^{PH} = \frac{\beta_1}{\alpha_1} \tag{B6}$$

B2.0.1 Estimation We use maximum likelihood and estimate the model in discrete time (Kalbfleich & Prentice 1980, Ch. 4). The probabilities to enter employment and enter ES after $(t + 11)$ months in unemployment for individual i are specified as

$$\lambda_i^J(t) = 1 - \exp(-e^{(\beta_0 + \beta_t + \beta_1 Z_i(t) + \beta_2 TR_i(t) + \beta_3 PS_i(t) + \mathbf{X}_i(t)'\boldsymbol{\beta})}),$$

and

$$\lambda_i^{ES}(t) = 1 - \exp(-e^{(\alpha_0 + \alpha_t + \alpha_1(t) Z_i(t) + \alpha_2 SR_i(t) + \alpha_3 PS_i(t) + \mathbf{X}_i(t)'\boldsymbol{\alpha})}),$$

respectively. Here $\mathbf{X}_i(t)$ is a vector of control variables and α_t and β_t are estimates of $g_0(t)$ and $h_0(t)$, respectively.

C The municipalities used in the IV estimations

The 69 municipalities in the 7 local labour markets used in the IV estimations are displayed on the map in *Figure C1.* The bulk of the observations derive from the local labour market around Stockholm, but there is some geographic dispersion and all municipalities are not located in the vicinity of the three largest cities (Stockholm, Gothenburg and Malmö). However, no local labour markets from northern Sweden are used.

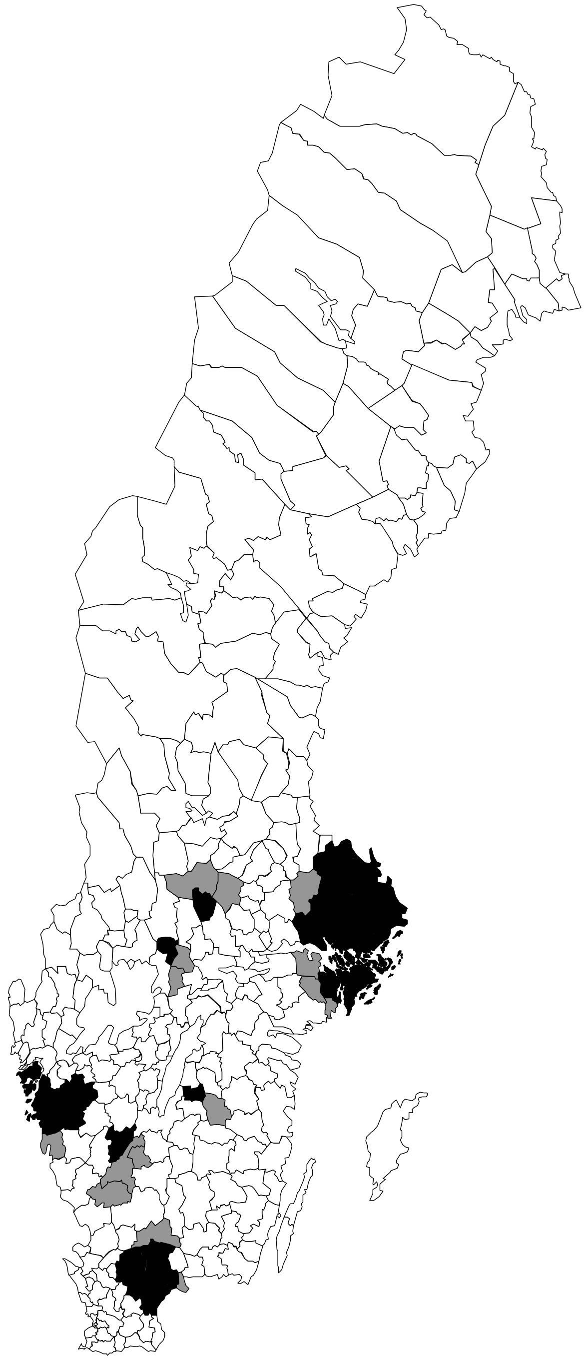

Figure C1: The municipalities used in the IV estimations. The grey areas are PS municipalities and the black areas are non-PS municipalities in the same local labour markets

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