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AN EVALUATION OF THE SWEDISH SYSTEM OF ACTIVE LABOUR MARKET PROGRAMMES IN THE 1990S

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Abstract: We investigate the presence of short- and long-term effects from joining a Swedish labour market program vis-à-vis more intense job search in open unemployment. Overall, the impact of the program system is found to have been mixed. Joining a program has increased employment rates among participants, a result robust to a misclassification problem in the data. On the other hand it has also allowed participants to remain significantly longer on unemployment benefits and more generally in the unemployment system, this being particularly the case for those entitled individuals entering a program around the time of their unemployment benefits exhaustion.

Keywords: Active labour market programs, evaluation, matching, propensity score, treatment effects, unemployment compensation, bounds, missing data.

JEL classification: C14, J38, J65, J68.

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1. Introduction

To researchers and policymakers with an interest in active labour market programs Sweden offers a particularly appealing and potentially very informative set-up. The country has historically relied heavily on such measures[1], a feature which has been related by many observers (e.g. Layard, Nickell and Jackman, 1991) to the low unemployment rates it has traditionally enjoyed and which has thus often come to be regarded as a model for other countries.[2]

From a methodological and modelling point of view, the Swedish institutional framework raises some challenges not previously addressed in the typical US program evaluation literature. In the standard program evaluation specification, the program is administered at a fixed point in time, and individuals are either treated (i.e. participate in the program) or not treated (i.e. do not participate). In Sweden by contrast not only are the programs ongoing, but *any* unemployed individual can potentially become a participant. In fact, it may be argued that those who are not observed to go on a program have not been treated *because* they have waited long enough to enrol and found a job in the meantime. Choosing as the non-treated those observed to *de facto* never to participate in a program would in this context amount to conditioning on their future (successful) outcomes.

Although a non-standard one, this evaluation problem is quite commonly encountered in practice, in particular in the evaluation of ongoing programs which individuals sooner or later will join provided they are still eligible (e.g. still unemployed). In such situations the classical treated/non-treated distinction holds unambiguously only conditional on time spent in unemployment.

In this paper we do not follow a parametric, structural approach to simultaneously model the program participation decision and the outcomes of interest. Instead, we determine a meaningful evaluation question and propose a non-parametric way to address it, in particular in terms of the choice of a valid comparison group. To anticipate the discussion, the effects we estimate relate to the impact of joining a program at a given time in unemployment compared to not joining at least up to then.[3] The comparison group thus comprises those individuals who are unemployed up until that time and do not participate in a program as yet.

Given that this definition of the comparison group includes individuals who may participate in

[1] Some measures (labour market training and relief work) date back to the early '30s. To give an idea of the recent scale of the programs, the equivalent of 4.5% of the labour force participated on average in such measures (excluding those for the disabled) in 1997, with government expenditure representing over 3% of GNP.
[2] The UK 'New Deal' program introduced in April 1998 shares some of the features of the Swedish set-up.
[3] This is a distinct parameter from the impact of joining a program compared to never joining at all, or from the impact of joining a program at time t_1 rather than at t_2.

a program in the future, the effect we estimate is not appropriate for a cost-benefit analysis of the programs. Yet it is the relevant parameter from a behavioural point of view since it mirrors the relevant decision open to the job-seeker and the program administrator: to join a program at a given time or to wait at least a bit longer, in the hope of finding a job and in the knowledge that one can always join later. As such it can be considered one of the relevant parameters in an institutional framework where programs continue to operate and remain available to all those still unemployed.

In addition to the methodological issues raised by the institutional context, a feature that makes the Swedish case of particular interest is the availability of exceptionally rich and highly representative administrative data sources by international standards. In particular, the data allow us to identify a larger number of destination states than is generally possible. We can thus evaluate the programs in terms of a whole range of outcomes, forming quite a comprehensive picture of the impact of the program joining decision. The main stated objective of the Swedish programs is to improve the re-employability of the unemployed; the most crucial outcome is thus the probability of being employed over time, assessing the extent to which the program joining decision has endowed participants with skills and good working habits that enhance their employment prospects. Further routes out of unemployment are also evaluated, such as the probability of having gone back to regular education or of having left the labour force. Other important outcomes are those experienced within the unemployment system: repeated participation in subsequent programs, unemployment probability over time and most crucially the probability of being on unemployment benefits over time. In fact, a distinctive feature of the pre-2001 Swedish labour market policy is that participation in a program would renew job-seekers' eligibility to comparatively generous unemployment compensation, and was therefore likely to reinforce the work disincentives associated with the benefit system. In addition to the effects on unemployment benefits receipt, we also directly examine the extent to which participation provides incentives to remain within the unemployment system by alternating between program spells and compensated unemployment spells.

A second notable feature of the data is that we are able to follow up individuals for 5 to 6 years. We can thus capture both short and long-term effects in terms of all our outcomes, whereas often in the literature program effects are evaluated at a given – and arbitrary – point in time (e.g. on the last observation day or after a year).

Lastly, in addition to recording the duration of stay in a labour market state of all unemployed individuals, the data also includes a wide array of demographic, human capital and labour market variables, as well as the caseworker's time-varying subjective appraisal of various factors relating to the overall situation, character and needs of service of the job-seeker. The richness of the data has motivated the matching approach followed in this paper.

The next section describes the Swedish labour market policy and institutional set-up, and Section 3 the data and sample selection. Section 4 outlines the evaluation problem in the Swedish context, formalising the evaluation question to be addressed, describing the matching approach and arguing the plausibility of its identifying assumption. Section 5 presents the set of empirical results. The treatment effects for the various outcomes 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 distinct treated sub-groups who choose to join a program after different amounts of time spent in open unemployment. A set of sensitivity and bounds analyses is additionally performed 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 database. The section also devotes particular attention to exploring the linkages between treatment effects, timing of participation and entitlement status. This because given the institutional link between program participation and unemployment insurance eligibility and renewability, entitlement to unemployment benefits may not only play an important role in the timing of program participation, but it could also affect the size or even the sign of the various treatment effects. Section 6 concludes.

2. The Swedish labour market policy

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

The stated overall purpose of the labour market programs is to prevent long periods out of regular employment and to integrate unemployed and economically disadvantaged individuals into the labour force. There are various kinds of programs, ranging from labour market retraining to public sector employment such as relief work, to subsidised jobs, trainee replacement schemes, work experience schemes and job introduction projects, to programs for specific groups (the youth and the disabled), or self-employment and relocation grants. Most programs have a maximum duration of 6 months, though participants stay on average for 4 months.

It is worth pointing out that 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.[4]

Unemployment compensation is provided in two forms, the most important one being unemployment insurance (UI). UI benefits are relatively generous by international standards (daily compensation being 80% of the previous wage[5]) and are available for 60 calendar weeks, more than twice the maximum duration in the US. To be eligible to UI, an unemployed person registered at a public employment office and actively searching for a job must have been working for at least 5 months during the 12 months preceding the current unemployment spell.[6] 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 assistance is KAS, intended mainly for new entrants in the labour market who usually are not members of any UI fund. Daily benefits are significantly lower than UI (around half) and are paid out for 30 weeks. Eligibility depends on a work condition similar to the one for UI, which can however be replaced by the education condition of having finished at least one year of school in excess of the nine compulsory ones.

The passive and active components of the Swedish labour market policy used to be closely linked. A 5-month participation in any program would count as employment and thus allow individuals not only to become eligible for their first time (until 1996) but also to qualify for a renewed spell of unemployment compensation (until February 2001). Hence despite the fact that the period during which an unemployed job-seeker can receive unemployment benefits is fixed, it used to be possible to effectively extend it indefinitely by using program participation to renew eligibility. Program participation could thus actually reinforce the work disincentives associated with the benefit system, an important feature of the Swedish labour market policy which requires special consideration when assessing program effectiveness in the 1990s.

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[4] An example is the Gateway period of the UK New Deal program for the unemployed.
[5] This maximum level of compensation has changed a few times during the 1990s. The system also has a ceiling.
[6] There is also a membership condition, requiring payment of the (almost negligible) membership fees to the UI fund for at least 12 months prior to the claim.

3. Data and sample selection

The dataset used in the paper is the result of combining two main sources, which reflect the program component (Händel) and the benefit component (Akstat) of the labour market policy.

Händel is the unemployment register, of which the various databases contain information on *all* unemployed individuals registered at the public employment offices. This longitudinal event history dataset, maintained by the National labour Market Board (AMS) and available from 1991 onwards, provides each individual's labour market status information over time (e.g. unemployed, on a given program, temporarily employed), together with important personal 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 starting from 1994, originates from the unemployment insurance funds and provides additional information for those unemployed individuals who are entitled to UI or KAS, in particular on the amount and type of compensation paid out, previous wage and working hours.

The end result is a very large and representative[7] dataset, with information (to the day) about the duration of stay in a labour market state, an array of demographic and human capital variables and, for entitled individuals, additional information on type of entitlement, unemployment benefit recipiency and previous working conditions.

We focus on the inflow into unemployment in 1994, the year when the unprecedented recession that had hit the Swedish economy in the early 90s was at its most severe.[8] Additionally, we restrict our sample to individuals who became unemployed for their first time[9] in that year, aged 18 to 55 and with no occupational disabilities. These criteria lead to a sample of 116,130 individuals, followed from the moment they register in 1994 to the end of November 1999.[10]

Descriptive statistics for our sample at inflow into unemployment are presented in Appendix A.

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[7] Over 90% of the unemployed do register at an employment office (from a validation study by Statistics Sweden, quoted in Carling, Edin, Harkman and Holmlund, 1996, Footnote 7).

[8] From less than 3% in 1989 and 1990, unemployment jumped to 9% in 1992, reaching its peak of 13.5% in 1994.

[9] 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. Our sample is also relatively young (median age of 27). We can thus be reasonably confident that most of our individuals are indeed first-time unemployed.

[10] Following Carling, Holmlund and Vejsiu (2001) unemployment durations have been slightly adjusted in order to disregard short interruptions of the spells. Two adjacent unemployment spells separated by a short (≤7 days) break

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

4.1 Evaluation question

The Swedish institutional set-up poses a few interesting methodological issues which have to be resolved before deciding on the evaluation strategy. Object of the evaluation is a system with a wide array of different ongoing programs, which take place continuously over time and are open to all registered job-seekers; unemployed individuals in turn can be – and in fact often are – treated at different times during their observed unemployment history. In such a context crucial choices relate to the definition of the treatment of interest and of the comparison treatment.

Since this paper uses data on a sample of individuals who register as unemployed for their first time, the focus here is on the *first* treatment individuals may receive within their first unemployment experience, with any subsequent program participation being viewed as an outcome of that first treatment. Furthermore, the Swedish active labour market policy is considered in its totality: all the various programs are aggregated into one 'program', so that the 'treatment' is any program which a first-time unemployed can join. This is because the aim here is to analyse some aspects of the overall functioning of the Swedish unemployment system, a system comprising both a collection of different programs and a closely intertwined unemployment benefit component.[11]

As to the 'comparison treatment', one cannot simply choose a group who was never treated.[12] An unemployed individual will, in principle, join a program at some time, provided he remains unemployed long enough. In fact, bringing this reasoning to its limit, one could argue that the reason an unemployed individual has not been observed to go on a program is *because* he has found a job (before). In the Swedish institutional set-up the definition of non-participants cannot thus be the standard one, namely those individuals who are never observed 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.[13]

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, the key choice faced by the unemployed at any given moment is not whether to participate or not to participate *at all*, but whether to join a program now or not to par-

have been merged into one long spell. A similar adjustment has been made when an individual's first period of registration is a short non-unemployment spell immediately followed by an unemployment spell.

[11] Sianesi (2001a) disaggregates this treatment into its main components to look at their differential effectiveness.

[12] Cf. also Carling and Larsson (2000a, b).

ticipate *for now*, searching longer in open unemployment and knowing that one will always be able to join later on. Correspondingly we let the parameter of interest mirror the relevant choice open to the eligible and evaluate the average effect, for those observed to join a program after a given number of months spent in open unemployment, of joining when they did compared to waiting longer than they have. We now turn to the formalisation of this discussion.

4.2 Evaluation approach

To formalise the causal inference problem to be addressed[14], it is convenient to view U, elapsed unemployment duration since registration at the employment office, as discrete.

The eligibles, or population of interest, at time u are those still openly unemployed after u months. For the eligibles at u, treatment receipt is denoted by $D^{(u)}$, i.e. $D^{(u)}=1$ if joining a program at u and $D^{(u)}=0$ if not joining (at least up to) u. The comparison group for individuals joining at month u thus consists of all those with observed unemployment duration of at least u who chose not to join as yet.

The outcome of interest is individual labour market status over time, $\left\{Y_t^{(u)}\right\}_{t=u}^{T}$ (in our application T=60 months). The (u) superscript is a reminder that $Y_t^{(u)}$ is defined for $t=u, u+1, \ldots, T$ and possibly depends on treatment exposure at u. Correspondingly, let $Y_t^{1(u)}$ and $Y_t^{0(u)}$ denote potential labour market states at t ($t \geq u$) if joining a program in one's u^{th} month and if not joining any at least up until u months, respectively.[15]

For each u, interest lies in the time series of Δ_t^u, the average impact at time t, for those joining a program in their u^{th} month of unemployment, of joining at u compared to waiting longer in open unemployment:

$$\Delta_t^u \equiv E(Y_t^{1(u)} - Y_t^{0(u)} \mid D^{(u)}=1) = E(Y_t^{1(u)} \mid D^{(u)}=1) - E(Y_t^{0(u)} \mid D^{(u)}=1) \quad \text{for } t=u, u+1, \ldots T \qquad (1)$$

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[13] Very recent work by Fredriksson and Johansson (2003) formalises this intuition.

[14] Standard references for to the prototypical evaluation problem include the comprehensive work by Heckman, LaLonde and Smith (1999), as well as Heckman and Robb (1985), Heckman, Ichimura and Todd (1997, 1998), Heckman, Ichimura, Smith and Todd (1998) and Rosenbaum and Rubin (1983, 1985). For the potential outcome framework, the main references are Fisher (1935), Neyman (1935), Rubin (1974), Roy (1951) and Quandt (1972).

[15] Note that the stable unit-treatment value assumption has to be made (Rubin, 1980, Rubin, 1986, Holland, 1986), requiring in particular that an individual's potential outcomes depend only on his own participation, not on the treatment status of other individuals in the population (thus ruling out cross-effects or general equilibrium effects).

Since the observed duration of the program is endogenous[16], measurement of Δ_t^u starts at time u, the moment the treated join their program.[17] The treatment is thus starting a program (in a given month), also commonly referred to in the literature as the 'intention to treat'. Since the causal effect starts to work upon entering the program, any lock-in effect whilst on the program is viewed as a constituent part of the effect.

While the first term of (1) is identified in the data by $E(Y_t^{(u)} \mid D^{(u)}=1)$, some assumption needs to be invoked to identify the unobserved counterfactual $E(Y_t^{0(u)} \mid D^{(u)}=1)$. The conditional independence assumption (CIA) postulates that given a set of observed characteristics $X=x$, the (counterfactual) distribution of $Y_t^{0(u)}$ for individuals joining a program in their u^{th} month is the same as the (observed) distribution of $Y_t^{0(u)}$ for individuals deciding to wait longer than u: [18]

$$Y_t^{0(u)} \perp D^{(u)} \mid X=x \quad \text{for } t=u, u+1, \ldots \tag{2}$$

The required counterfactual is thus identified under (2):

$$\begin{aligned} E(Y_t^{0(u)} \mid D^{(u)}=1) &= E_X\left[E(Y_t^{0(u)} \mid X, D^{(u)}=1) \mid D^{(u)}=1\right] \\ &\overset{CIA}{=} E_X\left[E(Y_t^{0(u)} \mid X, D^{(u)}=0) \mid D^{(u)}=1\right] = E_X\left[E(Y_t^{(u)} \mid X, D^{(u)}=0) \mid D^{(u)}=1\right] \end{aligned}$$

In the last term the observed outcomes of the $D^{(u)}=0$ group are averaged with respect to the distribution of X in the $D^{(u)}=1$ group. For the matching procedure to have empirical content, it is thus also required that $P(D^{(u)}=1|X)<1$ over the set of X values where we seek to make a comparison, which guarantees that all individuals treated at u have a counterpart in the group of the non-treated at least up to u for each X of interest (the common support condition).

By focusing on the process of choosing and re-weighting observations within the common support, matching methods are able to eliminate two of the three potential sources of bias identified by Heckman, Ichimura, Smith and Todd (1998): the bias due to the difference in the supports of X in the treated and non-treated groups and the bias due to the difference between the two groups in

[16] Some programs require participants to continue job-searching activities. The offices too continue to search for them, since participants are still registered and requested to be 'at the labour market disposal'. Individuals are in fact required to drop out of a program if a 'suitable' job is found for them.
[17] This is similar to e.g. Ham, Eberwein and Lalonde (1997), who in addition to the impact of being assigned to the (experimental) training group also consider the impact of *entering* training.
[18] The weaker version in terms of conditional mean independence actually suffices.

the distribution of X over its common support. Like standard OLS regression[19], however, matching is based on the identifying CIA in (2), which assumes away the third potential source of bias, namely selection on unobservables. In our case (2) requires that, conditional on X and elapsed unemployment duration u, there is no unobserved heterogeneity left which affects both program joining decisions and subsequent labour market states. The CIA thus requires detailed knowledge of the factors that drive participation, as well as access to data suitable to capture those participation determinants that are likely to also affect outcomes. In this paper, the choice of a matching approach was motivated by the richness of the available background information (including not only several direct indicators of individual heterogeneity but also the results of a survey study directly asking job-seekers and caseworkers about their decision criteria), coupled with the growing emphasis in the literature on less parametric methods.[20]

The following discussion makes a case for the CIA to represent a credible approximation and thus for matching to be considered a feasible strategy for our informational and institutional setup.

4.2.1 PLAUSIBILITY OF THE MATCHING ASSUMPTION

Assumption (2) requires us to observe – so that we can match on – all those variables X that, conditional on having spent a given amount of time in unemployment u, influence *both* the decision to participate in a program at that time, $D^{(u)}$, as well as the potential labour market outcomes that would occur where such decision to be postponed further, $Y_t^{0(u)}$. Note that in our context, $Y_t^{0(u)}$ represents the possibility, compared to being unemployed, not only of finding a job at any time after u, but also of joining a program at any time after u. The outcome variable $Y_t^{(u)}$ can then be viewed as a set of exhaustive and mutually exclusive binary indicators of individual labour market status at evaluation time t, say employment (E), program participation (P) and unemployment (J): $Y_t^{(u)} \equiv \left(\mathrm{I}_{\mathrm{E}t}^{(u)}, \mathrm{I}_{\mathrm{P}t}^{(u)}, \mathrm{I}_{\mathrm{J}t}^{(u)}\right)$ with $\mathrm{I}_{\mathrm{E}t}^{(u)} + \mathrm{I}_{\mathrm{P}t}^{(u)} + \mathrm{I}_{\mathrm{J}t}^{(u)} = 1$. Potential outcomes can be viewed in a similar way.

The CIA in (2) thus translates into:

$$P(\mathrm{I}_{\mathrm{E}t}^{0(u)} \mid D^{(u)}{=}1, X{=}x) = P(\mathrm{I}_{\mathrm{E}t}^{(u)} \mid D^{(u)}{=}0, X{=}x) \quad \text{for } t \geq u \qquad \text{(2a) and}$$

$$P(\mathrm{I}_{\mathrm{P}t}^{0(u)} \mid D^{(u)}{=}1, X{=}x) = P(\mathrm{I}_{\mathrm{P}t}^{(u)} \mid D^{(u)}{=}0, X{=}x) \quad \text{for } t \geq u \qquad \text{(2b)}$$

[19] For the potential bias of OLS for the average effect of treatment on the treated, see Angrist (1998). For a detailed comparison of OLS, fully interacted OLS and matching and an in-depth illustration in an application to the returns to education problem, see Blundell, Dearden and Sianesi (2003).

What is required is thus that conditional on having reached the same unemployment duration and conditional on all the relevant information observed, the fact that an unemployed individual goes into a program in a given month while another waits longer is not correlated with the future labour market states the joining individual would have experienced had he instead not entered the program at that time. This ensures that the waiting individuals' (observed) probability distribution of subsequently finding a job or of later joining a program is the same as the (counterfactual) distribution for the observably-similar treated individuals had they decided to wait longer too.

The plausibility of this version of the CIA should be discussed in relation to the richness of the available dataset as well as the selection process into the Swedish programs. In our application the choice of the relevant conditioning variables X can in fact benefit from the results of a Swedish survey which directly asks job seekers and placement officers about their criteria in deciding about program participation (Harkman, 2000, as reported in Carling and Richardson, 2001). We can thus consider which participation-related factors are likely to also affect outcomes, and discuss how far we can capture or proxy these crucial variables.

From this work it appears that an unemployed individual's decision to participate in any program or not largely depends on the *individual's subjective likelihood of employment*. In so far as individual perceptions are accurate enough, this subjective assessment of one's employment prospects will reflect actual potential labour market outcomes $\mathrm{I}_{\mathrm{E}t}^{0(u)}$. It is thus crucial to identify enough information apt to capture these individual perceptions about one's employability. 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.

As to the past employment history:

- All of our individuals register at the unemployment office for their first time[21], so that their only unemployment experience relates to the present unemployment spell;
- Entitlement status controls for the degree of labour market attachment due to the work requirement UI-recipients have to fulfil;
- For entitled individuals, additional important individual attributes which characterise the worker's overall earlier labour market situation are previous normal working hours (a proxy of the extent of past labour market involvement) and the pre-unemployment wage (conditional on

[20] One alternative would be to resort to a parametric regression model simultaneously modelling the bivariate distribution of the program joining decision and the outcome of interest.

qualifications, a summary statistic of individual productivity).

As to the present employment prospects:

- An individual's perception of his employment likelihood will probably change over time spent in unemployment; elapsed unemployment duration should thus capture important unobservables in this dimension (e.g. perceived or actual deterioration of human capital, stigma effect, loss of hope or motivation, etc.). More generally, in the presence of duration dependence and/or unobserved heterogeneity, the outflow to employment will be different for individuals with durations less than u for reasons unrelated to the programs. It is thus crucial to ensure that the comparison individuals have spent in unemployment at least the time it took the participants to join. Also note that given some (albeit loose) regulations, as well as incentives related to unemployment benefits, elapsed unemployment duration is an important X variable for directly explaining the joining decision.[22]
- Demographic characteristics such as age, gender and citizenship, as well as the occupation being sought are also important determinants of labour market prospects.
- Part-time unemployment spells denote individuals who are still maintaining contact with the regular labour market and are probably both subject to less human capital depreciation and in a better position to look for a (full-time) job, by exploiting their bargaining position, additional contacts and references.
- Human capital information is available in terms of both specific and general education and occupation-specific experience. The latter is a subjective indicator of experience for the profession being sought (none, some, good), and seems particularly important since it results from both observed and unobserved differences between characteristics of individuals (cf. Ham and LaLonde, 1996). This indicator can be viewed as a summary statistics of the amount (as well as 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.
- Most crucially, we exploit several direct indicators of individual heterogeneity relevant in terms of employment prospects. Specifically, we have retrieved information relating to an

[21] At least since the beginning of the Händel dataset, in August 1991, see footnote 9.

[22] Some programs for instance formally require 4 months of open unemployment prior to enrolment, while approaching unemployment benefit exhaustion may make individuals more likely to enter a program.

overall evaluation by the caseworker of the situation, character and needs of service of the job-seeker. This assessment relates to the job-seeker's degree of job readiness (if judged to be able to take a job immediately, or to be in need of guidance, or to be difficult to place); as well as to the job-seeker's preferences, inclinations and urgency to find a job (if willing to move to another locality, if looking for a part-time job, if already having a part-time job).We also exploit a summary statistic directly capturing selection into the programs (if the job-seeker has been offered a program and is waiting for it to start). Note finally that the caseworker may update this subjective judgement during his client's unemployment spell, and that this time variation is captured and exploited in estimation.

Another way to view condition (2) is that individuals are myopic conditional on observables: given X, outcome-related information about the future ($t>u$) should play no role in individual decisions to join a program at a u or to else wait longer. Our 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 unemployed workers know that their former employer is going to call them back (e.g. they are seasonal workers, or have a credible agreement with their employer allowing the temporarily dismissed employee to collect unemployment benefits), they are likely to have no (or less) incentives to participate in the programs at any given month in unemployment; at the same time, they are observed to actually find employment. Additional observables included to control for potential anticipatory effects of this kind include the occupation/skill type of the job-seeker, as well as the month of registration, which should help capture seasonal unemployment.

More generally, though, (2a) 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, i.e. if $D^{(u)}=0$ because the individual knows that $\mathrm{I}^{0(u)}_{\mathrm{E}\tilde{t}}=1$ at some $\tilde{t} > u$. How serious this issue is going to be in our case thus largely depends on the typical time span between job offer and job commencement (and whether or not an individual who is going to start a job typically remains or is allowed to remain registered at the unemployment office in the meantime). Note also that if $\tilde{t}$ 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 program at a given time compared to later or never, thus requiring the CIA to also hold in terms of future program participation, (2b). Controlling for elapsed time spent in unemployment in conjunction with information regarding the entitlement status of an individual 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 the *caseworkers'* role in the program participation process, it appears that in Sweden they have quite a large degree of freedom.[23] We thus need to explicitly consider whether they act upon information which is unobserved to us and correlated with their clients' potential labour market outcomes. In addition to important characteristics of the job-seeker (in particular entitlement status for the 'work test', and educational qualifications for potential cream-skimming for training programs), we also observe the caseworkers' own subjective, synthetic and evolving evaluation of the overall situation and needs of service of their unemployed clients 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.

Again it is important to consider the possibility of anticipatory effects, this time in terms of future program participation; (2b) would be violated if $D^{(u)}$=0 because the individual knows that $\mathrm{I}_{\mathrm{P}\tilde{t}}^{0(u)}=1$ for some subsequent $\tilde{t}$. The institutional nature of the program system (a seemingly continuous flows of different programs often on an individual, *ad hoc* basis) should make it less likely for an unemployed job-seeker to have to turn down a program offer perceived as second-best in order to wait for a free slot on his first-choice program (this would also reduce the likelihood of an 'Ashenfelter dip' problem in terms of reduced job search prior to participation). Even if he did wait, though, he would not enter his first-best program with certainty, but would still be exposed to the possibility of finding a job or deciding (or be forced) to join another program in the meantime. 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

[23] From the survey by Harkman (2000) they in fact appear to be the driving force in the choice of the *type* of program. This information is exploited in the companion paper focussing on differential program impacts (Sianesi, 2001a).

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.

A final issue relates to the *local labour market conditions*, identified in the literature as a key variable to be controlled for (Heckman, Ichimura and Todd, 1997). In Sweden it would seem in fact very important to satisfy this requirement. County labour boards have the overall responsibility for the labour market policy in each respective county, and from the second half of the 90s municipalities have become increasingly involved in the decision-making as to labour market programs. This shift towards more decentralisation has given rise to new financial incentives (Lundin and Skedinger, 2000). In particular, municipal budgets may be favourably affected by moving unemployed individuals 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 job mobility among the unemployed. It is thus quite possible that counties or municipalities facing different labour market conditions may favour a different mix of program and unemployment policies.

In addition to county dummies, we have thus constructed the local 'program-rate', given by the number of participants in all 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) and is intended as a parsimonious way[24] to capture unobserved local aspects which are likely to be relevant for program joining decisions and individuals' potential labour market performance.[25]

In summary, the CIA in our case postulates that individuals unemployed for at least u months who are similar in terms of all the individual and local characteristics described, exogenously join a program at u rather than waiting longer than u. Matching leaves the source of random variation in this program joining decision unspecified. Sources of randomness could stem for instance from job-seekers' idiosyncratic preferences or random variation in their outlook on their employment prospects at a given time. On the placement officer's side, for given client characteristics, for given own judgement as to the job readiness of his client at a given time and for given employ-

[24] There are 289 municipalities and 484 employment offices in our data.

[25] The municipality program capacity at a given time may affect the possibility for a job-seeker to join a program at that time, while offices facing more unfavourable local conditions may be more active in placing individuals on programs (e.g. to lighten the burden on the municipal budget or to decrease the number of openly unemployed in the municipality).

ment office incentives regarding participation at that time, this randomness could be based on caseworkers' idiosyncratic preferences, incentives, experiences and propensity (and strictness) to apply the work test. One key point of the paper is that we can also exploit bottlenecks in the system, since we are able to condition on whether an individual has been offered and is waiting for a program, but cannot yet join (e.g. due to a lack of appropriate conditions related to the program, such as start dates of a training course, of a work-experience project, of an employee taking leave for a trainee replacement scheme, etc.).

4.2.2 IMPLEMENTATION

In concrete terms, the sample is stratified by (discretised) unemployment duration U = 1,2 …, U_{max}. In implementation, we set U_{max}=18, so that what we will be looking at is the impact of entering a program for groups of individuals that join within one and a half year of first registration; 94% of all treated are however observed to enter a program within such a time span.[26] Following this procedure also allows us to assess whether there is a differential program impact according to U, i.e. whether our treatment effect varies according to the time the individual has spent in unemployment before joining the program.[27] A very interesting group in this respect is the one observed to enter a program exactly at benefit exhaustion.

<u>Summarising the treatment effects</u>

In Section 5.3 we discuss the various treatment effects by month of placement Δ_t^u .

One may however wish to first have a synthetic overview of the general patterns of the various effects Δ_t^u . Since the treated group has in fact been divided into U_{max} exhaustive and mutually exclusive sub-groups (defined in terms of pre-program unemployment duration: $\{D=1\}$ = $\bigcup_{u=1}^{U_{max}} \{D^{(u)}=1\}$), it is algebraically possible to obtain an average of the various Δ_t^u's, weighted according to the observed month of placement distribution of the treated:

$$E_U(\Delta_t^u \mid D=1) = \sum_{u=1}^{U_{max}} E(Y_t^{1(u)} - Y_t^{0(u)} \mid D^{(u)}=1)\; P(D^{(u)}=1 \mid D=1) \tag{3}$$

Note that under the CIA in (2) for u=1, …, U_{max}, the causal effects pertain to the individual

[26] See Appendix B for the sample sizes of the two sub-groups by unemployment duration.

[27] Note that this amounts to assessing if the treatment effect for those who join a program after m months in unemployment is better or worse than the effect for the k^{th}-month joiners; not whether joining a program after m months leads these participants to experience better or worse outcomes than if they had joined after k months.

Δ_t^u's; averaging them into the 'overall' effect in (3) is done in Section 5.1 purely for reasons of presentational parsimony. As mentioned, Section 5.3 will then discuss deviations from these average patterns by placement time.

Propensity score matching

The conditional probability of being treated at u given the value of observed characteristics X, $P(D^{(u)}=1 \mid X) \equiv e(X; u)$, is the 'propensity score', a very useful variable when dealing with a highly dimensional X possibly including continuous covariates. As Rosenbaum and Rubin (1983) show, by definition treated and non-treated with the same value of the propensity score have the same distribution of the full vector X. It is thus sufficient to only match exactly on the propensity score to obtain the same probability distribution of X for treated and non-treated individuals in matched samples, so that if the CIA in (2) holds conditional on X, it will also hold conditional on $e(X; u)$.

A series of $U_{\max}=18$ probits has thus been estimated, each one modelling the probability of joining a program in month u, conditional on X and on having reached an unemployment duration of $u \in \{1, 2, \ldots 18\}$ months.[28] Time-varying variables are calculated in relation to the given unemployment duration u. Appendix B reports the estimates for five representative months.

Nearest neighbour matching on the propensity score has then been performed, always imposing a caliper of 1% to ensure common support. Overall, matching on the estimated propensity score balances the X variables in the matched samples extremely well (in fact better than the kernel versions we experimented with. See Appendix C for matching quality indicators). To adjust for the additional sources of variability introduced by the estimation of the propensity score as well as by the matching process itself, bootstrapped confidence intervals have been calculated.

5. Empirical findings

5.1 Outcomes over time

This section looks at various outcome measures over a 5-year period to investigate how unemployed individuals who join a program perform, on average, compared to a situation where they would have searched further in open unemployment. As stressed in 4.2.2, these 'overall' effects are just a way of calculating an average of the Δ_t^u's, meant to synthetically highlight the general trends and patterns in the treatment effects; the causal interpretation directly pertains to the treatment effects by month of placement, which will be separately considered in Section 5.3.

[28] This is equivalent to a discrete hazard model, with all the estimated parameters allowed to be duration-specific.

Table 5.1 summarises the outcomes considered and how they relate to one another.

Figure 5.1A depicts the treatment effect on the probability of program participation over time, starting at entry into the program and thus summarising both the (endogenous) duration of the program as well as possible repeated participation in subsequent programs. We find a relatively large and persistent effect: for 4 years since joining, participants are significantly more likely on average to be on a program than if they had further postponed their initial participation decision.

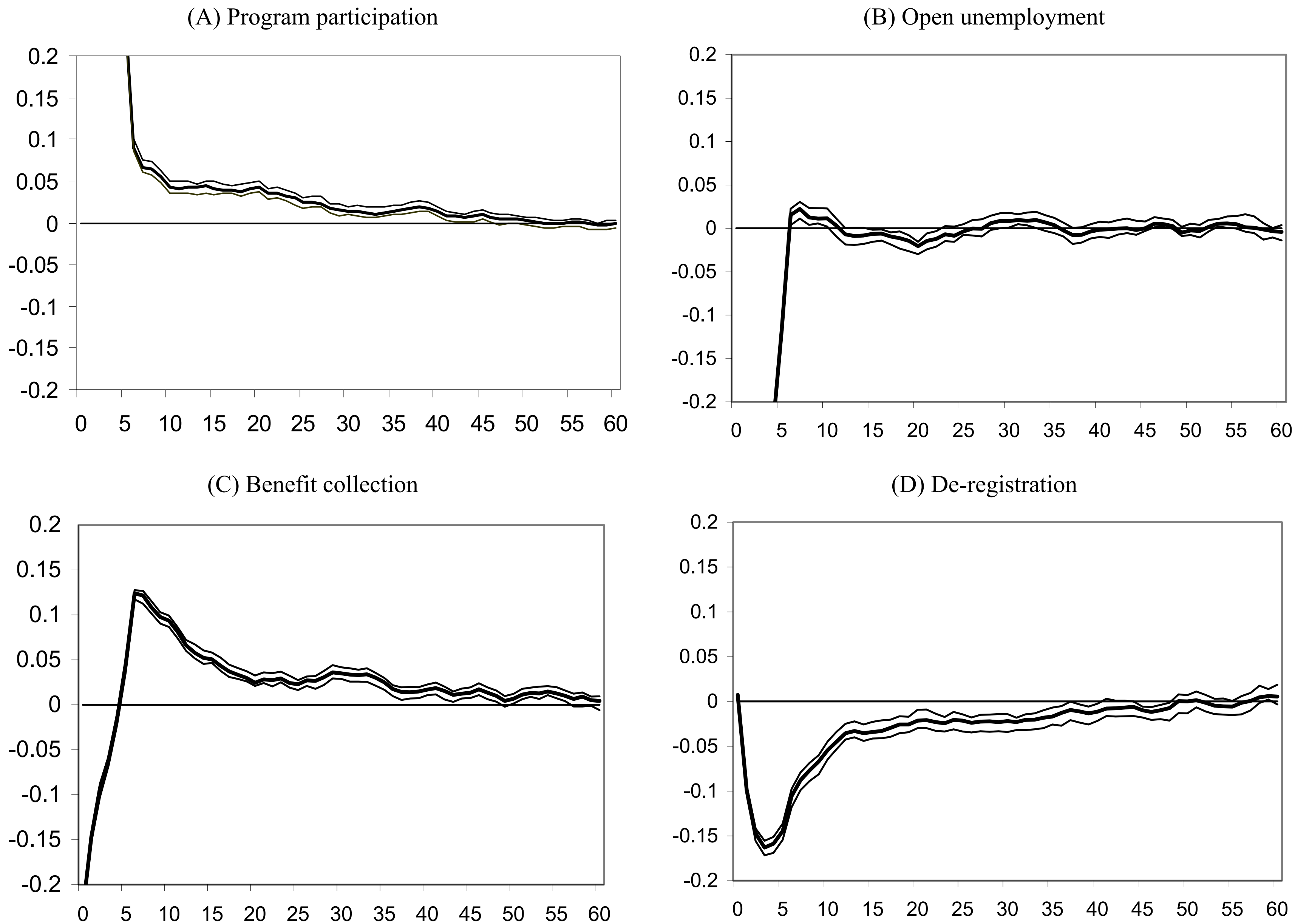
A serious indication about the influence of programs on subsequent labour market status is given by the unemployment probability, and in particular by the probability of being on unemployment benefits over time. While Figure 5.1B shows absolutely no treatment effect on the probability of being openly unemployed after the typical program duration, Figure 5.1C indicates that *as soon as* the program typically ends (i.e. after about 4 months), the negative effect (by construction, compensation while on programs is not counted as unemployment benefits) abruptly turns into a large positive one, with participants remaining sizeably and significantly more likely to be drawing benefits up to 3 years after having joined the program.

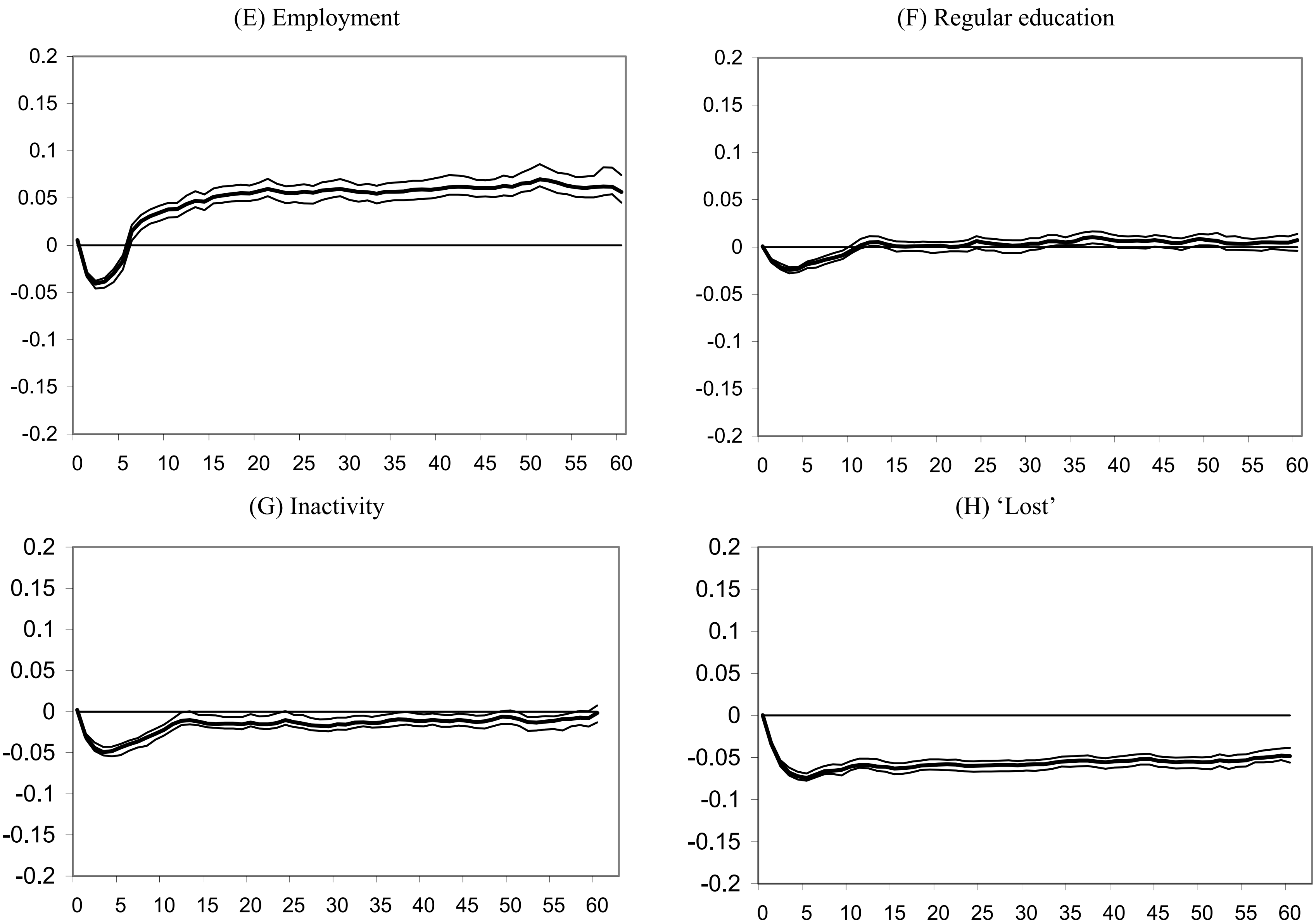
So far we have considered labour market states which are experienced within the unemployment system. The complement is the probability of not being registered at an employment office. This considers as a 'success' *all* the reasons for being de-registered: not only employment, but also being on regular education, having left the labour force or having been deregistered because of 'contact ended'. What we know about people being de-registered is that they are out of the official unemployment system and certainly not claiming benefits. When considering this type of outcome, programs do not seem to be beneficial; even though the initial sizeable negative lock-in effect is gradually reduced in size, the negative program effect persists up to the end of the 3rd year since program start (Figure 5.1D).

Table 5.1 Labour market states
(in brackets, the panels of Figure 5.1 where the corresponding treatment effects are shown)

- Registered at employment office
  - Openly unemployed (B)
    - Drawing unemployment benefits (C)
    - Uncompensated unemployment
  - On programme (A)
- Deregistered from employment office (D)
  - Employed (E)
  - Other exits
    - Regular education (F)
    - Out of the labour force (G)
    - Attrition ('lost') (H)

Figure 5.1 Treatment effect (% points) over time on the probability of





Notes: Time in month, with t=0 at program entry. See Appendix D for a table of results corresponding to Figure 5.1.
95 percent bias-corrected percentile bootstrapped confidence intervals (500 repetitions).

Both from the individual and social point of view, though, the key outcome when de-registered is the probability of being employed over time. Figure 5.1E shows that while on average joining a program initially reduces the chance of finding employment by up to 4 percentage points (the lock-in effect arising from reduced job search whilst on the program), when it typically ends it appears that participants perform significantly better than their (at least up to now) non-treated counterparts, displaying significantly higher and increasing employment shares over time. Over the first 5 years since program start, the treated seem to enjoy an average of 6% higher employment probability. Joining a program at some point thus seems to effectively reduce the expected overall time out of regular employment, on average.

How do these differing results in terms of de-registration and employment relate? To shed more light on this issue we need to look at the treatment effects on the remaining labour market states that make up the 'out of the unemployment system' one.

If programs enhance participants' human capital, they may find it easier to accumulate further human capital and may decide to deepen or specialise the acquired knowledge in the regular education system. Figure 5.1F however shows that beyond the initial negative lock-in impact, participants are no more likely to invest in further education than comparable individuals who have postponed their participation decision.

By contrast, joining seems to have a significantly negative effect on inactivity rates, which persists up to 5 years after the joining decision (Figure 5.1G). This is however a small treatment effect (around 1 percentage point), so that the suspicion arises that the divergent impact on employment rates and on de-registration rates may in fact be due to a negative impact on the last type of de-registration, the 'lost' status. In the following, 'lost' refers to an individual spell following de-registration, the reason of which has been recorded as 'contact ended'. This happens when a registered unemployed individual, having first missed an appointment at the official employment office, subsequently fails to contact the agency within a week. In fact, the negative program effect on 'lost' rates is decidedly large (Figure 5.1H).

The problem of the 'lost' individuals is a serious ones; in fact, it prevents us from fully observing the outcome of interest, that is the true labour market status these individuals find themselves in. We do not know which of these spells is in reality an employment spell the former unemployed did not report back to the agency, and which is by contrast still part of the preceding unemployment spell. Bring and Carling (2000) have traced back a sample of 'lost' individuals and found that around half of them had in fact found a job, which highlights how employment status may be critically under-reported in the available data. Since the large negative treatment effect on 'lost' rates would thus turn out to be in part a large negative effect on employment rates, our estimates of the employment effects in Figure 5.1E may be biased. Although likely to be upwards, the direction of the bias cannot be univocally established *a priori*, given that the probability of being in a lost spell over time, as well as the true status once in a lost spell (employed *versus* unofficially unemployed) may be systematically different between treated and non-treated individuals.

In conclusion, the robustness of the above evidence of a positive employment effect needs to be carefully checked against these lost spells.[29] We now turn to the results of various sensitivity, bounds and imputation analyses performed in this direction.

5.2 Accounting for a partially unobserved outcome variable

For simplicity of exposition, let us abstract from time and, initially, from the two groups. Y is an indicator variable for employment, L for the 'lost' state and D for treatment.

A simple sensitivity analysis without any additional external information looks at the estimated effects on employment rates under various assumptions about the percentage of 'lost' individuals who have in reality found a job. A misclassification rate of 0% would thus mean that the observed employment rates (thus the effect on employment rates in Figure 5.1E) are the true ones, while at the other extreme a 100% misclassification rate would imply that it is the sum of the observed employment rates and lost rates that represents the true employment rate. Note that this analysis assumes that the probability of being misclassified is the same for lost treated and lost controls, i.e. that outcome data Y are missing completely at random:

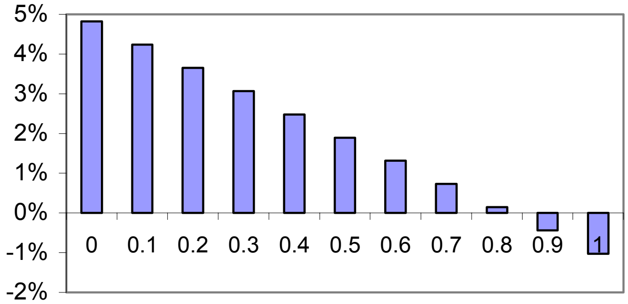
$$P(Y=1 \mid L=1, D=1) = P(Y=1 \mid L=1, D=0).$$

Figure 5.2 confirms that the observed average employment effect (4.8%) would in fact decline with more lost individuals having in reality found a job. With the almost 50% misclassification rate found in the survey by Bring and Carling (2000), it would be more than halved. Still, to have the effect disappear or change sign, one would need to assume that 80% or more of the lost individuals had in reality found a job.

A second step makes use of external information from the follow-up survey by Bring and Carling (2000) to impute to each 'lost' individual spell the probability of it in reality being an employment spell. The X's used by these authors do not however include previous program

[29] The presence of the lost individuals might also bias the estimates of the treatment effect on being unemployed, out of the labour force or on education. Outcomes conditional on the individual being registered at an employment office – i.e. program participation and benefit collection – are by contrast not affected. The focus in the following is on employment rates – the main stated objective of the Swedish programs and the only labour market status for which we have additional information from the follow-up survey.

Figure 5.2 Average treatment effects on employment probability
(averaged over the 5-year horizon since start of the program) by misclassification rate



participation.[30] We thus need to assume that the misclassification probability is independent of treatment status, this time however given observables X, i.e. that Y is missing at random:

$$P(Y=1 \mid X=x, L=1, D=1) = P(Y=1 \mid \mathrm{X=x}, L=1, D=0)$$

Using Bring and Carling (2000, Table 4) $\hat{\beta}$ coefficient estimates, the conditional probability of misclassification of a given lost individual with characteristics X is estimated by:

$$\hat{p}_i^Y \equiv \hat{P}\left(Y_i = 1 | L_i = 1, X = x_i\right) = \left(1 + e^{-\hat{\beta}' x_i}\right)^{-1}$$

Two alternative strategies are then pursued. We decide that a given lost individual has in reality found a job if his misclassification probability is larger than a given cutoff μ, that is if $\hat{p}_i^Y > \mu$ we consider that lost spell as an employment spell. The analysis of the treatment effect on employment probability is then performed as in Section 5.1 for various cutoffs μ.[31] Figure 5.3 – strikingly similar to Figure 5.2 – summarises the corresponding average employment effects; a positive effect does in fact persist up to a cutoff as low as 30%.

An alternative approach is to count a lost individual with an (estimated) misclassification probability $\hat{p}_i^Y$ as a ($\hat{p}_i^Y$)[th] of an employed individual. We can then estimate the employment rate (separately for the treated and the control group and at a given time period) as[32]:

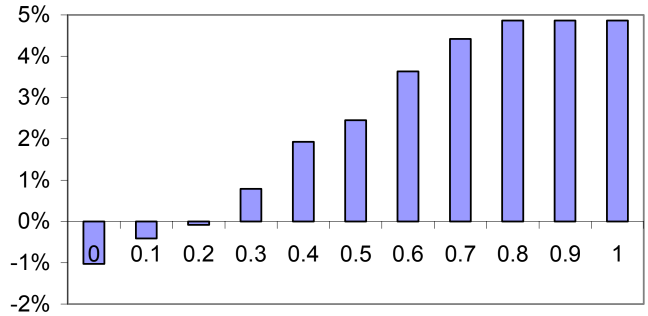
$$\hat{P}(Y=1) = N^{-1}\left(\sum\nolimits_{i\in\{L=0\}} Y_i + \sum\nolimits_{i\in\{L=1\}} \hat{p}_i^Y\right)$$

[30] Regressors include age group, gender, foreign status, human capital indicators (work experience, education), city region, and a few age-human capital interaction terms. Implicitly, we are also conditioning on non-entitlement: being registered is a prerequisite for drawing benefits, and in fact none of the lost spells in our data is characterised by unexpired eligibility.

[31] A cutoff of 0 corresponds to a 100% misclassification rate, while a cutoff of 1 to a 0% misclassification rate.

[32] Write the employment probability for a given group at a given time as $P(Y=1)=\Sigma_x P(Y=1|X=x)P(X=x)$. $P(X=x)$ can be estimated by $\#\{X=x\}/N$, where #{A} denotes the number of elements in set A and N is the total number of individuals in the group being considered. $P(Y=1| X=x)$ can be decomposed as: $P(Y=1|X=x,L=0)\ P(L=0|X=x) + P(Y=1|X=x,L=1)\ P(L=1|X=x)$. In our data we observe all terms except $P(Y=1|X=x,L=1)$, for which we use $\hat{p}_i^Y$, the estimated probability that a 'lost' individual with characteristics X has in reality found a job. $P(L=l \mid X=x)$ is estimated by $\#\{X=x, L=l\}/\#\{X=x\}$ for l=0,1; and $P(Y=1|X=x,L=0)$ by $\Sigma_{i\in\{X=x,L=0\}}\ Y_i\ /\ \#\{X=x,L=0\}$. Simplifying and integrating out the X's yields the formula in the main text.

Figure 5.3 Average employment effect by cut-off probability
(averaged over the 5-year horizon since start of the program)



where N is the total number of individuals (in the group and time period under consideration). The resulting treatment effect on employment over time is plotted in Figure 5.4 below. Even though visibly reduced from the observed one, joining a program seems to still have a long-lasting positive impact on employment rates over time, compared to similar individuals who have decided to wait longer.

In these last two types of analyses, we have used the imputed misclassification probability to estimate the employment probability of a lost individual irrespective of his treatment status – a regressor not included in the estimation by Bring and Carling (2000). This amounts to assuming that for a given set of X, the distribution of the probability that a lost individual has in reality found a job is the same in the treated and non-treated groups. In our case, treated individuals are those observed to enter a program, while all we know about non-treated individuals is that they not necessarily do so, making it not easy to argue if such an assumption is likely to be systematically violated, and if yes, in which direction. Still, since we are looking at outcome measures (probabilities) which are bounded, we can apply the core idea of the literature on non-parametric bounds in the presence of missing data to derive worst- and best-case bounds for the treatment effect on employment rates (e.g. Manski, 1990). The additional information from the survey is exploited to further tighten these bounds. Write the conditional misclassification probability of lost individuals with characteristics X, $P(Y=1 \mid X=x, L=1)$, as:

$$P(Y=1 \mid X=x,L=1,D=1)\, P(D=1 \mid X=x,L=1) + P(Y=1 \mid \mathrm{X}=x,L=1,D=0)\, [1-P(D=1 \mid X=x,L=1)]$$

For each lost individual, we know his treatment status D; his treatment probability given the lost status $P(D=1|X_i,L_i=1)\equiv e_i$[33]; and his misclassification probability $P(Y_i=1|X_i,L_i=1)\equiv \hat{p}_i^Y$.

[33] Due to the absence of a 'standard' D=0 group, the probability that a lost spell with characteristics X belongs to a treated as opposed to a 'non-treated' individual has been estimated separately by month of entry. In particular, for a given treated i, e_i is the estimated probability that a lost spell with characteristics X_i belongs to a treated individual as opposed to an individual who was still unemployed when treated i joined the program. An individual j who is used as control for a treated entering in month m_1 starts being evaluated from m_1 and if he has lost spells, the corresponding employment probability bounds are calculated using the probability that a lost spell

Hence we have the following equation in two unknowns:

$$\hat{p}_i^Y = P(Y{=}1 \mid X{=}x, L{=}1, D{=}1)\, e_i + P(Y{=}1 \mid \mathrm{X}{=}x, L{=}1, D{=}0)\cdot(1- e_i) \qquad (4)$$

The procedure to derive worst- and best-case bounds (where worst or best are from the point of view of treatment effectiveness) consists in letting a lost individual i of treatment status d_i count as a π_i-th of an employed individual, with $\pi_i \equiv P(Y_i{=}1 \mid X_i, L_i{=}1, D{=}d_i)$ obtained by setting $\overline{\pi}_i \equiv P(Y_i{=}1 \mid X_i, L_i{=}1, D{=}1{-}d_i)$ to its maximum or minimum, compatible with the given $\hat{p}_i^Y$ and e_i, as well as with all probabilities $P(\cdot)\in[0,1]$. So when calculating the best-case bounds, the probability of having in reality found a job which is assigned to a treated lost individual of characteristics X ($\pi \equiv P(Y{=}1|X,L{=}1,D{=}1)$) is the highest possible one obtained after setting $\overline{\pi}_i \equiv P(Y{=}1|X,L{=}1,D{=}0)$ in (4) to its minimum possible value given the constraints. Similarly, the probability that a non-treated lost individual of type X has in reality found a job is the lowest one obtained once setting $\overline{\pi}_i \equiv P(Y{=}1|X,L{=}1,D{=}1)$ in (4) to its maximum possible value given the constraints. And conversely when calculating worst-case bounds.

Table 5.2 displays the setting of $\overline{\pi}_i$ and the corresponding computation of π_i for the various cases, with the resulting bounds shown in Figure 5.4.

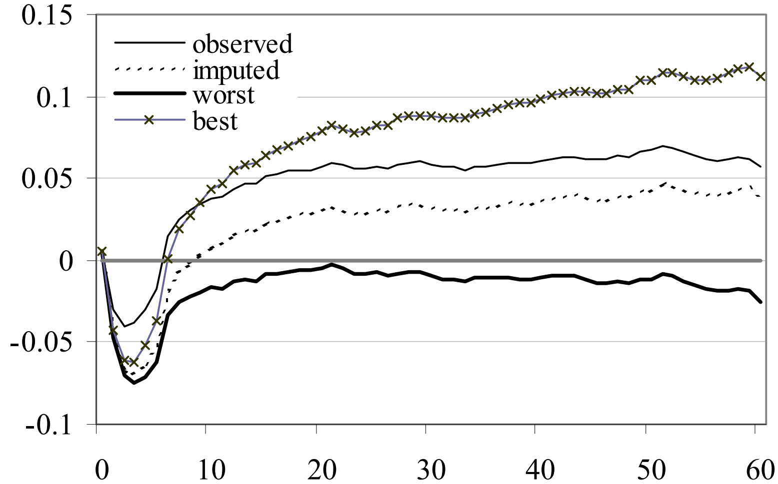
As expected, the treatment effect under the best-case scenario far surmounts the observed one. While the observed effect soon stabilises at around 6%, the favourable bound keeps rising, reaching double a level (12%) 5 years after program start. Quite interestingly, the upper

Table 5.2 Computation of π_i to derive worst- and best-case bounds

Worst-Case Scenario			Best-Case Scenario		
Treated			**Treated**		
assign the highest possible $\overline{\pi}_i$ compatible with $\hat{p}_i^Y$, e_i and $\pi_i \geq 0$			assign the lowest possible $\overline{\pi}_i$ compatible with $\hat{p}_i^Y$, e_i and $\pi_i \leq 1$		
If	Set $\overline{\pi}_i$ =	Thus π_i =	If	Set $\overline{\pi}_i$ =	Thus π_i =
$\hat{p}_i^Y \leq 1-e_i$	$\hat{p}_i^Y/(1-e_i)$	0	$\hat{p}_i^Y \geq e_i$	$(\hat{p}_i^Y-e_i)/(1-e_i)$	1
$\hat{p}_i^Y > 1-e_i$	1	$(\hat{p}_i^Y+e_i-1)/e_i$	$\hat{p}_i^Y < e_i$	0	$\hat{p}_i^Y/e_i$
Controls			**Controls**		
assign the lowest possible $\overline{\pi}_i$ compatible with $\hat{p}_i^Y$, e_i and $\pi_i \leq 1$			assign the highest possible $\overline{\pi}_i$ compatible with $\hat{p}_i^Y$, e_i and $\pi_i \geq 0$		
If	Set $\overline{\pi}_i$ =	Thus π_i =	If	Set $\overline{\pi}_i$ =	Thus π_i =
$\hat{p}_i^Y \geq 1-e_i$	$(\hat{p}_i^Y+e_i-1)/e_i$	1	$\hat{p}_i^Y \leq e_i$	$\hat{p}_i^Y/e_i$	0
$\hat{p}_i^Y < 1-e_i$	0	$\hat{p}_i^Y/(1-e_i)$	$\hat{p}_i^Y > e_i$	1	$(\hat{p}_i^Y-e_i)/(1-e_i)$

with his characteristics X_j belongs to an individual treated in month m_1 as opposed to an individual who was still unemployed after m_1 months.

Figure 5.4 Treatment effect on employment probability based on observed employment rates, imputed employment rates, worst-case and best-case bounds (% points)



Notes: Time in month, with t=0 at program entry.

bound on the employment effect is in fact always larger than the observed one in *absolute* size, entailing a larger lock-in effect during the first 5 months. Similarly, the figure confirms the expectation of a worst-case-bound treatment effect considerably lower than the observed one, with the former ranging between -3 and 0 percentage points after the lock-in phase.

The overall impression from the graph is that one may need to invoke assumptions particularly unfavourable to the treatment in order to have the treatment effect vanish or reverse sign.

The analyses in this section were meant to offer some qualitative evidence as to the robustness of the uncovered positive employment effect with respect to the problem of the lost individuals. Overall, the findings seem to indicate that the effect of participating in a program compared to postponing such a decision may remain positive under a variety of assumptions.

5.3 Treatment effects by month of placement and work-disincentives of the programs

Further interesting insights are gleaned when separately looking at the time series of the various treatment effects for different sub-groups of the treated based on the time they have spent in unemployment before being placed on a program. These are the causal effects that were previously summarised for convenience of presentation and discussion.

Table 5.3 reveals that for those individuals joining a program immediately (within their first month) or very late (in their 18th month) as well as around the time benefits expire (in their 15th month) the various treatment effects are considerably worse than those for individuals entering a program in intermediate periods (3rd-6th months).

Table 5.3 Average treatment effects by month of placement into the program (averaged over the 5-year horizon since the start of the program; % points)

Rates/Probabilities	Placement in u^{th} month:					
(% points)	1 to 18	1	3	6	15 *	18 *
On programs	7.7	7.0	7.0	9.3	9.5	12.0
	(7.5; 8.0)	(6.4; 7.7)	(6.3; 7.3)	(9.1; 9.9)	(7.7; 10.9)	(9.3; 14.7)
Open unemployment	-4.7	-5.0	-4.5	-5.7	-2.4	-4.3
	(-5.1; -4.2)	(-6.2; -4.0)	(-5.8; -3.8)	(-7.4; -4.1)	(-4.8; 1.2)	(-10.3; -0.2)
Benefit receipt	2.2	1.9	1.2	1.8	5.8	5.2
	(2.1; 2.6)	(1.3; 2.7)	(0.6; 1.5)	(0.5; 3.0)	(3.8; 9.2)	(1.3; 8.7)
Deregistered	-3.1	-2.0	-2.5	-3.6	-7.1	-7.7
	(-3.7; -2.6)	(-3.5; -0.6)	(-3.3; -1.5)	(-5.6; -2.1)	(-11.9; -3.6)	(-12.7; -0.5)
Employment:						
observed	4.7	2.5	4.8	5.7	2.9	4.1
	(4.1; 5.4)	(0.5; 4.2)	(2.8; 6.1)	(3.1; 8.0)	(-1.6; 5.1)	(-3.4; 8.7)
imputed	2.3	0.2	2.3	3.2	0.9	1.2
worst-case; best-case	-1.6; 7.3	-4.0; 6.0	-2.6; 8.5	-0.8; 8.0	-1.5; 3.7	-0.8; 4.5
Lost	-5.6	-5.4	-6.8	-5.4	-4.7	-5.6
	(-6.1; -5.2)	(-6.7; -3.9)	(-7.4; -5.5)	(-8.0; -3.9)	(-6.6; -2.3)	(-8.0; -1.6)
Inactivity	-1.6	0.9	-0.4	-3.4	-4.1	-3.8
	(-2.1; -1.1)	(-0.6; 2.4)	(-1.8; 1.0)	(-5.9; -1.9)	(-6.8; -1.4)	(-7.6; 0.5)
On education	0.1	0.3	0.3	-0.7	-1.8	1.5
	(-0.3; 0.4)	(-1.2; 1.0)	(-0.9; 1.4)	(-2.2; 0.5)	(-3.4; -1.4)	(0.1; 4.1)

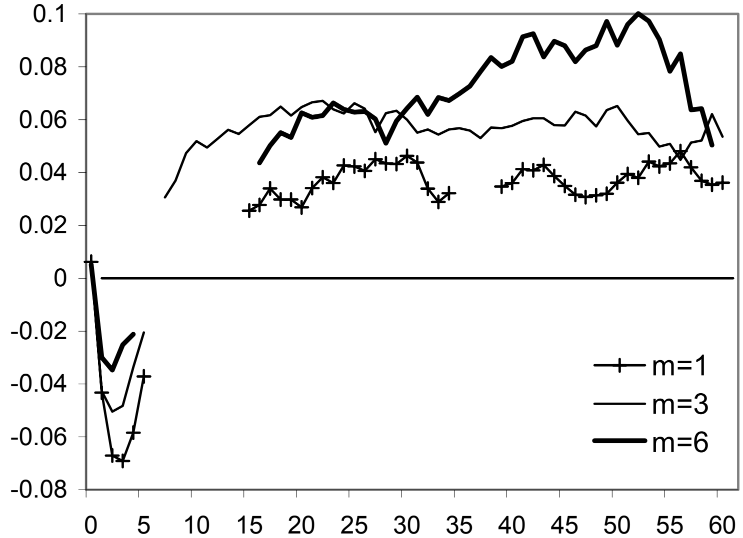
Notes: * averaged over 56 and 54 months respectively.
95% bias-corrected percentile bootstrapped confidence intervals (500 reps).

These general observations are confirmed also by the ordering of the point estimates of the temporal evolution of the employment effect by time of program entry (Figure 5.5). Not only do the medium- and long-term effects become increasingly better when moving from the 1st-month treated to the 3rd and then the 6th-month treated, but so does the negative initial lock-in effect change as well. By contrast for the joiners in months 15 and 18 (not shown), the employment effects after the lock-in are never significantly different from zero.

The differential effect for 'immediate' joiners may be explained by these individuals being possibly rushing the choice of the appropriate type of program as well as locking themselves too soon, thus foregoing initial job offers.

As to individuals entering a program at the time of benefit exhaustion, a likely explanation is that by renewing eligibility to compensation, program participation could end up strengthening the work disincentives associated with UI. Previous Swedish evidence on the importance of issues relating to unemployment benefits, work disincentive effects and program/be-

Figure 5.5 Treatment effect (% points) on employment probability over time for participants joining after 1, 3 or 6 months



Notes: Time in month, with t=0 at program entry. Only point estimates significant at 95% are shown.

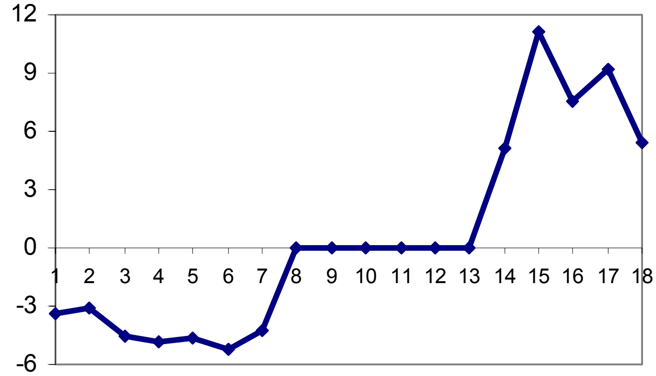
nefits cycling behaviour would in fact seem to overall support such a conjecture.[34] The remainder of the section is thus devoted to exploring the linkages between entitlement status on one hand, and timing of participation and especially treatment effects on the other.

Being entitled to UI significantly affects the incentives to join a program over time in unemployment. This is highlighted by the evolution of the marginal effect of UI status on participation probability over unemployment duration (Figure 5.6).[35] For up to the 8th month in unemployment, receiving benefits effectively discourages program participation. The effect of UI then becomes insignificant, while just around benefit exhaustion, individuals entitled to UI have an 11 percentage points higher likelihood of joining a program than observably identical non-entitled individuals.

Entitled individuals thus display a clear preference to join a programme only at benefit exhaustion, a time when they also enjoy preferential access (in the 90s those at risk of benefit exhaustion were guaranteed a place in a program). Although this may indicate that joining may often be done purely in order to escape benefit exhaustion, it could still be the case that

[34] E.g. Regnér (1997) provides some evidence that job-seekers may often enter labour market training just to renew benefits; Carling, Edin, Harkman and Holmlund (1996) show that UI-entitled individuals close to benefit exhaustion are significantly more likely to exit their unemployment spell to a program than those without unemployment compensation (cf. their Figure 3). Carling, Holmlund and Vejsiu (2001) find a significant and large negative UI effect on job finding rates. Ackum Agell, Björklund and Harkman (1995) find that prolonged spells of benefit-program periods are quite common in Sweden, while Hägglund (2000) detects a very interesting sensitivity of employment duration as well as time spent on a program to changes in the UI work requirement.

Figure 5.6 Marginal effect of UI-status on the probability of joining a program (percentage points difference in the treatment probability with respect to non-entitled with the same characteristics of UI individuals), by time unemployed prior to program



Note: Statistically insignificant effects are set to zero.

the programs manage to equip individuals with new skills and good working habits, quite independently of the motives that induced participants to join them in the first place.

This possibility does not seem to be supported by the overall evidence in Table 5.3, where the various treatment effects were found to be consistently among the worst for those entering a program after 15 months. However, it has to be noted that for individuals who are not entitled to unemployment benefits, month 15 is just like any other month. In order to explore this issue more directly, the treatment effects for 15th-month joiners have been calculated separately by entitlement status. The results of this exercise, shown in Figure 5.7, do in fact reveal a striking degree of impact heterogeneity.[36]

Since 76% of the those joining in month 15 are entitled, it is the treatment effect for the entitled sub-group that drives the overall effect.[37] For the entitled, the rather precisely estimated employment effect is never significant after the initial negative lock-in (Figure 5.7A), as was the case for the entire group. This is in sharp contrast to the overall positive (without even any lock-in effects), large (10-20 percentage points) and mostly significant effect on subsequent employment probability enjoyed by the non-entitled sub-group.

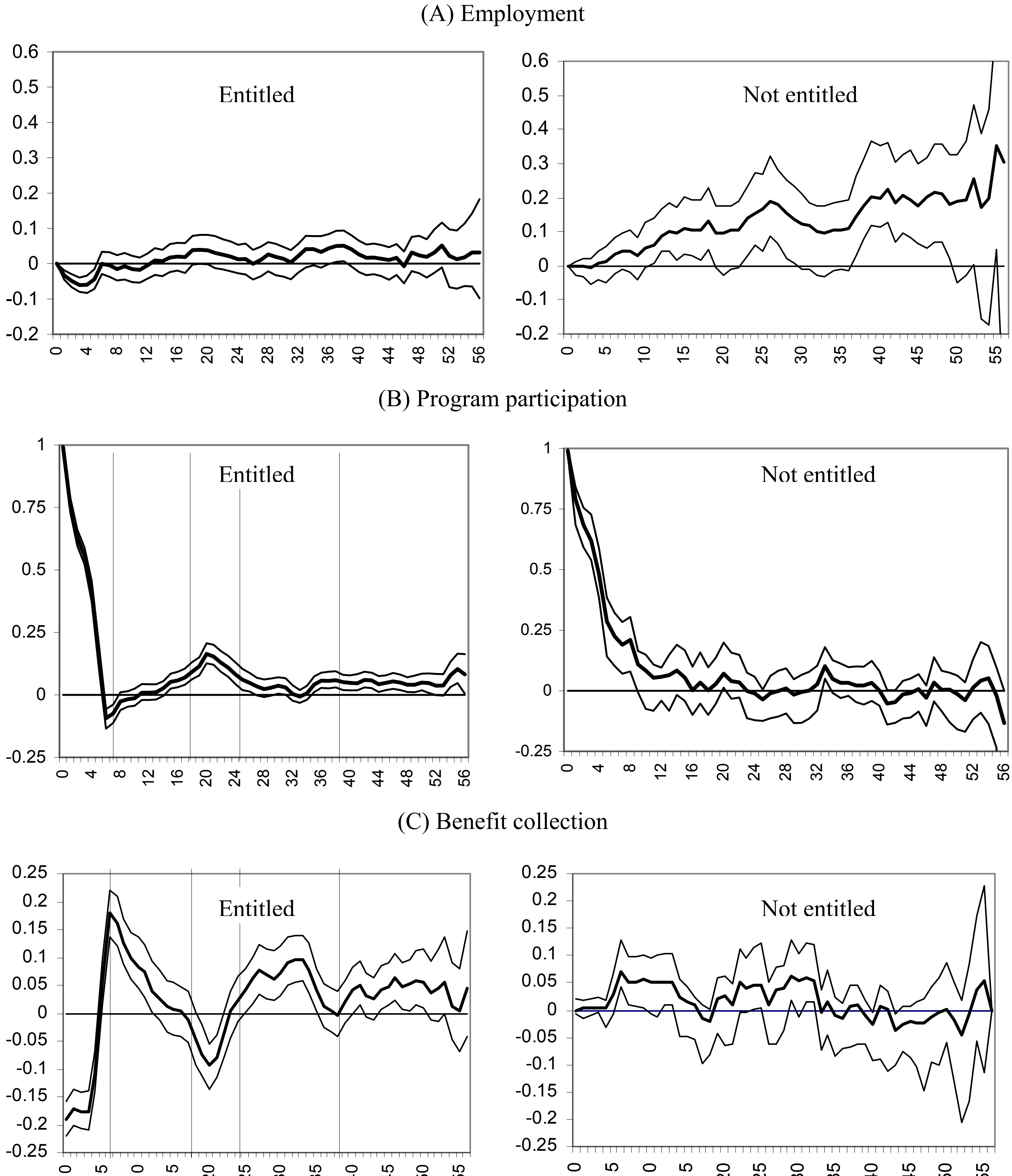
As to the treatment effect on program participation probability (Figure 5.7B), from 9 months after program entry onwards the non-entitled 15th-month joiners are no longer more likely to be on a program than their non-entitled counterparts who waited longer in open un-

[35] The marginal effect is the percentage points difference in the probability of entering a program in that month for individuals entitled to UI vis-à-vis non-entitled individuals with the same observed characteristics of UI-individuals.

[36] A similarly conspicuous heterogeneity by entitlement status was found for the 'overall' effects on employment probability by Sianesi (2001b).

[37] $E(Y_1 - Y_0|D=1) = E(Y_1 - Y_0|D=1, \text{entitled}) \cdot P(\text{entitled}|D=1) + E(Y_1 - Y_0|D=1, \text{not entitled}) \cdot P(\text{not entitled}|D=1)$.

Figure 5.7 Treatment effects over time (% points) for 15-month joiners by entitlement status



Time in month, t=0 at program entry; 95% bias-corrected percentile bootstrapped confidence intervals (500 reps).

employment. And for this sub-group, the only significant treatment effect on unemployment compensation probability is a tiny peak just after the usual program duration of 5-6 months (Figure 5.7C). Having joined a program makes them 5 percentage points more likely to be collecting benefits immediately afterwards than if they had not joined then (note that until 1996 program participation would allow one to become entitled for their first time).

As to entitled individuals joining a program around benefit exhaustion the distinct temporal pattern of the effect on program participation can be precisely mapped into the one of the effect on the probability of being collecting unemployment benefits (Figure 5.7B and C). From the moment they join the program to the benefit-renewing duration of 5 months, these individuals are significantly more likely to be still on the program, whilst considerably less likely to be collecting benefits than if they had not joined at (least up to) month 15. Quite uniquely to this sub-group, after the benefit-renewing 5 months on the program, these individuals become significantly *less* likely to be on a program than their matched counterparts. At exactly this time, the treatment effect on UI collection probability vertically jumps from -18 to +18 percentage points. This treatment effect then remains positive for around 14 months (the maximum period of compensated unemployment), after which the entitled treated become significantly less likely to be drawing benefits, while at the same time being 16 percentage points more likely to be on a program. A program which seems in fact to last long enough for these treated to then become 10 percentage points more likely to be drawing UI than their entitled counterparts who did not join a program at benefit exhaustion. This latter treatment effect lasts for another 14 months of maximum compensation, after which the entitled treated again display an 8 to 10% higher program participation probability.

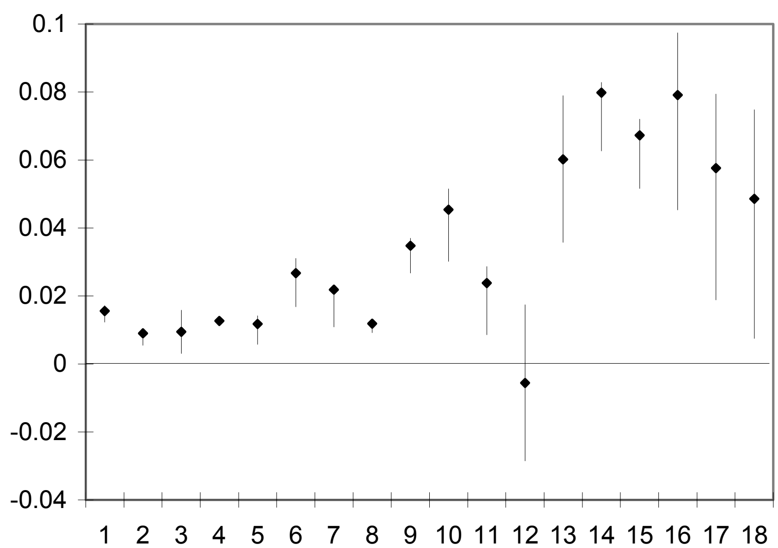
The linked patterns of these two treatment effects for those entitled individuals joining a program around benefit exhaustion would thus seem to be in large part explainable by 'cycling' behaviour. For a more explicit investigation, we propose the following working definition of a 'cycle'. An individual who registers (for his first time or anew) as unemployed (U^{new}), is then allowed to interrupt this spell by joining a program ($\hat{P}$) and to then resume it. However, if he then enters a new program, this is considered his first spell in a cycle. A *cycle* is then defined as the subsequent chain of alternating program (P) and unemployment (U) spells, in symbols $U^{new}\,\hat{P}\,U$**-P(..UP..)**, where the spells in bold denote the cycle. In the following we focus on a *compensated* cycle, defined as a cycle where in *each* unemployment spell, as well as in the one preceding the start of the cycle, the individual draws UI or KAS compensation (U^c), i.e. $U^{new}\,\hat{P}\,U^c$**-P(..**$\mathbf{U^c}$**P..)**.

Cycling itself may be considered a worrying phenomenon for a number of reasons; the fact that treated individuals keep going on various programs withouth exiting unemployment is clear evidence of a failure of the program system itself, while the importance of compensated cycling behaviour points to a likely failure in the way incentives are taken into account by the intertwined unemployment benefit-program institutional system.

Figure 5.8 shows the long-term (i.e. 48 months since program start) causal effect of joining a program on the compensated cycle probability by time of placement. By far the worst treatment effect is again displayed by those joining a program around benefit exhaustion (months 13 to 16). These groups have a 6-8 percentage points higher probability of being in the midst of a compensated cycle still 4 years since program entry than if they had not joined the program then. The corresponding figure for early joiners is just 1-1.5%.

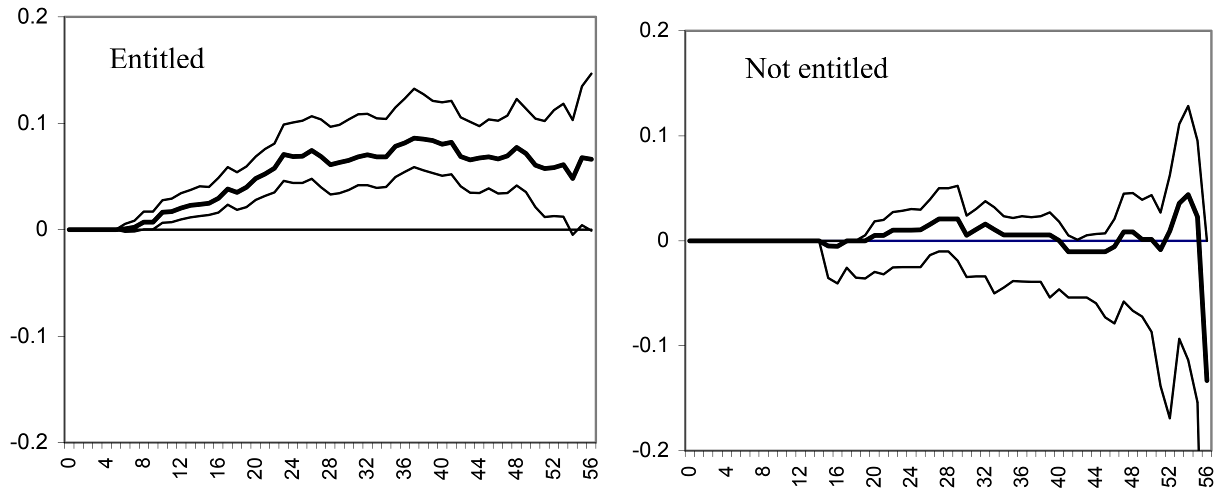
If we again focus on those joining at benefit exhaustion (month 15) and further break down the cycling treatment effect by entitlement, we find yet more confirmation of the crucial role that entitlement issues play in affecting the treatment effects. Figure 5.9 shows that the non-entitled treated are no more likely to keep alternating between compensated unemployment spells and subsequent program participation than if they had not joined the initial program. By contrast, entitled individuals joining at benefit exhaustion do in large part appear to view the program as an opportunity to renew their benefits and remain within the unemployment system; in the medium and long term they are 6-7 percentage points more likely to be in the midst of a compensated cycle than if they had not joined the initial program.

Figure 5.8 Treatment effect (% points) on compensated cycle probability 48 months since program entry, by month of placement



Notes: 95% bias-corrected percentile bootstrapped confidence intervals (500 reps).

Figure 5.9 Compensated cycle treatment effects for joiners in month 15 by entitlement status



Time in month since program entry. 95% bias-corrected percentile bootstrapped confidence intervals (500 reps).

Are treatment effects bound to be worse for entitled individuals? Not necessarily. A case in point are 6th-month joiners (among those with the best overall treatment effects, cf. Table 5.3).

As summarised in Table 5.4, compared to 15th-month joiners the ranking of the various effects by entitlement status is reversed, with entitled individuals enjoying either similar or more favourable treatment effects than non-entitled participants. In particular, there is no heterogeneity by entitlement in the treatment effects on employment and cycling, while the effect on overall time spent on programs is smaller for entitled than non-entitled individuals. Especially noteworthy are however the divergent effects on benefit collection probability.

Figure 5.10 shows that this large negative overall effect for entitled participants of -7.4 percentage points is driven not only by the short-term dynamics[38], but persists significant at around -5 percentage points for most of the medium- and long-term. This is in sharp contrast

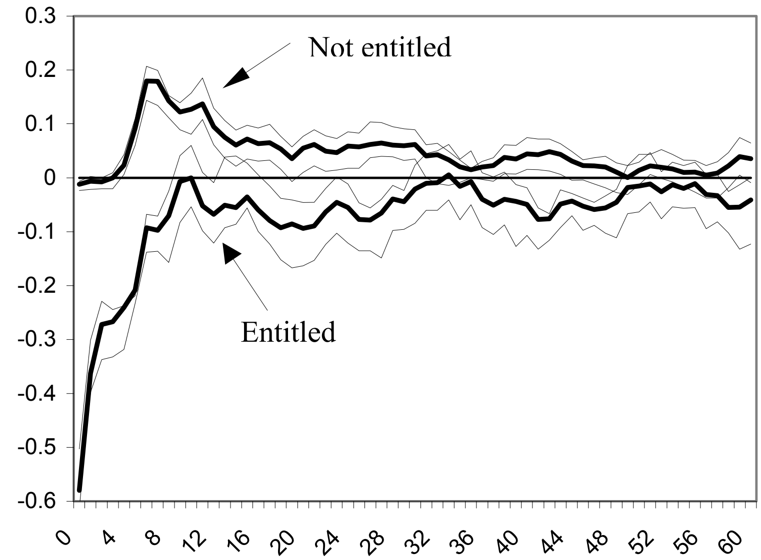
Table 5.4 Average treatment effects for 15th- and 6th-month joiners, by entitlement status (averaged over the 5-year horizon since the start of the program; % points)

	Month 15				Month 6			
	Entitled		Not entitled		Entitled		Not entitled	
Employed	1.3	(-2.0; 3.8)	13.1***	(5.2; 20.2)	7.0***	(2.8; 10.8)	6.6***	(5.3; 9.1)
On programs	10.2***	(9.0; 11.6)	9.2***	(3.4; 12.4)	6.4***	(4.7; 7.9)	9.3***	(7.5; 10.5)
Benefit receipt	2.2**	(0.4; 4.6)	1.6	(-2.2; 3.5)	-7.4***	(-11.2; -5.5)	4.7***	(3.3; 5.5)
Cycling	4.9***	(3.5; 7.0)	0.2	(-2.9; 1.0)	2.0**	(0.5; 3.7)	2.2***	(1.1; 3.0)

Notes: Month 15: averaged over 56 months, Month 6: over 60 months.
In brackets, 95% bias-corrected percentile bootstrapped confidence intervals (500 repetitions)
*** significant at 1%, ** at 5%, * at 10%.

[38] By construction, whilst on a program individuals receive compensation which is not classified as UI; at the same time, entitled treated would have been receiving UI if they had waited longer in open unemployment.

Figure 5.10 Treatment effects on compensated unemployment for joiners in month 6



Notes: Time in month, t=0 at program entry.
95% bias-corrected percentile bootstrapped confidence intervals (500 reps).

to the treatment effect for individuals who were not entitled upon joining a program after 6 months of unemployment. For them, exactly 5 months after program entry the treatment effect jumps from zero to a 17% higher probability of collecting benefits than if they had not joined then (this impact is in fact of the same size as the corresponding one for entitled individuals who joined at benefit exhaustion – cf. Figure 5.7C). For 6th-month joiners originally not entitled, one of the main effects from joining is thus in terms of becoming eligible to benefits. This group then remains significantly more likely to be in compensated unemployment for up to 3.5 years.

This section has shown how the various treatment effects may vary for the distinct groups who choose to join a program after different amounts of time spent in unemployment, and especially how these differential impacts are largely driven by the entitlement status of participants. Entitlement eligibility and renewability considerations are a most prominent driving force behind not only individual incentives to participate, but also and most crucially behind subsequent treatment effects.

6. Conclusions

The findings of this paper have highlighted how the most crucial issue as to the effectiveness of the Swedish program system in the 1990s seems to be the co-ordination and interaction between labour market programs and the unemployment insurance system. Up until 2001, a labour market program effectively came as a bundle of two conflicting components: intended to equip job-seekers with marketable skills to improve their opportunities on the labour market, it would at the same time allow them to renew eligibility to relatively generous unemployment compensation (and until 1996 even to become eligible for the first time). In order to display a positive effect, any productivity-enhancing component of the programs would thus have to be strong enough to outweigh the reinforced work disincentive associated with the entitlement renewability that participation allowed.

The results from the paper relate to how unemployed individuals joining a program perform, on average, compared to a hypothetical state where they would have waited longer in open unemployment. Overall, the impact appears to have been mixed, with evidence for both of the programs' components being at work. Unemployed individuals who go sooner on a program (compared to later or never) have a higher probability of being in employment from 6 months after joining the program for up to at least 5 years, an effect which seems quite robust to the misclassification problem of the 'lost' individuals. At the same time, there is visible evidence of the work disincentive element embedded in the institutional set-up of the programs: joining a program greatly increases the probability of being in benefit-compensated unemployment over time, of participating in further programs over time, and more generally of remaining within the unemployment system. When looking at the detailed mechanism, the positive effect on employment arises because the programs considerably reduce the probability of being unemployed *outside* the official unemployment system (and to a lesser extent of exiting the labour force). For unemployed job-seekers themselves it would seem that, on average, joining a program would pay: they enjoy higher employment rates, a much lower unemployment probability and when they do become unemployed, they are significantly more likely to be entitled to benefits.

Although these general patterns where found to be quite similar in terms of time spent in open unemployment before joining a program, some variation in treatment effects by month has been uncovered. In particular, for individuals entering a program around benefit exhaustion the various treatment effects are found to be among the worst than for any other group of treated. Further analyses disaggregating the impacts by entitlement status have highlighted how heterogeneity in the effects by time of placement is mostly driven by heterogeneity in the effects by entitlement status. Overall, incentives as to eligibility to and renewability of unemployment benefits seem to severely affect the various treatment effects from joining a program on subsequent labour market performance.

Note that since this analysis has lumped all the programs into one 'treatment', all the average effects discussed are implicitly averages also over program type, and thus relate to the actual participation mix among the different types of Swedish programs in the 1990s. Different programs may however have heterogeneous effects: while some may simply lock participants in rather useless and low-qualified tasks, others may indeed endow individuals with marketable transferable skills, whose return on the labour market may turn out to be large enough to outweigh the work disincentives created by the system. Sianesi (2001a), who applies the multiple-treatment matching framework recently developed by Imbens (2000) and Lechner (2001) to explore such a possibility, does indeed find considerable heterogeneity as to the effectiveness of the different measures.

A final caveat is that all these results rely on a non-parametric technique which assumes selection on observables. Despite the richness of the available dataset, the robustness of the conclusions obtained should be assessed by resorting to an alternative structural approach, explicitly modelling the sequence of choices facing unemployed workers and taking into account the endogeneity of selection into the different programs, which are intertwined with benefits eligibility and renewability.

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Appendix

A. Descriptive statistics of the sample at inflow into unemployment (*N*=116,130)
(%, unless otherwise stated)

Female	58.5	County:	
Age at entry (years)	28.7	Stockholm	22.4
Foreign citizen	19.5	Uppsala	3.9
Education:		Södermanland	2.8
Compulsory	20.5	Östergötland	5.0
Secondary	15.6	Jönköping	3.4
Secondary vocational	46.4	Kronoberg	1.9
University	17.5	Kalmar	2.5
Has education for job	59.1	Gotland	0.5
Experience for job:		Blekinge	1.7
None	28.1	Malmöhus	12.4
Some	22.3	Halland	2.8
A lot	43.1	Göteborg and Bohus	16.0
Missing information	6.5	Värmland	3.2
Entitlement:		Örebro	2.8
None	62.9	Västmanland	3.0
UI	32.5	Kopparberg	2.9
KAS	4.6	Gävleborg	2.8
Daily wage (SEK) (for entitled)	553	Västernorrland	2.8
Worked 20h/week (for entitled)	1.6	Jämtland	1.5
Worked 30h/week (for entitled)	3.7	Västerbotten	3.1
Worked 40h/week (for entitled)	80.0	Norrbotten	2.8
Sector:		Registration month:	
Professional, technical work	14.2	January	9.9
Health, nursing, social work	13.9	February	6.8
Admin, managerial, clerical work	12.8	March	7.9
Sales	11.9	April	6.9
Agriculture, forestry, fishery	2.0	May	8.7
Transport, communication	3.5	June	18.6
Production	19.3	July	6.9
Services	11.1	August	10.4
Other	11.3	September	7.0
Looks for part-time job	5.0	October	6.5
Interlocal job seeking	15.9	November	5.3
Registers as part-time unemployed	9.3	December	5.6
Casewoker assessment at entry:			
Job ready	68.4		
Needs guidance	7.8		
Offered a program	0.02		
Difficult to place	10.9		
Special category	12.9		
Local program rate at entry	23.8		

B. Estimation of the propensity score by month of placement (marginal effects)

	month=1	month=5	month=10	month=15	month=18
Female	-0.002 (3.09)**	-0.002 (0.90)	-0.002 (1.01)	-0.002 (0.64)	-0.013 (3.57)**
Age at entry	-0.002 (6.30)**	-0.006 (9.38)**	-0.002 (3.12)**	0.003 (2.38)*	0.004 (3.15)**
Age^2	0.000 (4.70)**	0.000 (7.92)**	0.000 (2.53)*	-0.000 (2.67)**	-0.000 (3.50)**
Foreign citizen	0.004 (3.74)**	-0.005 (1.89)	0.003 (0.88)	0.010 (1.87)	-0.004 (0.75)
Education (vs. compulsory)					
Secondary	0.007 (5.02)**	0.017 (5.26)**	0.007 (1.84)	-0.001 (0.24)	-0.005 (1.00)
Secondary vocational	0.004 (3.23)**	0.013 (5.35)**	0.011 (3.94)**	0.002 (0.44)	-0.002 (0.50)
University	0.000 (0.30)	0.011 (3.12)**	0.012 (3.11)**	-0.003 (0.44)	-0.012 (2.44)*
Has education for job	-0.000 (0.25)	0.003 (1.46)	0.002 (0.99)	0.003 (0.88)	0.001 (0.29)
Experience for job (vs.none)					
Some	0.004 (4.41)**	-0.019 (5.67)**	-0.018 (4.75)**	-0.020 (2.56)*	-0.011 (1.82)
A lot	0.001 (1.04)	-0.002 (0.72)	-0.001 (0.31)	0.003 (0.42)	0.009 (1.48)
Missing information	-0.009 (6.14)**	-0.005 (2.17)*	-0.000 (0.01)	0.000 (0.02)	0.005 (1.00)
Entitlement (vs. none)					
UI	-0.025 (13.22)**	-0.029 (6.04)**	-0.008 (1.59)	0.038 (5.00)**	0.018 (2.31)*
KAS	-0.016 (9.98)**	-0.006 (1.46)	0.004 (0.79)	-0.010 (0.94)	0.003 (0.30)
Daily wage	0.000 (3.82)**	0.000 (1.62)	0.000 (1.34)	-0.000 (0.74)	-0.000 (1.40)
Worked 20h/week	-0.006 (0.99)	-0.008 (0.80)	-0.001 (0.14)	0.009 (0.64)	-0.007 (0.49)
Worked 30h/week	-0.007 (1.66)	-0.003 (0.40)	-0.006 (0.89)	-0.009 (0.94)	-0.003 (0.28)
Worked 40h/week	-0.010 (6.93)**	-0.005 (1.60)	-0.003 (0.91)	0.005 (0.88)	-0.001 (0.25)
County (vs. Stockholm)					
Uppsala	-0.001 (0.34)	0.001 (0.25)	0.012 (1.91)	-0.002 (0.17)	0.003 (0.35)
Södermanland	0.019 (6.35)**	0.002 (0.30)	0.013 (1.86)	-0.001 (0.13)	0.008 (0.92)
Östergötland	0.016 (6.50)**	0.005 (1.09)	0.001 (0.17)	0.003 (0.35)	-0.011 (1.89)
Jönköping	0.019 (6.37)**	0.005 (1.08)	0.006 (1.05)	0.015 (1.48)	0.002 (0.26)
Kronoberg	0.027 (7.13)**	0.016 (2.41)*	0.025 (2.58)**	0.021 (1.33)	0.033 (2.35)*
Kalmar	0.026 (7.88)**	0.023 (3.28)**	0.043 (4.39)**	0.003 (0.19)	0.012 (1.00)
Gotland	0.047 (6.24)**	0.030 (2.21)*	0.012 (0.68)	-0.003 (0.13)	-0.005 (0.41)
Blekinge	0.017 (4.48)**	0.007 (1.00)	0.017 (1.72)	0.002 (0.16)	-0.005 (0.99)
Malmöhus	0.014 (7.60)**	0.005 (1.49)	0.009 (2.12)*	0.013 (1.94)	0.000 (0.05)
Halland	0.011 (3.93)**	0.010 (1.79)	0.006 (0.88)	0.003 (0.26)	-0.008 (1.82)
Göteborg and Bohus	0.009 (5.36)**	-0.003 (1.11)	0.006 (1.61)	0.002 (0.47)	-0.005 (0.54)
Värmland	0.035 (10.51)**	0.018 (2.95)**	0.013 (1.70)	0.001 (0.11)	0.029 (2.17)*
Örebro	0.014 (4.77)**	0.008 (1.39)	0.021 (2.59)**	0.020 (1.42)	-0.004 (0.47)
Västmanland	0.006 (2.32)*	0.012 (2.23)*	-0.008 (1.29)	0.023 (1.93)	-0.005 (0.43)
Kopparberg	0.022 (7.17)**	0.029 (4.37)**	0.026 (2.86)**	-0.002 (0.13)	-0.007 (0.64)
Gävleborg	0.046 (12.52)**	0.007 (1.16)	0.010 (1.22)	0.027 (2.03)*	-0.013 (1.71)
Västernorrland	0.040 (11.77)**	0.003 (0.54)	0.019 (2.54)*	0.005 (0.43)	0.001 (0.05)
Jämtland	0.035 (7.97)**	-0.002 (0.31)	0.016 (1.52)	0.049 (2.71)**	-0.005 (0.36)
Västerbotten	0.038 (11.55)**	0.014 (2.09)*	0.010 (1.06)	0.025 (1.61)	0.010 (0.69)
Norrbotten	0.028 (8.80)**	0.009 (1.65)	0.002 (0.22)	0.028 (1.95)	-0.008 (1.48)
Sector (vs.professional/technical)					
Health, nursing, social	-0.004 (3.12)**	0.000 (0.06)	0.001 (0.22)	-0.008 (1.31)	-0.008 (1.37)
Admin, managerial	-0.005 (4.06)**	0.000 (0.01)	0.010 (2.35)*	-0.004 (0.56)	-0.012 (2.38)*
Sales	-0.007 (5.95)**	-0.001 (0.24)	0.001 (0.33)	-0.003 (0.49)	0.006 (0.45)
Agriculture	0.004 (1.62)	-0.003 (0.59)	-0.005 (0.52)	0.006 (0.40)	0.011 (0.31)
Transport, communic.	-0.011 (5.96)**	-0.004 (0.82)	0.035 (0.95)	0.000 (0.00)	-0.008 (1.15)
Production	-0.004 (2.92)**	-0.000 (0.11)	-0.004 (0.73)	0.004 (0.60)	-0.008 (1.61)
Services	-0.008 (6.37)**	-0.004 (1.06)	0.005 (1.32)	-0.001 (0.09)	-0.008 (1.51)
Other	0.002 (1.02)	0.012 (2.77)**	0.007 (1.44)	0.011 (1.24)	0.003 (0.45)
Looks for part-time job	-0.007 (3.88)**	-0.012 (3.11)**	0.022 (3.81)**	-0.012 (1.80)	-0.013 (2.39)*
Interlocal job seeking	-0.001 (0.97)	0.005 (2.23)*	-0.010 (2.50)*	0.008 (1.41)	0.001 (0.14)
Registration month (vs.Jan)					
February	-0.002 (1.16)	-0.019 (4.27)**	0.010 (3.07)**	-0.004 (0.57)	-0.001 (0.13)
March	0.000 (0.11)	-0.001 (0.21)	-0.002 (0.44)	-0.014 (1.95)	0.058 (4.32)**
April	-0.006 (3.83)**	0.046 (8.15)**	-0.002 (0.57)	-0.022 (3.18)**	0.051 (3.54)**
May	-0.004 (1.87)	0.043 (7.18)**	0.005 (1.16)	-0.017 (2.57)*	0.042 (2.86)**

June	-0.016 (8.31)**	0.023 (4.35)**	0.007 (1.51)	0.008 (1.12)	0.035 (2.65)**
July	-0.008 (4.00)**	0.005 (0.98)	0.011 (2.56)*	0.014 (1.80)	0.006 (0.50)
August	0.023 (10.78)**	0.004 (0.92)	0.002 (0.50)	-0.007 (0.98)	0.043 (2.81)**
September	0.016 (7.89)**	0.014 (2.62)**	-0.005 (0.95)	-0.013 (2.07)*	0.022 (1.61)
October	0.007 (3.76)**	0.009 (1.79)	-0.020 (4.73)**	-0.021 (3.57)**	0.032 (2.11)*
November	0.003 (1.58)	0.013 (2.39)*	-0.017 (4.30)**	-0.005 (0.63)	0.022 (1.57)
December	0.009 (4.43)**	0.014 (2.58)*	0.002 (0.47)	0.003 (0.36)	0.024 (1.82)
First registers as part-time unemployed	0.018 (2.42)*	0.031 (4.35)**	-0.002 (0.42)	-0.001 (0.17)	0.003 (0.40)
Part-time unemployed	-0.019 (6.92)**	-0.035 (12.53)**	0.012 (1.75)	-0.077 (15.66)**	-0.043 (8.53)**
Casewoker assessment					
Job ready	0.002 (1.25)	0.030 (11.35)**	-0.035 (10.82)**	0.043 (7.40)**	0.035 (7.06)**
Needs guidance	0.008 (3.78)**	0.038 (11.81)**	0.024 (7.56)**	0.011 (2.50)*	0.023 (5.25)**
Offered a program	-0.006 (1.18)	0.138 (13.04)**	0.027 (8.46)**	0.151 (11.60)**	0.137 (10.21)**
Difficult to place	-0.016 (12.27)**	0.004 (1.58)	0.142 (14.48)**	-0.018 (4.34)**	-0.009 (2.54)*
Special category	-0.018 (12.39)**	-0.012 (3.79)**	-0.002 (0.88)	-0.018 (2.83)**	-0.005 (0.93)
Local program rate	0.001 (12.32)**	0.002 (5.39)**	-0.010 (2.58)**	0.001 (1.46)	0.000 (0.65)

Robust z-statistics in parentheses: * significant at 5%; ** significant at 1%
Pseudo-R^2 for all 18 specifications are presented in Appendix B, col. (4).

C. Indicators of covariate balancing, before and after matching, by month

Month (1)	No. Treated Before (2)	No. Non-treated Before (3)	Probit ps-R^2 Before (4)	Probit ps-R^2 After (5)	$Pr>\chi^2$ After (6)	Median bias Before (7)	Median bias After (8)	No. lost to CS After (9)
1	4,149	98,656	0.192	0.004	0.9758	9.5	1.1	0
2	3,999	84,064	0.151	0.006	0.5678	8.1	1.2	1
3	4,728	67,342	0.165	0.006	0.2494	8.4	1.2	2
4	3,653	52,685	0.165	0.003	0.9997	6.3	1.1	3
5	2,591	43,378	0.147	0.006	0.9945	6.7	1.5	1
6	1,913	36,834	0.128	0.009	0.9676	7.0	1.8	0
7	1,681	31,595	0.117	0.011	0.9299	5.8	2.0	3
8	1,361	27,214	0.110	0.011	0.9942	4.7	1.6	4
9	1,135	23,899	0.120	0.013	0.9947	6.8	1.6	2
10	979	21,161	0.142	0.011	1.0000	6.9	2.2	4
11	859	18,844	0.120	0.015	0.9994	6.5	2.7	1
12	745	16,923	0.138	0.023	0.9604	4.8	2.4	3
13	821	14,914	0.151	0.012	0.9999	4.8	2.1	2
14	965	13,028	0.198	0.004	0.9992	6.7	2.2	7
15	803	11,016	0.214	0.006	0.9894	7.2	2.4	3
16	671	9,956	0.198	0.007	0.9987	6.0	3.6	4
17	498	8,828	0.195	0.009	0.9997	7.1	3.1	3
18	382	7,880	0.194	0.013	1.0000	7.3	2.8	2

Notes:

(1) Elapsed month in open unemployment.

(2) Number of treated (i.e. joining a program at that month in unemployment).

(3) Number of potential comparisons (i.e. still openly unemployed at that month and not joining at that month).

(4) Pseudo-R^2 from Probit estimation of the conditional joining probability at that month, giving an indication of how well the regressors X explain the participation probability.

(5), (6), (8) and (9) are post-matching indicators based on nearest-neighbour matching (1% caliper).

(5) Pseudo-R^2 from a Probit of D on X on the *matched* samples, to be compared to (4). From the corresponding linear probability model, after matching the 67 regressors explain only 1.8 percent of the variance of D on average across treatment months.

(6) *P*-value of the likelihood-ratio test after matching. The joint significance of the regressors is always rejected. (Before matching it was never rejected at any significance level, with $Pr>\chi^2$=0.0000 always).

(7), (8) Median absolute standardised bias before and after matching, median taken over all the 67 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.

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

Note that the standardization allows comparisons between variables X and for a given variable X, comparisons before and after matching.

(9) Number of treated individuals falling outside of the common support (based on a caliper of 1%).

D. Selected 'overall' results over time: Average effect on the probability of being in various labour market states *t* months after program entry.
(absolute percentage points)

	t=3	*t*=6	*t*=12	*t*=24	*t*=36	*t*=48	*t*=60
on program	51.2	9.0	4.2	2.9	1.5	0.5	-0.1
	(50.9; 51.9)	(8.6; 10.0)	(3.5; 4.9)	(2.1; 3.5)	(1.0; 2.2)	(-0.1; 1.0)	(-0.7; 0.4)
unemployed	-35.0	1.5	-0.7	-0.9	-0.3	0.3	-0.4
	(-35.9; -34.4)	(0.4; 2.3)	(-1.9; 0.0)	(-1.6; 0.2)	(-0.9; 0.7)	(-0.1; 1.0)	(-1.4; 0.4)
on benefits	-6.3	12.4	7.0	2.8	2.2	1.3	0.6
	(-13.0; -5.2)	(4.3; 13.4)	(6.4; 7.7)	(2.9; 3.1)	(2.1; 2.9)	(1.3; 1.9)	(0.2; 1.4)
employed	-3.9	1.5	4.4	5.5	5.7	6.2	5.6
	(-4.5; -3.5)	(0.5; 2.1)	(3.6; 5.3)	(4.6; 6.3)	(4.8; 6.7)	(5.2; 7.2)	(4.5; 7.4)
deregistered	-16.3	-10.6	-3.5	-2.1	-1.3	-0.7	0.5
	(-17.2; -15.6)	(-11.8; -9.8)	(-4.2; -2.5)	(-3.1; -1.2)	(-2.7; -0.5)	(-2.1; -0.1)	(-0.3; 1.9)
on education	-2.4	-1.6	0.5	0.6	0.9	0.7	0.7
	(-2.8; -2.1)	(-2.2; -1.3)	(0.1; 1.1)	(-0.1; 1.1)	(0.2; 1.5)	(0.0; 1.1)	(-0.4; 1.4)
inactive	-4.9	-4.0	-1.1	-1.0	-1.1	-1.0	-0.2
	(-5.3; -4.3)	(-4.7; -3.5)	(-1.7; -0.1)	(-1.6; 0.0)	(-1.8; -0.3)	(-1.8; -0.2)	(-1.3; 0.7)
'lost'	-6.8	-7.0	-5.9	-6.0	-5.4	-5.5	-4.8
	(-7.1; -6.2)	(-7.4; -6.4)	(-6.3; -5.1)	(-6.7; -5.5)	(-6.1; -4.8)	(-6.3; -5.0)	(-5.6; -3.9)

Notes: 95 percent bias-corrected percentile confidence intervals from bootstrapping (500 reps).