Differential effects of Swedish active labour market programmes for unemployed adults during the 1990s

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Abstract

The paper evaluates the differential performance of the six main types of Swedish programmes that were available to adult unemployed workers entitled to unemployment benefits in the 1990s: labour market training, workplace introduction, work experience placement, relief work, trainee replacement and employment subsidies. On the basis of a large and particularly rich administrative dataset, propensity score multiple-treatment matching methods are applied to investigate the differential performance of the programmes both relative to one another and vis-à-vis more intense job search in open unemployment. Outcomes being assessed are short- and long-term employment rates as well as the probability of collecting unemployment benefits over time.

Compared to waiting longer in open unemployment, all the programmes initially reduce their participants' employment probability in the short term (lock-in effect). Positive findings on more long term employment prospects are confined to job subsidies alone. Participation in trainee replacement makes no difference to deputies' subsequent labour market outcomes. Individuals joining any of the remaining programmes later display either the same (workplace introduction) or lower employment rates coupled with a higher benefit collection probability than if they had searched further as openly unemployed. A likely factor behind these disappointing results is the use of such types of programmes simply as a way to re-qualify for unemployment benefits. As to the pair-wise comparison of the six programmes, the central finding is again that the more similar a programme is to a regular job, the higher the programme's benefits to its participants, with employment subsidies by far the best performer, followed by trainee replacement. Several macroeconomic studies have however documented large and negative displacement and dead-weight effects for exactly these types of programme, which highlights the difficult trade-off faced by labour market policy.

Keywords: Active labour market programmes, evaluation, multiple-treatment matching, treatment effects.

JEL classification: C14, J38, J65, J68.

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1. Introduction

Sweden occupies a special place when it comes to labour market programmes: a long-standing reliance on such measures has been accompanied by traditionally low unemployment rates by European standards, two features which several observers have often related to one another (e.g. Layard, Nickell and Jackman, 1991).

The deep and sudden recession of the early 1990s has however posed new challenges to the Swedish labour market policy, when expenditure on the country's extensive offer of labour market programmes reached 3 percent of GDP. Concomitantly, interest has been rising in evaluating how successful such large-scale measures have been.

Sianesi (2002a) has for instance looked at the performance over the last decade of the Swedish system in its entirety, combining all the programmes into one and focusing in particular on the interactions between the unemployment benefit system and the programme system. The next natural step was to disaggregate the programme system into its main components in order to take account of the fact that the core of the '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 programmes provide direct incentives to move back into employment by either facilitating individuals' job search, providing wage subsides or fostering the acquisition of work contacts and references; other measures by contrast 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.

Different programmes may in fact differ in their effects, making it interesting to evaluate the relative effectiveness of the various measures, ideally aiming at identifying the best performing ones. Such information could prove very useful not just for Swedish policy-makers and unemployed workers, but also for those countries who in the last two decades have been introducing or expanding their active labour market policy.[3] Although with obvious

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[3] Examples include the UK, where the 'New Deal for the Young Unemployed', introduced in April 1998 and sharing some of the features of the Swedish set-up, offers five types of 'treatments'; France, where a series of measures targeted at unemployed youth were introduced during the late 80s; or Switzerland, where an ambi-

care, general lessons as to what type of programme is more effective can be cross-analysed and shared across countries (see e.g. Martin and Grubb, 2001).

The present evaluation of the six main Swedish programmes that were available to adult individuals at the height of the economic recession in 1994 aims at complementing recent research being increasingly carried out in this direction.[4]

The differential performance of these programmes is investigated both relative to one another and vis-à-vis more intense job search in open unemployment. More precisely, the evaluation concerns the effect, for participants in a given programme, of joining that programme compared to joining another available programme, as well as compared to waiting longer in open unemployment.

When looking at the relative effectiveness of one programme 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 this analysis are individuals entitled to unemployment benefits: they have exclusive access to some types of programmes and enjoy 'special' conditions on programmes of wider access (e.g. they are in principle granted the right to some types of programme when approaching benefit exhaustion). Special policy interest in examining this group arises from the fact that one of the programmes was created just for entitled individuals, so that a natural question concerns the actual effectiveness of this special measure. Furthermore, since up to February 2001 participation in a Swedish programme used to renew job-seekers' eligibility to unemployment compensation, we are focusing on that one group whose participation incentives are most likely to have been affected and for whom the trade-off between productivity-enhancing components of

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tious array of programmes was set up during the 90s. In fact, both at the OECD (OECD, 1996) and European Union (European Commission, 1998) level, labour market programmes are increasingly viewed as important measures to reduce long-term unemployment.

[4] Examples of microeconometric studies looking at the relative effects of Swedish programmes include Carling and Gustafson (1999) for self-employment subsidies *versus* subsidised jobs, Frölich, Heshmati and Lechner (2000) and Melkersson (1999a, b) for programmes targeted at the disabled, Larsson (2000) for youth programmes, Johansson and Martinson (2000) for two types of labour market training programmes, and Carling and Richardson (2001) for the relative efficiency of eight of the Swedish programmes. Evaluations of differential programme impacts outside the Swedish context include the recent work by Gerfin and Lechner (2000) for Switzerland and by Brodaty, Crépon and Fougère (2000) as well as Bonnal, Fougère and Sérandon (1997) for France, and the earlier work by Ridder (1986) for the Netherlands.

the programmes and the reinforced work disincentive associated with the benefit system should have been at its sharpest.

We consequently concentrate on two important types of outcomes. Given that an explicit aim of the active labour market policy is to improve the employability of unemployed workers, employment rates over time will be considered, summarising possible programme effects on both job finding probability and survival in employment once an occupation has been found. This will allow to address the issue of what type of programme – if any – is most beneficial to participants in terms of their employment prospects in the short and in the long run (five years). To capture the influence that benefit renewability considerations are likely to exercise on the impacts of the programmes, special attention is also devoted to the differential programme effects on individuals' benefit collection probability over time.[5]

The next section outlines the Swedish labour market policy and describes the six programmes being evaluated. Section 3 describes data and sample choice and offers a 'naïve' first evaluation of the programmes based on the raw data. 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 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 components: a benefit system that supports individuals while unemployed and various active labour market programmes offered in order to facilitate the re-employment of unemployed job seekers.

Unemployment compensation is provided in two forms[6], the most important one being unemployment insurance (UI). UI benefits are very generous by international standards –

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[5] We do not consider differential programme effects on wages or earnings. Although this would provide interesting information on potential programme effects on individual productivity, as highlighted by Carling and Richardson (2001) increased income has never been an explicit objective of the Swedish labour market policy; programmes have by contrast traditionally represented a measure to keep a compressed wage structure.
[6] Individuals not entitled to any form of unemployment benefits may receive means-tested social insurance.

the income-related daily compensation is 80 percent of the previous wage[7] – and are available for a long duration – 60 calendar weeks. An (even part-time) unemployed person registered at a public employment office and actively searching for a job is eligible for unemployment benefits if in addition to a membership condition[8], the work condition is satisfied: the claimant must have been working for at least five months during the twelve months preceding the current unemployment spell. In addition, an offer of 'suitable' work – or of a labour market programme – must be accepted; refusal to accept a job/programme might lead to expulsion from compensation.

The second form of unemployment compensation is cash labour market assistance (KAS). This supplementary compensation 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 nine 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 labour market programme for five months (or completion of a training course) would count as employment and thus qualify for a renewed spell of unemployment compensation. Consequently, in spite of the period during which an unemployed job-seeker can receive unemployment benefits being fixed, it used to be in fact possible to extend it indefinitely by using programme participation to renew eligibility.

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

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[7] This maximum level of compensation has changed a few times during the 1990s; note also that the system has a ceiling in terms of the amount of daily compensation.

[8] The claimant must have paid the membership fees to the UI fund for at least 12 months prior to the claim Though low in absolute terms, fees vary considerably between UI funds reflecting unemployment differentials between industries and occupations covered by the funds (e.g. in 1986, the yearly fee ranged from SEK 60 to 550, with the average hourly wage rate for manufacturing workers at SEK 55).

This evaluation focuses on the six main programmes open to adult unemployed workers in the middle of the 90s: labour market training, workplace introduction, work experience placement, relief work, trainee replacement and employment subsidies.[9]

To gain access to any programme, one needs to be registered at a local official employment office. The six programmes 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.[10] All the individuals in our chosen sample satisfy the eligibility rules in terms of registration, age and entitlement criteria, while we shall control very carefully for unemployment duration prior to programme start.

Whilst on a programme, participants either receive the stipulated wage and other benefits on their 'temporary' workplace, or the equivalent of the unemployment benefit they would have enjoyed as openly unemployed. Most programmes have a maximum duration of six months (under special circumstances renewable for another six), though participants stay an average of four to five months.

Table 2.1 contrasts the main features of the programmes being evaluated.

Labour market training (AMU), by far the most expensive measure, is intended to augment participants' human capital with formal, full-time vocational[11] teaching of new skills.

A second type of programme offers workplace traineeship to maintain and enhance contact with working life and gain practical experience, good working habits and references

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[9] Two programmes are excluded from the analysis on the basis that they are targeted to (or attract) quite specific sub-groups of unemployed individuals: self-employment grants (for individuals wishing to establish their own new business, with both a business idea and a financial plan approved by the offices) and vocational rehabilitation (for persons 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 programmes.

[10] Larsson (2000) finds the waiting period rule to be *de facto* regarded as a formal requirement for youth practice too.

[11] 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 programmes, rather than directly into a job.

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 programmes, offers unemployed individuals a period of workplace training.

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 subsidises* not only represent a temporarily subsidised job opportunity to acquire job-specific human capital, but they are aimed at influencing an employer's hiring process: the engagement is implicitly expected to continue after completion of the programme.

Thus while all the programmes aim at improving participants' employment prospects, two important dimensions that distinguish the various types is the kind of skills provided and the way they are provided. 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 kinds of programme 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[12], 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.

A final consideration relates to the first row in Table 2.1, which highlights that in Sweden the state to which programme participants can be compared to is not one of being completely left on one's own to look for a job, but the baseline 'package' offered by the employment offices. An individual registered as openly unemployed has access to various employment services, not only in terms of the increasingly computerised job information and matching of vacancies to applicants, but also in terms of the 'job-seeker activities', which include search-skill-enhancing activities such as training courses on how to apply for a job and motivation-raising activities. In some countries this kind of assistance is in fact considered a programme in its own right.[13]

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[12] 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.

[13] An example is the Gateway period of the new UK New Deal programme for the unemployed.

Table 2.1 Synoptic table of the main features of the programmes

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

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).

3. Data, sample selection, and a preliminary look at the raw data

The dataset constructed to capture the institutional framework just described is the result of combining two main sources, which reflect the programme component (Händel) and the benefit component (Akstat) of the labour market policy. Händel, the unemployment register maintained by the National Labour Market Board (AMS), contains information on all unemployed individuals registered at the public employment offices. Available from 1991, it provides each individual's labour market status information over time, together with important characteristics of the job-seeker and of the occupation sought. 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 who are entitled to UI or KAS.

As to sample choice, we need individuals who are homogeneous in those basic characteristics which determine eligibility to the programmes under examination. Only then will it be relevant to examine their outcomes had they chosen a competing type of programme. As motivated in the previous sections, the choice of this paper is to focus on adult individuals 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 all the information from the Akstat dataset.

A sample of over 30,800 adult individuals has thus been selected who entered the employment offices for their first time[14] and in the same calendar year 1994 (when unemployment was still at its highest), registered as openly unemployed[15] and were entitled to either UI or to KAS. Additionally, individuals whose first programme was start-up grants,

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[14] Strictly speaking, one cannot exclude that our individuals have had contact with the unemployment office before August 1991, date when Händel starts.

[15] In particular, given that the main purpose of the programmes is to enhance the re-employability of the unemployed, those registering as employed or directly entering as programme participants (possibly anticipating a risk of unemployment) are excluded from the sample.

vocational rehabilitation or non-vocational training are dropped from the analysis (see foot-notes 9 and 11). Our individuals, all in the 25-54 age group and with no occupational disabilities, are then followed until the end of November 1999.[16]

An exploratory first look at the raw data allows one to gather a general idea of the paths participants in the various programmes follow after their respective programme. A few interesting features emerge from Table 3.1, showing the share of each type of participant moving on to a different labour market state directly after the programme. The exceptional performance of employment subsidies jumps to the eye: three quarters of participants *directly* after programme completion exit the unemployment register, and practically all for a regular job. The ranking of the various other programmes, lagging far behind and with replacement schemes as second best, is in line with *a priori* expectations about the degree of relevance of the experience gained on the programmes. If we accept that after such schemes participants would often need to spend some time job-seeking, the superiority of replacement schemes remains, although training now also performs quite well. Quite interestingly, a large fraction of former participants (around a third of the remaining unemployed pool) return to the *same* kind of programme.

After this crude 'tracking' of participants' early moves, the raw data can be further explored by looking at employment rates over time for participants in the six programmes, starting from the moment they join and following them up to five years. The raw differential outcomes visualised in Figure 3.1 again clearly confirm the 'star' performance of employment subsidies. Still in line with *a priori* expectations, the second-best performer appears to be trainee replacement. It is interesting to note that labour market training, though not one of the best performing measures in the short-term, seems to catch up later on: employment rates of former trainees equal if not surpass those of former participants in re-

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[16] 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 programme shorter than a week and followed by another programme was merged to that subsequent programme.

placement schemes. API and ALU seem to perform roughly equally well, with API offering slightly better outcomes in the short term.

Table 3.1 Transitions from the first programme onwards (% of respective participants)

	Type of programme					
	Train-ing	ALU	API	Relief	Replace-ment	Sub-sidies
Number of participants	1,387	2,983	425	654	483	426
(as percent of participants)	(21.8)	(46.9)	(6.7)	(10.3)	(7.6)	(6.7)
Participants who directly after the programme (U**P**)[a]						
(a) found employment	8.9	11.3	14.3	15.3	22.5	72.8
(b) other exit [b]	2.0	2.6	3.8	6.6	5.6	2.6
Out of those who after the programme fell back into unemployment (U**P**U)[a], then:						
(a) found employment	29.5	23.8	25.0	35.4	47.1	42.0
(b) other exit [b]	10.9	14.3	18.2	12.2	14.6	18.0
(c) same type of programme	27.5	35.4	34.8	16.7	21.4	8.0

Notes: [a] U: open unemployment spell; **P**: first programme spell, i.e. the respective 'treatment'. [b] exit from the labour force (including for regular education) or de-registered for 'contact lost'.

Figure 3.1 Raw data: employment probability over time, by type of first programme (Time in months; t=0 at programme start)
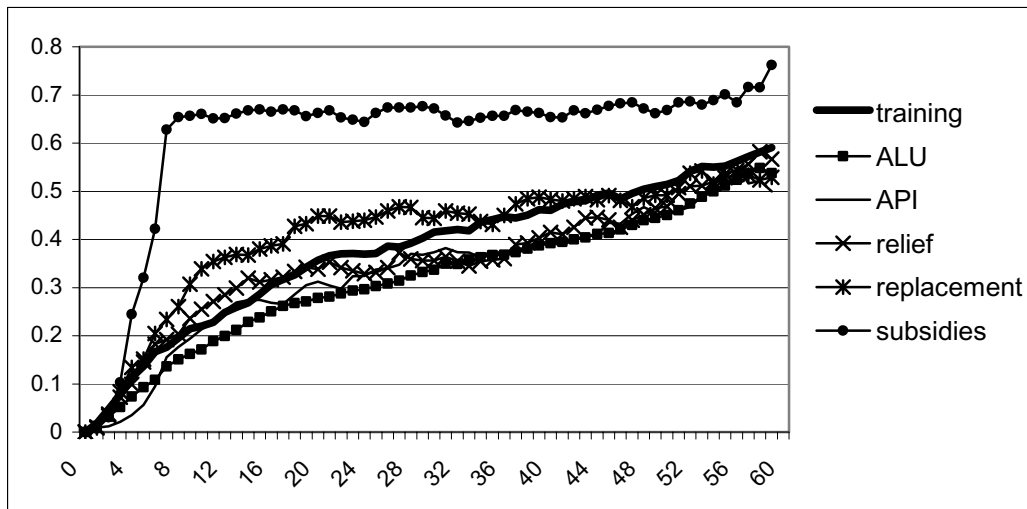
This simple picture emerging from the raw data, though interesting and in line with expectations, cannot however be taken as showing the causal effects of the programmes. Such differential performance may be wholly or partially attributable to a selection effect: individuals going into the different programmes are likely to systematically differ in terms of characteristics that also influence their labour market performance. Visual inspection of se-

lected average characteristics in Table 3.2 clearly shows that participants in the different programmes are not a random sample from the population, but are in fact quite distinctive groups. There seem to be several variables – such as skills, qualifications and employment histories – that influence programme assignment and which are most likely to affect subsequent outcomes.

4. Methodology

4.1 The evaluation problem in a multiple-treatment framework

In the prototypical evaluation problem, the effect on some outcome of a single 'treatment' of interest is assessed relative to another comparison treatment (the latter generally corresponding to the non-administration of the treatment of interest).

When it comes to the evaluation of a country's active labour market policy, however, the 'treatment' is no longer homogeneous, but is made up of various kinds of programmes which may well differ in terms of their effects on the outcome of interest. In such a context, a natural question arises as to the relative effectiveness of the different types of measures.

This sub-section sketches the framework recently 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.

More precisely, let a set of $K+1$ different kinds of mutually exclusive treatments[17] be available to any given individual. As a concrete example, the choice set of an unemployed individual may contain K types of programmes as well as a 'no-programme' option.

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[17] Or equivalently, in a dose-response model, the treatment of interest is allowed to take on integer values between 0 and K.

Table 3.2 Selected individual descriptive statistics, by type of exit from first open unemployment spell

	Programme participants						Exits from unemployment			
	Train-ing	ALU	API	Relief	Replace-ment	Subsidy	Em-ployed	Exit labour force	Regular educat.	Attri-tion
Age at entry (years)	38	41	37	40	38	39	38	35	33	37
Gender (% female)	43.5	43.7	50.4	26.8	79.3	33.8	47.0	77.4	66.2	49.4
Foreign (%)	6.1	7.4	21.2	9.2	3.7	4.7	4.6	7.1	5.9	10.2
Education (%): compulsory	29.6	33.5	35.3	33.5	18.0	32.6	26.0	24.9	22.2	30.4
vocational upper secondary	53.2	41.1	35.3	50.3	48.9	47.4	45.3	45.0	41.2	39.5
University	8.5	17.6	22.1	9.0	26.7	12.9	22.3	19.7	19.8	20.9
Educat. for job sought (% yes)	64.5	64.5	59.1	66.8	77.4	67.6	73.4	67.8	54.6	64.4
Experience (%): some	9.8	12.0	15.5	11.8	17.0	12.7	11.5	15.2	19.6	14.2
good	83.6	79.9	64.2	82.7	71.6	83.1	82.7	76.2	59.3	77.9
KAS (%)	5.8	6.3	13.6	14.8	5.2	12.7	8.2	6.1	6.1	15.8
Previous wage (SEK, daily)	641	667	602	665	555	647	665	591	587	617
Prev. working hours (% 40)	84.0	83.1	79.8	86.2	67.7	87.6	81.1	75.2	72.6	76.7
Sector (%)										
admin., manag. and clerical	19.8	16.5	18.1	6.4	8.7	16.4	13.4	17.6	16.2	12.0
sales	12.0	13.5	15.1	9.0	5.2	23.0	10.5	13.6	10.6	12.6
production	31.5	25.2	18.6	48.9	6.0	22.5	26.2	11.0	11.0	18.9
services	10.2	10.1	14.6	9.9	9.1	8.9	9.6	13.6	9.6	15.0
Looks for part-time job (%)	3.9	6.5	5.6	4.0	9.7	4.2	7.2	11.9	6.7	7.8
Part-time unemployment (%)	11.5	12.3	22.1	7.3	35.4	15.5	33.2	36.6	21.4	38.3
Needs guidance (%)	16.8	11.4	19.5	10.6	4.6	7.7	3.3	6.6	6.4	5.9
Unempl. duration (days)	232	349	507	277	217	319	249	329	208	413
Observed days on programme	116	148	141	137	125	146				
Number	1,387	2,983	425	654	483	426	15,972	2,680	2,456	2,739
Percent of total (= 30,863)	4.5	9.7	1.4	2.1	1.6	1.4	51.8	8.7	8.0	8.9

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 to each of the *K*+1 states: Y^0, Y^1, …, Y^K, with Y_i^k denoting the outcome *Y* for individual *i*, if *i* were to receive treatment *k*. Let $T \in \{0, 1, …, K\}$ denote the actual assignment to a specific treatment, so that $T_i=k$ if individual *i* receives treatment *k*. Since each individual receives only one of the treatments, his remaining *K* potential outcomes are unobserved counterfactuals.

Note that for this representation to be meaningful, the stable-unit-treatment-value (SUTVA)[18] assumption 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 condition ruling out the possibility of general equilibrium or cross-effects.

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 $(K+1) \bullet K$ 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'*:

$$E(Y^k-Y^{k'}|T=k) = E(Y^k|T=k) - E(Y^{k'}|T=k) \quad \text{for } k, k' \in \{0, 1, …, K\}, \ k \neq k'.^{19}$$

In our case, this amounts to assessing the average effect for an individual registering as unemployed in Sweden of participating in programme *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 since no individual can be in more than one state at the same time, all but one of the *K*+1 potential outcomes are not observed for any given individual.[20] One such

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[18] First expressed by Rubin (1980) and further discussed in Rubin (1986) and Holland (1986).

[19] Note that 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 programmes systematically differ in characteristics related to the outcome.

[20] 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 (1998), as well as Heckman and Robb (1985), Heckman, Ichimura and Todd (1997,

assumption often invoked in evaluation exercises is the conditional independence assumption (CIA), an extension of which would allow us to identify all the counterfactuals:[21]

$$T \perp (Y^0, Y^1, ..., Y^K) \mid X=x, \ \forall \ x \in C^*$$

This identifying assumption (termed 'strong unconfoundedness' by Imbens, 2000) requires the existence of a set of observable characteristics X (variables unaffected by the treatments, defined as 'attributes' by Holland, 1986) such that, conditional on their values x, 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).

Note however that a weaker form would in fact suffice to identify the conditional treatment effects we are interested in: [22]

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

Since we are just interested in the pair-wise comparison of the various kinds of treatments, we can relax strong unconfoundedness by requiring conditional independence to hold only for the sub-populations receiving either treatment k or treatment k' (see Lechner, 2001): 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.

The unobserved counterfactuals can thus be identified as:

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

where the inner expectation is identified due to CIA (*) 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

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(1997, 1998), Heckman, Ichimura, Smith and Todd (1998), Rosenbaum and Rubin (1983, 1985) and Rubin (1974).

[21] Its weaker form in terms of conditional mean independence would suffice.

[22] Again, the requirement could just be in terms of conditional mean independence.

to be performed over the common support region[23]; the estimated treatment effect has then to be redefined as the mean treatment effect for those treated k falling within the common support.[24]

Formally, define the (generalised) propensity score as the conditional probability of receiving a given type of treatment given X:

$$P^k(X) \equiv Pr(T=k|X)$$

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

$$0 < P^k(X) < 1 \quad \text{for } X \in C^* \text{ and } k=0, 1, \ldots, K. \text{ [25]}$$

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

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. $T \perp X \mid b(X)$. A necessary and sufficient condition for a function of X to be a balancing score is to be at least as fine as the (generalised) propensity score $P^k(X)$:[26]

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

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, say for k and k':

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

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[23] Alternatively, identification would rely on (parametrically) extrapolating from regions of C^* that have positive probabilities for both the treatment states being compared to occur.

[24] Note that if the treatment effect varies among individuals, restricting to the common subset may actually change the parameter being estimated.

[25] 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. $P^{k'}(X) > 0 \ \forall X \in C^*: P^k(X) > 0$.

[26] Cf. Theorem 2 by Rosenabaum and Rubin (1983) and proposition 1 in Lechner (2001).

which is verified iff

$$E[Pr(T=k|X, T \in \{k, k'\})|b(X)] = Pr(T=k|X, T \in \{k, k'\}) \equiv P^{k/kk'}(X)$$

$$0 < P^{k/kk'}(X) < 1.$$

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':[27]

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

Under the CIA, the required counterfactual can thus be estimated as follows:

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

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

4.2 Multiple-treatment matching in the Swedish institutional set-up

An important initial clarification concerns the definition of the 'no-programme' state in Sweden.[28] In general, sooner or later an unemployed individual will go on a programme, provided he remains unemployed 'long enough'. In other words, if unemployed individuals in Sweden are not observed to go into a programme, it can be argued that it is *because* they have found a job (before). Using as no-programme group those individuals who are observed not to enter any programme (thus *de facto* observed to leave the unemployment register) would *a priori* set programme participants at a disadvantage.

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[27] Cf. also Brodaty, Crépon and Fougère (2000).
[28] The discussion of an absent 'non-treatment' group was initiated by Carling and Larsson (2000a, b).

A connected important feature in the programme selection process in Sweden is the fact that unemployed job-seekers and case-workers are most likely to take their decisions sequentially over time in unemployment. In particular, at any given moment the relevant decision is between joining a programme now or not participating *for now*, in the knowledge that one can always join later on. The key choice faced by the unemployed in Sweden is thus a decision between either participating in a programme now or else searching longer in open unemployment whilst availing themselves of the services offered by the employment offices. Correspondingly, when looking at the inflow into unemployment, a natural parameter to evaluate in the Swedish institutional set-up (in addition to the pair-wise comparisons of the various programmes) is the average effect of joining a given programme compared to further postponing the participation decision by not joining any programme *at least up to then*.

The aim of the paper thus consists in quantifying the differential effectiveness on subsequent labour market performance (e.g. employment probability over time) of seven different types of treatments: labour market training, work experience, job introduction, relief work, trainee replacement, job subsidies and searching longer in open unemployment.

Implementation

In Section 4.1 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-programme nature of the environment the individuals face. As Lechner (2001) clearly points out, when interested in comparing two programmes 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 programme considered are not needed for identification.

However, considerable attention should be devoted to the specification of the treatment probabilities, and it is in fact their estimation which offers an opportunity to capture and take account of the multiplicity of options open to individuals.

In the Swedish context in particular, it was argued 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 programmes, 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 single individual unemployment spell of a given number of days 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 conditional probability of choosing option k after having spent U months in unemployment, $P(T_i=k \mid U_i, X_i)$, is then estimated[29] and the corresponding balancing scores constructed. Since our interest in the estimation of the balancing score purely lies in its ability to balance the characteristics of the matched sub-groups being pair-wisely compared, this criterion has guided the choice, for each pair-wise comparison, of which specification to use as a basis for matching.[30]

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[29] 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. Time-varying observables other than elapsed unemployment duration U are defined conditional on U_i or on calendar time, and 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 employment office of the individual. A thorough discussion of the conditioning variables is deferred to the next sub-section.

[30] See TableA1 in the Appendix for the definition of the indicators used to assess matching quality, as well as for the final choice of specification for each pair-wise comparison. Specifications which have been tried are: multinomial logit on the full set of exits, on an aggregation of some of them; a series of binomial probits; Mahalanobis-metric matching on the balancing score and unemployment duration if the latter resulted unsatisfactorily balanced; Mahalanobis-metric matching on both participation probabilities in the case of the multinomial logit; imposing a caliper (i.e. a maximum tolerable distance between the scores of a treated and his matched control) when differences in the matched scores were deemed excessive.

Matching estimators can be implemented in wide variety of ways[31]; the analyses of this paper are based on one-to-one matching, performed with replacement. Since pair-wise comparisons are performed across all (differently-sized) sub-samples, each sub-group will act both as a treated group and as (several) comparison groups, entailing thus the need to use a given individual more than once in a given comparison. The variance has to be adjusted accordingly: the more times a comparison observation is used, the larger the related standard error of the estimated effect.[32]

To compare programme k and programme k' for participants in programme k, each k-participant is matched to that k'-participant based on the balancing score. The differential performance of the two matched groups then starts being evaluated from entry into the respective programme.

To estimate the average effect of joining a given programme k compared to waiting longer (than they have) for participants in programme k, the corresponding balancing score is calculated for each k-participant and each waiting spell. The procedure then follows closely the 'stratification' approach in Sianesi (2002a). In particular, k-participants and waiting individuals are stratified by unemployment duration $U=1, 2, \ldots, U_{max(k)}$. For a given unemployment duration $U=u$, those k-participants who enter the programme in their u^{th} month are matched to the most similar individuals who are still unemployed after u months. The evaluation of the average effect of joining programme k in one's u^{th} month of unemployment compared to not joining any programme at least up to one's u^{th} month (i.e. compared to waiting in open unemployment longer than u months) starts from entry into

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[31] See e.g. Heckman, Ichimura and Todd (1997 and 1998), Heckman, Ichimura, Smith and Todd (1998), Dehejia and Wahba (1999), Rosenbaum and Rubin (1985), Cochran and Rubin (1973).

[32] The variance of conditional average effect of treatment k relative to treatment k' at time t is calculated by assuming independent observations, fixed weights, homoskedasticity of the outcome variable within the k-group and within the k' comparison group and that the variance of the outcome does not depend on the balancing score (as to the latter, bootstrapping would have been performed if it had not been for the amount of computer time needed to estimate the multinomial model on such a large sample):

$$\frac{1}{N_t^k}Var(Y_t \mid T_t^M = k) + \frac{\sum_{i \in \{T_t^M = k'\}} \omega_i^2}{(N_t^{k'})^2}Var(Y_t \mid T_t^M = k')$$

where N_t^k is the number of matched treated k present at time t, $T^k=1$ denotes the matched k-participants, $T^{k'}=1$ the matched k' comparisons and ω_i is the number of times comparison i has been used, where $\sum_{i \in \{T_t^{k'}=0\}} \omega_i = N_t^k$.

programme k, namely from $U=u$. Subsequently aggregating all the $U_{max(k)}$ effects of programme k by time of entry would then recover the average effect, for those observed to join programme k, of joining when they did compared to waiting longer than they have, where the average is taken with respect to the observed entry distribution into programme k.[33]

When interpreting the results on the effect of joining a given programme rather than waiting longer in open unemployment it is thus important to keep in mind that the chosen comparison group does not reflect a no-programme state, but rather a possibly postponed participation. In particular, the matched comparison group may turn out to partly consist of individuals enrolling into the same[34] or another programme later on. What is crucial to this end is that the CIA is satisfied in our case, namely that the probability distribution of subsequent outcomes (employment, de-registration for other reasons or participation in the various programmes) for the then openly unemployed matched comparisons is the same as the one for the observably-similar treated individuals had they then decided to wait longer as well.

4.3 Plausibility of the matching approach in the Swedish context

The method just outlined relies on the central assumption that we observe – and thus can match on – all those differences between the various treatment groups that are likely to affect their outcomes. The plausibility of the CIA should be discussed in relation to the richness of the available dataset as well as the selection process into the Swedish programmes. To this end it may be useful to separately consider:

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[33] Figure A1 in the Appendix shows our sample's entry distribution into the six programmes by month in open unemployment. ALU and API stand somewhat apart in that entry is much more concentrated around the time of UI exhaustion.

[34] Note to this respect that a given treated individual may also turn out to act as control in the estimation of the effect of joining a given programme compared to waiting longer. In fact, the *relative* time scale, which is implicit in the set-up of the problem and central in the way it has been addressed, makes it quite intuitive to think of him as different persons, whose contributions start at different origins. In particular, a given treated individual counts as one treated person, whose contribution starts being evaluated at the moment he enters the programme (i.e. from *his U*), and may count as control person for 'otherwise similar' treated individuals who have joined the same programme before him. In this latter case, under the CIA his outcome represents the waiting counterfactual outcome for his matched treated individuals, where evaluation begins when his matched treated enter the programme (i.e. from his matched treated individuals' *U*).

(1) the decision between waiting further in open unemployment or joining a (i.e. any) programme;

(2) the decision to choose one specific programme among the available ones.

As to decision (1), we need to control for all variables that, conditional on having spent a given amount of time in unemployment, influence both the decision to join a programme as well as potential future labour market performance were such decision to be postponed further. From work by Harkman (2000, as reported in Carling and Richardson, 2001) it appears that an unemployed individual's decision to participate in any programme or not to participate may depend on the individual's subjective likelihood of employment. Although unobserved to us, we are however able to control for several factors which may be highly correlated with it. In particular, several pieces of information are used to capture and characterise the employment history of the individuals under examination. All our individuals register at the unemployment office for their first time, so their only unemployment experience relates to the present unemployment spell. If – as it is likely – an individual takes his decisions over time spent in unemployment, controlling for elapsed unemployment duration should allow us to capture important unobservables (e.g. perceived deterioration of human capital, stigma effect, loss of hope or motivation, etc.). Similarly, all our individuals, being entitled to unemployment benefits, are also characterised by a good degree of labour market attachment due to the work requirement they have to fulfil.[35] Indicators of having run out of unemployment benefits or of benefits expiring have also been included, since such a situation would make an individual more likely to join a programme or, if having to wait longer, more likely to enter a programme later on or to intensify his job search (or lower his reservation wage). Another important individual attribute is the pre-unemployment wage, a summary statistic of the worker's past labour market situation.

Similarly, we have controlled for factors relating both to employment prospects and either to potential returns from programme participation or affecting the opportunity cost or psychological cost of participation (age, gender, previous stock of human capital in terms

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[35] An indicator of KAS entitlement controls for the additional way to fulfil the 'work' requirement.

of both specific and general education and job-specific experience[36], occupation being sought, citizenship, part-time unemployment status[37]).

The Swedish unemployment offices are characterised by decentralisation, which gives job officers quite a large degree of freedom. Also, although in general an unemployed job-seeker must be willing to participate, this may not always apply to individuals receiving unemployment compensation; for them, the proposal of a programme can be used as a 'work test', the turning down of which may entail suspension from benefits. To capture this selection by caseworkers, additional useful information has been included which relates to an overall evaluation by the officer of the situation and character of the unemployed job-seeker – if already part-time employed, if looking for a part-time job, if willing to move to another locality, if judged to be able to take a job immediately, or to be in need of guidance, or to be difficult to place. Such individual traits are potential indicators of unobserved heterogeneity and are quite likely to affect the joining decision as well as the counterfactual outcomes in terms of subsequent participation or employment probability. An interesting piece of information in the Swedish dataset is an unemployment spell characterised by having been offered a labour market programme. Having gone through the selection process and having been offered a place makes it more likely for the individual to join a programme 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.

The possibility of anticipatory effects in terms of future employment would violate the CIA underlying the estimation of the joining *versus* waiting effects. In particular, if some unemployed workers 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 the temporarily dismissed employee to collect unemployment benefits), they are likely to have no (or

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[36] Difference in prior work experience is important since it results from both observed and unobserved individual characteristics (cf. Ham and LaLonde, 1996). Our subjective indicator of experience for the profession sought (none, some, good) can be viewed as a summary statistic of the amount – as well as effectiveness, transferability and obsolescence – of previous human capital accumulation, on-the-job training and learning-by-doing.

[37] 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.

less) incentives to participate in programmes 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, the CIA would be violated in the presence of hidden job offers, that is if an individual waiting longer has decided to do so *because* he *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/is allowed to remain registered at the unemployment office in the meantime).

Turning now to decision (2), i.e. the selection mechanism into the various programmes, the CIA requires the evaluator to have access to all the variables that influence both the choice between the programmes as well as potential future outcomes that would occur had the individual chosen an alternative programme. Note 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; benefit renewability rules and individual compensation while on the programmes are similarly comparable across programmes.

Harkman (2000) finds that while individual self-selection into different programmes is likely to be a minor issue in Sweden (unemployed workers tend to value the various programmes equally), the caseworkers do seem to have clear ideas about which type of programme 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 and correlated with labour market outcomes. We do however observe not only important characteristics of the unemployed client, but also the caseworker's own subjective and synthetic evaluation of the overall situation and needs of service of his unemployed client as described above. In a sense, the caseworker reveals 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 conditional on all this information, programme as-

signment is unrelated to outcomes; caseworkers or employment offices act idiosyncratically given worker characteristics (based e.g. on their preferences, incentives, experiences, colleagues' opinions). Carling and Richardson (2001), who carefully examine the factors that determine which programme 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 relates to the gradual shift towards more decentralised decision-making as to labour market programmes that has taken place in Sweden in the second half of the 1990s and to the concomitant emergence of new financial incentives (cf. Lundin and Skedinger, 2000): municipal budgets may be favourably affected by moving unemployed individuals from social assistance (funded by the local authorities) to programmes (financed by the central government); some programmes (e.g. relief work) may subsidise labour in the services typically provided by the local authorities; and programmes may serve as a means of maintaining the local municipal tax base, by reducing migration among the unemployed. In addition to county indicators, a set of local indicators at the individual's municipality / employment office level over time have thus been constructed to further control for the possibility that individual joining decisions and/or office-specific programme selection criteria may be based on local unobserved characteristics in turn correlated with individuals' potential labour market performance. Such controls include the local 'programme-rate', given by the share of registered unemployed job-seekers participating in any programme; the local 'offer-rate', representing the proportion of unemployed workers who have been offered a programme out of all openly unemployed; and a series of single programme ratios, reflecting the programme mix at that office and at that time.

5. Empirical findings

In this section, differential programme effects and the effect of joining a given programme vis-à-vis waiting longer in unemployment are assessed in relation to two important outcomes: individuals' employment rates over time and the probability of being in a compensated unemployment spell over time.

5.1 Employment probability over time

The effect of joining programme *A* (compared either to joining another programme or to searching longer in open unemployment) on the employment probability[38] of participants in programme *A* is calculated from the start of the programme to five 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 the programme vis-à-vis the comparison treatment originates from a differential job search while on the programme. Compared to open unemployment, job search is clearly reduced because less time is left due to participation itself. Different programmes may however also differentially reduce the intensity of job search while participating in the respective programme: they may for instance leave different amounts of time and energy for job search or may entail different 'promises' once completed (e.g. employment subsidies may induce participants to focus on the job at hand to 'impress' the employer in order to increase the likelihood to remain with the firm afterwards).

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

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[38] An individual counts as employed if registered at the unemployment office as employed (e.g. temporary or looking for a new job) or if de-registered for having found employment.

Finally, a differential degree of job longevity may be the result of the different extent to which the programmes improve the individual's working habits, skills, adaptability or ability to learn on the job.

As to the effect from participating in a given programme compared to longer job-search as openly unemployed, all the programmes considered are found to have a negative impact on their respective participants' short-term employment prospects. As shown in Figure 5.1, joining any of these programmes initially locks participants in, reducing their chances of being in employment by an over 15 percent probability in each case. However, the more long-term effect of joining a programme is found to critically depend on the type of programme the individual has entered. In particular, for our sample of entitled adult unemployed workers it seems more worthwhile to intensively search longer in open unemployment rather than joining labour market training, ALU or relief work. Even after the programme typically ends, these participants subsequently enjoy lower employment rates than if they had postponed the joining decision further. These negative effects persist over a substantial time horizon before turning insignificant (around one and a half years in the case of training, almost three years for ALU and over four and a half years for relief work). A possible explanation, to be explored below, is that these programmes may not provide participants − and especially participants entitled to unemployment benefit − with skills marketable enough to make the working option sufficiently attractive; these programmes may thus end up being typically used by entitled individuals simply as a passport to renewed eligibility.

Participants in API and in trainee replacement on the other hand are just as well off as if they had waited longer. By contrast, the decision to join a job subsidy programme rather than searching further in open unemployment results in significantly and persistently higher employment rates (up to 40 percentage points) soon after the programme typically ends.

Figure 5.1 Average effect on employment probability over time of joining the specified programme compared to waiting longer in open unemployment for participants in the specified programme
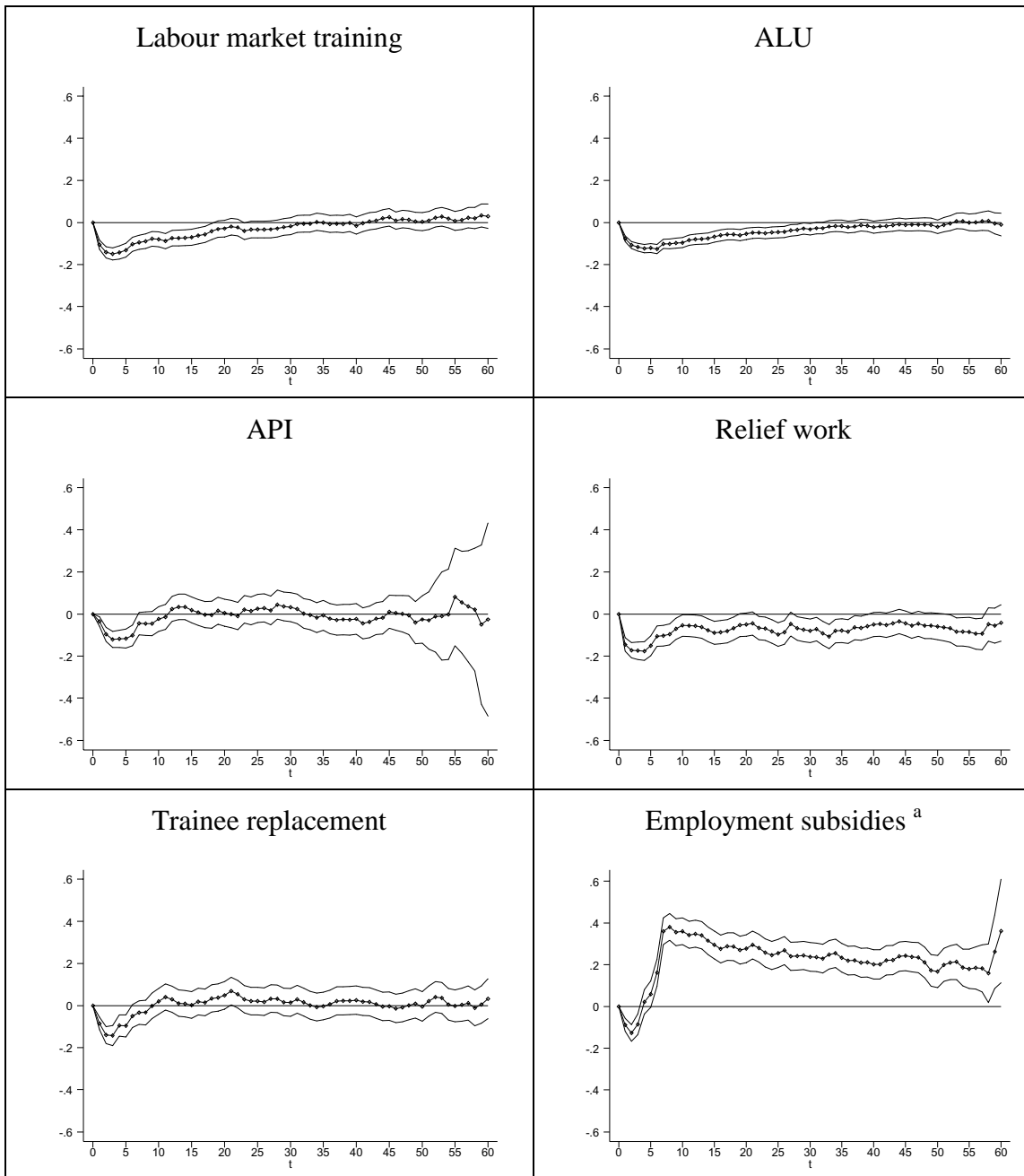
Notes: Time in months, from programme start. 95% confidence intervals bands.
  [a] Employment probability obviously refers to a regular (i.e. non-subsidised) job.

Table 5.1 summarises the above results as well as the main picture that emerges from the series of graphs plotting the differential programme effects on employment probability over time for all the pair-wise comparisons of the programmes.[39] Note that although later in the section ALU and API will be explicitly contrasted, in the table and the following discussion these two programmes centred on work experience have been lumped into one type of treatment, 'work practice'. The two measures have in fact a very similar overall aim, nature and implementation (in particular the requirement of not performing regular tasks), this at least formal equivalence having being sanctioned by the employment offices themselves in January 1999, when the two measures were collapsed into the new work practice scheme. ALU's additional eligibility requirement is also not binding in our sample of unemployment-benefit entitled individuals.

Table 5.1 Informal summary of the various conditional average treatment effects on employment probability over 5-year horizon since programme start

Comparison ↓	**Training**	**Work practice**[a]	**Relief**	**Replacement**	**Subsidies**
Waiting	◆ lock-in ◆ negative up to 30m ◆ then 0	◆ lock-in, ◆ negative up to 30m for ALU only ◆ then 0	◆ lock-in ◆ then negative	◆ lock-in ◆ then 0	◆ short lock-in ◆ then large positive
Training		0	mostly 0	positive	large positive
Work practice	0		mostly 0	positive	large positive
Relief	0	0		mostly 0 positive up to 15m	large positive
Replacement	negative then zero from 30m	negative	0 (neg. but insignificant at 95%)		large positive
Subsidies	large negative	large negative	large negative	mostly negative	

Notes: This summary takes informal account of the statistical significance of the estimated effects; for the complete set of results, see the Appendix.
m = month(s).
[a] ALU and API combined.

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[39] See the Appendix for the full set of results.

Turning to the results concerning the relative performance of the different programmes, both *a priori* expectations and the general picture arising from the raw data appear to be confirmed. The star programme is again clearly job subsidies – not surprisingly, given the job promise they generally entail. Individuals having joined this programme enjoy a much higher (20 to 40 percentage points) employment probability over time than if they had joined an alternative programme. In addition, participants in any of these other programmes (with the possible exception of trainee replacement schemes) would have fared considerably better had they gone on job subsidies instead. The second best performing programme is confirmed to be trainee replacement. Since the task performed is by construction a useful one, for which the firm was willing to pay a regular employee, the presumption that this programme should teach market-relevant skills is corroborated by the result that former deputies have considerably better outcomes than if they had joined any other of the remaining programmes (in particular, training or work practice). Conversely, trainees and work practice participants would have improved their labour market performance had they joined a replacement scheme. As to the remaining programmes – labour market training, work practice and relief work, they do not seem to perform much differently from one another.

5.2 Unemployment-benefit collection probability over time

Since we are looking at individuals who are entitled to unemployment benefits and for whom the eligibility-renewability property of the programmes is likely to represent a particularly attractive feature likely to affect incentives, we additionally consider the differential treatment effects on the probability of being effectively drawing unemployment compensation over time.[40]

The performance of job subsidy participants stands out again: they are significantly less likely to be on unemployment benefits over time than if they had participated in any other programme, and participants in the other programmes would have been less likely to be drawing benefits over time had they gone on a subsidised job, the only exception again being replacement schemes, participants in which do not seem to perform substantially differ-

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[40] The results are displayed in the Appendix.

31

ently in this dimension than if they had gone on subsidised jobs.[41] What is even more striking is the negative, mostly significant effect on the likelihood of compensated unemployment of joining employment subsidies compared to waiting longer in open unemployment.

In fact, employment subsidies is the only programme to display a negative effect on benefit collection probability compared to postponing the participation decision. While replacement schemes have a zero effect beyond the initial five months[42], participants in training, API, ALU and relief work all have a significantly higher likelihood of compensated unemployment over time than if they had waited longer in unemployment – clear evidence in favour of the likely role played by benefit renewability considerations in the above finding of a negative treatment effect on employment rates displayed by these latter measures.

Coming back to the pair-wise comparison of the programmes in terms of compensated unemployment probability, replacement schemes have a negative effect compared to training, but no effect compared to the other programmes. Conversely, participants in training, work practice and relief work would have been less likely to be in compensated unemployment had they joined a replacement scheme instead. Again, these three kinds of programme do not perform significantly differently from one another in terms of benefit collection probability. Interestingly, in the case of relief work and especially work practice participants, clear evidence of unemployment-programme 'cycling' effects is visible, with significant positive effects (compared both to some other programmes and especially to the waiting longer option) arising between the 6th and 20th month (i.e. after programme end and up to the maximum 14 months of compensated unemployment), and often between the 27th and 38th month (a second cycling spell, starting from the end of a second programme).
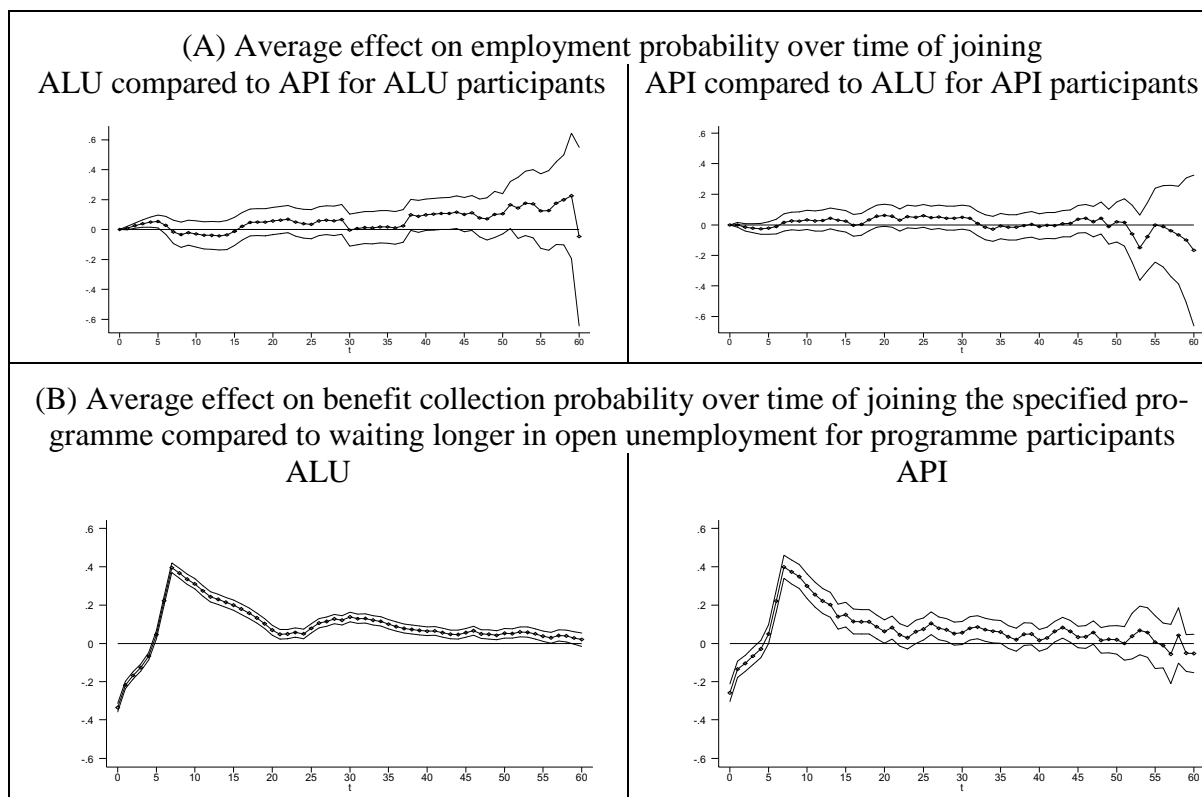
5.3 API versus ALU

As to the two work practice measures, their potentially different effectiveness is of particular interest, since while sharing the basic features of API, ALU is exclusively reserved to individuals entitled to unemployment benefits and has been explicitly introduced to prevent

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[41] The initial positive effect from start of the replacement programme to up to 5 months reflects the fewer *direct* programme-employment transitions that deputies experience compared to subsidised workers.

[42] By construction, individuals do not draw unemployment compensation while on the programme.

them from running out of compensation. In terms of employment probability over time, while participants in one of the two programmes would not have fared better had they joined the other programme instead (Figure 5.2A), compared to waiting longer in open unemployment the performance of ALU is visibly worse than the one of API (Figure 5.1). In addition, ALU participants are somewhat more likely to be drawing benefits on a 'cycling' basis compared to waiting longer than do API participants had they waited longer too (see the clearly delineated second hump[43] for ALU in Figure 5.2B). Thus even when conditioning on unemployed individuals entitled to benefits, there seems to be scope for the explicit, close link between entitlement renewability and programme (as institutionalised in the case of ALU) to impact on the programme's effectiveness on the labour market performance of its participants.[44]

Figure 5.2 Differential performance of ALU and API

(A) Average effect on employment probability over time of joining
ALU compared to API for ALU participants | API compared to ALU for API participants

(B) Average effect on benefit collection probability over time of joining the specified programme compared to waiting longer in open unemployment for programme participants
ALU   API

Notes: Time in months, from programme start. 95% confidence intervals bands.

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[43] From the 27th ($\approx$6+14+6) to the 40th ($\approx$27+14) month.
[44] For more analyses of the linkages between entitlement, programme participation, benefit exhaustion and 'cycling' behaviour, see Sianesi (2002a and b).

5.4 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 seriously under-reported employment status is 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 status (employed *versus* unofficially unemployed) once in a lost spell may be systematically different among individuals taking the various treatments, i.e. entering one of the available programmes or searching longer in open unemployment. 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. It would thus seem important to check the robustness against these lost spells of the findings on employment rates.

Following Sianesi (2002a)[45], 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. As shown in the Appendix (Figure A12, to be contrasted with Figure 5.1 above)[46], the conclusions discussed above remain in fact virtually unaffected, in particular regarding the positive employment effect of job subsidies and the negative ones of relief work, ALU and training.

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[45] The conditional probability that a lost individual ($L=1$) with characteristics X has in reality found employment ($Y=1$) can be decomposed as: $P(Y=1|X=x, L=1)=P(Y=1|X=x, L=1, D=1) \, P(D=1|X=x, L=1)+P(Y=1|X=x, L=1, D=0) \, [1-P(D=1|X=x, L=1)]$, 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 $P(D=1|X_i, L_i=1) \equiv p^D_i$ and based on the survey we can impute his misclassification probability $P(Y_i=1|X_i, L_i=1) \equiv p^Y_i$.

The procedure to derive worst- and best-case bounds consists in assigning $P(Y_i=1|X_i, L_i=1, D=d_i)$ by setting $P(Y_i=1|X_i, L_i=1, D=1-d_i)$ to its maximum or minimum, compatible with the given p^D_i and p^Y_i, as well as with all probabilities being in [0; 1].

[46] The full set of results is available from the author upon request.

6. Discussion and conclusions

The analyses in this paper have investigated the differential performance of six main types of Swedish programmes both relative to one another and vis-à-vis more intense job search in open unemployment.

Starting from this latter comparison, the results concerning programme effects on employment and compensated unemployment have been discouraging for all the programmes considered except job subsidies (and possibly replacement schemes).

Several factors (in addition to a possible violation of the identifying assumption underlying the method chosen for analysis) 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[47] (though it may be argued that it is exactly in such difficult times when effective labour market programmes would be most needed). There is also the connected issue of the scale of the programmes; the massive use of large-scale programmes in the 1990s is likely to have resulted in inefficient programme administration.[48]

An additional most likely explanation however relates to the use of the programmes simply as a way to re-qualify for unemployment benefits, with programmes ending up locking their participants – and in particular those entitled to unemployment compensation – in the unemployment system.

In fact, when looking at these six programmes taken as a whole compared to waiting longer in open unemployment, the results – both in terms of employment rates and of benefit collection probability over time – for the sub-sample of entitled adults considered here are considerably worse than those obtained for the sub-sample of adults not entitled to unemployment benefits (cf. Sianesi, 2002b).[49] Contrasting these two sets of results would thus

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[47] See e.g. 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).

[48] In principle there could also be a stigma effect linked to participation in these programmes; this is however not confirmed by Swedish evidence, according to which employers view former programme participants more favourably than openly unemployed individuals. For a review of the relevant survey studies, see Calmfors, Forslund and Hemström (2001).

[49] In particular, joining one of the six programmes (rather than waiting longer in open unemployment) has a persistently negative employment effect for the entitled, but a significant and substantial positive effect for the non-entitled.

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 several programmes.

In particular, the present analysis has found that individuals joining labour market training, workplace practice schemes or relief work subsequently display lower employment rates coupled with a higher benefit collection probability than if they had searched further as openly unemployed.

As to the pair-wise comparison of the effectiveness of the six programmes, it is interesting to start by considering the work by Carling and Richardson (2001), a Swedish study most similar in aim and sample selection[50] to the present one. The present study can be seen as a 'robustness' analysis (using a different methodology from their hazard regression model), as well as complementing the previous one, in which programmes are evaluated along one dimension: their ability to reduce unemployment duration (measured from start of the programme), thus ignoring what happens once a job is found.[51]

It is thus both reassuring and interesting to notice how their main finding is confirmed in our analyses looking at further types of outcomes. Those programmes providing (subsidised) workplace experience and on-the-job training at an employer are relatively more effective in terms of participants' subsequent labour market performance than vocational classroom training courses. In addition, the more relevant the kind of task performed, the higher the programme ranks. More specifically, the top six programmes (from the eight) emerging from their results in term of unemployment duration (cf. their Table 3) are: 1. job subsidies, 2. trainee replacement, 3. work practice (API), 4. labour market training, 5. relief work and 6. work practice (ALU).

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[50] They examine the relative efficiency of eight Swedish programmes – the same six programmes examined here plus self-employment grants and computer/activity centres – for adult unemployed becoming unemployed for their first time in slightly later years than ours (between 1995 and 1997).

[51] They also do not consider the impact of the option of intensive job search in open unemployment, and thus do not investigate whether participation in any programme is better or worse than postponing the participation decision. On the other hand, they examine (providing a negative answer) the issue of whether the programmes' relative efficiency is affected by how long an individual has been unemployed before joining, or if it depends on participants' demographics and skills.

Even more generally, the underlying similarity of results across studies looking at different countries with varying labour market structures and policies may indicate a general validity of the overall conclusions.[52]

Coming back to the present evaluation, the best performer overall is undisputedly employment subsidies, followed by trainee replacement. As to the remaining types of programmes, they do not seem to perform in a significantly different way between one another.

Turning to the cost side, it is quite remarkable to notice how the ranking of the programmes in terms of their effectiveness is almost perfectly reversed when taken in terms of their expensiveness (1. labour market training, 2. ALU, 3. relief work, 4. trainee replacement, 5. API and 6. job subsidies).

It is however important not to jump at the hasty conclusion that employment subsidies are *the* solution – the most effective programme 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 programme be extended.

As to the scope of the analysis, it is important to bear in mind that the programme'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 programme: since job subsidies generally entail the 'promise' of a job, it is likely that potential candidates are considered quite carefully. Even though we control for a host of factors likely to underlie the case-worker's judgement and despite Carling and Richardson's (2001) finding of no selection bias for this programme, 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 selection bias (also possibly in the form of anticipatory

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[52] 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) and Bonnal, Fougère and Sérandon (1997) for France, and Ridder (1986) for the Netherlands.

effects) 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 employment subsidy programme were to take (some of) the jobs that participants in the other programmes 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 programmes 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 programme. Both survey and econometric Swedish studies do in fact find sizeable (around 65-70 percent) direct displacement effects arising from those Swedish programmes that generate subsidised employment.[53]

Finally, it is obviously unthinkable to generalise such a measure to *all* unemployed jobseekers: it would simply become just a way to subsidise firms' hirings, resulting in huge dead-weight 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 schemes. Still among the cheapest programmes, 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

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[53] For more details, see Calmfors, Forslund and Hemström (2001).

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time it sends out a message that the individual 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. Finally, our partial-equilibrium estimates are likely to be an underestimate of the programme's effect, since they do not take into account the 'double-dividend' effect 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 programme 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[54], 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 *same* employer. Finally, survey studies have in fact uncovered displacement effects of the same order as employment subsidies (e.g. AMS, 1998).[55]

In conclusion, the present analysis unambiguously joins previous micro studies in finding that the closer to regular, relevant employment in the competitive labour market, the higher the programme'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 programmes the potential for negative crowding-out and dead-weight effects is largest. Taken together[56], the various results clearly highlight the difficult trade off faced by labour market policy.

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[54] 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.)

[55] 42 percent as an average across survey studies, see Calmfors, Forslund and Hemström (2001).

[56] See in particular Calmfors, Forslund and Hemström (2001).

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Appendix

Table A1: Specification chosen and indicators of resulting matching quality

Treated	Comparisons	Specification	Median bias	Bias in U
training	waiting	MNL, all states	1.85	–
experience	waiting	MNL, all states	2.12	–
introduction	waiting	MNL, all states	3.30	–
relief	waiting	MNL, all states	2.55	–
replacement	waiting	MNL, all states	2.35	–
subsidy	waiting	Probit	3.07	–
training	work practice	Probit, finer on U	3.90	0.41
training	relief	MNL, all states	6.57	3.58
training	replacement	Probit, finer on U	5.55	2.77
training	subsidy	Probit, finer on U	7.98	2.74
work practice	training	Probit, finer on U	4.20	0.73
work practice	relief	MNL, fewer states	4.86	1.78
work practice	replacement	Probit, finer on U	6.08	2.63
work practice	subsidy	Probit, finer on U	4.67	1.04
relief	training	Probit, finer on U	3.16	1.48
relief	work practice	Probit, finer on U	2.79	0.09
relief	replacement	Probit	7.00	1.36
relief	subsidy	MNL, all states, finer on U	7.83	3.22
replacement	training	Probit, finer on U	4.18	0.06
replacement	work practice	Probit, finer on U	5.26	0.98
replacement	relief	Probit, finer on U	11.16	1.53
replacement	subsidy	Probit	9.85	5.17
subsidy	training	Probit, finer on U	3.14	1.18
subsidy	work practice	Probit, finer on U	3.40	0.66
subsidy	relief	Probit, finer on U	6.14	0.29
subsidy	replacement	MNL, all states, finer on U	9.29	0.93
experience	introduction	MNL, all states, finer on U	10.87	2.5
introduction	experience	MNL, all states, finer on U	4.36	0.31

Notes: MNL: multinomial logit model. Finer on U: Mahalanobis-metric matching on the balancing score b and unemployment duration U, defined for individual i and individual j as $d(i,j) = (P_i - P_j)' \, S^{-1} \, (P_i - P_j)$, with $P_m = [b_m, U_m]'$ and S the pooled within-sample covariance matrix of P.
Median bias: median overall absolute percentage bias, where the median is taken over the post-matching absolute standardised differences of 70 variables in estimation of the choice model (the various programme rates are excluded from calculation of the median). For a given regressor, the standardised difference after matching is defined as the difference of the sample means in the treated and matched comparison sub-samples as a percentage of the square root of the average of the sample variances in the treated and comparison groups (cf. Rosenbaum and Rubin, 1985).

Figure A1 Programme entry distribution, by month in unemployment (%).
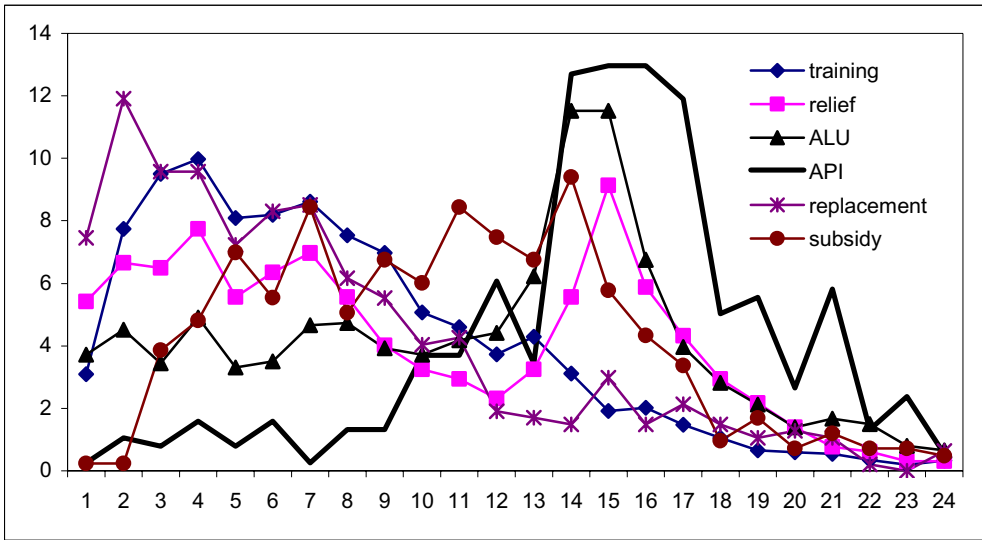
Figures A2-A6.
Differential average effects on **employment probability** over time of the specified pro-
gramme compared to the various alternatives for participants in the specified programme.

(percentage points; e.g. 0.2 is a 20 percentage points higher probability;
t-axis: months since joining the programme)

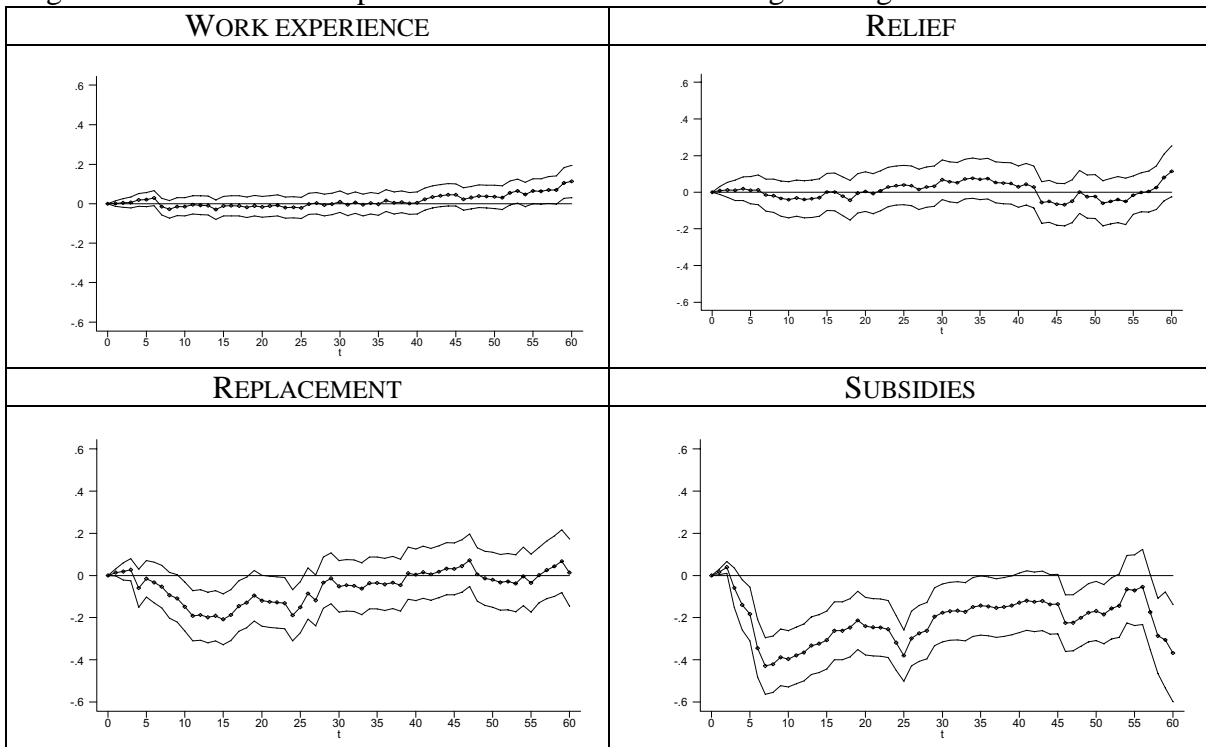Figure A2: TRAINING compared to … for individuals taking training

44

Figure A3: WORK EXPERIENCE compared to … for individuals taking work experience

TRAINING	RELIEF

REPLACEMENT	SUBSIDIES
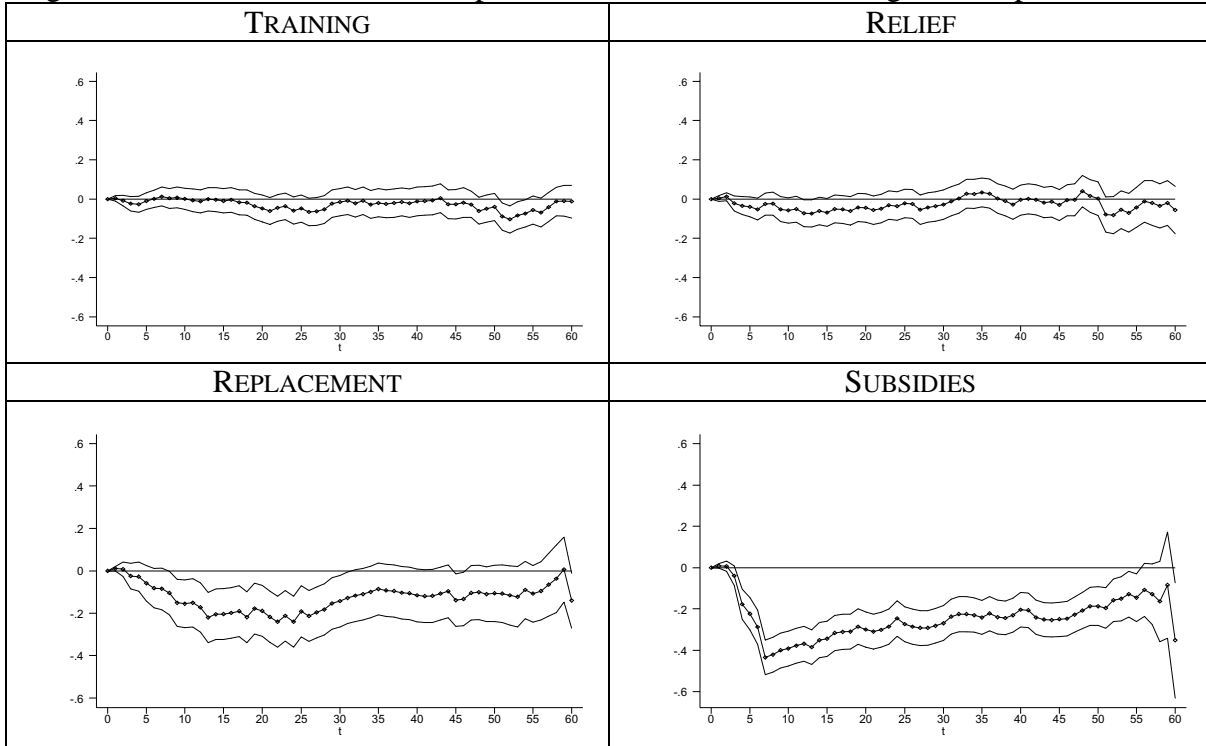
Figure A4: RELIEF compared to … for individuals taking relief

TRAINING	WORK EXPERIENCE

REPLACEMENT	SUBSIDIES

45
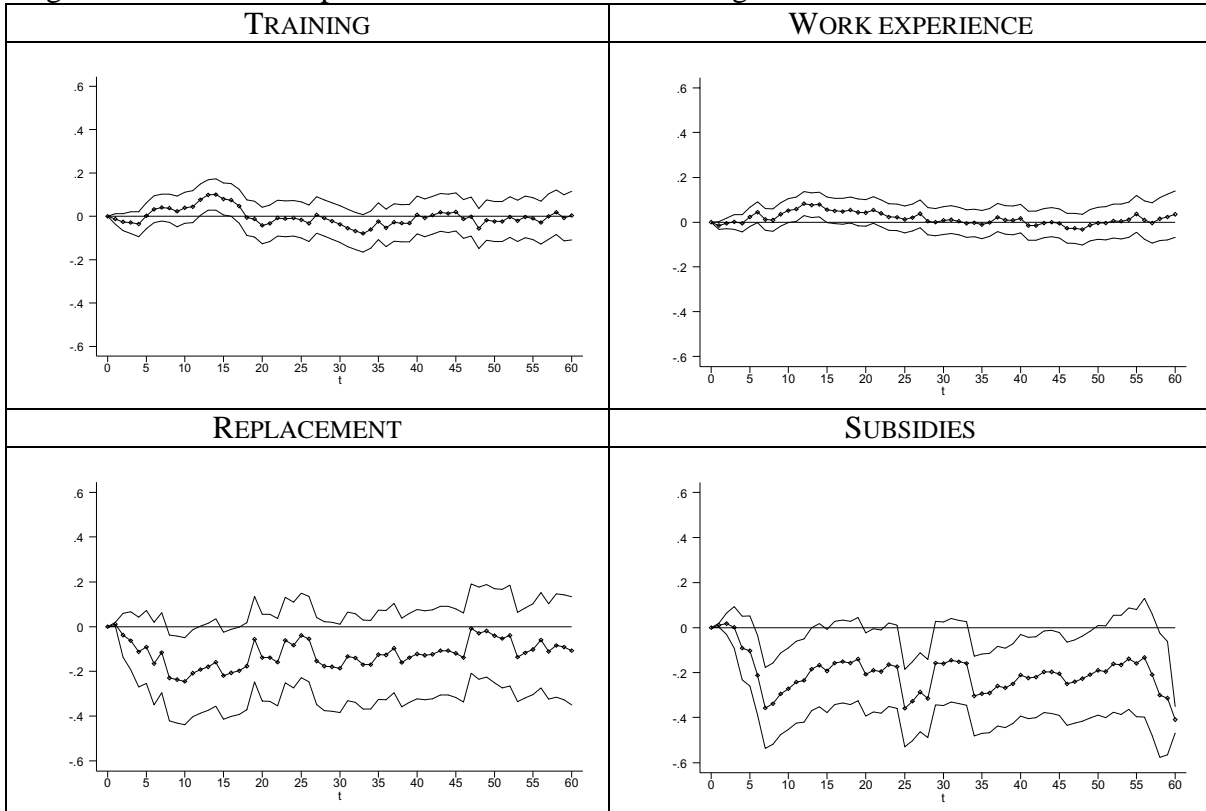
Figure A5: REPLACEMENT compared to … for individuals taking replacement

TRAINING	WORK EXPERIENCE

RELIEF	SUBSIDIES
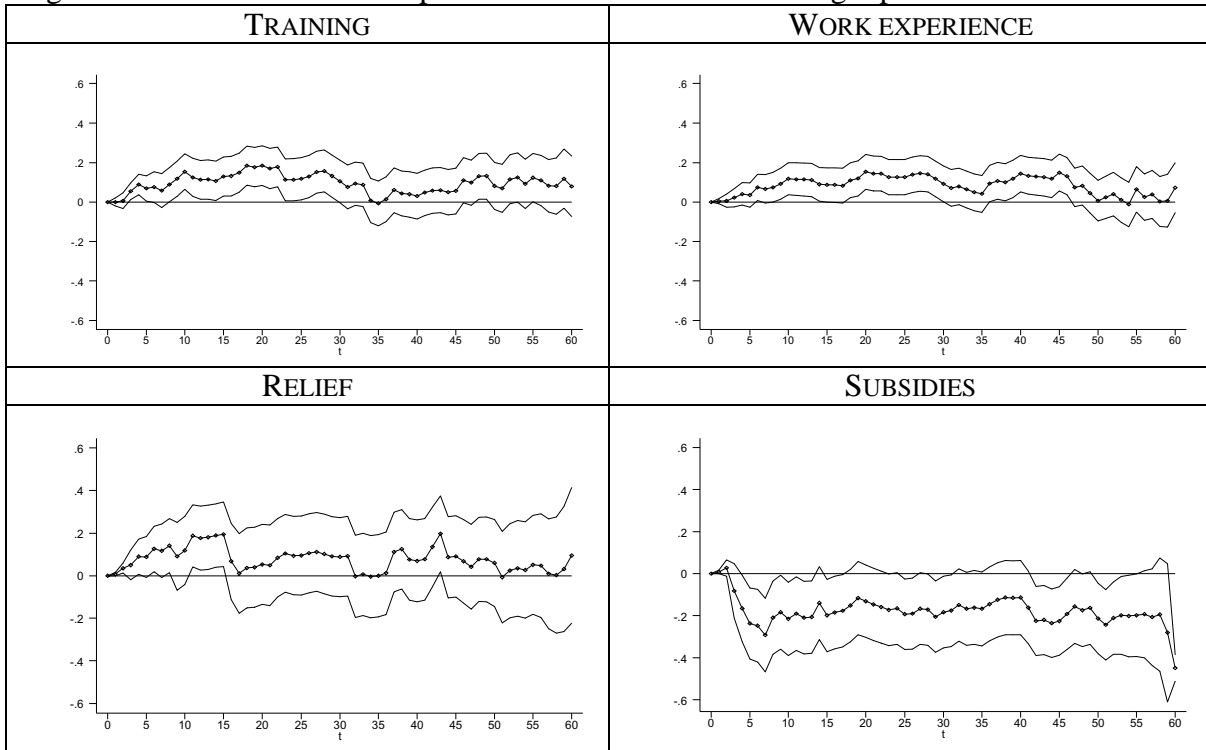
Figure A6: SUBSIDIES compared to … for individuals taking subsidies

TRAINING	WORK EXPERIENCE

RELIEF	REPLACEMENT

46
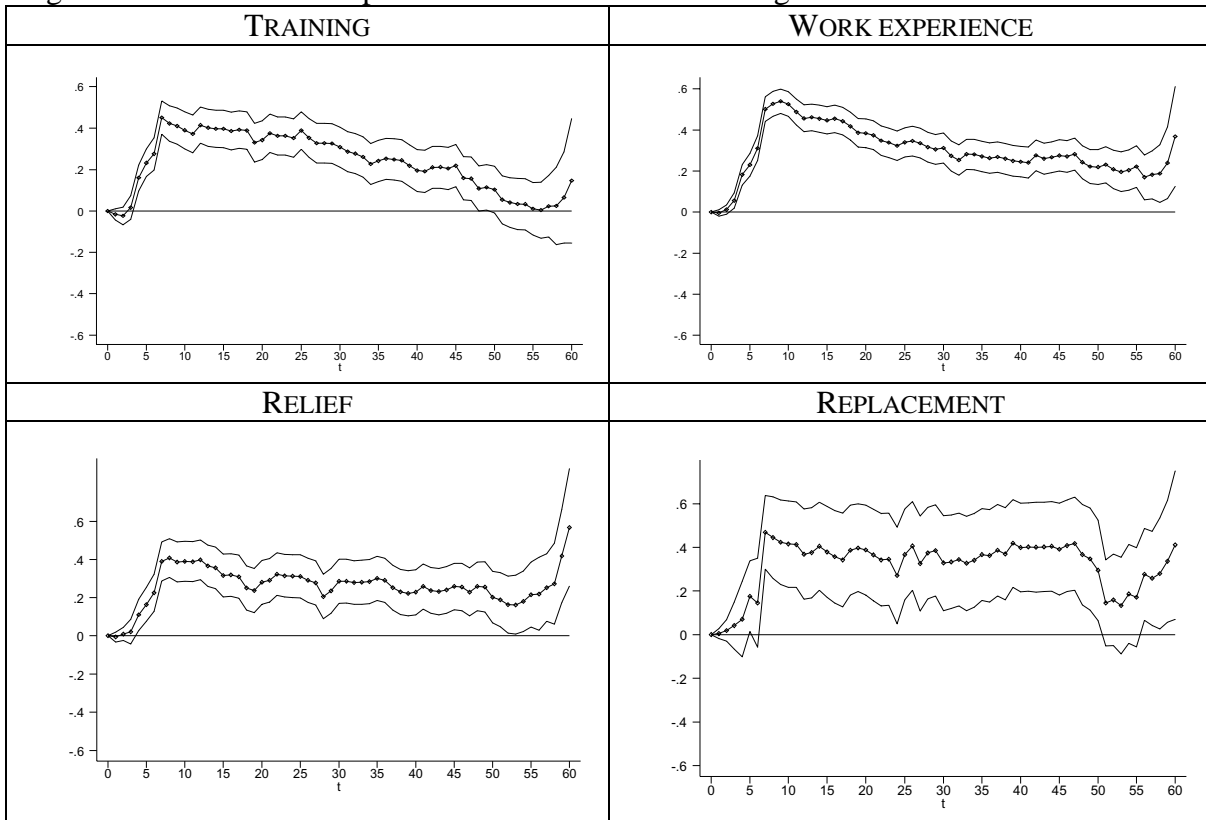
Figures A7-A11.
Differential average effects on **compensated unemployment probability** over time of the specified programme compared to the various alternatives for participants in the specified programme.

(percentage points; *t*-axis: months since joining the programme)

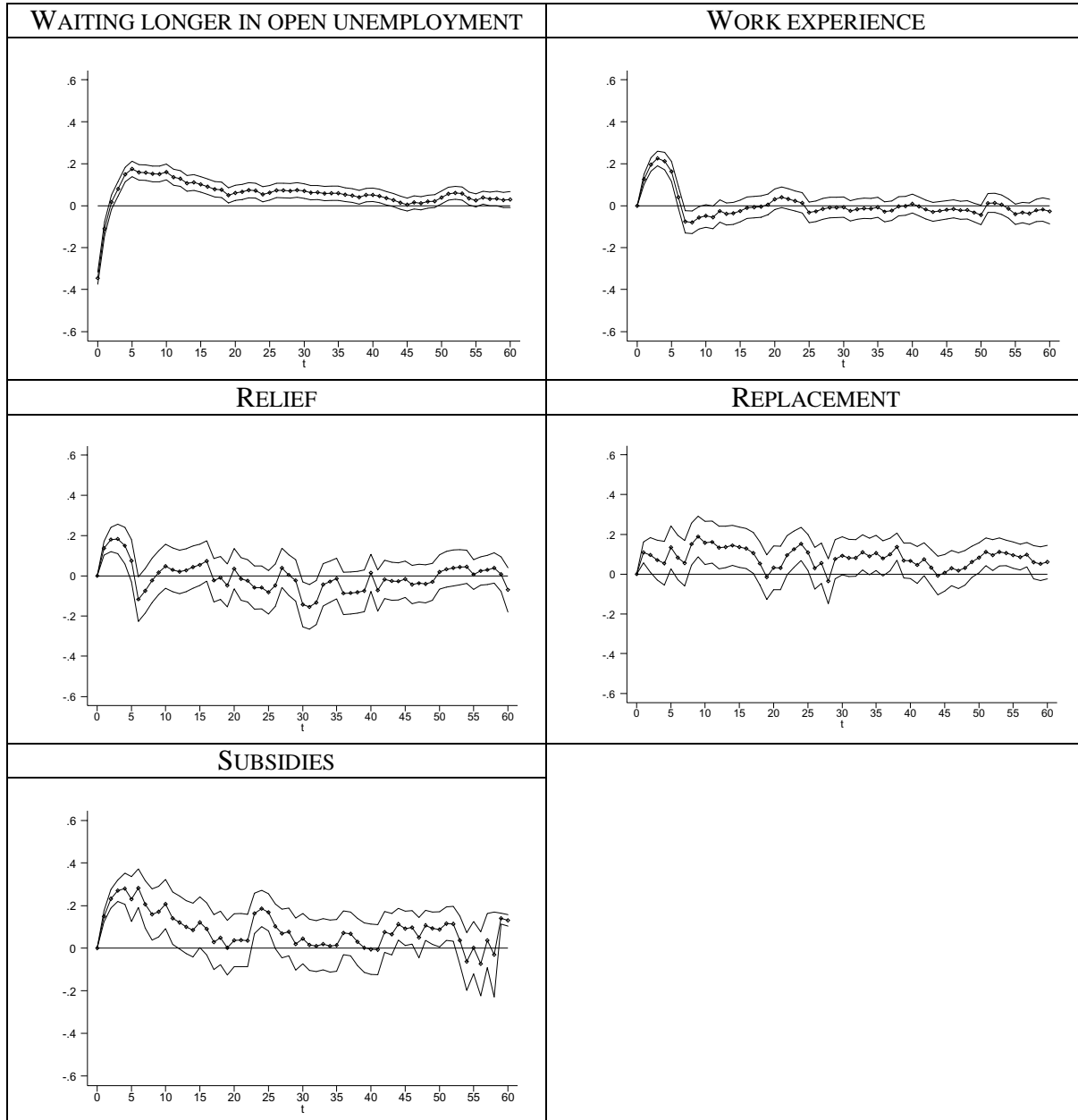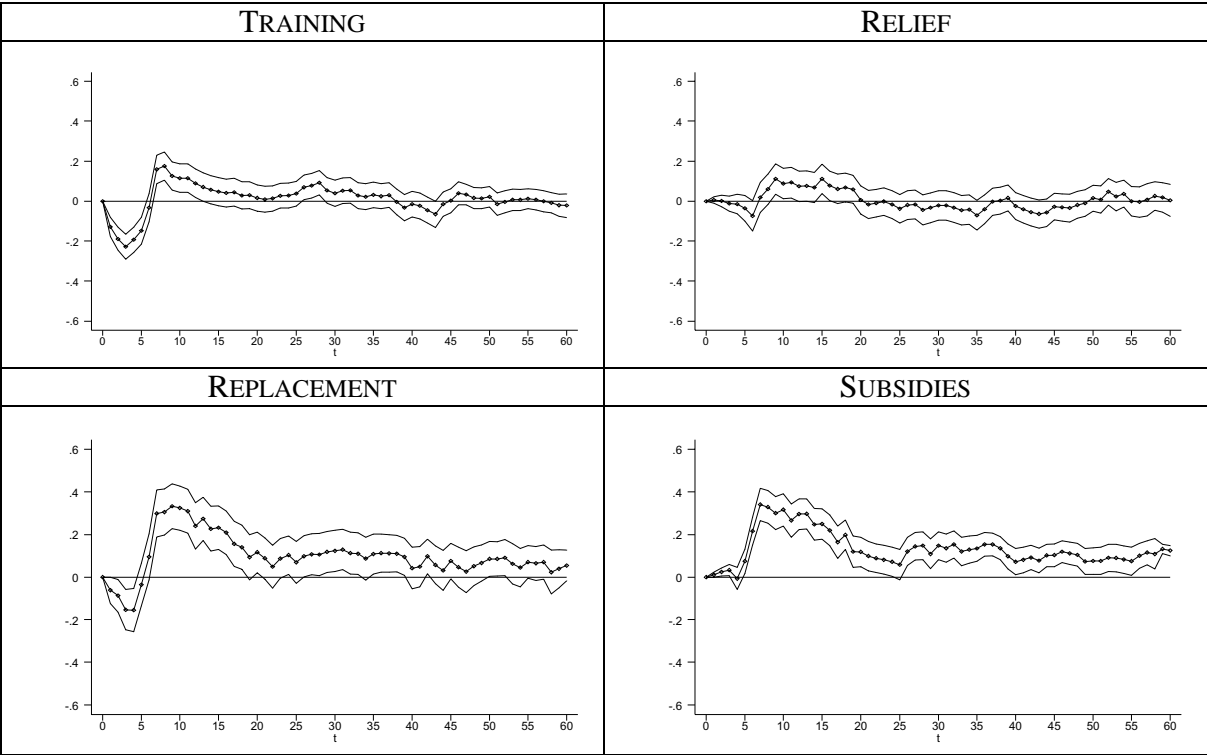Figure A7: TRAINING compared to … for individuals taking training

WAITING LONGER IN OPEN UNEMPLOYMENT	WORK EXPERIENCE

RELIEF	REPLACEMENT

SUBSIDIES	

Figure A8: WORK EXPERIENCE compared to … for individuals taking work experience

Note: See Figure 5.2 in the main text for the effect of the two work practice schemes compared to waiting longer.

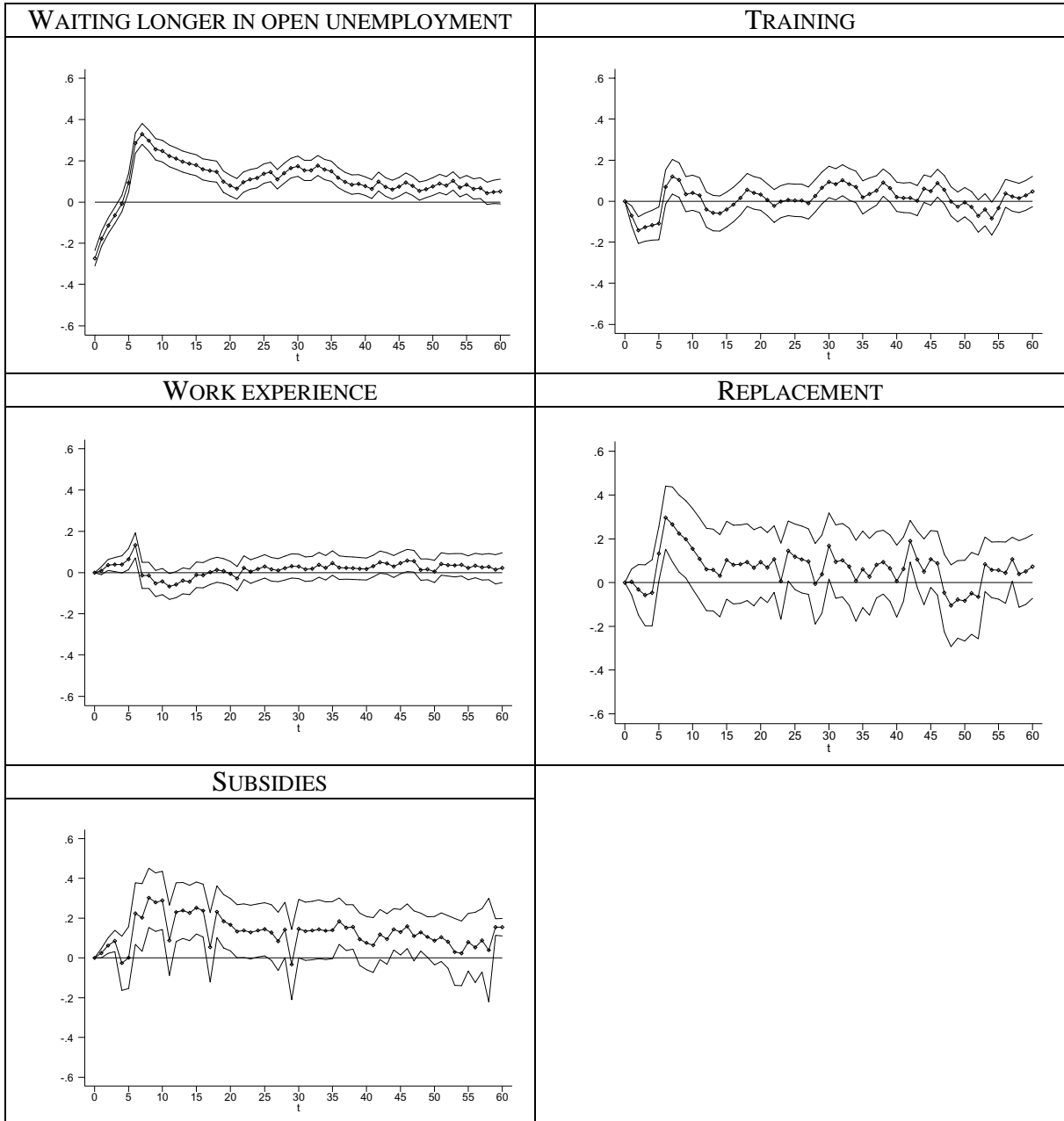Figure A9: RELIEF compared to … for individuals taking relief

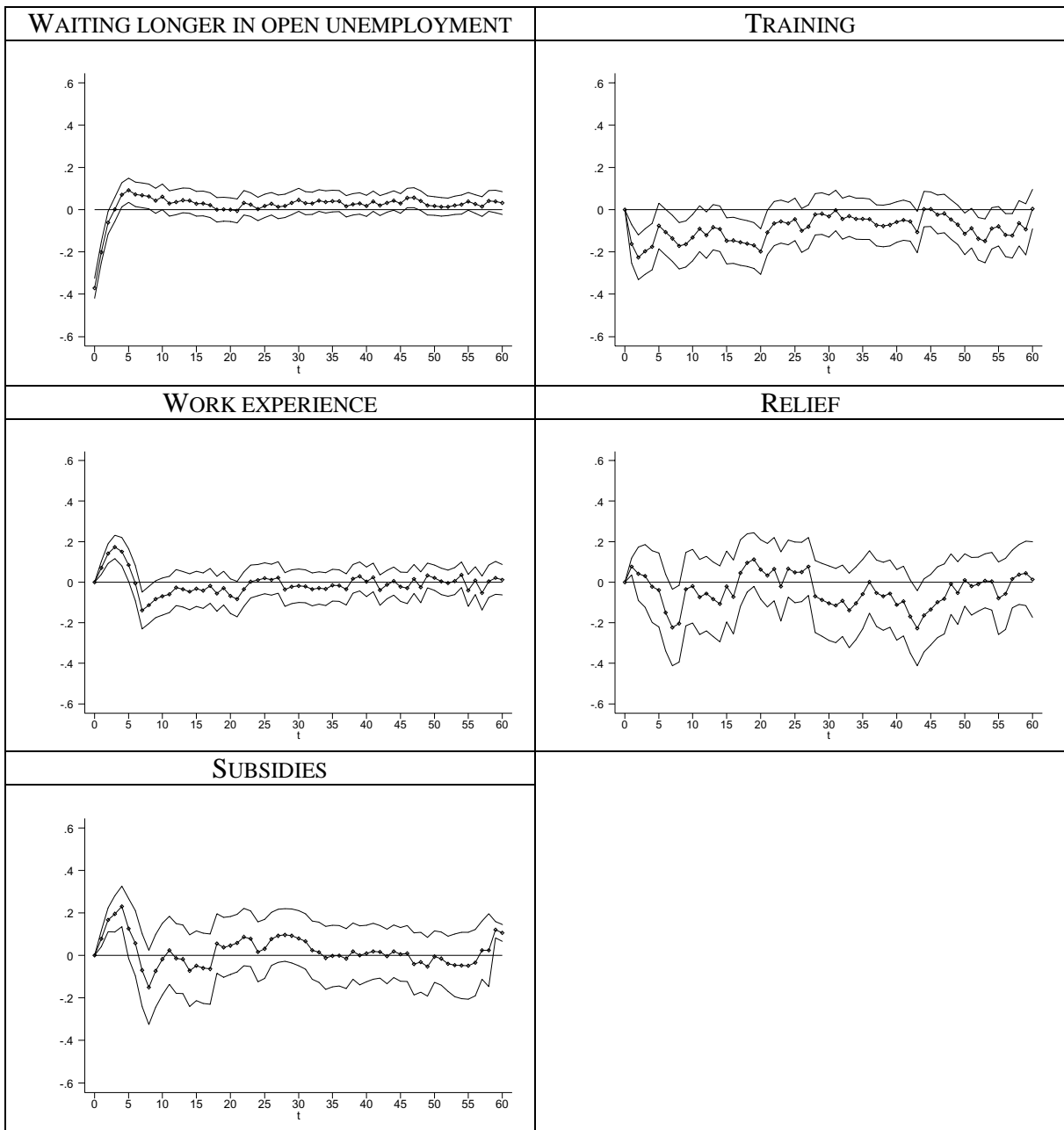Figure A10: REPLACEMENT compared to … for individuals taking replacement

WAITING LONGER IN OPEN UNEMPLOYMENT

TRAINING

WORK EXPERIENCE

RELIEF

SUBSIDIES

Figure A11: SUBSIDIES compared to … for individuals taking subsidies

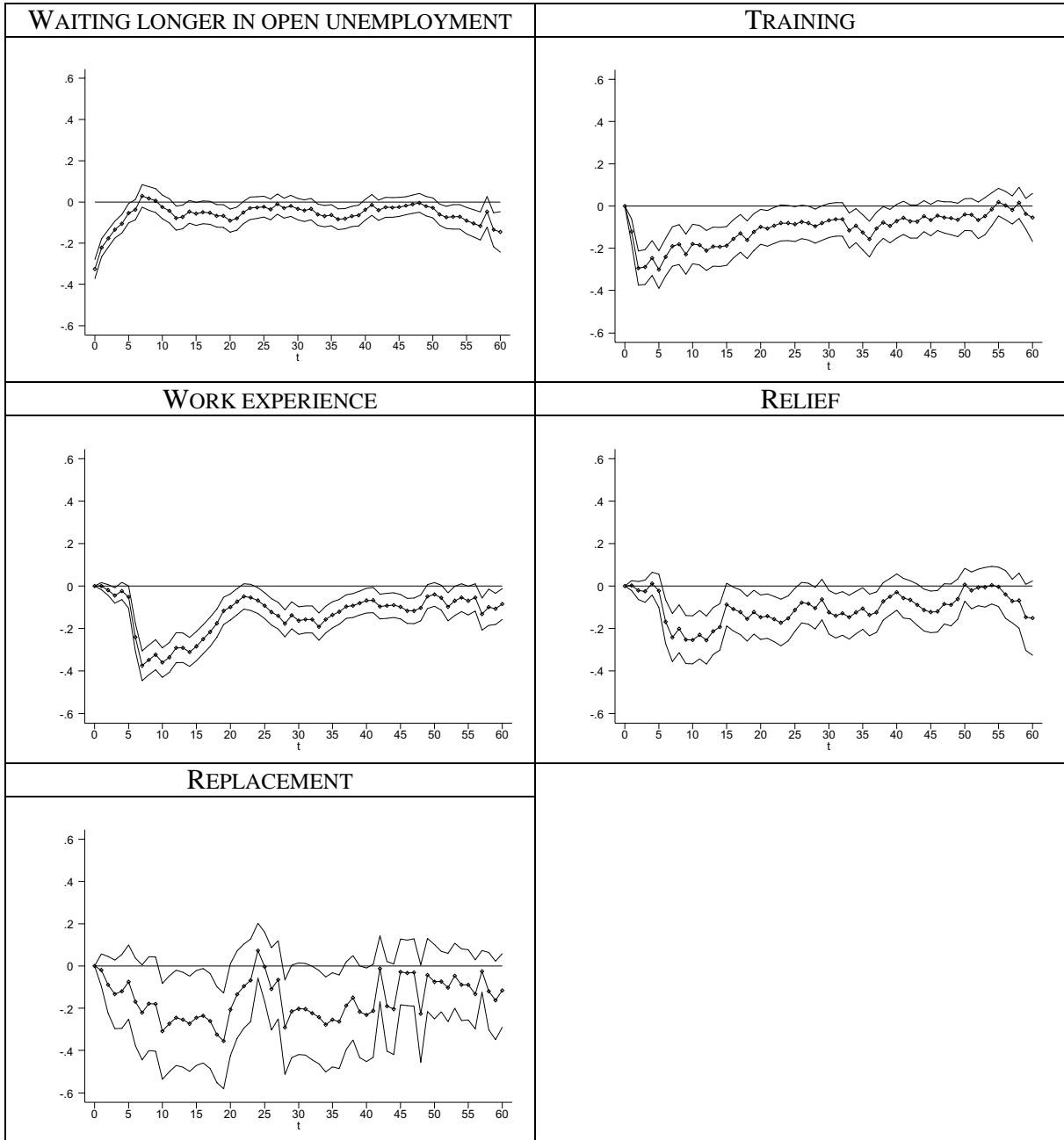| WAITING LONGER IN OPEN UNEMPLOYMENT | TRAINING |
| WORK EXPERIENCE | RELIEF |
| REPLACEMENT | |

Figure A12 Average effect on employment probability over time of joining the specified programme compared to waiting longer in open unemployment for participants in the specified programme: estimated effect and best- and worst- case bounds
(Time in months, from programme start)