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DIFFERENTIAL EFFECTS OF SWEDISH ACTIVE LABOUR MARKET PROGRAMMES FOR UNEMPLOYED ADULTS DURING THE 1990s

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Differential Effects of Active Labour Market Programs for the Unemployed

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Abstract: The differential performance of six Swedish active labour market programs for the unemployed is investigated in terms of short- and long-term employment probability and unemployment-benefit dependency. Both relative to one another and compared to more intense job search, the central finding is that the more similar to a regular job, the more effective a program is for its participants. Employment subsidies perform best by far, followed by trainee replacement and, by a long stretch, labour market training. Relief work and two types of work practice schemes appear by contrast to be mainly used to re-qualify for unemployment benefits.

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

JEL classification: C14, J38, J65, J68.

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1. Introduction

Labour market interventions have been an important element of public policy in the US, Europe and several other OECD countries for over two decades. In 1997, EU states even committed themselves to ensure that every young unemployed is offered training, retraining, work practice, a job or other employability measure before reaching 6 months of unemployment, the same applying to unemployed adults before they reach 12 months.¹ The OECD has also explicitly called for an expanded use of such measures, together with a ‘reinforcement of their effectiveness’.² In fact, confronted with high and persistent unemployment³, several OECD countries have been introducing or expanding their active labour market policy in the 1980s and especially 1990s.

But this surge in interest in active labour market programs (ALMPs) and concomitant rise in public spending begs the obvious question of whether these numerous measures do indeed work and whether they all work equally well. In fact, although programs such as job broking, re-training, job creation in the public sector and subsidised employment are each aimed at improving the labour market opportunities of the unemployed, their effectiveness may strongly differ.

Despite lower public outlays, the US have traditionally been at the forefront in the evaluation of ALMPs. The focus of this literature has typically been on whether individual programs work (see Friedland *et al.*, 1997, and Heckman *et al.*, 1999, ch.10, for extensive surveys). Since these programs generally offer a combination of active measures, the extent to which the uncovered effects arise from skills-building activities as opposed to the other lower-cost activities that are bundled with them remains largely unclear.⁴ Furthermore, even though different programs have been looked at, they have not been compared against one another, but rather to the ‘no-program’ state (or to a state of unspecified alternatives). An unequal performance of different programs compared to ‘no-program’ may however also arise from the different type of individuals participating in the various measures. Hence these comparisons do not allow one to properly assess whether the programs are well targeted – in the sense that those who participate in program A perform better than if *they* had gone on program B –, or whether there are programs that are uniformly better than others.

¹ EU Council Resolution No.13200/97.

² The OECD Job Strategy was launched in 1994. See OECD (1994), as well as (1996).

³ Although the US does not face as high an unemployment rate as the average OECD or European country, in some areas and for some subgroups, notably the low-skilled and ethnic minorities, the figures are rather comparable (e.g. in 2003, an average 8% rate for the OECD and 8.8% for OECD-Europe, though higher than the overall 6% in the US, are on the lower side of the over 10% rate for the low-skilled and blacks in the US).

⁴ See the discussion in Friedlander *et al.* (1997). Some progress has been made in determining the differential effects of a couple of service combinations in the JTPA and JOBS evaluations.

This paper exploits the potentially highly instructive institutional set-up in Sweden to directly assess the relative performance of different programs.⁵ Building on a long-standing experience of ALMPs (dating back to the 1930s), Sweden has been extensively relying on a rich menu of such measures. This case is thus of special interest, since the same target population is exposed to the same wide set of well-established programs whilst facing the same macro conditions and institutional framework.⁶ A clear outlier in terms of ALMPs expenditure⁷, Sweden has also traditionally enjoyed low unemployment rates by European standards, and was thus often regarded as a model (see e.g. Layard *et al.*, 1991). Indeed, the broad types of Swedish programs we consider closely resemble those available in other developed countries in terms of design and services offered (e.g. the Swiss and German ALMPs, or the multiple options offered by the New Deal in the UK).

The available longitudinal data record the labour market history (to the day) of all unemployed registered at public employment offices since 1991. These representative and very detailed administrative data allow one to capture both short- and long-term program effects, as well as how the effects of the various programs may differentially evolve over time. In the literature effects are by contrast often evaluated at a given – and arbitrary – point in time, such as on the last observation day, or after a year. This misses out on the dynamics: the effect of a given program may differ in the short- from the long term, and different programs may take different lengths of time to produce an impact on labour market outcomes. Long enough follow-ups are needed to assess how sustained the returns to participation in different programs are.

Finally, broad lessons can be learnt from the Swedish case as to the interdependence between passive and active measures, the subject of recent policy debate.⁸ Passive unemployment benefits support individuals whilst unemployed, but also affect work incentives. If – as in Sweden until 2001

⁵ Microeconomic studies looking at the relative effects of different programs include Frölich *et al.* (2004), Larsson (2003) and Carling and Richardson (2004) for Sweden, and e.g. Gerfin and Lechner (2002) and Gerfin *et al.* (2004) for Switzerland; Brodaty *et al.* (2001) and Bonnal *et al.* (1997) for France, and the earlier work by Ridder (1986) for the Netherlands.

⁶ The evaluation of universal and long-established programs does not incur in a number of potential biases. First, scale bias would arise if the scaling up of pilots to universal participation changes community norms or combines with patterns of social interaction or information diffusion which might affect program impacts (Garfinkel *et al.*, 1992). Entry effects relate to the possibility that once a tested program is made permanent and extended to the entire eligible population, the entry or exit of individuals from the criteria that define eligibility might be affected (Moffitt, 1992). Finally, as offices learn about the administration and delivery of a new program, the actual ‘treatment’ might differ from the one that was evaluated.

⁷ While the average OECD country spent between 0.7 and 0.9% of its GDP on ALMPs during the 1980s and 1990s, the figure for Sweden remained well above 1%, peaking to over 3% in the severe recession of the 1990s. The programs also involve a very large proportion of the Swedish labour force; in 1997 for instance, the equivalent of 4.5% of the labour force participated on average in such measures (excluding those for the disabled).

⁸ Cf. guideline II of the 1998 Employment Guidelines (EC, 1998) and the ensuing national employment plans.

– participation in an ALMP renews entitlement to unemployment compensation, not only are incentives to search and accept job offers likely to be reduced, but this benefit-renewing feature of the programs may seriously undermine their own effectiveness.

This paper evaluates the six programs that were available to entitled unemployed adults in 1994: labour market training, two work experience programs (workplace introduction and work experience placement), public relief work, trainee replacement and job subsidies.⁹ Their differential performance is investigated both relative to one another and vis-à-vis more intense job search in open unemployment. For participants in a given program we thus estimate the effect of joining that program compared to joining an alternative program, as well as compared to waiting longer in open unemployment.¹⁰ Effects are evaluated month by month for 5 years since program entry.

Helping the unemployed back into work is a primary aim of ALMPs, and the stated one in Sweden. We thus focus on employment rates over time, summarising program effects on both job finding probability and job attachment while employed. This allows us to investigate if and how the various programs differentially endow participants with skills and working habits that enhance their employment prospects in the short- and long-run. Special attention is also devoted to differential program impacts on individual benefit collection probability over time, allowing us to directly capture the influence that benefit renewability may have on the effects of the various programs.

The next section outlines the Swedish labour market policy and the six programs being evaluated. Section 3 describes data and sample choice. Section 4 clarifies the evaluation questions to be addressed and sketches the evaluation approach in the multiple-treatment framework. Our data include a wide array of demographic, human capital and labour market variables, as well as the case-worker's time-varying subjective appraisal of various factors relating to the overall situation, character and needs of service of the job-seeker. Such informational richness has motivated the matching approach taken in the paper. Section 4 devotes considerable attention to the discussion of the plausibility of the underlying identifying assumption. The set of empirical findings is presented in Section 5, before concluding in Section 6 with a summary and overall appraisal of the results.

⁹ Sianesi (2001) offers a purely non-technical overview of issues for the evaluation of the Swedish active labour market policy as well as an appraisal of related empirical results, including some selected results from this paper drawn together with other findings from Sianesi (2004) and from additional analyses.

¹⁰ The latter choice of comparison state is driven by an institutional set-up where individuals *will* sooner or later join a program provided they are still unemployed (see Section 4.1). In such a context, Sianesi (2004) has proposed looking at the impact of joining a program at a given time in unemployment compared to not joining any program at least up to then. While Sianesi (2004) has combined all the programs into one treatment in order to look at the performance of the Swedish system in its entirety, the present paper adapts this strategy to a multiple treatment framework. Disaggregating

Our findings highlight some interesting conclusions. The magnitude and even the sign of the various treatment effects were found to change over time since program entry, which underscores the importance of being able to look at outcomes in both the short- and longer term. Overall, program effects were found to be widely heterogeneous across program type, with different types of measures often displaying diametrically different impacts on individual outcomes. In particular, those programs providing subsidised job experience and on-the-job training (i.e. job subsidies and trainee replacement) were found not only to be cheaper, but considerably more effective than vocational classroom training in terms of participants' labour market performance. Further, the more closely the task performed on a program resembles regular work, the higher are the benefits of the program to its participants. In fact, those programs on which participants did not perform particularly relevant tasks (i.e. relief work, workplace introduction and work experience placement) appeared to be simply used as vehicles to renew benefit eligibility, inducing job-seekers to drift into long-term unemployment and benefit dependency.

2. The Swedish labour market policy

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

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

This evaluation focuses on the six main programs open to adult unemployed workers in the mid '90s: labour market training, workplace introduction (API), work experience placement (ALU), relief work, trainee replacement and employment subsidies.¹¹ With the possible exception of trainee replacement¹², these programs typify those commonly available in other OECD countries. Table 2.1 contrasts their main features, and places them within the OECD classification of ALMPs.

To gain access to any program, one needs to be registered at a local official employment office.

the program treatment into its main components allows us not only to evaluate one program against another, but also to assess how heterogeneous the impacts of the different programs are compared to open unemployment.

¹¹ Two programs are excluded from the analysis on the basis that they are targeted to (or attract) quite specific sub-groups of the unemployed: self-employment grants (for those wishing to establish their own new business, with both a business idea and a financial plan approved by the offices) and vocational rehabilitation (for those with occupational disabilities needing specialised resources for in-depth counselling and job-preparation measures).

¹² Job rotation programs of this type are available e.g. in Austria, Finland, Norway and Portugal.

Table 2.1 Synoptic table of the main features of the programs

Program	Aim	Eligible	Employer	Training	Task	Compensation ^a	Cost ^b	OECD ^c Classification (% spending)
EMPLOYMENT SERVICES	fill job openings quickly; job search assistance and training			job seeker activities		UI/KAS if entitled		Public employment services & administration (17%)
LABOUR MARKET TRAINING (AMU)	equip individuals with skills to find jobs more easily	>20	private & public providers	vocational classroom training		TA/BA; course free	1,992	Labour market training (23%)
WORK PRACTICE								
Work experience placement (ALU)	prevent exhaustion of benefits while maintaining contact with working life and enhancing working habits	entitled ≥20	90% public & non-profit		otherwise not performed	TA/BA	1,328	
Workplace introduction (API)	maintain contact with working life; obtain workplace training, job-experience and references	≥20	private & public	practical vocational training	otherwise not performed	TA/BA	999	Subsidised employment: Direct job creation in public & nonprofit sectors (15%)
TEMPORARY JOB								
Relief work	pecially created temporary jobs to maintain working skills and habits, also to avoid benefit exhaustion	>25	2/3 in public sector (municipalities & state organizations)		otherwise not performed	according to collective agreement	1,315	
Trainee replacement	enhance skills of employee while providing an unemployed individual with work experience	≥20	80% public sector	on-the-job practice	regular	according to collective agreement	1,095	–
EMPLOYMENT SUBSIDIES	increase chance of job offers; establish permanent employment relation	≥6m unemplm. ≥20	private sector only; from '97 some industries excluded	on-the-job practice	regular	according to collective agreement	853	Subsidies to private-sector empl. (& start-up grants) (15%)

Notes: Information has been gleaned from various sources, in particular, Swedish Institute (1997). ^aTA is training allowance equivalent to the UI or KAS the individual would have been entitled to; BA is the basic amount (SEK103 per day; in 2004 USD less than 15) if the individual is not entitled. ^bTotal monthly cost per participant in 1998 expressed in 2004 USD (AMS, 1998, reported in Carling and Richardson, 2004, Table 1). ^cCategory within the OECD classification of ALMPs and respective % out of total spending on ALMPs by the average OECD country in 2000 (OECD database on Labour Market Programs). The remaining two categories are measures for youth (13%) and for the disabled (17%).

The six programs under consideration are additionally open to adults only (over 20 or 25), while ALU requires the individual to be entitled to unemployment compensation, and job subsidies are targeted at the long-term unemployed. The latter may often be regarded as a mere guideline, though, since 20% of the job subsidy participants in our data have spent less than the required 6 months in open unemployment prior to joining. Our sample satisfies the eligibility rules in terms of registration, age and entitlement, while we shall control very carefully for unemployment duration.

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

Labour market training, by far the most expensive measure, is intended to augment participants’ human capital via full-time, formal teaching of new vocational¹³ skills.

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

A third kind of measure provides unemployed workers with a temporary job. *Relief work* involves specially created temporary jobs, mostly in the public sector. The oldest measure for creating employment, it has diminished in importance during the 1990s, becoming primarily used for individuals at risk of losing their unemployment benefits (Swedish Institute, 1997). In particular, individuals 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* offer a temporarily subsidised job opportunity to acquire job-

¹³ To reduce the heterogeneity in courses offered, we only consider vocational training. Non-vocational courses are by contrast aimed at helping workers with basic educational insufficiencies to move on to further education or to other programs, rather than directly into a job.

specific human capital.

While all the programs aim at improving participants' employment prospects, they differ in the kind of skills they provide and in the way they provide them. At the one end of the spectrum, labour market training provides vocational classroom training of new skills deemed in demand. API has a strong emphasis on practical vocational training; similarly, ALU and relief work may provide participants with job experience and improve their working habits. Participants in these three programs are however prevented – at least formally – from performing tasks that a regularly employed individual would otherwise do. Although it is likely for such a rule to be often interpreted more as a recommendation than as a strict guideline¹⁴, 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 implicitly seek to influence an employer's hiring decisions towards retaining the worker after completion of the program.

A final consideration relates to the first row in Table 2.1. Individuals searching for a job as openly unemployed can benefit not just from standard job information and matching of vacancies to applicants, but also from the 'job-seeker activities', which include search-skill-enhancing activities (e.g. training courses on how to apply for a job) and motivation-raising activities. In Sweden, the 'no-treatment' status to which program participation has to be compared to is thus not a complete absence of intervention, but these baseline services offered by the employment offices. In some countries this kind of assistance is in fact considered a program in its own right.¹⁵

As to unemployment compensation, benefits are provided in two forms¹⁶, the most important one being unemployment insurance (UI). UI benefits are quite generous by international standards (daily compensation being 80% of the previous wage¹⁷) and are available for a total of 60 weeks, more than twice the maximum duration in the US. To be eligible to UI an unemployed person registered at an employment office and actively searching for a job must have been working for at least 5

¹⁴ Circumstantial evidence in Hallström (1994; from Ackum Agell, 1995) shows that sponsors, participants and case-workers believe that these projects do often replace jobs that are part of the organisers' normal activity.

¹⁵ An example is the Gateway period of the UK New Deal program for the unemployed.

¹⁶ Those not entitled to any unemployment benefits may receive means-tested social insurance.

¹⁷ This maximum level has changed a few times during the 90s. The system also has a ceiling.

months during the 12 months preceding the current unemployment spell.¹⁸ Once receiving UI, an offer of ‘suitable’ work – or of a labour market program – must be accepted; refusal to accept a job/program might lead to expulsion from compensation (the ‘work test’).

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

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

3. Data and sample selection

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

As to sample choice, individuals need to be homogeneous in those basic characteristics which determine eligibility to all the programs under examination. Only then will it be relevant to examine their outcomes had they chosen a competing program. We focus on adults (aged over 25) who are entitled to unemployment benefits. In addition to their exclusive access to one of the programs

¹⁸ There is also a membership condition, requiring payment of the (almost negligible) membership fees to the UI fund for at least 12 months prior to the claim.

(ALU), entitled jobseekers are of special policy interest, since they are the group whose participation incentives are most likely to be affected and for whom the trade-off between productivity-enhancing effects of the programs and the reinforced work disincentive associated with the benefit system should be at its sharpest. An added advantage compared to non-entitled individuals is in terms of data quality and availability: since registration at an employment office is a pre-requisite for drawing benefits, our chosen sub-sample is a particularly representative one of the sub-population of interest. The information for benefit recipients is thus especially reliable, but also much richer, since it includes the Akstat data.

We look at the inflow into unemployment in 1994, the year when the unprecedented recession that had hit the Swedish economy in the early 90s was at its most severe.¹⁹ Mainly dictated by the need to exploit the information in Akstat, the choice of this year yields a sample of above-average quality unemployed compared to the inflow in an average year. On the other hand, it is particularly interesting to assess how successful the massive use of ALMPs during the recession was in bringing their pool of participants back into work. It is exactly in periods of high unemployment when effective programs would be most needed.

We further restrict our sample to adult individuals who became unemployed for their first time²⁰ in that year and were entitled to either UI or KAS. Additionally, individuals whose first program was start-up grants, vocational rehabilitation or non-vocational training are dropped from the analysis (see footnotes 11 and 13). These criteria lead to a sample of 30,800 individuals, followed from the moment they register in 1994 to the end of November 1999.

Descriptive statistics of the various treatment groups are presented in Appendix B. Visual inspection of average characteristics clearly shows that participants in the different programs are in fact quite distinctive groups. There seem to be several demographic, human capital and labour market variables which systematically differ between the groups and which are most likely to affect subsequent labour market performance.

¹⁹ From less than 3% in 1989 and 1990, unemployment jumped to 9% in 1992, reached its peak of 13.5% in 1994 and then stayed around 13% until 1997.

²⁰ Since Händel starts in August 1991, strictly speaking we can only ensure that individuals registering in 1994 have not been unemployed at any time during the previous three years. Given however that it was exactly between these three years that Sweden experienced unprecedentedly high unemployment, the requirement is likely to be quite binding, making us reasonably confident that most of our individuals are indeed first-time unemployed.

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

4.1 Evaluation questions and evaluation approach

The Swedish institutional set-up poses a few interesting methodological issues which have to be resolved before deciding on the evaluation strategy. In particular one needs to be very explicit about the definition of the relevant treatments. While Section 2 has already described the six programs of interest, these take place continuously over time and are open to all registered job-seekers; unemployed individuals are in fact often observed to participate in different programs at different times during their unemployment history. As our sample is one of first-time unemployed, we focus on the *first* program individuals may receive within their first unemployment experience.²¹ Furthermore, since the observed program duration is endogenous²², we start measuring the causal effect at entry into the program and view any lock-in effect whilst on the program as a constituent part of the effect. The treatments are thus in terms of *joining* a given program right after having become unemployed (thus not in terms of *completing* a given program, nor of taking *only* a given program).

When evaluating the effect of a program on some outcome, an essential part of the research question concerns the comparison state. In addition to the effect of joining a given program compared to joining a different one, interest often lies in assessing the effect of joining a given program relative to no program participation at all. In the standard US program evaluation specification, the program is administered at a fixed point in time, and individuals are either treated or non treated. In Sweden, by contrast, not only are the programs ongoing, but any job-seeker can potentially join one and will eventually join one at some time, provided he remains unemployed long enough. Indeed one could argue that the reason an unemployed individual has not been observed to go on a program is *because* he has found a job before enrolling. In the Swedish institutional set-up the definition of non-participants cannot thus be the standard one, namely those individuals who are observed not to enter any program. Since such individuals would *de facto* be observed to leave the unemployment register, this approach would amount to selecting a comparison group based on future (and successful) outcomes.²³ Although a non-standard one, this evaluation problem is quite commonly encountered, in particular in the evaluation of ongoing programs which individuals sooner or later will join

²¹ The sequence of programs an individual goes through is not exogenously determined at the start of the unemployment spell. In our framework, any subsequent program participation is viewed as an outcome of that first treatment. Although subsequent take-up of other programs can thus be assessed in its own right (as done by Sianesi, 2004), to conserve space here we look at employment and compensated unemployment as direct measures of labour market performance.

²² Some programs require participants to continue job searching. The offices too continue to search; participants are in fact required to drop out of a program if a 'suitable' job is found for them.

provided they are still eligible (e.g. still unemployed).

The program participation process in Sweden is such that once an individual has become unemployed, he and his case-worker are most likely to take their decisions sequentially over time in unemployment. In particular, at any given moment the relevant decision is not whether to participate in one of the programs or not to participate *at all*, but whether to join a program now or not to participate *for now*, searching longer in open unemployment and knowing that one will always be able to join later on. An evaluation question relevant for this set-up, proposed by Sianesi (2004), lets the parameter of interest mirror this key choice open to the eligibles and evaluates the average effect, for those observed to join a given program after a given number of months spent in open unemployment, of joining that program when they did compared to further postponing the participation decision by not joining any program at least up to then. Specifically, the comparison group comprises those individuals who are unemployed up until that time – and would thus be eligible to join a program – but who do not join any at least as yet.

We thus investigate the differential effectiveness of seven different types of treatments: joining labour market training, API, ALU, relief work, trainee replacement or job subsidies, as well as searching longer in open unemployment.

In the following we briefly sketch the framework of Imbens (2000) and Lechner (2001), leaving it to Appendix A to adapt it more formally to the institutional set-up just described.

Let $D \in \{0, 1, \dots, 6\}$ denote actual assignment to a specific treatment – one of the 6 programs or the ‘waiting’ option – and Y^k the potential outcome an individual would experience were he to receive treatment k . We are interested in assessing the average effect for participants in program k of participating in that program compared to receiving a different treatment k' :

$$E(Y^k - Y^{k'} | D=k) = E(Y^k | D=k) - E(Y^{k'} | D=k) \quad \text{for } k, k' \in \{0, 1, \dots, 6\}, k \neq k' \quad 24 \quad (1)$$

While the first term of (1) is observed in the data, identifying assumptions²⁵ need to be invoked to estimate all the counterfactuals of the type $E(Y^{k'} | D=k)$, that is all the outcomes participants in k would have experienced, on average, had they taken any treatment other than k . One such assumption postulates that conditional on the value of observable characteristics X , the (counterfactual) dis-

²³ Very recent work by Fredriksson and Johansson (2003) formalises this intuition. The discussion of an absent ‘no-treatment’ group was initiated by Carling and Larsson (2000a, b).

²⁴ Note that in general this parameter is not symmetric: $E(Y^k - Y^{k'} | D=k) \neq -E(Y^{k'} - Y^k | D=k')$ if participants in the two programs systematically differ in characteristics related to the outcome.

²⁵ Identification assumptions and estimation of treatment effects in non-experimental studies have been extensively looked at; for a comprehensive survey see Heckman *et al.* (1999).

tribution of $Y^{k'}$ for individuals joining program k is the same as the (observed) distribution of $Y^{k'}$ for individuals receiving treatment k' :²⁶

$$D \perp (Y^k, Y^{k'}) \mid X=x, D \in \{k, k'\} \quad \text{for } k, k' \in \{0, 1, \dots, K\}, k > k' \quad (2)$$

Methods like matching sample from (or reweigh) group k' to produce a matched group in which the distribution of pre-treatment observables X is as similar as possible to the distribution in group k . In order to adjust non-parametrically for observed differences in X between the two groups, however, all participants in k need to have a counterpart in group k' (common support requirement).²⁷

Without imposing functional form assumptions or restricting impact heterogeneity across individuals, matching methods can eliminate the bias due to the difference in the supports of X in the two groups being compared and the bias due to the difference between the two groups in the distribution of X over its common support. Matching is however based on identifying assumption (2), which assumes away the third potential source of bias identified by Heckman *et al.* (1998), selection on unobservables.²⁸ Before turning to implementation and interpretational issues in section 4.3, the next section thus discusses the plausibility of (2) in this application. To summarise some of the main arguments, selection into the programs over time in unemployment is required to be unrelated to labour market outcomes *given* a set of observables. In this analysis the relevant observables can be chosen according to the findings of a Swedish survey directly asking job-seekers and caseworkers about their decision criteria. The resulting set of controls is in turn particularly rich: employment office and local conditions, detailed job-seeker's characteristics and the caseworkers' own subjective and time-evolving judgment of the client's character, overall prospects and needs. Furthermore, while the survey study identified the caseworker as the relevant decision-maker as to program choice, from the received econometric evidence officers do not appear to be allocating their unemployed clients to the different programs so as to maximize their subsequent employment prospects. The next section thus makes a case for assumption (2) to represent a credible approximation and hence for matching to be considered a feasible strategy in this informational and institutional setup.

²⁶ Its weaker form in terms of conditional mean independence would suffice.

²⁷ If there are regions where the support of X does not overlap for the two groups, matching has to be performed over the common support region and the estimated treatment effect redefined as the mean effect for those treated k falling within the common support. Alternatively, identification would rely on (parametrically) extrapolating from regions with positive probabilities for both the treatment states being compared to occur.

²⁸ Note that a difference-in-differences approach is precluded here; outcomes are employment and benefit collection, and in the pre-program period all individuals are, by construction, unemployed collecting benefits.

4.2 Plausibility of the matching assumption

Assumption (2) requires us to observe (and thus match on) all those differences between the various treatment groups that are likely to affect their outcomes. We thus need detailed knowledge of the factors that drive participation, as well as access to data suitable to capture those determinants of participations that are also relevant to outcomes. In our application, not only do we have access to detailed background information (including several direct indicators of individual heterogeneity), but the choice of the relevant conditioning variables can benefit from the results of a Swedish survey directly asking job-seekers and placement officers about their participation decision criteria (Harkman, 2000, as reported in Carling and Richardson, 2004). What we need to consider is whether in deciding about participation these agents have access to and act upon information correlated with labour market performance but that we cannot capture in our data. It is useful to separately consider (A) the decision between waiting further in open unemployment or joining a (i.e. any) program and (B) the decision to choose one specific program.

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

The only unemployment experience of individuals in our sample relates to the present unemployment spell, a fact which is greatly informative of their labour market history. Entitlement status further reflects a certain degree of labour market attachment due to the work requirement UI-recipients have to fulfil.²⁹ Previous working hours (a proxy of the extent of past labour market involvement) and the pre-unemployment wage (conditional on qualifications, a summary statistic of individual productivity) are additional important individual attributes which characterise the worker's overall earlier labour market situation; interestingly, such information is not necessarily known by the caseworker. As to present employment prospects, we control for elapsed unemployment duration, benefit receipt status, demographics, several dimensions of human capital and a

number of direct indicators of individual heterogeneity.

Specifically, individual perceptions about one's employment likelihood are likely to change over time spent in unemployment. Elapsed unemployment duration should thus capture important unobservables in this dimension; more generally, in the presence of duration dependence and/or unobserved heterogeneity, it is crucial to ensure that the comparison individuals have spent in unemployment at least the time it took the treated to join. Elapsed unemployment duration is also an important X variable for directly explaining the joining decision, given some (albeit loose) regulations (e.g. for job subsidies), as well as incentives related to unemployment benefits running out. As to the latter, for a given elapsed unemployment duration we additionally control for benefits expiring at that time or for benefits having run out by then.³⁰ A binding ceiling in terms of compensation amount finally controls for job-seekers facing a compensation rate effectively below 80%.

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

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

Finally and most crucially, we exploit several direct indicators of individual heterogeneity likely to be highly relevant in terms of employment prospects. Specifically, we have retrieved information as to an overall evaluation by the officer of the situation, character and needs of service of the job-seeker. This assessment relates to the job-seeker's degree of job readiness (if judged to be able to take a job immediately, to be in need of guidance or to be difficult to place), as well as to the job-seeker's preferences, inclinations and urgency to find a job (if willing to move to another locality, if looking for a part-time job, if already having a part-time job). We also exploit a summary statistic

²⁹ An indicator of KAS entitlement controls for the alternative way to fulfil the 'work' requirement.

³⁰ Elapsed unemployment duration is only an imperfect predictor of benefit receipt, since job-seekers may decide to save their benefits for later, be suspended etc.

directly capturing selection into the programs (if the job-seeker has been offered a program and is waiting for it to start). Note in particular that the caseworker may update and revise this subjective judgement during his client's unemployment spell. This time variation in the assessment of the prospects and needs of the job-seeker is an additional key feature we can exploit to control for the program joining decision over time in unemployment.

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

Our evaluation question concerns the effect of joining a given program at a given time compared to later or never, thus requiring (2) to also hold in terms of future program participation, viewed as an outcome. Controlling for elapsed time spent in unemployment is once again crucial, in that approaching benefit exhaustion would make an individual more likely to join a program or, if having to wait longer, more likely to enter a program later on or to intensify job search (or lower one's reservation wage). As to anticipatory effects, (2) would be violated if an individual decided not to par-

ticipate at time u because he knows that he will join at some later date. This is not very likely.³¹ Additionally, as mentioned above, a very interesting piece of information in the data is an open unemployment sub-spell where the job-seeker is waiting to enter a labour market program. Having gone through the assignment process and having been offered a place makes it more likely for the individual to join a program rather than waiting; had he not joined now, he would be more likely to join later on or to decrease his job search in anticipation of joining. Like the caseworkers' subjective judgements, this offer (or waiting-for-a-program) status changes over time in unemployment.

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

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

We do however observe not only important characteristics of the job-seeker (e.g. educational qualifications for possible cream-skimming for training programs), but also the caseworker's own subjective, synthetic and evolving evaluation of the overall situation and needs of service of his client as described above. In a sense, the caseworker reveals, updates and records in the data a synthetic appraisal of various factors, including some which may have been originally unobserved to us. Our assumption then translates into the requirement that caseworkers act idiosyncratically given worker characteristics and their own assessment of their client.

A final issue relevant for both decisions (A) and (B) relates to the local labour market conditions, identified in the literature as a key variable to be controlled for (Heckman *et al.*, 1997). In Sweden,

³¹ The institutional nature of the system (a continuous flow of different programs often on an individual, *ad hoc* basis) should make it less likely for a job-seeker to have to turn down a program offer perceived as second-best in order to wait for a free slot on his first-choice program (this would also reduce the likelihood of an 'Ashenfelter dip' problem in terms of reduced job search prior to participation). Even if he did wait, though, he would not enter his first-best program with certainty, but would still be exposed to the possibility of finding a job or deciding (or be forced) to join another

county labour boards have overall responsibility for labour market policy in each respective county, and from the mid 90s municipalities have become increasingly involved in the decision-making as to ALMPs. This shift towards decentralisation has given rise to new financial incentives (Lundin and Skedinger, 2000). In particular, municipal budgets may be favourably affected by moving the unemployed from social assistance (funded by the local authorities) to programs (financed by the central government); some programs (e.g. relief work) may subsidise labour in the services typically provided by the local authorities; and programs may serve as a means of maintaining the local municipal tax base by reducing geographical mobility among job-seekers. It is thus quite possible that counties or municipalities facing different labour market conditions may favour a different balance between program and unemployment policies, as well as a different mix of programs.

In addition to county dummies, we have thus constructed the local ‘program rate’, given by the number of participants in the six programs as a proportion of all individuals registered (as openly unemployed or program participants) at the individual’s municipality. This time-varying indicator provides information as to the local program capacity (e.g. in terms of slots available). Similarly, a series of time-varying single ‘program ratios’ at the municipality level, given by the share of participants in a given program over all participants in the six programs, is meant to reflect the program mix at that municipality and time. Such indicators are intended as a parsimonious way³² to capture unobserved local aspects which are likely to be relevant for program joining/choice decision and individuals’ potential labour market performance.³³

In sum, for individuals who have reached the same unemployment duration and who are similar in terms of all the individual and local characteristics described, the decision to join a program at that time rather than at least not yet, or the decision to join a given program rather than another need to be random, i.e. to depend on factors unrelated to future potential outcomes. Sources of this required random variation conditional on our X ’s can stem from job-seekers’ idiosyncratic preferences or random variation in their outlook on their employment prospects at a given time. On the officer’s side, for given client characteristics, own judgement as to the client’s job readiness at a given time and employment office incentives regarding participation at that time, this randomness

program in the meantime.

³² There are 289 municipalities and 484 employment offices in our data.

³³ Local program capacity at a given time may affect the possibility for a job-seeker to join a program at that time as well as the officers’ scope for program selection, while offices facing more difficult local conditions may be more active in placing individuals on programs (e.g. to lighten the burden on the municipal budget or to decrease the number of openly unemployed in the municipality). Similarly, the program mix may affect program choice, while offices facing high unemployment may favour different programs (e.g. the cheapest, or those which subsidise the municipality).

can be based on caseworkers' idiosyncratic preferences, incentives and experiences, as well as propensity (and strictness) to apply the work test.

One key point is that we can also exploit bottlenecks in the system, since we are able to condition on whether an individual has been offered and is waiting for a program, but cannot yet join, due e.g. to start dates of a training course, a work-experience project or an employee taking leave for a replacement scheme. A similar lack of appropriate conditions related to a program may also provide randomness in the decision to join a program rather than another one.

Given this very rich set of observables, we thus require that selection into the programs is fully driven by factors, such as the ones just described, unrelated to potential outcomes. In fact, caseworkers do not seem, in general, to be allocating their clients so as to maximize their subsequent outcomes. For Sweden, Frölich (2001) has found that compared to the observed assignment to four types of rehabilitation programs, an optimal allocation of participants would have yielded large gains in the form of an over 20% higher re-employment rate.³⁴

4.3 Implementation

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

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

$$D \perp X | b(X), D \in \{k, k'\} \tag{3}$$

or equivalently such that³⁵

$$E[Pr(D=k | X, D \in \{k, k'\}) | b(X)] = Pr(D=k | X, D \in \{k, k'\}) \equiv P^{k/kk'}(X) \quad \text{and} \quad 0 < P^{k/kk'}(X) < 1.$$

In our case of separate pair-wise comparisons of the various treatments, the conditioning variable (balancing score) of minimal dimension which ensures the balancing of observables in the two sub-

³⁴ Analogous conclusions are reached by Lechner and Smith (2003) for Switzerland (see also the similar evidence they review for other countries).

³⁵ Cf. Theorem 2 by Rosenbaum and Rubin (1983) and proposition 1 in Lechner (2001). See also Brodaty *et al.* (2000).

populations of interest k and k' is thus still given by a scalar, the conditional choice probability of treatment k given either treatment k or k' :

$$P^{k|kk'}(X) = \frac{Pr(D = k | X)}{Pr(D = k | X) + Pr(D = k' | X)}$$

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

$$E(Y^{k'} | D = k) = E_{p^{k|kk'}}[E(Y^{k'} | D = k', P^{k|kk'}(X)) | D = k]$$

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

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

However, estimation of the treatment probabilities does offer an opportunity to capture the multiplicity of options open to individuals. In the Swedish context we 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 month $U=u$ in their first unemployment spell, they can ‘decide’ between a set of $M=11$ exhaustive and mutually exclusive options: to participate in one of the six available programs, to continue searching for a job full-time as openly unemployed, to find (or decide to accept) a job, to go on regular education, to leave the labour force through another channel, or to drop out of the unemployment register for reasons unknown to the officials. By modelling the effect of unemployment duration on exit type, one can thus simultaneously take account of the various exit routes from unemployment, of right-censoring and of the effect of time-varying characteristics on individual choices.

As to the practical implementation, each individual unemployment spell is split into monthly spells. Each of these new sub-spells is characterised by the duration month u the new sub-spell re-

fers to, by a corresponding treatment indicator and by those characteristics pertaining to, and events taking place during that u^{th} month of unemployment. The probability for an individual with characteristics x of choosing option m after having spent u months in unemployment, $Pr(D=m/U=u, X=x)$, is then estimated³⁶ and the corresponding balancing scores $b(X)$ constructed.

Note that all we are interested in (ignoring potential efficiency considerations) is achieving (3). Our interest in the estimation of $b(X)$ as well as in the choice of matching method (which determines how the conditioning on $b(X)$ is performed) purely lies in their combined ability to balance the characteristics of the matched sub-groups being pair-wisely compared. The resulting quality of the matched samples has thus guided the choice, for each pair-wise comparison, of the specification for $b(X)$ and of the matching estimator (cf. Appendix C). To adjust for the additional sources of variability introduced by the estimation of the balancing score and by the matching process itself, bootstrapped confidence intervals have been calculated.

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

As to the average effect, for participants in a given program k , of joining program k compared to waiting longer in open unemployment, we outline the approach formalised in Appendix A. First the corresponding balancing score is calculated for each k -participant and each waiting spell. Participants in k and waiting individuals are then stratified by unemployment duration $U=1,2,\dots, U_{\max}$. For a given u , those k -participants who enter the program in their u^{th} month are matched to the most similar individuals who are still unemployed after u months and hence in the ‘risk set’. Under assumption (2) the subsequent differential performance of the two matched groups identifies the average effect of joining program k in one’s u^{th} month of unemployment compared to not joining any program at least up to one’s u^{th} month. This effect starts being evaluated from entry into program k , namely from u . Note that as the comparison group comprises individuals who are unemployed up until that time and do not participate in any program at least as yet, it does not reflect a no-program state, but rather a possibly postponed participation. In particular, (2) ensures that the probability distribution of outcomes after u – including subsequent program participation – for the matched com-

³⁶ This can be thought of as a discrete-time, competing-risks hazard model. The conditioning set of observables X denotes fixed individual characteristics as well as time-varying characteristics both of the individual and of the macro local conditions they face. Time-varying observables other than elapsed unemployment duration U are defined conditional on U_i or on calendar time. They include two main sets of controls: those relating to the individual’s unemploy-

parisons is the same as the counterfactual one for the observably-similar participants in k , had they at time u decided to wait longer as well.

For reasons of presentational parsimony, one can finally aggregate all the effects of program k by time of entry, weighted according to the observed entry distribution into program k . Though a clear casual interpretation only pertains to the effects by month of entry (see Appendix A), this summary measure offers a synthetic overview of the general patterns of the effects of program k by month of placement. We will however explicitly investigate whether and how the effect of a given program differs according to the time participants have spent in unemployment before joining.

5. Empirical findings

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

Outcomes of interest are individual employment and benefit collection probabilities over time.

In particular, the effect of joining program A (compared to either joining another program or searching longer in open unemployment) on the employment probability of participants in program A is calculated from program start to 5 years on and summarises various components: a ‘lock-in’ effect, an effect on the probability of finding a job and an effect on job longevity.³⁷

The differential lock-in effect of a program vis-à-vis the comparison treatment originates from a differential job search whilst on the program. Compared to open unemployment, job search is reduced because less time is left due to participation itself. Different programs may however also differentially reduce the intensity of job search whilst participating: they may for instance leave different amounts of time and energy for job search or may entail different expectations once completed

ment experience so far (i.e. up to $U=u$) and those capturing the local conditions prevailing at $U=u$ at the individual’s office.

³⁷ Without additional assumptions it is not possible to separate out the economic mechanisms – search intensity, productivity or job attachment – leading to the identified effects (see e.g. Ham and LaLonde, 1996).

(e.g. job subsidies may induce participants to focus on the job at hand to ‘impress’ the employer in order to increase their chances of remaining with the firm afterwards).

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

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

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

5.1 *Joining versus waiting*

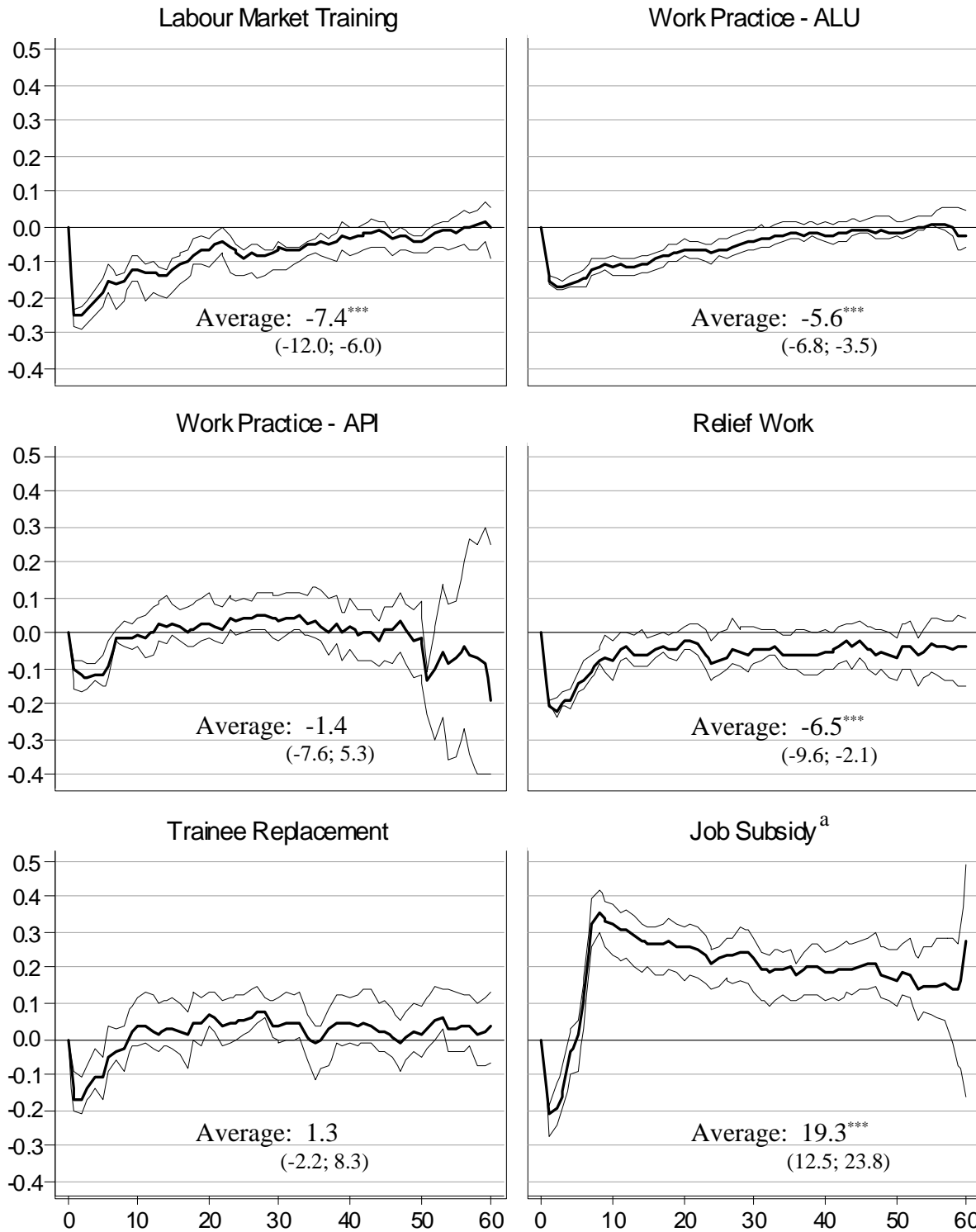
The effect of joining a given program, compared to longer job-search as openly unemployed, on their respective participants’ employment prospects over time is shown in Figure 5.1.

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

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

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

Figure 5.1 Treatment effect on employment probability of joining a given program compared to waiting longer in open unemployment, over time and averaged over 5 years (% points)



Notes: Time in months, from program start. ^a Employment probability refers to a regular (i.e. non-subsidised) job. 95 percent bias-corrected percentile bootstrapped confidence intervals (500 reps).

** significant at 1%, * at 5%, . at 10%.

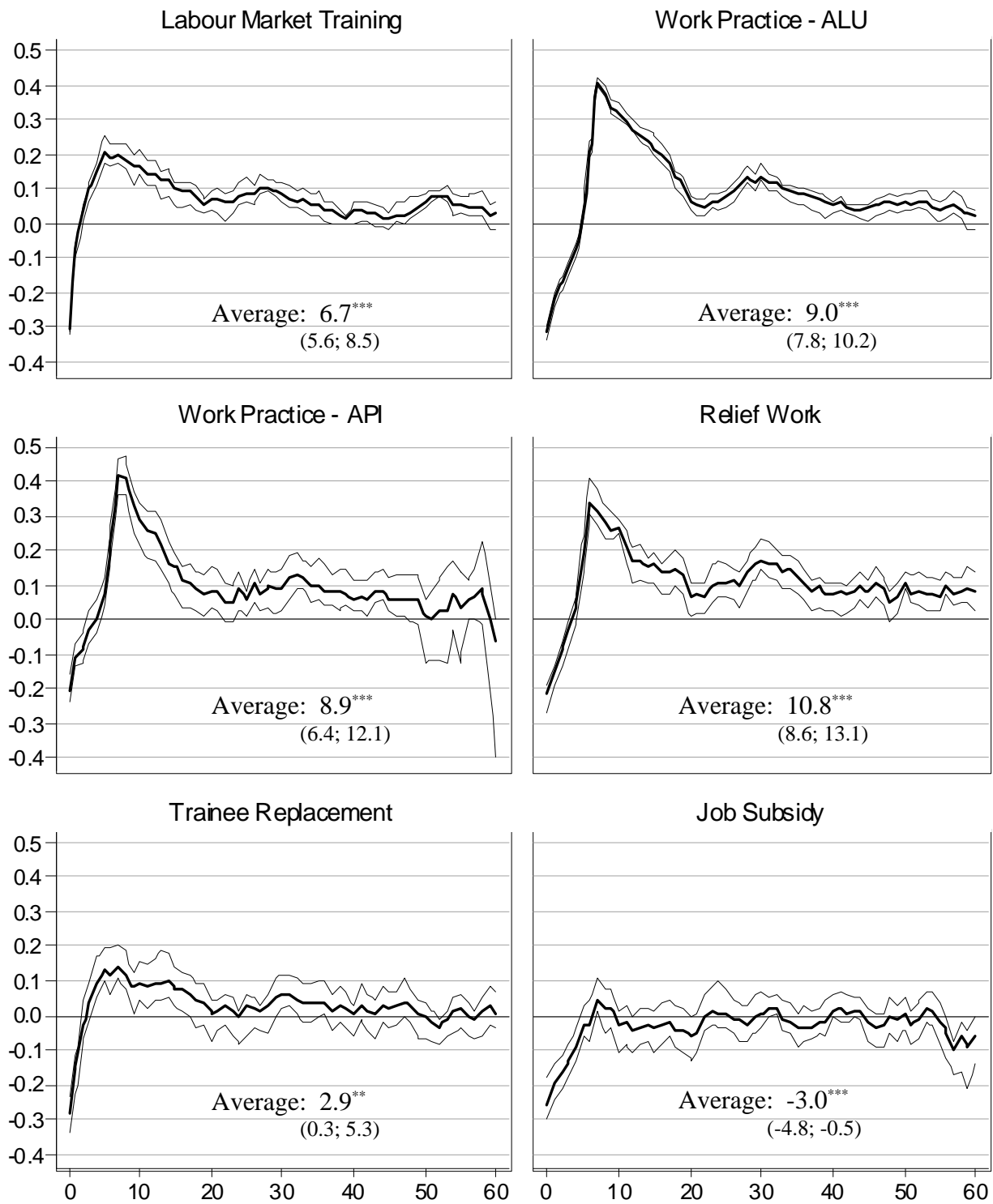
magnitude and bordering significance only in the medium term, with API participants only just 2-3 percentage points more likely to be employed than if they had waited further.

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

This supposition seems in fact confirmed by the various program effects on the probability of collecting benefits over time (Figure 5.2)³⁸. Participants in labour market training, ALU and relief work all have a significantly higher likelihood of compensated unemployment over time than if they had waited longer in unemployment; benefit renewability considerations thus appear to play an important role in the above finding of a negative treatment effect on employment rates for these measures. Further evidence of the likely failure in the way incentives are taken into account by the intertwined unemployment benefit-program institutional system is the finding that participants in these three programs appear to be drawing benefits on a ‘cycling’ basis. In particular, after the benefit-renewing duration of 5 months on their program, training participants are 20 percentage points, ALU and relief work participants around 40 percentage points more likely to be drawing compensation than if they had postponed entering their respective program. This treatment effect then remains positive until around month 20 – that is, for the maximum period of compensated unemployment of 14 months. After another 5 months – likely to be spent on another eligibility-renewing program – these participants become again significantly more likely to be drawing benefits, a treatment effect which lasts for another 14 months of maximum compensation. Over our observation window, even a third hump is recognisable.

³⁸ Note that compensation whilst on a program is not defined as unemployment benefits.

Figure 5.2 Treatment effect on benefit collection probability of joining a given program compared to waiting longer in open unemployment, over time and averaged over 5 years (% points)



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

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

To summarise, if we consider the treatment effects averaged from time of entry into the program to 5 years on, the employment prospects of those unemployed who join labour market training, relief work and ALU are all reduced by around 6-7 percentage points. API and trainee replacement participants are not significantly affected in this dimension, whilst participants in job subsidies enjoy a 19 percentage points higher employment probability overall. These latter participants are also the only ones to subsequently display a significantly lower benefit collection probability (-3 percentage points) than if they had waited further in open unemployment. By contrast, while trainee replacement participants are overall 3 percentage points more likely to be drawing benefits over our 5-year observation window, the overall effect is a 6.7 percentage points higher compensation probability for participants in labour market training and as high as 10-11 percentage points for participants in API, ALU and relief work.

Treatment effects by month of placement

The time series and averaged effects discussed above offer a synthetic overview of the general patterns of the effects of joining a given program (compared to waiting longer) by month of entry. It is interesting to investigate the extent to which these overall average patterns vary for the sub-groups of participants who entered their respective program after a different amount of time in open unemployment.³⁹ A direct causal interpretation pertains in fact to the various effects of joining a given program at a given month compared to delaying participation in any program at least up to then.

We divide participants in the six programs into groups based on the observed entry distribution into the respective program by month in unemployment (Table 5.1).⁴⁰ As a measure of the treatment

³⁹ Differential impacts by time of placement may arise from various sources: the extent of the lock-in effect may vary; program effects may depend on participants' characteristics and individuals joining at different times may differ in such characteristics; and employers may differently value (or stigmatise) participants who have joined a program at different times in their unemployment spell.

⁴⁰ To obtain reasonable sample sizes for the treated groups to be separately analysed, 3 to 4 months had to be aggregated. Due to this aggregation, this is a slightly different procedure from the one used to derive the 'overall' effects

impacts on employment and benefit collection probability, the two panels of Table 5.2 present the average of the respective month-by-month effects over the 5-year horizon since program start.

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

In fact, training as well as trainee replacement do not seem to be chiefly used as vehicles to escape benefit exhaustion. Not only do merely 11 and 8% of participants in these two programs join between months 13 and 16, but for these entrants the treatment effects on subsequent benefit collection probability are small and insignificant. This is in sharp contrast to relief work, API and especially ALU. By far the largest share of participants in these programs joins around benefit exhaustion (cf. Table 5.1), and the treatment effects on subsequent benefit collection probability are positive, large and highest for such entrants than for earlier ones.

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

above, or the effects by month of placement in Sianesi (2004). In particular, comparison units for, say, entrants between months 1 and 3 are now constrained to be unemployed *for at least 3 months*. While the effects in Table 5.2 are thus slightly more favourable to participants, the focus is here on comparing the effects for different entrant groups.

Table 5.1 Participants' distribution by time of placement:
Total number and percentage of which has joined at different months

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

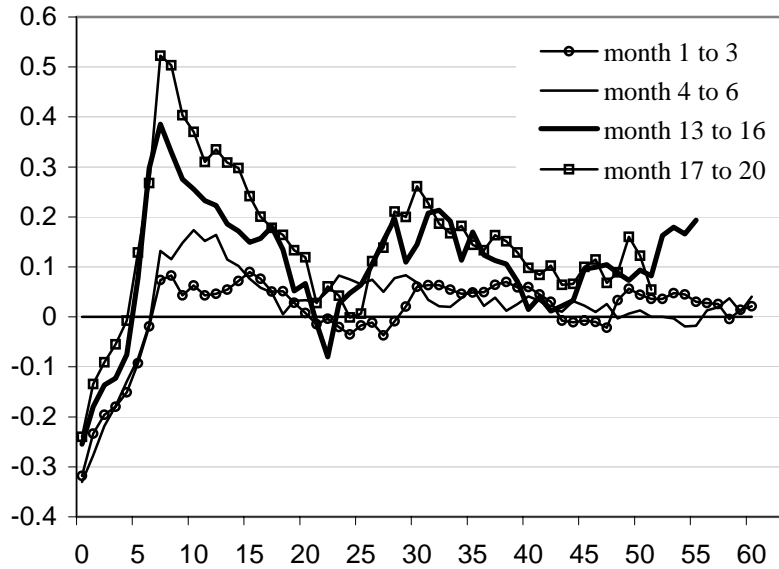
Notes: *m* = months. In **bold**, the groups of participants separately analysed.

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

(A) Employment probability						
	<i>m</i> = 1-3	<i>m</i> = 4-6	<i>m</i> = 8-10	<i>m</i> = 11-12	<i>m</i> = 13-16	<i>m</i> = 17-20
Labour market training	-6.2** (-15;-0.9)	-5.9** (-13.6;-1.7)	3.5 (-3.6;11.6)	-3.8 (-16.1;3.2)	1.3 (-11.9;10.3)	
Work practice – ALU	1.8 (-3.7;10.7)	0.6 (-1.7;9.5)	-2.8 (-8.1;3.8)	-7.1** (-14.5;-0.8)	-2.5 (-7.2;2.6)	-0.2 (-1.8;3.6)
Work practice – API					1.1 (-7.5;13.7)	-3.4 (-18.9;5.0)
Relief work	-3.9 (-14.8;7.8)	-3 (-13.6;7.8)	-12.7*** (-25.3;-5.1)		-1.9 (-10.4;5.3)	
Trainee replacement	0.4 (-10.4;4.4)	-5.9 (-20.7;1.9)		11.1*** (6.1;21.9)	7.6 (-11;23.2)	
Job subsidies		17.3*** (6.6;22.7)		30.8*** (24.4;42.8)	34.3*** (28.9;44.7)	
(B) Benefit collection probability						
	<i>m</i> = 1-3	<i>m</i> = 4-6	<i>m</i> = 8-10	<i>m</i> = 11-12	<i>m</i> = 13-16	<i>m</i> = 17-20
Labour market training	5.8*** (3.6;9.4)	6.9*** (3.8;10.3)	6.5*** (2.7;10.3)	8*** (2.7;14.8)	1.4 (-8.8;6.2)	
Work practice – ALU	0.9 (-3.4;3.1)	2.2 (-1.7;4.8)	8.1*** (6.4;12.8)	7*** (1.8;9)	10.4*** (6.2;12.5)	14.7*** (9.6;17.5)
Work practice – API					9.0*** (1.9;13.2)	14.4*** (5.2;20.2)
Relief work	6.3* (-2.1;10)	7.7*** (3.9;14.2)	12.7*** (9.6;20.1)		13.4*** (6.5;18.4)	
Trainee replacement	2.4 (-1.4;7.7)	0.7 (-6.5;4.4)		2.7 (-2.9;9.6)	5.2 (-4.6;17.2)	
Job subsidies		-8.4*** (-15.7;-6.1)		-7.5*** (-15.7;-2.8)	-5.8** (-14.7;-0.3)	

Notes: *m*=months. Average over 60 months except for *m*=13-16 (over 54 months) and *m*=17-20 (over 51 months).
In brackets, 95% bias-corrected percentile bootstrapped confidence intervals (500 reps);
*** significant at 1%, ** at 5%, * at 10%.

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



Notes: Time in months, from program start.

The size of the treatment effect on benefit collection is in fact found to steadily increase with placement time. This same pattern was uncovered also for participants in relief work; where from 6 percentage points for early joiners, the treatment effect gradually rises to over 13 for individuals joining around the time benefit expire.

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

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

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

5.2 Differential program effects

So far the six programs have each been separately compared to the ‘waiting longer’ alternative. Since the composition of individuals participating in the various programs clearly differs (cf. Table A1), such comparisons do not allow us to assess whether the programs are well targeted, nor whether there are absolute winners among them. This section directly looks into these important questions by comparing the programs against one another and estimating the differential effects on employment and on compensated unemployment probabilities for all such pair-wise comparisons.

Table 5.3 displays the treatment effects averaged over 5 years since entry into the program (the complete time series of the two treatment effects for all pair-wise comparisons is shown in Figures A1-A5 in the Appendix). Although later in the section ALU and API are explicitly contrasted, in the initial discussion they are lumped into one type of treatment, ‘work practice’. As seen in Section 2, the two measures have a very similar overall aim, nature and implementation. In particular, they are centred on work experience and prevent participants from performing regular tasks.⁴¹ This at least formal equivalence has in fact been sanctioned by the employment offices themselves in January 1999, when the two measures were collapsed into the new Work Practice Scheme.

As to the relative performance of the different programs in terms of employment, the star program is clearly job subsidies again. Participants in this program enjoy a much higher employment probability over time than if they had joined any of the alternative programs, with the gain decreasing from over 40 percentage points at the time the programs typically end to a still significant and substantial 10 percentage points towards the end of our observation period. Over these 5 years, job subsidy participants are overall around 25 percentage points more likely to be in employment than if they had joined training, work practice or relief work, and 16 percentage points more than if they had joined a replacement scheme. In addition, participants in any of these other programs would have fared considerably better (20-25 percentage points) had they gone on job subsidies instead.

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

⁴¹ ALU’s additional UI-entitlement requirement is not binding in our sample of entitled individuals.

Table 5.3 Average differential effects of joining program A rather than program B for participants in program A, averaged over the 5-year horizon since program start (% points)

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

(B) Benefit collection probability					
	Comparison				
	Training	Work practice	Relief	Replacement	Subsidies
Training		-1.3 (-2.6;0.5)	1.0 (-0.8;3.6)	9.0 ^{***} (6.7;12.6)	10.0 ^{***} (6.2;14.2)
Work practice^a	2.2 ^{***} (0.8;3.9)		0.7 (-0.8;3.1)	8.6 ^{***} (5.7;12.3)	13.8 ^{***} (11.8;16.3)
Relief	4.2 ^{***} (2.5;6.5)	0.6 (-0.8;2.2)		4.6 ^{**} (0.5;9.0)	12.6 ^{***} (7.6;17.3)
Replacement	-5.8 ^{***} (-8.3;-2.5)	-2.6 ^{**} (-4.9;-0.4)	-3.8 ^{**} (-6.5;-0.4)		7.1 ^{***} (2.7;9.5)
Subsidies	-10.1 ^{***} (-11.9;-7.7)	-12.8 ^{***} (-14.2;-11.0)	-14.0 ^{***} (-17.2;-11.2)	-6.7 ^{***} (-8.9;-3.3)	

Notes: ^a ALU and API combined.

Average over 60 months.

In brackets, 95% bias-corrected percentile bootstrapped confidence intervals (500 reps);

*** significant at 1%, ** at 5%, * at 10%.

participants is the least when measured relative to replacement schemes, while replacement participants would have gained less from job subsidies than participants in the other measures would have.

Trainee replacement is in fact found to be the second best performing program overall, corroborating the presumption that it should teach market-relevant skills, given that the task performed is by construction a useful one, for which the firm was willing to pay a regular employee. While being just as likely to be employed over time than if they had joined relief work, former deputies enjoy 6 percentage points higher employment chances overall than if they had joined training or work prac-

tice. Conversely, participants in work practice, training and even relief work would have considerably improved their labour market prospects had they joined a replacement scheme instead.⁴² Similar conclusions also hold in terms of benefit collection: deputies are significantly less likely to be in compensated unemployment over time than if they had joined training, relief work or work practice, while participants in these three measures would have been significantly less likely to be collecting benefits had they enrolled in a replacement scheme.

Out of these remaining programs – labour market training, work practice and relief work – training seems to be the best-performing one; trainees are overall 4 percentage points more likely to be employed than if they had gone on work practice, while work practice as well as relief work participants would have had 3-4 percentage points higher employment chances had they gone on training. In terms of compensated unemployment, while trainees are just as likely to be drawing benefits than if they had enrolled in work practice or relief, participants in these measures would be 2 and 4 percentage points less likely to be collecting benefits overall had they joined training.

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

API versus ALU

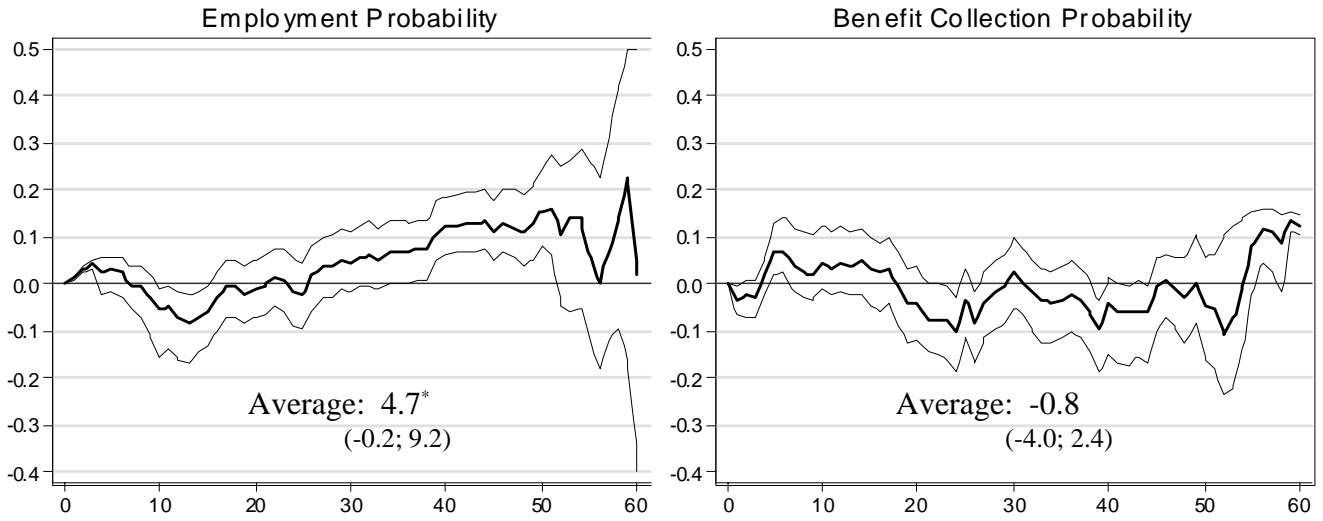
As to the two work practice measures, their potentially different effectiveness is of particular interest: while sharing their basic features, ALU is exclusively reserved to those entitled to unemployment benefits and has been explicitly introduced to prevent them from running out of compensation. Compared to longer search in open unemployment, Figures 1 and 2 have shown that while the two measures are substantially equivalent in terms of their large positive impact on benefit collection, in terms of employment prospects the performance of ALU is visibly worse than the one of API. These effects however relate to two different groups – individuals participating in ALU and individuals participating in API. By contrast focus in the following is on how participants in one of the two programs would have fared had they joined the other program instead.

Figure 5.4 interestingly shows that as far as employment is concerned, the two programs seem to be well targeted. In particular, participants in ALU overall gain 4.7 percentage points higher em-

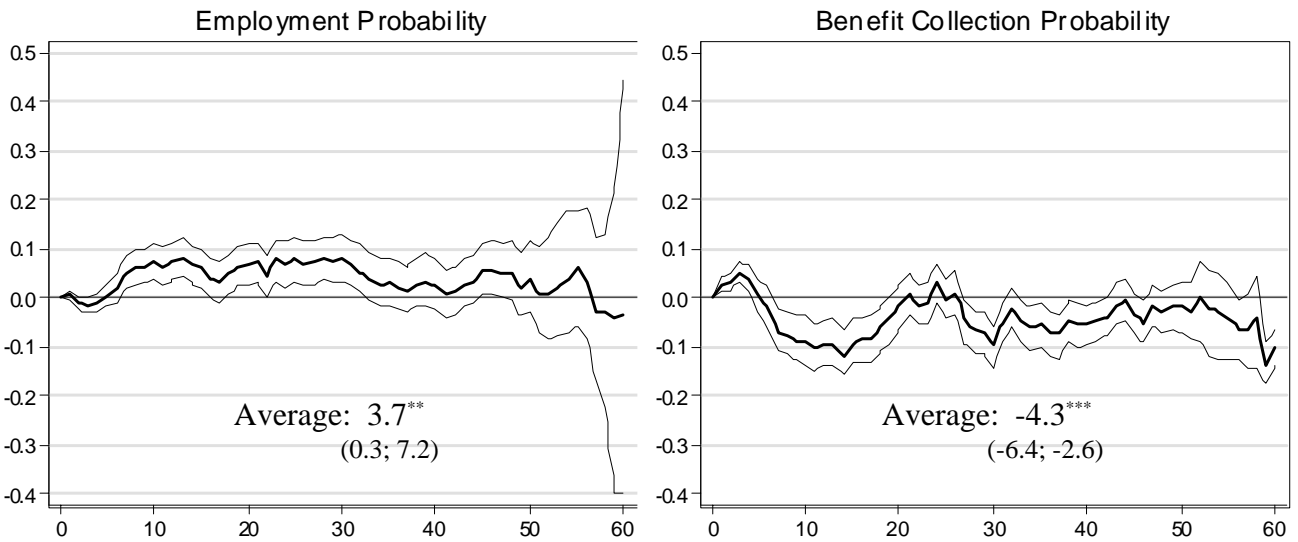
⁴² In fact, the gain participants in these three programs would have enjoyed from going on trainee replacement (treatment on the non-treated) would have been roughly double the size of the average benefit deputies enjoy from their own

Figure 5.4 Differential performance of ALU and API, over time and averaged over 60 months

(A) ALU compared to API for ALU participants



(B) API compared to ALU for API participants



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

ployment probability averaged over the observation period from having joined their program rather than API. Participants in the latter program too do benefit from their choice, enjoying a 3.7 percentage points higher employment probability overall than if they had joined ALU instead. Quite interestingly, the differential gains from the two programs arise at different times: the positive API ef-

participation (treatment on the treated).

fect for its participants lasts until the end of the 3rd year since joining, while the gain from ALU for its participants occurs only during the 4th year.

As to benefit collection probability, participants in ALU are just as likely to be drawing benefits than if they had joined API. Note that both ALU and API allow our sample of *entitled* individuals to renew their benefits; this finding simply means that the extent to which those individuals joining ALU remain in compensated unemployment is unaffected by which of the two work practice scheme they enrol in. By contrast, participants in API are significantly less likely to be collecting compensation than if they had joined ALU. Put it differently, API participants would have been significantly more likely (by over 4 percentage points overall) to be drawing benefits had they joined ALU instead. Furthermore, they would have collected such compensation on a cycling basis.

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

5.3 The problem of the ‘lost’ individuals

A final issue concerns an attrition problem in the Händel dataset, whereby a registered unemployed individual, having first missed an appointment at the official employment office and subsequently failing to contact the agency within a week, is simply de-registered – thus lost from the data – without information on whether a job has been found or whether the individual is still unemployed. Bring and Carling (2000), who have tried to trace back a sample of ‘lost’ individuals, have found that around half of them had in fact found a job, highlighting how employment status may be badly under-reported in the official data. More critically, though, it is quite possible that the probability of being in a lost spell over time, as well as the true state (employed *versus* unregistered unemployed) once in a lost spell may be systematically different among individuals taking the various treatments. Although in our sample of entitled individuals this attrition problem is considerably less severe than in the full sample, almost 9% of our individuals do become ‘lost’ after their first (registered) unemployment spell (see Table 3.2), while the probability of being lost over time steadily rises to 12% over our 5-year horizon. The robustness of our findings as to employment effects thus needs to be carefully checked against these lost spells.⁴³

⁴³ Benefit collection, being conditional on registration at an employment office, is not affected.

Following Sianesi (2004), the additional information from the Bring and Carling survey has been exploited to perform best- and worst-case bounds analysis on all the pair-wise comparisons of the treatments.⁴⁴ These robustness checks, shown in Figures A6 and A7 in the Appendix, leave the conclusions discussed above in fact virtually unchanged, in terms of both the differential performance of the various programs, as well as the positive employment effect of job subsidies and the negative ones of relief work, ALU and training compared to longer job search.

6. Discussion and conclusions

This paper has investigated the differential performance of six main types of Swedish programs both relative to one another and vis-à-vis more intense job search in open unemployment.

Starting from the latter comparison, all the programs were found to initially lock in their participants, significantly reducing their employment probability.⁴⁵ As to the treatment effects on more long-term employment prospects as well as on unemployment benefit collection, job subsidies proved to be an extremely successful measure. Trainee replacement schemes were found to perform rather satisfactorily, while results for all the other programs have been discouraging.

In particular, individuals joining labour market training, a work practice scheme (API or ALU) and relief work were found to subsequently display *lower* employment rates coupled with a higher benefit collection probability than if they had searched further as openly unemployed.

ALMPs in OECD countries have in fact a mixed track record. Negative effects of public training programs are far from unheard of; in the US, there is experimental evidence of some of these measures being ineffective – sometimes even counterproductive – for economically disadvantaged males and especially youth (see e.g. Heckman *et al.*, 1999, Friedlander *et al.*, 1997). Negative training effects have also been uncovered in Europe (e.g. recently by Lecher *et al.*, 2005, for Germany, who link these findings to training participants having become locked in a declining sector).

⁴⁴ The conditional probability that a lost individual ($L=1$) with characteristics X has in reality found employment ($Y=1$) can be decomposed as: $P(Y=1|X,L=1) = P(Y=1|X,L=1,D=1) \cdot P(D=1|X,L=1) + P(Y=1|X,L=1,D=0) \cdot [1 - P(D=1|X,L=1)]$, where for each pair-wise treatment comparison, $D=1$ denotes the treatment and $D=0$ the comparison treatment. For each lost individual i , we know treatment his status D , we can estimate his treatment probability given the lost status $P(D=1|X,L=1) \equiv p_i^D$ and based on the survey we can impute his misclassification probability $P(Y=1|X,L=1) \equiv p_i^Y$. In order to derive worst- and best-case bounds, we assign $P(Y=1|X,L=1,D=d)$ by setting $P(Y=1|X,L=1,D=1-d)$ to its maximum or minimum, compatible with the given p_i^D and p_i^Y , as well as with all probabilities being in $[0; 1]$.

⁴⁵ All survey studies of search activity in Sweden have in fact uncovered sizeable lock-in effects: irrespective of timing, program and age group, individuals on a program search less frequently and in fewer ways than the openly unemployed (see Calmfors *et al.*, 2001, for references).

As for Sweden in the mid 90s, several factors may account for the discouraging findings uncovered here. It might be more difficult to put participants back into stable work in periods of high unemployment, or the massive use of ALMPs in the 90s might have resulted in inefficient program administration.^{46, 47} However, an additional most likely explanation relates to the use of programs simply as a way to re-qualify for unemployment benefits, with programs ending up locking participants – and in particular those entitled to unemployment compensation – in the unemployment system.⁴⁸ In fact, when exploring the extent to which the treatment effects vary for participants who joined their program after different amounts of time, we found relief work, API and especially ALU to be chiefly used as vehicles to escape benefit exhaustion. Note that these are exactly those programs formally preventing participants from performing labour-market relevant tasks. The first broad lesson from the Swedish experience thus seems to be that the use of programs predominantly as ‘parking slots’ for the unemployed and as gateways to renewed entitlement has, maybe unsurprisingly, caused job-seekers to drift into long-term unemployment and benefit dependency.

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

Scrutinising the Swedish experience to derive general lessons as to which type of program works best, we thus found that those programs providing (subsidised) workplace experience and on-the-job training at an employer are relatively more effective for participants’ subsequent labour market success than vocational classroom training courses. Furthermore, the more relevant and the closer to the competitive labour market the kind of task performed, the higher the program ranks.

The similarity of results across studies looking at different countries with varying labour market structures and policies may in fact indicate a general validity of these overall conclusions. Specifically, in several OECD countries subsidised employment has been found to have a greater impact than public training or direct job creation measures (see e.g. the papers in footnote 5 and, for a review, Martin and Grubb, 2001). Recent work comparing the effectiveness of the four options of the

⁴⁶ Bloom *et al.* (2001) find evidence that high unemployment as well as large caseloads reduce the impacts of welfare-to-work programs in the US. As to labour market training in Sweden, Calmfors *et al.* (2001) highlight a switch from positive effects in the 80s to negative ones in the 90s.

⁴⁷ By contrast, stigma effects from program participation are not likely to apply in Sweden, where employers view former participants more favourably than openly unemployed individuals (see the survey studies in Calmfors *et al.*, 2001).

New Deal for Young People in the UK similarly finds that the employment option performs best compared to full-time education and training, the voluntary sector and the environmental task force (Bonjour *et al.*, 2001; Dorsett, 2004). The finding that the ‘work first’ approach of the employment program dominates the human capital approach of the training measure is also in line with the meta-analysis of US welfare-to-work programs by Greenberg *et al.* (2004).⁴⁹

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

It is however important not to jump to the hasty conclusion that job subsidies are *the* solution – the most effective program as well as the cheapest. Several issues can be raised both in terms of the effective magnitude of the uncovered effects and in terms of their general applicability should the scope of the program be extended. A first issue concerns the validity of the CIA assumption for participants in this program: since job subsidies informally entail the expectation that the engagement will then continue, it is likely that potential candidates are considered quite carefully. Even though we control for a host of factors underlying the caseworker’s judgement, it may still be the case that subsidised participants are slightly ‘better’ on average than matched comparisons. Still, it would be hard to argue that remaining selection bias could account for all of the very 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, though, 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

⁴⁸ Findings in Sianesi (2001 and 2004) considering the overall program system lend support to the conjecture that for individuals entitled to unemployment compensation, the eligibility renewability rules can significantly distort the incentives for participation and wipe out potential productivity-enhancing effects of the programs.

⁴⁹ The US literature is not directly comparable to the present, and more generally, to European evaluations. Apart from mainly looking at a different outcome (earnings rather than employment) and target population (the economically disadvantaged rather than displaced workers), the US literature on ‘employment subsidy measures’ has in fact looked at treatments which include not only hiring subsidies, but also public service employment (the counterpart of relief work in Sweden) and temporary work experience in the public and non-profit sectors (the counterpart of work practice). The definition of treatment (see Heckman *et al.*, 1999, table 1) is thus largely different too, and in fact likely to account for why these ‘employment subsidy measures’ have at times been found to be ineffective, especially for males, in the US. An additional problem, mentioned in the introduction, is that the US programs mostly come as packages of different measures, which makes it very hard to isolate where the effect is coming from. For example, Friedlander *et al.* (1997) point out that it remains unclear whether the positive earnings effects from JTPS stem from skills acquisition, program assistance, or the wage subsidy given to employers as part of the activity.

general equilibrium effects which may spill over to other groups. In particular, substitution would take place if participants in the job subsidy program were to take (some of) the jobs that participants in the other programs or ‘waiting’ unemployed individuals would have been offered in the absence of the subsidies. The impact of the subsidy would thus be at the expense of worsened conditions either for participants in other programs or for openly unemployed individuals finding it more difficult to get jobs. The estimated effect would in this case overestimate the net social impact of the subsidy program. Both survey and econometric Swedish studies do in fact find sizeable (around 65-70%) direct displacement effects arising from those Swedish programs that generate subsidised employment.⁵⁰ Finally, it is obviously unthinkable to generalise such a measure to *all* unemployed job-seekers: it would simply become just a way to subsidise firms’ hirings, resulting in huge dead-weight effects (i.e. subsidising hiring that would have taken place anyway).

In the light of our results and of the above considerations, a more promising measure might appear to be trainee replacement. Still among the cheapest, 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: during the temporary employment spell it provides relevant job-specific training and can be used as a cheap screening device of individual unobserved productivity. At the same time it sends out a message that the individual 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.⁵¹ Finally, our partial-equilibrium estimates are likely to underestimate the program’s full effect, since they do not take into account the ‘double-dividend’ arising from the possibility offered to the replaced employees of increasing their human capital through training. Nevertheless, a few issues call once again for caution. The ‘double-dividend’ from the subsidised training of the replaced employee may in fact often turn out to be dead-weight loss instead⁵², while Harkman *et al.* (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 re-

⁵⁰ These results come from the Swedish macro studies (reviewed in Calmfors *et al.*, 2001), but apply more generally. Using macro data for OECD countries, Boone and Van Ours (2004) find that expenditure on labour market training is most effective, on public employment services has some effect and on subsidized jobs is not effective at all in bringing down aggregate unemployment.

⁵¹ These arguments are in line with the complaint by Swedish employers about the excessively large number of labour market measures, which prevents them from being able to make informed judgements as to the exact content and value of each program. Part of the success of job subsidies and trainee replacement may thus stem from the fact that they offer firms a cheap way to screen workers’ productivity in regular and known activities.

⁵² Since 80 to 90% 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.)

placement with the *same* employer. Finally, survey studies have in fact uncovered displacement effects of the same order as job subsidies (e.g. AMS, 1998).⁵³

In conclusion, this analysis unambiguously joins previous micro studies in finding that the more it resembles regular employment in the competitive labour market, the higher the program's benefits to its participants. It is however essential to consider this evidence in the light of that arising from the macroeconomic literature, which has widely documented that exactly for these types of programs the potential for negative crowding-out and dead-weight effects is largest.⁵⁴ Taken together, the various results clearly highlight a difficult trade off to be faced by labour market policy.

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⁵³ As an average across survey studies, 42 percent (see Calmfors *et al.*, 2001).

⁵⁴ See in particular the evidence reviewed in Calmfors *et al.* (2001).

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Appendix

A. Technical Appendix

In this Appendix the framework of Imbens (2000) and Lechner (2001), which generalises Rosenbaum and Rubin's (1983) binary potential outcome approach to the multiple-treatment case, is adapted to the institutional set-up and evaluation questions described in Section 4.1.

Let U , elapsed unemployment duration since inflow into unemployment, be viewed as discrete and measured in months. The population of interest (eligibles) at time u are those still openly unemployed after u months. The eligibles at u can choose from a set of $K+1$ mutually exclusive 'treatments': joining one of the $K=6$ programs or waiting longer than u in open unemployment. Interest lies in the causal average effect of a treatment relative to another treatment on individual labour market status over time, $\{Y_t^{(u)}\}_{t=u}^T$ (in our application $T=60$ months). The (u) superscript is a reminder that the time series of outcomes $Y_t^{(u)}$ starts being defined from $t=u, u+1, \dots, T$ and possibly depends on treatment exposure at u . A set of potential outcomes at t ($t \geq u$) is correspondingly associated to each of the $K+1$ treatments: $Y_t^{0(u)}, Y_t^{1(u)}, \dots, Y_t^{K(u)}$ with $Y_t^{k(u)}$ ($k \neq 0$) denoting the potential outcome at t if joining program k in one's u^{th} month and $Y_t^{0(u)}$ the potential outcome at t if not joining any program at least up until u .

Let $D^{(u)} \in \{0, 1, \dots, K\}$ denote actual treatment assignment, that is $D^{(u)}=k$ if joining program k at u and $D^{(u)}=0$ if not joining any (at least up to) u .¹ Since any given individual receives only one of the treatments, his remaining K potential outcomes are unobserved counterfactuals.

We are interested in the generalisation of the 'effect of treatment on the treated': the $(K+1)K$ pair-wise comparisons of the average effect of treatment k relative to treatment k' conditional on assignment to treatment k . Specifically, for each u , interest lies in the time series of $\Delta_t^u(k, k')$, the average impact at time t , for those joining program k in their u^{th} month of unemployment, of joining program k at u compared to receiving a different treatment k' at u :

$$\Delta_t^u(k, k') \equiv E(Y_t^{k(u)} - Y_t^{k'(u)} | D^{(u)}=k) = E(Y_t^{k(u)} | D^{(u)}=k) - E(Y_t^{k'(u)} | D^{(u)}=k) \quad (1)$$

¹ For this representation to be meaningful, the stable-unit-treatment-value assumption (SUTVA – Rubin, 1980) has to be fulfilled, requiring treatment status as well as all the potential outcomes of a given individual to be independent from the treatment status of others, the latter ruling out the possibility of general equilibrium effects.

for $t=u, u+1, \dots, T$ and for $k \in \{1, \dots, K\}, k' \in \{0, 1, \dots, K\}, k \neq k'$.

Since the observed program duration is endogenous, measurement of $\Delta_t^u(k, k')$ starts at time u ; the program treatments have in fact been defined as *joining* a given program (in a given month), so that the causal effect starts to work upon joining. Note in particular that the comparison group for $\Delta_t^u(k, 0)$ is made up of all those still unemployed at u , *irrespective* of what happens after u . Some of them may later go on a program, while some others may find a job before ever joining one. For both comparisons and those joining a program at u , whatever happens after u is viewed as an outcome of the joining *versus* waiting decision at u . Finally note that the various treatment effects by month u are always based on a comparison of individuals who have reached the same elapsed duration in unemployment: some join program A at u , some join program B at u and some do not join any program at least up to u .²

To identify all the counterfactuals $E(Y_t^{k'(u)} | D^{(u)}=k)$, we invoke an extension of the conditional independence assumption that conditional on the value of observables X , the (counterfactual) distribution of $Y_t^{k'(u)}$ for individuals joining program k after u months is the same as the (observed) distribution of $Y_t^{k'(u)}$ for individuals joining program k' at u or, for $k'=0$, deciding to wait longer than u :³

$$D^{(u)} \perp (Y_t^{k(u)}, Y_t^{k'(u)}) | X=x, D^{(u)} \in \{k, k'\} \quad \text{for } k, k' \in \{0, 1, \dots, K\}, k \neq k' \text{ and } t=u, u+1, \dots, T \quad (2)$$

The unobserved counterfactuals can thus be identified:

$$E(Y_t^{k'(u)} | D^{(u)}=k) = E_X [E(Y_t^{k'(u)} | D^{(u)}=k, X) | D^{(u)}=k] = E_X [E(Y_t^{k'(u)} | D^{(u)}=k', X) | D^{(u)}=k]$$

with the inner expectation identified due to (2) and the outer expectation taken with respect to the distribution of X for participants in k . The latter highlights how in order to adjust for differences in X , all participants in k need to have a counterpart in the k' -group. Let the (generalised) propensity score $p^k(X; u)$ be the conditional probability of receiving treatment k after u months in unemployment: $p^k(X; u) \equiv Pr(D^{(u)}=k | X=x)$. The common support requirement for all pair-wise conditional parameters then translates into: $0 < p^k(X; u) < 1$ for $k=0, 1, \dots, K$.⁴

² Those joining a program or finding a job drop out of the eligibles exposed to the treatments in later months.

³ Its weaker form in terms of conditional mean independence would suffice.

⁴ 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. $p^{k'}(X; u) > 0 \forall X: p^k(X; u) > 0$.

The treatment effects by month of entry, $\Delta_t^u(k, k')$, can then be averaged over the observed entry distribution in program k :

$$\begin{aligned} \Delta_t(k, k') &\equiv E_U[\Delta_t^u(k, k') | D = k] \\ &= \sum_{u=1}^{U_{\max}} Pr(D^{(u)} = k | D = k) E(Y_t^{k^{(u)}} - Y_t^{k'^{(u)}} | D^{(u)} = k) \equiv \sum_{u=1}^{U_{\max}} \omega^u(k) \cdot \Delta_t^u(k, k') \end{aligned} \quad (3)$$

with $D=k$ denoting participants in program k at any time and U_{\max} the longest observed pre-program unemployment duration, so that $Pr(D=k) = \sum_{u=1}^{U_{\max}} Pr(D^{(u)}=k)$. The weights $\omega^u(k) \equiv Pr(D^{(u)}=k | D=k)$ are given by the share of participants in program k observed to join that program at the various months u .

For the various program-to-program comparisons – i.e. for $k' \in \{1, \dots, 6\}$ – the parameters in (3) have the usual causal interpretation as the average effect of joining program k compared to joining program k' for those observed to enter program k , and are thus directly informative of the relative performance of the different programs. Stratifying by month of entry is simply a careful way to control for (or match on) unemployment duration before joining the respective program.

As to the separate effectiveness of the single programs compared to longer search in open unemployment – i.e. for $k'=0$ – however, the parameters in (3) no longer have a straightforward causal interpretation given to the indeterminate nature of Y^0 . Though for this comparison it is the effects by month of entry $\Delta_t^u(k, 0)$ that have a clear causal interpretation, their algebraic weighted average in (3) allows one to gain a synthetic overview of their general patterns and can be useful as a more concise summary measure.

B. Selected descriptive statistics, by type of exit from first open unemployment spell

	Program participants						Exits from unemployment			
	Training	ALU	API	Relief	Replac.	Subsidy	Employed	Inactivity	Education	Attrition
Female	43.5	43.7	50.4	26.8	79.3	33.8	47.0	77.4	66.2	49.5
Age (years)	38.4	40.5	36.5	39.6	37.6	38.5	37.7	34.5	32.8	37.1
Foreign citizen	6.1	7.4	21.2	9.2	3.7	4.7	4.6	7.1	5.9	10.2
Educ.: Compulsory	29.6	33.5	35.3	33.6	18.0	32.6	26.0	24.8	22.2	30.4
Secondary	8.7	7.9	7.3	7.2	6.4	7.0	6.5	10.4	16.8	9.2
Sec. vocational	53.2	41.0	35.3	50.2	48.9	47.4	45.2	45.0	41.2	39.5
University	8.5	17.6	22.1	9.0	26.7	12.9	22.3	19.8	19.8	20.9
Has education for job	64.5	64.6	59.1	66.9	77.4	67.6	73.5	67.9	54.6	64.4
Experience: None	6.1	7.5	19.3	5.5	11.0	3.5	5.3	7.1	15.2	6.8
Some	9.8	12.0	15.5	11.8	17.0	12.7	11.5	15.2	19.6	14.2
Much	83.5	79.9	64.2	82.7	71.6	83.1	82.7	76.1	59.3	77.9
KAS	5.9	6.2	13.6	14.7	5.2	12.7	8.2	6.1	6.2	15.8
Daily wage (SEK)	641.1	667.7	601.9	665.1	555.2	646.6	665.3	590.8	587.4	617.0
Worked 20h/week	1.1	2.4	2.4	2.8	2.1	2.3	1.4	2.2	2.0	2.1
Worked 30h/week	3.2	2.8	3.5	1.7	7.2	1.2	4.1	4.7	5.5	4.4
Worked 40h/week	84.4	83.1	79.8	86.2	67.7	87.6	81.1	75.2	72.6	76.6
Sector:										
Professional, technical	8.7	15.2	15.3	9.2	16.6	12.7	14.8	11.6	16.0	13.7
Health, nursing, social	6.6	9.9	7.3	5.7	51.3	3.5	16.0	24.0	23.3	17.0
Admin, managerial,	19.7	16.5	18.1	6.4	8.7	16.4	13.4	17.6	16.1	12.0
Sales	12.1	13.4	15.1	9.0	5.2	23.0	10.5	13.6	10.6	12.7
Agric., forest, fishery	0.9	1.8	0.7	3.2	0.2	1.9	2.2	1.3	1.2	1.4
Transport, communica-	6.4	5.0	4.9	4.8	2.3	8.5	5.8	4.6	3.8	6.8
Production	31.5	25.2	18.6	48.8	6.0	22.5	26.2	11.0	11.0	18.9
Services	10.2	10.1	14.6	10.0	9.1	8.9	9.7	13.6	9.6	15.1
Other	3.8	2.9	5.4	2.9	0.6	2.6	1.4	2.7	8.4	2.5
Looks for part-time job	3.9	6.5	5.6	4.0	9.7	4.2	7.2	11.8	6.7	7.8
Interlocal job seeking	17.7	15.3	15.5	17.3	12.2	13.4	14.6	8.5	8.4	11.6
Part-time unemployed	11.4	12.2	22.1	7.4	35.2	15.3	33.0	36.6	21.3	38.2
Caseworker judgement ^a :										
Job ready	91.5	95.5	92.9	95.9	91.5	97.4	81.6	76.6	83.6	77.2
Needs guidance	16.2	11.3	19.1	10.6	4.6	7.7	3.2	6.5	6.4	5.9
Offered a program	9.7	3.9	8.0	4.3	0.6	3.5	0.5	0.7	0.5	0.9
Difficult to place	5.9	2.5	4.0	1.5	1.2	2.1	2.0	11.4	7.2	5.4
Local program rate	24.9	24.5	25.3	24.1	24.2	23.7	23.3	23.5	18.8	23.3
County: Stockholm	6.0	19.6	32.0	26.1	9.9	30.5	22.2	20.2	25.9	35.8
Malmöhus	21.2	13.4	10.8	9.4	13.9	14.8	12.6	12.9	12.7	13.6
Göteborg/M.	11.6	18.3	22.1	16.1	12.2	13.8	15.1	16.7	16.9	14.3
Month of entry: Jan	15.8	13.9	6.1	19.8	16.4	8.7	16.6	15.3	7.2	13.7
Jun	11.0	9.6	8.7	6.6	10.1	10.6	10.1	9.9	33.3	10
Dec	5.1	6.0	13.4	5.5	5.0	10.3	6.5	6.0	3.9	6.1
Median unempl. duration	182	365	456	213	152	304	122	213	91	274
Median progr. duration	91	154	154	147	105	153				
Number	1,387	2,983	425	654	483	426	15,972	2,680	2,456	2,739

Note: Age is measured at entry into unemployment; all other time-varying variables at program entry or at de-registration.

^a Caseworkers' assessment can be updated during the unemployment spell; the figures thus add up to over 100.

C. Indicators of matching quality

Treatment	N ₁	Comparison	N ₀	Probit ps-R ² before ⁽¹⁾	Probit ps-R ² after ⁽²⁾	Pr>χ ² after ⁽³⁾	Med bias before ⁽⁴⁾	Med bias after ⁽⁵⁾	% lost to CS ⁽⁶⁾
training	1384	waiting			0.0155	0.7474		2.19	0.0
ALU	2973	waiting			0.0096	0.1452		1.92	0.2
API	425	waiting			0.0403	0.9592		3.60	0.2
relief	652	waiting			0.0244	0.9829		2.69	0.0
replacement	483	waiting			0.0216	0.9679		3.34	0.0
subsidy	426	waiting			0.0296	0.9994		3.05	0.0
training	1384	work practice	3398	0.2417	0.0095	0.5772	6.15	1.48	0.0
		relief	652	0.2558	0.0344	0.0110	10.01	3.72	4.4
		replacement	483	0.4054	0.0407	0.0073	10.61	4.20	10.4
		subsidy	426	0.2888	0.0982	0.0000	10.11	4.93	1.7
work practice	3398	training	1384	0.2417	0.0296	0.0000	6.15	2.90	1.3
		relief	652	0.1547	0.0464	0.0000	8.34	4.49	1.8
		replacement	483	0.3606	0.1280	0.0000	10.16	6.09	0.0
		subsidy	426	0.1398	0.0225	0.0011	6.42	2.30	2.1
relief	652	training	1384	0.2558	0.0165	0.9577	10.01	2.23	1.5
		work practice	3398	0.1547	0.0047	1.0000	8.12	1.70	0.0
		replacement	483	0.5336	0.3079	0.0000	15.45	6.87	0.0
		subsidy	426	0.2677	0.0132	0.0000	9.50	5.89	0.0
replacement	483	training	1384	0.4054	0.0429	0.0011	10.45	4.24	9.73
		work practice	3398	0.3606	0.0192	0.0034	10.12	2.42	4.34
		relief	652	0.5336	0.4997	0.0000	15.12	8.93	0.0
		subsidy	426	0.5609	0.4800	0.0000	12.82	6.43	0.0
subsidy	426	training	1384	0.2888	0.0218	0.8349	10.12	2.86	0.5
		work practice	3398	0.1398	0.0045	1.0000	6.43	1.32	1.6
		relief	652	0.2677	0.0453	0.3931	9.50	3.38	0.0
		replacement	483	0.5609	0.4844	0.0000	12.81	4.68	0.0
ALU	2973	API	425	0.2523	0.0508	0.0000	11.12	4.00	27.9
API	425	ALU	2973	0.2523	0.0071	0.9995	11.18	1.29	1.4

Notes:

Work practice is API and ALU combined.

⁽¹⁾ Pseudo-R² from a Probit of D on X , giving an indication of how well the regressors X explain the participation probability.

⁽²⁾ Pseudo-R² from a Probit of D on X on the *matched* samples, to be compared to (1).

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

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

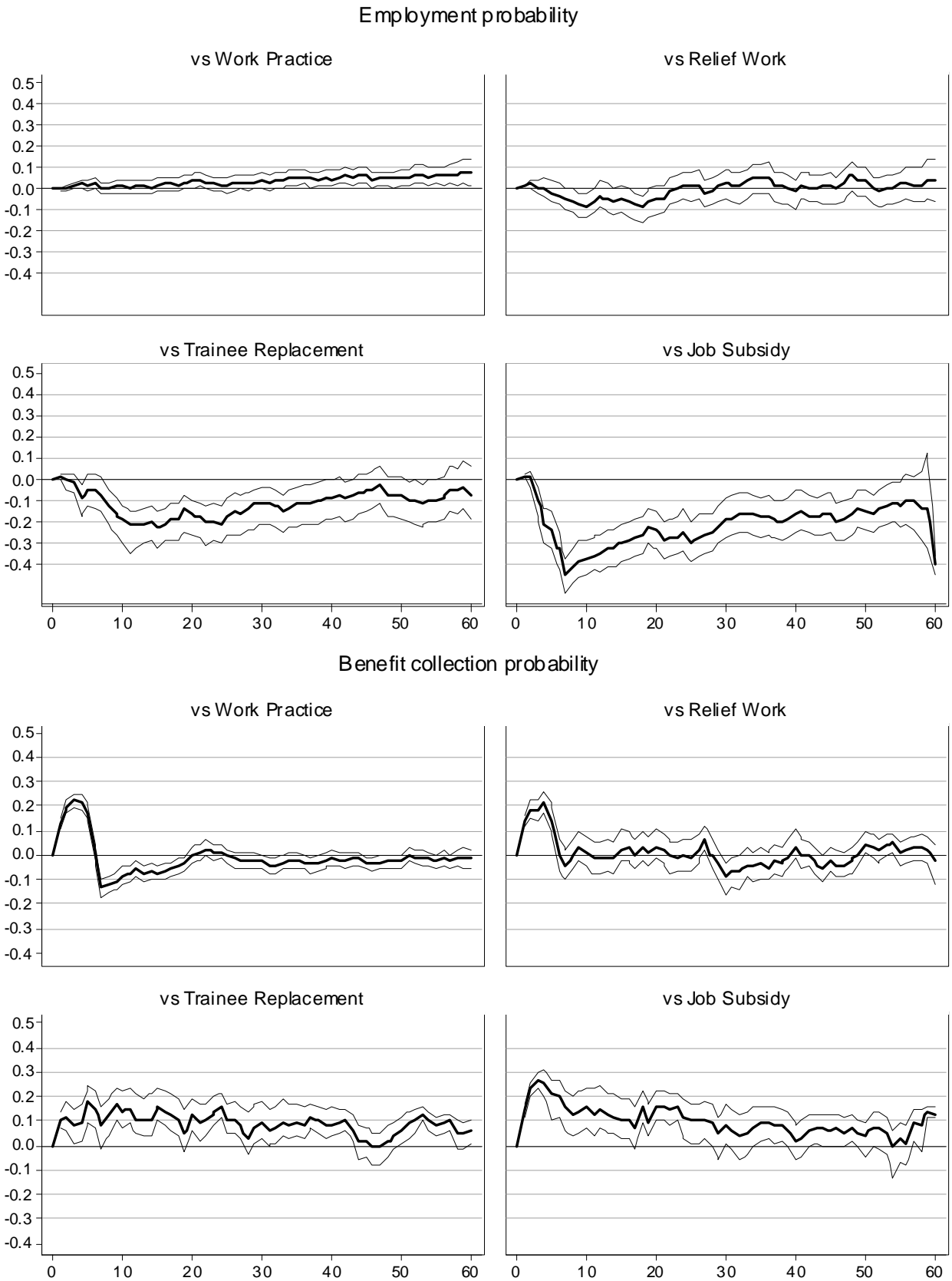
Following Rosenbaum and Rubin (1985), for a given covariate X , the standardised difference *before* matching is the difference of the sample means in the full treated and non-treated sub-samples as a percentage of the square root of the average of the sample variances in the full treated and non-treated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

$$B_{before}(X) \equiv 100 \cdot \frac{\bar{X}_1 - \bar{X}_0}{\sqrt{(V_1(X) + V_0(X))/2}} \quad B_{after}(X) \equiv 100 \cdot \frac{\bar{X}_{1M} - \bar{X}_{0M}}{\sqrt{(V_1(X) + V_0(X))/2}}$$

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

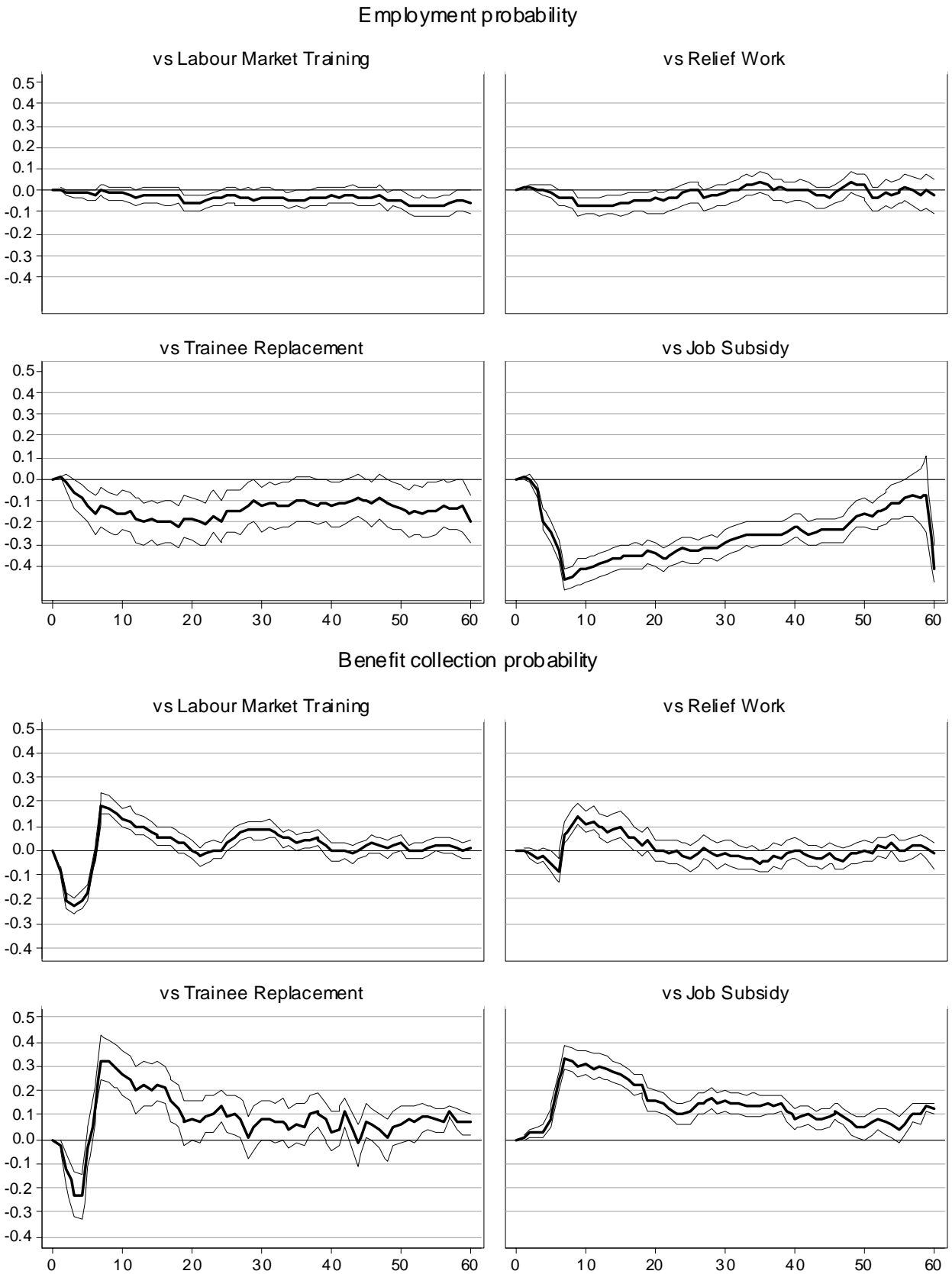
⁽⁶⁾ Percentage of the treated individuals falling outside of the common support.

Figure A1 LABOUR MARKET TRAINING: Differential treatment effects over time (% points).



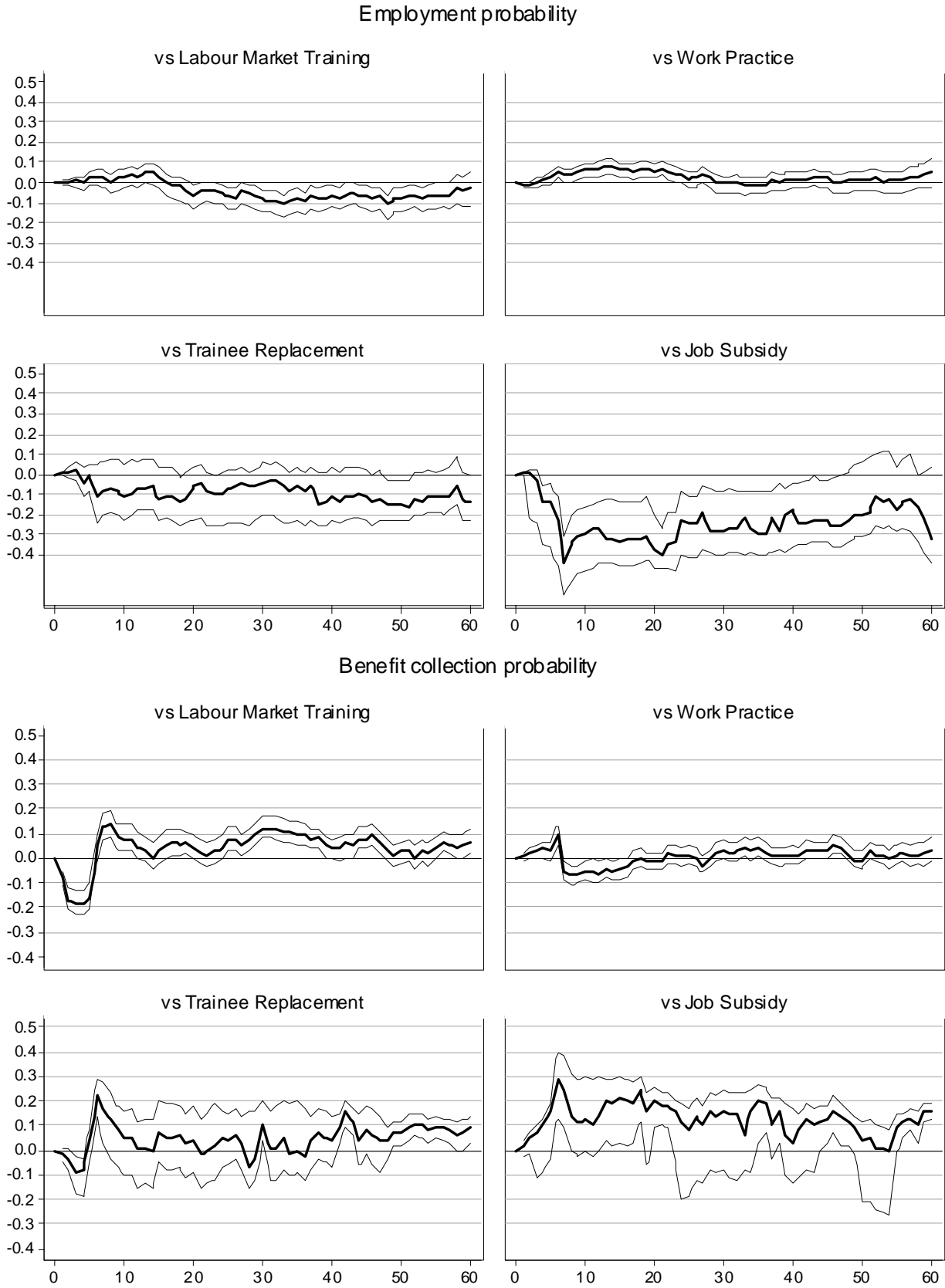
Notes: Time in months, from program start. 95% bias-corrected percentile bootstrapped confidence intervals (500 reps).

Figure A2 WORK PRACTICE : Differential treatment effects over time (% points).



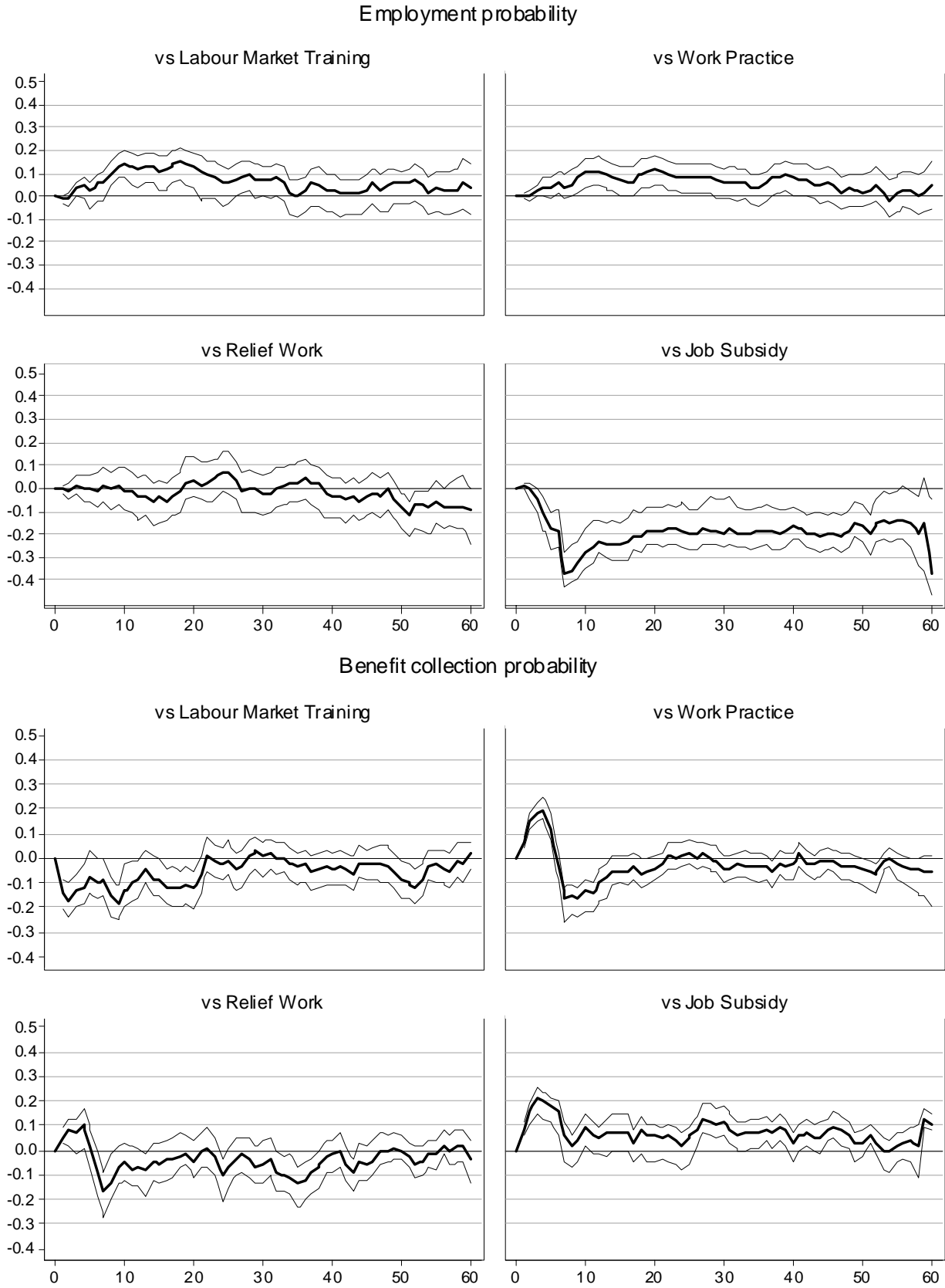
Notes: Time in months, from program start. 95% bias-corrected percentile bootstrapped confidence intervals (500 reps).

Figure A3 RELIEF WORK: Differential treatment effects over time (% points).



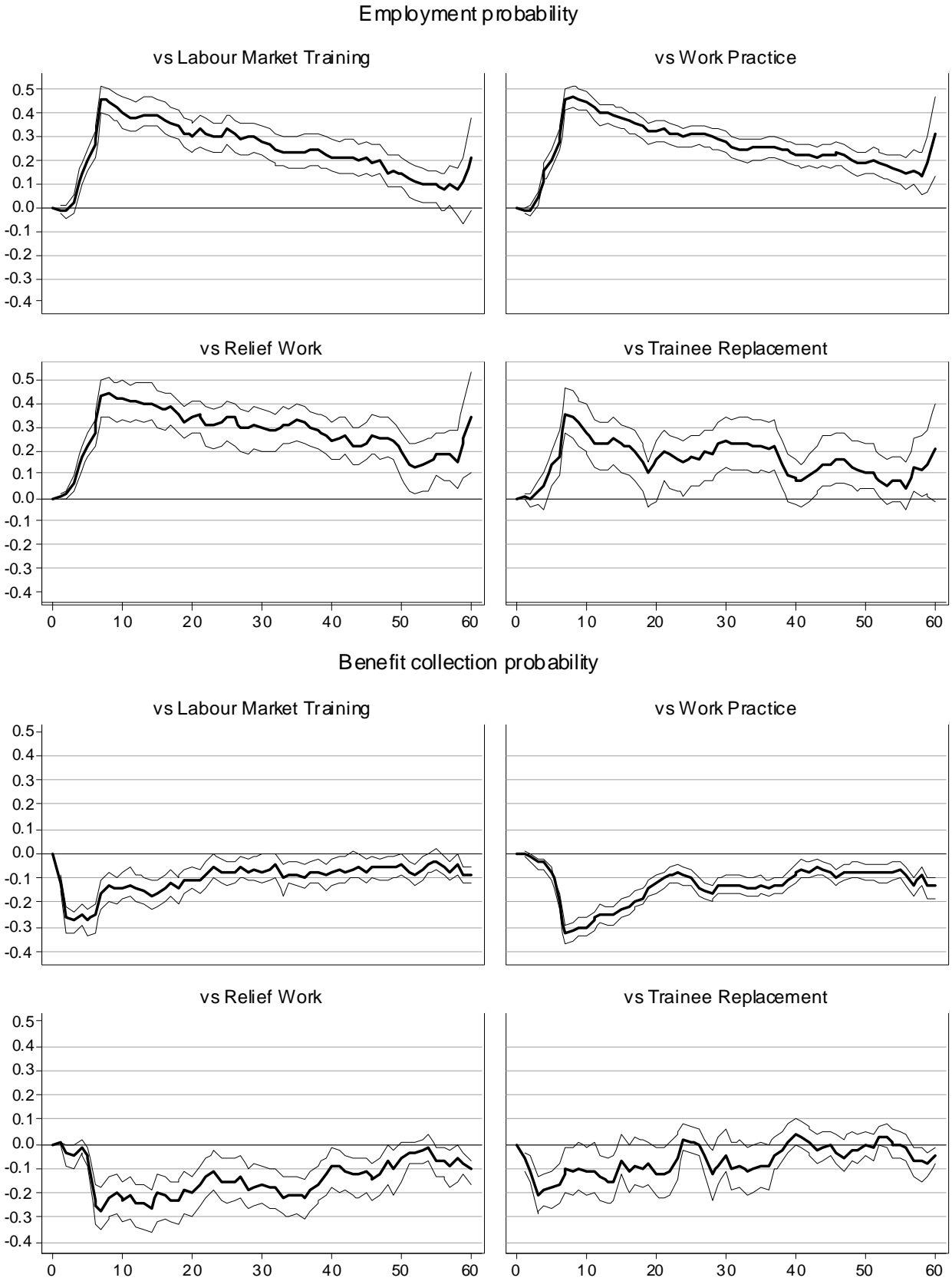
Notes: Time in months, from program start. 95% bias-corrected percentile bootstrapped confidence intervals (500 reps).

Figure A4 TRAINEE REPLACEMENT: Differential treatment effects over time (% points).



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

Figure A5 JOB SUBSIDIES: Differential treatment effects over time (% points).



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

Figure A6 Treatment effects on employment probability over time of joining a given program compared to waiting longer in open unemployment: Estimated effects and best- and worst- case bounds

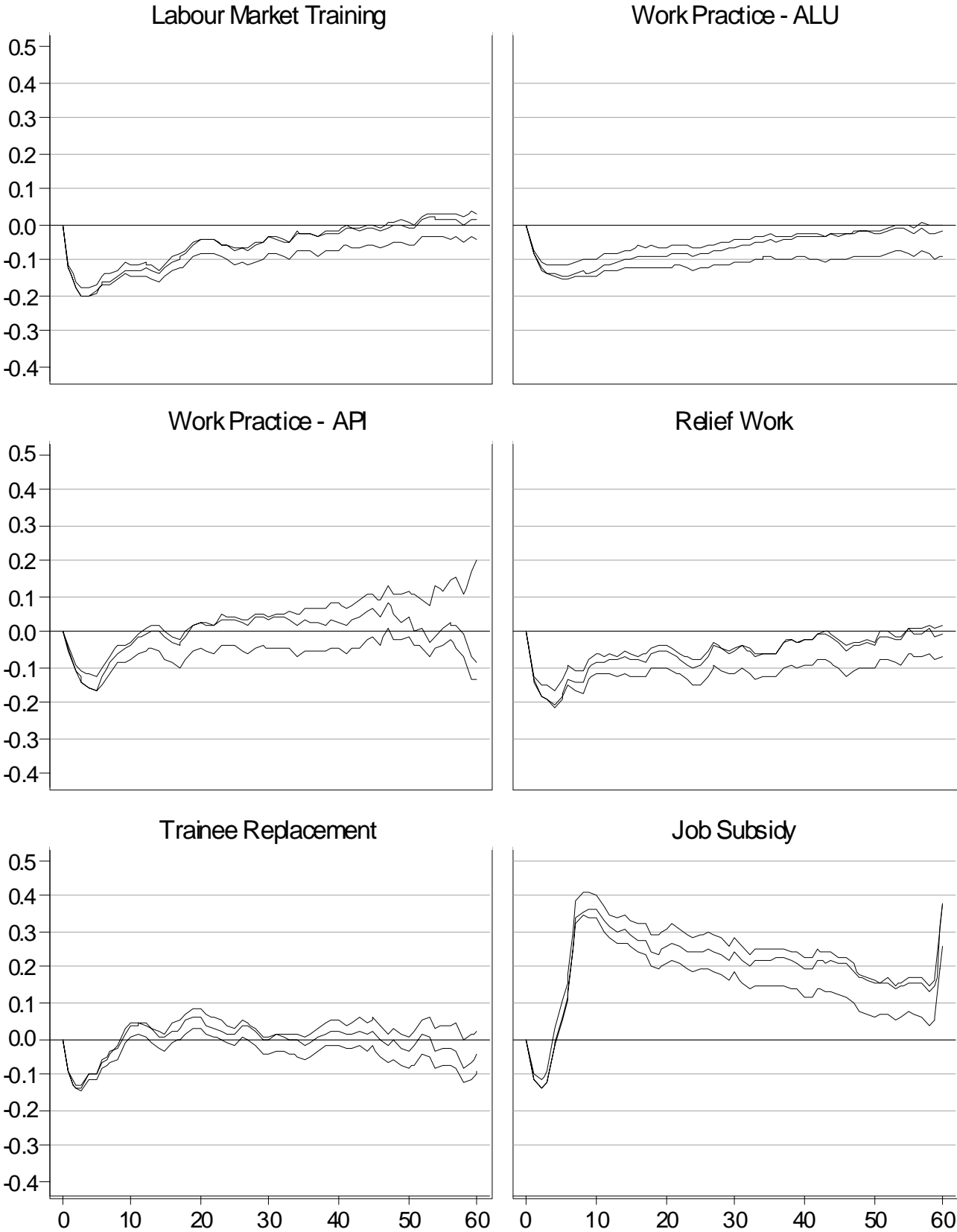
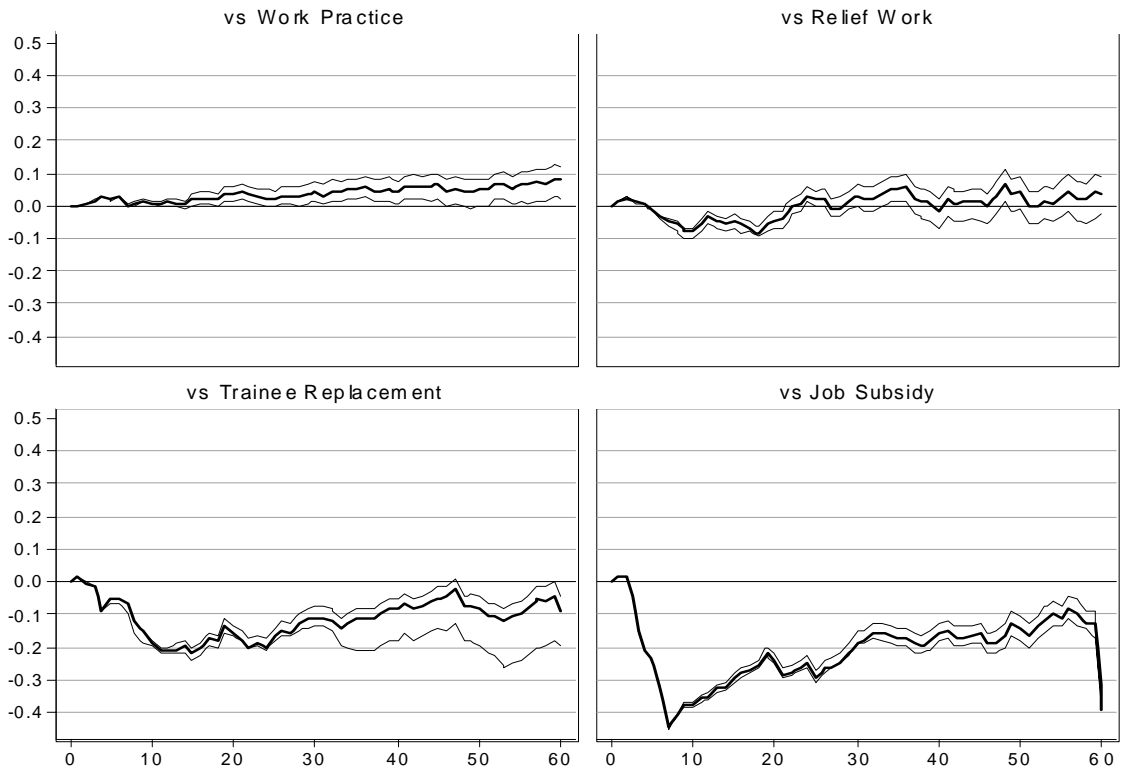
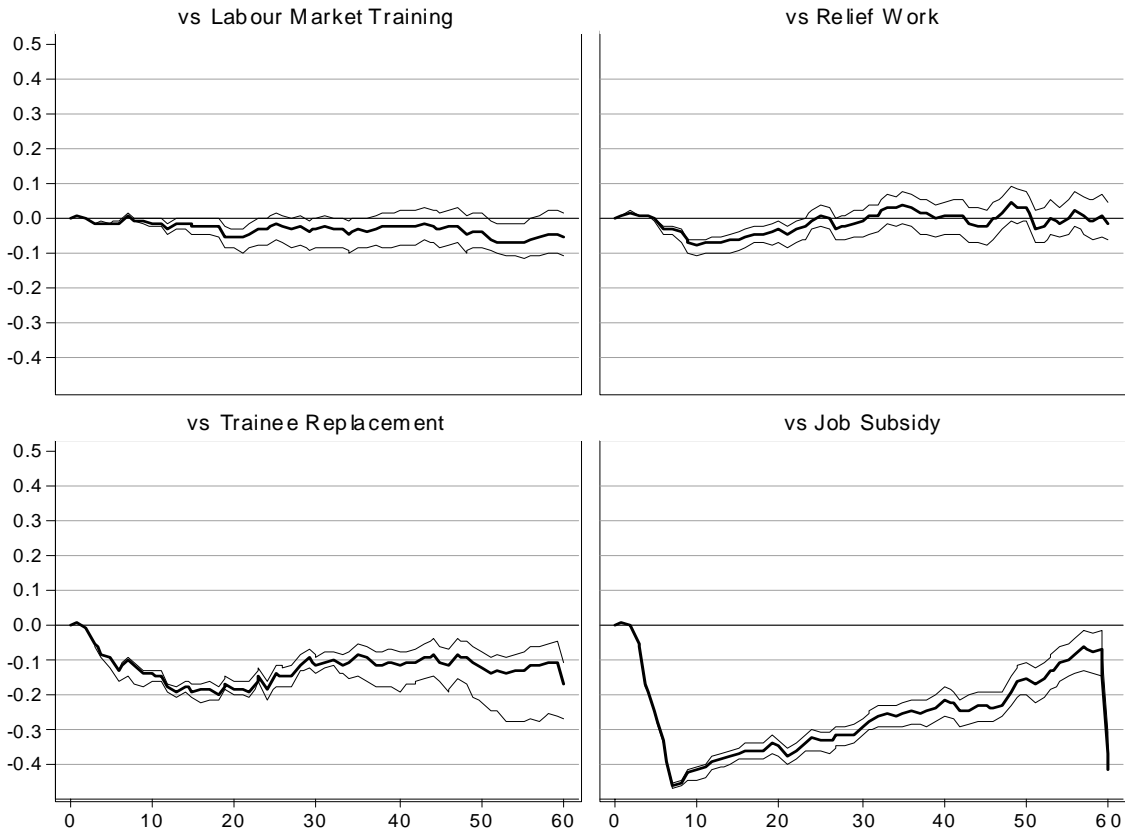


Figure A7 Differential treatment effects on employment probability over time:
Estimated effects and best- and worst- case bounds

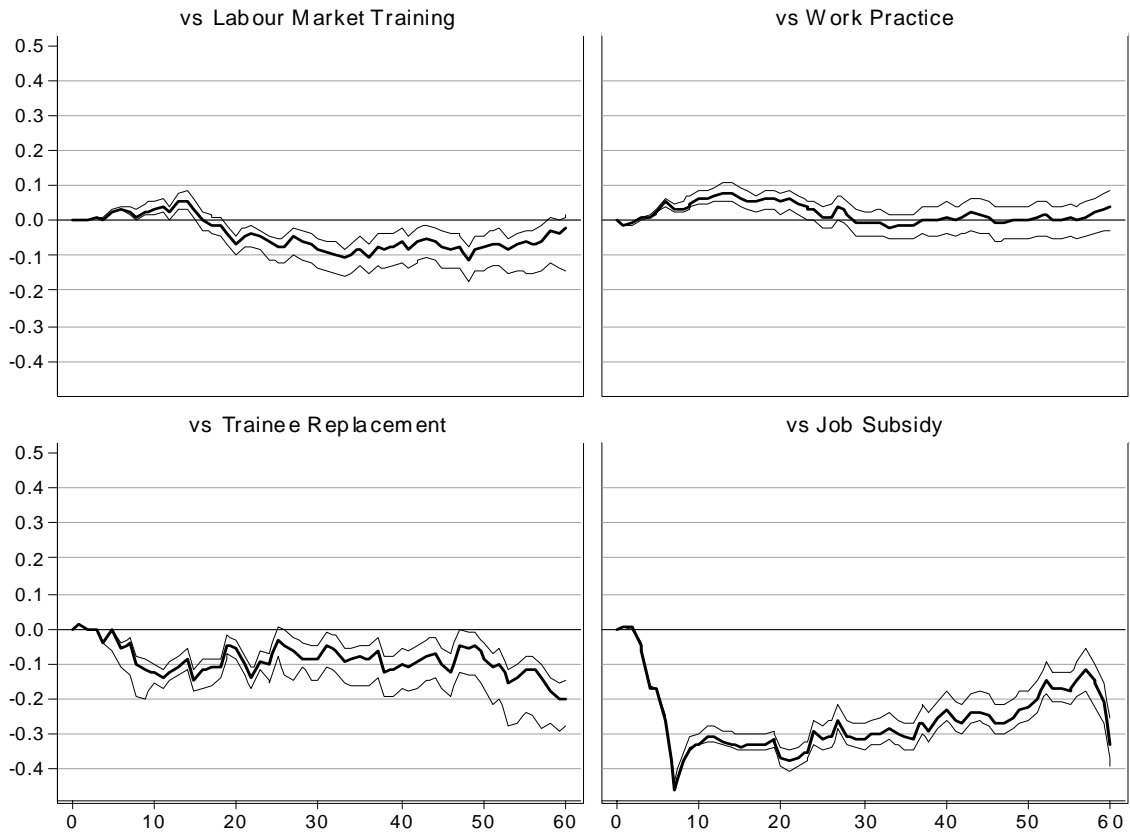
Labour Market Training



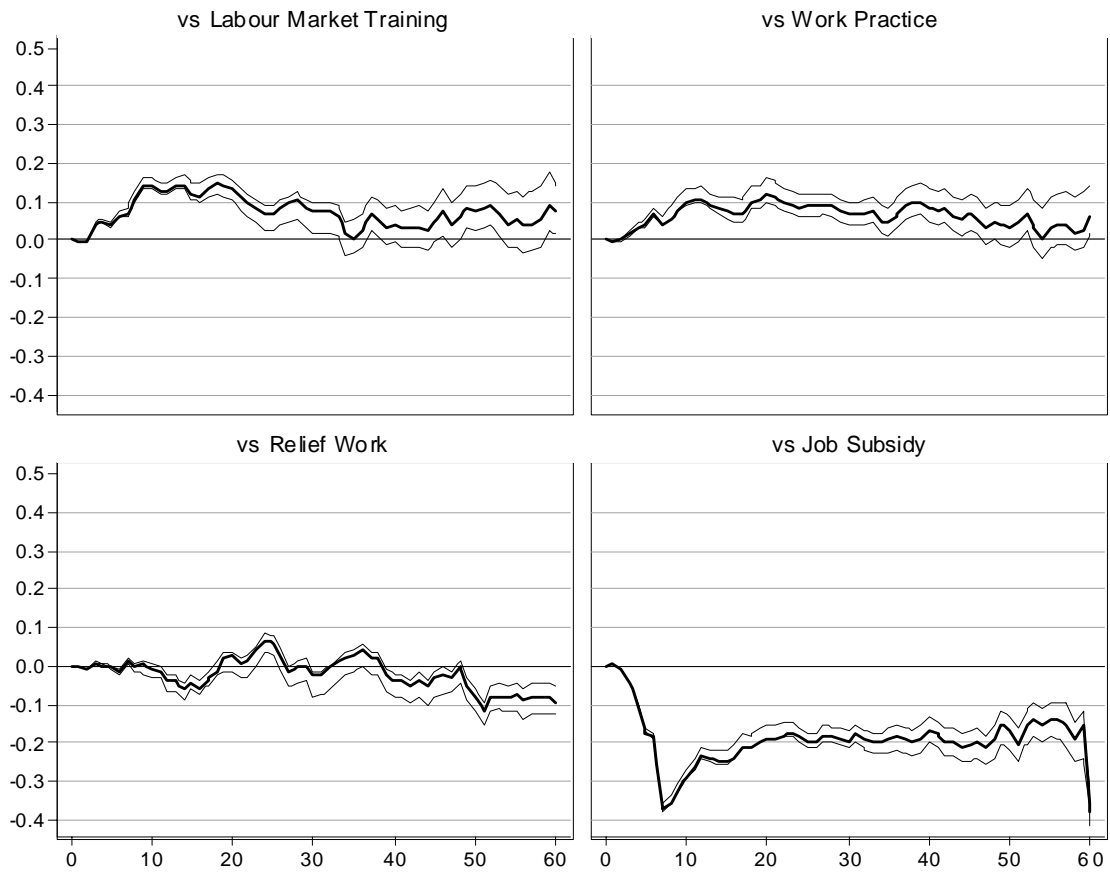
Work Practice



Relief Work



Trainee Replacement



Job Subsidies

