Differential Effects of Swedish Active Labour Market Programs for Unemployed Adults in the 1990s

Barbara Sianesi

Revised: November 2003
(First draft: October 2001)

Abstract: The differential performance of Sweden’s labour market training, workplace introduction, work experience, relief work, trainee replacement and employment subsidies is investigated in terms of short- and long-term employment rates and unemployment-benefit collection probability. Both relative to one another and compared to more intense job search in open unemployment, the central finding is that the more similar a program is to a regular job, the higher the program’s benefits to its participants. Employment subsidies is by far the best performer, followed by trainee replacement, whilst the other programs appear to be often used as a way to re-qualify for unemployment benefits.

Keywords: Active labour market programs, evaluation, multiple treatments, propensity score matching, treatment effects.

JEL classification: C14, J38, J65, J68.

Acknowledgements: I am indebted to my supervisor, Costas Meghir, for helpful comments and continuous guidance. I thank Anders Harkman and Per Johansson for invaluable information and detailed comments, and Jaap Abbring, Erich Battistin, Richard Blundell, Kenneth Carling, Anders Forslund, Hide Ichimura, Francis Kramarz, Edwin Leuven, Katarina Richardson, Jeff Smith and participants at the IVA conference, the Second Villa Mondragone Workshop in Economic Theory and Econometrics in Frascati (Rome), the Network of Excellence conference on “The Evaluation of Labour Market Policies” in Amsterdam, the Econometric Society Winter European Meeting in Budapest, the UCL Applied Micro seminar and the ESEM 2003 in Stockholm for discussions and suggestions. Thanks also to Kerstin Johansson for the municipality-level data and to Helge Benmmerk and Altin Vejsiu and again Anders Harkman for helpful institutional information and data issues clarifications. A very special thank to Susanne Ackum Agell for encouraging this research and organising financial support through the IFAU. Financial support from the ESRC Centre for the Microeconomic Analysis of Fiscal Policy at the IFS is also gratefully acknowledged.

Address for correspondence:
Institute for Fiscal Studies
7 Ridgmount Street – WC1E 7AE London
Tel: +44 (0)20 7291 4831
Fax: +44 (0)20 7323 4780
Email: barbara_s@ifs.org.uk
1. Introduction

Sweden occupies a special place when it comes to labour active market programs (ALMPs). A long-standing reliance on such measures\(^1\) has been accompanied by traditionally low unemployment rates by European standards, so that Sweden came often to be regarded as a model for other countries (e.g. Layard, Nickell and Jackman, 1991).\(^2\)

The core of this ‘Swedish model’ is an institutional environment where unemployed individuals can potentially choose among a wide array of options, each one aimed at improving their labour market opportunities in different ways. Some types of programs provide direct incentives to move back into employment by either facilitating individuals’ job search, providing wage subsidies or fostering the acquisition of work contacts and references. Other measures provide incentives to improve individual productivity and skills via formal training or work experience, thus expanding the range of work possibilities and making the working option more attractive. Since different programs may have heterogeneous effects, it is interesting to evaluate their relative effectiveness, both relative to another one and relative to no program participation. Ideally one would wish to identify the best performing programs, since general lessons as to what type of program is more effective are being shared across countries (see e.g. Martin and Grubb, 2001).

The Swedish institutional set-up can thus be very informative for policymakers with an interest in ALMPs. For researchers in the field it also offers interesting challenges, in that it raises some methodological and modelling issues not previously addressed in the typical US program evaluation literature. In the standard specification, the program is administered at a fixed point in time, and individuals are either treated (i.e. participate in the program) or not treated (i.e. do not participate). In Sweden by contrast the definition of the ‘non-treated’ is not so straightforward. Not only are the programs ongoing, but any job-seeker can potentially join one and will eventually join one if he remains unemployed long enough. Those unemployed individuals who are not observed to go into any program have in fact waited long enough to enrol and found a job in the meantime. Although a non-standard one, this evaluation problem is quite commonly encountered,

\(^1\) Some measures date back to the early ‘30s. As to the recent scale of the programs, in 1997 the equivalent of 4.5% of the labour force participated on average in such measures (excluding those for the disabled), with government expenditure representing over 3% of GNP.

\(^2\) E.g. the New Deal for Young People program in the UK shares some of the features of the Swedish set-up and offers five types of ‘treatments’.
in particular in the evaluation of ongoing programs which individuals sooner or later will join provided they are still eligible (e.g. still unemployed).

Sianesi (2004) has proposed an evaluation question relevant for this set-up as well as a non-parametric approach to address it. The present paper implements this strategy in a multiple treatment framework. To mirror the relevant decision open to the job-seeker and the caseworker, the effects we estimate relate to the impact of joining a specific program at a given time in unemployment compared to not joining any program at least up to then.

In addition to policy and methodological interest, evaluations of the Swedish programs can benefit from exceptionally rich and highly representative administrative data sources by international standards. Often in the literature program effects are evaluated at a given – and arbitrary – point in time (e.g. on the last observation day, or after a year). By contrast, being able to follow up individuals for 5 to 6 years allows us to capture both short and long-term effects. In addition, not only does the data record the labour market history (to the day) of all unemployed individuals registered at public employment offices, but it also includes a wide array of demographic, human capital and labour market variables, as well as the caseworker’s time-varying subjective appraisal of various factors relating to the overall situation, character and needs of service of the job-seeker. The richness of the data has motivated the matching approach taken in this paper.

As to the specific details of the evaluation carried out in this paper, we now briefly outline the units, treatments and outcomes considered.

When looking at the relative effectiveness of one program compared to another, one needs to consider a group of unemployed job-seekers who, at least formally, could have chosen any of the measures under consideration. Focus of our analysis are individuals entitled to unemployment benefits. They enjoy not only special conditions (e.g. they are in principle granted the right to a program when approaching benefit exhaustion), but also exclusive access to one of the programs (work experience placement), which was specifically created for them. A natural question thus concerns the actual effectiveness of this measure. Further special policy interest in this group arises from the fact that up to February 2001 participation in a program would renew job-seekers’ eligibility to unemployment compensation. By considering individuals who are entitled to unemployment compensation we are thus focusing on that one group whose participation incentives are most likely to have been affected and for whom the trade-off between potentially productivity-enhancing components of the programs and the reinforced work disincentive associated with
the benefit system should have been at its sharpest. Sianesi (2001 and 2004), who devotes particular attention to exploring the linkages between program effects and entitlement status, in fact found entitlement to be a most prominent driving force behind subsequent treatment effects.

We focus on the six Swedish ALMPs open to adult individuals entitled to unemployment benefits at the height of the economic recession in 1994: labour market training, workplace introduction, work experience placement, relief work, trainee replacement and employment subsidies.

The differential performance of these programs is investigated both relative to one another and vis-à-vis more intense job search in open unemployment. For participants in a given program we thus estimate the effect of joining that program compared to joining an alternative program, as well as compared to waiting longer in open unemployment. Effects are evaluated month by month for 5 years since program entry. The explicit aim of the Swedish ALMPs is to improve the re-employability of unemployed workers; we thus focus on employment rates over time, summarising possible program effects on both job finding probability and survival in employment once in a job. This will allow us to investigate if and how the various programs differentially endow participants with skills and good working habits that enhance their employment prospects in the short- and long-run. Special attention is also devoted to the differential program impacts on individual benefit collection probability over time. This allows us to directly capture the influence that benefit renewability may have on the effects of the various programs.

The next section outlines the Swedish labour market policy and institutional set-up and describes the six programs being evaluated. Section 3 describes data and sample choice. Section 4 highlights the evaluation problem in a multiple-treatment framework and how it has been addressed in the Swedish context, as well as discussing the plausibility of the underlying identifying assumption. Section 5 presents the set of empirical findings, before concluding in Section 6 with a summary and overall appraisal of the results.

2. The Swedish labour market policy

The Swedish labour market policy has two main and interlinked components: a variety of active labour market programs and an unemployment benefit system.

The stated overall purpose of the Swedish labour market programs is to prevent long periods out of regular employment and to integrate unemployed and disadvantaged individuals into the labour force. There are various kinds of programs, some specifically targeted at particular groups
(e.g. the young or the disabled) and the rest open to anyone registered at an employment office.

This evaluation focuses on the six main programs open to adult unemployed workers in the mid-90s: labour market training, workplace introduction, work experience placement, relief work, trainee replacement and employment subsidies.\(^3\) Table 2.1 contrasts their main features.

To gain access to any program, one needs to be registered at a local official employment office. The six programs under consideration are additionally open to adults only (over 20 or 25), while work experience placement requires the individual to be entitled to unemployment compensation, and employment subsidies are targeted at the long-term unemployed. The latter may often be regarded as a mere guideline, though, since 20 percent of the employment subsidy participants in our data have spent less than the required six months in open unemployment prior to joining. All the individuals in our sample satisfy the eligibility rules in terms of registration, age and entitlement criteria, while we shall control very carefully for unemployment duration.

Whilst on a program, participants either earn the stipulated wage and other benefits on their ‘temporary’ workplace, or the equivalent of the unemployment benefit they would have drawn as openly unemployed. Most programs have a maximum duration of six months (under special circumstances renewable for another six), though participants stay an average of 4-5 months.

*Labour market training* (AMU), by far the most expensive measure, is intended to augment participants’ human capital with formal, full-time vocational\(^4\) teaching of new skills.

A second type of program offers workplace traineeship to maintain and enhance contact with working life and gain practical experience, good working habits and references from which to later benefit on the regular labour market. *Work experience placement* (ALU) was introduced at the deepening of the recession in 1993 with the explicit aim to prevent entitled individuals from exhausting their benefits. In fact, individuals need to be eligible to either UB or KAS to participate in this scheme, which can involve almost any kind of activity (the most frequent tasks being in administration and construction). *Workplace introduction* (API), which replaced a number of older job-experience programs, offers unemployed individuals a period of workplace training.

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\(^3\) Two programs are excluded from the analysis on the basis that they are targeted to (or attract) quite specific subgroups of unemployed individuals: self-employment grants (for those wishing to establish their own new business, with both a business idea and a financial plan approved by the offices) and vocational rehabilitation (for those with occupational disabilities needing specialised resources for in-depth counselling and job-preparation measures). Findings by Carling and Richardson (2001) do in fact support the view that participants in self-employment grants may have better employment prospects due to unobserved characteristics than participants in the other programs.
### Table 2.1 Synoptic table of the main features of the programs

<table>
<thead>
<tr>
<th>PROGRAM</th>
<th>AIM</th>
<th>ELIGIBLE</th>
<th>EMPLOYER</th>
<th>TRAINING</th>
<th>TASK</th>
<th>COMPENSATION(^a)</th>
<th>EMPLOYER INCENTIVES</th>
<th>COST(^b)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>EMPLOYMENT SERVICES</strong></td>
<td>fill job openings quickly, job search assistance and training</td>
<td>&gt;20</td>
<td>priv. and publ. providers</td>
<td>job seeker activities</td>
<td>UI/KAS if entitled</td>
<td>TA/BA course free</td>
<td></td>
<td></td>
</tr>
<tr>
<td><strong>LABOUR MARKET TRAINING (AMU)</strong></td>
<td>equip individuals with skills to find jobs more easily</td>
<td>&gt;20</td>
<td>priv. and publ. providers</td>
<td>vocational classroom training</td>
<td>TA/BA</td>
<td>free labour</td>
<td>13,940</td>
<td></td>
</tr>
<tr>
<td><strong>WORK PRACTICE</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Work experience placement (ALU)</td>
<td>prevent exhaustion of benefits while maintaining contact with the regular labour market and enhancing good working habits</td>
<td>entitled ≥20</td>
<td>90% public and non-profit</td>
<td>otherwise not performed</td>
<td>TA/BA</td>
<td>pay tuition to government (2,000 SEK/month)</td>
<td>9,294</td>
<td></td>
</tr>
<tr>
<td>Workplace introduction (API)</td>
<td>contact with working life to get workplace training, job-experience and references</td>
<td>≥20</td>
<td>private and public</td>
<td>practical vocational training</td>
<td>otherwise not performed</td>
<td>TA/BA free Labour</td>
<td>6,993</td>
<td></td>
</tr>
<tr>
<td><strong>TEMPORARY JOB</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Relief work</td>
<td>specially created temporary jobs to maintain working skills and habits, also to avoid benefit exhaustion</td>
<td>&gt;25</td>
<td>2/3 in public sector (municipalities and state organizations)</td>
<td>otherwise not performed</td>
<td>according to collective agreement</td>
<td>grant 50% of labour cost up to fixed amount (SEK 7,000/month)</td>
<td>9,201</td>
<td></td>
</tr>
<tr>
<td>Trainee replacement</td>
<td>enhance skills of employee while providing an unemployed individual with work experience in a regular job</td>
<td>≥20</td>
<td>80% in public sector</td>
<td>on-the-job practice replaces regular employee</td>
<td>according to collective agreement</td>
<td>grant 50% of labour cost up to SEK 7,000/month; deduction of training costs; educational grant up to SEK 20,000 per employee</td>
<td>7,665</td>
<td></td>
</tr>
<tr>
<td>Employment Subsidies</td>
<td>establish permanent employment relation</td>
<td>≥6m unempl. ≥20</td>
<td>private sector only; from 97 some industries excluded</td>
<td>on-the-job practice</td>
<td>normal</td>
<td>according to collective agreement</td>
<td>grant 50% of labour cost up to fixed amount (SEK 7,000/month)</td>
<td>5,968</td>
</tr>
</tbody>
</table>

Notes: Information has been gleaned from various sources, in particular, Swedish Institute (1997). \(^a\) TA is training allowance equivalent to the UI or KAS the individual would have been entitled to; BA is the basic amount (SEK 103 per day) if the individual is not entitled. \(^b\) Total monthly cost per participant (SEK); such information is from AMS (1998) and has been taken from Carling and Richardson (2001, Table 1).
A third kind of measure provides unemployed workers with a temporary job. Relief work involves specially created temporary jobs, mostly in the public sector. Though relief work is the oldest measure (dating back to 1933) for creating employment, it has diminished in importance during the 1990s, now being primarily used for individuals at risk of losing their unemployment benefits (Swedish Institute, 1997). In particular, unemployed UI fund members who run out of compensation are in principle granted the right to a relief job. In a trainee replacement scheme, an unemployed individual replaces a regularly employed worker who is on leave for education. This measure thus allows an unemployed worker to acquire valuable work experience, while creating an opportunity for firms to update the skills of their employees.

Finally, employment subsidies offer a temporarily subsidised job opportunity to acquire job-specific human capital while being also aimed at influencing an employer’s hiring process: the engagement is implicitly expected to continue after completion of the program.

While all the programs aim at improving participants’ employment prospects, they differ in the kind of skills they provide and in the way they provide them. At the one end of the spectrum, labour market training provides vocational classroom training of new skills deemed in demand. API has a strong emphasis on practical vocational training; similarly, ALU and relief work may provide participants with job experience and improve their working habits. Participants in these three programs are however prevented – at least formally – from performing tasks that a regularly employed individual would otherwise do. Although it is likely for such a rule to be often interpreted more as a recommendation than as a strict guideline, to the extent it is adhered to, the type of on-the-job practice acquired may not be expected to be particularly marketable.

Like the two work practice schemes and relief work, trainee replacement and employment subsidies offer the opportunity to invest in job-specific human capital; in these cases, though, the participant does in fact replace ordinary labour. Finally, while trainee replacement – a deputyship for the employee on study leave – is intrinsically a temporary opportunity to gain job-specific experience, employment subsidies, with the implicit agreement that the employer will then hire the individual on a regular and indefinite basis, almost entail the ‘promise’ of a permanent job.

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4 To reduce the heterogeneity in courses offered, the focus of this evaluation is on vocational training. Like Carling and Richardson (2001), we exclude participants in non-vocational courses, which are aimed at helping workers with basic educational insufficiencies to move on to further education or to other programs, rather than directly into a job.

5 Circumstantial evidence in Hallström (1994; reported in Ackum Agell, 1995) shows that all parties involved (sponsors, participants and the employment officers) believe that these projects often do replace jobs that are part of the organisers’ normal activity.
A final consideration relates to the first row in Table 2.1. Individuals searching for a job as openly unemployed can benefit not just from standard job information and matching of vacancies to applicants, but also from the ‘job-seeker activities’, which include search-skill-enhancing activities (e.g. training courses on how to apply for a job) and motivation-raising activities. In Sweden, the ‘no-treatment’ status to which program participation has to be compared to is thus not a complete absence of intervention, but these baseline services offered by the employment offices. In some countries this kind of assistance is in fact considered a program in its own right.\(^6\)

As to unemployment compensation, benefits are provided in two forms\(^7\), the most important one being unemployment insurance (UI). UI benefits are quite generous by international standards (daily compensation being 80% of the previous wage\(^8\)) and are available for a total of 60 weeks, more than twice the maximum duration in the US. To be eligible to UI an unemployed person registered at an employment office and actively searching for a job must have been working for at least 5 months during the 12 months preceding the current unemployment spell.\(^9\) Once receiving UI, an offer of ‘suitable’ work – or of a labour market program – must be accepted; refusal to accept a job/program might lead to expulsion from compensation (the ‘work test’).

The second form of unemployment compensation is KAS. This supplementary system, mainly designed for new entrants in the labour market who usually are not members of any UI fund, is roughly half as generous as UI, both in terms of amount and duration of benefits. Claimants are subject to a work condition similar to the one for UI, which can however be replaced by the education condition of having finished at least one year of school in excess of the 9 compulsory ones.

The passive and active components of the Swedish labour market policy used to be closely linked: up to February 2001, participation in a program for 5 months (or completion of a training course) would count as employment and thus qualify for a renewed spell of unemployment compensation. Even though the period during which a job-seeker can receive benefits was nominally fixed, it thus used to be possible to extend it indefinitely by using program participation to renew eligibility. Programs could thus actually end up reinforcing the work disincentives associated with the benefit system, this being especially the case for individuals entitled to benefits.

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\(^6\) An example is the Gateway period of the new UK New Deal program for the unemployed.

\(^7\) Individuals not entitled to any form of unemployment benefits may receive means-tested social insurance.

\(^8\) This maximum level of compensation has changed a few times during the 90s. The system also has a ceiling.

\(^9\) There is also a membership condition, requiring payment of the (almost negligible) membership fees to the UI fund for at least 12 months prior to the claim.
3. Data and sample selection

Our dataset is constructed from two main sources, reflecting the program component (Händel) and the benefit component (Akstat) of the labour market policy. Händel is the unemployment register and contains information on all unemployed individuals registered at the public employment offices. Available from 1991, this longitudinal event history dataset provides each individual’s labour market status information over time, together with important characteristics of the job-seeker and of the occupation sought. The information regarding the reason for ending the registration spell (e.g. obtained employment, gone on regular education or left the workforce) has been used to impute the individual’s labour market status in between registration periods. Akstat, available from 1994, originates from the unemployment insurance funds and records information – in particular on unemployment benefit receipt, previous wage and working hours – for individuals entitled to benefits.

As to sample choice, individuals need to be homogeneous in those basic characteristics which determine eligibility to all the programs under examination. Only then will it be relevant to examine their outcomes had they chosen a competing program. As motivated above, our choice is to focus on adults entitled to unemployment benefits. An additional advantage compared to non-entitled individuals is in terms of data quality and availability: since registration at an employment office is a pre-requisite for drawing benefits, our chosen sub-sample is a particularly representative one of the sub-population of interest. The information for benefit recipients is thus especially reliable, but also much richer, since it includes the Akstat data.

We focus on the inflow into unemployment in 1994, the year when the unprecedented recession that had hit the Swedish economy in the early 90s was at its most severe.\textsuperscript{10} We further restrict our sample to adult individuals who became unemployed for their first time\textsuperscript{11} in that year and were entitled to either UI or KAS. Additionally, individuals whose first program was start-up grants, vocational rehabilitation or non-vocational training are dropped from the analysis (see footnotes 9 and 11). These criteria lead to a sample of 30,800 individuals, followed from the

\textsuperscript{10} From less than 3% in 1989 and 1990, unemployment jumped to 9% in 1992, reaching its peak of 13.5% in 1994.

\textsuperscript{11} Since Händel starts in August 1991, strictly speaking we can only ensure that individuals registering in 1994 have not been unemployed at any time during the previous three years. Given however that it was exactly between these three years that Sweden experienced unprecedentedly high unemployment, the requirement is likely to be quite binding, making us reasonably confident that most of our individuals are indeed first-time unemployed.
moment they register in 1994 to the end of November 1999.\textsuperscript{12} Descriptive statistics of the various treatment groups are presented in Appendix A1. Visual inspection of average characteristics clearly shows that participants in the different programs are not a random sample from the population, but are in fact quite distinctive groups. There seem to be several demographic, human capital and labour market variables which systematically differ between the groups and which are most likely to affect subsequent labour market performance.

4. The evaluation problem in the Swedish institutional set-up

4.1 Evaluation questions

The Swedish institutional set-up poses a few interesting methodological issues which have to be resolved before deciding on the evaluation strategy. In particular one needs to be very explicit about the definition of the relevant treatments. While Section 2 has already described the six programs of interest, it has to be noted that these take place continuously over time and are open to all registered job-seekers; unemployed individuals are in fact often observed to participate in different programs at different times during their unemployment history. As our sample is one of first-time unemployed, we focus on the \textit{first} program individuals may receive within their first unemployment experience. Furthermore, since the observed program duration is endogenous\textsuperscript{13}, we start measuring the causal effect at entry into the program; any lock-in effect whilst on the program is hence viewed as a constituent part of the effect. The treatments are thus in terms of \textit{joining} a given program\textsuperscript{14} after having become unemployed (for one’s first time).

When evaluating the effect of a program on some outcome, an essential part of the research question concerns the comparison state. In addition to the effect of joining a given program compared to joining another one, interest often lies in assessing the effect of joining a given program

\textsuperscript{12} Some minor adjustments have been made to the data to deal with negative or short spells. As to negative durations, after correcting what clearly appeared to be mistakes, the history of the remaining 5700 individuals involved has been deleted from one spell before the negative one onwards. As to spells shorter than one week, two adjacent unemployment spells separated by a short break have been merged into one long spell. A similar adjustment has been made when an individual’s first period of registration at the employment office is a short non-unemployment spell immediately followed by an unemployment spell. Finally, an individual’s first program shorter than a week and followed by another program was merged to that subsequent program.

\textsuperscript{13} Some programs require participants to continue job-searching activities. The offices too continue to search for them, since participants are still registered and requested to be ‘at the labour market disposal’. Individuals are in fact required to drop out of a program if a ‘suitable’ job is found for them.
relative to no program participation at all. The definition of the ‘no-program’ group is however not straightforward in Sweden.\textsuperscript{15} An unemployed individual will, in principle, join a program at some time, provided he remains unemployed long enough. Indeed one could argue that the reason an unemployed individual has not been observed to go on a program is because he has found a job (before). In the Swedish institutional set-up the definition of non-participants cannot thus be the standard one, namely those individuals who are observed not to enter any program. Since such individuals would \textit{de facto} be observed to leave the unemployment register, this approach would amount to selecting a comparison group based on future (and successful) outcomes.\textsuperscript{16}

The program participation process in Sweden is such that once an individual has become unemployed, he and his case-worker are most likely to take their decisions sequentially over time in unemployment. In particular, at any given moment the relevant decision is not whether to participate in a program or not to participate \textit{at all}, but whether to join a program now or not to participate \textit{for now}, searching longer in open unemployment and knowing that one will always be able to join later on. Letting the parameter of interest mirror the key choice open to the eligible, we evaluate the average effect, for those observed to join a given program after a given number of months spent in open unemployment, of joining a given program when they did compared to further postponing the participation decision by not joining any program at least up to then.

We thus investigate the differential effectiveness of seven different types of treatments: joining one of six programs (labour market training, work experience, job introduction, relief work, trainee replacement, job subsidies) and searching longer in open unemployment.

\textbf{4.2 Evaluation approach in a multiple-treatment framework}

This sub-section sketches the framework developed by Imbens (2000) and Lechner (2001), which generalises Rosenbaum and Rubin’s (1983) potential outcome approach for the case of a single treatment to the case where a whole range of treatments is available.

Let a set of $K+1$ different kinds of mutually exclusive treatments be available to any given individual. In our case, the choice set of an unemployed individual contains $K=6$ types of programs as well as a ‘waiting’ option. Interest lies in the causal average effect of a treatment relative to another treatment on some outcome $Y$. A set of potential outcomes is correspondingly associated

\textsuperscript{14} In the literature this is often referred to as the ‘intention to treat’.

\textsuperscript{15} The discussion of an absent ‘no-treatment’ group was initiated by Carling and Larsson (2000a, b) and taken up in Sianesi (2001 and 2004).
to each of the $K+1$ states: $Y^0$, $Y^i$, …, $Y^K$, with $Y^k_i$ denoting the outcome $Y$ for individual $i$, if $i$ were to receive treatment $k$. Let $T \in \{0, 1, \ldots, K\}$ denote actual assignment to a specific treatment, so that $T_i = k$ if individual $i$ receives treatment $k$.\(^{17}\) Since each individual receives only one of the treatments, his remaining $K$ potential outcomes are unobserved counterfactuals.

A number of interesting parameters can now be defined (see Lechner, 2001), but in what follows the focus will be on the generalisation of the popular ‘effect of treatment on the treated’: the $(K+1)K$ pair-wise comparisons of the average effect of treatment $k$ relative to treatment $k'$ conditional on assignment to treatment $k$, for all combinations of $k$ and $k'$:

\[
E(Y^k - Y^{k'} | T=k) = E(Y^k | T=k) - E(Y^{k'} | T=k) \quad \text{for } k, k' \in \{0, 1, \ldots, K\}, \; k \neq k'.\(^{18}\)
\]

In our case, this amounts to assessing the average effect for an unemployed individual of participating in program $k$ compared to a hypothetical state in which he received treatment $k'$.

The first term, the average outcome following treatment $k$ for individuals who have participated in $k$, is observed in the data. This is however not the case for all the counterfactuals of the type $E(Y^k | T=k)$, i.e. all the outcomes participants in $k$ would have experienced, on average, had they taken any treatment other than $k$. Identifying assumptions thus need to be invoked to overcome the fundamental missing data problem that only one of the $K+1$ potential outcomes is observed for any given individual.\(^{19}\) One such assumption often invoked in evaluation exercises is the conditional independence assumption, an extension of which (termed ‘strong unconfoundedness’ by Imbens, 2000) would allow us to identify all the counterfactuals:\(^{20}\)

\[
T \perp (Y^0, Y^i, \ldots, Y^K) | X=x, \; \forall \; x \in C^* \quad (1)
\]

It requires that conditional on the value of a set of observable characteristics $X$ (themselves unaffected by the treatments), the treatment indicator $T$ is independent of the entire set of potential outcomes (over the set $C^*$ of $X$ values for which the treatment effect is defined).

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\(^{16}\) Very recent work by Fredriksson and Johansson (2003) formalises this intuition.

\(^{17}\) For this representation to be meaningful, the stable-unit-treatment-value assumption (SUTVA – Rubin, 1980) has to be fulfilled, requiring treatment status as well as all the potential outcomes of a given individual to be independent from the treatment status of others, the latter ruling out the possibility of general equilibrium effects.

\(^{18}\) In general this parameter is not symmetric: $E(Y^k - Y^{k'} | T=k) \neq -E(Y^{k'} - Y^k | T=k')$ if participants in the two programs systematically differ in characteristics related to the outcome.

\(^{19}\) Identification assumptions and estimation of treatment effects in non-experimental studies have been extensively looked at. Standard references in the evaluation literature include the comprehensive survey by Heckman, LaLonde and Smith (1999), as well as Heckman and Robb (1985), Heckman, Ichimura and Todd (1997, 1998), Heckman, Ichimura, Smith and Todd (1998), Rosenbaum and Rubin (1983, 1985) and Dehejia and Wahba (1999).

\(^{20}\) Its weaker form in terms of conditional mean independence would suffice.
A weaker form would though suffice to identify the conditional treatment effects of interest:

$$T \perp (Y^k, Y^{k'}) \mid X=x, \forall x \in C^*, T \in \{k, k'\} \quad \text{for } k, k' \in \{0, 1, \ldots, K\}, \; k > k'$$ (2)

Being just interested in the pair-wise comparison of the various kinds of treatments, we can relax (1) by requiring the conditional independence to hold only for the sub-populations receiving either treatment $k$ or treatment $k'$: all the (outcome-relevant) differences between individuals choosing treatment $k$ and those selecting into treatment $k'$ need to be captured by variables the evaluator can control for (cf. Lechner, 2001). The plausibility of (2) in our application is discussed in the next section. The unobserved counterfactuals can thus be identified as:

$$E(Y^{k'} \mid T=k) = E_X[E(Y^{k'} \mid T=k, X) \mid T=k] = E_X[E(Y^k \mid T=k', X) \mid T=k]$$

where the inner expectation is identified due to (2) and the outer expectation is taken with respect to the distribution of $X$ for participants in $k$.

The latter highlights how in order to adjust for differences in $X$, sufficient overlap is required in the distribution of $X$ by treatment status. In particular, all participants in $k$ need to have a counterpart in the $k'$-group for each $X$ for which we seek to make a comparison. If there are regions where the support of $X$ does not overlap for the two groups, matching has to be performed over the common support region\(^{22}\); the estimated treatment effect has then to be redefined as the mean effect for those treated $k$ falling within the common support.

Formally, define the (generalised) propensity score as the conditional probability of receiving a given type of treatment given $X$ as $P^k(X) \equiv Pr(T=k \mid X)$.

The common support requirement for all pair-wise conditional parameters then translates into:

$$0 < P^k(X) < 1 \quad \text{for } X \in C^* \quad \text{and } k=0, 1, \ldots, K.$$ (3)

By choosing and re-weighting observations within the common support, matching methods are able to eliminate two of the three potential sources of bias identified by Heckman, Ichimura, Smith and Todd (1998): the bias due to the difference in the supports of $X$ in the treated and non-treated groups and the bias due to the difference between the two groups in the distribution of $X$ over its common support. Matching is however based on identifying assumption (2), which assumes away the third potential source of bias, namely selection on unobservables. The following

\(^{21}\) Again, the requirement could just be in terms of conditional mean independence.

\(^{22}\) Alternatively, identification would rely on (parametrically) extrapolating from regions of $C^*$ that have positive probabilities for both the treatment states being compared to occur.

\(^{23}\) To just compare treatment $k$ with $k'$ for participants in $k$, one would need to have some participants in $k'$ with those $X$’s at which there are participants in $k$, i.e. $P^k(X)>0 \forall X \in C^*$: $P^k(X)>0$. 


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discussion makes a case for assumption (2) to represent a credible approximation and thus for matching to be considered a feasible strategy in our informational and institutional setup.

### 4.3 Plausibility of the matching assumption

Assumption (2) requires us to observe (and thus match on) all those differences between the various treatment groups that are likely to affect their outcomes. We thus need detailed knowledge of the factors that drive participation, as well as access to data suitable to capture those determinants of participations that are also relevant to outcomes. In our application, not only do we have access to detailed background information (including several direct indicators of individual heterogeneity), but the choice of the relevant conditioning variables can benefit from the results of a Swedish survey directly asking job-seekers and placement officers about their participation decision criteria (Harkman, 2000, as reported in Carling and Richardson, 2001). What we need to consider is whether in deciding about participation these agents have access to and act upon information correlated with labour market performance but that we cannot capture in our data.

It is useful to separately consider (A) the decision between waiting further in open unemployment or joining a (i.e. any) program and (B) the decision to choose one specific program.

For decision (A) we need to control for all variables that, conditional on having reached a given unemployment duration, influence both the decision to join a program as well as potential future labour market performance were such decision to be postponed further. From Harkman (2000) it appears that job-seekers largely base their decisions to participate in any program or not to participate on their subjective likelihood of employment. In so far as individual perceptions are accurate enough, this subjective assessment will reflect actual potential employment outcomes. It is thus crucial to identify enough information apt to capture these individual perceptions. We accordingly control for a whole set of variables intended to characterise the individual’s past employment history as well as his current employment prospects, including his assessment thereof.

The only unemployment experience of individuals in our sample relates to the present unemployment spell, a fact which is greatly informative of their labour market history. Entitlement status further reflects a certain degree of labour market attachment due to the work requirement UI-recipients have to fulfil.24 Previous working hours (a proxy of the extent of past labour market involvement) and the pre-unemployment wage (conditional on qualifications, a summary statistic

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24 An indicator of KAS entitlement controls for the alternative way to fulfil the ‘work’ requirement.
of individual productivity) are additional important individual attributes which characterise the worker’s overall earlier labour market situation. As to present employment prospects, we control for elapsed unemployment duration, benefit receipt status, demographics, several dimensions of human capital and a number of direct indicators of individual heterogeneity.

In particular, individual perceptions about one’s employment likelihood will probably change over time spent in unemployment. Elapsed unemployment duration should thus capture important unobservables in this dimension; more generally, in the presence of duration dependence and/or unobserved heterogeneity, it is crucial to ensure that the comparison individuals have spent in unemployment at least the time it took the participants to join. Also note that elapsed unemployment duration is an important $X$ variable for directly explaining the joining decision, given some (albeit loose) regulations (e.g. for job subsidies), as well as incentives related to unemployment benefits running out. As to the latter, for a given elapsed unemployment duration we additionally control for benefits expiring at that time or for benefits having effectively run out by then. A binding ceiling in terms of compensation amount finally controls for job-seekers effectively facing a compensation rate lower than 80%.

Demographic characteristics such as age, gender and citizenship, as well as the occupation being sought are also important determinants of labour market prospects. Part-time unemployment spells denote individuals who are still maintaining contact with the regular labour market and are probably both subject to less human capital depreciation and in a better position to look for a (full-time) job, by exploiting their bargaining position, additional contacts and references.

Human capital information is available in terms of both specific and general education and occupation-specific experience. The latter is a subjective indicator of the amount of experience for the job being sought. It can be viewed as a summary statistics of the amount, effectiveness, transferability and obsolescence of previous human capital accumulation, on-the-job training and learning-by-doing, but also – together with the subjective indicator of education for the profession sought – as a self-assessment by the unemployed individual of the strength of his own chances of re-employment.

Finally and most crucially, we exploit several direct indicators of individual heterogeneity likely to be highly relevant in terms of employment prospects. Specifically, we have retrieved

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25 Elapsed unemployment duration is only an imperfect predictor of benefit receipt, since job-seekers may decide to save their benefits for later, be suspended etc.
information as to an overall evaluation by the caseworker of the situation, character and needs of service of the job-seeker. This assessment relates to the job-seeker’s degree of job readiness (if judged to be able to take a job immediately, to be in need of guidance, or to be difficult to place), as well as to the job-seeker’s preferences, inclinations and urgency to find a job (if willing to move to another locality, if looking for a part-time job, if already having a part-time job). We also exploit a summary statistic directly capturing selection into the programs (if the job-seeker has been offered a program and is waiting for it to start). Note in particular that the caseworker may update and revise this subjective judgement during his client’s unemployment spell. This time variation in the assessment of the prospects and needs of the job-seeker is an additional key feature we can exploit to control for the program joining decision over time in unemployment.

Another way to view condition (2) is that individuals are myopic conditional on observables: given $X$, outcome-related information about the future should play no role in individual decisions to join a program at $u$ or to else wait longer. The discussion of individually perceived employment prospects as the prime determinant of the program joining decision has thus to also consider the possibility of anticipatory effects in terms of future employment. In particular, if some job-seekers know that their former employer is going to call them back (e.g. they are seasonal workers, or have a credible agreement with their employer allowing them to temporarily collect unemployment benefits), they are likely to have fewer incentives to participate in the programs at any given month in unemployment; at the same time, they are observed to actually find employment. Additional observables included to control for potential anticipatory effects of this kind include the occupation/skill type of the job-seeker, as well as the month of registration, which should help capture seasonal unemployment. More generally, though, (2) would be violated if an individual waiting longer has decided to do so because he has received a job offer and hence knows that he will be hired shortly. How serious this issue is going to be in our case thus largely depends on the typical time span between job offer and job commencement (and whether or not an individual who is going to start a job typically remains or is allowed to remain registered at the unemployment office in the meantime). Note also that if the time of job commencement is not too near, a caseworker’s decisions may provide additional randomness in program participation patterns, since for entitled individuals the proposal of a program can be used as a ‘work test’, whereby refusal to participate may entail suspension from benefits.

Our evaluation question concerns the effect of joining a given program at a given time com-
pared to later or never, thus requiring (2) to also hold in terms of future program participation, viewed as an outcome. Controlling for elapsed time spent in unemployment is once again crucial, in that approaching benefit exhaustion would make an individual more likely to join a program or, if having to wait longer, more likely to enter a program later on or to intensify job search (or lower one’s reservation wage). Again it is important to consider the possibility of anticipatory effects, in that (2) would be violated if an individual decided not to participate at time \( t \) because he knows that he will join at some later date. This is not very likely.\(^{26}\) Additionally, as mentioned above, a very interesting piece of information in the data is an open unemployment sub-spell where the job-seeker is waiting to enter a labour market program. Having gone through the assignment process and having been offered a place makes it more likely for the individual to join a program rather than waiting; had he not joined now, he would be more likely to join later on or to decrease his job search in anticipation of joining. Like the caseworkers’ subjective judgements, this offer (or waiting for a program) status changes over time in unemployment.

Turning now to decision (B), namely the choice of one among the available programs, condition (2) requires the evaluator to have access to all the variables that jointly influence such a choice and potential future outcomes. Note first that all our individuals have access to the same choice set, the only relevant recommendation being the one requiring a certain length of the unemployment period prior to enrolment, for which we carefully control. Benefit renewability rules and individual compensation while on the programs are similarly comparable across programs.

Harkman (2000) finds that individual self-selection into different programs is likely to be a minor issue in Sweden, in that unemployed workers tend to value the various programs equally. By contrast the caseworkers do seem to have clear ideas about which type of program is suitable for their clients, based on individual characteristics. Since the relevant decision-maker thus appears to be the caseworker, the only issue we need to focus on is whether he acts upon information which is unobserved to us yet correlated with clients’ potential outcomes.

We do however observe not only important characteristics of the unemployed client (e.g. educational qualifications for possible cream-skimming for training programs), but also the case-

\(^{26}\) The institutional nature of the program system (a seemingly continuous flows of different programs often on an individual, \textit{ad hoc} basis) should make it less likely for an unemployed job-seeker to have to turn down a program offer perceived as second-best in order to wait for a free slot on his first-choice program (this would also reduce the likelihood of an ‘Ashenfelter dip’ problem in terms of reduced job search prior to participation). Even if he did wait, though, he would not enter his first-best program with certainty, but would still be exposed to the possibility of finding a job or deciding (or be forced) to join another program in the meantime.
worker’s own subjective, synthetic and evolving evaluation of the overall situation and needs of service of his unemployed client as described above. In a sense, the caseworker reveals, updates and records in the data a synthetic appraisal of various factors, including some which may have been originally unobserved to us. Our assumption then translates into the requirement that caseworkers act idiosyncratically given worker characteristics and their own assessment of their client. Carling and Richardson (2001), who carefully examine the factors that determine which program the job-seeker ends up joining, do in fact provide reassuring evidence that the administrative selection process appears to be unrelated to the outcome.

A final issue relevant for both decisions (A) and (B) relates to the local labour market conditions, identified in the literature as a key variable to be controlled for (Heckman, Ichimura and Todd, 1997). In Sweden it would seem in fact very important to satisfy this requirement. County labour boards have the overall responsibility for labour market policy in each respective county, and from the mid 90s municipalities have become increasingly involved in the decision-making as to ALMPs. This shift towards decentralisation has given rise to new financial incentives (Lundin and Skedinger, 2000). In particular, municipal budgets may be favourably affected by moving the unemployed from social assistance (funded by the local authorities) to programs (financed by the central government); some programs (e.g. relief work) may subsidise labour in the services typically provided by the local authorities; and programs may serve as a means of maintaining the local municipal tax base by reducing geographical mobility among job-seekers. It is thus quite possible that counties or municipalities facing different labour market conditions may favour a different balance between program and unemployment policies, as well as a different mix of programs. As to the latter, Carling and Richardson (2001) do in fact find that a job-seeker’s observed characteristics have much less influence than his employment office affiliation in determining which program the individual is allocated to.

In addition to county dummies, we have thus constructed the local ‘program rate’, given by the number of participants in the six programs as a proportion of all individuals registered (as openly unemployed or program participants) at the individual’s municipality. This time-varying indicator provides information as to the local program capacity (e.g. in terms of slots available). Similarly, a series of time-varying single ‘program ratios’ at the municipality level, given by the share of participants in a given program over all participants in the six programs, is meant to reflect the program mix at that municipality and time. Such indicators are intended as a parsimoni-
ous way\textsuperscript{27} to capture unobserved local aspects which are likely to be relevant for program joining/choice decision and individuals’ potential labour market performance.\textsuperscript{28}

In sum, for individuals who have reached the same unemployment duration and who are similar in terms of all the individual and local characteristics described, the decision to join a program at that time rather than at least not yet, or the decision to join a given program rather than another need to be random, that is they depend on factors unrelated to future potential outcomes. Sources of this required random variation conditional on our $X$’s can stem from job-seekers’ idiosyncratic preferences or random variation in their outlook on their employment prospects at a given time. On the placement officer’s side, for given client characteristics, for given own judgement as to the client’s job readiness at a given time and for given employment office incentives regarding participation at that time, this randomness can be based on caseworkers’ idiosyncratic preferences, incentives and experiences, as well as propensity (and strictness) to apply the work test. One key point is that we can also exploit bottlenecks in the system, since we are able to condition on whether an individual has been offered and is waiting for a program, but cannot yet join, e.g. due to start dates of a training course, of a work-experience project, of an employee taking leave for a trainee replacement scheme, etc. A similar lack of appropriate conditions related to a program may also provide randomness in the decision to join a program rather than another one.

### 4.4 Balancing scores and implementation

An important practical result by Rosenbaum and Rubin (1983) for the single treatment case $T \in \{0,1\}$ is that the propensity score $Pr(T=1|X)$, a scalar giving the probability of being treated conditional on $X$, provides a parsimonious way to adjust for differences in the (generally large) full set of characteristics $X$ between treated and non-treated groups, formally: $T \perp X | Pr(T=1 | X)$.

More generally, a balancing score $b(X)$ is a function of $X$, such that conditional on it, the characteristics $X$ are ‘balanced’ across the treatment groups, i.e. $T \perp X | b(X)$. A necessary and suffi-

\textsuperscript{27} There are 289 municipalities and 484 employment offices in our data.

\textsuperscript{28} The municipality program capacity at a given time may affect the possibility for a job-seeker to join a program at that time as well as the scope for caseworker program selection, while offices facing more unfavourable local conditions may be more active in placing individuals on programs (e.g. to lighten the burden on the municipal budget or to decrease the number of openly unemployed in the municipality). Similarly, the program mix may affect program choice, while offices facing high unemployment may favour different kinds of program, e.g. the cheapest, or those which subsidise the municipality.
cient condition for a function of $X$ to be a balancing score is to be at least as fine as the (generalised) propensity score $P^k(X)$:

$$E[Pr(T=k \mid X) \mid b(X)] = Pr(T=k \mid X) \equiv P^k(X) \quad \text{and} \quad 0 < P^k(X) < 1 \quad \text{for} \quad k=0, 1, \ldots, K.$$ 

Since we are however just interested in the separate pair-wise comparisons of the various treatments, we need to find a balancing score ensuring the balancing of the $X$’s in the two sub-populations of interest for each separate comparison. For $k$ and $k'$ we need to find a $b(X)$ such that

$$T \perp X \mid b(X), T \in \{k, k'\}$$

or equivalently such that

$$E[Pr(T=k \mid X, T \in \{k, k'\}) \mid b(X)] = Pr(T=k \mid X, T \in \{k, k'\}) \equiv P^{k|kk'}(X) \quad \text{and} \quad 0 < P^{k|kk'}(X) < 1.$$ 

In our case of separate pair-wise comparisons of the various treatments, the conditioning variable (balancing score) of minimal dimension which ensures the balancing of observables in the two sub-populations of interest $k$ and $k'$ is thus still given by a scalar, the conditional choice probability of treatment $k$ given either treatment $k$ or $k'$:

$$P^{k|kk'}(X) = \frac{Pr(T=k \mid X)}{Pr(T=k \mid X) + Pr(T=k' \mid X)} \equiv \frac{P^k(X)}{P^k(X) + P^{k'}(X)}$$

Under (2), the required counterfactual can then be estimated as

$$E(Y^{k'} \mid T=k) = E_{p^{k|kk'}}[E(Y^{k'} \mid T=k', P^{k|kk'}(X)) \mid T=k].$$

One way to apply such results is to control for systematic differences in observed characteristics by matching participants in $k$ to individuals receiving $k'$ based on a balancing score $b(X)$. For any pair of treatments $k$ and $k'$, under assumption (2) the average outcome experienced by the matched pool of $k'$-participants identifies the counterfactual outcome participants in $k$ would have experienced, on average, had they taken treatment $k'$ instead.

Both the identification conditions and the balancing scores have been defined just taking account of the two sub-samples participating in the two treatments which are the object of a given comparison, de facto ignoring the multi-program nature of the environment the individuals face. As Lechner (2001) clearly points out, when interested in comparing two programs for participants in one of those two, the existence of multiple treatments can in fact be ignored, since individuals who do not take part in either program considered are not needed for identification.

However, estimation of the treatment probabilities offers an opportunity to capture and take account of the multiplicity of options open to individuals. In the Swedish context we argued

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above that it is also important to model the month-by-month decision-making process of the individual/caseworker. A way to accomplish this is to model the effect of unemployment duration (as well as of both fixed and time-varying characteristics) on the various options open to an individual at any given point of time. In particular, all our individuals start by registering as (first-time) unemployed. At any given point \( U=u \) in their first unemployment spell (our empirical units will be months), they can ‘decide’ between a set of 11 exhaustive and mutually exclusive options: to participate in one of the six available programs, to continue searching for a job full-time as openly unemployed, to find (or decide to accept) a job, to go on education in the regular system, to leave the labour force through another channel, or to drop out of the unemployment register for reasons unknown to the officials. By modelling the effect of unemployment duration on exit type, one can thus simultaneously take account of the various exit routes from unemployment, of right-censoring and of the effect of time-varying characteristics on individual choices.

As to the practical implementation, each individual unemployment spell is split into monthly spells. Each of these new sub-spells is characterised by the duration month \( u \) the new sub-spell refers to, by a corresponding treatment indicator and by those characteristics pertaining to, and events taking place during that \( u^{th} \) month of unemployment. The probability for an individual with characteristics \( x \) of choosing option \( k \) after having spent \( u \) months in unemployment, \( \Pr(T=k|u,x) \), is then estimated\(^{31} \) and the corresponding balancing scores \( b(X) \) constructed.

Note that all we are interested in (ignoring potential efficiency considerations) is achieving (3), i.e. \( T \perp X | b(X) , T \in \{k,k'\} \). Our interest in the estimation of \( b(X) \) as well as in the choice of matching method (which determines how the conditioning on \( b(X) \) is performed) purely lies in their combined ability to balance the characteristics of the matched sub-groups being pair-wisely compared. The resulting quality of the matched samples has thus guided the choice, for each pairwise comparison, of the specification for \( b(X) \) and of the matching estimator (cf. Appendix A2). To adjust for the additional sources of variability introduced by the estimation of the balancing score and by the matching process itself, bootstrapped confidence intervals have been calculated.

\(^{30}\) Cf. also Brodaty, Crépon and Fougère (2000).

\(^{31}\) This can be thought of as a discrete-time, competing-risks hazard model.

The conditioning set of observables \( X \) denotes fixed individual characteristics as well as time-varying characteristics both of the individual and of the macro local conditions he faces, as discussed in Section 4.3. Time-varying observables other than elapsed unemployment duration \( U \) are defined conditional on \( U_i \) or on calendar time. They include two main sets of controls: those relating to the unemployment experience of the individual so far (i.e. up to \( U=u \)) and those capturing the local conditions prevailing at \( U=u \) at the individual’s employment office.
To compare program \( k \) and program \( k' \) for participants in program \( k \), each \( k \)-participant is matched to one or more \( k' \)-participants based on the balancing score. The differential performance of the two matched groups starts being evaluated from entry into the respective program.

As to the average effect, for participants in a given program \( k \), of joining program \( k \) compared to waiting longer in open unemployment, we here outline the approach developed and formalised in Sianesi (2004). First the corresponding balancing score is calculated for each \( k \)-participant and each waiting spell. Participants in \( k \) and waiting individuals are then stratified by unemployment duration \( U = 1, 2, \ldots, U_{\text{max}(k)} \). For a given \( u \), those \( k \)-participants who enter the program in their \( u \)-th month are matched to the most similar individuals who are still unemployed after \( u \) months. Under the CIA the subsequent differential performance between the two matched groups identifies the average effect of joining program \( k \) in one’s \( u \)-th month of unemployment compared to not joining any program at least up to one’s \( u \)-th month. This effect starts being evaluated from entry into program \( k \), namely from \( U = u \). Note that as the comparison group comprises individuals who are unemployed up until that time and do not participate in any program at least as yet, it does not reflect a no-program state, but rather a possibly postponed participation. Under the CIA, the probability distribution of subsequent outcomes, \textit{including} participation in the programs, for the matched comparisons openly unemployed at \( u \) is the same as the one for the observably-similar \( k \)-participants had they at time \( u \) decided to wait longer as well.

For reasons of presentational parsimony, one can finally aggregate all the \( U_{\text{max}(k)} \) effects of program \( k \) by time of entry, weighted according to the observed entry distribution into program \( k \).\textsuperscript{32} This allows one to gain a synthetic overview of the general patterns of the effects of program \( k \) by month of placement. We will however also investigate whether and how the effect of a given program differs according to the time participants have spent in unemployment before joining.

5. Empirical findings

We start in Section 5.1 with the effects of joining a given program vis-à-vis more intense job search in open unemployment. The various effects by month of placement are first summarised in an overall average to highlight their general patterns and trends over time. They are subsequently separately discussed to explore the extent to which the effects vary for the groups of participants
who joined the program after different amounts of time. Section 5.2 then considers the pair-wise program effects. Section 5.3 finally performs an additional set of sensitivity and bounds analyses to assess the robustness of the estimated employment effects to the problem of a partly unobserved outcome variable arising from an attrition/misclassification problem in the data.

Outcomes of interest are individual employment and benefit collection probabilities over time. In particular, the effect of joining program $A$ (compared to either joining another program or searching longer in open unemployment) on the employment probability\footnote{Table A2 in the Appendix shows our sample’s entry distribution into the six programs by month in open unemployment. ALU and API stand somewhat apart in that entry is much more concentrated around UI exhaustion.} of participants in program $A$ is calculated from program start to 5 years on and summarises various components: a ‘lock-in’ effect, an effect on the probability of finding a job and an effect on job longevity.

The differential lock-in effect of a program vis-à-vis the comparison treatment originates from a differential job search whilst on the program. Compared to open unemployment, job search is reduced because less time is left due to participation itself. Different programs may however also differentially reduce the intensity of job search whilst participating: they may for instance leave different amounts of time and energy for job search or may entail different ‘promises’ once completed (e.g. job subsidies may induce participants to focus on the job at hand to ‘impress’ the employer in order to increase their chances of remaining with the firm afterwards).

Differential effects on job-finding probabilities may originate from various channels: job search being improved (e.g. \textit{via} contacts and references from an employment program) or being more intense (e.g. while in full-time open unemployment); the acquisition of new marketable skills making the working option more attractive and/or the individual more in demand (e.g. \textit{via} training); or the revelation of previously unknown individual productivity to temporary or potential employers (e.g. on a trainee replacement scheme or job subsidy program).

Lastly, a differential degree of job attachment may result from the different extent to which the programs improve participants’ working habits, skills, adaptability or ability to learn on the job.

Our second outcome of interest is the individual probability of being drawing unemployment compensation over time. For our sample of individuals entitled to unemployment benefits, the eligibility-renewability property of the programs represents a particularly attractive feature likely to affect incentives. Different programs may be differentially used as vehicles to renew eligibil-
ity, or may differentially lock-in participants in the unemployment system. Of particular interest in this regard is a comparison of the two work practice measures, ALU and API.

5.1 Joining versus waiting

The effect of joining a given program, compared to longer job-search as openly unemployed, on their respective participants’ employment prospects over time is shown in Figure 5.1.

All the programs considered are found to have a negative impact in the short-term. Joining any of these programs initially locks participants in, reducing their chances of being in employment by between 15 and 25 percentage points in each case.

The more medium and longer term effects of joining a program are however found to radically vary according to which program the individual has joined.

Entering a job subsidy program rather than searching further in open unemployment significantly pays off in terms of persistently higher employment rates soon after the program typically ends (35 percentage points) and up to five years on (20-25 percentage points). Participants in trainee replacement benefit considerably less from having joined their program; after the initial lock-in phase, they have a mostly significant, 4-7 percentage points higher employment probability over time than if they had waited longer. The positive effect for participants in API is even smaller in magnitude and bordering significance only in the medium term, with API participants only just 2-3 percentage points more likely to be employed than if they had waited further.

By contrast, for our sample of entitled unemployed adults it seems more worthwhile to intensively search longer in open unemployment rather than joining labour market training, ALU or relief work. In fact, even after the program typically ends, participants in these programs subsequently enjoy significantly lower employment rates than if they had postponed their joining decision further. For participants in training the effect remains significantly negative for over 4 years before disappearing, and for participants in ALU it persists for 2 and a half years. Finally, participants in relief work remain 4-8 percentage points less likely to be employed over practically the whole observation period. These three programs do not thus seem to provide participants – and especially participants entitled to unemployment benefit – with skills marketable enough to make the working option sufficiently attractive. One possibility is for these programs to be typically used to renew eligibility and so remain within the unemployment system.
Figure 5.1 Treatment effect on employment probability of joining a given program compared to waiting longer in open unemployment, over time and averaged over 5 years (% points)

Notes: Time in months, from program start. a Employment probability obviously refers to a regular (i.e. non-subsidised) job. 95 percent bias-corrected percentile bootstrapped confidence intervals (500 reps). ** significant at 1%, *** at 5%, * at 10%.
Figure 5.2 Treatment effect on benefit collection probability of joining a given program compared to waiting longer in open unemployment, over time and averaged over 5 years (% points)

Notes: Time in months, from program start.

**95 percent bias-corrected percentile bootstrapped confidence intervals (500 reps).**

*Significant at 1%, ** at 5%, * at 10%.*
This supposition seems in fact confirmed by the various program effects on the probability of collecting benefits over time (Figure 5.2). Participants in labour market training, ALU and relief work all have a significantly higher likelihood of compensated unemployment over time than if they had waited longer in unemployment; benefit renewability considerations thus appear to play an important role in the above finding of a negative treatment effect on employment rates for these measures. Further evidence of the likely failure in the way incentives are taken into account by the intertwined unemployment benefit-program institutional system is the finding that participants in these three programs appear to be drawing benefits on a ‘cycling’ basis. In particular, after the benefit-renewing duration of 5 months on their program, training participants are 20 percentage points, ALU and relief work participants around 40 percentage points more likely to be drawing compensation than if they had postponed entering their respective program. This treatment effect then remains positive until around month 20 – that is, for the maximum period of compensated unemployment of 14 months. After another 5 months – likely to be spent on another eligibility-renewing program – these participants become again significantly more likely to be drawing benefits, a treatment effect which lasts for another 14 months of maximum compensation. Over our observation window, even a third hump is recognisable.

API too is found to have a strong positive effect on subsequent benefit collection probability; this program did not however negatively affect participants’ employment prospects. Joining a replacement scheme increases the likelihood of compensated unemployment by around 10 percentage points after the initial 5 months and up to one a half years, after which the treatment effect declines to zero and insignificance. Finally, job subsidies is the only program to display a negative, at times significant effect (around -2 percentage points) on benefit collection probability over time compared to postponing the participation decision.

To summarise, if we consider the treatment effects averaged from time of entry into the program to 5 years on, the employment prospects of those unemployed who join labour market training, relief work and ALU are all reduced by around 6-7 percentage points. API and trainee replacement participants are not significantly affected in this dimension, whilst participants in job subsidies enjoy a 19 percentage points higher employment probability overall. These latter participants are also the only ones to subsequently display a significantly lower benefit collection probability (-3 percentage points) than if they had waited further in open unemployment. By con-

34 Note that compensation whilst on a program is not defined as unemployment benefits.
trast, while trainee replacement participants are overall 3 percentage points more likely to be drawing benefits over our 5-year observation window, the overall effect is a 6.7 percentage points higher compensation probability for participants in labour market training and as high as 10-11 percentage points for participants in API, ALU and relief work.

**Treatment effects by month of placement**

The time series and averaged effects discussed above offer a synthetic overview of the general patterns of the effects of entering a given program (compared to waiting longer in open unemployment) by month of entry. It is interesting to investigate whether and the extent to which these overall average patterns vary for the various sub-groups of participants who joined their respective program after a different amount of time spent in open unemployment.\(^{35}\) We thus divide participants in the six programs into groups based on the observed entry distribution into the respective program by month in unemployment (see Table 5.1).\(^{36}\)

As a measure of the treatment impacts on employment and benefit collection probability, the two panels of Table 5.2 present the average of the respective month-by-month effects over the 5-year horizon since program start.\(^{37}\)

The persistence of the negative lock-in effect on employment uncovered for labour market training in Figure 5.1 proves to be most pronounced for those individuals joining this program upon entry into unemployment and within half a year. These participants are possibly rushing the choice of an appropriate type of program as well as foregoing initial job offers they would have received had they waited longer in open unemployment. By contrast, the overall employment prospects of later joiners are not significantly affected by having joined training compared to a situation where they would have further postponed their joining decision. In terms of benefit collection probability, however, all groups are significantly and positively affected (6-7 percentage points higher probability), the notable exception being those entering in months 13 to 16.

---

\(^{35}\) Differential impacts by time of placement may arise from various sources. The extent of the initial lock-in effect may vary. Program effects may also depend on participants’ characteristics and individuals joining at different times may differ in such characteristics. Employers may also attach different value (or stigma) to participants who have joined a program at different times in their unemployment spell.

\(^{36}\) To obtain reasonable sample sizes for the treated groups to be separately analysed, 3 to 4 months had to be aggregated. Due to this aggregation, this is a slightly different procedure from the one used to derive the ‘overall’ effects above, or the effects by month of placement in Sianesi (2004). In particular, comparison units for, say, entrants between months 1 and 3 are now constrained to be unemployed for at least 3 months. While the effects in Table 5.2 are thus slightly more favourable to participants, the focus is here on comparing the effects for different entrant groups.

\(^{37}\) The full time series of the two treatment effects by month and program are available upon request.
**Table 5.1** Participants’ distribution by time of placement: Total number and percentage of which has joined at different months

<table>
<thead>
<tr>
<th></th>
<th>Total No.</th>
<th>$m = 1\text{-}3$</th>
<th>$m = 4\text{-}6$</th>
<th>$m = 8\text{-}10$</th>
<th>$m = 11\text{-}12$</th>
<th>$m = 13\text{-}16$</th>
<th>$m = 17\text{-}20$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Training</td>
<td>1,384</td>
<td>22.0</td>
<td>27.4</td>
<td>19.7</td>
<td>7.1</td>
<td>11.2</td>
<td>3.3</td>
</tr>
<tr>
<td>ALU</td>
<td>2,973</td>
<td>12.0</td>
<td>11.2</td>
<td>12.2</td>
<td>9.0</td>
<td>34.4</td>
<td>9.4</td>
</tr>
<tr>
<td>API</td>
<td>425</td>
<td>2.1</td>
<td>3.3</td>
<td>5.9</td>
<td>8.5</td>
<td>39.3</td>
<td>22.4</td>
</tr>
<tr>
<td>Relief work</td>
<td>652</td>
<td>19.3</td>
<td>19.2</td>
<td>13.3</td>
<td>4.8</td>
<td>24.5</td>
<td>9.7</td>
</tr>
<tr>
<td>Replacement</td>
<td>483</td>
<td>29.0</td>
<td>24.8</td>
<td>20.9</td>
<td>8.1</td>
<td>5.8</td>
<td></td>
</tr>
<tr>
<td>Job subsidies</td>
<td>426</td>
<td>4.2</td>
<td>18.5</td>
<td>33.8</td>
<td>23.9</td>
<td>6.3</td>
<td></td>
</tr>
<tr>
<td><strong>Total</strong></td>
<td>6,343</td>
<td>15.0</td>
<td>16.6</td>
<td>14.1</td>
<td>8.3</td>
<td>25.9</td>
<td>8.5</td>
</tr>
</tbody>
</table>

*Notes: $m =$ months. In **bold**, the groups of participants separately analysed.*

**Table 5.2** Average treatment effects of joining compared to waiting longer by time of placement, averaged over the 5-year horizon since program start (% points)

### (A) Employment probability

<table>
<thead>
<tr>
<th></th>
<th>$m = 1\text{-}3$</th>
<th>$m = 4\text{-}6$</th>
<th>$m = 8\text{-}10$</th>
<th>$m = 11\text{-}12$</th>
<th>$m = 13\text{-}16$</th>
<th>$m = 17\text{-}20$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Labour market training</td>
<td>-6.2**</td>
<td>-5.9**</td>
<td>3.5</td>
<td>-3.8</td>
<td>1.3</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-15;0.9)</td>
<td>(-13.6;-1.7)</td>
<td>(-3.6;11.6)</td>
<td>(-16;13.2)</td>
<td>(-11.9;10.3)</td>
<td></td>
</tr>
<tr>
<td>Work practice – ALU</td>
<td>1.8</td>
<td>0.6</td>
<td>-2.8</td>
<td>-7.1**</td>
<td>-2.5</td>
<td>-0.2</td>
</tr>
<tr>
<td></td>
<td>(-3.7;10.7)</td>
<td>(-1.7;9.5)</td>
<td>(-8.1;3.8)</td>
<td>(-14.5;-0.8)</td>
<td>(-7.2;2.6)</td>
<td>(-1.8;3.6)</td>
</tr>
<tr>
<td>Work practice – API</td>
<td>1.1</td>
<td>-3.4</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(-7.5;13.7)</td>
<td>(-18.9;5.0)</td>
</tr>
<tr>
<td>Relief work</td>
<td>-3.9</td>
<td>-3</td>
<td>-12.7***</td>
<td></td>
<td>-1.9</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-14.8;7.8)</td>
<td>(-13.6;7.8)</td>
<td>(-25.3;-5.1)</td>
<td></td>
<td>(-10.4;5.3)</td>
<td></td>
</tr>
<tr>
<td>Trainee replacement</td>
<td>0.4</td>
<td>-5.9</td>
<td>11.1***</td>
<td></td>
<td>7.6</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-10.4;4.4)</td>
<td>(-20.7;1.9)</td>
<td>(6.1;21.9)</td>
<td></td>
<td>(-11;23.2)</td>
<td></td>
</tr>
<tr>
<td>Job subsidies</td>
<td>17.3***</td>
<td>30.8***</td>
<td>34.3***</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(6.6;22.7)</td>
<td>(24.4;42.8)</td>
<td>(28.9;44.7)</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

### (B) Benefit collection probability

<table>
<thead>
<tr>
<th></th>
<th>$m = 1\text{-}3$</th>
<th>$m = 4\text{-}6$</th>
<th>$m = 8\text{-}10$</th>
<th>$m = 11\text{-}12$</th>
<th>$m = 13\text{-}16$</th>
<th>$m = 17\text{-}20$</th>
</tr>
</thead>
<tbody>
<tr>
<td>Labour market training</td>
<td>5.8***</td>
<td>6.9***</td>
<td>6.5***</td>
<td>8***</td>
<td>1.4</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(3.6;9.4)</td>
<td>(3.8;10.3)</td>
<td>(2.7;10.3)</td>
<td>(2.7;14.8)</td>
<td>(-8.8;6.2)</td>
<td></td>
</tr>
<tr>
<td>Work practice – ALU</td>
<td>0.9</td>
<td>2.2</td>
<td>8.1***</td>
<td>7***</td>
<td>10.4***</td>
<td>14.7***</td>
</tr>
<tr>
<td></td>
<td>(-3.4;3.1)</td>
<td>(-1.7;4.8)</td>
<td>(6.4;12.8)</td>
<td>(1.8;9)</td>
<td>(6.2;12.5)</td>
<td>(9.6;17.5)</td>
</tr>
<tr>
<td>Work practice – API</td>
<td>9.0***</td>
<td></td>
<td></td>
<td></td>
<td>14.4***</td>
<td></td>
</tr>
<tr>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
<td>(1.9;13.2)</td>
<td>(5.2;20.2)</td>
</tr>
<tr>
<td>Relief work</td>
<td>6.3*</td>
<td>7.7***</td>
<td>12.7***</td>
<td></td>
<td>13.4***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-2.1;10)</td>
<td>(3.9;14.2)</td>
<td>(9.6;20.1)</td>
<td></td>
<td>(6.5;18.4)</td>
<td></td>
</tr>
<tr>
<td>Trainee replacement</td>
<td>2.4</td>
<td>0.7</td>
<td>2.7</td>
<td></td>
<td>5.2</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-1.4;7.7)</td>
<td>(-6.5;4.4)</td>
<td>(-2.9;9.6)</td>
<td></td>
<td>(-4.6;17.2)</td>
<td></td>
</tr>
<tr>
<td>Job subsidies</td>
<td>-8.4***</td>
<td>-7.5***</td>
<td>-5.8***</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-15.7;-6.1)</td>
<td>(-15.7;-2.8)</td>
<td>(-14.7;-0.3)</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

*Notes: $m =$ months. Average over 60 months except for $m=13$-16 (over 54 months) and $m=17$-20 (over 51 months). In brackets, 95% bias-corrected percentile bootstrapped confidence intervals (500 reps); *** significant at 1%, ** at 5%, * at 10%.*
In fact, training as well as trainee replacement do not seem to be chiefly used as vehicles to escape benefit exhaustion. Not only do merely 11 and 8% of participants in these two programs join between months 13 and 16, but for these entrants the treatment effects on subsequent benefit collection probability are small and insignificant.\textsuperscript{38} This is in sharp contrast to relief work, API and especially ALU. By far the largest share of participants in these programs joins around benefit exhaustion (cf. Appendix A.3), and the treatment effects on subsequent benefit collection probability are positive, large and highest for such entrants than for earlier ones.

For ALU in particular, over one third of participants join around benefit exhaustion, and whilst there does not seem to be any significant heterogeneity in employment effects by time of placement (except possibly entrants in months 11 and 12), there clearly is substantial heterogeneity in benefit collection effects. More specifically, the compensated unemployment probability of entitled individuals entering ALU early on is not significantly affected. By contrast, not only do entitled participants joining at and after benefit exhaustion have a 10-15 percentage points significantly higher probability of collecting benefits over time than if they had waited longer in open unemployment, but the treatment effect for them displays visibly more pronounced ‘cycling’ features, as clearly shown in Figure 5.3.

\textbf{Figure 5.3} Treatment effect on benefit collection probability over time of joining ALU compared to waiting longer, by time of placement (% points)

![Treatment effect on benefit collection probability over time of joining ALU compared to waiting longer, by time of placement (% points)](image)

Notes: Time in months, from program start. 95\% bias-corrected percentile bootstrapped confidence intervals (500 reps).

\textsuperscript{38} Only 39 individuals join trainee replacement in months 13-16, and for them the effect is not significant either.
The size of the treatment effect on benefit collection is in fact found to steadily increase with placement time. This same pattern was uncovered also for participants in relief work; where from 6 percentage points for early joiners, the treatment effect gradually rises to over 13 for individuals joining around the time benefit expire.

With regard to API, there seems to be no large heterogeneity in treatment impacts for its two main groups of participants; the overall finding of an insignificant employment effect together with a very large positive effect on benefit collection is confirmed for those joining around benefit exhaustion and very late.

As to trainee replacement, a noteworthy finding is that the largest employment effect (11 percentage points) is enjoyed by those joining in months 8 to 10; the employment prospects of earlier joiners are by contrast not significantly affected compared to a situation where they would have searched longer in open unemployment.

Employment subsidies is confirmed as a very successful program for each of its three main groups of participants, significantly and substantially increasing their employment chances while cutting their benefit collection reliance. Given that this program should only be open to individuals who have been unemployed for over 6 months, it is of interest to note that for participants admitted before then, the employment effect is roughly halved in size compared to later joiners.

### 5.2 Differential program effects

This section discusses the differential effects on employment and on compensated unemployment probabilities for all the pair-wise comparisons of the programs.

Tables 5.3 displays the treatment effects averaged over 5 years since entry into the program, while Figures A1-A5 in the Appendix plot the complete time series of the two treatment effects.

Although later in the section ALU and API are explicitly contrasted, in the initial discussion these two programs are lumped into one type of treatment, ‘work practice’. As seen in Section 2, the two measures have a very similar overall aim, nature and implementation. In particular, they are centred on work experience and prevent participants from performing regular tasks.\(^{39}\) This at least formal equivalence has in fact been sanctioned by the employment offices themselves in January 1999, when the two measures were collapsed into the new Work Practice Scheme.

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\(^{39}\) ALU’s additional benefit-entitlement requirement is not binding in our sample of entitled individuals.
Table 5.3 Average differential effects of joining program A rather than program B for participants in program A, averaged over the 5-year horizon since program start (% points)

(A) Employment probability

<table>
<thead>
<tr>
<th>Comparison</th>
<th>Training</th>
<th>Work practice</th>
<th>Relief</th>
<th>Replacement</th>
<th>Subsidies</th>
</tr>
</thead>
<tbody>
<tr>
<td>Training</td>
<td>3.9***</td>
<td>-0.1</td>
<td>-11.5***</td>
<td>-21.4***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(1.3;6.6)</td>
<td>(-5.2;3.6)</td>
<td>(-19.1;-6.0)</td>
<td>(-27.3;-14.4)</td>
<td></td>
</tr>
<tr>
<td>Work practice a</td>
<td>-3.4***</td>
<td>-1.5</td>
<td>-13.6***</td>
<td>-26.1***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-5.7;-0.9)</td>
<td>(-4.7;1.1)</td>
<td>(-20.7;-6.8)</td>
<td>(-30.3;-21.5)</td>
<td></td>
</tr>
<tr>
<td>Relief</td>
<td>-4.3***</td>
<td>2.6</td>
<td>-9.0**</td>
<td>-24.1***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-8.6;-1.4)</td>
<td>(-0.4;4.9)</td>
<td>(-15.1;-1.6)</td>
<td>(-32.4;-16.1)</td>
<td></td>
</tr>
<tr>
<td>Replacement</td>
<td>6.3***</td>
<td>5.8**</td>
<td>-2.1</td>
<td>-19.3***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.8;9.7)</td>
<td>(0.3;10.1)</td>
<td>(-7.5;3.1)</td>
<td>(-23.9;-11.8)</td>
<td></td>
</tr>
<tr>
<td>Subsidies</td>
<td>23.9***</td>
<td>26.1***</td>
<td>27.6***</td>
<td>16.1***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(19.5;28.4)</td>
<td>(21.9;28.9)</td>
<td>(21.8;31.9)</td>
<td>(9.5;22.4)</td>
<td></td>
</tr>
</tbody>
</table>

(B) Benefit collection probability

<table>
<thead>
<tr>
<th>Comparison</th>
<th>Training</th>
<th>Work practice</th>
<th>Relief</th>
<th>Replacement</th>
<th>Subsidies</th>
</tr>
</thead>
<tbody>
<tr>
<td>Training</td>
<td>-1.3</td>
<td>1.0</td>
<td>9.0***</td>
<td>10.0***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-2.6;0.5)</td>
<td>(-0.8;3.6)</td>
<td>(6.7;12.6)</td>
<td>(6.2;14.2)</td>
<td></td>
</tr>
<tr>
<td>Work practice a</td>
<td>2.2***</td>
<td>0.7</td>
<td>8.6***</td>
<td>13.8***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.8;3.9)</td>
<td>(-0.8;3.1)</td>
<td>(5.7;12.3)</td>
<td>(11.8;16.3)</td>
<td></td>
</tr>
<tr>
<td>Relief</td>
<td>4.2***</td>
<td>0.6</td>
<td>4.6**</td>
<td>12.6***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(2.5;6.5)</td>
<td>(-0.8;2.2)</td>
<td>(0.5;9.0)</td>
<td>(7.6;17.3)</td>
<td></td>
</tr>
<tr>
<td>Replacement</td>
<td>-5.8***</td>
<td>-2.6**</td>
<td>-3.8**</td>
<td>7.1***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-8.3;-2.5)</td>
<td>(-4.9;-0.4)</td>
<td>(-6.5;-0.4)</td>
<td>(2.7;9.5)</td>
<td></td>
</tr>
<tr>
<td>Subsidies</td>
<td>-10.1***</td>
<td>-12.8***</td>
<td>-14.0***</td>
<td>-6.7***</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-11.9;-7.7)</td>
<td>(-14.2;-11.0)</td>
<td>(-17.2;-11.2)</td>
<td>(-8.9;-3.3)</td>
<td></td>
</tr>
</tbody>
</table>

Notes:  
* ALU and API combined.
Average over 60 months; for the complete time series of the effects, see Appendix.
In brackets, 95% bias-corrected percentile bootstrapped confidence intervals (500 reps);
*** significant at 1%, ** at 5%, * at 10%.

As to the relative performance of the different programs in terms of employment, the star program is clearly job subsidies again, maybe not surprisingly given the informal job ‘promise’ they often entail. Participants in this program enjoy a much higher employment probability over time than if they had joined any of the alternative programs, with the gain decreasing from over 40 percentage points at the time the programs typically end to a still significant and substantial 10 percentage points towards the end of our observation period. Over these 5 years, job subsidy par-
Participants are overall around 25 percentage points more likely to be in employment than if they had joined training, work practice or relief work, and 16 percentage points more than if they had joined a replacement scheme. In addition, participants in any of these other programs would have fared considerably better (20-25 percentage points) had they gone on job subsidies instead.

The performance of job subsidies stands out also in terms of benefit collection probability: these participants are significantly less likely to be on unemployment benefits over time than if they had joined any other program, and participants in the other programs would have been less likely to be drawing benefits over time had they gone on a subsidised job. In terms of both employment and benefit collection outcomes, trainee replacement, although considerably outperformed by job subsidies, appears to be quite distinct compared to the remaining measures. Specifically, the gain for job subsidy participants is the least when measured relative to replacement schemes, while replacement scheme participants would have gained less from job subsidies than participants in the other measures would have.

Trainee replacement is in fact found to be the second best performing program overall, corroborating the presumption that it should teach market-relevant skills, given that the task performed is by construction a useful one, for which the firm was willing to pay a regular employee. While being just as likely to be employed over time than if they had joined relief work, former deputies enjoy 6 percentage points higher employment chances overall than if they had joined training or work practice. Conversely, participants in work practice, training and even relief work would have considerably improved their labour market prospects had they joined a replacement scheme instead. 40 Similar conclusions also hold in terms of benefit collection: deputies are significantly less likely to be in compensated unemployment over time than if they had joined training, relief work or work practice, while participants in these three measures would have been significantly less likely to be collecting benefits had they enrolled in a replacement scheme.

Out of these remaining programs – labour market training, work practice and relief work – training seems to be the best-performing one; trainees are overall 4 percentage points more likely to be employed than if they had gone on work practice, while work practice as well as relief work participants would have had 3-4 percentage points higher employment chances had they gone on training. In terms of compensated unemployment, while trainees are just as likely to be drawing

40 In fact, the gain participants in these three programs would have enjoyed from going on trainee replacement (treatment on the non-treated) would have been roughly double the size of the average benefit deputies enjoy from their own participation (treatment on the treated).
benefits than if they had enrolled in work practice or relief, participants in these measures would be 2 and 4 percentage points less likely to be collecting benefits overall had they joined training.

As to relief and work practice, they do not perform significantly differently from one another, either in terms of employment or of compensated unemployment. Interestingly, for work practice participants there is evidence of unemployment-program ‘cycling’ effects of the type described in Section 5.1 also in some of the program-to-program comparison (cf. Appendix A2).41

**API versus ALU**

As to the two work practice measures, their potentially different effectiveness is of particular interest: both API and ALU share their basic features, but ALU is exclusively reserved to individuals entitled to unemployment benefits and has been explicitly introduced to prevent them from running out of compensation.

Compared to longer search in open unemployment, Figures 1 and 2 have shown that while the two measures are substantially equivalent in terms of their large positive impact on benefit collection, in terms of employment prospects the performance of ALU is visibly worse than the one of API. These effects however relate to two different groups – individuals participating in ALU and individuals participating in API. By contrast focus in the following is on how participants in one of the two programs would have fared had they joined the other program instead.

Figure 5.4 interestingly show that as far as employment is concerned, the two programs seem to be well targeted. In particular, participants in ALU overall gain 4.7 percentage points higher employment probability averaged over the observation period from having joined their program rather than API. Participants in the latter program too do benefit from their choice, enjoying a 3.7 percentage points higher employment probability overall than if they had joined ALU instead. Quite interestingly, the differential gains from the two programs arise at different times: the positive API effect for its participants lasts until the end of the 3rd year since joining, while the gain from ALU for its participants occurs only during the 4th year.

As to benefit collection probability, participants in ALU are just as likely to be drawing benefits than if they had joined API. Note that both ALU and API allow our sample of entitled individuals to renew their benefits; this finding simply means that the extent to which those individu-

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41 A minor remark concerns the favourable effect on benefit collection probability of work practice and relief work – compared to training and trainee replacement – in the very short-term (Figures A1 to A4). This is due to the longer duration typically spent on the former programs (around 150 days, compared to only 90 and 105 days on training and
Figure 5.4 Differential performance of ALU and API, over time and averaged over 60 months

(A) ALU compared to API for ALU participants

(B) API compared to ALU for API participants

Notes: Time in months, from program start. 95 percent bias-corrected percentile bootstrapped confidence intervals (500 reps); ** significant at 1%, * at 5%, * at 10%.

As joining ALU remain in compensated unemployment is unaffected by which of the two work practice scheme they enrol in. By contrast, participants in API are significantly less likely to be collecting compensation than if they had joined ALU. Put it differently, API participants would have been significantly more likely (by over 4 percentage points overall) to be drawing benefits trainee replacement – cf. Table A1), coupled with the fact that compensation whilst on a program is not counted as unemployment compensation.
had they joined ALU instead. Furthermore, they would have collected such unemployment compensation on a cycling basis.

In conclusion, within our sample of unemployed individuals entitled to benefits, both ALU and API seem to be well targeted when assessed one against the other. The findings for API participants do however point to how – even once conditioning on entitled individuals – the explicit, close link between entitlement renewability and program participation (as institutionalised in the case of ALU) may adversely affect a program’s impact on compensated unemployment.

5.3 The problem of the ‘lost’ individuals

A final issue concerns an attrition problem in the Händel dataset, whereby a registered unemployed individual, having first missed an appointment at the official employment office and subsequently failing to contact the agency within a week, is simply de-registered – thus lost from the data – without information on whether a job has been found or whether the individual is still unemployed. Bring and Carling (2000), who have tried to trace back a sample of ‘lost’ individuals, have found that around half of them had in fact found a job, highlighting how employment status may be critically under-reported in the official data. More critically, though, it is quite possible that the probability of being in a lost spell over time, as well as the true state (employed versus unregistered unemployed) once in a lost spell may be systematically different among individuals taking the various treatments. Although in our sample of entitled individuals this attrition problem is considerably less severe than in the full sample, almost 9% of our individuals do become ‘lost’ after their first (registered) unemployment spell (see Table 3.2), while the probability of being lost over time steadily rises to 12% over our 5-year horizon. The robustness of our findings as to employment effects thus needs to be carefully checked against these lost spells.\textsuperscript{42}

Following Sianesi (2004), the additional information from the Bring and Carling survey has been exploited to perform best- and worst-case bounds analysis on all the pair-wise comparisons of the treatments.\textsuperscript{43} As shown in Appendix A6 and A7, the conclusions discussed above remain in

\textsuperscript{42} Outcomes such as benefit collection, being conditional on registration at an employment office, are not affected.

\textsuperscript{43} The conditional probability that a lost individual \((L=1)\) with characteristics \(X\) has in reality found employment \((Y=1)\) can be decomposed as: \(P(Y=1|X=x, L=1) = P(Y=1|X=x, L=1, D=1) \cdot P(D=1|X=x, L=1) + P(Y=1|X=x, L=1, D=0) \cdot [1 - P(D=1|X=x, L=1)]\), where for each pair-wise treatment comparison, \(D=1\) denotes the treatment and \(D=0\) the comparison treatment. For each lost individual, we know his treatment status \(D\), we can estimate his treatment probability given the lost status \(P(D=1|X_i, L_i=1)\) as \(p_{D}^{i}\) and based on the survey we can impute his misclassification probability \(P(Y=1|X_i, L_i=1)\) as \(p_{Y}^{i}\). The procedure to derive worst- and best-case bounds consists in assigning \(P(Y=1|X_i, L_i=1, D=d)\).
fact virtually unaffected, both in terms of the differential performance of the various programs and regarding the positive employment effect of job subsidies and the negative ones of relief work, ALU and training compared to longer job search.

6. Discussion and conclusions

This paper has investigated the differential performance of six main types of Swedish programs both relative to one another and vis-à-vis more intense job search in open unemployment. Starting from the latter comparison, the results concerning treatment effects on employment and compensated unemployment have been discouraging for all the programs considered except job subsidies (and possibly replacement schemes).

Several factors may account for such disappointing findings. It might for instance be more difficult to put participants back into stable work in periods of high unemployment 44 (though it may be argued that it is exactly in such difficult times when effective labour market programs would be most needed). There is also the connected issue of the scale of the programs, with their massive use in the 1990s likely to have resulted in inefficient program administration 45.

An additional most likely explanation however relates to the use of the programs simply as a way to re-qualify for unemployment benefits, with programs ending up locking their participants – and in particular those entitled to unemployment compensation – in the unemployment system. Findings in Sianesi (2001 and 2004) considering the overall program system lend support to the conjecture that for individuals entitled to unemployment compensation, the eligibility renewability rules are likely to significantly distort the incentives for participation and thus wipe out potential productivity-enhancing effects of the programs. The present analysis has in fact found that individuals joining labour market training, either work practice scheme (API or ALU) and relief work subsequently display lower employment rates coupled with a higher benefit collection probability than if they had searched further as openly unemployed. When exploring the extent to which these effects vary for participants who joined their program after different amounts of

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44 See for instance the switch from positive effects for Swedish labour market training in the 1980s to negative ones in the 1990s. For more details, see Calmfors, Forslund and Hemström (2001).

45 In principle there could also be a stigma effect linked to program participation. This however does not seem to be the case in Sweden, where employers view former program participants more favourably than openly unemployed individuals (for a review of the relevant survey studies, see Calmfors, Forslund and Hemström, 2001).
time, relief work, API and especially ALU were found to be chiefly used as vehicles to escape benefit exhaustion.

As to the differential effects on employment and on compensated unemployment probabilities for the pair-wise comparisons of the programs, job subsidies was again found to be undisputedly the best performer, followed by trainee replacement, and – by a very long stretch – labour market training. As to relief work and work practice, they did not seem to perform in a significantly different way from one another.

Scrutinising the Swedish experience to derive general lessons as to which type of program works best, we thus found that those programs providing (subsidised) workplace experience and on-the-job training at an employer are relatively more effective for participants’ subsequent labour market performance than vocational classroom training courses. Furthermore, the more relevant and the closer to the competitive labour market the kind of task performed, the higher the program ranks. Not only are these conclusions in line with Carling and Richardson (2001), a Swedish study most similar in aim and sample selection to the present one, but the underlying similarity of results across studies looking at different countries with varying labour market structures and policies may indicate a general validity of these overall conclusions.46

Turning to the cost side, it is quite remarkable to notice how the ranking of the programs in terms of their effectiveness is roughly reversed when taken in terms of their expensiveness.

It is however important not to jump at the hasty conclusion that job subsidies are the solution – the most effective program as well as the cheapest. Several types of issues can be raised to point out potential problems both in terms of the effective magnitude of the uncovered effects and in terms of their general applicability should the scope of the program be extended.

As to the scope of the analysis, the program’s effects have been evaluated for a rather specific sub-group of the population – the declared target group of individuals who have been relatively long in unemployment (although note that Carling and Richardson (2001) find their results unaffected by time spent in unemployment prior to participation).

A second issue concerns the validity of the identifying CIA assumption for participants in this program: since job subsidies generally entail the informal ‘promise’ of a job, it is likely that po-

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46 For a summary of other Swedish evidence in line with the present results, see the review by Calmfors, Forslund and Hemström (2001). For OECD countries see the review by Martin and Grubb (2001) and e.g. Gerfin and Lechner (2000) for Switzerland, Brodaty, Crépon and Fougère (2000), Bonnal, Fougère and Sérandon (1997) and Kramarz and Philippon (2001) for France, and Ridder (1986) for the Netherlands.
tential candidates are considered quite carefully. Even though we control for a host of factors likely to underlie the caseworker’s judgement and despite Carling and Richardson’s (2001) finding of no selection bias for this program, it may still be the case that subsidised participants are slightly ‘better’ on average than matched comparisons. Nonetheless, it would be hard to argue that remaining selection bias could account for all of the large positive effects seemingly displayed by job subsidies in terms of all comparisons and outcomes considered.

Even if the direction of the estimated effects may appear reliable, however, it may not be possible or even desirable to focus attention and funds on this kind of measure. As to the sheer possibility of extending it, scope is in fact limited: the public sector cannot use such grants, and following EU regulations in 1997 neither do employers in the synthetic fibre, automotive, steel, shipyard, fishery and transport industries. Apart from legal feasibility, the desirability of a widespread use of this measure may not be warranted once it is considered that our estimates ignore potential indirect and general equilibrium effects which may spill over to other groups. In particular, substitution would take place if participants in the job subsidy program were to take (some of) the jobs that participants in the other programs or ‘waiting’ unemployed individuals would have been offered in the absence of the subsidies. The impact of the subsidy would thus be at the expense of worsened conditions either for participants in the other programs or for openly unemployed individuals finding it more difficult to get jobs or getting worse jobs. The estimated effect would in this case overestimate the net impact of the subsidy program. Both survey and econometric Swedish studies do in fact find sizeable (around 65-70 percent) direct displacement effects arising from those Swedish programs that generate subsidised employment.47

Finally, it is obviously unthinkable to generalise such a measure to all unemployed job-seekers: it would simply become just a way to subsidise firms’hirings, resulting in huge deadweight effects (i.e. subsidising hiring that would have taken place anyway).

In the light of the present and previous results and of the above considerations, a more promising measure might appear to be trainee replacement. Still among the cheapest programs, it was shown to perform quite satisfactorily. In fact, it shares some of the features likely to be at the root of the success of job subsidies (short of the job promise): in terms of the present temporary employment, it provides relevant job-specific training and can be used as a cheap screening device of individual unobserved productivity. At the same time it sends out a message that the individual

47 For more details, see Calmfors, Forslund and Hemström (2001).
has been gaining (or maintaining) relevant skills, thus making the job seeker more attractive to potential future employers, who value the fact that a job is being performed in the regular competitive market.\textsuperscript{48} Finally, our partial-equilibrium estimates are likely to be an underestimate of the program’s effect, since they do not take into account the ‘double-dividend’ arising from the possibility offered to the replaced employees of increasing their human capital through training.

Nevertheless, even though at first sight the potential of this program appears particularly promising, a few issues need once again to be considered. The ‘double-dividend’ from the subsidised training of the replaced employee may in fact often turn out to be dead-weight loss instead\textsuperscript{49}, while Harkman, Johansson and Okeke (1999) found evidence of dead-weight in terms of the deputies as well, with a large share of participants alternating between regular short-term jobs and trainee replacement with the \textit{same} employer. Finally, survey studies have in fact uncovered displacement effects of the same order as employment subsidies (e.g. AMS, 1998).\textsuperscript{50}

In conclusion, the present analysis unambiguously joins previous micro studies in finding that the more it resembles regular employment in the competitive labour market, the higher the program’s benefits to its participants. It is however essential to consider these findings in the light of those arising from the macroeconomic literature, which has widely documented that exactly for these types of programs the potential for negative crowding-out and dead-weight effects is largest. Taken together\textsuperscript{51}, the various results clearly highlight the difficult trade off faced by labour market policy.

References


\textsuperscript{48} These arguments are in line with the complaint by Swedish employers about the excessively large number of labour market measures, which prevents them from being able to make informed judgements as to the exact content and value of each program. Part of the success of job subsidies – and trainee replacement – may thus in fact arise from the fact that they offer firms a cheap way to screen workers’ productivity in regular and known activities.

\textsuperscript{49} Since 80 to 90 percent of employers taking part in the scheme are within sectors (health care and related branches in the public sector) with a long-standing system for further training funded by the employer, it seems likely that a good part of the sponsored training would have occurred anyway. (I thank Anders Harkman for this information.)

\textsuperscript{50} 42 percent as an average across survey studies, see Calmfors, Forslund and Hemström (2001).

\textsuperscript{51} See in particular Calmfors, Forslund and Hemström (2001).


### Table A1: Selected descriptive statistics, by type of exit from first open unemployment spell

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<th>Program participants</th>
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<td>Age (years)</td>
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<td>Worked 20h/week</td>
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<td>Worked 40h/week</td>
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<td>Professional, technical</td>
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<td>Sales</td>
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<td>Looks for part-time job</td>
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<td>Interlocal job seeking</td>
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<td>Job ready</td>
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<td>Needs guidance</td>
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<td>Difficult to place</td>
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<td>Local program rate</td>
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<td>County: Stockholm</td>
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<td>Malmöhus</td>
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<td>Jun</td>
<td>11.0 9.6 8.7 6.6 10.1 10.6</td>
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| Note: Age is measured at entry into unemployment; all other time-varying variables at program entry or at de-registration.
Table A2 Indicators of matching quality

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<th>(Pr &gt; \chi^2) after (3)</th>
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Notes:

(1) Pseudo-\(R^2\) from a Probit of \(D\) on \(X\), giving an indication of how well the regressors \(X\) explain the participation probability.

(2) Pseudo-\(R^2\) from a Probit of \(D\) on \(X\) on the matched samples, to be compared to (1).

(3) \(P\)-value of the likelihood-ratio test after matching, testing the null of joint insignificance of all the regressors. (Before matching, \(Pr > \chi^2 = 0.0000\) always).

(4), (5) Median absolute standardised bias before and after matching, median taken over all the regressors.

Following Rosenbaum and Rubin (1985), for a given covariate \(X\), the standardised difference before matching is the difference of the sample means in the full treated and non-treated sub-samples as a percentage of the square root of the average of the sample variances in the full treated and non-treated groups. The standardised difference after matching is the difference of the sample means in the matched treated (i.e. falling within the common support) and matched non-treated sub-samples as a percentage of the square root of the average of the sample variances in the full treated and non-treated groups.

\[
B_{before}(X) = 100 \cdot \frac{\bar{x}_1 - \bar{x}_0}{\sqrt{\frac{1}{n_1} \sum (x_i - \bar{x}_1)^2 + \frac{1}{n_0} \sum (x_i - \bar{x}_0)^2}}
\]

\[
B_{after}(X) = 100 \cdot \frac{\bar{x}_{1M} - \bar{x}_{0M}}{\sqrt{\frac{1}{n_1} \sum (x_i - \bar{x}_{1M})^2 + \frac{1}{n_0} \sum (x_i - \bar{x}_{0M})^2}}
\]

The standardization allows comparisons between variables and, for a given variable, comparisons before and after matching.

(6) Percentage of the treated individuals falling outside of the common support.
Figure A1  **Labour Market Training**: Differential treatment effects over time (% points).

**Employment probability**

- vs Work Practice
- vs Relief Work
- vs Trainee Replacement
- vs Job Subsidy

**Benefit collection probability**

- vs Work Practice
- vs Relief Work
- vs Trainee Replacement
- vs Job Subsidy

Notes: Time in months, from program start. 95% bias-corrected percentile bootstrapped confidence intervals (500 reps).
Figure A2  WORK PRACTICE: Differential treatment effects over time (% points).

**Employment probability**

- vs Labour Market Training
- vs Relief Work
- vs Trainee Replacement
- vs Job Subsidy

**Benefit collection probability**

- vs Labour Market Training
- vs Relief Work
- vs Trainee Replacement
- vs Job Subsidy

Notes: Time in months, from program start. 95% bias-corrected percentile bootstrapped confidence intervals (500 reps).
Figure A3  RELIEF WORK: Differential treatment effects over time (% points).

**Notes:** Time in months, from program start. 95% bias-corrected percentile bootstrapped confidence intervals (500 reps).
Figure A4  **Trainee Replacement**: Differential treatment effects over time (% points).

**Notes**: Time in months, from program start. 95% bias-corrected percentile bootstrapped confidence intervals (500 reps).
Figure A5  JOB SUBSIDIES: Differential treatment effects over time (% points).

Employment probability

vs Labour Market Training vs Work Practice

vs Relief Work vs Trainee Replacement

Benefit collection probability

vs Labour Market Training vs Work Practice

vs Relief Work vs Trainee Replacement

Notes: Time in months, from program start. 95% bias-corrected percentile bootstrapped confidence intervals (500 reps).
Figure A6  Treatment effects on employment probability over time of joining a given program compared to waiting longer in open unemployment: Estimated effects and best- and worst-case bounds
Figure A7  Differential treatment effects on employment probability over time: Estimated effects and best- and worst-case bounds

Labour Market Training

Work Practice

Job Subsidy

Trainee Replacement

Relief Work

Labour Market Training

-0.4
-0.3
-0.2
-0.1
0.0
0.1
0.2
0.3
0.4
0.5
0
10
20
30
40
50
60
0
10
20
30
40
50
60