IFAU – INSTITUTE FOR
LABOUR MARKET POLICY
EVALUATION

# Evaluating social programs: active labor market policies and social insurance

Laura Larsson

DISSERTATION SERIES 2002:1

Presented at the Department of Economics, Uppsala University

The Institute for Labour Market Policy Evaluation (IFAU) is a research institute under the Swedish Ministry of Industry, Employment and Communications, situated in Uppsala. IFAU's objective is to promote, support and carry out: evaluations of the effects of labour market policies, studies of the functioning of the labour market and evaluations of the labour market effects of measures within the educational system. Besides research, IFAU also works on: spreading knowledge about the activities of the institute through publications, seminars, courses, workshops and conferences; creating a library of Swedish evaluational studies; influencing the collection of data and making data easily available to researchers all over the country.

IFAU also provides funding for research projects within its areas of interest. There are two fixed dates for applications every year: April 1 and November 1. Since the researchers at IFAU are mainly economists, researchers from other disciplines are encouraged to apply for funding.

IFAU is run by a Director-General. The authority has a traditional board, consisting of a chairman, the Director-General and eight other members. The tasks of the board are, among other things, to make decisions about external grants and give its views on the activities at IFAU. Reference groups including representatives for employers and employees as well as the ministries and authorities concerned are also connected to the institute.

Postal address: P.O. Box 513, 751 20 Uppsala
Visiting address: Kyrkogårdsgatan 6, Uppsala
Phone: +46 18 471 70 70
Fax: +46 18 471 70 71
ifau@ifau.uu.se
www.ifau.se

This doctoral dissertation was defended for the degree of Doctor in Philosophy at the Department of Economics, Uppsala University, September 27, 2002. The thesis has been published by IFAU as three Working Papers: 2000:1, 2002:6 and 2002:10.

ISSN 1651-4149

Doctoral dissertation presented to the Faculty of Social Sciences 2002


**Abstract**

LARSSON, Laura, 2002, Evaluating Social Programs: Active Labor Market Policies and Social Insurance; Department of Economics, Uppsala University, *Economic Studies* 64, 126 pp, ISBN 91-87268-71-X.

This thesis consists of three self-contained essays.

**Essay I** evaluates two Swedish active labor market programs for youth during the early 1990s, namely youth practice and labor market training. A non-parametric matching approach is applied to estimate the average program effects. The results indicate either zero or negative effects of both programs on earnings, employment probability and the probability of entering education in the short run. The long-run effects are mainly zero or slightly positive. The results also suggest that youth practice was more effective – or 'less harmful' –than labor market training.

**Essay II** examines the incentive effects caused by the interactions between unemployment insurance (UI) and sickness insurance (SI), two important components of Sweden's social insurance system. There are two main topics of interest: how the sickness report rate and the length of the subsequent sick period among the unemployed are affected by (*i*) the limit of 300 workdays for UI benefits, and (*ii*) the difference in maximum compensation between UI and SI benefits. Results obtained by duration analysis suggest that sick reports increase as the UI benefit expiration date approaches. There is also evidence of an incentive effect on the sick-report rate because SI offers greater compensation than UI. But neither of these factors seems to have a significant effect on the length of the sick period.

**Essay III** (with Kenneth Carling) evaluates a youth measure introduced in the late 1990s, and still in practice. The main purpose of the measure is to prevent long-term unemployment by guaranteeing an assignment to some labor market program within 100 days of unemployment. To identify the effect of the measure, three conditions are used: The guarantee covers individuals aged 24 but not 25, one fifth of the municipalities do not provide the guarantee, and the guarantee existed in 1998 but not in 1997. No evidence is found that the measure did significantly improve the future labor market situation of the youth, which suggests that early intervention in the unemployment spell is not important.

## Table of contents

**Essay II: Sick of being unemployed? Interactions between unemployment and sickness insurance in Sweden**



**Essay III: Does early intervention help the unemployed youth?**

## Acknowledgements

At the age of five, my plan was to become a circus trapeze dancer when I grew up. Ten years later, I was sure that I would become a stewardess. After that, my fear of heights increased, while my certainty about the future decreased. Eventually, it so happened that I went to a lecture in economics, visited Uppsala as an exchange student, and, all of a sudden, there I was: five years of hard work with a thesis in front of me. And hard work indeed it has been, but today I am happy and proud of what I have accomplished. I am also humble enough to realize that this work would not have been possible without the help of many people around me. In particular, I would like to thank:

My advisor, Bertil Holmlund, for his outstanding professional skills and undoubted judgement. Not only has he been quick and constructive in his feedback to whatever I have asked him to read, he has also taken the time to encourage me in moments of doubt and despair. Further, I am grateful for all comments and guidance from my co-advisor Per-Anders Edin.

Towards the end of my first year as a graduate student, I had the privilege of meeting Susanne Ackum Agell, who invited me to work at IFAU, the Institute for Labour Market Policy Evaluation. I cannot think of a better place to write a thesis! I am deeply grateful for the keen interest Susanne has shown in all my work and for her confidence in me, but above all for her time and support last year when it all looked dark.

I also want to thank my co-author of Essay III, Kenneth Carling, for being such a great working partner. It is hard to imagine a better complement to me: To Kenneth, research is like solving riddles, and he is not afraid of giving his imagination a free rein when seeking for the solution. My chief contribution, in turn, comes when it is time to put the story on paper. We also share a similar sense of humor, which has made Wednesdays the best of all weekdays during the last few years.

Various other people, whose names are found in the three essays, have provided valuable help in reading and commenting on parts of this thesis. Thank you all for that! However, I want to mention one of them here: Peter Fredriksson. Considering the amount and quality of research he produces, it is inconceivable how he has found the time to help me with all my writings (not to mention all the other people I know he helps).

Working with all the wonderful people at IFAU has been intellectually stimulating and socially unbeatable. It is great to have colleagues who, after providing such stringent and clever comments in a seminar, spend the coffee break discussing the idea of breeding short-leg elks to prevent fatal car accidents. It has been a pleasure to come to work! A special thanks goes to Reza and the ”ladies downstairs”: without your patient help, this thesis would definitely not be completed today.

Since my office has been at IFAU, my ties to the Department of Economics have been quite loose. Even so, I am grateful to many people there. Participating in the Labor Economics seminars on Fridays has been very inspiring, and also a good way of ensuring that I visit the Department somewhat regularly. Eva Holst deserves special thanks for her advice in the preparation of this manuscript.

Being able to share the “ups and downs” during these five years with persons who experience exactly the same turns is, of course, worth a great deal. I have really appreciated the company of my graduate fellows Fredrik Andersson, Mikael Carlsson, Stefan Eriksson, Johanna Jacob, Tobias Lindhe, and Oskar Nordström Skans. I hope we remain friends for life!

To all my “off-work” friends, far and near, I just want to send a big hug – you are the best!

Turning to my family, I want to thank my parents Johanna and Kaarlo for raising me to a curious and open-minded person, and encouraging me to make my own choices, but most of all for loving me for who I am. Knowing that I am always welcome home when I need a break has made these years much easier. My brother Mikko has done his best (and managed!) to cheer me up by dragging me around to movies and pubs, and I really appreciate our conversations. I am also in deep gratitude to my beloved grandmothers Aili and, in memory, Eila, who have, both in their own ways, been such excellent role models and talk-mates.

Finally, my husband Peter; you are the most reliable, strong, and patient person I have ever known. My gratitude for your love and support is huge, and my main concern for the future is to think of a way to compensate you for all that.

Financial support from the Yrjö Jansson Foundation is also gratefully acknowledged.

Uppsala, August 2002
Laura Larsson

# Introduction*

Unapplied knowledge is knowledge shorn of its meaning.
A. N. Whitehead

## Why evaluation?

According to Webster's dictionary, evaluation is "to determine the worth or significance of something". In lay terms, this means inquiring whether something is good for its purpose. Professional evaluation of a government policy is concerned with the very same question: does the policy correspond to its purpose?

Obviously, this is an important question for a taxpayer concerned about the efficient use of public funds. It also seems natural that the tolerance for misuse is lower during recessions, when increasing expenses on various social programs are combined with decreasing tax revenues. The persistently high European unemployment during the past decade has certainly contributed to the awakened interest in policy evaluation among the governments in these countries.[1] In North America, there is a much longer tradition of evaluation research, and thus, much can be learned in Europe by looking at the results, as well as the methods, of North American evaluation studies.

It is not only politicians who are excited about evaluation, however. There is also a lively ongoing debate on among academic researchers on both sides of the Atlantic, on the methods and practice of evaluation. In the most recent version of *Handbook of Labor Economics*, two chapters, by Heckman *et al.* (1999), and Angrist & Krueger (1999), respectively, are more or less dedicated to these questions. Why is there so much to write about? After all, political interest does not always imply academic interest. What is then the driving force for so many academic researchers to choose evaluation instead of some other (interesting) field?

* Comments from Bertil Holmlund, Peter Larsson, Christina Lönnblad, Erik Mellander, and Oskar Nordström Skans are gratefully acknowledged.

[1] A concrete example of this in Sweden is the founding of IFAU, Institute for Labour Market Policy Evaluation, in 1997.

I see a couple of potential explanations. First, I am sure that most researchers in this field have a genuine interest in social problems, such as unemployment and poverty, in common. We – I include myself in this group – want to understand the emergence of these problems but also contribute to solving them by evaluating measures designed to abate them. Second, the excitement among decision-makers implies that one's work easily reaches a broad public, which is rewarding for most researchers.[2]

The field also provides tremendous possibilities for methodological development. In fact, I believe this to be the main reason why so many recognized researchers, who would easily find funding for any research, choose evaluation. Both the quality and the amount of data available for evaluation research have improved remarkably in the last decade, which is not to say that more cannot be done.[3] The comprehensive data allow for testing and developing various econometric methods, which may eventually produce better research. Furthermore, as the fundamental problems in evaluation – like selection or endogeneity – are common to basically all empirical economics, the methodological development within evaluation is also of use for the study of many other questions.

This doctoral dissertation consists of three self-contained essays dealing with the evaluation of government policies directed at unemployed people in Sweden. An obvious first question to raise in an evaluation is whether the policy produced the intended outcome. For example, an active labor market program aims at improving the participants' chances of getting a job. Thus, the question is whether this is actually the case. Essays I and III in this thesis focus on such direct effects of youth programs.

Another important question is whether the policy was a worthwhile social investment. Besides the direct benefits, answering this question requires knowledge about the cost of the program, as well as its potential unintended, indirect effects. For example, wage subsidies may indeed raise the participants' chances of getting a job, but decrease employment among the non-subsidized, as they provide incentives for firms to hire subsidized workers instead of mem-

---

[2] At least within labor economics, the excitement has also improved the opportunities to receive funding for evaluation research.

[3] The importance of good data for evaluation research has been emphasized by e.g. Heckman *et al.* (1998).

bers of the regular workforce. Incentive effects arising in the interaction between two social insurance programs constitute the topic of Essay II.

In the remainder of this chapter, I will present the results of my three essays in more detail, and shortly discuss the related literature under the headings *Active labor market policies for the youth*, and *Interactions between social insurance programs*.

## Active labor market policies for the youth

A fundamental objective of active labor market policies is to improve the labor market prospects of individuals who are either unemployed or at the risk of unemployment. In practice, the goal is often a higher degree of employment and higher wages after participation in a labor market program. In Europe, the programs have traditionally been directed at unemployed individuals, whereas in North America, disadvantaged people trapped in jobs with low-wages and low job security have been an important target group.

According to conventional definitions, active labor market policies comprise job broking activities with the aim of matching the unemployed with vacancies, labor market training, targeted wage subsidies, and direct public sector job creation. Furthermore, most countries provide special youth programs that are often a mixture of the above activities, and exclusively directed at young unemployed.[4] In 2000, an average OECD country devoted 13 percent of all spending on active labor market policies to these special youth programs (Martin & Grubb, 2001). Besides these special programs, at least the elderly youth are also often allowed to participate in regular programs.

Reviewing the evaluations of youth programs outside Sweden leads to fairly pessimistic conclusions about how these programs work: they do not seem to be of any significant help, at least not to disadvantaged youths, that is, youths with low education and skills. Most of the literature so far is from North America, and the results from there are almost exclusively negative. Quoting Heckman  LaLonde and Smith (1999),

"… we believe that neither the experimental or non-experimental literature provide much evidence that employment and training programs improve U.S. youths' labor market prospects" (p.2068).

[4] The definition of youth varies somewhat among countries; in Sweden the upper age limit for youth is usually set to 25 years.

The results for European countries are not much brighter, see for example the surveys by Heckman *et al.* (1999), and Martin & Grubb (2001). The slight difference in results does not necessarily indicate that European programs work better, but rather that the target group is more heterogeneous in previous education and skills in Europe than in North America. It is possible that the programs work better for advantaged than for disadvantaged individuals. So far, very few European evaluations have considered this potential heterogeneity in program effects, however.[5]

The previous literature on Swedish youth programs mainly consists of studies of the 1980s, and the results vary from clearly positive to zero, or even negative. However, these studies are based on small samples and cannot be generalized to the population at large. (See Ackum, 1991, Edin & Holmlund, 1991, and Korpi, 1994). Regnér (1997) uses more comprehensive data from the early 1990s to evaluate the effect of labor market training on future earnings, and finds a significantly negative effect for youths.

Essays I and III in this thesis add to the evaluation literature of youth programs, providing a number of new results for Sweden in the 1990s. Essay I is an evaluation of the two largest programs for the youth, aged 20-24 years, in the first half of the 1990s, namely youth practice (in Swedish *ungdomspraktik*) and labor market training. Youth practice was a mixture of training and subsidized work in both the private and the public sector. The aim of Essay I is to determine whether the programs improved the individuals' future labor market prospects as compared to job search as openly unemployed.[6] Furthermore, the two programs are compared to each other. The effects are measured in terms of future earnings, employment probability, and the probability of studies provided by the regular educational system.

The main findings of Essay I are: (*i*) Both programs had negative effects on earnings and employment probability one year after the program, whereas the effects after two years are mainly zero; (*ii*) Labor market training had a de-

[5] There are some hopeful signs among all the negative results, though. Evidence from North American studies suggests that the best results (for disadvantaged youths) are obtained by sustained interventions starting in early childhood. Furthermore, programs with a close link to the local labor market, that are a mixture of education, work experience, and other supporting services, and that provide pathways to further education seem to work better than other programs. Results from Ireland and U.K. suggest that "market-oriented programs" and intensive job search services produce some positive results. (Martin & Grubb, 2001.)

[6] 'Openly unemployed' refers to an unemployed not participating in any active labor market program.

creasing effect on the transition rates to regular education; (*iii*) Youth practice was better, or "less harmful", than labor market training in all respects. (*iv*) Even though the estimated effects do seem to vary among individuals, the results do not indicate that the programs would have worked differently for advantaged and disadvantaged youths, respectively.

Essay III focuses on a youth program introduced in 1998, referred to as *Utvecklingsgarantin* (henceforth UVG-program). In essence, the UVG-program was a blend of the conventional features in many other programs, as it consists of training and work schemes, thus reminding us of youth practice. The novelties were, first, that the youth were guaranteed an assignment to some program no later than 100 days after becoming unemployed, given that they were still openly unemployed. That is, had they not been assigned to any other program within the 100-day period, they would automatically be assigned to the UVG-program. The argument was that long-term open unemployment is devastating for the future labor market prospects of the youths. Second, the municipalities and not the State were responsible for the program.

The aim of Essay III is to examine whether the youths' labor market prospects were improved by the 100-day guarantee. The effect is considered to be better, the less the individual is unemployed during the subsequent 1.5 years. The main findings are: (*i*) On average, the guarantee did not make any difference in terms of future unemployment; (*ii*) The probability of finding a job increased slightly during the first 100 days of unemployment, that is, already before participating in the UVG-program, suggesting that the youths perceive the UVG-program more as a threat than a promise; (*iii*) The effect was the same – zero – for advantaged and disadvantaged youths.

Furthermore, Essay III finds that the UVG-program was not a real guarantee: on average, it only implied an increase from around 25 to 30 percent in the probability of being assigned to some program within the promised 100-day period. This does not explain the results, however. The effect was also zero in municipalities where a considerably larger number of youths were assigned to programs within 100 days.

In sum, both studies suggest that Swedish youth programs do not work any better than their counterparts outside Sweden. The essays also contribute to the methodological literature of non-experimental policy evaluation. Both use data based on Employment Service records that contain detailed information on unemployment spells, potential participation in active labor market programs, and a rich set of individual characteristics. Essay I provides an example of how to use matching on the propensity score to estimate the effect of multiple programs, an approach first introduced by Imbens (2000) and Lechner (2001).[7] A considerable part of Essay I is allotted to a discussion of advantages and potential problems associated with the method. Furthermore, the results obtained by matching are compared to those obtained through other well-known methods. Essay III, in turn, uses an identification strategy that may be referred to as a 'natural experiment' approach (see e.g. Angrist & Krueger, 1999). Like matching, this approach has lately become increasingly popular in empirical labor economics.

## Interactions between social insurance programs

Social insurance, just as active labor market policies, is an important component of the modern welfare state. What exactly is considered as social insurance programs varies, but for the purposes of this thesis, it is suitable to follow Krueger & Meyer (2001) and define them as: "…compulsory, contributory government programs that provide benefits to individuals if certain conditions are met" (p. 2).

As in all insurance systems, the objective of the social insurance system is to pool risks. The basic idea with these programs is to guarantee the individuals comprised by the program compensation in case of an accident (e.g. sickness or disability) or an event for which they cannot plan adequately (e.g. retirement). There is also an ethical aspect associated with social insurance: individuals deserve a minimum level of support, irrespective of their actions. Unemployment Insurance and Parents' Insurance are further examples of social insurance programs.

The benefits of a social insurance program for the individual are quite obvious: the more generous the compensation, the greater is the protection against risk. However, higher compensation levels may induce individuals to change their behavior towards a greater risk, and thus increase the cost of the system.

There is a large literature on the incentive effects of social insurance programs. An extensively studied area is the effect of the programs on labor supply, that is, individuals' decision of whether to participate in the labor force and how much to work (for an excellent survey, see Krueger & Meyer, 2001).[8] The

---

[7] Rubin (1977), Rosenbaum & Rubin (1983, 1984, and 1985), and Rubin & Thomas (1992) describe matching in the binary case with only one program.
[8] Another large research area is the effect of social insurance on capital markets.

program that has probably generated most research, both theoretical and empirical, is unemployment insurance (UI). Even though the empirical results (and the quality) of these studies vary, the bottom line seems to be that increased UI benefit generosity causes longer spells of unemployment, and probably higher overall unemployment.[9]

Another popular topic, especially in Europe, is the effect of sickness insurance (SI) on worker absenteeism. Once more, the results from empirical studies indicate that higher benefits induce less willingness to work. Johansson & Palme (2002) provide a Swedish example: They use panel data on work absence behavior among blue-collar workers to analyze the effect of a major sickness insurance reform, and find that the cost of absence has a significant effect on workers' behavior.[10]

Considering the magnitude of the social insurance programs in most countries, it is obvious that many individuals are simultaneously comprised by several programs. Thus, it is surprising that so little research has been done on the incentive effects arising in the interactions among programs. Exactly which questions are interesting to examine naturally depends on the institutional setting in each country.

Essay II in this thesis deals with the potential incentive effects implied by the designs of Swedish UI and SI systems. Unemployed people are also eligible for SI as long as they are registered at the local employment offices as job seekers and if they have previously been employed. Both UI and SI benefits are based on the employee's wages before unemployment, up to a ceiling above which the benefit is constant. For most of the 1990s, the replacement ratio has been the same in both systems, whereas the ceiling for SI benefits has been about 35-40 % higher than for UI benefits. Thus, there is a possibility for benefit arbitrage: by reporting sick, an unemployed person with previously high wages receives an SI benefit that is higher than the UI benefit. The first question in Essay II is whether unemployed individuals with high previous earnings exploit this possibility.

[9] Besides Krueger & Meyer (2001), Atkinson & Micklewright (1990) and Holmlund (1998) are excellent surveys of various effects of UI.

[10] For international studies, see Allen (1981), Barmby *et al.* (1991), Barmby *et al.* (1994) and Brown & Sessions (1996). Broström *et al.* (1998), Cassel *et al.* (1996), Edgerton & Wells (2000), Henrekson & Persson (2001), and Johansson & Palme (1996) are further examples of Swedish studies.

Furthermore, for the majority of the unemployed, the UI benefit period is limited to 300 workdays. After that, the benefit expires. While receiving SI benefits, unemployed people ‘reserve’ their UI benefits, thus postponing the expiration date. A second question for Essay II is, thus, whether the UI time limit – combined with the ability to report sick to lengthen the maximum compensation period – has an effect on the reported sicknesses among the unemployed.[11]

The strong connection between income and health, as documented in a series of studies, makes it difficult to identify the effect of differing benefit ceilings.[12] Higher income is shown to correlate with better health, thereby implying a lower probability of sickness. There are several potential explanations to this. High wage earners may have healthier living habits, or employers may discriminate against individuals with bad health and offer them lower wages. Nevertheless, this positive correlation between wages and health would imply that individuals with previous wages above the UI ceiling have a lower probability of being on SI instead of UI. Thus, wages are expected to have two opposite effects on the probability of being on SI. A challenge for the empirical strategy of Essay II is to separate the *incentive effect* from the *health effect*.

The three main findings from the empirical analysis are: (*i*) There is evidence of an incentive effect increasing the probability of reporting sick for those who can benefit from reporting sick; (*ii*) The probability of reporting sick increases as the UI benefit expiration date approaches, and (*iii*) Neither of these factors has a significant effect on the length of the sick period, that is, the probability of staying on SI benefits.

Altogether, these results suggest that economic incentives play a different role for the choice to *remain* on SI benefits than for the choice to *switch* to SI benefits. It may also be the case that the increased benefit from collecting SI reduces the threshold for a few days’ sick period due to minor illness, thereby decreasing the average length of SI periods.

Being the first study on a wide area of complicated questions, the main contribution of Essay II is that it serves as a first glance at the data, pointing to some interesting patterns in the behavior of the unemployed. Further analysis,

[11] Previous studies on UI benefits in Sweden indicate that as the end of the 300 workday limit approaches, the transition rate from unemployment to employment increases (see e.g. Carling *et al.*, 1996).

[12] For a summary of studies concerning the interplay between health and labor market outcomes, see Currie & Madrian (1999).

both theoretical and empirical, is still needed before we can draw distinct conclusions about which mechanisms cause this behavior. Methodologically, the essay provides an example of possible identification strategies to study the effect of limited UI benefits and discriminate between the various effects of wages.

## References

Ackum, S (1991), "Youth unemployment, labor market programs and subsequent earnings", *Scandinavian Journal of Economics* 93(4), 531-541.

Allen, S (1981), "An empirical model of work attendance", *The Review of Economics and Statistics* 71, 77-87.

Angrist, J & A Krueger (1999), "Empirical strategies in labor economics", in O Aschenfelter & D Card (eds), *Handbook of labor economics*, Volume III, North-Holland.

Atkinson, A & J Micklewright (1990), "Unemployment compensation and labor market transitions: a critical review", *Journal of Economic Literature* 29, 1679-1727.

Barmby, T, C Orme & J Treble (1991), "Worker absenteeism: an analysis using microdata", *Economic Journal* 101, 214-229.

Barmby, T, J Sessions & J Treble (1994), "Absenteeism, efficiency wages and shirking", *Scandinavian Journal of Economics* 96, 561-566.

Broström, G, P Johansson & M Palme (1998), "Assessing the effect of economic incentives on incidence and duration of work absence", Working paper series in economics and finance 288, Stockholm School of Economics.

Brown, S & J Sessions (1996), "The economics of absence: Theory and evidence", *Journal of Economic Surveys* 10, 23-53.

Carling, K, P-A Edin, A Harkman & B Holmlund (1996), "Unemployment duration, unemployment benefits, and labor market programs in Sweden", *Journal of Public Economics* 59, 313-334.

Carling, K, B Holmlund & A Vejsiu (2001), "Do benefit cuts boost job finding? Swedish evidence from the 1990s", *The Economic Journal*, 766-790.

Cassel, C-M, P Johansson & M Palme (1996), “A dynamic discrete choice model of blue collar absenteeism in Sweden 1991”, Umeå Economic Studies 425, Umeå University.

Currie, J & B Madrian (1999), “Health, health insurance and the labor market”, in O Ashenfelter & D Card (eds), *Handbook of Labor Economics*, Volume III, North-Holland.

Edgerton, D & C Wells (2000), “A model for the analysis of sick leave in Sweden. Inference using the HUS data”, mimeo, Lund University.

Edin, P-A & B Holmlund (1991), “Unemployment, vacancies and labor market programs: Swedish evidence”, in F Padoa Schioppa (ed), *Mismatch and Labor Mobility*, Cambridge University Press.

Heckman, J & J Hotz (1989), “Choosing among alternative nonexperimental methods for estimating the impact of social programs: The case of manpower training”, *Journal of the American Statistical Association* 84(408), 862-880.

Heckman, J, H Ichimura, P Todd & J Smith (1998), “Characterizing selection bias using experimental data”, *Econometrica* 66(5), 1017-1098.

Heckman, J, R LaLonde & J Smith (1998), “The economics and econometrics of active labor market programs”, in O Aschenfelter & D Card (ed), *Handbook of Labor Economics*, Volume III, North-Holland.

Henrekson M & M Persson (2001), “The effects on sick leave of changes in the sickness Insurance System”, SSE/EFI Working Paper 444, Stockholm School of Economics.

Holmlund, B (1998), “Unemployment insurance in theory and practice”, *Scandinavian Journal of Economics* 100, 113-141.

Imbens, G (2000), “The role of propensity score in estimating dose-response functions”, *Biometrica* 87(3), 706-710.

Johansson, P & M Palme (1996), “Do economic incentives affect work absence? Empirical evidence using Swedish micro data”, *Journal of Public Economics* 14(1), 161-194.

Johansson, P & M Palme (2002), “Assessing the effect of public policy on worker absenteeism”, *Journal of Human Resources*, 37(2), 381-409.

Korpi, T (1994), *Escaping unemployment. Studies in the individual consequences of unemployment and labor market policy*, Ph.D. Thesis, Swedish Institute for Social Research.

Lechner, M (2001), “Identification and estimation of causal effects of multiple treatments under the conditional independence assumption”, in M Lechner & F Pfeiffer (eds), *Econometric Evaluation of Labour Market Policies*, Physica/Springer.

Krueger, A & B Meyer (2001), “Labor supply effects of social insurance”, forthcoming in A Auerbach & M Feldstein (eds), *Handbook of Public Economics*.

Martin, J & D Grubb (2001), “What works and for whom: a review of OECD countries’ experiences with active labour market policies”, IFAU Working Paper 2001:14, Institute for Labour Market Policy Evaluation.

Regnér, H (1997), *Training at the job and training for a new job: Two Swedish studies*, Ph.D. Thesis, Swedish Institute for Social Research.

Rosenbaum, P & D Rubin (1983), “The central role of the propensity score in observational studies for causal effects”, *Biometrika* 70, 41-55.

– (1984), “Reducing bias in observational studies using subclassification on the propensity score”, *Journal of the American Statistical Association* 79, 516-524.

– (1985), “Constructing a control group using multivariate matched sampling methods that incorporate the propensity score”*, The American Statistician* 39, 33-38.

Rubin, D (1977), “Assignment to treatment group on the basis of a covariate”, *Journal of Educational Statistics* 2, 1-26.

Rubin D & N Thomas (1992), “Characterizing the effect of matching using linear propensity score methods with normal covariates”, *Biometrika* 79, 797-809.

# Essay I

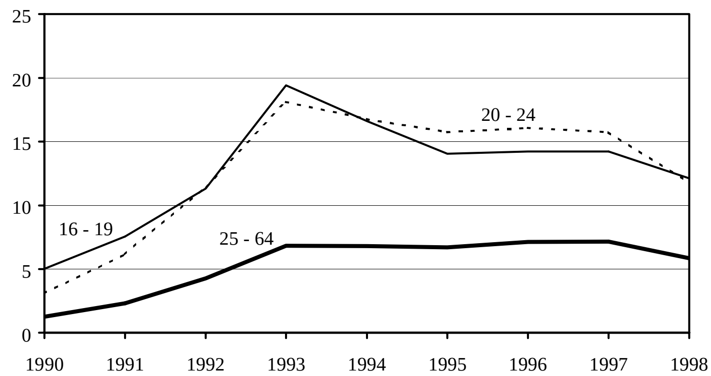
# Evaluation of Swedish youth labor market programs*

## 1 Introduction

It is a well-known fact in many European countries that youth unemployment is more sensitive to fluctuations in the business cycle than adult unemployment. Traditionally, this has also been the case in Sweden. The unemployment rates of the youth labor force have also been higher. Thus, the explosive rise in youth unemployment during the crisis of the 1990s is hardly surprising: from a level of around 3 percent in 1990, the unemployment rate for individuals aged 20–24 rose to above 18 percent in 1993, as shown by Figure 1. For the youngest age group, the level of unemployment was even higher until 1994. Adult (aged 25–64) unemployment rose from slightly more than 1 percent to 7 percent. After the peak in 1993, the situation has improved for the young cohorts, whereas adult unemployment remained on the same level until 1997.

* The paper has been accepted to be published in the *Journal of Human Resources*. A previous, shorter version of the paper: “Utvärdering av ungdomsåtgärder”, has been published in *Ekonomisk Debatt* 4, 2001. I am grateful to Per-Anders Edin, Denis Fougère, Bertil Holmlund, Per Johansson, Jochen Kluve, Winfried Koeniger, Michael Lechner, Christina Lönnblad, Julie Sundqvist, Gerard van den Berg, and two anonymous referees for their helpful comments. Previous versions of this paper were presented at the IZA Summer School in Munich, EALE 1999, the IWH workshop in Halle, and seminars at IFAU and the Department of Economics in Uppsala.

**Figure 1** Unemployment rate in Sweden by age 1990–1998.




*Source*: Statistics Sweden, Labor Force Surveys

In response to rising unemployment figures, the Swedish government increased its spending on active labor market policy in order to improve the chances of the unemployed to return to regular employment. In 1992, a new large-scale program called youth practice, targeted at unemployed youth, was introduced. Since participants in active labor market programs are defined either as employed or as being outside the labor force, the immediate effect of such programs is that unemployment falls.[1] But this is solely a matter of accounting, whereas the longer-term effects remain largely uncertain. Thus, the evaluation of active labor market programs has become an increasingly important issue.

This paper evaluates the two most comprehensive active labor market programs in Sweden for youth, aged 20-24 years, in the first half of the 1990s, namely *youth practice* and *labor market training*. The objective is to determine the effects of the programs as compared to the outcome if the individual had continued to search for a job as openly unemployed.[2] The effects are measured in terms of earnings, employment probability, and the probability of entering studies provided by the regular educational system. The focus is on the direct

[1] In principle, participants in training programs (including youth practice) are excluded from the work force, whereas subsidized work programs are defined as employment.
[2] 'Openly unemployed' refers to the unemployed not participating in any active labor market program.

effects of the programs; no attempt is made to assess the general equilibrium implications.[3]

Identification of the average treatment effects is based on the conditional independence assumption (CIA), according to which participation in the various programs is independent of the post-program outcome, conditional on observable factors influencing both the decision to participate and the outcome. Given the CIA, matching on the propensity score using the multiple treatment approach introduced by Imbens (2000) and Lechner (2001) can be applied to obtain unbiased estimates of the average treatment effects on both the treated and the population. Here, part of the analysis is devoted to discussing the plausibility of the CIA in this context. Indirect tests of the CIA, as suggested by Heckman & Hotz (1989), are discussed, and the matching method is compared to some alternative, well-known methods for estimating average treatment effects based on different identifying assumptions.

Previous microeconomic studies of active labor market programs for Swedish youth report varying results. Edin & Holmlund (1991) and Korpi (1994) find negative effects on post-program employment, but positive or insignificant effects on the re-employment probability in subsequent unemployment spells. Ackum (1991) and Regnér (1997) mainly estimate negative program effects on earnings. However, except for Regnér (1997), these studies use the same small data set from the 1980s, and apply methods which rely on restrictive parametric assumptions. None of the previous studies evaluates the effects of youth practice.

Consequently, this study contributes to the Swedish and the international literature in several ways. First, it provides a number of new results on the effects of youth programs in Sweden. Second, it applies recently developed methodology to program evaluation. Third, it offers an example of how to make use of data based on comprehensive Employment Service records.

The paper is organized as follows. The evaluation problem, as well as the identification and estimation of average treatment effects under the conditional independence assumption are addressed in Section 2. The labor market

[3] For a theoretical macroeconomic framework for studying both the direct and indirect effects, see Layard *et al.* (1991). Dahlberg *et al*. (1999) estimate the displacement effects of various active labor market programs, and find that programs providing subsidized labor displace on average 65 percent of the corresponding regular employment. Youth practice is regarded as such a program. Labor market training is not found to have any significant displacement effect, however.

programs and the data are described in Section 3. Section 4 outlines the econometric analysis based on the propensity score matching approach, while Section 5 considers the sensitivity of the results. Section 6 contains a discussion of alternative identification strategies and ways of (indirectly) testing conditional independence and, finally, Section 7 concludes.

# 2 Econometric evaluation strategies

## 2.1 The evaluation problem

This study attempts to determine and compare the outcomes of three alternative strategies available to a young unemployed individual: to participate in either youth practice or labor market training, or to continue searching for a job as openly unemployed. In other words, the aim is to determine the causal effect of a program compared to (*i*) the no-program state, and (*ii*) the other program. Following Lechner (2001), among others, this multiple evaluation problem may be introduced as follows.

Consider participation in $(M + 1)$ mutually exclusive treatments, denoted by an assignment indicator $T \in \{0, 1,\ldots, M\}$. Let the zero category indicate the *no-treatment* alternative. Moreover, denote variables unaffected by treatments, often called *attributes* (Holland, 1986) or *covariates*, by $X$. The outcomes of the treatments are denoted by $\{Y^0, Y^1,\ldots, Y^M\}$ and, for any participant, only one of the components can be observed in the data. The remaining $M$ outcomes are called counterfactuals. The number of observations in the population is $N$, such that $N = \sum_{m=0}^{M} N^m$, where $N^m$ is the number of participants in treatment $m$.

The evaluation problem is to define the effect of treatment $m$ compared to treatment $l$, for all combinations of $m, l \in \{0, 1,\ldots, M\}$, $m \neq l$. More formally, the outcomes of interest in this study are shown in the following equations:

$$(1) \qquad \theta_0^{ml} = E\left(Y^m - Y^l \mid T = m\right) = E\left(Y^m \mid T = m\right) - E\left(Y^l \mid T = m\right),$$

$$(2) \qquad \gamma_0^{ml} = E\left(Y^m - Y^l\right) = EY^m - EY^l.$$

$\theta_0^{ml}$ in eq. (1) denotes the expected average treatment effect of treatment $m$, relative to treatment $l$, for participants in treatment $m$ (sample size $N^m$). In the binary case, where $m = 1$ and $l = 0$, this is usually called the ‘treatment-on-the-treated' effect. $\gamma_0^{ml}$ in eq. (2) is the corresponding expected effect for an individual drawn randomly from the whole population ($N$).[4]

The evaluation problem is characterized by missing data: the counterfactual $E(Y^l \mid T = m)$ for $m \neq l$ cannot be observed, since it is impossible to observe the same individual in several states at the same time. Thus, the true causal effect of treatment $m$ relative to treatment $l$ can never be identified. However, the *average* causal effects defined by equations (1) and (2) can be identified under the conditional independence assumption; see subsection 2.3.[5]

## 2.2 Matching as an evaluation estimator

In experimental studies, participants are randomly assigned to treatment(s) from a large group of eligible applicants. In a binary case, a comparison between the treated and the control group, which consists of the individuals not assigned to the treatment, yields an unbiased estimate of the average treatment effect. Similarly, in a multiple case, an unbiased estimate of the average effect of one treatment compared to another is obtained by comparing the two randomly assigned treatment groups. This is not the case in non-experimental studies, because the various treatment groups are likely to differ from each other in a non-random way. Hence, the objective of a non-experimental evaluation study is to construct a comparison group that is as close as possible to the experimental control group. One method suggested for solving this problem is matching.

Matching methods have been developed and widely used in the statistics and medical literature (Rubin 1977; Rosenbaum & Rubin 1983, 1984, 1985; Rubin & Thomas 1992), but are relatively new to economics and labor market policy evaluation. In short, matching involves pairing individuals from various

---

[4] Note that the latter expected effect is symmetric in the sense that $\gamma_0^{ml} = -\gamma_0^{lm}$, whereas the same is not valid for the treatment effect on the treated, that is $\theta_0^{ml} \neq -\theta_0^{lm}$, as long as participants in treatments $m$ and $l$ differ in a non-random way.

[5] Moreover, to make causal analysis possible, the stable-unit-treatment-value assumption (SUTVA) must be satisfied for all individuals in the population. The SUTVA has several consequences, the most important of which in our context is that the potential outcomes for an individual are independent of the treatment status of other individuals in the population. Thus, cross effects and general equilibrium effects are excluded. The term 'stable-unit-treatment-value' refers to another implication of the assumption, namely that the treatment status of an individual (or 'unit') is unrelated to the treatment status of other individuals. For a more detailed description and discussion of the SUTVA, see, for example, Angrist *et al.* (1996).

treatment groups who are similar in terms of their observable characteristics. When selection into treatments and the outcome are based exclusively on these observable characteristics, matching yields unbiased estimates of the average treatment effects.

## 2.3 Conditional independence assumption

The crucial assumption behind matching is that all differences affecting the selection and the outcome between the groups of participants in treatment *m* and treatment *l* are captured by (to the evaluator) observable characteristics, *X*. In the evaluation literature, this assumption is called conditional independence, or unconfoundness. In the multiple case considered in this paper, the conditional independence assumption (CIA) is formalized as[6]

$$(3) \qquad \{Y^0, Y^1, ..., Y^M\} \coprod T \mid X = x, \forall x \in \chi,$$

where $\coprod$ is a symbol for independence and $\chi$ denotes the set of covariates for which the average treatment effect is defined. In words, the CIA requires treatment *T* to be independent of the entire set of outcomes, given *X*. That is, given all the relevant observable characteristics (*X*), when choosing among the available treatments (including the no-treatment alternative), an individual does not base her decision on the *actual* outcomes of the various treatments.[7] Individuals can, however, base their decisions on expected outcomes, as long as these are determined by *X* only. This implies that individuals expect their outcomes to equal the mean outcomes for people with similar (observed) characteristics. Moreover, in order for the average treatment effect to be identified, the probability of treatment *m* must be strictly between zero and one:

[6] The significance and consequences of the CIA in the binary case of one treated and one non-treated state have been explored and formalized by Rubin (1977) and Rosenbaum & Rubin (1983). The analysis of the multiple case presented here closely follows the analyses in Lechner (2001) and Imbens (2000).

[7] Naturally, for identification of a single treatment with fixed *m* and *l*, it is sufficient to assume pair-wise independence $Y^l \coprod T = m, l \mid X = x, \forall x \in \chi$. Moreover, instead of conditional independence as in eq. (3), it is sufficient to assume conditional *mean* independence, which is a somewhat weaker assumption. However, in practical applications, it is difficult to find a situation where the latter, but not the former, is fulfilled. For a thorough discussion on identifying assumptions, see Heckman *et al.* (1998a).

(4) $\quad 0 < P^m(x) < 1,\ where\ P^m(x) = E[P(T = m \mid X = x)],\ \forall m = 0,1,...,M$ .

In the binary case of two treatments, Rosenbaum & Rubin (1983) show that if the CIA is valid for *X*, it is also valid for a function of *X* called the *balancing score* *b*(*X*), such that $X \coprod T \mid b(X)$. The balancing score property holds even for the multiple case:

(5)
$$\{Y^0, Y^1, ..., Y^M\} \coprod T \mid X = x,\ \forall x \in \chi$$
$$\rightarrow \quad \{Y^0, Y^1, ..., Y^M\} \coprod T \mid b(X) = b(x), \quad \forall\ x \in \chi, \quad if$$
$$E[P(T = m \mid X = x) \mid b(X) = b(x)] = P[T = m \mid X = x] = P^m(x),$$
$$0 < P^m(x) < 1,\ \forall\, m = 0,1,...,M\ .$$

The main advantage of the balancing score property is the decrease in dimensionality: instead of conditioning on all the observable covariates, it is sufficient to condition on some function of the covariates. In the binary case of two treatments, the balancing score with the lowest dimension is the propensity score $P^1(x) = E[P(T = 1 \mid X = x)]$. In the case of multiple treatments, a potential and quite intuitive balancing score is the *M*-dimensional vector of propensity scores [$P^1(x)$, $P^2(x)$,…, $P^M(x)$]. Lechner (2001) shows, however, that the dimension can be further reduced to two, or even one. This is illustrated in the following section, which addresses identification of the average treatment effects.

## 2.4 Identification

Let us begin by considering the identification and estimation of the average treatment effect on the treated, $\theta_0^{ml}$ . The mean outcome of treatment *m* for participants in *m*, $E(Y^m \mid T = m)$, is identified and estimated by, for example, the sample mean. Lechner (2001) and Imbens (2000) show that the latter part of eq. (1), the mean outcome of treatment *l* for participants in *m*, $E(Y^l \mid T = m)$, can also be identified in sufficiently large samples, given conditional independence. To estimate it, they show that instead of the *M*-dimensional balancing score, the dimension of the condition set can be reduced to [$P^m(x)$, $P^l(x)$]. Thus,

(6) $$E(Y^l \mid T = m) = E\left[E\left(Y^l \mid P^m(X), P^l(X), T = l\right) \mid T = m\right].$$

Lechner (2001) shows that the dimension can be further reduced:

(7) $$E\left(Y^{l} \mid T=m\right)=E\left[E\left(Y^{l} \mid P^{l|ml}(X), T=l\right) \mid T=m\right],$$

where $P^{l|ml}$ is the conditional choice probability of treatment $l$, given either treatment $m$ or $l$. Both (6) and (7) are suggested for estimating the average treatment effect on the treated.[8]

The identification and estimation of the average treatment effect for the whole population, $\gamma_0^{ml}$, may be carried out in several ways. Lechner (2001) suggests the following:

(8) $$\begin{aligned}\gamma_0^{ml} &= E\left(Y^{m} \mid T=m\right)P(T=m) \\ &+ \underset{P^{m}(X)}{E}\left[E\left(Y^{m} \mid P^{m}(X), T=m\right) \mid T \neq m\right]P(T \neq m) \\ &- E\left(Y^{l} \mid T=l\right)P(T=l) + \underset{P^{l}(X)}{E}\left[E\left(Y^{l} \mid P^{l}(X), T=l\right) \mid T \neq l\right]P(T \neq l).\end{aligned}$$

In words, (8) implies that the average treatment effect on the population is identified by a weighted sum of the treatment effects on all subsamples. For a more detailed description of the identification of $\theta_0^{ml}$ and $\gamma_0^{ml}$, see Imbens (2000) and Lechner (2001).

# 3 The programs and the data

Conditional independence cannot be regarded as a plausible assumption unless one is acquainted with the institutional settings – what was the purpose and content of the program? who participated and why? – and has reliable data on all these factors.

[8] $P^{l|ml}$ is identified as

$$E\left[P^{l|ml}(X) \mid P^{l}(X), P^{m}(X)\right]=E\left[\frac{P^{l}(X)}{P^{l}(X)+P^{m}(X)} \mid P^{l}(X), P^{m}(X)\right]=P^{l|ml}(X).$$

## 3.1 Description of the programs

Youth practice (*ungdomspraktik*) was launched in July 1992, during the most severe period of rising unemployment in Swedish postwar history. By January 1993, the stock of participants aged 20-24 in youth practice reached its peak at 60,000 which corresponds to approximately 10 percent of the population in this age group.[9] Simultaneously, labor market training, the second largest program for that cohort, decreased from about 25,000 to 15,000 participants. During the period July 1992 – July 1993, participants in these two programs on average accounted for 85 percent of all people in this age group taking part in any program; in the following year, the share was 75 percent.[10] In October 1995, youth practice was replaced by new programs.

Youth practice consisted of a subsidized work program aimed at providing working experience for the young unemployed with a high school diploma.[11] Participants were placed in both the private and the public sector, and the program period was generally six months. For individuals aged 20-24, the allowance for participation was SEK 338[12] per day, of which the employers paid only a very small fraction. In the relatively rare cases where the participant was entitled to unemployment benefits, she received an allowance equal to the benefit.

According to the program regulations, participation should be preceded by at least four months' active job search as openly unemployed. In addition, participants should be a supplementary resource for the employer and not displace regular employment, and they should allocate 4-8 hours a week to job-seeking activities at the local employment office. In practice, however, participants often worked with tasks that would otherwise have required hiring a regular employee, and allocated very little time to job seeking.[13] Moreover,

[9] Unemployed individuals aged 18-19 were eligible for youth practice but not for training. Thus, they are excluded from the study in order to fulfil the balancing score property, $X \coprod T \mid b(X)$.

[10] Thus, it seems plausible to focus on the evaluation of these two programs only.

[11] Formally, the program was supposed to be a 'mixture of subsidized work and training' in the sense that it would improve the participants' human capital. However, implementation studies show that the tasks were often very simple, so that the share of training was more or less negligible (see e.g. Hallström, 1994, and Schröder, 1995).

[12] Approximately USD 36.5, June 2002.

[13] For example, participants might assist with simple administrative tasks in a firm, or take care of children at a day-care center.

the length of pre-program unemployment varied noticeably from two or three days to several months.

Labor market training, which has existed in various forms for decades and is still in effect, is aimed at improving the skills of the unemployed job seeker in order to match her to labor demand. Thus, it has traditionally been directed at individuals with low education and skills. However, the Swedish high school system seldom prepares fully trained workers, so that individuals with a high school diploma are part of the target group. The program consists of courses of various length and content, both vocational and non-vocational.[14] The age limit and the size of the allowance have changed over time, but during the period under study, the minimum age limit for participating in the program was 20 years. Moreover, the size of the allowance was the same in labor market training as in youth practice and, according to the program regulations, participants should continue their job-seeking activities during the program. Table 1 summarizes the differences between the two programs.

**Table 1** Differences between youth practice and labor market training

| | Youth practice | Labor market training |
|---|---|---|
| Content of the program | Subsidized work | Training courses |
| Duration of the program | Generally six months (some variation) | No general rule, up to 12 months (much variation) |
| Formal target group: | | |
| Age | 18-24 years | 20-65 years |
| Education | High school diploma (some variation) | Low/wrong type of education for labor demand |
| Work experience | Little work experience | Low/wrong type of experience for labor demand |
| Labor market status before assignment to the program. | Unemployed for at least four months | Unemployed or at risk for unemployment |

Typically, an unemployed individual, in consultation with a placement officer at the local employment office, decided whether to participate in any of

[14] Although the heterogeneity of the program is ignored in the main analysis, results from an analysis where vocational and non-vocational courses are treated separately are reported in Section 5.

the programs and which program to choose. The reason for wanting to participate varied. Except for individuals who were eligible for unemployment benefits (and who thus received the same amount as participants in the programs), participation in either of the programs implied a financial benefit. Moreover, surveys among job seekers and placement officers indicate that many job seekers believed that participation in a program would improve their chances of finding a job, and many regarded youth practice as a 'real job' (see e.g. Hallström, 1994, Schröder, 1995, and Eriksson, 1997).

An individual interested in youth practice was usually encouraged by the placement officer to find an employer willing to offer placement. This was intended to increase the individual's power of initiative. Consequently, individuals who managed to find an employer on their own might have a better chance of participating than those who needed assistance from the local employment office. Sometimes, employers took the initiative and offered placement in youth practice if the local employment office arranged the financing.

Rejecting an offer to participate could, in principle, lead to suspension from unemployment benefits, if the unemployed person was entitled to any. However, in a situation where local employment offices were deluged with job searchers, those who needed help the most, comprising the least educated and experienced – and not entitled to benefits – were most likely to receive an offer, with perhaps one exception. In Sweden, unemployment benefits expire after 300 unemployment days unless the individual has qualified for a new 300-day period by working or participating in a labor market program for at least six months.[15] Therefore, unemployed people close to the benefit expiration date may have been more likely to be assigned into a program; see subsection 3.3.

To conclude, it is reasonable to assume that the more experienced and better educated the unemployed individual and the shorter her unemployment period, the lower the probability of being offered and assigned to a program. Moreover, having a high school diploma should increase the propensity for youth practice relative to labor market training.

[15] The exact rules for qualifying for unemployment benefits are somewhat more complicated.

### 3.2 Description of the data

The data used in this study, a random sample of approximately 200,000 individuals, were collected from the databases maintained by the Swedish National Labor Market Board and Statistics Sweden. The former database includes records of all individuals who have been registered with the Employment Service, whereas the latter records the annual earnings of all individuals residing in Sweden. For each individual in this study, registration dates, labor market status, and individual characteristics between August 1991 and March 1997 were combined with information on annual earnings for the years 1985–1995. A more exact description of the variables used in the empirical analysis is given in Tables A1–A3 in the Appendix. Details regarding the outcome variables are given in subsection 3.5.

In the Employment Service records, each job seeker is registered under some 'job-seeker category' defining her labor market status. Examples of such categories are full-time openly unemployed, part-time openly unemployed, or participant in a labor market program. When signing up with the Employment Service, the unemployed persons are asked to fill out a 'search form' that contains questions about individual characteristics, such as year of birth, citizenship, formal education, previous labor market experience, type of job they are looking for, etc. If an individual wishes to apply for several jobs, she is asked to give each application either a high or a low priority. The job seeker's county of residence and the code of the local employment office she visited are also recorded.

During a period in the Employment Service register, an individual may – and probably will – change categories prior to de-registration. In other words, an individual may have entered the register as openly unemployed, then participate in some labor market program, and again be openly unemployed before de-registration due to, for example, the transition to a regular job. All the relevant dates are provided in the data. The reason for de-registration is also recorded.

The database at Statistics Sweden covers all individuals residing in Sweden at the end of December each year. Information on earnings is based on firms' reports to the tax authorities. Earnings are measured on a yearly basis, and there is no information about the number of working hours. As a dependent variable in the empirical analysis of earnings, I used the annual sum of work-related income including the allowance for maternity or sickness leave and other work-related allowances from the social insurance system.

Unemployment benefits are, of course, not included in this variable. The variation in the dependent variable can thus reflect changes in both wage rates and working hours.

## 3.3 Is it plausible to assume conditional independence?

The description of the programs indicates that the level of education, previous work experience, and pre-program unemployment history are important factors in determining whether an individual will participate in any program, as well as in which of the programs. These factors are also likely to influence the future labor market outcome and thus, in order for conditional independence to be plausible, they should be included in the estimation of the propensities.

The importance of labor market history prior to a program is emphasized in various evaluation studies, starting with Aschenfelter (1978). Examples of more recent studies that all point to pre-training earnings as one of the most essential factors to be controlled for in a labor market program evaluation are Hotz *et al.* (1999), Dehejia & Wahba (1999), and Heckman *et al.* (1998b).

Annual earnings for the preceding year, pre-training unemployment periods, level of education and work experience are all included in the data available for this study.[16] Moreover, the data provide detailed information on other personal characteristics (see Table A2). Information is missing on whether the job searcher is entitled to unemployment benefits which, as discussed above, may provide an incentive to participate in a program. However, there are two arguments that may alleviate this potential shortcoming.

First, entitlement requires work experience which, in turn, implies labor earnings. Thus, by controlling for the latter two, we indirectly control for entitlement. Second, the mean pre-program unemployment periods in the participant samples are far from 300 days, which is the benefit exhaustion limit.[17] Consequently, it is reasonable to assume that the participation decision of these individuals, even if entitled to benefits, is not significantly influenced by qualification for a new benefit period.

[16] The search form includes a question as to whether the job seeker thinks she has the relevant work experience for the type of work she wants. In the remainder of the paper, this is referred to as 'specific work experience'.

[17] Table A1 in the Appendix shows that only 7-8 percent of the program participants had been unemployed more than 270 days before the start of the program.

A factor often suggested as causing selection bias is ‘motivation’ or some other unobservable personal quality of the job searcher that makes her more or less successful on the job market, and that also plays a role in the program assignment process.[18] It may be that the most highly motivated job seekers show the most interest in a program, and are thus most likely to be assigned to it. The opposite is also plausible: caseworkers may be more eager to help the unemployed who are the least motivated. Either way, the estimated program effect will turn out to be biased.

In the Employment Service data, each openly unemployed job seeker is assigned a grade indicative of her readiness to take a job if employment is found. Examples of grades are ‘can take a job directly’ or ‘needs guidance’. This grading is based on the employment officer’s assessment of the job seeker and thus provides a measure of the job seeker’s expected success on the job market.

Finally, the willingness to assign people into programs in general, and into the two programs under study in particular, varied among the local employment offices. It may be that the willingness to assign into programs is correlated with the ability to match the unemployed people with employers. Thus, variables based on records from the local employment offices are also included in the estimation of the propensities.

The bottom line is that the available data include *much*, but *not necessarily all,* information on factors which affect the selection and the outcome. The crucial question – that is left to the reader to decide – is whether there is *sufficient* information to justify the conditional independence assumption.

Later on, in Section 6, I discuss different ways of indirectly testing the plausibility of the CIA, either through pre-program outcome tests suggested by Heckman and Hotz (1989) or by applying various methods to the same problem and comparing the results. In short, I find that different methods produce somewhat different estimates for the program effects, but the sign of the effects is essentially the same across methods. Moreover, the pre-program outcome tests – as far as it is possible to apply and draw conclusions from them – provide support for the conditional independence assumption.

[18] In fact, very few evaluations based on the CIA explicitly discuss this motivational factor. One nice exception is the study by Gerfin & Lechner (2002) that applies rich Swiss data that actually does include such a variable.

## 3.4 Sample construction

From the database, I collected all individuals aged 20 to 24 who registered with the Employment Service during 1992 and 1993 as openly unemployed for the first time and with the grade 'can take a job directly'. This procedure yielded 10,579 individuals. From this group, I then collected all individuals who, after having been openly unemployed, directly enrolled in youth practice or labor market training. The final group consisted of 1,657 youth practice participants and 606 labor market training participants.[19]

A potential comparison group consisted of individuals who entered the register as openly unemployed during the same period, and never participated in any of the programs. There were slightly more than 5,000 such individuals. All of them could, in principle, have been used as the group of non-participants in the empirical analysis. However, as already pointed out, the length of the unemployment period immediately before starting a program is an important factor in determining whether an individual will participate in any program and to which program she will be assigned. Hence, in order to be able to use this information when estimating the propensities, I created a hypothetical starting date for non-participants. The following procedure is similar to the *random* procedure suggested by Lechner (1999).

First, the group of participants (here, participants in practice and training are regarded as a single group) and the group of non-participants were divided into subgroups by the month of registration with the Employment Service. Then, each of the non-participants in a subgroup was randomly assigned an observation of 'length of pre-program unemployment' from the distribution of the contemporaneous group of participants. In cases where the non-participant's actual unemployment period was shorter than the assigned pre-program unemployment period, the individual is removed from the sample. This procedure deleted approximately 60 percent of the sample and left me with slightly more than 2,000 non-participants.[20]

---

[19] By restricting the program period to represent a second 'job seeker category', heterogeneity in the participants' unemployment history could be reduced. Moreover, choosing the *first* program for every unemployed also appears to be the easiest way of handling multiple program participation. For a discussion of this dynamic program evaluation problem, see among others Gerfin & Lechner, 2002, and Lechner & Miguel, 2001. I also removed observations with negative program periods or other curious dates from the complete sample of 10,579 individuals.

[20] This group of non-participants consists of individuals with, on average, longer unemployment periods than the original group of 5,000 individuals, because the risk of being excluded from the

It should be noted, however, that this group of non-participants does not necessarily represent a world without programs; such a construction is possible only in a case where the individuals know that choosing not to participate implies that they will never take part in that particular program. This is not a realistic assumption for Sweden, however, where most programs continue to exist, and the unemployed who have not succeeded in finding a job (or are de-registered from the Employment Service for some other reason) are offered new possibilities to participate.[21] Thus, in a strict sense, the 2,000 individuals in the comparison group represent the alternative not to participate but to *wait*¸ when the first chance is offered to them. But they are referred to as non-participants because they never participated in any program.

Tables A1–A2 in the Appendix report descriptive statistics of some selected variables for the three groups. There are clear differences in both the program characteristics as well as the individual characteristics among participants in various states. As shown in Table A1, the duration of both pre-program unemployment and the program itself are shorter among participants in labor market training as compared to youth practice participants.

Moreover, the sample of labor market training participants consists of individuals who were registered with the Employment Service quite early and thus also started the program earlier than the youth practice participants. There are also differences in age, citizenship, education and experience among the three groups.

Table A3 lists some selected statistics from the local employment offices. I assume the probability of being assigned into one of the three states to depend on, among other things, the proportion of all unemployed assigned to any program at the specific local employment office, the month before the actual assignment. That proportion may be considered a measure of how readily the office assigns individuals to programs. Furthermore, the decision between the two programs is assumed to be dependent on the ratio between participants in these programs. Given that these figures also reflect local labor market

---

sample due to a 'too late' assigned start of the program is higher, the shorter is the individual's unemployment period. This is desirable, however, because the aim is to match participants with non-participants who were unemployed long enough to be *potential* program participants.

[21] The time limit for unemployment benefits, along with the possibility of renewing benefit entitlement by participating in programs, presumably strengthen the incentives to participate when approaching the 300-day limit. Thus, at least among the entitled, the probability of participating within 300 days, given that the individual is still unemployed, is very close to unity.

conditions and/or the effectiveness of the offices in finding jobs, they should be included in the propensity estimations.

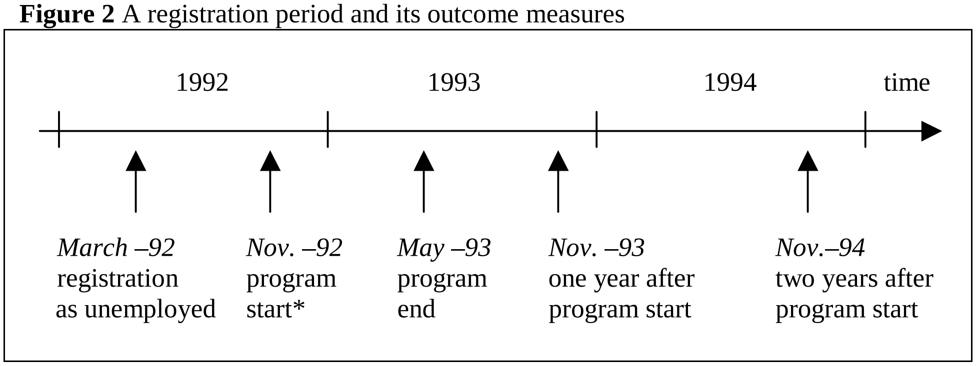
As expected, the share of youth practice assignments is above the country average in the sample of youth practice participants. A corresponding pattern holds for training. Somewhat surprisingly, though, the total number of program assignments is below the country average in all three samples. As expected, however, the ratio is lowest in the sample of non-participants.

## 3.5 What is the outcome of interest?

An explicit aim of active labor market policy is to improve the employability of the unemployed people. Hence, a higher probability of future employment and higher earnings are obvious measures of a program's success. However, especially in the case of youth, a possible track to stable future employment might be regular education. Thus, in addition to employment probability and earnings, I used the probability of transition from unemployment to studies as a third measure of success.

Earnings are measured by a continuous variable, whereas dummy variables were constructed for the other outcome measures. Figure 2 illustrates the way the various outcomes are defined for a hypothetical individual in the sample.

**Figure 2** A registration period and its outcome measures




* Program start is hypothetical for non-participants.

This individual signs up with the Employment Service in March 1992. In November 1992, she enrolls in youth practice for a period of six months. She is defined as 'employed within one year (two years) after program start' if she is de-registered from the Employment Service due to regular employment by November 1993 (1994). Analogous definitions are used for regular education.[22]

Earnings one and two years after the start of a program are measured in a slightly less precise manner because I only had access to an annual sum of earnings with no information on working hours. For an individual who enrolled in a program during the first half of a calendar year, 'earnings one year after program start' comprise the annual sum of earnings for the following calendar year. For individuals who started their program in July–December, I instead use the average of the following two calendar years to avoid counting (zero) earnings during or directly after the start of the program. Thus, for the hypothetical individual in the above example, 'earnings one year (two years) after program start' are the average of her earnings in 1993 and 1994 (1994 and 1995). Non-participants' earnings are similarly defined using the hypothetical start of a program as described in subsection 3.4.[23] Table 2 shows that, in the raw data, all average outcome measures except the long-term study effect are highest for non-participants and lowest for participants in labor market training.

[22] The data provide solely one kind of information; an individual who is both employed and a student is classified according to her 'main activity'.

[23] I also estimated the treatment effects using earnings for the subsequent calendar year for all participants independent of the starting date of the program. As expected, the results for 'earnings one year after the program start' are clearly more negative. For example, the effect of youth practice on participants is 10 percentage points lower than the effect reported in Table 3. The relative effectiveness of the programs, however, is not affected to any large extent by the different definitions of the outcome variable, nor by the effect after two years, which indicates that the earnings effect is stabilized quite rapidly after a program ends. The results can be obtained from the author on request.

**Table 2** Sample means of the outcome measures

| | Non-participants (1) | YP (2) | LMT (3) |
|---|---|---|---|
| Earnings one year after program start, (SEK) | 73,750 | 52,110 | 44,120 |
| Earnings two years after program start, (SEK) | 89,300 | 74,770 | 66,700 |
| Employed within 12 months after program start, (percent) | 37 | 29 | 24 |
| Employed within 24 months after program start, (percent) | 42 | 41 | 39 |
| Started regular studies within 12 months after program start, (percent) | 11 | 10 | 5 |
| Started regular studies within 24 months after program start, (percent) | 12 | 13 | 9 |
| Number of observations | 2,024 | 1,657 | 606 |

*Note*: SEK 100 ≈ USD 10.8 (June 2002). The low value of mean annual earnings is due to a large share of zero earnings.

# 4 Empirical application

## 4.1 Estimation of the propensity

The matching algorithm applied in this study is, in many respects, similar to that in Lechner (2001), and it is outlined in detail in Appendix B.[24] The discrete choice model for estimating the propensities is a multinomial logit model with three alternatives:

$$\text{(10)} \qquad \Pr(T_i = l) = \frac{\exp(X_i \eta_l)}{\sum_{m=0}^{M} \exp(X_i \eta_m)},$$

where $m$ indexes the choice, and $i$ the individual. $X$ is a vector of covariates. The choice alternatives are no treatment ($T$ = 0), youth practice ($T$ = 1), and

[24] Heckman, Ichimura and Todd (1998a) suggest other possible estimators. Estimators based on non-parametric kernel regressions have somewhat better asymptotic properties, whereas the main advantage of the estimators suggested by Lechner (2001) is their computational simplicity.

labor market training ($T$ = 2) and thus, $M$ = 2.[25] To test the assumption of the independence of irrelevant alternatives (IIA) underlying the multinomial logit model, I estimated binomial logit models for all three comparisons: (0,1); (0,2); and (1,2). The estimated coefficients of the binomial and multinomial models are similar and thus, the IIA assumption is considered to be sufficiently valid.[26]

The results in Table A4 in the Appendix show that the statistical significance of various explanatory variables differs across the two programs. However, the variables for pre-program unemployment history, as well as those from the local employment offices seem to be highly significant in general. It is shown in Section 5 that they play an important role for the results: excluding them from the propensity estimation would significantly alter the results.

The predictive power of the model is reported in Table A5, and I consider it satisfactory: approximately 60 percent of the observations are predicted correctly when the highest of the propensities determines the prediction. At least 70 percent of the observations in the subsamples of non-participants and youth practice participants are correctly predicted. Outcomes in the smallest subsample of labor market training, though, are merely predicted correctly in 7 percent of the cases.[27] However, the crucial outcome of interest is the match quality produced by the model, discussed in the next subsection.

A correct estimation of the average treatment effects, $\theta_0^{ml}$ and $\gamma_0^{ml}$, requires common support for the treatment and the comparison group, or $0 < P^m(x) < 1$ for all $m$ = 0,1,…, $M$. In practice, this implies that some of the observations are excluded from the sample, if the propensity distributions do not cover exactly the same interval. In other words, an observation in the subsample $m$ with an (estimated) propensity vector equal to $\{p_1^*, p_2^*, \ldots, p_M^*\}$ was excluded from the sample if any of these propensities was outside the distribution of that

[25] The specification of the multinomial logit is based on likelihood-ratio tests for omitted variables in a binary framework.
[26] To be more exact, the Hausman test (see, for example, Chapter 9 in Greene, 1993) could be applied to check whether the estimated coefficients differ significantly from each other. Nevertheless, matching based on the predicted probabilities from the binomial logit framework produces results similar to those presented in Tables 3–4 in this section. These results can be obtained from the author upon request.
[27] The distributions of the predicted propensities should also be considered. In a broad outline, a good model produces large differences in the mean of predicted propensities across the various groups. This is the case for propensities to participate in youth practice and to not participate in any program, whereas the distributions of propensities to participate in labor market training look very similar. Once again, this may be a result of the small size of this subsample compared to the other subsamples.

specific propensity in any of the other subsamples $l$.[28] Due to this common support requirement, approximately 200 observations were deleted, leaving a sample size of 4,084.

### 4.2 Matching

In the binary case of two treatments, the subsample of non-participants generally consists of a large number of observations, and it is thus plausible to use each comparison unit only once. This is not meaningful in the multiple case, since pair-wise comparisons were done across all subsamples, and for some comparisons, the potential comparison group is much smaller than the treatment group. Thus, matching was done with replacement, whereby each comparison unit was allowed to be used more than once, given that it was the nearest match for several treated units. The covariance matrix for the estimates of average effects, proposed in Lechner (2001), considers the risk of 'over-using' some of the comparison units: the more times each comparison is used, the larger is the standard error of the estimated average effect.

A detailed description of the matching algorithm is outlined in Appendix B. The pair-wise matching procedure was carried through six times altogether. Each individual in the treated subsample $m$ was matched with a comparison in subsample $l$. The criteria for finding the nearest possible match was to minimize the Mahalanobis distance of $[P^m(X), P^l(X)]$ between the two units.

Furthermore, covariates in the matched samples ought to be balanced according to the condition $X \coprod T \mid b(X)$, referred to as the balance of the covariates. Following Lechner (2001), the match quality is judged by the mean absolute standardized biases of the covariates. The results show that the covariates are sufficiently balanced by the reported model specification.

### 4.3 Results

Aggregating the pair-wise differences over the common support yields an estimate of the average treatment effects on the treated, $\theta_0^{ml}$. Average treatment effects on the population, $\gamma_0^{ml}$, are obtained by taking weighted sums

[28] This procedure assumes that there are no gaps in the empirical distributions, which is the case here.

of the treatment effects on the treated.[29] The exact expressions for $\theta_0^{ml}$ and $\gamma_0^{ml}$ are found in Lechner (2001).

### 4.3.1 Average treatment effect on the treated

Table 3 reports the effects of the six different treatments on the treated effects. Each estimated effect is reported in both absolute and relative terms. By presenting the absolute size of the effects, it is possible to compare the magnitude of the effects between the treated and the non-treated. The relative effects indicate the extent of the magnitude of the effect and help to explain how the results are changed due to the sensitivity analysis in Section 5.

[29] The weights for calculating the average population effect of treatment *m* compared to treatment *l* are based on the number of times each unit is used in all comparisons, that is, not only the comparisons between treatments *m* and *l*. Consequently, the average population effect may differ quite considerably from the average of the treatment effects on the treated, $\left(\theta_0^{ml} + (-\theta_0^{lm})\right)/2$.

**Table 3** Results for the mean treatment effect on the treated: $\theta_0^{ml} = E\left(Y^m \mid T = m\right) - E\left(Y^l \mid T = m\right)$, expressed in absolute terms.

| | (1) | (2) | (3) | (4) | (5) | (6) |
|---|---|---|---|---|---|---|
| | YP – Non | Non – YP | LMT – Non | Non – LMT | YP – LMT | LMT – YP |
| Earnings one year after program start (SEK) | **-14,565** | **16,380** | **-23,440** | **27,100** | **15,560** | -6,690 |
| | **(- 3.82)** | **(4.03)** | **(-5.22)** | **(7.16)** | **(3.92)** | (-1.50) |
| | ***-22%*** | ***29%*** | ***-35%*** | ***59%*** | ***42%*** | *-13%* |
| Earnings two years after program start (SEK) | -3,330 | 5,060 | **-14,080** | **14,450** | 11,480 | -2,170 |
| | (-0.50) | (0.76) | **(-2.33)** | **(2.22)** | (1.45) | (-0.34) |
| | *-4%* | *6%* | ***-17%*** | ***20%*** | *18%* | *-3%* |
| Employment within 12 months after program start (percentage points) | **-0.07** | **0.10** | **-0.10** | **0.11** | **0.06** | -0.01 |
| | **(-2.46)** | **(3.25)** | **(-3.30)** | **(3.77)** | **(2.03)** | (-0.16) |
| | ***-18%*** | ***37%*** | ***-30%*** | ***41%*** | ***27%*** | *-2%* |
| Employment within 24 months after program start (percentage points) | 0.02 | 0.03 | -0.01 | -0.02 | 0.03 | 0.00 |
| | (0.82) | (1.00) | (-0.31) | (-0.64) | (0.71) | (-0.05) |
| | *6%* | *8%* | *-3%* | *-5%* | *6%* | *0%* |
| Studies within 12 months after program start (percentage points) | -0.01 | 0.00 | -0.03 | **0.06** | **0.06** | **-0.04** |
| | (-0.42) | (0.05) | (-1.69) | **(3.77)** | **(3.20)** | **(-2.00)** |
| | *-7%* | *1%* | *-33%* | ***102%*** | ***127%*** | ***-42%*** |
| Studies within 24 months after program start (percentage points) | 0.01 | -0.00 | 0 | **0.05** | **0.04** | -0.02 |
| | (0.64) | (0.21) | (0) | **(2.40)** | **(2.06)** | (0.96) |
| | *10%* | *-4%* | *0%* | ***62%*** | ***51%*** | *-20%* |
| No. of observations* | 1,592 – 711 | 1,912 – 722 | 580 – 439 | 1,852 – 459 | 1,592 – 425 | 580 – 388 |

*Notes*: **Bold type** indicates statistical significance at the 5 percent level. *t*-values in parentheses, *relative effects in italics*. For a description of the dependent variables, see Figure 2. *The number of observed earnings two years after program start is somewhat lower than for the other outcome variables.

First, let us compare the programs to the state of no participation shown in the first four columns. Columns (1) and (3) report the program effects on program participants, as compared to non-participation, whereas the *potential* effects on those who did not participate in any program are listed in columns (2) and (4). The last two columns report the effects of youth practice as compared to training, first on participants in practice and then in training.

In general, there is little heterogeneity between the groups; for example, the effects of youth practice compared to non-participation are roughly the same for participants and non-participants. The short-term effects on both earnings and employment are significantly negative for both programs and all groups throughout.[30] However, after two years from the start of a program, they are more positive and the only significantly negative results are found for the effects of labor market training on earnings. Youth practice does not seem to have any effect on the probability of entering education, whereas participation in labor market training would have significantly decreased the study probability of non-participants, as shown in column (4).

A comparison of the two programs indicates that practice was better than training for those actually participating in it in terms of all outcome measures. All effects reported in column (5) are statistically significant and positive except for the long-term employment effect. For the group of participants in labor market training, the difference between the programs seems to be less significant, although in the same direction, as for youth practice participants.

### 4.3.2 Average treatment effect on the population

Table 4 reports the estimated average treatment effects on the population. These results confirm the impression given by Table 3: in the short run, both programs result in lower earnings, as well as a lower probability of employment, compared to the outcome without any program. Similar to the treatment-on-the-treated results, the negative effects more or less disappear in the course of time. Youth practice has no effect on the probability of studies, while the effect of labor market training is significantly negative.

All in all, youth practice seems to have been 'less harmful' than labor market training, except for the effect on employment probability, where the difference is statistically insignificant.[31]

---

[30] Recall that, in practice, short term also refers to the time *after* the end of a program. The immediate earnings effect during participation may well be positive since compensation is received while participating. Individuals entitled to UI receive compensation equal to the UI benefits and can therefore not gain from participation, but individuals not entitled to UI receive either nothing or some supplementary benefit as openly unemployed. Thus, for them, the compensation of SEK 338 per working day when participating does presumably exceed income as openly unemployed.

[31] The result that subsidized employment is relatively more effective than training is supported by other Swedish studies, see among others Carling & Richardson (2001).

**Table 4** Results for the average treatment effect on the population: $\gamma_0^{ml} = E\left(Y^m - Y^l\right) = EY^m - EY^l$, expressed in absolute terms.

| | (1)<br><br>YP – Non | (2)<br><br>LMT – Non | (3)<br><br>YP – LMT |
|---|---|---|---|
| Earnings one year after program start (SEK) | **-15,740**<br>**(-4.12)**<br>***-23%*** | **-27,760**<br>**(-7.46)**<br>***-39%*** | **12,020**<br>**(3.73)**<br>***30%*** |
| Earnings two years after program start (SEK) | -2,320<br>(-0.49)<br>*-3%* | 2,900<br>(0.64)<br>*5%* | -5,220<br>(-1.36)<br>*-7%* |
| Employment within 12 months after program start (percentage points) | **-0.09**<br>**(-3.00)**<br>***-23%*** | **-0.12**<br>**(-4.34)**<br>***-33%*** | 0.03<br>(1.26)<br>*11%* |
| Employment within 24 months after program start (percentage points) | -0.01<br>(-0.26)<br>*-2%* | 0.01<br>(0.24)<br>*3%* | -0.02<br>(-0.62)<br>*-5%* |
| Studies within 12 months after program start (percentage points) | 0.00<br>(0.10)<br>*0%* | **-0.06**<br>**(-3.75)**<br>***-50%*** | **0.06**<br>**(3.56)**<br>***150%*** |
| Studies within 24 months after program start (percentage points) | 0.01<br>(0.51)<br>*8%* | -0.03<br>(-1.93)<br>*-25%* | **0.04**<br>**(2.64)**<br>***44%*** |

See notes to Table 3.

# 5 Heterogeneity and sensitivity analysis

Let us now examine the robustness of the results reported in Section 4. First, the sensitivity of the results to the availability of the covariates is explored. Second, I examine heterogeneity among various types of individuals, and between various types of labor market training. Third, the definition of the outcome variables is changed in order to examine whether the negative program effects could be a result of declining search activity during participation in a program.

## 5.1 Availability of the covariates

Pre-program earnings and unemployment, local employment office variables, and education and experience were excluded one by one from the propensity estimation in order to check the sensitivity of the results to the availability of these suggested key covariates. As an example, Table 5 shows the changes in the short-term effects of youth practice on participants.

**Table 5** Sensitivity to the availability of covariates; average effects of youth practice on its participants

| | (i) | (ii) | (iii) | (iv) | (v) | (vi) |
|---|---|---|---|---|---|---|
| | Youth practice – non-participation | | | | | |
| Earnings one year after program start (SEK) | **-14,565**<br>**(- 3.82)** | **-10,900**<br>**(-3.04)** | **-8,780**<br>**(-3.81)** | **-9,000**<br>**(-3.29)** | **-13,390**<br>**(-4.99)** | **-21,640**<br>**(10.0)** |
| Employment within 12 months after program start (percentage points) | **-0.07**<br>**(-2.46)** | -0.04<br>(-1.45) | **-0.06**<br>**(-2.74)** | -0.03<br>(-1.02) | -0.02<br>(-0.57) | **-0.08**<br>**(-5.16)** |
| Studies within 12 months after program start (percentage points) | -0.01<br>(-0.42) | -0.02<br>(-1.30) | -0.02<br>(-1.53) | **-0.04**<br>**(-2.29)** | -0.03<br>(-1.59) | -0.01<br>(-0.97) |
| | Youth practice – labor market training | | | | | |
| Earnings one year after program start (SEK) | **15,560**<br>**(3.92)** | **13,950**<br>**(3.37)** | **8,530**<br>**(2.72)** | **11,980**<br>**(3.68)** | **15,270**<br>**(4.39)** | **8,000**<br>**(2,85)** |
| Employment within 12 months after program start (percentage points) | **0.06**<br>**(2.03)** | **0.07**<br>**(2.30)** | 0.04<br>(1.48) | 0.04<br>(1.41) | 0.06<br>(1.85) | **0,05**<br>**(2.41)** |
| Studies within 12 months after program start (percentage points) | **0.06**<br>**(3.20)** | **0.05**<br>**(2.74)** | **0.04**<br>**(2.08)** | 0.02<br>(1.32) | **0.06**<br>**(3.66)** | **0,05**<br>**(4.20)** |

Columns: (i) All variables included (main model); (ii) pre-program earnings excluded; (iii) pre-program unemployment excluded; (iv) local employment office variables excluded; (v) education and experience excluded; (vi) unadjusted differences.

The results are indeed sensitive to a reduction in information. The initially strong negative earnings and employment effects of youth practice as compared to non-participation become less negative when any of the covariates are excluded. Note, however, that the unadjusted differences are more negative than the initial estimates obtained by matching on all covariates. Given that our main model is correctly specified, this suggests that excluding some of the key covariates may sometimes be worse than excluding all of them.

The results further indicate that the importance of a covariate depends on the comparison group. Pre-program unemployment is an example: if it is excluded, the employment and earnings effects of youth practice become less negative when compared to non-participation, but less positive when compared to training. The covariates also play a different role for different outcome variables. For instance, controlling for education and experience seems to be important when examining the employment effect, but less so for the earnings effect.

Information on the relative program magnitude at the local employment office is always essential when measuring the effect of youth practice. Excluding pre-program unemployment also has an impact on most of the estimates. Moreover, these two variables are important for the estimated effect of labor market training on participants.[32]

## 5.2 Heterogeneity among individuals

I have examined the variation in the estimated effects *(i)* between sexes, *(ii)* among the cohorts of program participants, and *(iii)* among individuals with various propensities to participate in the programs. In short, there is some heterogeneity in all respects.

The programs generally seem to have been slightly better for women than for men. The earnings effects are more or less the same for both sexes, whereas the effects on both study and employment probability differ significantly. This holds for the effects on the treated as well as for population effects. Both programs, but labor market training in particular, have more negative short-term effects on employment for men than for women. Youth practice seems to be superior to, or at least as good as, labor market training for both sexes in all respects.

The state of the business cycle also has an impact. As shown in Table A1 in the Appendix, the dates for the start of a program (or the hypothetical start of a program) vary considerably among the individuals in the three subsamples. In the analysis in Section 4, I did not consider time variation, that is, the fact that 'one year after the start of a program' may imply early 1993 for one individual

[32] Comprehensive results for all effects can be obtained from the author on request.

and early 1995 for another. If labor demand or study opportunities vary over the period, the results may be influenced by the systematic difference in registration dates among the samples.

Besides correcting for a potential bias, an analysis where participant groups are divided into subgroups by the year of the start of a program may also reveal heterogeneity in the treatment effects among various cohorts of participants. In fact, there turns out to be a considerable amount of variation between the subgroups. The earnings and employment effects of the programs are the least favorable for those who enrolled in a program in 1992, then gradually improve for the latter cohorts of 1993 and 1994. Regarding the study effects, labor market training seems to have a clearly negative effect on the cohorts of 1992 and 1993. For the latest cohort, the employment and study effects of both programs compared to non-participation are estimated to be positive, though mainly statistically insignificant.

Heterogeneity was also examined with respect to the propensity of a treatment. A positive correlation between the propensity of a treatment and the treatment effect would indicate that the criteria for assignment are correct. Consequently, a negative correlation, or no correlation at all, implies that the selection rules are not optimal. Plotting the differences in earnings and study probability for each matched pair against the propensity of the treatment indeed reveals a great deal of variation, but no correlation. The effect of labor market training on employment as compared to non-participation seems to be slightly more positive, the higher is the propensity of labor market training. In general, however, the average effects of the programs compared to non-participation and to each other appear to be approximately the same for individuals likely and not likely to be selected into a program, respectively. This, in turn, may imply a non-optimal selection criteria.

## 5.3 Heterogeneity between various types of labor market training

Labor market training is a relatively heterogeneous program consisting of courses of various content and length. In a broad outline, the courses are divided into vocational and non-vocational categories. The latter are often preparatory in the sense that participants already have *ex ante* plans to participate in further programs. An example of such courses is Swedish for immigrants. Consequently, participants in these courses are not expected to de-register from the Employment Service as quickly as participants in vocational courses or other programs. The effects of non-vocational courses may thus be less advantageous than those of vocational courses.

Approximately 34 percent of the labor market training participants took a non-vocational course.[33] To examine whether the effects differ between the two types of training, I applied an analysis where vocational and non-vocational courses were treated as separate programs. In short, the results show that the type of training has a relatively small effect. The estimated earnings and employment effects of vocational training are only marginally higher (less negative) than those of non-vocational training. Hence, the strongly negative average effects of labor market training remain robust, even when the various types of training are considered.

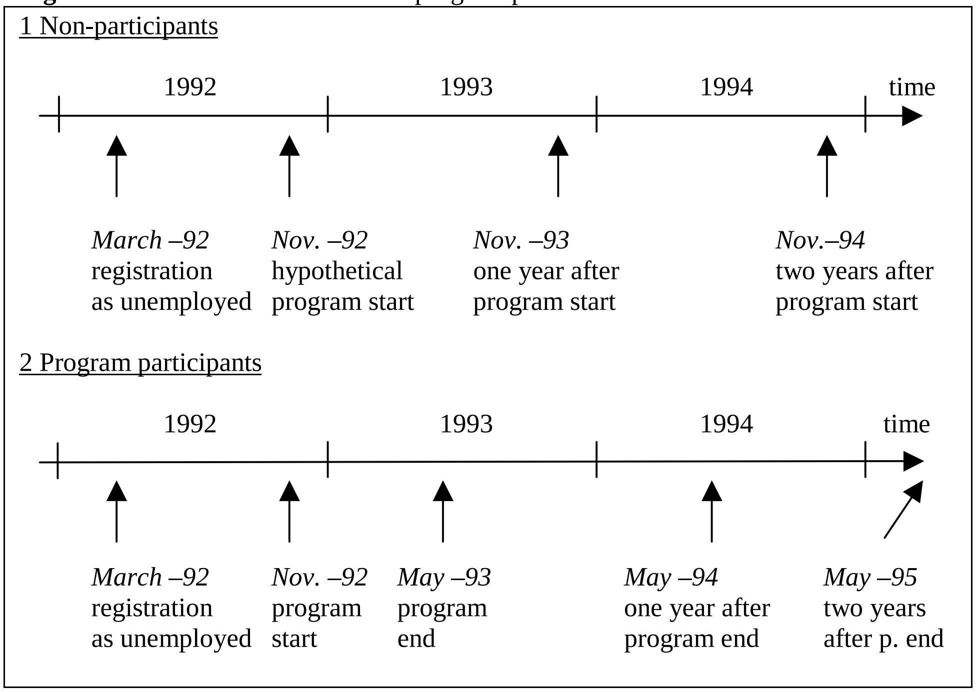
## 5.4 Definition of the outcome variables

The analysis in Section 4 is based on the assumption that individuals who participated in the programs continued their job search during the program, as required by the program regulations. Thus, the program period is included in the outcome measures of the participants. However, in practice, search activity may diminish considerably during participation in a program. (For evidence, see for example Ackum Agell, 1996, or Edin & Holmlund, 1991.) Thus, it might be argued that the program period should be excluded when defining the outcome variables, as illustrated in Figure 3.[34]

[33] In the sample of 606 labor market training participants used in this study, 518 observations include information on the type of course.

[34] Counting the time from the end of a program instead of the start may, however, imply an endogeneity bias, since the length of participation is not necessarily exogenous.

**Figure 3** Outcome measures when a program period is excluded




As before, the time span 'within one or two years after' begins with the hypothetical start of a program for non-participants, while for program participants, it instead begins *at the end of a program*. In this analysis, earnings after the start of a program are defined as follows. For all program participants with a program *end*, and for all non-participants with a hypothetical program *start* during, say, 1992, earnings one year after the *end/start* of the program are the annual sum of earnings for the calendar year 1993. Consequently, more positive effects of the programs as compared to the state of no participation would be expected. Moreover, since the average participation period in labor market training is shorter than the average period in youth practice, more positive average effects of practice as compared to training would also be expected.

The results are more or less as anticipated: the earnings and employment effects of both programs are diminished, whereas the study effects are more or less unchanged compared to the effects in Section 4. The effects of youth practice on participants are, in fact, estimated to be slightly positive, though statistically insignificant. Labor market training seems to have negative effects even when the program period is excluded, however. When estimated for the whole population, all three short run-effects are statistically significant and negative. All in all, it seems that the negative effects of youth practice may be explained by declining search activity among participants during the program, whereas further explanations are needed to account for the deleterious effects of labor market training.

# 6 Discussion on identification

The fundamental problem of an evaluator is to choose the right estimator. The decision should be based on available data and the design of the program(s), but, in the end, it will always be subjective. As Heckman, LaLonde and Smith (1999) formulated it, “there is no magic bullet”.

In this study, I have based the analysis on the conditional independence assumption, according to which the data provide information on all factors affecting selection as well as the outcome. This is a strong assumption which, as is always the case with identifying assumptions, cannot be tested directly. However, an indirect way of testing its plausibility, suggested by among others Heckman & Hotz (1989), is to apply the matching estimator to the outcome variables prior to the program period. According to such a test, an insignificant difference in the pre-program outcomes between two groups provides support for conditional independence.

However, as pointed out earlier, all available information on pre-program labor market history should to be included in estimation of the propensities. Once this is done, application of a pre-program outcome test is not meaningful, since a correct matching procedure implies that the pre-program outcome variables are balanced across the samples. That is also the case in this study. Pre-program earnings are included in the estimation of the propensities, and the results for balance of the covariates show that the differences in mean pre-program earnings are negligible among the three samples.

Exact information on employment and study spells, in turn, are not included in the data. However, the samples are constructed so that all individuals were unemployed immediately prior to the start of a program, which implies that the share of employed or students at this point in time is the same – zero – in all three samples. Moreover, the length of pre-program unemployment is used for matching and, once again, the test for balance of the covariates shows insignificant differences in this variable between groups.

Another idea for testing the plausibility of the identifying assumption, or at least the robustness of the results, could be to apply various estimators to the same problem to see whether the results differ. It is not obvious how the results of such comparisons should be interpreted, however. Let us say that all methods produce similar estimates for the program effect. What, then, does this say about the validity of the identifying assumptions underlying the various estimators? It might imply that there is no selection at all, a viewpoint supported by e.g. Heckman *et al.* (1999). If so, it would nevertheless convince the evaluator that the estimated program effect is the true one.

Another, perhaps more pessimistic way of interpreting such results is to argue that they only imply that some or all identifying assumptions are invalid without pointing out which ones. According to this view, different estimators *should* produce various results since they are based on various identifying assumptions, and something is wrong – but we do not know what – if they produce the same results.

Bearing these alternative interpretations in mind, I compared the results obtained by matching with some alternative, well-established estimators.[35] The first approach involves the standard OLS regression for the continuous dependent variable, and a probit model for the discrete dependent variables. As in the matching approach, identification of the average treatment effects in these models requires conditional independence. Moreover, the estimators are based on further parametric restrictions.

The results are reported in Table A6 in the Appendix. The set of covariates included in the OLS and the probit estimations are the same as those used to estimate the propensity scores. A comparison of Table A6 with Table 3 shows that, in this specific case, OLS and probit on the one hand, and matching on the other, produce fairly similar estimates of the average treatment effects on the population. But this is not very surprising since identification is based on the same assumption.

One substantial difference compared to the results obtained by matching, however, is an improvement in the employment effects of youth practice. Table A6 reports a practically zero short-term effect and a significantly positive effect

[35] A more extensive account of these approaches and the results is available from the author on request.

in the long run, whereas the effects obtained from the matching framework are clearly more negative. Consequently, the difference between the employment effects of practice and training is more obvious in Table A6. Moreover, the long-term earnings effect of labor market training is estimated to be significantly negative by OLS, whereas matching obtains a zero effect. These differences are presumably explained by the parametric restrictions underlying the OLS and probit estimations. Matching allows for heterogeneity in the treatment effects in a more flexible way.

A second approach applied to the continuous dependent variable – earnings – is a multinomial generalization of the classical Heckman two-stage model presented by Lee (1983) and called a polychotomous selectivity model. The Lee model is similar to other selectivity models in that it is designed to adjust for both observed and unobserved selection bias. Thus, it does not require the conditional independence assumption to be valid. However, it rests on other strong assumptions, among them linearity in the outcome variable and joint normality in the error terms.

The results are shown in Table A7. The multinomial logit model underlying the inverse Mill’s ratios is exactly the same as the one used to estimate the propensity scores. The local employment office variables are now only assumed to affect the selection into programs but not earnings and are thus excluded from the earnings equation.

The results show fewer negative effects of the programs than matching. The difference between the effects of practice and training is also diminished. The long-term effect of labor market training is estimated to be less favorable than in the short run. Drawing conclusions about the existence of unobserved heterogeneity is not straightforward, however, because the standard errors for the parameter estimates for selection adjustment terms are very large. The precision of the estimates of the treatment effects is also low. In sum, the results seem to suggest that there may be some unobserved heterogeneity between the samples that implies a (moderate) negative bias in the estimated program effects presented in previous sections of this paper, but the evidence is not unequivocal.

# 7 Conclusions

The purpose of this study has been to evaluate labor market programs for youth in Sweden using three measures of effectiveness: post-program annual earnings, employment probability and the probability of entering regular education. More precisely, the programs evaluated are youth practice and labor market training. The age group examined is 20–24.

Identification of the average treatment effects is based on the *conditional independence assumption* (CIA), whereby participation in the various treatments, including the no-treatment state, is independent of the post-program outcomes conditional on observable exogenous factors. The results from the main analysis suggest that both youth practice and labor market training have negative short-term effects on earnings and employment, where 'short-term' refers to one year after the start of a program. Two years after the start of a program, however, the effects are no longer as obvious; most estimates for employment and earnings are statistically insignificant at the five-percent level. As regards the third measure of effectiveness, the probability of regular education, the results show no significant effects of youth practice, whereas labor market training may have had a negative effect, at least in the short run. Finally, a comparison of the two programs suggests that practice was better – or 'less harmful' – than training.

How robust are these results? Beginning with the question of identification, neither the pre-program outcome tests nor the comparison with results from other methods seem to give any reason to seriously doubt the plausibility of the CIA in this context. The traditional two-stage selectivity model does indeed yield somewhat different results for both programs than matching, at least in the short run, but the point estimates are nevertheless negative. Moreover, drawing conclusions from such methodological comparisons is not straightforward. Since no direct test for the fundamental identifying assumptions is available, it is ultimately up to the reader to judge the results by weighing in the institutional setting and the available data.

Sensitivity analysis in the matching framework confirms the presumed importance of controlling for pre-program earnings and unemployment, as well as education and experience in the propensity estimation: excluding any of these variables changes the estimated program effects, which generally become more positive. As concerns the choice of model, matching on the conditional propensities obtained from binomial logit estimations yields results very similar to those obtained by the multinomial logit model.

The effects are shown to be heterogeneous for various types of individuals, however. The effects are more favorable – less negative – for women than for men, and the effect of labor market training on earnings and employment seems to have been somewhat less negative for those who took a vocational course than for participants in non-vocational courses of a more preparatory nature.

An attempt to control for variation with respect to the business cycle suggests an additional source of heterogeneity. The results from separate analyses of the individuals enrolling in the programs during 1992, 1993, and 1994 show that the effects are more positive, the later the start of a program, and thus the better the business cycle.

Hence, the results from the sensitivity and heterogeneity analyses suggest that in the total sample of 4,000 individuals, there are subsamples for which the effects are not as negative as they are for the aggregate, on average. Moreover, a plausible explanation, provided by the sensitivity analysis, for the negative or non-existing earnings and employment effects of youth practice is that participants put less or no effort into finding a job during the program, despite regulations requiring active job search. This hypothesis is also supported by the fact that already after two years the effects exhibit quite remarkable improvement.

All in all, neither of the youth programs seems to work as intended. In an international perspective, this is not surprising. Surveys on existing evaluation studies by Martin (1998) and Heckman *et al.* (1999) show that most of the OECD countries have failed in active labor market programs for the youth. What could be the reason for these poor effects?

The results for youth practice might be explained by insufficient planning and follow-up, as pointed out in several implementation studies, as well as by low-qualified tasks that did not provide any human capital accumulation. Moreover, the results from the analysis of business-cycle variation may suggest that these problems were more severe when the program was relatively new. Given that search activity was very low during program participation, it seems to be more or less expected that the effect did not turn out to be positive.

An explanation for the negative results of labor market training requires more than what is suggested for youth practice, however. The program has existed for decades, and thus 'start-up problems' are not the answer.

Furthermore, excluding the program period still produces significantly negative effects. One potential explanation is that the courses simply do not fit the employers' requirements for labor, and that training thus has both professional and regional 'lock-in' effects on participants.

What policy conclusion can be drawn from these results? To find the answer, recall the interpretation of the non-treatment state described in Section 3. The institutional setting in Sweden implies that basically all the unemployed are assigned to labor market programs, given that they are unemployed long enough. Consequently, the group of non-participants collected from the database *does not* represent a world without active labor market programs; when deciding not to participate, these individuals know that they can – and probably will – enter a program at a later stage.

Thus, *it is incorrect to draw the conclusion that participants would have been better off had there been no programs at all*. Instead, my results suggest that it was better to wait and postpone the decision to participate.[36] The results may also be interpreted as good marks for the local employment offices' job-seeking service for openly unemployed. Moreover, they suggest that workplace practice is more effective than pure training, a result also found in several other Swedish studies.

[36] The timing of programs, in the sense of whether it is better to participate early or late in an unemployment spell, is an interesting field for a future study. An example, along with a detailed discussion of the problems in identifying the no-treatment state, is provided in Sianesi (2002). Unfortunately, the data available for my study do not provide enough information to convince me that identifying such effects is possible.

## References

Ackum, S (1991), "Youth unemployment, labor market programs and subsequent earnings", *Scandinavian Journal of Economics* 93(4), 531-541.

Ackum Agell, S (1996), "Arbetslösas sökaktivitet" (Search activity of the unemployed), FIEF Reprint Series No. 106, reprinted from Aktiv Arbetsmarknadspolitik (Active labor market policy), SOU 1996:34.

Angrist, J, G Imbens & D Rubin (1996), "Identification of causal effects using instrumental variables", *Journal of the American Statistical Association* 91, 444-455.

Aschenfelter, O (1978), "Estimating the effect of training programs on earnings", *Review of Economics and Statistics* 60, 47-57.

Carling, K & K Richardson (2001), "The relative efficiency of labor market programs: Swedish experience from the 1990's", IFAU Working Paper 2001:2, Institute for Labor Market Policy Evaluation.

Dehejia, R & S Wahba (1999), "Causal effects in non-experimental studies: Re-evaluating the evaluation of training programs", *Journal of the American Statistical Association* 94, 1053-1062.

Dahlberg, M & A Forslund (1999), "Direct displacement effects of labour market training programmes: The case of Sweden", IFAU Working Paper 1999:7, Institute for Labor Market Policy Evaluation.

Edin, P-A & B Holmlund (1991), "Unemployment, vacancies and labor market programs: Swedish evidence", in F Padoa-Schioppa (ed), *Mismatch and labor mobility*, Cambridge University Press.

Eriksson, M (1997), "Comparison of compensatory and non-compensatory models for selection into labour market training", Umeå Economic Studies, Umeå University.

Gerfin, M & M Lechner (2002), "Microeconometric evaluation of the active labour market policy in Switzerland", forthcoming in *Economic Journal*.

Greene, W (1993), *Econometric analysis*, Prentice Hall.

Hallström, N-E (1994), "Ungdomspraktikens implementering. En utvärdering av ungdomspraktikens genomförande i åtta kommuner i tre län" (Implementation of youth practice. An evaluation of youth practice in eight municipalities in three counties), EFA report 28, Ministry of Labor.

Heckman, J & J Hotz (1989), "Choosing among alternative nonexperimental methods for estimating the impact of social programs: The case of manpower training", *Journal of the American Statistical Association* 84 (408), 862-880.

Heckman, J, H Ichimura & P Todd (1998a), "Matching as an econometric evaluation estimator", *Review of Economic Studies* 65, 261–294.

Heckman, J, H Ichimura, P Todd & J Smith (1998b), "Characterizing selection bias using experimental data", *Econometrica*, 66 (5), 1017-1098.

Heckman, J, R LaLonde & J Smith (1998), "The economics and econometrics of active labor market programs", in O Aschenfelter O & D Card (eds), *Handbook of labor economics*, Volume III, North-Holland.

Holland, P (1986), "Statistics and causal inference", *Journal of the American Statistical Association* 81, 945-970.

Hotz, J, G Imbens & J Mortimer (1999), "Predicting the efficacy of future training programs using past experience", NBER Technical Working Paper, 238.

Imbens, G (2000), "The role of propensity score in estimating dose-response functions", *Biometrica* 87(3), 706-710.

Korpi, T (1994), *Escaping unemployment. Studies in the individual consequences of unemployment and labor market policy*, Ph.D. Thesis, Swedish Institute for Social Research.

Layard, R, S Nickell & R Jackman (1991), *Unemployment, macroeconomic performance and the labor market*, Oxford University Press.

Lechner, M (1999), "Earnings and employment effects of continuous off-the-job training in East Germany after unification", *Journal of Business & Economic Statistics* 17, 74-90.

– (2001), "Identification and estimation of causal effects of multiple treatments under the conditional independence assumption", in M Lechner & F Pfeiffer (eds), *Econometric evaluation of labour market policies*, Physica/Springer.

Lechner, M & R Miquel (2001), "A potential outcome approach to dynamic programme evaluation – Part I: Identification", Discussion Paper 2001-07, University of St. Gallen.

Lee, L (1983), "Generalized econometric models with selectivity", *Econometrica* 51, 507-512.

Martin, J (1998), "What works among active labor market policies: Evidence from OECD countries' experiences", Labor market and social policy – occational papers 35, OECD.

Regnér, H (1997), *Training at the job and training for a new job: Two Swedish studies.* Ph.D. Thesis, Swedish Institute for Social Research.

Rosenbaum, P & D Rubin (1983), "The central role of the propensity score in observational studies for causal effects", *Biometrika 70*, 41-55.

– (1984), "Reducing bias in observational studies using subclassification on the propensity score", *Journal of the American Statistical Association* 79, 516-524.

– (1985), “Constructing a control group using multivariate matched sampling methods that incorporate the propensity score”, *American Statistician* 39, 33-38.

Rubin, D (1974), “Estimating causal effects of treatments in randomized and non-randomized studies”, *Journal of Educational Psychology* 66, 688-701.

– (1977), “Assignment to treatment group on the basis of a covariate”, *Journal of Educational Statistics* 2, 1-26.

Rubin, D & N Thomas (1992), “Characterizing the effect of matching using linear propensity score methods with normal covariates”, *Biometrika* 79, 797-809.

Schröder, L (1995), “Ungdomars etablering på arbetsmarknaden – från femtiotalet till nittiotalet” (Establishment of youth on the labor market – from the fifties to the nineties), EFA rapport 38, Ministry of Labor.

Sianesi, B (2002), “Differential effects of Swedish active labour market programmes for unemployed adults during the 1990s”, IFAU Working Paper 2002:5, Institute for Labour Market Policy Evaluation.

## Appendix A: Descriptive statistics and estimation results

**Table A1** Descriptive statistics of registration records and pre-program characteristics

| | Non-participants (1) | YP (2) | LMT (3) |
|---|---|---|---|
| Registration with ES | | | |
| Mean | Nov –92 | Dec –92 | July –92 |
| Median | Nov –92 | Nov –92 | May –92 |
| Assigned/true duration of pre-program unemployment in days (mean) | 67.6 | 121.5 | 112.6 |
| Pre-program unemployment at least four months (percent) | 16.3 | 42.4 | 35.8 |
| Pre-program unemployment at least 270 days (percent) | 0.3 | 6.8 | 8.1 |
| Annual earnings one year before registration (mean) | 74,700 | 50,900 | 70,400 |
| Assigned/true program start | | | |
| Mean | Feb –93 | April –93 | Nov –92 |
| Median | Jan –93 | March –93 | Sept –92 |
| Duration of program in days (mean) | – | 146.6 | 131.3 |
| Number of observations | 2,024 | 1,657 | 606 |

*Notes*: *Program start* of non-participants is a hypothetical date randomly assigned by the procedure described in the text. *Duration of pre-program unemployment* of non-participants is based on this hypothetical date. SEK 100 ≈ USD 10.8 (June 2002).

**Table A2** Descriptive statistics of selected individual characteristics

| | Non-participants (1) | YP (2) | LMT (3) |
|---|---|---|---|
| Age (mean) | 22.75 | 21.46 | 22.38 |
| Female (percent) | 44 | 44 | 37 |
| Non-Nordic (percent) | 4 | 5 | 13 |
| Regional characteristics (percent): | | | |
| Forest county | 21 | 21 | 26 |
| City county | 41 | 29 | 35 |
| Other county | 39 | 50 | 39 |
| Education (percent): | | | |
| Compulsory | 14 | 12 | 18 |
| High school 1-2 years | 41 | 41 | 40 |
| High school 3-4 years | 31 | 39 | 34 |
| University | 14 | 9 | 7 |
| Specific education* (percent): | | | |
| No | 42 | 51 | 52 |
| Yes | 58 | 49 | 48 |
| Experience* (percent): | | | |
| None | 34 | 45 | 40 |
| Some | 32 | 35 | 34 |
| Good | 34 | 21 | 26 |
| Number of observations | 2,024 | 1,657 | 606 |

*Notes: Age* is an approximation for the age when registered with the Employment Service as openly unemployed. It is calculated as the difference between the year of registration and year of birth (precise data on dates of birth were unavailable). *Compulsory education* also includes individuals with less than the legally required 9-10 years. *High school education* is divided into two groups depending on duration.

* *Specific education* and *experience* refer to the qualifications required for the job applied for, with the variables based on information given by job seekers when entering the Employment Service records. For individuals who have applied for several jobs, and have thus reported various levels of education and experience, I have used the observation with the highest level of experience. Information on both education and experience is *missing* for approximately 16.1 percent of the complete sample.

**Table A3** Descriptive statistics from local employment offices, expressed as deviations from the contemporary country mean (percentage points).

| | Non-participants (1) | YP (2) | LMT (3) |
|---|---|---|---|
| Share of program participants of all registered unemployed | -1.84 | -0.77 | -0.30 |
| Share of youth practice of all program participants | -0,60 | 1.28 | -0.99 |
| Share of labor market training of all program participants | -0.05 | -1.27 | 1.48 |
| Number of observations | 2,024 | 1,657 | 606 |

*Notes:* The figures in the table were calculated as follows. For each local employment office and each month, I calculated the three various 'share of *something*' variables. Next, I took the difference from the country mean in the same month. I then took the mean of these deviations for each of the three groups. Thus, -1.84 in the first row of column (1) shows that the local employment offices of non-participants were less inclined to assign individuals to programs than all offices in the country on average.

**Table A4** Results from the multinomial logit estimations

| | Youth practice | | | Labor market training | | |
|---|---|---|---|---|---|---|
| | Coef. (1) | St.error (2) | RRR (3) | Coef. (4) | St.error (5) | RRR (6) |
| Constant | -57.7 | 8.45 | – | 0.17 | 9.81 | – |
| Personal characteristics: | | | | | | |
| Female | 0.15 | 0.08 | 1.17 | -0.14 | 0.10 | 0.87 |
| Age | 5.59 | 0.76 | 268 | -0.04 | 0.88 | 0.96 |
| Age^2 | -0.14 | 0.02 | 0.87 | -0.00 | 0.02 | 1.00 |
| Non-Nordic | 0.24 | 0.18 | 1.27 | 1.22 | 0.18 | 3.38 |
| Regional characteristics: | | | | | | |
| Forest county | -0.14 | 0.11 | 0.87 | 0.35 | 0.13 | 1.42 |
| City county | -0.61 | 0.09 | 0.54 | -0.18 | 0.12 | 0.83 |
| Education[1]: | | | | | | |
| High School 1-2 years | 0.28 | 0.13 | 1.33 | -0.16 | 0.15 | 0.86 |
| High School 3-4 years | 0.23 | 0.13 | 1.25 | -0.07 | 0.16 | 0.93 |
| University | 0.18 | 0.18 | 1.19 | -0.52 | 0.23 | 0.60 |
| Specific education[2]: | | | | | | |
| Yes | -0.27 | 0.09 | 0.76 | -0.15 | 0.12 | 0.86 |
| Missing | -0.19 | 0.14 | 0.82 | 0.03 | 0.18 | 1.03 |
| Experience[2]: | | | | | | |
| Some | -0.11 | 0.11 | 0.90 | 0.00 | 0.14 | 1.00 |
| Good | -0.37 | 0.12 | 0.69 | -0.40 | 0.16 | 0.67 |

| | | | | | | |
|---|---|---|---|---|---|---|
| Pre-program labor market status: | | | | | | |
| Duration of pre-program unemployment (days) | 0.01 | 0.00 | 1.01 | 0.01 | 0.00 | 1.01 |
| Earnings 1 year before reg. with ESR (in SEK 10,000) [3] | -0.04 | 0.01 | 0.96 | 0.00 | 0.01 | 1.00 |
| Local employment office variables[4]: | | | | | | |
| Share of program part. of all registered unemployed | 1.94 | 0.47 | 6.98 | 2.57 | 0.59 | 13.0 |
| YP of all program part. | 0.88 | 0.37 | 2.40 | 0.58 | 0.47 | 1.79 |
| LMT of all program part. | 0.77 | 0.21 | 0.93 | 1.28 | 0.59 | 3.58 |
| Missing | 0.77 | 0.21 | 2.16 | 0.90 | 0.23 | 2.45 |
| Log likelihood: -3,559.3, LR chi2 (38): 1440.9, Pseudo R2: 0.1683 | | | | | | |

*Notes*: Non-participants are used as the reference category. Columns (1) and (4) report coefficients $\beta^{YP}$ and $\beta^{LMT}$, and columns (2) and (5) show the standard errors of the estimated coefficients. Bold type indicates statistical significance at the 5 percent level. Relative risk ratios (RRR) in columns (3) and (6) report the exponentiated value of the coefficient, $\exp(\beta^{YP})$. It is interpreted as the relative probability (or risk) ratio for a one-unit change in the corresponding variable, when risk is measured as the risk of the category relative to the reference category. *Age* is an approximation for the age when registered with the Employment Service as openly unemployed. [1)] Compulsory education is the reference level. [2)] *Specific education* and *experience* refer to the qualifications required for the job applied for, with the variables based on information provided by job seekers when entering the Employment Service records. For individuals who have applied for several jobs, and have thus reported various levels of education and experience, I have collected the observation with the highest level of experience. The dummy variable *Missing* indicates the observations for which both *education* and *experience* are missing (approximately 16.1 percent of the complete sample). The reference level is no specific education or experience. [3)] ESR stands for Employment Service Register. [4)] The variables from the local employment offices were computed as deviations from the contemporaneous country mean. Missing observations are set to zero, and denoted by the dummy variable *Missing* equal to one.

**Table A5** Predictive power of the multinomial logit model

| Predicted outcome: | True outcome: Non-participation | YP | LMT | Total: |
|---|---|---|---|---|
| Non-participation | 1,541 (76.1%) | 491 (29.6%) | 332 (54.8%) | 2,364 (55.1%) |
| YP | 461 (22.8%) | 1,153 (69.6%) | 233 (38.5%) | 1,847 (43.1%) |
| LMT | 22 (1.1%) | 13 (0.8%) | 41 (6.8%) | 76 (1.8%) |
| Total: | 2,024 (100%) | 1,657 (100%) | 606 (100%) | 4,287 (100%) |

**Table A6** Results from a linear regression / probit analysis.

| | YP – Non (1) | LMT – Non (2) | YP – LMT (3) |
|---|---|---|---|
| | | OLS Regression | |
| Earnings one year after program start, (SEK) | **-10,350** **(-4.43)** | **-23,830** **(-8.03)** | **13,480** **(4.43)** |
| Earnings two years after program start, (SEK) | 90 (0.03) | **-11,680** **(-3.05)** | **11,770** **(2.94)** |
| | | Probit | |
| Employment within 12 months after program start, (percentage points) | -0.03 (-1.89) | **-0.10** **(-4.29)** | **0.07** **(2.77)** |
| Employment within 24 months after program start, (percentage points) | **0.04** **(2.32)** | -0.00 (-0.09) | 0.05 (1.87) |
| Studies within 12 months after program start, (percentage points) | **-0.02** **(-2.15)** | **-0.05** **(-4.04)** | **0.04** **(2.53)** |
| Studies within 24 months after program start, (percentage points) | -0.01 (-0.88) | **-0.03** **(-2.11)** | 0.02 (1.44) |

*Notes*: **Bold type** indicates statistical significance at the 5 percent level. Results for the probit model are reported as marginal changes *dF*/*dx*. *t*-values in parentheses. The marginal change is defined as a change in probability due to a one-unit change in the covariate, dProb(E=1)/dx or dProb(S=1)/dx. Thus, -0.01 in the last row in column (1) should be interpreted as follows. A change in the dummy variable for youth practice from 0 to 1 implies a 1 percentage point decrease in the probability of entering studies within 24 months after the start of a program.

**Table A7** Results from the estimation of Lee's selectivity model.

| | Earnings one year after program start | Earnings two years after program start |
|---|---|---|
| YP – Non-participation | -4,310 | 2,040 |
| | (-0.76) | (0.26) |
| LMT – Non-participation | -12,940 | -21,350 |
| | (-0.86) | (-1.02) |
| YP – LMT | 8,640 | 23,380 |
| | (0.56) | (1.09) |
| Selection adjustment terms: | | |
| $\lambda_1$ | -6.340 | -2,050 |
| | (-1.14) | (-0.28) |
| $\lambda_2$ | -6,870 | 6,250 |
| | (-0.73) | (0.48) |

*Notes*: Standard errors were calculated using White heteroscedasticity robust variance estimator. *t*-values in parentheses.

## Appendix B: Matching algorithm

The matching algorithm, estimators and covariance matrixes applied in this paper follow Lechner (2001). The procedure is outlined below.

1. Collect the participant samples and the largest possible sample of non-participants, and randomly assign the start of the program dates for non-participants from the distribution of participants (by month). Eliminate all non-participants assigned a date after their actual de-registration from open unemployment.

2. Specify and estimate a multinomial discrete choice model to obtain the (estimated) propensities $P(T = 0|X)$, $P(T = 1|X)$, $P(T = 2|X)$. Test for omitted variables in a binomial framework. Compute the conditional probabilities $P^{m|ml}(X)$.

3. Common support: eliminate all observations outside the defined common support.

4. Apply the following procedure to match each observation in group $T = m$ with an observation in the comparison group, $T = l$:

   (i) Choose an observation from group *m*, and remove it from that pool.
   (ii) Find an observation in group *l* that is as close as possible to the one collected in step in terms of predicted probabilities. The distance can be measured by a Mahalanobis distance metric. Alternatively, base the proximity on the conditional probability $P^{m|ml}(X)$. Do not remove that observation so that it can be used again.
   (iii) Repeat (i) and (ii) until there is no observation left in group *m*.
   (iv) Repeat (i)-(iii) for all combinations of *m* and *l*.

5. Test for the balance of the covariates. If the covariates are not balanced, refine the specification of the discrete choice model, and repeat steps 2-4.

6. Use the comparison groups formed in 4(iv) to compute the respective conditional expectations by the sample mean. Note that the same observation may appear several times in the sample.

7. Compute the estimates of the treatment effects using the results of step 6, and compute their covariance matrix

# Essay II

# Sick of being unemployed? Interactions between unemployment and sickness insurance in Sweden*

## 1 Introduction

The sickness insurance (SI) expenses have increased drastically in Sweden in the late 1990s, caused by more as well as longer absence periods due to sickness. Consequently, the public debate has been heated about reasons for the absence and measures for preventing the rising figures. The debate's focus has all but exclusively been on the employed, presumably because most of the expenses are caused by an employee's absence from work. Economic research on absence from work due to sickness in Sweden points to the same findings. Examples of studies conducted on employee absenteeism include: Broström *et al.* (1998); Cassel *et al.* (1996); Edgerton & Wells (2000); Henrekson & Persson (2001); and Johansson & Palme (1996 and 2002). Currie & Madrian (1999) summarize international research on the subject.

The recent rise in absence from work is far from the only question of interest in this context, however. Besides employed workers, unemployed people who report in sick are also able to receive SI benefits. According to government estimates for 1999, unemployed people, including students, reported about 20 % of the total sick days. Interactions between the SI system and other components of the social insurance system are very important when examining the behavior of this group.

---

* I am grateful for comments from Kenneth Carling, Per-Anders Edin, Peter Fredriksson, Bertil Holmlund, Maarten Lindeboom, and Judy Petersen, as well as seminar participants at the Department of Economics, Uppsala University, and at the Conference on Social Insurance and Pensions Research held in Aarhus, Denmark, 16-18 November 2001. I also thank Per Johansson for his programming help for the empirical analysis.

This paper examines sick periods among the unemployed, and particularly incentive effects caused by interactions between the unemployment insurance (UI) and SI systems. The interplay between various social insurance programs is a largely unexplored research area inside and outside Sweden. For example, Krueger & Meyer (2001) point out that the overlap among insurance programs is a fruitful area for future research in the US and Europe.

Institutional settings, specific for every country, define which questions are interesting to examine. In Sweden, for example, the designs of UI and SI imply two potential incentive effects. First, UI and SI benefits are based on the employee's wages before unemployment, up to a ceiling above which the benefit is constant. For most of the 1990s, the replacement ratio has been the same in both systems, whereas the ceiling for SI benefits has been about 35-40 % higher than for UI benefits. Thus an unemployed individual, who earned a high wage while employed, receives greater benefits from SI than UI and can thus benefit from reporting sick. To determine whether or not the unemployed exploit this possibility, this paper examines the evolution of sick-report rates and the length of subsequent sick periods for *high-wage* earners and *low-wage* earners, who are unemployed.

Second, for the majority of the unemployed, the UI benefit period is limited to 300 workdays. After that, the benefit expires. While receiving SI benefits, unemployed people *reserve* their UI benefits, thus postponing the expiration date. Previous studies on UI benefits in Sweden indicate that as the end of the 300 workday limit approaches, the transition rate from unemployment to employment increases (see Carling *et al.*, 1996). In this study I examine whether the UI time limit – combined with the ability to report sick to lengthen the maximum compensation period – has an effect on reported sicknesses among the unemployed.

The remainder of this paper is organized like this: section 2 presents the central features of Sweden's UI and SI systems; section 3 discusses theoretical issues; section 4 presents the data; section 5 shows the empirical strategy and results; and section 6 contains concluding remarks.

## 2 UI and SI benefits in Sweden

The benefits for UI and SI systems are income related. UI consists of two parts: a fixed *basic amount* of compensation and an *income-related* amount that is determined by previous earnings. To qualify for the income-related benefit, a person must comply with the *membership condition* and the *work condition*. To fulfil the *membership condition,* the person must have been a member of an UI fund for at least a year before unemployment, whereas the *work condition* defines the minimum number of days the person must have worked before unemployment.[1] If unemployed persons comply with just one of these conditions, they only receive the basic amount, which during the 1998-1999 study period was SEK 240 per working day (SEK 5,280 per month). Both the replacement ratio and the ceiling of the income-related UI benefits have changed several times during the 1990s, but during the 1998-1999 study period, the compensation was 80 % of previous earnings up to a ceiling of 80 % of SEK 15,950 per month.

The SI system provides income-related compensation in case of sickness. For employed workers, the employer is responsible for the compensation of the first 14 days of sickness (28 days until 31 March 1998); after that, regional social insurance offices take over. Unemployed people are also eligible for SI as long as they are registered at local employment offices as job seekers and if they were previously employed.[2] For the unemployed, the regional social insurance offices pay out the SI benefits from the beginning. For both the employed and unemployed, the first sick day is not compensated.[3] To receive additional compensation, the insured person must show a doctor's certificate

[1] The *work condition* requires that before unemployment, the person has worked at least 70 hours per month, for six months, within a 12-month period. Or the person could qualify for the condition by working at least 450 hours during a continuous six-month period.

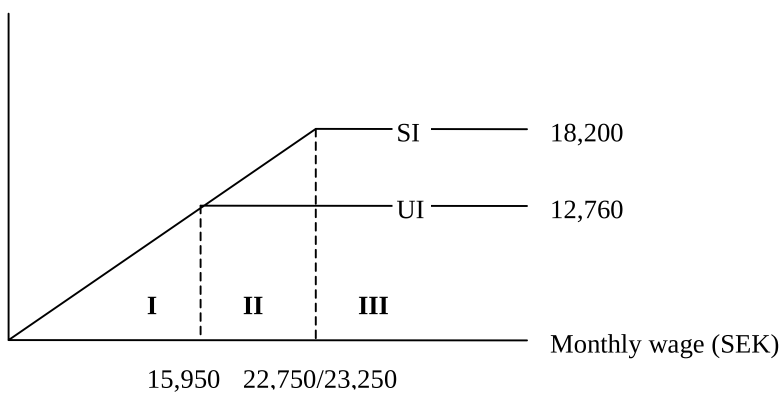
[2] Compared to the work condition of UI, the rules according to compensation from SI are not as strict, because it is required that employment before the unemployment period was *intended to continue*. In principle, even employment for one month might qualify the unemployed person for SI benefits, even though the regional social insurance offices are recommended to claim regular earnings for at least one year before unemployment (Telephone conversation with Ann-Sofie Åkerman, social insurance office Uppsala, 16 February 2001).

[3] If the previous SI period ended less than a week before, or if the person has a chronic illness and is subsequently covered by a special condition, he or she receives SI compensation on the first sick day.

after seven days and then again after four weeks. Given that the person does this, there is no formal time limit for the compensation.

For most of the 1990s, the replacement ratio of SI has been the same as in the UI system, whereas the ceiling has consistently been much higher: in 1998, it was 80 % of SEK 22,750 per month, and in 1999, it was 80 % of SEK 23,250 per month.[4] Figure 1 illustrates the structure of the two benefits. Unequal ceilings imply that an unemployed person, with a previous monthly wage above SEK 15,950, benefits from reporting sick. So the higher the wages while employed, the greater the difference between UI and SI benefits while unemployed.[5] How much the unemployed gains by reporting sick also depends on the length of the subsequent sick period due to the uncompensated first sick day. But for simplicity, Figure 1 does not account for the uncompensated day.

**Figure 1** UI and SI benefits in the late 1990s




*Note*: SEK 100 equals to about UDS 10.3 (August 2002). The limit for maximum SI benefits was SEK 22,750 in 1998 and SEK 23,250 in 1999.

Moreover, the UI benefits are time-limited. After qualifying for UI, an unemployed person up to age 56 is guaranteed to receive benefits for a maximum of 60 weeks (300 workdays), either continuously or with breaks in

[4] Formally, the maximum level of SI is defined by the *base amount*, a measure generally used as an index for social insurance. The income ceiling for SI is 7.5 times the base amount, which was SEK 36,400 in 1998 and SEK 37,200 in 1999.

[5] Many workers are provided with (often by their employer) various private agreements that increase their actual UI or SI compensation. But it is impossible to obtain information about these agreements, and thus their existence is ignored in this study.

the unemployment period. Unemployed people, age 57 and older (age 55 and older until 31 December 1997), receive benefits for 450 workdays.[6] In other words, in the beginning of the very first unemployment period, the person has 300 (450) benefit days to receive.

But very few are continuously unemployed that long; unemployment is often interrupted by for example periods of work and studies. In the beginning of a second (or subsequent) unemployment period, the number of benefit days depends on how long the break has been, and whether the person has worked during the break. If the break does not exceed a year, and the person has not worked enough to fulfil the work condition again, then he or she is entitled to what is left of the 300 (450) days after the first unemployment period. If the person has fulfilled the work condition during the break, the number of benefit days is again 300 (450). Finally, if the break exceeds a year, and the person has not fulfilled the work condition, he or she is no longer entitled to UI benefits.[7]

Until February 2001, it was possible to use active labor market programs as a measure to qualify the unemployed for new benefit periods. Programs that lasted for at least six months were enough to comply with the working condition, and according to results in Sianesi (2001), this praxis was actually used at local unemployment offices. Nevertheless, the time limit may have an effect on the behavior of unemployed people.

## 3 Theoretical issues

In the economic literature, absence from work has been traditionally analyzed within the framework of a labor supply model. Absence from work emerges in a situation where the employment contract obliges the worker to supply a certain amount of labor that exceeds the worker's optimal labor supply, determined by utility maximization over income and leisure, subject to income and time constraints. Absence is associated with a cost in terms of lost income. Examples of such models and empirical applications are provided by Allen (1981), Barmby *et al.* (1991), Barmby *et al.* (1994), and Brown & Sessions (1996).

[6] The UI system was reformed again in February 2001, and today nobody is entitled to 450 days.
[7] It is allowed to have a longer break than one year if the person has, for example, been on maternity leave or studied full-time.

In the labor supply framework, the worker's health is assumed to affect his or her marginal rate of substitution between income and leisure: the more sick the worker, the higher the value of leisure relative to income. Barmby *et al.* (1994) incorporate health explicitly in their theoretical model by including an index of sickness in the utility function of the worker: higher values of the index imply higher level of sickness. The index, denoted by $\sigma$, is assumed to be a random variable. In other words, the worker is exposed to health shocks that entail a new level of sickness, and thus a new utility maximization problem.

Given certain (realistic) assumptions on the form of the utility function, a solution to the utility maximization problem implies a unique value of sickness, $\sigma^*$, for which the worker is indifferent between work attendance and absence, given the costs and benefits associated with the two states. For levels of sickness above this *reservation level of sickness* the worker optimizes his or her utility by staying at home.

Generally, such a framework should be applicable even to the unemployed: a transition to SI benefits is associated with more leisure than collecting UI benefits, since the unemployed worker is obliged to put effort on job search while on UI but not on SI benefits. If so, then a *reservation level of sickness* can be derived for the unemployed worker determining the value of sickness for which he or she is indifferent between UI and SI benefits.

The effect of different benefit ceilings on the unemployed worker's tendency to switch to and stay on SI benefits can then be analyzed in terms of the *reservations level of sickness*. In the Barmby *et al.* (1994) model, it is straightforward to show that an increase in the sick pay lowers the *reservation level of sickness* leading to more absence from work. A higher sick pay implies a lower cost associated with absence and alters thus the worker's budget constraint. Given that leisure is a normal good, this leads to a decrease in the optimal labor supply of the worker. Similarly, an increase in SI benefits relative to UI benefits lowers the unemployed worker's *supply of job search*, implying higher probability to switch to and stay on SI benefits.

The effect of time-limited UI benefits can be considered by combining the model of Barmby *et al* (1994) with the standard job search model by Mortensen (1977).[8] One of the most important implications derived from the Mortensen model is that the unemployed worker's reservation wage declines as

[8] For modifications of the Mortensen (1977) model, see for example Burdett (1979), Mortensen (1990) and van den Berg (1990, 1994).

the worker approaches the date of benefit expiration, implying a rise in the exit rate to employment. This is due to a change in the relative value of unemployment: The value decreases as the elapsed duration of the benefit period increases, whereas the value of employment remains the same.[9]

So a general implication is that the closer the unemployed worker is to UI benefit expiration, the more attractive all other states in relation to unemployment become. Consequently, when the unemployed worker is exposed to health shocks as in the Barmby *et al.* model, and has the opportunity to reserve UI benefit days and avoid job search by collecting SI benefits, it reasonable to expect that the *reservation level of sickness* decreases as the worker approaches the expiration date, implying a higher probability to switch to and stay on SI benefits.

In sum, combining the results from the theories of absenteeism and job search, we would expect an increase in the SI benefits in relation to UI benefits to have a positive effect on the unemployed workers' probability of a transition from UI to SI, and on the duration of the subsequent SI period. Moreover, we would expect the transition rates from UI to SI, as well as the duration of SI spells, to be higher for workers that are close to UI benefit expiration.

# 4 Data and sampling

## 4.1 Data

Data for the empirical analysis are obtained from LINDA (stands for longitudinal individual database), which is a register-based database with about 300,000 individuals (for a detailed description of LINDA, see Edin *et al.*, 2001). The two main data sources for this analysis (both of which are in LINDA) are unemployment period data (AKSTAT) from unemployment insurance funds, and sickness period register (*sjukfallsregister*, SFR) from the National Social Insurance Board. Demographic variables collected from other data sources are also included in LINDA.

[9] Carling *et al.* (1996) incorporate labor market programs into the model and show that the size of the effect on the exit rate to employment now depends on how the unemployed value the programs. The empirical evidence for Sweden indicates that even in the presence of such programs, exit rate to employment increases as the unemployed approach the date for UI benefit expiration.

AKSTAT[10] consists of four tables per calendar year that contain information on all benefit payment decisions for unemployed people who are entitled to either basic-amount or income-related UI benefits.[11] Each UI benefit payment, which is paid out weekly, is regulated by two decisions: one determines the size of the benefit, based on previous wages; the other determines the duration of the benefit. In principle, each insured, unemployed person is entitled to receive compensation for 300 workdays (450 workdays for people ages 57 or older). But, these benefit days can be paid out for several unconnected spells of unemployment, which often results in a new spell starting with less than 300 (450) workdays.

Moreover, the benefit level may also change between two unemployment spells, given that the person has worked and earned a different wage. So at the start of an unemployment period, to determine the number of remaining days until the UI benefits expire, all previous periods that belong to the same 300 (450) workdays' decision must be traced back to the date of the decision. This data must then be combined with information on the actual size of the benefit's decision to determine the amount of the UI benefit during the unemployment period in question.

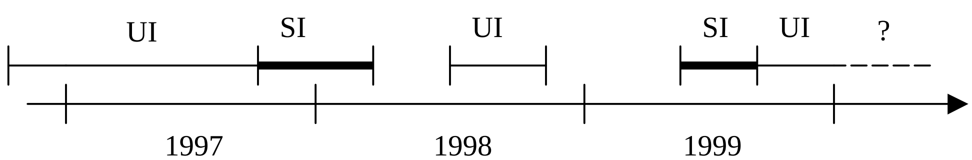
SFR includes records on SI benefits for all individuals who are entitled to them, including employed and unemployed people. For each sick report, start and end dates are included along with information on the *type* and *extent* of the benefit. Regular SI benefits for illnesses, rehabilitation benefits, and benefits for preventive care are examples of the *type* of benefit, whereas the *extent* defines whether or not the benefit is paid out on a full-time or part-time basis. Most periods are for full-time, regular SI benefits for illnesses. The data also include information on the previous wage, which defines the level of the SI benefit. But the data do *not* include additional detailed information on medical diagnoses or other indications on the state of the illnesses.

[10] A National Labor Market Board database: the AKSTAT contains administrative information taken from the various unemployment benefit funds. This includes information on funds that are paying unemployment benefits, the amounts paid, and wages from previous employment. AKSTAT was established in 1994.

[11] Most benefit payments in AKSTAT refer to UI benefit payments, either income-related or basic-amount, for those people who are openly unemployed. In addition, UI funds pay allowances for some of the active labor market programs available to the unemployed. But during the study period, the extent of these programs was very small.

Combining AKSTAT with SFR results in a database with unemployment spells for the 1994-1997 period, and both UI and SI spells for the 1997-1999 period. Figure 2 illustrates an example. The gaps in the figure imply some activity other than UI or SI, for example, work, studies or active labor market programs, but no additional information is provided in the data.[12] Of course, not all of the unemployed are observed sick during the study period: each year, about 20-23 % of about 30,000 unemployed also have a SI spell during the same year.

**Figure 2** Example of UI and SI spells in the data




## 4.2 Sampling procedure

All people who begin an open unemployment spell with UI benefits between January 1998 and August 1999 were selected from AKSTAT.[13] Thus SI and UI benefit payments during 1997 are used as control variables to eliminate heterogeneity bias in the empirical analysis.[14] These people are then followed from the start of the UI spell until their transition to SI benefits. For simplicity, all SI payments are considered the same, irrespective of the type or extent of the payment.

[12] Labor market programs could, in principle, be observed by combining the data with records from the local employment offices (HÄNDEL). But attempts to combine these data sets have revealed that they do not match well. Even if combined with HÄNDEL, the data would still not include, e.g., employment spells, and thus for the purpose of this study, the cost of combining the data – the loss of observations that do not match – is considered higher than the benefit.
[13] The UI benefit data are reported weekly, and thus the exact inflow is restricted to 5 January 1998 – 3 September 1999. Appendix A contains a detailed description of the sampling procedure.
[14] To be strict, conditioning on previous UI and SI benefit periods and treating them as predetermined variables is valid only in the absence of unobservable heterogeneity (for example in terms of health) among the individuals. Section 5 describes how the identification of the expiration effect and the effect of various ceilings takes into account the potential systematic health differences among individuals.

UI spells that end for some reason other than sickness are treated as censored – as the 1998 UI spell of the example person demonstrates in Figure 2. The first part of the analysis focuses on the probability of changing from UI to SI benefits. In the second part, the sub-sample of people who have switched to SI benefits is then used to study the probability of people returning to UI benefits.

Collecting the inflow to UI benefits between January 1998 and August 1999 results in a sample size of 17,951 individuals, out of which 829 (4.6 %) changed from UI to SI benefits. But some of the observations are excluded, either due to deficient data quality or to reduce heterogeneity in the sample. For example, the people entitled to 450 workdays of UI compensation are excluded. Differences in the maximum duration of benefits may have an effect on the behavior of the unemployed. Moreover, the behavior of people close to the age of retirement may differ from the behavior of younger people due to different choices concerning, for example, sickness pensions and early retirement pensions.

Eventually, the sample size is reduced to 12,538 UI spells (sample A in Table 1), including 575 transitions to SI benefits. The transition from SI to UI is studied with a sample of 575 people (sample B in Table 1), out of which 311 return to UI benefits. Table 1 shows descriptive statistics on selected individual characteristics in the two samples: individuals who report sick are, on average, older, less educated, and have been sick and unemployed to a greater extent during the previous year, compared with the total sample of the unemployed. The proportion of women is also higher in the sample of sick.

**Table 1** Sample characteristics (means)

| | Sample A: unemployed | Sample B: sick |
|---|---|---|
| Demographics | | |
| Age | 34.6 | 38.8 |
| Female | 0.580 | 0.619 |
| Non-Nordic citizen | 0.118 | 0.144 |
| Married | 0.314 | 0.353 |
| Children, age 15 or younger | 0.336 | 0.362 |
| Children, age 16 or older | 0.049 | 0.064 |
| Length of education | | |
| Compulsory | 0.201 | 0.304 |
| Upper secondary, max 2 years | 0.381 | 0.409 |
| Upper secondary, 3-4 years | 0.206 | 0.139 |
| University | 0.207 | 0.143 |
| Missing | 0.005 | 0.005 |
| Type of education | | |
| General | 0.275 | 0.365 |
| Aesthetic, classical | 0.043 | 0.028 |
| Pedagogic | 0.046 | 0.033 |
| Administration, trade | 0.118 | 0.179 |
| Industrial, handicraft | 0.205 | 0.186 |
| Transport, communication | 0.013 | 0.019 |
| Social and health care | 0.121 | 0.093 |
| Agriculture, woods, fishing | 0.018 | 0.014 |
| Service, civil guard, military | 0.047 | 0.042 |
| Missing, non-assignable | 0.045 | 0.040 |
| UI benefits, 1 year prior | | |
| None | 0.484 | 0.469 |
| 1-50 days | 0.247 | 0.129 |
| 51-100 days | 0.128 | 0.127 |
| More than 100 days | 0.141 | 0.275 |
| UI benefits, 2 years prior | | |
| None | 0.485 | 0.352 |
| 1-50 days | 0.180 | 0.111 |
| 51-100 days | 0.139 | 0.113 |
| More than 100 days | 0.276 | 0.424 |
| Basic amount, 1 (2) year(s) prior | 0.007 (0.004) | 0.002 (0.002) |
| SI benefits, 1 year prior | | |
| None | 0.810 | 0.502 |
| 1-50 days | 0.140 | 0.315 |
| 51-100 days | 0.026 | 0.075 |
| More than 100 days | 0.024 | 0.108 |
| No. of individuals | 12,538 | 575 |

Table 2 presents some characteristics of the pre-unemployment wage distributions, based on the information collected from AKSTAT. With respect to wages, the samples are very similar, implying that individuals with wages high enough to benefit from reporting sick are not over-represented among the sick in the raw data.

**Table 2** Previous wage characteristics

| | Sample A: unemployed | Sample B: sick |
|---|---|---|
| Monthly wage (MW), mean | 14,392 | 14,194 |
| Proportion of individuals with: | | |
| MW ≤ 15,950 | 0.685 | 0.699 |
| 15,950 < MW ≤ 22,750/23,250* | 0.291 | 0.273 |
| MW > 22,750/23,250* | 0.024 | 0.028 |

*Notes*: In AKSTAT, information on previous wages is reported either as an hourly, daily, weekly or monthly wage and marked with a code that indicates the type of wage. The variable monthly wage (MW) is then calculated according to this formula: MW = (22/5)*weekly wage; MW = 22*daily wage; MW = 22*8*hourly wage. Due to incorrect types of wage codes, some observations of MW are clearly too high. So in the empirical analysis, observations with absurdly high wages are excluded. The limit is set to SEK 50,000 per month, but even other specifications are tested without any significant effect on the results

* The limit for maximum SI benefits was SEK 22,750 in 1998 and SEK 23,250 in 1999.

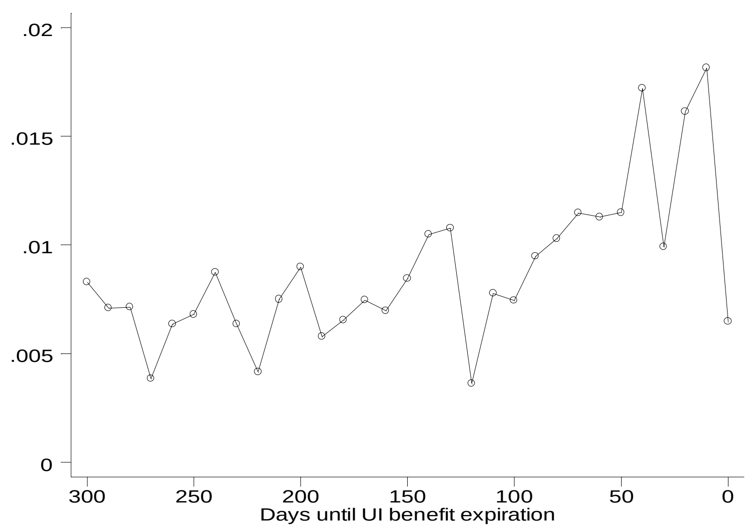
Besides wages, a variable of main interest is the number of days until the expiration date of the UI benefits at the start of unemployment and sick spells. Table 3 reveals considerable variation in that regard: nearly half of the unemployment spells start with less than 270 benefit days remaining.

Figure 3 presents the evolution of the empirical hazard rate for changing to SI benefits with respect to the number of days until the UI benefits expire. Note that time measures the distance to the expiration date. There is an upward trend in the transition rate, indicating a growing tendency to report sick as the expiration date approaches.

**Table 3** UI and SI spell characteristics

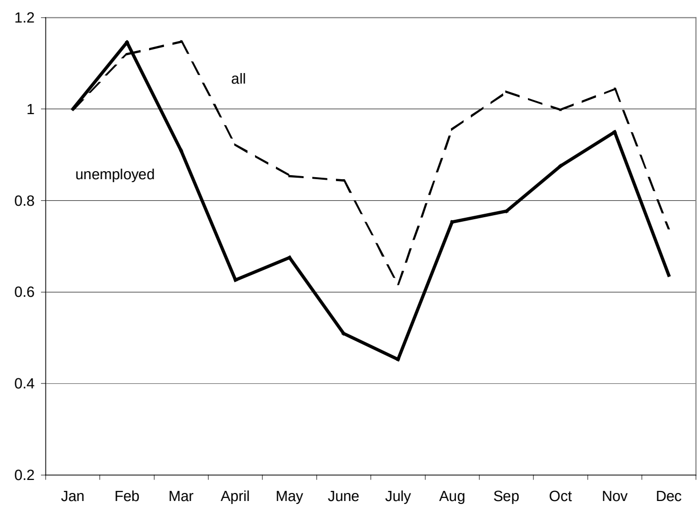
| | Sample A: UI spells | Sample B: SI spells |
|---|---|---|
| Proportion of spells lasting more than: | | |
| 1 week | 84.4 | 62.6 |
| 4 weeks | 61.8 | 38.4 |
| 8 weeks | 45.4 | 26.6 |
| 12 weeks | 30.1 | 21.6 |
| 26 weeks | 11.1 | 12.7 |
| 52 weeks | 2.1 | 6.4 |
| Proportion of spells that start with No. of days until UI benefit expiration: | | |
| Less than 31 days | 2.8 | 11.0 |
| 31-90 days | 5.9 | 13.2 |
| 91-150 days | 8.9 | 14.8 |
| 151-210 days | 11.8 | 19.8 |
| 211-270 days | 17.7 | 22.8 |
| More than 270 days | 53.0 | 19.5 |
| Transition to SI benefits no. (%) | 575 (4.6) | |
| Transition to UI benefits no. (%) | | 311 (54.1) |
| Censored | 2.9 | 13.9 |
| No. of spells | 12,538 | 575 |

**Figure 3** Transition rates to SI benefits



Finally, sick reports vary remarkably over a year. Figure 4 presents the number of sick reports among the unemployed during each month of 1999, divided by the average stock of unemployed people that month, *relative* to sick reports in January. The dotted line represents the total number of sick reports, again relative to January, of both employed and unemployed people. The figure reveals similar patterns for both groups: sick-report rates are highest between January and March and lowest during the summer months.

**Figure 4** Seasonal variations in sick reports




*Notes*: Data are obtained from the SFR that are included in the LINDA database. The number of sick reports among the unemployed is calculated as (number of direct flows from UI to SI in a month)/(average stock of UI recipients each week that month). The number of sick reports in January 1999 was 9,425 in the entire LINDA population, and 376 among the UI recipients.

# 5 Empirical analysis

## 5.1 Identification strategy

The empirical analysis consists of two parts: (1) the transition from UI to SI benefits and (2) the return from SI to UI benefits, i.e. the length of the sick period. The empirical strategy is to analyze data in terms of a discrete hazard model. The discrete hazard function is given by

(1) $$h(t|x, UIdays, wage) = \Pr(T = t|T \geq t, x, UIdays, wage), \quad t = 1,...,k-1$$

where $T = t$ denotes failure in the [$t$-1, $t$) interval. $h(t|x, UIdays, wage)$ is thus the conditional probability of failure in that interval, given that the interval is reached and given a vector of time-constant covariates $x$, the number of days until the UI benefits expiration *UIdays*, and previous wage that determines the level of UI and SI benefits; $k$ is the maximum spell length.[15] First, when focus is on the transition from UI to SI benefits, time measures the length of the UI spell, and failure is exit from UI to SI benefits. When focus is on the transition back to unemployment, time measures the length of the SI spell, and failure is exit from SI to UI benefits.

The effect of the UI benefit expiration is identified by the variation in the initial number of benefit days at the start of each unemployment spell. This variation allows us to separate the expiration effect from the duration of the unemployment spell. Thus the number of days until benefit expiration, *UIdays*, is included in the hazard function for UI spells as a time-variant variable, diminishing by one for each day of unemployment. Moreover, in the hazard for SI spells, I include the number of remaining days at the start of the spell as a time-invariant variable. In the equation for transition from UI to SI, we would expect the parameter estimate of *UIdays* to be negative: the more days that are left until expiration, the smaller the probability of sick reports. On the contrary, in the equation for transition from SI to UI, *UIdays* is expected to obtain a positive sign.

The strong connection between income and health as documented in a series of studies makes it difficult to identify the effect of differing benefit ceilings.[16] As discussed in the theoretical section, we would expect a higher probability of sickness for people above the UI benefit ceiling due to economic incentives. But higher income is shown to imply better health for the individual, indicating a lower probability of sickness for those above the ceiling. A challenge for the empirical strategy is thus to separate the *incentive* or *ceiling effect* from the *health effect*.

[15] For an overview of duration models, see e.g., Fahrmeier & Tutz (1994), Lancaster (1990) and Kiefer (1988).
[16] For a summary of studies concerning the interplay between health and labor market outcomes, see Currie & Madrian (1999).

The general identification strategy I use is to let the effect of previous wages vary below and above the UI benefit ceiling. Recall from Figure 1 that the samples could be divided into three categories on the basis of the potential benefits from a SI period. In wage categories I and III, a change in wage does not alter the benefits from reporting sick in relation to UI benefits, whereas in category II, the benefits from a sick period increase as wages increase.

Previous wages are included in hazard equations as a spline function with knots at the threshold values that equal to the ceiling values for UI and SI benefits. Thus consider the following equation of the hazard rate $h(t)$:

$$(2) \qquad h(t) = h_0(t)\exp\begin{pmatrix} f(x, z(t), \Omega) + \alpha UIdays + \beta wage + \gamma_{II} D^{II} \\ + \delta_{II} D^{II} wage + \gamma_{III} D^{III} + \delta_{III} D^{III} wage \end{pmatrix},$$

where $h_0(t)$ is the baseline hazard at time $t$, and $f(.)$ is a function of the time-invariant covariates $x$ and time-varying covariates $z(t)$. $D^{II}$ and $D^{III}$ are dummies for the wage categories, such that $D^{II} = 1$ if wage > SEK 15,950 = $w_{II}^*$, and $D^{III} = 1$ if wage > SEK 22,750 = $w_{III}^*$ during 1998, wage > SEK 23,250 = $w_{III}^*$ during 1999. Moreover, as shown in Figure 1, there are no discrete jumps at the threshold values in the difference between UI and SI benefits, and thus the spline function is restricted to be continuous. In other words, the segments are required to join at the knots, implying

$$(3a) \qquad h\left(w_{II}^* \middle| D^{II} = 0\right) = h\left(w_{II}^* \middle| D^{II} = 1\right), \text{ and}$$

$$(3b) \qquad h\left(w_{III}^* \middle| D^{III} = 0\right) = h\left(w_{III}^* \middle| D^{III} = 1\right).$$

(14b)

Equations (3a) and (3b) imply linear restrictions on the parameters $\beta$, $\delta_{II}$ and $\delta_{III}$. Inserting the restrictions into equation (2), the hazard equation can be written as:

$$(4) \qquad h(t) = h_0(t)\exp\begin{pmatrix} f(x, z(t), \Omega) + \alpha UIdays + \beta wage \\ + \delta_{II} D^{II}\left(wage - w_{II}^*\right) + \delta_{III} D^{III}\left(wage - w_{III}^*\right) \end{pmatrix},$$

where $\beta$ captures the wage effect for individuals with wages in category I; ($\beta + \delta_{II}$) for those in category II; and ($\beta + \delta_{II} + \delta_{III}$) for those in category III. (Wage - $w_{III}^{*}$) is included in the estimations as a time-variant variable to allow for the discrete change in the ceiling value of $w_{III}^{*}$ from 22,750 to 23,250 in the turn of the year 1998-1999.

The model is estimated in discrete time, assuming that both the hazard and the factors do not change within each time-interval. The log-likelihood function, for $n$ random observations, can be written as

$$
\begin{aligned}
&\ln L(\varpi_1, \varpi_2, \alpha, \beta, \delta_{II}, \delta_{III}, \eta) = \\
(5) \qquad &\sum_{i=1}^{n} \left\{ c_i \ln \left[ 1 - \exp \left[ -\exp \begin{pmatrix} x_i\varpi_1 + z_i(t)\varpi_2 + \alpha UIdays_i \\ + \beta wage_i + \delta_{II} D_i^{II}\left(wage_i - w_{II}^{*}\right) \\ + \delta_{III} D_i^{III}\left(wage_i - w_{III}^{*}\right) + \eta(t_i) \end{pmatrix} \right] \right] \right. \\
&\left. - \sum_{s=1}^{t_i} \exp \begin{pmatrix} x_i\varpi_1 + z_i(t)\varpi_2 + \alpha UIdays_i + \beta wage_i \\ + \delta_{II} D_i^{II}\left(wage_i - w_{II}^{*}\right) + \delta_{III} D_i^{III}\left(wage_i - w_{III}^{*}\right) + \eta(s) \end{pmatrix} \right\},
\end{aligned}
$$

where $\eta_t = \ln \int_t^{t+1} h_0(u)du$. In the first part of the analysis, $c_i = 1$ indicates the transition to SI benefits; in the second part, a return to UI benefits. The function is maximized with respects to its arguments.[17] The baseline hazard from UI to SI is estimated for time-intervals of 4 weeks up to 16 weeks.[18]

Besides the specification shown in equation (5), a specification with two splines is estimated, implying $D^{III} = 0$, such that the two categories, II and III, that imply a possibility to gain from a SI period, are treated as one.

[17] This model is found, e.g., in Carling *et al*. (1996) and with minor modifications in Andersson & Vejsiu (2001). Asymptotic standard errors are calculated by using the BHHH estimator.

[18] The time intervals in the baseline hazard from SI to UI are 2, 4, and 4 weeks up to 10 weeks. In the UI period data, one week corresponds to 5 days, and in the SI period data, one week corresponds to 7 days.

## 5.2 Empirical results

Table 4 reports the estimated results for the transition from UI benefits to SI benefits. Appendix B presents the estimates for the baseline hazard. The four first lines in the table present the variables of chief interest: $\beta$, $\delta_{II}$ and $\delta_{III}$, which are the components for the estimated wage effects in the three intervals; and *days until UI expiration*. Columns (1)-(3) report results for a specification with two splines – all individuals who can benefit from reporting sick are treated as one – whereas results for a specification with different splines for those below and above the SI ceiling are presented in column (4).

The parameter estimates for $\beta$, $\delta_{II}$ and $\delta_{III}$ indicate that there is a difference in the effect of wages on the hazard to SI benefits between individuals with wages below and above the ceiling for UI benefits. An increase in wages of SEK 1,000 implies a decrease in the sickness rate with 3.2-4.1 % for individuals with a wage below the UI benefit ceiling, whereas for those above the ceiling, the effect of wage increases on the transition rate to SI, ($\beta + \delta_{II}$), is non-existent as long as the group above the ceiling is treated as one.[19]

Introduction of the third spline provides further support for the incentives effect: the wage effects are shown to be positive, but only for the group with wages above the UI ceiling, but below the SI ceiling. The parameter estimates in column (4) imply that for this group, an increase in wages by SEK 1,000 increases the sick-report rate by about 7.5 %. For people above the SI ceiling, the sick rate decreases by slightly more than 6 % due to a corresponding wage rise.

This result is expected: people with wages between the ceilings are the only ones whose SI benefits increase in relation to UI benefits as wages increase. For people in the highest and lowest wage categories, the surplus between SI and UI benefits is not changed by a wage increase. Consequently, for these groups, the wage effect should only consist of the health effect, and thus be estimated negative.

For the benefit expiration effect, the results also correspond to what the theoretical discussion implied. The estimated effect of a 10 more days until the benefit expiration varies between –0.034 and –0.038, which implies that being

[19] The percentage effect is calculated as $100 * \frac{\lambda^* - \lambda}{\lambda} = 100 * \left[\exp(n\beta_X) - 1\right]$, where $\lambda^* - \lambda$ is the difference in the hazard rate when the variable X is increased by *n* units.

10 days closer to expiration is associated with about a 3-4 % higher sick-report rate. Statistically, this effect is highly significant and robust across the specifications.

Among the other variables, a person's SI benefit history is a strong, significant factor in explaining the sick-report rate. Those with 1-50 sick days before the actual unemployment spell during the last year are almost three times as likely to report sick as individuals who never had sick days. Moreover, an increase in the number of sick days is associated with a higher sick-report rate.

In contrast with previous sick days, having any UI benefits during the last year is associated with the same, or slightly lower, sick-report rate as compared to no unemployment days at all. This, together with the clearly negative estimate for days until benefit expiration, supports the hypothesis that it is not unemployment *per se* that increases the sick-report rate, but the approaching expiration date.

As in many previous studies, the sick-report rate is found to be increasing for people who are older, and higher for women than for men. The results indicate further that non-Nordic citizens have lower sick-report rates, though the parameter estimate is not statistically significant.

The regional dummies capture high sick rates in the northern parts of Sweden, as reported in Appendix B. In relation to other counties, living in a *forest county* increases the expected sick-report rate of an unemployed person by more than 60 %. Results for the seasonal dummies are similar to Figure 4: the probability of the transition to SI benefits is highest early in a year. Among the demographic variables, marital status and the existence of children do not appear to play an important role for sick reports.

Finally, none of the educational variables in columns (4) and (5) turns out to play a significant role. This is somewhat surprising, because education level should reflect the socio-economic status of the individual, which in turn is shown to be positively correlated with health (as shown in a series of previous studies). What may be less surprising is that the type of education does not have a significant effect: it is mainly included as a proxy for the industry that the individual worked for before unemployment, but the correlation between them is probably very low.

**Table 4** Estimated results, transition to SI benefits. Standard errors in italics.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Wage effect (SEK 1,000): | | | | |
| $\beta$ | -.033 *.014* | -.037 *.014* | -.035 *.015* | -.042 *.015* |
| $\delta_{II}$ | .033 *.028* | .045 *.028* | .055 *.029* | .114 *.039* |
| $\delta_{III}$ | - | - | - | -.135 *.069* |
| Days until UI expiration (10 days) | -.034 *.005* | -.035 *.005* | -.038 *.005* | -.038 *.005* |
| Regional dummies* | yes | yes | yes | yes |
| Dummies for quarter of inflow* | yes | yes | yes | yes |
| Demographic characteristics | | | | |
| Age | | .032 *.005* | .024 *.005* | .023 *.005* |
| Female | | .414 *.129* | .388 *.132* | .411 *.133* |
| Non-Nordic citizen | | -.099 *.136* | -.176 *.137* | -.171 *.137* |
| Married | | -.310 *.181* | -.293 *.184* | -.289 *.185* |
| Married and female | | .233 *.210* | .301 *.213* | .296 *.213* |
| Children, age 15 or younger | | .058 *.175* | .072 *.173* | .078 *.177* |
| Children, age 15 and female | | .055 *.207* | -.022 *.208* | -.029 *.210* |
| Children, age 16 or older | | .264 *.305* | .373 *.308* | .342 *.308* |
| Children, age 16 and female | | -.348 *.378* | -.497 *.397* | -.462 *.379* |
| Length of education | | | | |
| Compulsory | | .234 *.204* | .143 *.206* | .145 *.206* |
| Upper secondary, max 2 years | | ref. | ref. | ref. |
| Upper secondary, 3-4 years | | -.215 *.141* | -.127 *.143* | -.130 *.143* |
| University | | -.459 *.153* | -.262 *.155* | -.273 *.155* |
| Missing | | .084 *.635* | .389 *.206* | .400 *.643* |
| Type of education* | | yes | yes | yes |
| UI benefits, 1 year prior | | | | |
| 1-50 days | | | -.572 *.159* | -.580 *.159* |
| 51-100 days | | | -.271 *.162* | -.282 *.162* |
| More than 100 days | | | -.015 *.135* | -.028 *.155* |
| UI benefits, 2 years prior | | | | |
| 1-50 days | | | -.036 *.165* | -.019 *.166* |
| 51-100 days | | | -.069 *.162* | -.060 *.162* |
| More than 100 days | | | .147 *.135* | .166 *.135* |
| Basic amount, 1 (2) year(s) prior* | | | Yes | yes |
| SI benefits, 1 year prior | | | | |
| 1-50 days | | | 1.06 *.100* | 1.07 *.101* |
| 51-100 days | | | 1.24 *.170* | 1.25 *.170* |
| More than 100 days | | | 1.77 *.147* | 1.79 *.147* |
| Log-likelihood | -4,520 | -4,464 | -4,342 | -4,340 |

* Results reported in Appendix B.

Turning to the second part of the empirical analysis, Table 5 reports the estimated results for the return rates from SI to UI benefits. Appendix B presents estimates for the baseline hazard. Again, the variables of main interest are the three wage variables and the days until the UI benefits expire.

The hypothesis that being above the maximum UI benefit level would imply longer SI spells cannot be accepted; if anything, it should be rejected. Though statistically insignificant, a positive point estimate of $\delta_{II}$ implies that the estimated effect of wages may be more positive above, rather than below, the UI ceiling. This indicates that an increase in the wage is associated with either the same or a relatively higher return rate – and thus shorter SI spell – for those who would gain from having a relatively longer SI spell.

The benefit-expiration effect is estimated positive as expected. But with such large standard errors, the hypothesis – that having fewer days left until expiration is associated with longer SI spells – cannot be accepted either.

Among the other variables, only age seems to have a statistically significant effect on the length of a SI spell: an extra year implies about a 2 % lower probability of returning to UI benefits. Standard errors for all other parameter estimates are very large. Thus based on these results, the general conclusion is that very little can be said about what influences the length of a sick spell during unemployment. Why is that?

The relatively small sample size of about 600 people is a potential explanation. But I consider it difficult to increase the sample size even if the data contain information about several sick spells. For the purpose of this study, which is to examine the interplay between UI and SI systems, I want to be sure that UI benefits are the alternative income source for the sick person. Thus I am only interested in those sick spells preceded by insured unemployment. Table A1 in the Appendix shows data that include 3,769 such people. But I see an advantage in terms of reducing heterogeneity by using the same sample to study both the transition from UI to SI, and vice versa. After all, the question is how the structure of the insurance systems affects the probability of *both* becoming sick and staying sick. A potential subject for future studies is however to apply another sampling method to analyze the length of an SI spell separately from its probability of occurrence.

**Table 5** Estimated results, transition to UI benefits. Standard errors in italics.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Wage effect (SEK 1,000): | | | | |
| $\beta$ | .008 *.020* | .006 *.021* | .006 *.022* | .006 *.022* |
| $\delta_{II}$ | .008 *.039* | .034 *.039* | .031 *.040* | .028 *.040* |
| $\delta_{III}$ | - | - | - | .007 *.088* |
| Days until UI expiration (10 days) | .004 *.006* | .002 *.006* | .005 *.007* | .005 *.007* |
| Regional dummies* | yes | yes | yes | yes |
| Dummies for quarterly inflow* | yes | yes | yes | yes |
| Demographic characteristics | | | | |
| Age | | -.017 *.007* | -.016 *.007* | -.016 *.007* |
| Female | | -.082 *.182* | -.080 *.187* | -.082 *.189* |
| Non-Nordic citizen | | -.104 *.187* | -.114 *.188* | -.115 *.188* |
| Married | | -.282 *.269* | -.294 *.272* | -.293 *.274* |
| Married and female | | .283 *.307* | .307 *.310* | .305 *.311* |
| Children, age 15 or younger | | .147 *.235* | .155 *.240* | .152 *.245* |
| Children, age 15 and female | | .035 *.281* | .023 *.288* | .023 *.288* |
| Children, age 16 or older | | .663 *.403* | .652 *.404* | .654 *.404* |
| Children, age 16 and female | | -.741 *.501* | -.694 *.505* | -.695 *.505* |
| Length of education | | | | |
| Compulsory | | -.090 *.274* | -.078 *.276* | -.077 *.276* |
| Upper secondary, max 2 years | | ref. | ref. | ref. |
| Upper secondary, 3-4 years | | -.075 *.203* | -.110 *.206* | -.109 *.206* |
| University | | .085 *.200* | .063 *.203* | .064 *.203* |
| Missing | | -1.34 *1.08* | -1.34 *1.08* | -1.34 *1.08* |
| Type of education | | yes | yes | yes |
| UI benefits, 1 year prior | | | | |
| 1-50 days | | | .074 *.206* | .074 *.206* |
| 51-100 days | | | .186 *.197* | .186 *.197* |
| More than 100 days | | | .081 *.160* | .081 *.160* |
| SI benefits, 1 year prior | | | | |
| 1-50 days | | | -.088 *.139* | -.089 *.139* |
| 51-100 days | | | .240 *.256* | .240 *.256* |
| More than 100 days | | | -.335 *.230* | -.336 *.230* |
| Log-likelihood | -1,529.00 | -1,516.58 | -1,513.90 | -1,513.89 |

*Notes:* Due to a small sample size, a reduced number of control variables were included. So dummies for UI benefits two years prior and basic amount benefits, which did not turn out to be statistically significantly in Table 4, were excluded.

## 5.3 How robust are the results?

One of the key concerns in this study is identification of the ceiling effect: are the sick-report rates higher and subsequent sick periods longer for people with wages above the threshold value? The chosen strategy is to let the effect of wages vary between different wage categories, both below and above the threshold as defined by the institutional framework. Concerning the transition from UI to SI, the results show a significant difference, which is then taken as evidence for the ceiling effect on the probability of sickness among the unemployed.

To check the robustness of this result, I estimated specifications with various threshold values and report the results in Table 6. To begin with, I estimate a specification with the two *old* knots at the ceiling values, plus two additional thresholds: one below the UI ceiling at a wage equal to SEK 10,950; and one between the UI and SI ceilings at a wage equal to SEK 18,950. The results in column two support the original results for the three spline specifications in column one: the threshold values equal to the UI and SI ceilings are the most important for the variation in the wage effect. The parameter estimates in column two are no longer statistically significant, but nevertheless, they indicate that the wage effect is more positive in the interval between the two ceilings than outside of it.

Columns three to five present estimated wage effects in two spline specifications when the threshold is varied within the interval (14,950; 16,950). We would expect the difference between the wage effects to be smaller for threshold values that are slightly below or above the true ceiling value of 15,950, because the groups are *contaminated*. But this is not the case: the estimated wage effects are more or less the same in all three columns. How should this be interpreted?

The rules for the UI and SI benefit payments provide a potential explanation. UI compensation is paid out for workdays only, with a maximum of five days a week. The sum of annual earnings, divided by the sum of workdays per year, determines the size of the daily UI compensation. SI benefits, however, are paid out seven days a week, with the size determined by the sum of the annual wage divided by 365. Consequently, the amount of received SI benefits depend on whether the sick period lasts over a weekend or not. For example, compensation from SI is higher for a sick period from Thursday until Monday, than from Monday until Wednesday, even though the number of workdays – and thus the lost UI benefits – are the same.

The crucial consequence of these rules – which can explain the results in Table 6 – is that even unemployed people with previous wages below SEK 15,950 may benefit from sick periods that last for more than a weekend, whereas people with previous wages that are slightly above the ceiling do not benefit from sick periods that fall during the middle of the week.[20]

**Table 6** Wage effects with various threshold values (TV), transition to SI benefits. Standard errors in italics.

| | Several splines: | | Two splines: | | |
|---|---|---|---|---|---|
| | (1) $TV_{III}$=15,950 $TV_V$=22,750* (column 4, Table 4) | (2) $TV_{II}$=10,950 $TV_{III}$=15,950 $TV_{IV}$=18,950 $TV_V$=22,750* | (3) TV=14,950 | (4) TV=15,950 (column 3, Table 4) | (5) TV=16,950 |
| Wage effect (SEK 1,000): | | | | | |
| $\beta$ | -.042 *.015* | -.041 *.028* | -.039 .016 | -.035 *.015* | -.032 *.014* |
| $\delta_{II}$ | | -.003 *.053* | .056 *.028* | .055 *.029* | .054 *.030* |
| $\delta_{III}$ | .114 *.039* | .128 *.084* | | | |
| $\delta_{IV}$ | | -.026 *.126* | | | |
| $\delta_V$ | -.135 *.069* | -.116 *.117* | | | |
| No. of observations | 12,538 | 12,538 | 12,538 | 12,538 | 12,538 |

# 6 Concluding remarks

This paper focuses on the interplay between unemployment insurance and sickness insurance, two major parts of Sweden's social insurance system. The specifications of these two insurance programs provide possibilities for benefit arbitrage: by reporting sick, an unemployed person with previously high wages receives an SI benefit that is higher than an UI benefit. The empirical analysis presents some evidence for the arbitrage hypothesis: an increase in wages seems to have a different effect on the sick-report rate for unemployed people

[20] The data do not include that many sick spells that occur over the weekend. Among the unemployed, Tuesday seems to be the most usual day to report sick, and Friday is the day when sick spells seem to end. Among the employed, sick spells start more often on Monday and end on Friday. See Johansson & Palme, (2002).

who can benefit from reporting sick, than for those who cannot. But no such difference is found for the length of a sick spell.

A wage increase has a significantly negative effect on the sick-report rate for low-wage earners (those below the threshold for maximum UI benefits) and high-wage earners (those above the threshold for maximum SI benefits). This reflects the well-known correlation between high income and good health. But for people whose wages fall between these two thresholds, the wage effect on the sick-report rate is clearly positive. This is the only group whose relative compensation of SI compared to UI benefits increase as wages increase.

Thus given that the connection between health and income is strong for all wage levels, the statistically significant difference in the estimated slope concerning the sick-report rate can be interpreted as evidence for an incentive effect that works in the opposite direction from the health effect. To my knowledge, no previous studies have found that the health effect (the positive correlation between health and wages) would only exist for the lowest and highest wage levels but not for middle wages. Thus I find it plausible to conclude that the unequal structure of these two insurance systems seems to imply an increase in the number of sick reports.

I do not, however, find evidence for such an incentive effect that would lengthen the sick periods. In other words, economic incentives seem to play a different role for the choice to *stay* on SI benefits than for the choice to *switch* to SI benefits. It may be that the increased benefit from collecting SI lowers the threshold for a few days' sick period due to minor illness, thereby decreasing the average length of SI periods. After all, regional social insurance offices require a doctor's certificate to pay out more than a week's worth of compensation, which implies that it may be difficult to let other factors besides health determine the length of a SI spell.

But all of the parameter estimates in the model have very large standard errors, possibly due to a relatively small sample size. So very little can be said about what determines the length of a sick spell. Thus a subject for future research is to analyze the length of a sick spell in more detail, using a larger sample, and separate from the probability of occurrence.

Furthermore, the empirical analysis clearly demonstrates that the probability of a sick report increases the closer a person is to the expiration date of UI benefits. The economic explanation is that SI benefits are used as means to save UI benefit days, and thus, to postpone the drop in income after all of the UI benefit days are used. The need to postpone becomes more obvious as the expiration date approaches, thereby increasing the willingness to report sick on the few UI benefit days that remain.

Of course, the approaching drop in income may cause stress, which in turn may have effects on the person's actual health. Thus the possibility that at least some part of the increase in sick reports is explained by increased illness cannot be excluded. Nevertheless, the result adds to the practically non-existent knowledge on sickness behavior among the unemployed in Sweden.

But one should note that this study is a partial equilibrium analysis. The results do not indicate how the total number of sick reports would change if the benefits were harmonized, and neither can they be used to predict the effect of a changed duration of UI benefits on statistics about sickness among the unemployed. Such extensive reforms would most likely have effects on many transitions in the labor market, besides those between UI and SI. The *economic significance* of the results is thus not obvious.

In sum, this study serves above all as a first glance at the data, pointing to some interesting patterns in the behavior of the unemployed. More analysis, both theoretical and empirical, is still needed before we can draw distinct conclusions about which mechanisms cause this behavior. A more thorough analysis of the duration of SI periods, a comparative study of SI periods among the unemployed and employed workers, and development of a theoretical model are examples of topics for future work.

# References


Allen, S (1981), "An empirical model of work attendance", *The Review of Economics and Statistics* 71, 77-87.

Andersson, F & A Vejsiu (2001), "Determinants of plant closures in Swedish manufacturing", IFAU Working Paper 2001:6, Institute for Labour Market Policy Evaluation.

Barmby, T, C Orme & J Treble (1991), "Worker absenteeism: An analysis using microdata", *Economic Journal* 101, 214-229.

Barmby, T, J Sessions & J Treble (1994), "Absenteeism, efficiency wages and shirking", *Scandinavian Journal of Economics* 96, 561-566.

Broström, G, P Johansson & M Palme (1998), "Assessing the effect of economic incentives on incidence and duration of work absence", Working paper series in Economics and Finance. 288, Stockholm School of Economics.

Brown, S & J Sessions (1996), "The economics of absence: Theory and evidence", *Journal of Economic Surveys* 10, 23-53.

Burdett, K (1979), "Unemployment insurance as a search subsidy: A theoretical analysis", *Economic Inquiry* 17, 333-343.

Carling, K, P-A Edin, A Harkman & B Holmlund (1996), "Unemployment duration, unemployment benefits, and labor market programs in Sweden", *Journal of Public Economics* 59, 313-334.

Carling, K, B Holmlund & A Vejsiu (2001), "Do benefit cuts boost job finding? Swedish evidence from the 1990s", *Economic Journal*, 766-790.

Cassel, C-M, P Johansson & M Palme (1996), "A dynamic discrete choice model of blue collar absenteeism in Sweden 1991", Umeå Economic Studies 425, Umeå University.

Currie, J & B Madrian (1999), “Health, health insurance and the labor market”, in O Ashenfelter & D Card (eds), *Handbook of labor economics*, Volume III, North-Holland.

Edgerton, D & C Wells (2000), “A model for the analysis of sick leave in Sweden. Inference using the HUS data”, mimeo, Lund University.

Edin, P-A & P Fredriksson (2001), “LINDA - Longitudinal INdividual DAta for Sweden”, Working Paper 2001:6, Department of Economics, Uppsala University.

Fahrmeier, L & G Tutz (1994), *Multivariate statistical modelling based on general linear models*, Springer.

Henrekson M & M Persson (2001), “The effects of sick leave in changes in the sickness insurance system”, SSE/EFI Working Paper 444, Stockholm School of Economics.

Johansson, P & M Palme (1996), “Do economic incentives affect work absence? Empirical evidence using Swedish micro data”, *Journal of Public Economics* 14(1), 161-194.

Johansson, P & M Palme (2002), “Assessing the effect of public policy on worker absenteeism”, *Journal of Human Resources*, 37(2), 381-409.

Kiefer, N (1988), “Economic duration data and hazard functions”, *Journal of Economic Literature* 26, 646-679.

Krueger, A & B Meyer (2001), “Labor supply effects of social insurance”, forthcoming in A Auerbach, & M Feldstein (eds), *Handbook of public economics*.

Lancaster, T (1990), *The econometric analysis of transition data*, Cambridge University Press.

Mortensen, D (1977), “Unemployment insurance and job search decisions”, *Industrial and Labor Relations Review* 30, 505-517.

Mortensen, D (1990), "A structural model of unemployment insurance benefit effects on the incidence and duration of unemployment", in Y Weiss & G Fishelson (eds), *Advances in the theory and measurement of unemployment*, Macmillan.

Sianesi, B (2001), "An evaluation of the active labour market programmes in Sweden", IFAU Working Paper 2001:5, Institute for Labour Market Policy Evaluation.

Van den Berg, G (1990), "Nonstationarity in job search theory", *Review of Economic Studies* 57, 255-277.

Van den Berg, G (1994), "The effects of changes of the job offer arrival rate on the duration of unemployment", *Journal of Labor Economics* 12, 478-498.

## Appendix A: Sample construction

Table A1 illustrates the various steps in the sampling procedure. First, people observed using AKSTAT during the 1998-1999 study period are collected. The spells observed in AKSTAT are open unemployment with income-related UI benefits (*BERSTYP*=2), open unemployment with basic amount (*BERSTYP*=12), uncompensated qualifying period of five days (*BERSTYP*=1), or participation in these four active labour market programmes: work experience scheme (*arbetslivsutveckling ALU, BERSTYP*=3 or 13); temporary public work for older people (*offentligt tillfälligt arbete OTA, BERSTYP*=4, 14 or 44); project work (*projektarbete*, *BERSTYP*=6 or 16); and temporary severance pay (*tillfälligt avgångsersättning TAE, BERSTYP*=7, 17 or 23). Thus far, no regard is paid to the type of spell.

From the beginning, it is required that they are included in LINDA for all three years – 1997, 1998 and 1999 – to maximise chances to be able to observe their previous UI spells. The sample of 33,436 people with at least one spell during 1998-1999 is then merged with SFR (*SJUKFALLSREGISTRET*) by using the personal ID code (*BIDNR*) common to AKSTAT, SFR, and LINDA.

During 1998-1999, 10,680 people were observed with AKSTAT and SFR. But most cases, UI and SI spells are not directly connected to one other: only 4,650 spells have a direct transition from AKSTAT to SFR, and only 3,769 have changed from open unemployment with UI benefits (*BERSTYP*=2) to SI benefits. No regard is paid to the type (*FALLKOD*) or extent of the SI benefits. *FALLKOD* takes these values: (1) for regular SI benefits for illness; (3) for rehabilitation; (4) for preventive SI benefits; (5) for SI benefits for students; and various combinations of all of these. The extent of SI benefits is either (1) full-time, (3) three-quarters, (2) half-time or (4) one-quarter.

Between 5 January 1998 and 3 September 1999, 17,951 people have a UI spell as openly unemployed (hereafter referred to as UI spell), and sooner or later, about 4.6 % of these people change directly to SI benefits. But as reported in Table 1, the sample size is diminished by 5,413 persons, resulting in a sample size of 12,538 UI spells, out of which 575 include a transition to SI benefits.

**Table A1** Sample construction

| | | |
|---|---|---|
| No. of people observed in AKSTAT during 1998-1999 | | 33,436 |
| No. of people observed in both AKSTAT and SFR during 1998-1999 | | 10,680 |
| No. of people with at least one transition from AKSTAT to SI benefits | | 4,650 |
| No. of people with at least one transition from open unemployment with UI benefits (*BERSTYP*=2) to SI benefits | | 3,769 |
| No. of transitions from AKSTAT to SI benefits | | 7,421 |
| No. of transitions from open unemployment with UI to SI benefits | | 5,341 |
| | No. of spells | No. of spells with exit to SI |
| UI spell starting 5 January 1998 – 3 September 1999 | 17,951 | 829 |
| Sample size after following exclusions: | | |
| UI or SI spell history 1994-1997 incorrect | 17,801 | 818 |
| Impossible to calculate *days until UI expiration*, decision not traced back to 1994-1997 AKSTAT | 16,913 | 767 |
| Days until UI expiration negative | 15,908 | 707 |
| Days until UI expiration more than 450 | 15,870 | 701 |
| UI spell length non-positive | 13,691 | 687 |
| 450 allowed days (for people 54/57 years of age or older) | 12,633 | 576 |
| Days until UI expiration more than 310 | 12,569 | 575 |
| Previous wage higher than SEK 50,000** | 12,538 | 575 |

*Notes:* I collected only people who are included in all three LINDA samples from 1997-1999. Most of the LINDA sample is unchanged from year to year, but a small fraction is replaced because some people die or emigrate, and new cohorts and immigrants are collected into the sample.

* Until now, I have not applied an age restriction. The age varies between 18 and 66 years.

** Specifications where previous wages are imputed, instead of excluding the 31 observations, produce identical results.

# Appendix B: Tables

**Table B1** Estimates for the baseline hazard, educational, regional and seasonal variables, transition to SI benefits. Standard errors in italics.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| Regional dummies:* | | | | |
| City county | -.059 *.097* | .012 *.100* | .043 *.101* | .049 *.101* |
| Forest county | .381 *.118* | .457 *.121* | .491 *.122* | .496 *.122* |
| Other | ref. | ref. | ref. | ref. |
| Quarter of inflow into UI: | | | | |
| January-March | .296 *.097* | .286 *.100* | .139 *.104* | .139 *.104* |
| April-June | ref. | ref. | ref. | ref. |
| July-September | .122 *.124* | .196 *.127* | .156 *.131* | .153 *.131* |
| October-December | .311 *.162* | .398 *.165* | .291 *.176* | .280 *.176* |
| Type of education: | | | | |
| General | | ref. | ref. | ref. |
| Aesthetic, classical | | -.217 *.328* | -.204 *.330* | -.212 *.331* |
| Pedagogic | | .246 *.314* | .193 *.315* | .192 *.315* |
| Administration, trade | | .099 *.204* | .040 *.206* | .037 *.206* |
| Industrial, handicraft | | .118 *.207* | -.017 *.209* | -.036 *.209* |
| Transport, communication | | .336 *.362* | .270 *.366* | .274 *.366* |
| Social and health care | | .028 *.230* | -.018 *.209* | -.016 *.232* |
| Agriculture, woods, fishing | | .096 *.407* | .091 *.414* | .089 *.415* |
| Service, civil guard, military | | -.089 *.278* | -.141 *.281* | -.127 *.281* |
| Missing, non-assignable | | .137 *.289* | -.036 *.298* | -.030 *.298* |
| Baseline: | | | | |
| 1-20 workdays | -5.50 *.219* | -6.88 *.352* | -6.73 *.378* | -6.67 *.378* |
| 21-40 workdays | -6.40 *.232* | -7.82 *.364* | -7.63 *.388* | -7.57 *.389* |
| 41-60 workdays | -6.78 *.248* | -8.21 *.380* | -8.08 *.401* | -8.02 *.401* |
| 61-80 workdays | -6.78 *.260* | -8.21 *.384* | -8.13 *.408* | -8.08 *.408* |
| 81-418 workdays | -7.05 *.223* | -8.58 *.366* | -8.54 *.383* | -8.48 *.383* |
| No. of observations | 12,538 | 12,538 | 12,538 | 12,538 |

* City counties are Stockholm, Gothenburg, and Malmö (*LAN*=1, 12, 14). Forest counties are Värmland, Kopparberg, Gävleborg, Västernorrland, Jämtland, Västerbotten, and Norrland (*LAN*=17, 20-25).

**Table B2** Estimates for the baseline hazard, educational, regional and seasonal variables, transition to UI benefits. Standard errors in italics.

| | (1) | | (2) | | (3) | | (4) | |
|---|---|---|---|---|---|---|---|---|
| Regional dummies:* | | | | | | | | |
| City county | -.109 | *.130* | -.079 | *.136* | -.073 | *.137* | -.074 | *.137* |
| Forest county | -.002 | *.162* | -.075 | *.169* | -.092 | *.172* | -.093 | *.172* |
| Other | ref. | | ref. | | ref. | | ref. | |
| Quarter of inflow into UI: | | | | | | | | |
| January-March | -.092 | *.156* | -.090 | *.165* | -.087 | *.166* | -.088 | *.166* |
| April-June | ref. | | ref. | | ref. | | ref. | |
| July-September | -.221 | *.163* | -.180 | *.169* | -.206 | *.170* | -.207 | *.170* |
| October-December | -.380 | *.164* | -.308 | *.171* | -.283 | *.172* | -.284 | *.172* |
| Type of education: | | | | | | | | |
| General | | | ref. | | ref. | | ref. | |
| Aesthetic, classical | | | -.188 | *.451* | -.156 | *.456* | -.157 | *.456* |
| Pedagogic | | | .172 | *.399* | .256 | *.401* | .256 | *.401* |
| Administration, trade | | | -.078 | *.290* | -.065 | *.290* | -.062 | *.291* |
| Industrial, handicraft | | | -.127 | *.290* | -.116 | *.291* | -.115 | *.291* |
| Transport, communication | | | .377 | *.436* | .455 | *.439* | .454 | *.439* |
| Social and health care | | | .198 | *.309* | .233 | *.311* | .233 | *.311* |
| Agriculture, woods, fishing | | | -.231 | *.644* | -.245 | *.649* | -.245 | *.649* |
| Service, civil guard, military | | | .319 | *.366* | .307 | *.367* | .307 | *.367* |
| Missing, non-assignable | | | .377 | *.381* | .455 | *.390* | .455 | *.390* |
| Baseline: | | | | | | | | |
| 1-14 days | -3.25 | *.309* | -2.55 | *.504* | -2.65 | *.516* | -2.65 | *.518* |
| 14-28 days | -4.81 | *.354* | -4.07 | *.538* | -4.17 | *.549* | -4.17 | *.549* |
| 28-56 days | -5.42 | *.381* | -4.64 | *.562* | -4.75 | *.573* | -4.75 | *.573* |
| 56-715 days | -6.75 | *.365* | -5.90 | *.543* | -6.01 | *.561* | -6.01 | *.562* |
| No. of observations | 575 | | 575 | | 575 | | 575 | |

* City counties are Stockholm, Gothenburg, and Malmö (*LAN*=1, 12, 14). Forest counties are Värmland, Kopparberg, Gävleborg, Västernorrland, Jämtland, Västerbotten. and Norrland (*LAN*=17, 20-25).

# Essay III

# Does early intervention help the unemployed youth?*

## 1 Introduction

Acting on a pre-election promise, the new Swedish government declared after the election in 1994 that no youth should stay openly unemployed, i.e. not participating in any labor market program, for more than 100 days. At the time of the declaration, unemployment, including youth unemployment, reached its post-war highest level.

The declaration swiftly came into operation by the government convincing the municipalities to offer labor market programs to the youth. A municipal program was first introduced for the youngest unemployed, and after two years, a similar program even comprised the older youth, i.e. up to 24 years . This was an untraditional choice as such programs are usually run by the State (through the National Labor Market Board, AMS). But, at the time, the employment offices were under considerable pressure due to the exceptionally high unemployment rate, and putting some of the responsibility for the youth on the municipalities served as a means of diminishing the pressure on the offices.

This paper focuses on the program directed at the unemployed aged 20-24, referred to as Utvecklingsgarantin (UVG).[1] It was introduced in January 1, 1998, and is still in practice. In essence, the UVG-program is a blend of the conventional features in many other programs as it consists of vocational rehabilitation (training), work schemes, and (to a lesser extent) on-the-job training. What is novel is the fact that the youth are guaranteed an assignment

* Written together with Kenneth Carling. We gratefully acknowledge comments from Fredrik Andersson, Per-Anders Edin, Anders Forslund, Peter Fredriksson, Bertil Holmlund, Christina Lönnblad, Knut Røed, as well as seminar participants at IFAU and Dalarna University. We also thank Lena Ståhl for valuable discussions about the UVG-program and Helge Bennmarker and Lena Ståhl for help with the data collection.

[1] Henceforth we refer to this program as the UVG-program or the UVG-guarantee.

to the UVG-program no later than 100 days after becoming unemployed, given that they are still openly unemployed.

Our goal in this paper is thus to determine the *effect of a guarantee for program participation* on the subsequent labor market attachment. By guaranteeing the assignment to a program within 100 days, long-term open unemployment is avoided. It has been argued elsewhere that long-term open unemployment might be devastating for future labor market prospects. On the other hand, such a guarantee might provide an attractive alternative to regular employment, and thereby extend the time the youth stay detached from working life.

Furthermore, the guarantee may also induce an increased job-finding rate among the youth, if considered more as a threat than a guarantee. Black *et al*. (2002) provide evidence for such a pattern as they evaluate the WPRS system in the US.[2] The program implies a 'guarantee' for mandatory employment and training services to individuals with long expected unemployment spells, and they find a sharp increase in the exit from unemployment *prior* to the start of services.

To identify the causal effect of the UVG-guarantee, we make use of three conditions: first, it covers individuals aged 24 but not 25, implying that we might be able to apply a regression-discontinuity design for the study.[3] Second, the municipalities volunteered for being responsible for the UVG-program, and not all of them chose to do so. Thus, an alternative identification strategy is to compare the volunteering with the non-volunteering municipalities. Third, the data are repeated cross sections, so that we can also compare the behavior of the age group before and after the introduction of the guarantee, that is in 1997 and 1998.

The remainder of this paper is organized as follows: in Section 2, we describe the institutional settings and the UVG-program. Section 3 presents a search-theoretic framework for our empirical analysis, and Section 4 discusses the identification strategy. In Section 5, we show the empirical results, and the final section concludes.

---

[2] The initials WPRS stand for "Worker Profiling and Reemployment Services". The length of the unemployment spell of an Unemployment Insurance (UI) claimant is predicted. In order to continue receiving benefits, individuals with long predicted spells or high predicted probabilities of UI benefit exhaustion must accept to receive employment and training services early in their spell.

[3] For a discussion and applications of the regression discontinuity approach, see *e.g*. Angrist & Krueger (1999) and Hahn *et al*. (2001).

## 2 The design of the UVG-program

The UVG-program differs from traditional youth labor market programs in at least two significant ways. First, it implies a guarantee for some kind of activity within 100 days of (open) unemployment. Second, it is run by the municipalities instead of the National Labor Market Board.

In 1994, the Government had promised to prevent the youth from being unemployed for more than 100 days. By the end of 1997, the promise had still not been realized for the youth aged 20-24. The local employment offices were overcrowded by job seekers, and the caseworkers had no time to help their clients as effectively as desired. Thus, the idea to let the municipalities take over the responsibility for the unemployed youth seemed attractive for at least two reasons. The local employment offices would be able to allocate more resources to taking care of the adult unemployed while the municipalities took care of the youth. Furthermore, many argued that a decentralization of labor market policy to the municipal level – closer to the local labor market – would improve the quality of the programs.

From January 1, 1998, the municipalities could voluntarily agree with the National Labor Market Board to provide the UVG-program for the unemployed aged 20-24.[4] Except for minor modifications and a change of name, the program is still in practice in 2002. The municipalities have the opportunity to either continue or stop providing the program at the beginning of each calendar year.

This agreement implies that the local employment offices are responsible for the youth during the first 90 days of unemployment. If the individual is still unemployed after 90 days, he or she is sent to the municipal UVG-office which, in turn, has 10 days to assign the unemployed to some (appropriate) activity.

The content of the activity varies among participants. The possibility to combine different measures in order to adjust the program to the individual is novel to UVG. According to studies on the implementation of UVG, during the first years, approximately 60 percent of the assignments were into work-place

[4] The upper age limit is set to the 25$^{th}$ birthday: if the individual is registered as unemployed more than 100 days before her 25$^{th}$ birthday , she is covered by the guarantee to be assigned to UVG. In practice, the interpretation of the age limit has varied among municipalities, which we discuss in more detail in Section 4.

practice; roughly 15 percent into training; in the rest of the cases, the program consisted of a combination of both training and practice (SK, 1999, and US, 2000). This approximately reflects the distribution of the traditional youth labor market programs provided by the National Labor Market Board.

The local employment office assigning the individual to the program pays the municipality a constant compensation of SEK 150 (USD 15.5) per participant and working day which is meant to cover the cost of administration and the actual program. Implementation studies indicate that the *actual* cost per participant has varied considerably among municipalities, possibly implying a variation in the quality of the program (SK, 1999).

Compensation to participants is not included in the above amount. The size of the compensation depends on what the individual received as openly unemployed. Individuals qualified for unemployment insurance (UI) benefits receive an amount equal to the UI benefits during UVG. This is also the case for those qualified for (means tested) social assistance. If the individual rejects an offer to participate in UVG without any acceptable reason, she can lose the benefits.[5] UVG-participants without any previous compensation for unemployment receive a moderate compensation of SEK 1,967 (USD 203) per month. All three groups thus have financial incentives to accept an UVG offer.[6]

By 1999, approximately four municipalities out of five had agreed to provide the UVG-program.[7] In most cases, the reason for *not* providing UVG – according to the municipalities themselves (SK, 1999) – was that the number of long-term unemployed aged 20-24 was low. We may thus expect the economic environment to differ systematically between the municipalities that do and do not provide the UVG-guarantee. We will return to how this selection of municipalities affects the identification of the guarantee effect.

We have access to the Employment Service database (HÄNDEL) which contains all individuals registered as job seekers from 1991 and onwards. HÄNDEL includes information on the length of spells on unemployment, as

---

[5] The rules on this issue were clear: rejection will lead to loss of benefits. But in practice, the rule was not always strictly applied. According to an implementation study (US, 1999), one third of the participants felt that they were forced into the program.
[6] Unlike other labor market programs, participation in UVG could not be used to qualify for renewed entitlement to UI benefits.
[7] This figure is based on a survey of the Swedish Municipalities' Organization, SK (1999). Information on which municipalities provided the program in 1998 is difficult to obtain, as described in the Appendix.

well as data on some individual characteristics, including information on the municipality. For a detailed description of the data, see the Appendix.

Considering the design of the program, we would expect the mean pre-program unemployment period to be reduced in municipalities providing the program. In fact, no one aged below 25 should be observed to be openly unemployed for more than 100 days. The data reveals that this was not the case, however. In 1997, roughly 25 percent of the unemployed individuals aged 22-24 were assigned to a program within the promised period. After the introduction of UVG, in 1998, the corresponding share was 30 percent. Thus, the pre-program unemployment was indeed reduced but not to the expected extent.

Why the reduction was so moderate is not clear, but the local employment offices seem to have been reluctant to assign unemployed individuals to the UVG-program, either due to a distrust towards the municipal authorities or the relatively high cost of an assignment to the UVG-program for the employment office.[8]

Thus, what treatment do we evaluate? In general, the treatment is "being covered by the UVG-guarantee". The UVG-program reduced the pre-program unemployment periods for all participants, independent of the program. It was not a large-scale program, however: in 1998, a majority of all program participants aged 22-24 years were still assigned to other programs than the UVG. Only approximately 12 percent of all participants were assigned to the UVG. The treatment thus mainly consisted of a faster assignment to some of the traditional programs but, to some extent, also of participation in a new (and possibly better) program.

## 3 Theoretical framework

In this section, we outline a simple model of an unemployed worker's job search to illustrate the expected impact of a guarantee such as the UVG-program. Let us begin by examining the situation without the UVG-guarantee, our comparison state. Two issues then affect the value of unemployment: the time limit of 300 days of the unemployment insurance (UI) benefits, and the

[8] Compared with other labor market programs, the cost of SEK 150 (USD 15.5) per participant and working day is high. Recall that the compensation to the participant is not included in this amount.

possibility to participate in all labor market programs except the UVG-program.

From previous studies, both theoretical and empirical, we know that the job finding rate increases as the benefit exhaustion is approached (see e.g. Mortensen, 1977). This is due to a decrease in the value of unemployment over time which, in turn, implies a decline in the worker's reservation wage. After the exhaustion date, the hazard is constant, given the stationarity of the wage offer distribution.

In the presence of labor market programs, however, the pattern may be different if the programs can be used to avoid UI benefit exhaustion. Until recently, this has been the case in Sweden. The evolution of the job finding rate now depends on how the unemployed worker values the program: the more attractive is the program, the smaller is the increase in the hazard rate. Theoretically, even decreasing exit rates from unemployment could be observed. Empirical evidence from Sweden suggests a slightly increasing job finding rate as the benefit exhaustion approaches, however (see Carling *et al.*, 1996).

Labor market programs may, of course, have an impact even after the assignment to the program. If programs are effective, they may lead to more job offers, implying higher job finding rates and better jobs after participation. During participation, however, the search activity is often observed to diminish, implying lower job finding rates. Better jobs after participation may also imply a lower risk of re-unemployment.

We can think of at least four potential effects of the introduction of the UVG-guarantee in this framework. Recall that time-limited UI benefits and the possibility to participate in all other programs except the UVG characterize our comparison state. First, if the UVG-program is of better quality than the other available programs – as argued by the municipalities – we should find an increase in the job finding rate and a decrease in the re-unemployment rate during and after participation. During participation, the effect also depends on how much time participants in the UVG-program can allocate to job search compared to participants in other programs.

Second, the relative effectiveness of the UVG-program may also affect the job finding rates before participation, if unemployed workers are aware of UVG being better than other programs.[9] If so, we would expect the hazard to

[9] Naturally, workers may care about other aspects than program effectiveness – for example the content of the program and the compensation level – when deciding on participation.

increase less prior to participation in the presence of the UVG. These effects should, however, be moderate, considering that only 12 percent of the participants were assigned to UVG; the majority still participated in other programs.

Third, the introduction of the time limit of 100 days *per se* may alter the form of the hazard during the first 100 days of unemployment, even if the unemployed workers value UVG as much as all other programs. Recall that rejecting an offer to participate in UVG disqualifies the unemployed from UI benefits and social assistance. Moreover, supposedly, the guarantee implies that after 100 days of unemployment, the probability of being offered the UVG-program is equal to unity. Consequently, the benefits expire after 100 days unless the individual accepts to participate in UVG. Thus, given that all programs are equally attractive to the unemployed workers, we would expect the job finding rate to increase more quickly in the presence of UVG since the UI benefits are now exhausted earlier.

Fourth, the guarantee implies a quicker assignment to programs and thus, a reduction in the pre-program unemployment spells. If long-term unemployment makes an individual less attractive for the employers or reduces her search activity, shorter pre-program unemployment spells should imply increased job-finding rates during and after program participation. Such an effect could be interpreted as a positive impact of *early* as compared to *late* participation.

In sum, the net treatment effect depends on the signs and magnitudes of these four effects. Due to the low assignment rate to the UVG-program, the third and fourth effect should dominate. Thus, we would expect to find an increase in the job finding rate, at least during the first 100 days of unemployment. Furthermore, if preventing long-term unemployment is indeed important, we should find an increase in the employment rate and a decrease in the re-unemployment rate even after the first 100 days.

# 4 Identification of the treatment effect

## 4.1 What is the comparison state?

The question in most evaluation studies is *what was the effect of the treatment compared to what would have happened had the individual not received the treatment*. The identification of such an effect requires the existence of a no-treatment state. In the previous literature, it has been argued that the design of Swedish labor market policy during the 1990s implies that such a state is difficult to identify (Sianesi, 2002. For a discussion in Swedish, see Carling & Larsson, 2000).

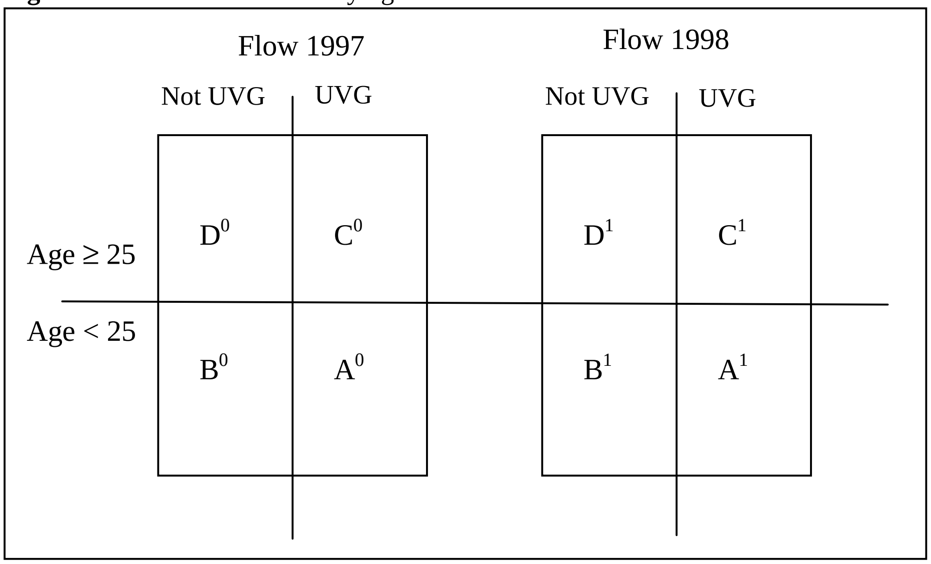
The reason, in short, is that it is virtually impossible to avoid participating in a program given that unemployment lasts sufficiently long. The probability of being assigned to a program sooner or later is close to unity. The relevant comparison state in the Swedish set-up is thus not *no treatment at all* but *no treatment now but perhaps later*. Consequently, in a strict sense, as long as no group is excluded from the treatment, the evaluation studies are only able to identify the effect of program timing.

The design of the UVG-program provides an exception, however. The age limit at the 25th birthday, and the fact that not all municipalities provide the program imply that a no-treatment state exists. The comparison in this study is thus between *a world with a guarantee* of program participation within 100 days of unemployment and *a world without such a guarantee*. Naturally, all the other programs exist in both worlds.

## 4.2 Identification

Having access to repeated cross sections before and after the introduction of the UVG-program on January 1, 1998, we can use three dimensions to identify the effect of UVG: time, age and municipality. This is illustrated in Figure 1.

**Figure 1** Dimensions for identifying the treatment effect




*Notes*: 'Not UVG' refers to a municipality that did not provide the UVG-program during 1998, whereas 'UVG' refers to a municipality that did so.

Group $A^1$, which consists of individuals younger than 25 who entered the unemployment registers during 1998 in a municipality providing UVG, is the only group directly affected by UVG. Depending on the assumptions of the indirect effects of UVG or other changes in the environment, the treatment effect can be identified by some of the following equations:

(1) $\alpha^1 = (A^1 - A^0)$

(2) $\alpha^2 = (A^1 - A^0) - (B^1 - B^0)$

(3) $\alpha^3 = (A^1 - A^0) - (C^1 - C^0)$

(4) $\alpha^4 = \{(A^1 - A^0) - (B^1 - B^0)\} - \{(C^1 - C^0) - (D^1 - D^0)\}$,

where $A^i$, $B^i$, $C^i$, $D^i$ ($i$ = 0, 1) now denote the labor market outcome for each group. $\alpha^1$ compares the outcome of the treated group with the outcome of the corresponding age group that flowed into unemployment in the same municipalities the year before UVG was introduced. This "before-after" estimator is only valid if there were no changes in the overall state of the youth labor market other than the introduction of the UVG-program between 1997 and 1998.[10]

The estimators $\alpha^2$, $\alpha^3$, and $\alpha^4$ identify the treatment effect through comparison groups. We may obtain an unbiased estimate of the treatment impact by any of these as long as the UVG-program did not indirectly affect the labor market of groups B and C, and all municipalities and age groups experienced a similar business cycle improvement.

Disregarding the indirect effects so far, let us consider the implications of the changes in the economic environment on the choice of the estimator. $\alpha^2$ is valid as long as the business cycle improved to the same extent for an age group in municipalities with and without the UVG-guarantee. If, on the other hand, changes in the economic environment differed between municipalities but were identical for the youth below and above the age of 25, $\alpha^3$ is a valid estimator. Finally, $\alpha^4$ will take care of both the municipality-specific and the age-group specific business cycle change, and thus appears to be an attractive estimator.

However, UVG may have had indirect or "spill-over" effects on groups B or C. For example, the municipalities choosing not to provide it may have put an additional effort into taking care of that age group to legitimate their choice. In that case, group B will be affected, and $\alpha^2$ will produce a downward biased estimate of the true impact of treatment, even if the change in the business cycle is the same in the different municipalities.

Furthermore, the fact that the municipal offices took over the responsibility for the young unemployed below 25 may also have allowed the employment offices to take better care of the older youth. If so, $\alpha^3$ will produce a downward biased estimate. In the presence of either of these indirect effects, $\alpha^4$ will also be biased.

The evolution of pre-program unemployment rates from 1997 to 1998 provides a measure of the indirect effects. As already noted, the program assign-

[10] The before-after estimator usually refers to a strategy for comparing an individual with herself, and thus requires longitudinal data. Heckman & Robb (1985) show that repeated cross-sectional data are sufficient to construct a before-after estimator as long as the expected no-program outcome after the introduction of the program equals the no-program outcome before the introduction. Another way of stating this assumption is to claim that the approximation error averages out.

ment rate within 120 days rose from around 25 to 30 per cent in group A.[11] Figure 2 shows the program assignment rates before and after the introduction of UVG for all four groups. The assignment rate is calculated as the number of individuals assigned within 120 days, divided by the total number of unemployed individuals excluding those exiting unemployment within 120 days for other reasons than program participation.[12]

**Figure 2** The estimated probability of being assigned to any program within 120 days (%). Standard errors in parentheses.

| | Flow 1997 | | Flow 1998 | |
|---|---|---|---|---|
| | Not UVG | UVG | Not UVG | UVG |
| Age ≥ 25 | 23.5 (0.52) | 19.6 (0.17) | 23.8 (0.50) | 20.4 (0.17) |
| Age < 25 | 31.8 (0.52) | 25.4 (0.17) | 33.9 (0.53) | 30.0 (0.19) |

*Notes:* The young age group consists of individuals aged 22-24, the old age group includes individuals aged 25-27.

As expected, the program assignment rate increased most among individuals directly affected by the introduction of UVG.[13] However, the pre-program period of the age group below 25 was also shortened in non-providing municipalities. Figure 2 also shows that these municipalities were relatively efficient

[11] By setting the limit to 120 days instead of 100, we make sure that our results do not depend on a short delay in registering the assignment.

[12] This is a sufficient measure, since we found the program assignment hazard rates to be roughly constant in the first year.

[13] The 4.6 percentage point increase corresponds to approximately 18 percent. Most of the increase seems to be due to the introduction of UVG; in 1998, around 12 percent of all program participants in our sample were assigned to the UVG-program.

in assigning individuals to programs already in 1997, which provides an explanation for why they did not conclude an agreement on the program. The increase nevertheless suggests that UVG may have had an indirect effect on group B, implying that $\alpha^2$ produces a downward biased estimate of the guarantee effect. Consequently, $\alpha^4$ may also be biased.

The program assignment rate among the older youth does not seem to have changed significantly from 1997 to 1998, however, suggesting that we should use $\alpha^3$ to estimate the treatment effect. A further argument for using $\alpha^3$ is that groups B and D are relatively small, implying a low estimate precision. However, the main reason why we prefer $\alpha^3$ to $\alpha^2$ and $\alpha^4$ is related to selection: an individual's date of birth may be regarded as random, whereas the decision made by the local authorities to provide the UVG-guarantee was far from random.

In theory, we may use the age limit of 25 to estimate the treatment effect by a sharp regression-discontinuity design. However, there are two practical problems. First, the standard errors increase as we approach the age limit and second, the interpretation of the age limit varied between municipalities and individuals, implying that in practice, the limit was not sharp. Some municipalities assigned individuals close to their 25th birthday to the program whereas other municipalities were very strict about the age limit.

# 5 Empirical results

## 5.1 The dimensions of identification in practice

The identification strategy is based on information on whether and when the individual's municipality began providing the UVG-guarantee, and the individual's age when registering with the Employment Service (ES). Furthermore, the time dimension is based on the date of entry into the ES records: individuals entering during 1997 (1998) are included in the inflow 1997 (1998). The following example illustrates the construction of the different groups.

An individual registering with ES in February 1998 is included in group A (UVG-providing municipality, age < 25) if

(i) the municipality where she lives has started providing the UVG-program some time during 1998, and if

(ii) she was at least 22 years in February 1998, and did not celebrate her 25th birthday before March 1998.

Thus, if her municipality did not start providing the UVG-program in 1998, she is included in either of the non-providing municipality groups B or D, depending on her age. Furthermore, if she was at least 25 but not yet 28 in February 1998, she is included either in group C or D.

We apply an identical age definition to the inflow in 1997. The municipality dimension is now based on the 1998 information: an individual living in a municipality that started providing the UVG-program some time in 1998 is included in group A or C, depending on her age.[14]

## 5.2 The outcome measures

We can follow the individuals in the Employment Service records until 22 June 2000. The effect of UVG is defined using various outcome measures. Since the goal of UVG – similar to all active labor market programs – is to shorten the unemployment period and increase the chances of getting a job, we examine the job finding rate during the first unemployment period in 1998 (1997 for the comparison groups $A^0$, $B^0$, $C^0$, $D^0$).

We reckon, however, that the best measure of the effect of the guarantee is obtained when the first *and* (potential) subsequent unemployment spells are examined simultaneously. The share of days an individual is registered with the Employment Service (ES) as a job seeker within a period of 1.5 years after the start of the initial unemployment period captures all spells of unemployment, employment, and regular education during that period. The variable thus provides a measure of future employment stability.[15]

## 5.3 The net treatment effect

Figure 3 shows the share of days registered in the ES records as a job seeker during the 18 months period after the start of the unemployment, thus reflecting

[14] Individuals in the late inflow in 1997 may have been covered by the UVG-program if they knew that the program was to be introduced in their municipality at the beginning of 1998. Furthermore, some of the early inflow in 1998 in group A may not have been covered by the UVG-program if their municipality did not start providing the program until the fall. Section 5.7 discusses these issues.

[15] The reason for choosing 1.5 years, or 539 days to be precise, is that we can follow the sample until 22 June 2000. Thus, the maximum period we can observe for an individual whose unemployment starts on December 31, 1998 is 539 days. Naturally, it would be preferable to follow the individuals for a longer period of time to be able to say something about the long-term effects.

the net effect of the UVG-program on unemployment. The overall decrease in the share variable reflects the improvement in the state of the labor market from 1997 to 1998. Consequently, the before-after estimator produces the most favorable estimate of the treatment effect.

The sign of the estimated effect depends on which of the estimators $\alpha^1$, $\alpha^2$, $\alpha^3$, or $\alpha^4$ is chosen. In our opinion, the best comparison group consists of individuals above 25 in UVG-providing municipalities. According to $\alpha^3$, the UVG-program moderately decreased future unemployment by 0.6 percentage points. In relative terms, this corresponds to 1.3 percent. Comparing the treatment group to the corresponding age group in non-providing municipalities yields a slightly negative but statistically insignificant estimate. Figure 2 suggests that this result is downward biased, however.

In sum, we find no evidence for a strong net effect of the UVG-program in either direction. If anything, the results suggest that the UVG-program slightly decreased the number of days registered with ES, thus indicating a small positive treatment effect.

**Figure 3** Mean of *share of ES days* (%). Standard errors in parentheses.

| | Flow 1997 | | Flow 1998 | |
|---|---|---|---|---|
| | Not UVG | UVG | Not UVG | UVG |
| Age ≥ 25 | 49.3<br>(0.37)<br>n=6,583 | 47.9<br>(0.13)<br>n=55,438 | 48.1<br>(0.36)<br>n=7,131 | 46.4<br>(0.12)<br>n=59,075 |
| Age < 25 | 48.4<br>(0.33)<br>n=8,158 | 45.9<br>(0.12)<br>n=63,545 | 46.0<br>(0.34)<br>n=7,877 | 43.8<br>(0.12)<br>n=60,884 |

$\alpha^1 = -2.1$ (0.17)
$\alpha^2 = 0.3$ (0.50)
$\alpha^3 = -0.6$ (0.25)
$\alpha^4 = 0.6$ (0.71)

## 5.4 Dynamics of the treatment effect

To explore the composition of the net effect in more detail, we have examined the duration of the first unemployment spell. We are interested in the probability of employment.[16]

Figure 4 shows the change in mean length of the first unemployment spell for the four groups. The mean is calculated using results from empirical hazard estimations.[17] As for the net impact, the before-after estimator again produces the most favorable estimate for the treatment impact. As soon as the development of group A is related to any comparison group, the estimated effect turns to zero. In other words, the results do not suggest that the UVG-guarantee *on average* had any significant impact on the length of the first unemployment spell.

**Figure 4** Expected duration of unemployment. Standard errors in parentheses.

| | Flow 1997 | | Flow 1998 | |
|---|---|---|---|---|
| | Not UVG | UVG | Not UVG | UVG |
| Age ≥ 25 | 197<br>(4.7)<br>n=6,583 | 205<br>(1.6)<br>n=55,438 | 161<br>(3.4)<br>n=7,131 | 172<br>(1.2)<br>n=59,075 |
| Age < 25 | 169<br>(3.6)<br>n=8,158 | 177<br>(1.4)<br>n=63,545 | 132<br>(2.7)<br>n=7,877 | 142<br>(1.0)<br>n=60,884 |

$\alpha^1 = -35\ (1.72)$
$\alpha^2 = 2\ (4.82)$
$\alpha^3 = -2\ (2.64)$
$\alpha^4 = -1\ (7.80)$

[16] For the definition of employment and unemployment, see the Appendix. Alternative definitions (including e.g. temporary employment and part-time unemployment into employment) do not significantly alter the results.
[17] In the calculations, it is postulated that the hazard is constant after 1,110 days.

However, recall that the theory suggested that the UVG-guarantee might already have an impact during the first 100 days of unemployment, as the UI benefits expire unless the individual accepts to participate in the UVG. The results in Black *et al.* (2002) show that such an impact may exist even without the threat of UI benefit expiration, if the individuals consider the program to be worse than open unemployment.

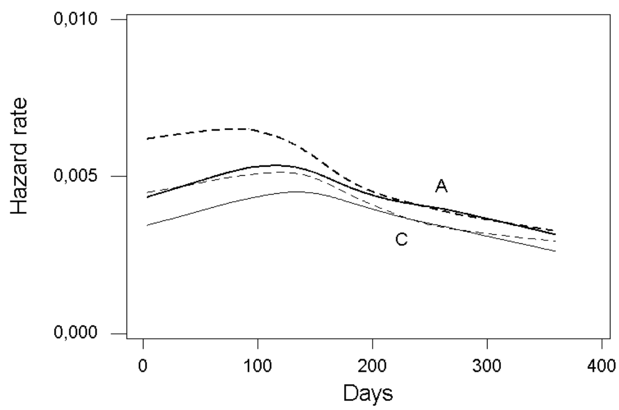
We use the same empirical hazard estimations as presented above to estimate the probability of finding a job within 120 days of unemployment. The results are reported in Figure 5. Once more, we consider $\alpha^3$ to be the most valid estimator, and thus, Figure 6 shows the evolution of the hazard for groups A and C.

**Figure 5** The estimated probability of finding a job within 120 days of unemployment. Standard errors in parentheses.

| | Flow 1997 | | Flow 1998 | |
|---|---|---|---|---|
| | Not UVG | UVG | Not UVG | UVG |
| Age ≥ 25 | 66.5 (0.65) | 63.8 (0.23) | 72.2 (0.59) | 68.8 (0.21) |
| Age < 25 | 68.5 (0.57) | 67.5 (0.21) | 76.3 (0.53) | 74.7 (0.20) |

$\alpha^1 = \phantom{-}7.2\ (0.29)$
$\alpha^2 = -0.3\ (0.83)$
$\alpha^3 = \phantom{-}2.2\ (0.43)$
$\alpha^4 = \phantom{-}0.1\ (1.25)$

**Figure 6** The empirical hazard rates for the treatment (A) and the comparison (C) group, 1997 (continuous line) and 1998 (dashed line).




The UVG-guarantee does indeed seem to have a positive impact on the probability of employment during the first 120 days. At the beginning of the unemployment spell, the impact is estimated to be roughly 10 percent, then decreasing to approximately zero for 120 days.[18] However, Figures 4, 5, and 6 together suggest that the positive impact on employment during the first 120 days is neutralized by a decreased probability of employment during and after participation, possibly due to decreased job search, or a "lock-in" effect, among participants. Recall from Figure 2 that the introduction of the UVG-guarantee seems to have increased the total volume of program participation. Given that the unemployed individuals search less while participating compared to when in open unemployment, we would expect to find an increased "lock-in" in group A.

## 5.5 What is the relation between dose and response?

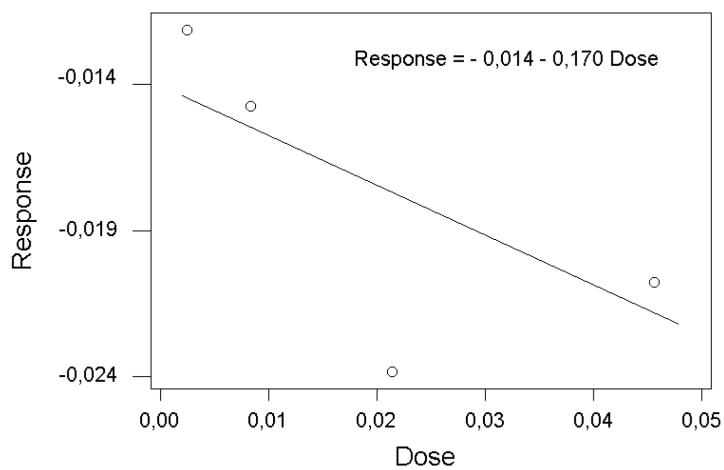
Sometimes the reason for the impact of a treatment being small is that the change in the economic environment from the treatment is small. Using the

[18] It should be noted that this impact is expressed in percent, whereas $\alpha^1$–$\alpha^4$ in Figure 5 are expressed in percentage points. Furthermore, the distribution of spells ending on various days is not uniform, and thus, summing the impact in Figure 6 over the 120-day period produces the 2.2 percentage point impact estimated by $\alpha^3$.

terminology of Imbens (2000), among others, the *response* to the treatment is weak due to a low treatment *dose*. As long as there is variation among units, we can explore the causal relation between them by regressing the response on the dose.

In our case, the treatment dose of a unit is the increase in the program participation rate in each municipality and age group. The response is the decrease in the *share of ES days* variable. Figure 7 shows the fitted line between the mean response and the mean dose of the four groups A-D: An increase in the program assignment within 120 days by one percentage point results in a 0.17 percentage point decrease in the net impact of the treatment, indicating a weak response to the treatment dose.

**Figure 7** Dose-response regression, four municipality groups

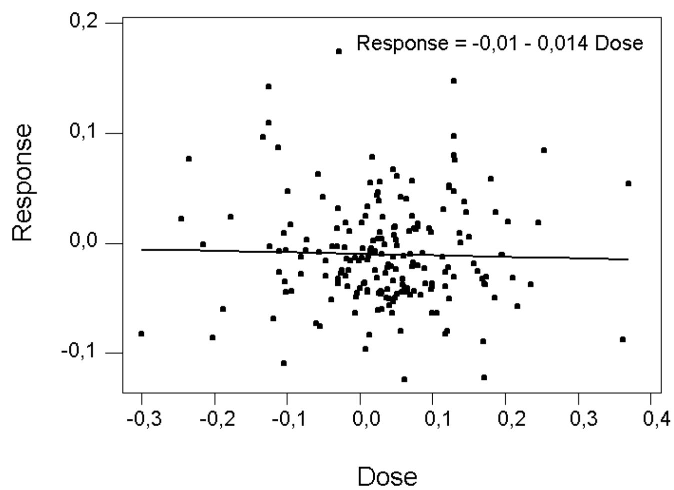



*Note*: The dose is defined as the change from 1997 to 1998 in the program assignment rate within 120 days; the response is defined as the change from 1997 to 1998 in the outcome variable *share of ES days*.

The standard errors in Figure 2 suggest, however, that we have a relatively large variation in the treatment dose among the municipalities. The same applies to the response measure, as reported in Figure 3. Presupposing that the municipality specific dose is exogenous, we can use this variation on the municipality level to estimate a similar regression. Figure 8 shows a relation between the dose and the response comparable to Figure 7.

In sum, the results suggest that the small impact of treatment cannot be explained by a low treatment dose. Even in municipalities where we observe quite large increases in the program assignment rate, the response is still weak. Thus, shortening open unemployment does not seem to play any important role for the success on the labor market during the following 18 months.

**Figure 8** Dose-response regression, all UVG-providing municipalities




*Notes*: See Figure 7.

## 5.6 Is the treatment effect common to all?

Variation in the impact of treatment across individuals is an important aspect in evaluating labor market programs. Individual characteristics, like gender or educational background, may be sources of such variation (For an example of Swedish youth programs, see Larsson, 2000).

The goal of the UVG-program – like the goal of most Swedish labor market programs – is to help those who need help most, i.e. individuals with a weak position on the labor market. We thus want to evaluate this goal by examining the variation in the impact of treatment across individuals with a *strong* versus a *weak initial position*. Initial refers to the state at the time when the individual registers as unemployed.

As an indicator of the individual's strength on the labor market, we use her history in the Employment Service register prior to the actual unemployment spell.[19] The more the individual has been registered with the ES, basically implying either open unemployment or participation in some labor market program, the weaker is her position on the labor market. Figure 9 reports the results for the strongest and the weakest quartile in each group A–D. In short, there is no considerable heterogeneity in the treatment effect between the strong and the weak; $\alpha^3$ produces almost identical estimates for the quartiles.[20]

[19] A detailed description on how the variable is defined is found in the Appendix.

[20] One may wonder whether previous unemployment adequately reflects the individual heterogeneity by which the UVG-effect varies. And more specifically, whether the absence of evidence of a heterogeneous effect is a consequence of this choice. We have therefore made a thorough investigation of this matter. We consider five measures of "strength on the labor market": previous income, unemployment duration, the time registered at the unemployment office, the caseworker's assessment of the need for job search assistance as well as the need for additional labor market training. These five variables are put into a measure model and a factor analysis is performed for the 1997-sample. The analysis suggests the presence of two factors that we label 'actual strength' (driven by the first three variables) and 'assessed strength' (driven by the last two variables). 13 additional variables are then used to predict the factor score for the 1997-sample through a regression model. The predictive variables relate to education, school-grades, family status, work experience as well as previous unemployment history. The regression model is thereafter used for predicting the individual factor-score for both the 1997 and 1998 samples, and to classify the individual's labor market strength. However, we find no evidence of a heterogeneous UVG-effect, and therefore, we decided to present the simpler analysis above.

**Figure 9** Mean of *share of ES days* (%) for the strongest (S) and the weakest (W) quartiles. Standard errors in parentheses.

| | Flow 1997 | | Flow 1998 | |
|---|---|---|---|---|
| | Not UVG | UVG | Not UVG | UVG |
| Age ≥ 25 | S: 43.8<br>(0.77)<br>W: 55.8<br>(0.65) | S: 43.1<br>(0.25)<br>W: 55.9<br>(0.25) | S: 40.3<br>(0.80)<br>W: 55.3<br>(0.63) | S: 40.3<br>(0.25)<br>W: 55.1<br>(0.23) |
| Age < 25 | S: 41.4<br>(0.65)<br>W: 57.4<br>(0.67) | S: 40.2<br>(0.22)<br>W: 55.1<br>(0.25) | S: 39.0<br>(0.63)<br>W: 55.8<br>(0.73) | S: 37.6<br>(0.22)<br>W: 54.4<br>(0.27) |

| Strongest: | Weakest: |
|---|---|
| $\alpha^1 = -2.6$ (0.31) | $\alpha^1 = -0.7$ (0.37) |
| $\alpha^2 = -0.2$ (0.96) | $\alpha^2 = 0.9$ (1.06) |
| $\alpha^3 = 0.2$ (0.47) | $\alpha^3 = 0.1$ (0.50) |
| $\alpha^4 = -0.9$ (1.51) | $\alpha^4 = 1.2$ (1.28) |

## 5.7 Additional checks of the results

Information on whether and when a municipality provided the UVG-guarantee is crucial for the identification of the treatment effect. Thus, we have checked the result with respect to a number of modifications in the municipality variable.

In the analysis presented so far, all municipalities that started providing the UVG-guarantee some time during 1998 are included in group A. In some cases, however, the individual registered as unemployed before the municipality started providing the program, and thus, group A may be contaminated. Nevertheless, the results are the same when all municipalities with a starting date later than January 1, 1998, are excluded. Neither do the results change when we exclude all 49 municipalities in the County of Västra Götaland, since we consider the records for that County to be unreliable (See the Appendix).

Another issue is whether the late inflow in 1997 was in fact covered by the UVG-program in municipalities that started providing the program in early 1998. The program may have affected the behavior already in 1997 if the individuals knew that it was about to be introduced. To check this, we have excluded the inflow after September both in 1997 and 1998: the results do not change notably.

Finally, we have tested different age restrictions, as well as excluded individuals non-eligible for unemployment insurance benefits. The results remain the same in both cases.

# 6 Conclusions

This paper is an evaluation of a youth measure called the UVG-program with the goal of preventing open unemployment spells longer than 100 days; open unemployment here referring to a state where the individual does not participate in a labor market program. The set-up of the program implies three possible dimensions for identification of the treatment effect: age, municipality, and time. We claim that this design allows us to compare *a world with a guarantee* with *a world without such a guarantee*.

We have four major findings. First, using the Employment Service (ES) records, we evaluate the overall impact of the guarantee on the subsequent labor market attachment. We estimate a modest decrease in the number of days the individual is registered with the ES during the 18 month period after the start of the unemployment. This decrease, however, is too small to indicate an appreciably more stable transition out of unemployment.

Second, we find a slightly increased probability of employment during the first 120 days of unemployment, similar to the results shown in Black *et al.* (2002), suggesting that the UVG-guarantee works more as a threat than a promise. This small positive impact is neutralized by a negative impact after the first 120 days, however. Thus, on average, the first unemployment spell is not significantly shorter in the group covered by the UVG-guarantee.

Third, the UVG-program was everything but a guarantee: it implied an increase from around 25 to 30 percent in the probability of being assigned to some program within the promised 100-day period. However, although we would expect more from a guarantee, the increase is still significant, varying among municipalities. Exploiting this variation in the program assignment rate between the municipalities, we estimate dose-response functions, and find no significant correlation between the program assignment – the dose – and the outcome variable – the response. Thus, the negligible impact is not explained by a small dose.

Fourth, we find no evidence that the estimated treatment effect would depend on individual characteristics reflecting the individual's initial attachment to the labor market. We consider this attachment to be better, the shorter is the individual's unemployment history.

Returning to the question raised in the title of this paper, do our results suggest that early intervention helps the unemployed youth? Naturally, the answer depends on the desired impact. In the very short run, the UVG-program indeed seems to have succeeded in slightly increasing employment. This small positive impact disappears in course of time, however, probably due to a low search activity during participation in the UVG-program and other labor market programs. The UVG-program increased the total volume of program participation, and thus, more individuals were "locked in" into a passive job search. The impact of a shorter unemployment history on employment stability during the following 18 months also seems to be negligible. Thus, our conclusion is that, at least in this specific case, shortening the unemployment spell does not seem to have played any significant role for the individual's labor market prospects within the subsequent 18 months.

The result that only less than a third of the target group was assigned to a program within the promised 100 days is noteworthy *per se*. To call for a 100 percent assignment is probably not desirable, since some of the individuals may have had definite job or study plans in the close future. But claiming this to be the case for seven out of ten is unrealistic. The reluctance to put the guarantee into effect at the local employment offices may have been due to the offices mistrusting the municipal authorities or economic disincentives. In any case, exploring the underlying reasons for this result for a future design of similar guarantees is crucial.

Finally, we believe that the identification strategy assures the internal validity of our results. The external validity of the results is a quite different question. For example, we cannot be sure that the impact would have been the same for significantly higher doses of the treatment, *i.e.* if the UVG-guarantee had been an *actual* guarantee of activity within 100 days, or for persons entering unemployment after 1998.

## References

Angrist, J & A Krueger (1999), "Empirical strategies in labor economics", in O Ashenfelter & D Card (eds), *Handbook of labor economics*, Volume III, North-Holland.

Black, D, J Smith, M Berger & B Noel (2002), "Is the threat of reemployment services more effective than the services themselves? Experimental evidence from the UI system", NBER Working Paper 8825.

Carling, K, P-A Edin, A Harkman & B Holmlund (1996), "Unemployment duration, unemployment benefits, and labor market programs in Sweden", *Journal of Public Economics* 59, 313-334.

Carling, K & L Larsson (2000), "Att utvärdera arbetsmarknadsprogram i Sverige: Rätt svar är viktigt, men vilken var nu frågan?" *Arbetsmarknad & Arbetsliv* 6(3), 185-192.

Hahn, J, P Todd & W van der Klaauw (2001), "Identification and estimation of treatment effects within a regression-discontinuity design", *Econometrica* 69(1), 201-209.

Heckman, J & R Robb (1985), "Alternative methods for evaluating the impact of interventions", in J Heckman & B Singer (eds), *Longitudinal analysis of labor market data*, Cambridge University Press.

Imbens, G (2000), "The role of propensity score in estimating dose-response functions", *Biometrica* 87(3), 706-710.

Larsson, L (2000), "Evaluation of Swedish youth labor market programmes", IFAU Working Paper 2000:1, Institute for Labour Market Policy Evaluation.

Mortensen, D (1977), "Unemployment insurance and job search decisions", *Industrial and Labor Relations Review* 30, 505-517.

Sianesi, B (2002), “Differential effects of Swedish active labor market programmes for unemployed adults during the 1990s”, IFAU Working Paper 2002:5, Institute for Labour Market Policy Evaluation.

SK, Svenska Kommunförbundet (1999), “Utvecklingsgarantin – som kommunerna ser det”, Kommunen – Tillväxten – Sysselsättningen 5.

US, Ungdomsstyrelsen (1999), “Utvecklingsgarantin för arbetslösa ungdomar i 10 kommuner”, Ungdomsstyrelsens utredningar 19.

US, Ungdomsstyrelsen (2000), “En av hundra – Utvecklingsgarantins tredje år”, Ungdomsstyrelsens utredningar 23.

# Appendix: Data

## Data sources

Data for the empirical analysis is obtained from sources included in the IFAU database. The population of the IFAU database consists of the entire Swedish population from 1990 to 1998. The most important source for this study is *HÄNDEL* which originates from the public employment offices in Sweden and contains information on spells of unemployment, participation in labor market programs and some individual characteristics, including the municipality code.

For the identification strategy of this study, it is important to know which of the individuals were covered by the UVG-guarantee. Thus, two pieces of information are crucial: whether and when the individual's municipality started providing the UVG-guarantee, and the individual's exact date of birth. To protect individual anonymity, the IFAU database only contains information on the year of birth. We have given a special order to Statistics Sweden for the month of birth for the individuals in our sample.

Exact information on which municipalities have concluded an agreement on the UVG-program and when the first agreement was concluded are not collected into any document. The agreement, if there was any, was made between the municipal labor market authority and the local employment office. Our procedure was thus to gather information from the local level.

In 1998, there were 288 municipalities in Sweden. Our first step was to contact the 21 county labor boards governing the local employment offices by e-mail. 13 of these were able to provide more or less exact information for a total of 162 municipalities. As a second step, we then contacted either the municipal labor market authority or the local employment office (or both) in the remaining 126 municipalities. Lena Ståhl at the Ministry of Industry helped us by gathering the information for municipalities in Stockholm County. Our attempt to obtain information from the archives at the National Labor Market Board was unsuccessful.

The first e-mails were sent on November 11, 2001, and by February 15, 2002 we had received information for 256 municipalities. The remaining 32 municipalities are excluded from our study. These municipalities are:

| | | | |
|---|---|---|---|
| Flen | Forshaga | Gagnef | Gnesta |
| Grums | Helsingborg | Höganäs | Kil |
| Kristinehamn | Lidingö | Ludvika | Lycksele |

| | | | |
|---|---|---|---|
| Malå | Mjölby | Norrtälje | Nyköping |
| Oxelösund | Tyresö | Täby | Sigtuna |
| Skurup | Sundbyberg | Svalöv | Svedala |
| Säffle | Tomelilla | Trelleborg | Vallentuna |
| Vaxholm | Vingåker | Östersund | Österåker |

Moreover, we considered the records for Ödeshög, Ydre and Boxholm to be unreliable, and thus, these were excluded. Finally, information for the 49 municipalities in Västra Götaland County seemed uncertain (all municipalities in this area were claimed to have started on Jan 1, 1998) and thus we checked the robustness of the results when Västra Götaland is excluded. According to our records, 198 municipalities started providing the UVG-guarantee some time during 1998. Table A1 shows the distribution of months:

**Table A1** Distribution of the starting months for the UVG-program during 1998

| Month | No. of municipalities |
|---|---|
| January | 118 |
| February | 15 |
| March | 25 |
| April | 23 |
| May | 6 |
| June | 7 |
| July | 2 |
| August | 1 |
| September | 1 |
| October | 0 |
| November | 1 |
| December | 0 |

## Sample construction

From the *HÄNDEL* database, we collect the entire inflow during 1997 and 1998 of individuals born in 1967-78. As inflow in 1997, we define all individuals who enter the Employment Service register during 1997; the same applies for 1998. Thus, the samples for 1997 and 1998 overlap to some extent. We observe the entire *HÄNDEL* history for these individuals, and we can follow them until 22 June 2000.

Table A2 summarizes the sampling procedure. From the original sample of 586 653 individuals, we exclude observations with incorrect or missing information. *INSPER* and *SOKATPER* are tables in the *HÄNDEL* database. *INSPER* contains information on the date of entry into and exit from the Employment Service register, whereas *SOKATPER* includes detailed information on the activities, or job search categories, in each registration spell. Examples of search categories (variable *SKAT*, *sökandekategori*) are open unemployment and participation in a program. Dates for the start and end of each *SKAT* are reported in *SOKATPER*.

**Table A2** Sample construction

| | No. of excluded obs. | sample size |
|---|---|---|
| All observations | | 586 653 |
| incorrect year of birth[1)] | 384 | 586 269 |
| municipality code missing | 25 | 586 224 |
| | Inflow 1997 | |
| | | 375 564 |
| overlapping INSPER spells[2)] | 56 210 | 319 354 |
| too old INSPER registration data[3)] | 166 | 319 188 |
| overlapping SOKATPER spells[4)] | 26 013 | 293 175 |
| information on whether the municipality provides UVG missing | 26 325 | 266 850 |
| 22-27 years at registration with ES | 133 126 | $\boxed{133\ 724}$ |
| | Inflow 1998 | |
| | | 345 781 |
| overlapping INSPER periods[2)] | 43 934 | 301 847 |
| too old INSPER registration data[3)] | 162 | 301 685 |
| overlapping SOKATPER periods[4)] | 22 907 | 278 778 |
| information on whether the municipality provides UVG missing | 24 243 | 254 535 |
| 22-27 years at registration with ES | 119 568 | $\boxed{134\ 967}$ |

[1)] Year of birth may be incorrect either within *INSPER*, such that an individual has a different year of birth for different registration periods, or between *INSPER* and *SYS9698*.
[2)] Individuals with fully or partly overlapping periods, periods of one day only, an incorrect order of serial numbers, double serial numbers, a negative period length, or a registration date after June, 22, 2000 (censoring date) in *INSPER* are excluded. However, observations with overlapping periods before the year of inflow and the same starting date for both periods are included, collecting only the latest of the double periods.
3) Age at the first registration must be at least 16 years, otherwise we assume the observation to be incorrect.
4) Individuals with the following incorrect information are excluded: registration date in *INSPER* different from the registration into first search category in *SOKATPER*, de-registration date in *INSPER* different from the de-registration from the last search category.

## Definition of some important variables

### Unemployment and employment

All search categories (*SKAT*, *sökandekategori*) are included in our definition of an unemployment spell. The end of the unemployment spell is determined by the date and reason for de-registration (*AVDM*, *avaktualiseringsdatum*, and *AVORS*, *avaktualiseringsorsak*). If *AVORS* = (1, 2, 3), the spell is defined to end in employment.

### Program participation

An individual is defined to participate in a program if her unemployment spell contains a search category *SKAT* = 42-83. *SKAT* = 66 stands for participation in the UVG-program.

### Share of ES days

The longest possible period for which we can follow an individual who registers with the ES records on December 31, 1998 is until June 22, 2000, i.e. 539 days. Thus, the numerator of the outcome variable *share of ES days* is 539. The denominator is the sum of days registered with ES from the date of (first) registration.

### History in the Employment Service register

The variable history in the ES register defines the number of days registered with the ES records since the first registration until the actual registration in 1997 or 1998. For comparability, this is expressed in relative terms as a share. In other words, the numerator is the sum of all unemployment spells (see the definition of an unemployment spell above) from the first registration until the actual registration. For individuals in the inflow 1997 (1998), the denominator is the sum of all calendar days from the first registration until December 31, 1997 (1998). Thus, the denominator is an approximation.

**Table A3** Mean unemployment history in the four groups, 1997 and 1998.

| Group | Share of unemployment days of all calendar days since the first unemployment spell, %. Standard deviation in parentheses. |
|---|---|
| **1997** | |
| $A^0$: municipality provides UVG, Age < 25 | 35.7 (23.3) |
| $B^0$: municipality does not provide UVG, Age < 25 | 37.9 (23.4) |
| $C^0$: municipality provides UVG, Age ≥ 25 | 37.8 (24.7) |
| $D^0$: municipality does not provide UVG, Age ≥ 25 | 40.6 (25.0) |
| **1998** | |
| $A^1$: municipality provides UVG, Age < 25 | 34.6 (22.8) |
| $B^1$: municipality does not provide UVG, Age < 25 | 36.2 (23.0) |
| $C^1$: municipality provides UVG, Age ≥ 25 | 39.9 (24.2) |
| $D^1$: municipality does not provide UVG, Age ≥ 25 | 42.7 (24.3) |

### Eligibility to unemployment benefits

Individuals are defined as eligible for UI benefits if their *KASNR* = 02-69. Thus, *KASNR* = 00, 98, 99 or missing indicates non-eligibility: 51 796 non-eligible individuals in the inflow 1997, and 42 841 non-eligible individuals in the inflow 1998.

## Publication series published by the Institute for Labour Market Policy Evaluation (IFAU) – latest issues

**Rapport** (some of the reports are written in English)

**2002:1** Hemström Maria & Sara Martinson ”Att följa upp och utvärdera arbetsmarknadspolitiska program”

**2002:2** Fröberg Daniela & Kristian Persson ”Genomförandet av aktivitetsgarantin”

**2002:3** Ackum Agell Susanne, Anders Forslund, Maria Hemström, Oskar Nordström Skans, Caroline Runeson & Björn Öckert "Follow-up of EU's recommendations on labour market policies"

**2002:4** Åslund Olof & Caroline Runeson "Follow-up of EU's recommendations for integrating immigrants into the labour market"

**2002:5** Fredriksson Peter & Caroline Runeson "Follow-up of EU's recommendations on the tax and benefit systems"

**2002:6** Sundström Marianne & Caroline Runeson "Follow-up of EU's recommendations on equal opportunities"

**2002:7** Ericson Thomas ”Individuellt kompetenssparande: undanträngning eller komplement?”

**2002:8** Calmfors Lars, Anders Forslund & Maria Hemström ”Vad vet vi om den svenska arbetsmarknadspolitikens sysselsättningseffekter?”

**2002:9** Harkman Anders ”Vilka motiv styr deltagandet i arbetsmarknadspolitiska program?”

**Working Paper**

**2002:1** Blundell Richard & Costas Meghir "Active labour market policy vs employment tax credits: lessons from recent UK reforms"

**2002:2** Carneiro Pedro, Karsten T Hansen & James J Heckman "Removing the veil of ignorance in assessing the distributional impacts of social policies"

**2002:3** Johansson Kerstin "Do labor market programs affect labor force participation?"

**2002:4** Calmfors Lars, Anders Forslund & Maria Hemström "Does active labour market policy work? Lessons from the Swedish experiences"

**2002:5** Sianesi Barbara "Differential effects of Swedish active labour market programmes for unemployed adults during the 1990s"

**2002:6** Larsson Laura "Sick of being unemployed? Interactions between unemployment and sickness insurance in Sweden"

**2002:7** Sacklén Hans "An evaluation of the Swedish trainee replacement schemes"

**2002:8** Richardson Katarina & Gerard J van den Berg "The effect of vocational employment training on the individual transition rate from unemployment to work"

**2002:9** Johansson Kerstin "Labor market programs, the discouraged-worker effect, and labor force participation"

**2002:10** Carling Kenneth & Laura Larsson "Does early intervention help the unemployed youth?"

**2002:11** Nordström Skans Oskar "Age effects in Swedish local labour markets"

**2002:12** Agell Jonas & Helge Bennmarker "Wage policy and endogenous wage rigidity: a representative view from the inside"

**2002:13** Johansson Per & Mårten Palme "Assessing the effect of public policy on worker absenteeism"

**2002:14** Broström Göran, Per Johansson & Mårten Palme "Economic incentives and gender differences in work absence behavior"

### Dissertation Series

**2002:1** Larsson Laura "Evaluating social programs: active labor market policies and social insurance"