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Microeconometric Studies on Unemployment and Business Cycles

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Foreword

Jouko Verho's doctoral dissertation concentrates on labor market issues by using a variety of microeconometric techniques ranging from hazard rate models to matching procedures. He also presents clear overviews of relevant strands of literature ranging from job search incentives and unemployment benefits to cyclical variation in work place accidents.

The first essay evaluates the effects of unemployment benefits on re-employment rates by exploiting a policy change in the Finnish unemployment benefit system in 2003. Verho shows that the increase in unemployment benefits raised median unemployment duration and the estimated average effect implies an average 16% decline in the re-employment rate. The second essay investigates the size of the wage effects for individuals that lost their job during the Finnish recession in the early 1990s. Verho uses data from firm closures and, to ensure the pure measurement of treatment effects of job loss, he exploits matching method. Verho finds that workers who lose their jobs during a recession experience a substantial penalty for the job loss. The third essay studies the main determinants of average unemployment duration in Finland by using a proportional hazard model and individual data from 1987 to 2000. The main conclusion is that the variation in the composition of unemployed individuals during the recession implies only a small increase in the average duration. Finally, the fourth essay uses Swedish data from the years 1997-2005 to study the cyclical sensitivity of workplace accidents. According to results workplace accidents are procyclical in Sweden but only in some specific subgroups.

This study is part of the research agenda carried out by the Research Unit of Economic Structure and Growth (RUESG). The aim of RUESG is to conduct theoretical and empirical research with respect to important issues in industrial economics, real option theory, game theory, organization theory, theory of financial systems as well as problems in labour markets, natural resources, taxation and time series econometrics.

RUESG was established at the beginning of 1995 and has been one of the National Centres of Excellence in research selected by the Academy of Finland. It has been financed jointly by the Academy of Finland, the University of Helsinki, the Yrjö Jahnsson Foundation, Bank of Finland and the Nokia Group. This support is gratefully acknowledged.

Helsinki, September 3, 2008

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Jouko Verho

Contents

Chapter I

Introduction

1 Background

What determines unemployment and how is it related to business cycles? This has been one of the key questions that economists have tried to answer since the 1930's Great Depression. Understanding the mechanisms how cyclical variation in the economy affects individuals is important for a number of reasons. An increase in unemployment risk is one of the main costs of recessions at individual level.

This thesis studies unemployment and business cycles empirically from two perspectives. The first question is: what determines unemployment duration at individual level? The second question is: how do unemployment and business cycles affect individual labour market outcomes? The analyses are based on individual level data from Finland and Sweden. The variation in Finnish unemployment during the 1990's provides a very interesting, although somewhat extreme, analysis setup for studying cyclical variation.

To understand the questions that are analysed in the thesis, it is useful to shortly discuss their relation to economic theory. Search theory models the behaviour of unemployed persons by taking into account the uncertainty of finding a new job.[1] An unemployed individual faces a trade-off between accepting an available job offer immediately and continuing to look for a better but uncertain job offer. When the unemployed individual knows what kind of offers to expect, he or she can form a decision rule based on the reservation wage. If an offered wage exceeds the reservation wage, the job is accepted because the wage loss from searching for another period is higher than the probability of getting an offer better enough.

Unemployment insurance reduces the income loss due to unemployment. From

[1] One of the first sequential search models was presented by McCall (1970). Mortensen (1986) gives a general presentation of the basic model and Rogerson, Shimer & Wright (2005) present a survey to more recent developments in job search theory.

society's point of view, benefits have a negative side effect because they increase the reservation wage and therefore lower the incentives to find a job. Because unemployment benefits are often paid by the government, the level of the benefits is a very policy relevant question. The effect of the benefit level on re-employment rate is analysed in the first essay of the thesis.

In the basic search model, the effect of business cycle can be thought to have an effect through changes in the number of wage offers. For example, the recession decreases the number of vacancies which leads to a lower job offer rate. Under sufficient assumptions about the wage distribution (see Mortensen, 1986), a lower job offer rate implies longer unemployment durations. However, individual search behaviour may not be the only explanation for the longer average unemployment durations during recessions. Another possibility is that the composition of unemployed individuals changes. If more individuals with unfavourable characteristics enter unemployment during recession, it also increases the average duration. The role of compositional variation is analysed in the third essay of the thesis.

The two other essays are not related to unemployment durations but analyse the effect of business cycle more directly. The second essay estimates the cost of unemployment for those who became unemployed during the Finnish recession of the early 1990's. Unemployment may have several negative implications for individuals in addition to the direct income loss. Individuals may lose human capital, i.e. skills that make workers productive. Some human capital may be firm specific that is not useful in other jobs. Further, learning at work is not possible during unemployment and long unemployment can deteriorate skills. Lastly, employers may interpret a long unemployment duration as a negative signal about the worker's skills because other employers have not hired the worker earlier. All these may decrease the future employment probability and wage level. This effect is often referred to as scarring.

Business cycle variation can also affect employed individuals. Ruhm (2000) interestingly claims that recessions are good for health. One of the mechanisms he suggests is that working conditions become more unhealthy during booms because of stress among other things. Several macrodata studies have confirmed Ruhm's empirical results since. The fourth essay studies this theory in more detail by focusing on workplace accidents. Unique Swedish health register data are used to investigate competing explanations for the results.

2 Summaries of the essays

This section provides brief summaries of the four essays in the order of their appearance.

2.1 Essay I: "The effect of unemployment benefits on re-employment rates: Evidence from the Finnish unemployment insurance reform"

The first essay (Chapter II) evaluates the impact of an unemployment benefit reform in Finland. The level of unemployment benefits is an important policy tool because it plays a key role in determining the incentives for unemployed individuals to search for a job. In 2003, the benefit level was increased in Finland for workers with long employment histories. The reform provides an excellent analysis setup to identify the causal effect of benefit level on re-employment rates.

Policy reforms with varying impact on individuals are essential in estimating the effect of unemployment benefits for the following reasons. The earnings-related benefits in Finland provide a source for variation as high earnings lead to a lower replacement rate. A problem arises in standard regression analysis because individuals with high earnings tend to have higher skills than individuals with low earnings. These differences in skills are practically never observed by the researcher which means that regressing unemployment duration with benefit level gives downward biased estimates. The solution is to find exogenous variation in replacement rate which is a very challenging task without a policy reform.

The Finnish 2003 benefit reform was connected to the abolishment of the severance pay. The lost lump-sum payment was compensated with a 15% average increase in benefits for the first 150 days of unemployment. Only individuals with over a 20 years of employment history (together with other criteria) were eligible for the increased benefits. This provides a setup for difference-in-differences analysis. Thus, because the eligibility can be determined also before the reform, non-eligible individuals can be used as a control group and eligible individuals as a treatment group. Then by comparing unemployment durations between the groups before and after the reform makes it possible to estimate the effect of the reform.

A few previous studies have attempted to estimate the effect of unemployment benefits in Finland but with relatively little success because there has not been any suitable policy reforms. In Sweden, the policy reforms have been more abundant. Carling, Holmlund & Vejsiu (2001) evaluate the 1996 reform and Bennmarker, Carling & Holmlund (2007) evaluate the 2001 and 2002 reforms. Both reforms affected individuals differently depending on their replacement rate allowing to use a difference-in-differences estimator. Another paper that is closely related to this study is Lalive, van Ours & Zweimuller (2006). They evaluate the 1989 reform in Austria that gave variation in the replacement rate and the duration of benefits.

The Finnish 2003 reform differs from previous studies because it is possible to identify the effect of benefits using groups with different employment history rather than different pre-unemployment wage. The problem with pre-unemployment wage is that individuals may be able to influence the wages near the threshold value. In addition, only newly unemployed individuals were eligible for the benefit increase. This makes it easier to account for the fact that some unemployed individuals may anticipate the change in the benefits and adjust their behaviour before the actual reform takes place.

The dataset used in the analysis is constructed by linking information from three administrative registers. A random sample of individuals entering unemployment between 2002 and 2004 was drawn from the registers of labour administration. Then all individuals were followed until the end of 2005. Because the reform affected only experienced workers, the sample was restricted to individuals from 37 to 55 years of age with no previous unemployment during the previous three years. To obtain information on benefit recipiency and pre-unemployment wages, data from the insurance authority and pension register were linked to the analysis data.

Search theory predicts that the effect of benefit level on the re-employment rate decreases as the maximum benefit duration becomes closer. This happens because the expected value of the remaining benefits decreases. Lalive et al. estimated a duration model that allows the effect of benefits to vary across the elapsed duration of unemployment. A similar flexible model specification is used in this study because it is interesting to study whether the effect of benefit increase is consistent with search theory.

According to the results, the increase in the benefits caused a substantial decline in the re-employment rate. The effect occurs only during the first 250 days of unemployment which is roughly in line with the prediction of search theory. The estimated average effect implies a 16% decline in the re-employment rate. This effect is large compared with most earlier studies which may be related to the fact that the analysed group consists of older workers.

2.2 Essay II: "Scars of recession: The long-term costs of the Finnish economic crisis"

The Finnish recession in the early 1990's provides an interesting setup to study the impact of unemployment. The recession had a huge impact on the economy: GDP dropped over 10% and the unemployment rate became fivefold between 1990 and 1993. The second essay (Chapter III) estimates the long-term effect of unemployment for those who were displaced during the recession. The studied outcomes are future earnings, employment and wages for prime working-aged men.

When estimating the effect of unemployment, the key question is where to find exogenous variation. The problem is that those who experience unemployment are generally a selected group of people based on unobservable characteristics. Previous studies have attempted to overcome the selection problem by modelling the selection process (e.g. Arulampalam, 2001), using an instrumental variable (e.g. Gregg, 2001) or using sibling fixed effects (Nordström Skans, 2004).

This study utilises the variation in unemployment created by plant closures during the recession. Mass layoffs and plant closures have been used as a source for exogenous variation in a large number of studies that estimate the cost of displacement. The idea is that usually firms lay off the least productive workers first but in mass layoffs workers are displaced randomly. Gibbons & Katz (1991) have formulated a theoretical model based on this idea. However, there are potential problems in this approach. Perhaps most importantly, individuals may anticipate the layoffs and leave the firm before displacement takes place. In addition, the analysis is often done using annual linked employee-employer data where it is difficult to identify mass layoffs accurately. Annual data may mask important worker flows between the observation points and it sometimes difficult to distinguish organisational changes from true layoffs based on the information on worker flows only.

In the study it is argued that the Finnish recession provides a good setup for using plant closures as a source of variation. The events that led to the recession were unexpected and took place quickly. Therefore it was very hard to anticipate for both firms and workers. Further, a large number of firms went bankrupt providing a sufficiently large sample size. A limitation of the study is that the results may not describe the typical costs of unemployment but are more specific to the analysis period.

The analysis dataset is a panel of 350,000 Finnish individuals from 1987 to 2000. The data contain information on the reason for unemployment which makes it possible to identify the plant closure cases. However, this strategy creates an additional challenge because not all displaced individuals register as unemployed. To overcome the potential sorting problem and to construct a valid comparison group for the unemployed individuals, those unemployed due to plant closure during the recession are matched to those who remained employed during the recession.

The effect of unemployment is estimated until the end of 1999 for those who worked in plant closure firms. Earnings losses are very large in 1994, 50% when compared to earnings in the group that remained employed during the recession. The earnings recover steadily but are still 25% lower in 1999. A low level of employment explains most of the initial earnings losses. After the recession, the effect on employment is -50% but the difference reduces to -10% in 1999. The wage loss changes less, from 23% to 14% in the period. Especially, the earnings losses are very large compared with previous studies but this is not surprising as they do not focus on similar recession periods.

2.3 Essay III: "Determinants of unemployment duration over the business cycle in Finland"

The third essay (Chapter IV) studies the sources of variation in unemployment duration. Again the Finnish recession provides a unique setup for the analysis. The average unemployment duration increased almost at the same rate as the unemployment rate between 1990 and 1993. This increase in durations could be caused simply by an outflow effect, i.e. lower labour demand and higher labour supply. An alternative explanation is that also the composition of unemployed individuals changed. The main question in the analysis is to what extent compositional variation contributed to the changes in the duration.

It is often assumed that those individuals who enter unemployment during recessions have less favourable characteristics considering employment Baker (see e.g. 1992). This could happen if firms lay off the least productive workers first and their share increases among unemployed. On the other hand, it is possible that a high number of mass layoffs, in fact, increases the share of individuals who are easily re-employed which could be the case especially during the Finnish recession.

Most of the previous studies have analysed aggregated data because large individual datasets with long follow-up periods are not commonly available. By definition, aggregated data provides quite limited information about individual level heterogeneity. However, it is possible to model the heterogeneity by using the estimation method of mixed proportional hazard model introduced by van den Berg & van Ours (1994). Abbring, van den Berg & van Ours (2002) apply this method to study cyclical variation in unemployment, and several other studies have applied the same method since. Among the few microdata studies analysing cyclical variation, Rosholm's (2001) analysis is the closest to this study. He uses Danish register data from 1981 to 1990 and his method of identifying the sources of variation in unemployment duration is similar to this study.

In the empirical analysis, the regional unemployment rate is used as a proxy for macroeconomic conditions. The idea is that this captures the outflow effect. Compositional variation is captured by including a rich set of individual characteristics in the model. Then unemployment durations until employment are modelled using a proportional hazard model. Influence of different components is obtained by using the estimated model to predict expected average durations. When the unemployment level is kept fixed and the individual characteristics are allowed to vary over time according to the composition of inflow in the analysis period, it gives the contribution of observed compositional variation to the unemployment duration.

The data used in this study are a 10% sample of the Finnish workforce followed from 1987 to 2000. Information on individual labour market transitions and a rich set of background variables is obtained from several administrative registers. The analysis period is challenging to model because the cyclical variation is very strong. To account for changes in the parameter values of the model over different phases of the business cycle, the model is estimated separately for four time periods. The results show that the composition of unemployed individuals changes in the analysis period, especially during the recession. For example, the average age and education of the unemployed individuals increase quite substantially in the period. However, this observed compositional variation implies only a relatively small increasing trend in the predicted average duration for the recession period.

Rosholm (2001) finds that the impact of compositional variation is important in Denmark and that the average quality of those entering unemployment improves during booms. The results of this study are more in line with previous macrodata studies which have found composition effects to be minor. However, the seasonality in predicted compositional variation is relatively strong which points to the conclusion that it should be taken into account when adjusting the active labour market policy.

2.4 Essay IV: "Workplace accidents and business cycle: Evidence from Swedish health registers"

The fourth essay (Chapter V) studies the effect of business cycle on workplace accidents. Although workplace safety is directly linked to the labour productivity and the costs of these accidents are considerable, economic studies on the topic are scarce. This study uses data on Swedish inhospital care to examine cyclicality in the incidence of accidents.

Several previous studies have indeed found that various health outcomes are cyclical (for survey, see Ruhm, 2006). The early studies that used time series methods suggested that mortality due to various diseases decreases when economy expands. This result seems plausible because during good times individuals should have more resources to invest in health, for example. However, Ruhm (2000) came to the opposite conclusion as he found mortality to increase during recessions using U.S. state level data. He argues that the previous results were biased because of spurious correlation between mortality and business cycle indicators. This could mean, for instance, that the improvements in medical care have a similar trend as the business cycle which would lead the estimated parameter to reflect changes in medical care rather than in economic conditions.

Why health outcomes are cyclical is still to a large extent an open question. Most studies have used macrodata which makes it difficult to learn what the exact mechanisms are behind the cyclicality. One of the mechanisms Ruhm suggests, that is closely related to workplace accidents, is that health is an input into production. More accidents would occur because hazardous working conditions, job-related stress and wearing overtime work become more common during booms. All these factors would increase accidents at workplace. However, this may not be the only explanation for procyclical accident rates. An alternative explanation is that the composition of labour force varies cyclically. Further, it is possible that procyclicality in accidents simply reflects strategic reporting behaviour of the workers as Boone & van Ours (2006) suggest. If employers interpret reported accidents as a negative signal of the worker's productivity, workers report less accidents during recessions when the probability of being laid off is higher.

The aim of this study is to investigate the explanatory power of these competing explanations using Swedish register data. The Swedish patient register covers all inhospital care spells in Sweden from 1997 to 2005. Linking these data to a population database, that contains detailed information on labour market outcomes and demographic variables, provides a unique analysis dataset. Data on the timing of accidents allows using the holiday season as a proxy for accidents taking place during leisure time. Individual level data allows constructing populations who are permanently and marginally employed. By comparing these populations, it is possible to disentangle whether compositional changes explain the cyclicality in accident rates. In addition, it possible to study cyclical variation by the severity of accidents to investigate the presence of strategic behaviour.

Following Ruhm (2000), the regional unemployment rate is used as proxy for business cycle. The regional and annual fixed effects are included in the model which means that the effect of business cycle on accidents is identified using the within-region variation in the unemployment rate. This identification strategy should be less vulnerable to spurious correlation than standard time series analysis. The results show that workplace accidents are procyclical in Sweden but only for some subgroups. For men, the cyclical variation is present for those in stable employment with secondary degree education. For women, accidents are weakly procyclical for those in non-stable employment. This suggests that compositional variation may contribute to the procyclical variation for women but not for men. Less severe accidents show weak procyclicality among men while more severe accidents show no cyclicality. For women, the pattern of cyclicality does not depend on the severity of accidents. Thus, strategic behaviour may play some role in explaining cyclicality among men while compositional variation seems to be more important for women.

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Chapter II

The effect of unemployment benefits on re-employment rates: Evidence from the Finnish unemployment insurance reform*

Abstract

In January 2003, the unemployment benefits in Finland were increased for workers with long employment histories. The average benefit increase was 15% for the first 150 days of the unemployment spell. In this paper we evaluate the effect of the benefit increase on the duration of unemployment by comparing the changes in the re-employment hazard profiles among the unemployed who became eligible for the increased benefits to the changes in a comparison group whose benefit structure remained unchanged. We find that the benefit increase reduced the re-employment hazards by on average 16%. The effect is largest at the beginning of the unemployment spell and disappears after the eligibility period for the increased benefits expires.

*Joint work with Roope Uusitalo

1 Introduction

In January 2003, the unemployment insurance (UI) benefits in Finland increased for workers with long employment histories. The average benefit increase was 15% for the first 150 days of unemployment spell. The benefit increase was part of a reform aiming to simplify the rules regarding unemployment benefits. As a part of the same reform a severance pay system that had existed until the end of 2002 was abolished.

The Finnish benefit reform provides a relatively clean policy experiment that can be used to evaluate the effect of UI benefits on the re-employment rates. The reform took place at a time when the macroeconomic environment was stable with aggregate unemployment rates almost constant over the four-year period that we use in the analysis. In addition, no other major policy reforms that might have had an effect on the re-employment rates were implemented simultaneously. These two facts together minimise the risk that our results would be contaminated by macroeconomic cycles or other policy changes.

The eligibility for increased UI benefits was based on the length of the previous work history and on the length of membership in a UI fund. This allows us to estimate the effect of the benefit increase by comparing the changes in the job-finding rates after the reform in the "treatment group" that became eligible for higher benefits to the changes in the "comparison group" whose benefit system was unchanged but otherwise was similar to the treatment group. This difference-in-differences approach overcomes the fundamental identification problem caused by the fact that UI benefits are linked to previous earnings. Previous earnings, again, may well be correlated with other factors affecting re-employment rates. Lack of independent variation in UI benefits in typical cross-section data makes it very difficult to disentangle the effect of the benefit level from other factors correlated with previous earnings and re-employment rates.[1]

We have access to administrative data on the dates of entry into and exit out of unemployment. Our data also include detailed information on the benefits, reported by the UI funds themselves. These data include the daily amounts of benefits, the dates when the benefits are paid out and, importantly, administrative information on the remaining benefit eligibility at the end of each quarter. The data also contain information on all variables that determine the eligibility for increased benefits.

Our paper is related to several previous papers that identify the causal effects of the level of unemployment benefits by using data on policy reforms that lead to different changes in benefits in different groups of unemployed workers.

[1]Krueger & Meyer (2002) provide a literature review on the effect of UI benefits on unemployment duration and a more detailed discussion on identification problem when using cross sectional variation in the replacement rates. Most previous Finnish UI benefits studies rely on this type of identification strategy (e.g. Kettunen, 1993).

Similar analyses have been performed earlier in Germany (Hunt, 1995), Sweden (Carling, Holmlund & Vejsiu, 2001; Bennmarker, Carling & Holmlund, 2007), Austria (Lalive, van Ours & Zweimuller, 2006) and the New York State (Meyer & Mok, 2007). Compared with these papers our set-up differs in several ways. We identify the effect of UI benefits based on the differences in the benefit changes across groups that differ mainly in the length of previous work experience, while most of the others are based on different changes across groups that differ in the pre-unemployment wage. We also examine a more experienced group that becomes unemployed after having been displaced from a permanent job. In our case the benefit increase involved only new entrants to unemployment, which makes it easier to account for possible anticipatory effects. Another potentially important difference is that our setting involves a removal of severance pay and replacing that with higher benefits paid conditional on remaining unemployed.

We evaluate the effect of benefit increase on the entire hazard profile instead of evaluating the effect on the average exit rates. This approach allows the effect of benefit increase to vary across elapsed duration of unemployment as predicted by the search theory. A similar approach has been used in Lalive et al. (2006) and Bennmarker et al. (2007). In contrast to these previous papers, we find that the increase in the unemployment benefits had a large and statistically significant negative effect on the job-finding rates during the first months after entry into unemployment. Consistent with the search theory, the effect diminishes over time and is not significantly different from zero after the first 250 days. Our results do not suggest that the unemployed would anticipate the changes in benefits; the re-employment hazard in the treatment group increases only after the higher benefits have already expired.

The remaining part of this paper is organised as follows. In Section 2, we present the details of the Finnish unemployment benefit system and the 2003 benefit reform. Section 3 describes the data and Section 4 the empirical methods. The main results are presented in Section 5. Extensions and robustness checks follow in Section 6. Section 7 concludes.

2 The Finnish unemployment benefit system

The Finnish unemployment benefit system consists of an unemployment allowance paid by the unemployment insurance funds and a flat-rate labour market subsidy paid by the State through the Social Insurance Institution. Membership of UI funds is voluntary, but roughly 85% of the workers belong to a fund, usually the one administered by their trade union.

Eligibility for the earnings-related unemployment allowance requires that the applicant has been employed for at least 43 weeks during the past 28 months before entering unemployment and has been a member of a UI fund for at least ten months before becoming unemployed. Those unemployed who do not belong to a UI fund, do not fulfil the employment condition, or who have exhausted their UI benefits are eligible for the labour market subsidy or flat-rate basic allowance. In 2002 the full rate of both the labour market subsidy and the basic unemployment allowance without child supplements was 22.75 euros per day, or 21% of the median wage.

The earnings-related allowance consists of a basic component equal to the basic allowance and an earnings-related component that is 45% of the difference between the previous daily wage and the basic component. There is no cap in the benefit level but the benefits are regressive so that monthly wages exceeding 2,047 euros (in 2002) increase the benefits by 20% only of the exceeding amount. For a median earner (2,300 euros/month) the earnings-related benefits are 52% of the pre-unemployment wage. For a low-income earner (1,500 euros/month) the replacement rate is 60% and for a high income earner (4,000 euros/month) 38%. In 2002, the average earnings-related benefit was 41.30 euros per day.

The earnings-related unemployment allowance can be paid for five days per week up to 500 days after which those who are still unemployed may receive the labour market subsidy. At the end of 2002, a total of 130,000 persons were receiving the earnings-related allowance, 19,000 the basic unemployment allowance and 151,000 the labour market subsidy.

An important feature of the Finnish Unemployment benefit system is a benefit extension for those who are over 54 when they become unemployed. These unemployed workers can receive earnings-related unemployment benefits up to age 60 and then apply for an unemployment pension. This benefit extension has dramatic effects for the unemployment rates for those over 54. (Hakola & Uusitalo, 2005; Kyyra & Wilke, 2007). To make sure that the changes in the early retirement schemes do not affect our estimates regarding the changes in the UI benefits we exclude all persons over 54 from the analysis.

2.1 The 2003 reform

Since January 1st 2003 those workers who became unemployed were eligible for increased earnings-related benefits if they (i) had lost a permanent job for "economic or production-related reasons", (ii) had been members of an UI fund for at least five years before losing their job, (iii) had at least 20 years of employment history, and (iv) had not received severance pay during the past five years.

The reform increased the earnings-related component of the unemployment allowance from 45% to 55% of the difference between the daily wage and the basic allowance. The increase also affected the higher earnings bracket. There the earnings-related component increased from 20% to 32.5% of the wages exceeding the threshold. The increased benefits could be paid up to 150 days, after which those still unemployed were eligible for the usual earnings-related benefits.

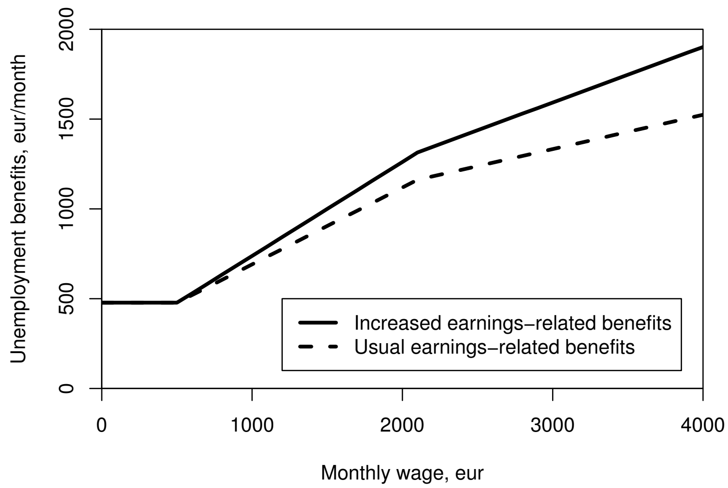

Figure 1: Earnings-related UI benefits as a function of the pre-unemployment wage.

Figure 1 displays the effect of the reform on the unemployment benefits by plotting the monthly UI benefits against the pre-unemployment monthly wage in 2003. On average, the reform increased the unemployment benefits for those unemployed workers who were eligible by 8.72 euros per day i.e. by about 15%. The replacement rate for an eligible median earner increased from 52% to 60%. The increases in the replacement rates were larger for high-income earners and smaller for low-income earners.

Figure 2 illustrates the time profile of the unemployment benefits for the median earner before and after the reform. For the unemployed who are not eligible for the increased earnings-related benefits the replacement rate is 52% for the entire 500-day eligibility period. After 500 days the unemployed can receive labour market support, which implies a drop in the replacement rate to 21% for the median earner. The reform increased benefits for the unemployed who were eligible for the increased earnings-related benefits over the first 150 days. For this group the reform creates a declining time sequence of benefits where the replacement rate for a median earner is 60% for the first 150 days, decreases then to 52% and decreases again to 21% after 500 days of unemployment.

According to the government proposal to Parliament (dated September 2002) the main motivation for the changes that took place in 2003 was to simplify legislation that governed the unemployment benefit system. In this spirit, it was proposed that the severance pay system that existed prior to 2003 would be merged into the unemployment benefit system. The government proposal noted that the severance pay system was created in 1970, when the unemployment insurance benefits were much lower and not all workers were covered by the unemployment

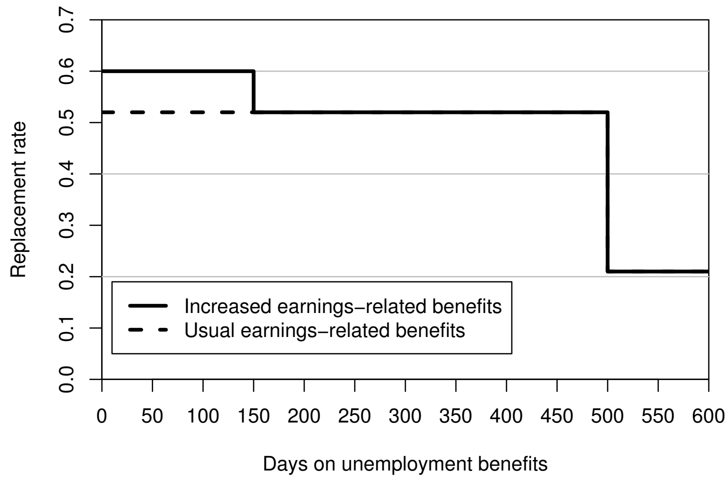

Figure 2: Replacement rate for a median earner without children.

insurance system. The proposal stated that the severance pay system had become a separate and unnecessary additional benefit.

Severance pay was a lump sum payment paid by the Redundancy Payment Fund for the workers who had lost a permanent job due to plant closing or downsizing and whose re-employment was expected to be difficult due to "age or other reasons". The lower age limit was 45 and the required continuous work history was 5 years on the previous employer or 8 years on the two previous employers. The size of the severance pay depended on age, previous earnings and number of years employed with somewhat different rules in different sectors. On average, severance pay corresponded to roughly one month's pay.

The government proposed replacing severance pay with higher earnings-related benefits for the first 130 days of the unemployment spell. The increase in benefits was calculated so that in absence of behavioural effects the expected direct cost for the UI funds would be unchanged. This also implies that the expected value of benefits for the unemployed evaluated at the start of the unemployment spell remained roughly constant. Therefore the reform should not have large effects on the incidence of unemployment. As only the unemployed with long work histories were eligible for severance pay, increased benefits were also tied to the length of the previous work history. Parliament eventually changed the proposal so that the length of the increased benefit period was extended to 150 days.

For most unemployed the reform implied replacing a lump-sum severance pay at a start of the unemployment spell with higher unemployment benefits for 150 days. The effect of the reform should therefore be interpreted as an effect of a change in the benefit profile rather than an effect of only increasing benefits. However, the eligibility rules for the severance pay were not entirely the same as the rules for increased benefits. There are small groups that were either would have been eligible for the severance pay but not eligible for the increased benefits (estimated 7% of the control group) or would not have been eligible for the severance pay but were eligible for increased benefits (estimated 27% of the treated group). Since we do not have sufficiently detailed data to determine exactly who would have been eligible for the severance pay, we will ignore them for most of the analysis, but return to the issue in the end where we analyse separately the effects of the reform according to our classification of the likely eligibility for the severance pay.

2.2 Other simultaneous changes

Change in the unemployment benefit system rarely takes place in isolation. Other macroeconomic changes and other changes in legislation that are implemented simultaneously may also affect the changes in unemployment duration. As noted by, for example, Card & Levine (2000) and Lalive & Zweimüller (2004), an increase in benefits may also be an endogenous policy response to an increase in unemployment. The effect may naturally also work in the opposite direction. Increasing unemployment may force the government to curb unemployment benefits in order to reduce the effects of increasing unemployment on the government budget. Both of these mechanisms would make the benefit level endogenous with respect to the re-employment probabilities and cause a bias in the estimated effect of the benefit change.

Finnish economic development during the past twenty years has been extremely volatile. Starting from a very low level of about 3% in 1990, the unemployment rate rose rapidly to around 17% in 1994. After that, unemployment declined to around 9% in 2001. Then the decline halted, and around the date when the UI reform was implemented the unemployment rate had been quite stable for two years. As illustrated in Figure 3, seasonally adjusted unemployment remained very close to 9% from the beginning of 2001 to the summer of 2004 and started to decrease only in the end of 2004. This is important for our analysis because it indicates that the increase in UI benefits in January 2003 was not a response to worsening re-employment opportunities but can safely be treated as an exogenous event with respect to job-finding rates.

Other changes in legislation that took place around the reform date had to do with an increase in the general benefit level and loosening of the employment condition. As we argue below, neither of these changes should have major impacts on our estimates for the reform effects.

Earnings-related benefits increased for all unemployed persons on March 1st 2002, ten months before the UI benefit reform that we analyse in this paper. This change increased the earnings-related component from 42% to 45% of the difference between the daily wage and the basic allowance. Since the change

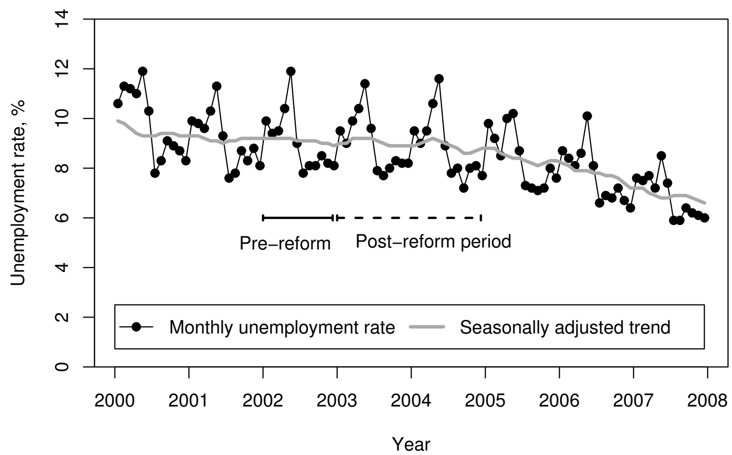

Figure 3: The monthly unemployment rate and its seasonally adjusted trend between 2001 and 2005. Source: Labour Force Surveys, Statistics Finland.

affected all the unemployed, its effects can be accounted for by using a difference-in-differences approach. We also experimented by restricting the sample so that only those who entered unemployment after March 1st 2002 were included in the sample, with no effects on the results.

In 2002, the general eligibility requirement for the unemployment allowance was that the unemployed should have 43 weeks (about 10 months) of employment history during the previous 2 years and 4 months before the start of the unemployment spell. In 2003, this condition was loosened so that after exhaustion of the 500-day benefit entitlement, only a 34-week employment spell was required to re-qualify for benefits. This made re-qualifying for UI benefits easier and could increase the incentives to search for temporary employment via the entitlement effect, but we would argue that the effect is likely to be minor. In any case, this change also affected all the unemployed workers, so we can control for the effect using a suitable difference-in-differences approach.

3 Data

We analyse the effects of the benefit reform using individual-level administrative data from the Ministry of Labour, the Insurance Supervisory Authority and the Finnish Center for Pensions.

The Ministry of Labour (MOL) register covers all job-seekers registered at the unemployment agencies. Since registering at an unemployment agency is a requirement for receiving UI benefits, practically all the unemployed workers are in the database. The data contain information on the initial and final dates of each unemployment spell. Also the reasons for the entry and exit are recorded in the data. Therefore, those who enter unemployment because they were fired for "economic or production-related reasons" and who, therefore, may be eligible for increased unemployment benefits can be identified from the data. We can also analyze exits from unemployment into employment, to out of the labour force and to various labour market programs separately. Background data on individuals is also available from the register, including sex, age, education, occupation, region and previous unemployment history. The major weaknesses of the dataset are that it contains no information on pre-unemployment wages, on the unemployment benefits or even on the eligibility for the earnings-related benefits.

We complement the information in the MOL database with information on the unemployment benefits from the registers of the Insurance Supervisory Authority (ISA). Each quarter the UI funds submit detailed reports of the benefits paid during the quarter to the ISA. These reports include daily benefit amounts and days compensated itemised by the individual and the four-week period. The benefits are further disaggregated so that increased benefits are reported separately. Data also include the date when the individual joined a UI fund, which is needed for determining eligibility for increased benefits. Another useful variable in the database is the remaining days of the benefit eligibility at the end of each quarter, a number that is extremely hard to calculate in a reliable way based on unemployment spell data alone.

The final piece of information required for determining the eligibility for higher benefits comes from the registers of the Finnish Centre for Pensions. The UI funds check the twenty-year work history requirement from the pension registers. We use the same source and add to each worker the information on the number of months worked after turning 18. This information has been recorded in the pension records since 1962, when the current earnings-based pension system was created.

The Finnish data protection laws make it difficult to use data on the entire population for research purposes. For this study, we managed to obtain a 50% sample from persons entering unemployment between January 1st 2002 and December 31st 2004. Since the reform increased the UI benefits for those with at least 20 years of work experience, the average eligible unemployed are well over forty years old. To allow flexible choices of comparison groups we included in the data all unemployed persons over 37 at the start of their UI spell. We follow these individuals until the end of 2005. By then all those unemployed whose unemployment spell started in 2002 or 2003 will have exhausted their 500-day benefit eligibility. Many unemployment spells that started in 2004 are still ongoing at the end of 2005. These spells are treated as censored observations at that point.

We also treat as censored observations all unemployment spells that end for any other reason than job finding, and all unemployment spells that are ongoing after 600 days.[2]

By drawing the sample from different registers, using the same criteria, we can match the data from different registers. While linking the individuals is relatively easy, linking the unemployment spell dates from different sources turned out to be burdensome. The details of the matching procedures used are in the Appendix.

In the final dataset used in the analysis the observation unit is an unemployment spell. Time is measured in days of benefit recipiency (5 days per week). We focus on the unemployed who lost a permanent job and keep only those who had no previous unemployment spells during the previous three years, counting backwards from the date of entry into unemployment. Only the unemployed who receive some earnings-related benefits are included, since the ISA data contains no information on those who are not receiving these benefits. All time-varying background information is recorded at the starting date of each spell.

Descriptive statistics

In Table 1 we report some descriptive statistics of the sample that is used in the analysis. We report these statistics separately before and after the reform and separately for the treatment group that became eligible for increased benefits and for the comparison group whose benefits remained unchanged.

There are some clear differences between the treatment group and the comparison group. Since the key criterion for eligibility was the length of the previous work history, it is natural that the treatment group has more work experience. The treatment group is also, on average, older and has higher earnings than the comparison group. On the other hand, the average level of education is lower in the treatment group, reflecting the fact that those with more education have, on average, less work experience at a given age and the fact that younger generations tend to have more education. Also, the occupational distribution is somewhat different. A large fraction of the treatment group had been employed in manufacturing occupations, while healthcare occupations are over-represented in the comparison group.

Since we will be evaluating the effects of increased UI benefits by comparing the changes in the re-employment rates between the eligible and ineligible unemployed, we will have to assume that the composition of the unemployed does not change in a different way among the eligible and the ineligible unemployed. In the second last column of Table 1, we present p-values from testing this assumption. We run simple linear regression models explaining each background

[2] The reason for exit is missing or unknown in 5% of the spells. Examining the labour market status at the end of the year reveals that most of these are employed. We therefore code all these as having found a job.

characteristic with the eligibility and post-reform dummies and their interaction, and test whether the coefficient of the interaction term is zero. For the categorical variables the test is based on a multinomial logit-model, where we explain probabilities that a categorical variables take certain values and test with a likelihood ratio-test that the effect of the interaction of the eligibility and post-reform dummies on these probabilities is zero.

For most background characteristics there are no signs of different changes in composition between the eligible and ineligible groups. Only the change in occupational distribution seems to be significantly different. Examining the changes in actual distributions reported in Columns 1–4 reveals that even these differences in changes appear to be small and should not be a major concern after including controls for occupation in the equations that we estimate. The increase in the UI benefits is naturally significantly larger in the eligible group, because their benefits were affected by the reform. The descriptive statistics on the reason for exit suggest that the reform might have had an effect on the re-employment rates. The fraction re-employed decreases in the eligible treatment group while it increases in the ineligible comparison group.

As reported in the fourth column of Table 1, only 69% of the group that should have been eligible for the increased benefits actually received higher benefits according to the ISA data. As reported in the second column, also about 8% of the ineligible group also received increased benefits according to the ISA database. This reflects classification errors in eligibility. We will discuss its implications after presenting the basic results. In the last column of Table 1 we report the descriptive statistics for the group that actually received increased benefits. A comparison of the actual recipients and all who should have been eligible after the reform reveals no large differences, which indicates that there are no clear signs of selectivity within the treatment group.

4 Methods

According to the search theory, an increase in the unemployment benefits increases the reservation wages and decreases the incentives to search for work affecting the exit rates from unemployment during the entire benefit period. The reduction in job finding rates is strongest at the beginning of the unemployment spell because at that point the change in the value of the remaining future benefits is the highest. By the time the unemployed have received increased UI benefits for 150 days, the benefits are reduced to the normal level, and the search intensity should increase to the pre-reform level. At this point the search intensity may be even higher than before the reform because of the “entitlement effect” i.e. the increase in the value of finding a job that could re-qualify for higher benefits.

To evaluate the effect of the benefit increase we have to model the effects on

Table 1: Descriptive statistics

	Ineligible		Eligible		Diff-in-diff	Actual
	Before	After	Before	After	p-value	recipients
Age	44.2	44.2	48.8	48.7	0.606	48.7
Male	0.44	0.45	0.56	0.56	0.476	0.50
Education					0.350	
Primary	0.16	0.11	0.32	0.25		0.24
Secondary 1	0.12	0.12	0.12	0.12		0.13
Secondary 2	0.39	0.40	0.37	0.38		0.38
Lower tertiary	0.18	0.19	0.11	0.14		0.16
Higher tertiary	0.15	0.18	0.08	0.11		0.10
Occupation					0.006	
Other	0.03	0.03	0.03	0.02		0.01
Specialist	0.15	0.17	0.10	0.12		0.11
Healthcare	0.10	0.10	0.04	0.02		0.01
Administration	0.17	0.18	0.14	0.15		0.18
Commercial	0.13	0.12	0.11	0.14		0.16
Transport	0.05	0.04	0.03	0.04		0.04
Construction	0.06	0.06	0.06	0.06		0.04
Industrial	0.23	0.20	0.41	0.40		0.39
Services	0.09	0.09	0.07	0.06		0.06
Previous wage, eur/mo	1866	1963	2026	2172	0.145	2174
Disability	0.05	0.06	0.05	0.05	0.487	0.04
Work experience	18.6	18.4	26.9	26.6	0.997	26.3
UI-fund membership duration	10.4	10.3	17.7	17.7	0.160	16.0
Daily benefits, eur	51.42	54.14	52.97	61.47	0.000	64.02
Receives increased benefits	0	0.08	0	0.69	0.000	1
Reason for entry					0.836	
Unknown	0.05	0.05	0.14	0.11		0.03
Displaced	0.22	0.24	0.86	0.89		0.80
Other	0.29	0.28	0.00	0.00		0.03
Temporary contract ended	0.44	0.43	0.00	0.00		0.14
Reason for exit					0.002	
Re-employed	0.47	0.49	0.44	0.40		0.38
Unknown	0.05	0.07	0.04	0.05		0.03
Exit from labour force	0.42	0.37	0.46	0.46		0.50
End of follow-up	0.06	0.08	0.06	0.09		0.09
N	5483	10327	1422	2652		2700

Note: The entries in the table are mean values calculated separately according to the eligibility status and separately for the unemployment spells starting before and after January 1st 2003. The p-values reported in fifth column are based on the test of the hypothesis that sample composition changes in a similar way in the eligible and in the ineligible groups. The rightmost column report mean values for those actually receiving increased benefits.

the exit hazards in a way that allows different effects at different points during the unemployment spell. We do this by specifying a proportional-hazard model with a flexible baseline hazard and time-varying effects of the benefit increase. Although the determinants of the hazard rate are also interesting, we are primarily interested in the changes in the baseline hazard that are due to the reform. The empirical hazard function is

$$\theta(t) = \lambda(t) \exp\{x\beta\} \tag{1}$$

where $\lambda(t)$ is a time-varying baseline hazard function, x a vector of time-invariant individual characteristics measured at the start of the unemployment spell, and t indexes weeks on benefits starting from the date of entry into unemployment. We assume that the baseline hazard function is constant within each four-week interval but place no restrictions on the change in the baseline hazard between these intervals. To reduce the noise in the estimates at long durations we aggregate the intervals where the hazard is assumed to be constant to 12 weeks after 48 weeks in unemployment.

$$\lambda(t) = \exp\{\sum_{i=1}^{12} \lambda_i I\left(4(i-1) < t < 4i\right) + \sum_{i=13}^{18} \lambda_i I\left(12(i-9) < t < 12(i-8)\right)\} \tag{2}$$

To identify the effects of the benefit increase on the hazard profile we then compare the changes in the interval-specific hazard rates in the treatment and the comparison group using a difference-in-differences approach

$$\lambda_i = \beta_{i0} + \beta_{i1} TREAT + \beta_{i2} REFORM + \beta_{i3} TREAT \times REFORM, \tag{3}$$

where $TREAT$ is an indicator of the eligibility for increased benefits and $REFORM$ an indicator that the unemployment spell started after January 1st 2003. We are primarily interested in the coefficients of the interaction terms (β_{i3}) that measure the differences in the changes of the hazard rates after the reform between the treatment and the comparison groups.[3]

We interpret the differences in the change of the hazard between the treatment and the comparison groups as the effect of the reform at a certain interval of elapsed unemployment duration. Strictly speaking, this interpretation is only valid at $t = 0$. If there is unobserved heterogeneity, and if the increase in the benefits in the treatment group lowers the re-employment hazards, the remaining

[3] Note that we do not restrict the duration dependence to be similar in the treatment and the comparison groups but we estimate all β_{i1} terms freely. However, to reduce the number of parameters we assume in the empirical analysis that duration dependence is constant over time i.e. that $\beta_{i2} = \beta_2$ for all $i = 1, \ldots, 18$.

Table 2: Median duration of unemployment, days.

	Before Jan 1st 2003	After Jan 1st 2003	Difference	Difference-in-differences
Comparison	127	118	-9	
	(3.1)	(1.9)	(3.6)	
Treatment	126.5	137	10.5	19.5
	(4.5)	(4.1)	(6.1)	(7.1)

Note: Bootstrapped standard errors with 2000 replications in parenthesis.

unemployed in the treatment group will be more employable than the remaining unemployed in the comparison group at dates $t > 0$. This could cause an upward bias in the effect estimates implying that if we find negative effects on re-employment hazards the true effect is even larger.

We also estimate a more restrictive model where the benefit increase has a constant proportional effect at all elapsed durations. This model is nested within the more general model, allowing a simple test of constant effects. Even if the constant effect model is rejected, the results are interesting, as they provide a point of comparison with most previous studies that have imposed this restriction.

5 Results

We first compare the changes in duration of unemployment in the treatment and the comparison groups after the reform. In Table 2 we report the median durations for all UI benefit spells without any restrictions on the reason for exit. It turns out that the median durations are very similar in the treatment and comparison groups before the reform. After the reform on January 1st 2003 the median duration declined in the comparison group but increased in the treatment group. The decline in unemployment duration in the control group is probably due to the business cycle effects. As we showed in Figure 3 the unemployment rate started to decline in the end of 2005. A simple difference-in-differences estimate comparing the changes in the treatment and comparison groups indicates that the reform increased the median duration by 19.5 days. The difference-in-differences estimate is highly significant with a bootstrapped standard error of 7.1 days.

The comparison of median durations does not tell whether the effect is due to changes in the job-finding rates or changes in the exit rates to other destinations. In addition, it provides no evidence on whether the effect is due to a decrease in the re-employment rates at the beginning of the unemployment spell as predicted by the search theory or to a change in the employment prospects for the long-term unemployed. These factors are accounted in the hazard model reported below.

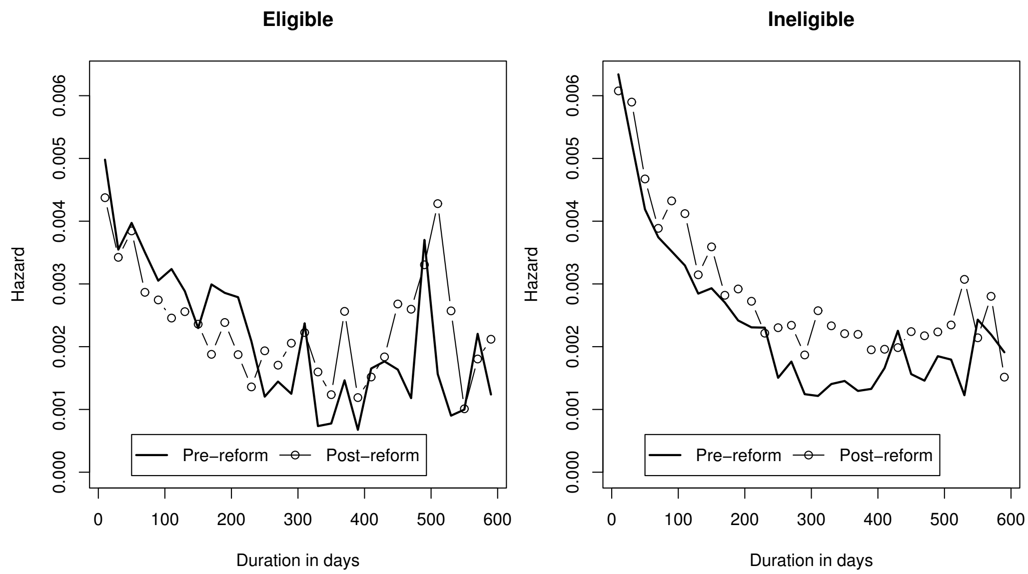

Figure 4: Re-employment rates before and after UI reform.

First in Figure 4 we display the unconditional hazard rates of exiting into employment in each four-week interval separately for the treatment and the comparison groups. Exits out of the labour force and into labour market programs, as well as ongoing spells after 600 days, and ongoing spells at the end of 2005 are treated as censored observations. The figure indicates that re-employment hazards decrease rapidly at the beginning of unemployment spells. This could be due to genuine duration dependence or heterogeneity in the re-employment rates. Since we are using single spell data, differentiating between duration dependence and heterogeneity is empirically difficult and since it is not a key question in this paper, we make no serious effort of doing it. As the unemployed approach the expiry date of unemployment benefits (500 workdays), the job-finding rate starts to increase in both groups, though the effect seems to be stronger among those eligible for increased benefits. The shape of the hazard rate is consistent with previous research (e.g. Meyer, 1990) and has been interpreted as evidence of the effect of the limited duration of UI benefits. Note, however, that this conclusion is not based on a comparison with some other group whose benefits do not expire after 500 days. In fact, Kyyra & Wilke (2007) use Finnish data to show that extending the duration of benefits beyond 500 days for workers over 54 dramatically reduced the job-finding rates throughout the unemployment spell, and not just close to the benefit expiry date.

Comparing the hazard rate before and after the reform reveals that the re-employment hazards decrease in the treatment group but only at the beginning

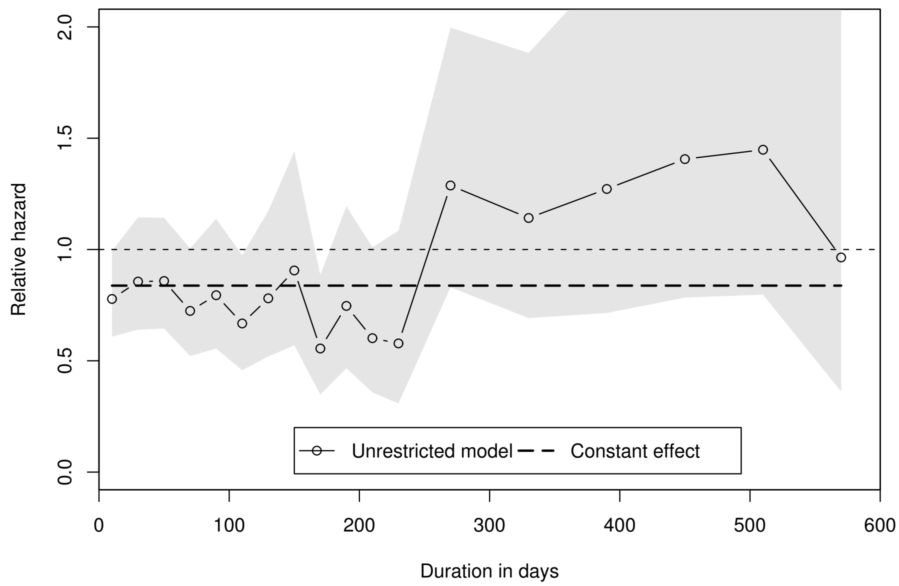

Figure 5: Effect of the reform on the re-employment hazard. Note: The grey area indicates 95% confidence intervals.

of the unemployment spell. After about 200 days on benefits, the hazard rates are higher than before the reform but the estimates are rather noisy. In the ineligible comparison group the increase in re-employment hazards is roughly constant across different points of elapsed duration.

To account for any observable differences in the composition of the treatment and the comparison groups we estimate a proportional-hazard model as described in the previous section. In addition to the treatment status and the reform effects we add to the model indicators of age, sex, disability, education (5 categories), broad occupation (9 categories), region (15 categories), previous work experience, duration of UI-fund membership, pre-unemployment wage, reason for entry into unemployment (5 categories) and indicators for the month and year when the unemployment spell started. Duration dependence is accounted for by 18 duration-specific dummies and the difference in duration dependence between the treatment and the control groups with a set of another 18 dummies. These parameter estimates can be found in the Appendix. Here we concentrate on the reform effects.

Figure 5 plots these estimates specified so that each point in the figure refers to the reform effect at a specific interval of elapsed benefit duration. The estimates are presented as relative hazards with 1 indicating no effect. We use four-week intervals up to 48 weeks in unemployment then aggregate the data into twelve-week intervals. The hollow circles report the unconstrained estimates where the effect of the reform on the re-employment hazard may vary freely across the elapsed duration of unemployment. These estimates indicate that the increase in benefits caused a substantial decline in the re-employment hazard but that the effect only occurs during the first 250 days of unemployment. After that, the effect is not significantly different from zero and the point estimates are positive. These estimates imply that the benefit increase increased the expected time until re-employment by 31 days or about 11%. Although many point estimates are not statistically significant, the estimated hazard profile is nicely in line with the predictions of search theory. However, taken at face value our estimates indicate that the unemployed do not anticipate the drop in benefits but increase their effort only after the higher benefits have expired.

The dashed line presents estimates from a model where the effect of the reform on the re-employment rates is restricted to be equal across all elapsed durations. The point estimate indicates a 16% decline in the hazard and the estimate is highly significant ($z = 3.4$, $p = 0.001$). According to a likelihood ratio test the restrictions implied by the constant-effect model are not rejected when tested against the unrestricted alternative ($p = 0.14$). We also experimented with a model where the log-hazard is a linear function of elapsed duration up to 150 days and constant thereafter following the idea by Bennmarker et al. (2007). The resulting estimates show that the reform had a significant negative effect at the start of the unemployment spell and that the effect decreases over time being close zero after 150 days as search theory would predict. Still, though not rejected in a likelihood ratio test against the unrestricted alternative, such a linear model does poor job in describing the pattern of the estimates in Figure 5. A somewhat better fit is achieved using an ad hoc two parameter step function that allows the effect to be different in the period with increased benefits and in the period when these benefits have expired. These estimates indicate a significant 25% decrease in the hazard during the first 150 days and an insignificant 6% decrease thereafter.

Our results can be best compared with Lalive et al. (2006) and Bennmarker et al. (2007) who also estimate the effects of benefit increases on the entire hazard profile. Lalive et al. find that the unemployed seem to be more sensitive to the change in the benefit duration than benefit level. According to their estimates a 15% increase in benefit level increased the time until re-employment by about 2% which is about a fifth of the effect that we find. Lalive et al. note that old workers react more strongly to a benefit change which may explain part of the difference. Bennmarker et al. report puzzling results according to which a 17% benefit increase decreased duration of unemployment for women. Their estimate for men is positive and larger than our estimates. Even for men the effect is smallest at the beginning of the benefit spell where the largest impact is expected.

6 Extensions

Anticipatory behaviour

One of the concerns in the previous research has been that the unemployed may anticipate the changes in the benefit system. Search theory assumes that the unemployed are aware of the expiry date of UI benefits and increase their search efforts before the benefits actually expire. In a similar way, the unemployed might already react to the change in the benefit system already before the reform date if the change can be anticipated. It would be awkward to assume that the unemployed are forward-looking with respect to their future benefit sequence but completely myopic with respect to a change in the benefit system. For example, Carling et al. (2001) note that a benefit reform had already affected the hazard rates of exiting unemployment already several months before the policy change.

In the Finnish UI reform the benefit increase applied only to those entering unemployment after January 1st 2003. The benefits remained unchanged for those already unemployed on the reform date. By comparing the change in the hazard profile before and after the reform, we therefore compare the unemployed whose benefit sequence changes for the entire unemployment spell and avoid the confusion between future changes in the system and future changes in the benefits under a given benefit system.

However, there might still be anticipatory effects if the change in the benefit system had an effect on the incidence of unemployment. We are primarily concerned about the potential effects of changing a lump-sum severance pay to higher benefits. Even though the expected value of increased benefits in the whole eligible population is roughly equal to severance pay, it is possible that those who expect to find jobs quickly would try to affect the timing of dismissals so that they could still be eligible for severance pay. Such strategic timing of dismissals could affect our results.

By calculating the descriptive statistics in Table 1 separately for the eligible and the ineligible group we could already demonstrate that the reform did not have major effects on the composition of the new entrants. Figure 6 attempts to provide further evidence on the question by reporting the monthly numbers of new entrants into unemployment around the reform date. The figure displays clear seasonal variation in the entry rates but no pattern that would suggest systematically higher entry rates just before the reform in the group eligible for severance pay. As a robustness check, we also dropped those entering unemployment in November or December, from the data with no notable changes in the results.

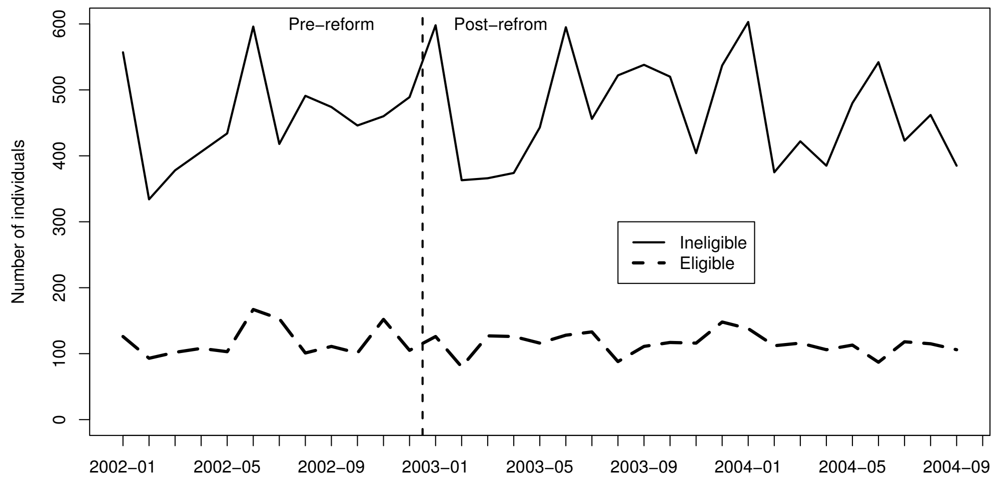

Figure 6: Number of new unemployment spells by month in the treatment and the comparison groups.

Placebo reforms

Our estimates are based on an identifying assumption that the re-employment rates would have evolved in a similar way in the treatment and the comparison groups if the UI reform had not occurred in 2003. This is a standard assumption in a difference-in-differences approach but is valid only if there are no group-specific pre-existing trends. A common way to test this assumption is to estimate the effects of placebo reforms by recoding the reform date to a pre-reform period when no changes took place. While choosing such placebo reform dates is somewhat arbitrary, finding that a reform that did not happen had no effects may increase confidence on the actual reform evaluation.

Unfortunately we can not implement such a specification test in a standard way as we only have data available from the start of 2002. Using only post-reform data and estimating the effects of a placebo reform in the beginning of 2004 is not quite as attractive since it would compare two post-reform periods and the results could be affected, for example, by an increase in the take-up rates over time as the unemployed become more aware of the benefit increase. Instead, we used data only for those unemployed who had less than 19 years work experience and were therefore not eligible for increased benefits. We arbitrarily coded the treatment group as those displaced who had at least 15 years work experience.

These estimates revealed no clear patterns and the constant effect estimate was clearly not significant ($p = 0.48$) Admittedly, also precision suffers as the sample size is reduced.

Classification errors

A potentially more relevant question has to do with classification error in the eligibility for benefits. The eligibility for increased unemployment benefits depends on the work history, the UI-fund membership and previous unemployment experiences. In an ideal case we could observe all these factors and evaluate the effect of benefit increase by comparing the changes in exit hazards between the eligible and ineligible groups.[4] Unfortunately, none of these criteria can be precisely determined from the data.

The problem in identifying eligibility based on twenty-year work history criteria is caused by the fact that according to the Unemployment Security Act the twenty-year work history requirement may also contain spells of maternity leave, sick leave, military service, and disability that are not recorded in the pension register.[5] There is also some uncertainty about the length of UI-fund membership. The length of UI-fund membership is recorded in the data only for the current UI fund. Therefore, individuals who switched UI funds during the previous five years may be falsely classified as not fulfilling the membership criteria. Third, we have no information on the recipiency of severance pay in the past. The unemployed who received severance pay during the five years prior to entry into unemployment may, therefore, be falsely classified into an eligible group though they are not entitled to increased benefits. We mitigated this problem by excluding from the data all those unemployed individuals who had a previous unemployment episode during the three years before entry into unemployment. In practice, this also limits the analysis to those displaced from a relatively stable career, which is also the main target group of the reform. Finally, some of the unemployed may not be aware that they might have a right to increased benefits. UI funds provide advice for the applicants, but since many applications are received by mail without a personal contact, not all claimants receive this information.[6]

However, since both actual benefits and the information used to determine benefit eligibility are included in the data, the accuracy of predictions can be assessed by comparing the rule-based classification with the actual recipiency of the increased benefits in the post-reform data. Table 3 presents a cross-tabulation

[4] This would also allow us to use these limits in a regression discontinuity framework to evaluate the effects of the benefit increase.

[5] When claiming increased benefits, the unemployed who are close to fulfilling the twenty-year work history criteria must provide the UI fund with documentation about periods of maternity leave, military service etc.

[6] This explanation is based on personal communication with UI-fund managers in February 2005.

Table 3: Eligibility for increased benefits vs. actual recipiency after the reform.

		Received increased UI benefits	
		No	Yes
Eligible for increased	No	9458	869
UI benefits	Yes	821	1831

of the data according to whether an unemployed should be eligible for increased benefits and whether she or he actually received increased benefits. Based on information on the work history, the length of UI-fund membership, and the reason for entering unemployment we can correctly predict 87% of the actual benefit recipiency.

Correcting the effects of misclassification in the treatment status

By defining the treatment status according to the eligibility criteria that are available in our data we have estimated the effect of "the intention to treat". In an experimental setting this would be equivalent to including drop-outs in the treatment group and including cross-overs, who are assigned to the control group but still participate in the program, in the comparison group. If the classification errors are random, the effect of the program assignment is a downward-biased estimate of program participation. This bias can be corrected by using the treatment assignment as an instrument for the treatment status.

In our case, the recipiency of increased benefits for eligible individuals is observed only after the reform is implemented. We use post-treatment data to estimate a first-stage equation that explains the recipiency of increased unemployment benefits with variables that are included in the eligibility criteria. Then we use these estimates to predict the treatment status in both the pre-reform and the post-reform data and use the predicted treatment status as an explanatory variable in our duration model. The method resembles the two-sample IV estimate (Angrist & Krueger, 1992; Björklund & Jäntti, 1997) where two different samples are used to construct the moments required for a consistent IV estimate.

Simply replacing the treatment indicator in a nonlinear duration model with the predicted treatment status would not only lead to biased standard errors but can also lead to inconsistent estimates, as shown, for example, in Cameron & Trivedi (2005, p. 198). A simple solution suggested by Angrist (2001) is to ignore the fact that the model is nonlinear and estimate a constant effect linear probability model instead. This does not recover the structural parameters of the duration model but, as long as the covariates are discrete, it provides an appropriate description of the underlying causal relationship.

A second issue that arises in this setting is that, because the treatment is

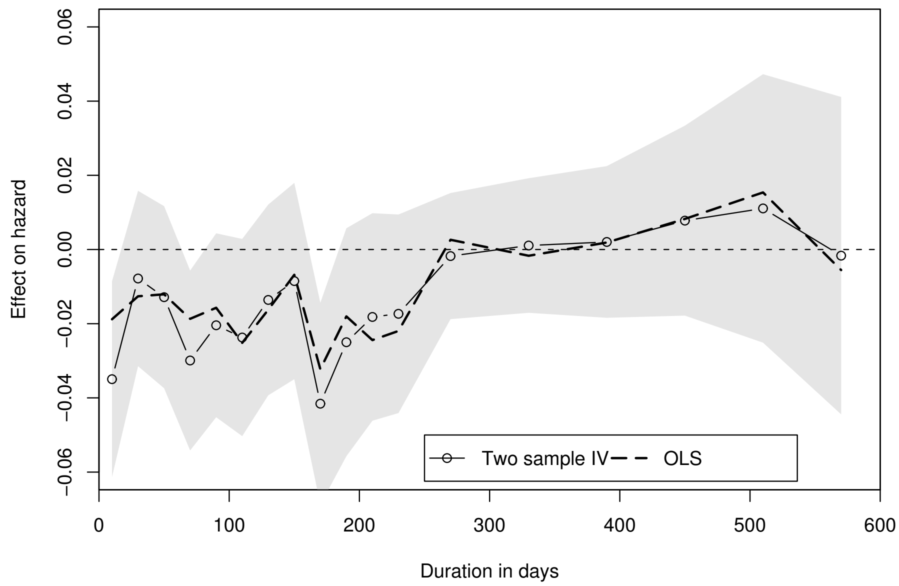

Figure 7: Effect of the reform based on discrete-time hazard function estimates. Note: The grey area indicates 95% confidence intervals of two-sample IV-estimates generated by bootstrapping with 1000 replications.

binary, a nonlinear first-stage model might be appropriate. However, conventional two-stage least squares estimates using a linear probability model in both the first-stage and the second-stage are consistent whether or not the first-stage is linear. (See Angrist, 2001). This argument generalises to an estimator where a linear prediction from the first-stage equation is plugged into the second-stage linear probability model. The only remaining issue has to do with biased standard errors. We deal with this by bootstrapping.

To implement the estimator we first estimate a linear probability model explaining benefit recipiency after the reform using all the covariates included in the duration model. We use these coefficients to calculate predicted probabilities of benefit recipiency in both the pre-reform and the post-reform data. We then formulate a discrete-time version of the duration model by splitting the unemployment spells into four-week intervals and explain job finding rates in each interval with the linear probability model using the original covariates and the predicted treatment status from the first-stage interacted with the reform dummy as described in equation (3). The second-stage is identified through omission of the interaction terms from the second stage.

Figure 7 reports the results from the discrete time hazard model. To ensure that the linear probability model and discrete hazard formulation produce similar results to our proportional hazard model results presented in Figure 5 we first present simple OLS results where job finding in each interval is explained by the rule-based assignment of benefit eligibility as in Figure 5. The line labelled as "two-sample IV" presents the results where eligibility rules are used as an instrument for benefit recipiency.

The effects reported in Figure 7 are measured as percentage point changes in the job-finding rates instead of proportional effects on the hazard rate. Qualitatively, the results from the discrete-time duration model are still reasonably similar to those based on the proportional-hazard model presented in Figure 5. The estimates show that job-finding rates decrease by about two percentage points in each four-week period during the first 250 days in unemployment. The difference between the OLS and the two-sample IV-estimates is small, indicating that misclassification concerning the treatment status has only a small effect on the estimates. Both estimates are close to zero after 250 days, but the standard errors are large at long durations.

Accounting for the severance pay

Our data allows us to estimate eligibility for the severance pay based on age, displacement status and the length of work history but we are not able to determine if the length of employment with the previous employers fulfils the eligibility criteria. Using the three first criteria we estimated that 74% of the sample would not have been eligible for either severance pay or increased benefits while around 15% would have been eligible for both systems had they been in place when they got unemployed. Using data on only these 89% of the unemployed in the sample results to virtually identical estimates than those presented in Figure 5. This implies that replacing a lump-sum payment with higher benefits caused a substantial increase in unemployment duration.

To isolate the effect of severance pay from the effect of increasing UI benefits, we also estimated the effects of the reform separately for the different groups based on estimated eligibility for the severance pay. Unfortunately this leads into rather small group sizes. Both the group that was not eligible for the severance pay but would have been eligible for increased benefits and the group that was eligible for the severance pay but not for increased benefits are smaller than 6% of the sample. This makes estimates that attempt to measure the effect of benefit increase on the hazard profile imprecise. A constant effect model indicates that the benefit increase decreased re-employment hazard by 10% among those not eligible for severance pay and by 20% among those eligible for the severance pay according to the pre-reform rules. Neither estimate is significantly different from the effect estimated using the whole sample (16%).

7 Conclusion

Concerns about the effect of job destruction on the most vulnerable groups increase the demand for social insurance provided by the unemployment benefits. While higher benefits may cushion the effect of job loss in groups that have the greatest difficulty in finding new employment, such benefit increases also have a side effect of decreasing the incentives to search for new jobs. In this paper we have evaluated the effects of improving unemployment benefits for a group of older workers. According to our results the effects of benefit increase on re-employment rates may be substantial. Based on our estimates one can calculate that a 15% increase in benefits for the first 150 days of unemployment increases the expected time until re-employment by 31 days or about 11%. This implies that the elasticity of time until re-employment with respect to the benefit level would be 0.75. However, since many unemployed individuals exit from the data for other reasons before finding work, this number cannot be directly interpreted as an effect on the duration of unemployment.

We also find that an increase in UI benefits decreases the re-employment hazard but the hazard rate returns to the pre-reform level once the period of increased benefits expires. We find no evidence that the unemployed anticipate the change in the benefit level by increasing their search effort before the benefits are decreased. In contrast, it seems that a decline in benefits increases re-employment rates only about one or two months after the benefits have been reduced. Taken at face value, this would imply that the unemployed are myopic and start searching more actively only after benefits have been reduced.

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Appendix 1: Data

The analysis data is constructed by linking data from three administrative registers. The primary data source is the database of the Ministry of Labour, which contains all unemployment spells and a rich set of covariates. However, this database contains no information on the unemployment benefits. Earnings-related benefits are administered by the Insurance Supervisory Authority. Further, information on work experience is obtained from the Pension Security Institute. The supplementary data are linked to the unemployment spell data by using individual identifiers and payment dates.

Sampling

A representative inflow sample was drawn from the unemployment spell database. The sample contains all unemployment spells that began between 1.1.2002 and 31.12.2004 for individuals born on an odd date before 1967. These individuals were followed until 31.12.2005. Unemployment spells with no match in the unemployment benefit data are excluded from the data. We believe that a majority of the excluded individuals are ineligible for earnings-related benefits and receive only the basic allowance or labour market support.

Unemployment spell data

The observation unit in the unemployment spell data is a spell. The data consists of 104,941 individuals between 37 and 66 years of age. They experience 474,144 unemployment spells from the beginning of 2002 to the end of 2004. To obtain a more consistent picture of the length of unemployment, consecutive spells with short interruptions are merged. Merging spells with less than a two-week break reduces the number of spells to 241,190.

Unemployment benefit data

The unemployment benefit data includes all unemployment insurance payments from 2002 to 2005. The observation unit is a payment report provided by the unemployment insurance fund. The reports contain the dates of compensation periods and the amount of daily allowance. An important variable in the data is a counter of used benefits days that is recorded at the end of each quarter. Earnings-related benefit is paid up to 500 working days except for those over 54 who may receive it until retirement. If the employment condition is fulfilled between spells, the eligibility is renewed.

The counter information is only updated quarterly. No essential information, therefore, is lost in merging subsequent payment reports on an individual level within a quarter. Before merging, inconsistent rows are removed (duplicated rows and payment periods within another period). In cases where two rows contain different values, high values of the daily allowance are preferred to low values. The same criterion is used for other earnings information such as previous salary.

Linking datasets

The unemployment spell data provides unambiguous information but the benefit data may contain conflicting information, due to discrepancies between reports. Our objective is to get a reliable estimate for the number of benefit days used at the beginning of each unemployment spell. When linking the datasets, we check that the matched report periods do not intersect with a subsequent unemployment spell. In case of multiple reports matching an unemployment spell, the report closest to the beginning of the spell is preferred. If benefit information is missing, we use subsequent reports to complete the data. Lastly, the work experience data is linked. Because the information is available only for the end of 2001 and 2002, the time out of unemployment between the date of information and the beginning of unemployment is computed. This sum should provide a fairly accurate estimate of the length of work experience at the time of unemployment.

Analysis sample

After linking the datasets, we have information on the amount of paid benefits and the number of benefit days for most of the unemployment spells. For some individuals with repeated short spells, no unique benefit report match was found for every spell. To complete missing information, the information is derived by the use of subsequent spells that begin within six months. After this operation, rows with incomplete information are removed, which leaves 192,973 rows in the dataset.

Many individuals experience multiple unemployment spells. Typically, this is either because of short employment spells between unemployment or participation in active labour market programmes. These individuals are not likely to be eligible for the increased benefit because the rules exclude those who have received severance pay earlier. Therefore, only the first unemployment spell is included, which restricts the number of rows to 97,618, which now equals the number of individuals. In addition, to take into account possible severance pay prior to 2003, all those individuals who have been unemployed during past three years before the beginning of the observed spell are removed. After this, the sample includes 34 082 individuals, of whom 39% fulfil the eligibility criteria for increased benefits. A large proportion of the sample consists of elderly people who are eligible for earnings-related allowance without a time limit. We focus only on individuals between 37 and 54 years of age, which gives us a sample of 19,884 individuals, of whom 20% are eligible for increased benefits.

Appendix 2: Coefficient estimates from an unrestricted model

		Coefficient	Std. Error
Intercept		-4.678	0.094
Age	(ref: 37-40)		
	41-46	-0.136	0.027
	47-54	-0.348	0.035
Sex	female	-0.057	0.024
Education	(ref: primary)		
	secondary 1	-0.050	0.041
	secondary 2	0.117	0.032
	tertiary 1	0.110	0.041
	tertiary 2	0.232	0.045
Occupation	(ref: agriculture)		
	specialist	-0.151	0.065
	health care	0.323	0.066
	administration	-0.293	0.064
	commercial	-0.187	0.065
	transport	0.050	0.074
	construction	0.584	0.067
	industrial	-0.191	0.062
	service	-0.022	0.067
Log wage	(ref: <1.37)		
	(1.37,1.63]	0.049	0.033
	(1.63,1.91]	0.080	0.034
	(1.91,2.36]	0.103	0.035
	>2.36	0.150	0.037
Region	(ref: Uusimaa)		
	Vars.Suomi	0.187	0.037
	Satakunta	-0.014	0.040
	Häme	0.067	0.045
	Pirkanmaa	0.161	0.062
	Kaak.Suomi	0.209	0.054
	E.Savo	-0.012	0.048
	P.Savo	0.131	0.052
	P.Karjala	-0.080	0.059
	K.Suomi	0.127	0.080
	E.Pohjanmaa	0.058	0.041
	Pohjanmaa	0.179	0.052
	P.Pohjanmaa	0.127	0.041
	Kainuu	0.075	0.041
	Lappi	0.263	0.058
Disability		-0.493	0.050

Experience	(ref: <12)		
	[12,17)	0.143	0.034
	[17,20)	0.208	0.038
	[20,23)	0.224	0.042
	[23,27)	0.235	0.046
	>=27	0.165	0.050
UI-fund membership	(ref: <3)		
	[3,5)	-0.174	0.038
	[5,7)	-0.138	0.040
	[7,15)	-0.197	0.030
	>=15	-0.226	0.032
Reason for entry	(ref: Unknown)		
	displaced	-0.571	0.040
	other	-0.861	0.044
	temporary	-0.164	0.041
Month of entry	(ref: January)		
	February	-0.117	0.046
	March	-0.095	0.047
	April	-0.100	0.048
	May	-0.153	0.048
	June	-0.121	0.044
	July	-0.044	0.044
	August	-0.137	0.045
	September	-0.189	0.046
	October	-0.165	0.046
	November	-0.135	0.047
	December	0.012	0.047
Year	(ref: 2002)		
	2003	0.068	0.026
	2004	0.142	0.025
Duration dependence	(weeks, ref: 1-4)		
	5-8	-0.060	0.039
	9-12	-0.267	0.043
	13-16	-0.412	0.047
	17-20	-0.347	0.048
	21-24	-0.386	0.050
	25-28	-0.605	0.057
	29-32	-0.496	0.057
	33-36	-0.678	0.064
	37-40	-0.685	0.067
	41-44	-0.741	0.071
	45-48	-0.866	0.078
	49-60	-1.002	0.056
	61-72	-0.953	0.061
	73-84	-1.033	0.072
	85-96	-0.915	0.078

	97-108	-0.843	0.088
	109-120	-0.763	0.106
Treatment group		0.127	0.106
Treatment * duration dependence			
	5-8	-0.265	0.157
	9-12	0.060	0.157
	13-16	0.089	0.168
	17-20	-0.110	0.180
	21-24	-0.006	0.183
	25-28	0.106	0.199
	29-32	-0.231	0.223
	33-36	0.221	0.211
	37-40	0.182	0.222
	41-44	0.218	0.233
	45-48	0.058	0.272
	49-60	-0.268	0.223
	61-72	-0.278	0.242
	73-84	-0.253	0.271
	85-96	-0.192	0.273
	97-108	0.117	0.276
	109-120	-0.346	0.382
Treatment effects			
	1-4	-0.251	0.125
	5-8	-0.156	0.148
	9-12	-0.153	0.146
	13-16	-0.323	0.167
	17-20	-0.230	0.183
	21-24	-0.404	0.193
	25-28	-0.248	0.208
	29-32	-0.099	0.237
	33-36	-0.588	0.238
	37-40	-0.292	0.240
	41-44	-0.508	0.265
	45-48	-0.548	0.321
	49-60	0.253	0.224
	61-72	0.132	0.256
	73-84	0.241	0.294
	85-96	0.341	0.298
	97-108	0.370	0.305
	109-120	-0.036	0.501
Log likelihood -68163			
n (spells) = 19884			
n (intervals) = 169632			

Chapter III

Scars of recession: the long-term costs of the Finnish economic crisis

Abstract

This study evaluates the long-term costs of unemployment in Finland by focusing on the deep recession period of 1991–1993. The number of plant closures increased sharply during the recession and the unemployment rate rose by more than 13 percentage points. In the analysis, prime working-age men who face unemployment due to plant closure are matched to those who remained employed during the recession. The effect of being unemployed during the recession is estimated for a 6 year follow-up period. In 1999, the unemployed individuals suffer a 25% loss in annual earnings, 10% reduction in employment and 14% wage scar.

1 Introduction

The development of the Finnish economy in the 1990s provides a unique setup for studying the costs of unemployment. After a boom in the late 1980's, the economy fell into a deep recession and the GDP dropped dramatically by 10.5% between 1990 and 1993. Consequently, the unemployment rate rose from 3.2% to 16.6%. The economy started to recover in 1994 but unemployment remained persistently high.

This study estimates the effect of becoming unemployed on future earnings and employment. This is done by analysing prime working-age men who were employed in a plant that closed down during the Finnish recession. The key idea is that plant closures provide an exogenous source of variation in unemployment as large groups of workers are displaced at once. The strategy mitigates the selection problem arising from employers' incentives to lay off the least productive workers first.

The studied outcomes are annual earnings, employment and wages in the post-recession period 1994–1999. These outcomes measure the deteriorating effect of unemployment on human capital and future employment prospects which are often referred to as scarring. This also includes the lost firm specific human capital which is typically the main focus in the displacement literature.

Mass layoffs and displaced workers have been analysed in a number of previous studies. These studies generally focus on situations where individuals are re-employed quickly and relatively little emphasis is placed on the macroeconomic conditions. The approach has not been used to study economic losses associated with severe recession previously.

The Finnish recession was an extreme economic event. The crisis provides ample possibilities to understand the consequences of a sudden increase in unemployment. A possible drawback is that the severity of the recession reduces the generalisibility of results. However, it is important for governments to understand the implications of severe recession if a similar event is ever to recur. Also similar structural changes take place in less severe economic downturns and studying a more extreme case is useful for understanding the mechanisms.

The analysed data set is a representative 10% sample of Finnish workers followed from 1987 to 2000. The register data includes detailed information on labour market history and annual earnings and contains a rich set of other individual characteristics. The key variable provides information on the reason for unemployment. It is obtained when displaced workers register as job seekers at a labour office and it is checked by the case workers. Using these data it is possible to identify plant closures. Such information is typically not available in register data. The data provide firm characteristics but does not contain a firm identifier.

Considering the validity of empirical analysis, the Finnish recession has several useful aspects. Many firms closed down during the recession making it possible to use plant closures instead of a broader definition of mass layoffs. The events leading to the recession were unexpected and took place quickly. This reduces the possibilities of firms and workers to anticipate the events which often weakens the validity of the analysis based on displacements. In the analysis, the individuals unemployed due to plant closure are compared with individuals who remained employed during the recession period.

To construct a valid comparison group, individuals are matched by the pre-recession income quantiles and a propensity score of being unemployed that is estimated using other characteristics. Matching accounts for compositional differences between the groups and for the fact that only displaced workers who register as unemployed are observed. A potential sorting bias arises from those who are able to find a new job before or immediately after the plant closure. It is argued that the severity of the recession makes the selection problem much smaller than during normal economic fluctuations. To informally test for selection on unobservables, the differences between the analysis groups are compared in the period before the recession.

The annual earnings, months in employment and wages of the individuals are observed until the end of 1999. The plant closure group suffers large and long-lasting losses in annual earnings. In 1999, the annual earnings penalty is 25% when compared with the mean earnings in the matched employed group. Most of the losses are explained by lower employment in the plant closure group than in the comparison group. The employment level is 10% lower and the estimated wage loss is 14% in 1999. This indicates strong unemployment persistence and noticeable wage scarring.

The paper is organised as follows. The next section discusses the related literature briefly. Section 3 presents the economic environment focusing on the institutional framework and the recession in Finland. The data are described in Section 4 and the empirical strategy is discussed in Section 5. Section 6 presents the results and the last section concludes.

2 Related studies

This study is related to two different branches of literature. Several recent British studies attempt to identify the causal effect of unemployment. Both Arulampalam (2001) and Gregory & Jukes (2001) analyse the effect of unemployment on men's hourly wages and use panel data methods to overcome the selection problem. Arulampalam (2001), for example, estimates a scarring effect of 14% three years after unemployment. Arulampalam (2002) uses a similar approach but focuses on unemployment persistence which she finds to be strong especially for older individuals.

Gregg (2001) and Gregg & Tominey (2005) study the effect of youth unemployment on adult labour market outcomes by using the local unemployment rate at age 16 as an instrument. The first study estimates that 3 months of youth unemployment leads around 1 month of unemployment 10 years later whereas the second study estimates a wage scar of 13–21% at the age of 42. The impact of youth unemployment is also analysed by Nordström Skans (2004) using Swedish data covering the same recession period analysed in this study. Using a model with family fixed effects, he finds that experiencing unemployment after completing vocational education reduces annual earnings by 17% after 5 years.

Most of the U.S. studies focus on the effect of displacement rather than unemployment in general. This may be partly explained by the fact that unemployment durations are much shorter in the U.S. than in Europe. These displacement studies estimate a somewhat different parameter than unemployment scarring studies. Yet it is interesting to compare the results. A regression framework where displaced individuals are compared with employed individuals was introduced by Jacobson, LaLonde & Sullivan (1993) and it has been later applied in a number of studies. They analyse quarterly earnings using data from Pennsylvania which suffered from declining manufacturing at the time. The study finds 25% long-term loss from displacement which is a relatively high estimate compared with other studies from the U.S.[1]

Kuhn (2002) provides an overview of displacement analyses and includes several international studies. For example, Bender, Dustmann, Margolis & Meghir (2002) perform a descriptive analysis on displaced workers in France and Germany. They find association between displacement and earnings losses when it takes longer than a year to find a new job. Although displaced workers have shorter unemployment durations than other separating workers, they still suffer from strong unemployment persistence.[2]

Huttunen, Møen & Salvanes (2006) analyse displaced workers in Norway. They find that displacement increases the probability of exit from the labour force but reduces annual earnings relatively little. The earnings loss is 5% after 2 years but it disappears after 7 years. The Swedish displacement study by Eliason & Storrie (2006) employs non-parametric matching estimators similar to this study. They follow displaced workers for 12 years and find a persistent effect on unemployment which is around 4 percentage points.

[1] Fallick (1996) and Kletzer (1998) provide surveys on the U.S. studies.
[2] Other German studies include Burda & Mertens (2001) and Couch (2001).

3 Economic environment

3.1 Institutional framework

The institutional framework affects both labour supply and demand decisions.[3] The legislation dictating how quickly firms are able to adjust the number of employees is particularly important for this analysis. Moreover, the unemployment insurance system and the wage setting are essential in determining unemployed individual's incentives for re-employment.

Finnish employers must provide a justified reason to lay off workers. The law allows displacing workers if there is an economic or production related reason or if a plant or an office is closed down. These reasons cannot be temporary and it must be the case that the employer is not able to offer other jobs for the workers. In practise, this prevents filling similar vacancies at least for three months. An advance notice of displacement must be given to the workers from one to six months before the layoff depending on the length of work history in the firm. In the case of bankruptcy, the advance notice must be given two weeks before the layoff.

At the time of the recession, displaced workers were eligible for earnings related unemployment benefits for 500 working days if they had worked more than six months (10 months since 1997) and were members of an unemployment fund. For the median income worker with earnings related benefits, the benefit level was 55% of pre-unemployment income (in 2003, gross income 1,178 euros/month). Otherwise they received the basic allowance which is substantially lower. For example, in 2003 the basic allowance without child supplements was 23 euros per working day.

Unemployed individuals must register to the labour office to receive benefits. Employees who have quit face a waiting period before they receive unemployment benefits. Before 1993 the period was six weeks but it has been increased since. Displaced unemployed individuals received benefits after a one-week waiting period. These rules create an incentive for unemployed to register. In addition, the information should be reliable in the relevant cases because a document about the displacement is required.

Displaced workers with long employment histories were eligible for a severance pay at the time of recession. A one-off payment varied depending on the employment history and it was typically slightly higher than a monthly salary. At the time of recession, the eligibility criteria for the severance pay were 43 years of age (45 years since 1995), five years continuous employment history and to be registered as unemployed.

Elderly people have an option for an early retirement scheme that affects considerably their re-employment incentives. Before 1997, individuals older than

[3] See Koskela & Uusitalo (2006) for more details on the Finnish labour market institutions.

53 years of age were entitled for earnings related benefits until retirement. The age limit was raised to 55 years in 1997. To account for this, all individuals in the analysis data are under 46 years of age at the end of 1990 which means that they are not entitled to the early retirement scheme during the analysis period.

Wage setting in Finland is dominated by the collective agreements between trade unions and employer organisations. Over 80% of the workers belong either to a union or an unemployment insurance fund. During the 1990's, the coverage of agreements was around 95% of workers which is among the highest rates in the OECD. Between 1987 and 1999, the wage setting was collective at the national level in nine years and at the industry level in four years. As the nominal wage increase is linked to the productivity increase across the whole economy, wages may reflect sector specific productivity changes poorly. There is no minimum wage legislation but collective labour contracts contain a set of job-complexity and education specific minimum wages.

3.2 The Finnish recession

The Finnish economy experienced dramatic events in the early 1990's.[4] In the late 1980's, the economic growth was rapid, 3.4% on average. As illustrated in Figure 1, the unemployment rate was low. Long-term unemployment was a rare event mainly because of active labour market policy. Before the recession started at the end of 1990, the economy was overheated. This was partly due to financial deregulation which led to an increase in private borrowing and risk taking. The tax system favoured debt financing of investments. In addition, firms had incentives to acquire foreign debt due to the difference between foreign and domestic interest rates.

Finnish currency, the markka, had a fixed exchange rate in the 1980's. In March 1989, the markka was revaluated as a late response to foreign capital inflow. The fixed markka started to face growing speculative pressure from 1990 onwards and the defence of markka led to an increase in the real interest rates. At the same time, the German unification raised interest rates in Europe which raised the rates in Finland even further. This caused serious trouble for heavily indebted firms. Also domestic demand declined and the export sector suffered from loss in price competitiveness.

The collapse of the Soviet Union in 1991 also contributed to the decline in the economy. The bilateral trade, which was 15% of total exports, dropped by 70%. In November markka was devaluated. As the recession started to become deeper, reductions in asset values and liquidity variables caused private consumption and investment to drop. This, combined with the drop in bilateral trade and high

[4] A more detailed discussion on the recession can be found, for example, in Honkapohja & Koskela (1999).

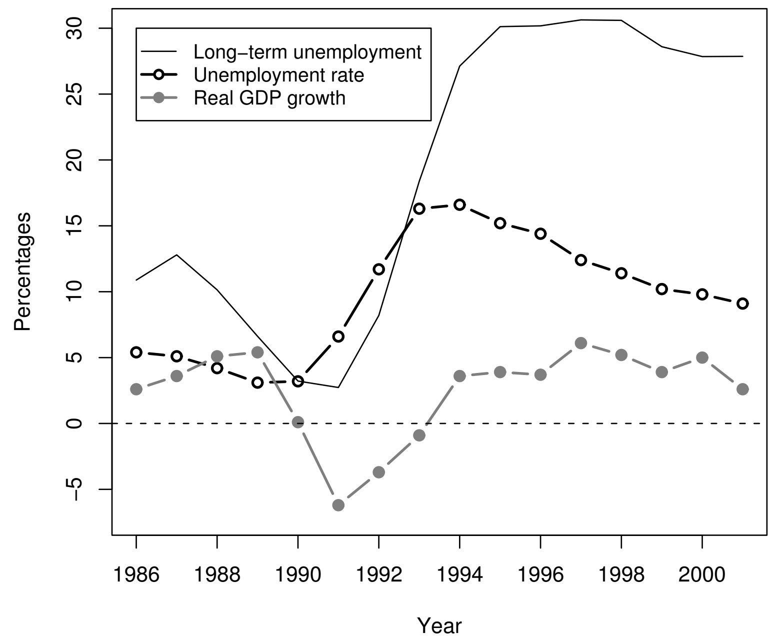

Figure 1: Unemployment rate, long-term unemployment (over 12 months) and real GDP growth in Finland (Statistics Finland; Labour Force Survey).

interest rates, forced many firms to be closed down. Especially firms with high foreign debt had problems.

The economic downturn came as a surprise for firms and policy makers. The foreign shocks were hard to anticipate but problems arising from increasing risk taking and indebtness of private sector might have been possible to perceive. However, this does not seem to be the case. In autumn 1990, all Finnish economic research institutes failed to forecast the sharp downturn in the late 1990 and 6% drop in GDP for 1991.[5]

The number of firm closures grew sharply when the recession started. Figure 2 shows the number of bankruptcy proceedings together with the short-term real interest rate in Finland. The number of proceedings more than doubled from the pre-recession level. One of the key factors causing problems for the indebted firms was the high real interest rates. The peak of 14% coincides with the year with the largest number of bankruptcy proceedings. As the firms had problems and laid off workers, the unemployment rate rose from 3.2% to 16.6%, in just three years.

[5]The difficulties in forecasting the recession are discussed in Vartia (1994).

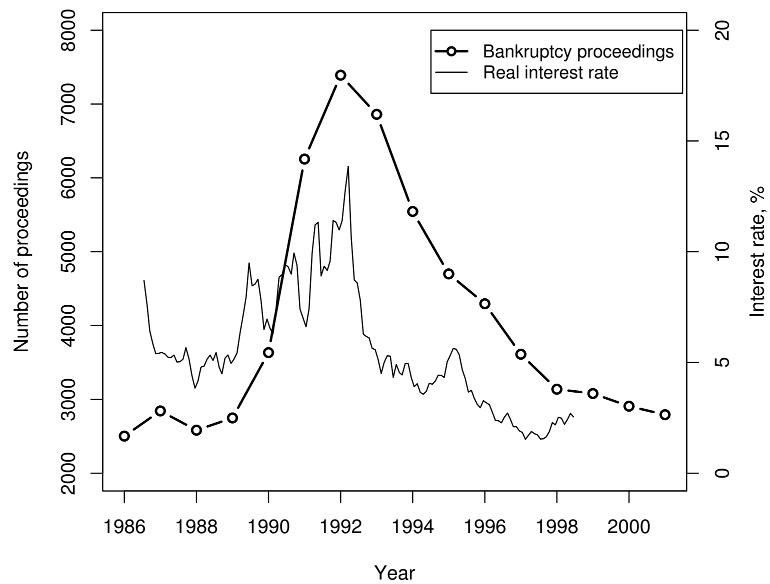

Figure 2: Number of started bankruptcy proceedings and the real interest rate (3 month Helibor - Consumer Price Index) in Finland (Statistical Year Book 2005; Bank of Finland).

4 Data

The dataset used in this study is based on the Employment Statistics database of Statistics Finland. It is a representative 10% sample of 12 to 75 years old individuals living in Finland in 1997.[6] The information in the data is combined from several administrative registers. In most cases information is reported annually for all individuals from 1987 to 2000.

4.1 Analysis sample

In the analysis, the focus is on men in prime working-age with stable employment in a private sector firm before the recession. The analysis sample consists of 22,474 men from 25 to 45 years of age at the end of 1990. They have worked more than 21 months between 1989 and 1990 with earnings information in the data for both years. They have not been unemployed between 1987 and 1990. In addition, it is required that individuals have been classified as workers and the employer is a

[6]The population is sampled in 1997 which means that emigration from Finland is not observed. High emigration because of the recession would be problematic for the analysis. However, the average annual proportion of 24–54 years old individuals emigrating between 1990 and 1997 was only 0.19% (approximately 4400 individuals, Statistics Finland).

private firm at the end of 1990. This implies that entrepreneurs, self-employed, farmers, students and public sector workers are excluded.[7]

The main reason for restricting the analysis sample is that the effect of unemployment is likely to depend on individual characteristics, like age and gender. Young unemployed individuals are more likely to exit from the labour force and continue to study. On the other hand, displaced elderly people have an option for early retirement. To ensure that individuals do not enter the early retirement scheme, only those who are under 55 years at the end of the analysis period are included in the sample. Women are excluded because they are generally more loosely attached to the labour markets than men.

Only private sector workers are included in the sample because public sector workers are much less likely to face displacement. A stable work history with no unemployment is used to exclude new labour entrants from the sample. The main motivation for excluding previously unemployed individuals is that the interpretation of the results becomes easier when the analysed unemployment spell is the first in the observation period. In addition, unemployment before the recession was low.

4.2 Variables

The structure of the sample selection and the definition of the treatment variable, the covariates and the outcomes are illustrated in Figure 3. Individuals unemployed due to plant closure are the main treatment group but descriptive statistics is also provided for all unemployed. The variable providing information on plant closure is available in the data since 1991 and it is collected when displaced workers register as job seekers at the labour office. Unemployed individuals need to register in order to qualify for benefits.

The information on the reason for unemployment is not complete. The reason is missing roughly for 15% of the individuals who had a work spell before the unemployment spell. The missing data are likely due to multiple records in the job seeker register. The data are typically linked to the last record of a given year which does not necessarily contain all the information. This causes misclassification of some individuals who belong to the plant closure group but they are still identified as unemployed.

Table 1 shows the distributions of the reason for unemployment. The reasons are: quit by own request, the end of probation period, the end of fixed-term contract, displacement due to individual reasons, an economic or production related reason and the closure of plant or office. The most common reason during the recession is economic or production related. The number of displacements not

[7]The original dataset includes 72,552 men aged 25 to 45 years. 29,816 of them are employed in private sector, 26,353 of them had the required number of months in employment. Further, 2,877 are removed because of previous months in unemployment and 1,002 had missing information.

Table 1: Annual frequencies of the different reasons of unemployment in the analysis sample.

	1991	1992	1993	1994	1995	1996	1997	1998	1999
Own request	80	127	65	61	39	39	35	62	44
End of probation	6	10	9	11	7	17	10	25	13
Fixed-term contract	383	582	422	607	280	334	233	243	178
Displaced (other)	41	52	16	21	19	29	23	31	21
Production related	964	1641	789	273	209	226	168	177	161
Plant closure	95	182	94	45	13	22	12	18	16

related to individual reasons drops after the recession while the number of other reasons for exits vary less. Between 1991 and the end of 1993, 6,257 individuals in the analysis sample experience unemployment and 371 individuals lose their jobs due to plant closure.

The outcome variables, months in employment and earnings, are observed annually. As illustrated in Figure 3, the follow-up continues until the end of 1999. The earnings information is obtained from tax registers. When averages are computed, also zero earnings are included. To account for variation in employment, monthly wages are computed by dividing annual earnings by months in employment. Weakness with this definition is that it does not take part time work into account and it is also inaccurate for those who have worked only for one or two months per year. Unfortunately more accurate measures, like the hourly wage, are not available in the data.

The rich set of covariates in the data makes it possible to control for various dimensions of firm and worker heterogeneity. All covariates used in the analysis are observed at the end of 1990. In the main analysis, Statistics Finland's two-

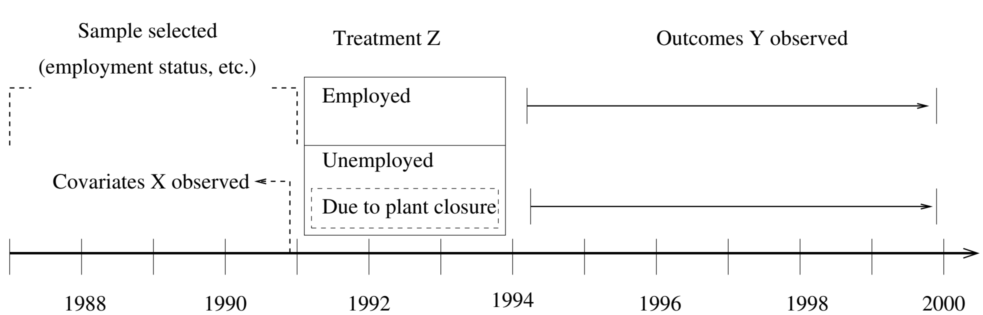

Figure 3: Construction of analysis sample and the definition of treatment variable Z, covariates X and outcomes Y by calendar year.

digit industry classification (69 categories) is used for the employing firm. When the descriptive statistics are provided, 17 broad industry categories are used for illustrative purposes. The firm location is described by 15 economic regions and by the statistical grouping of the area's degree of urbanisation.

In addition to labour market history variables, earnings and age, there is detailed information on other individual characteristics. Education is classified into five levels, socio-economic status has three levels as well as the native language variable. Family status indicates whether an individual is married, has children or has a partner. House ownership is used as a proxy for wealth and willingness to move to get a job. A detailed variable description is provided in the Appendix.

5 Empirical strategy

5.1 Plant closures as a source for variation

Individuals displaced due to plant closures have been analysed in a number of studies. Generally, it is thought that by focusing on mass layoffs there is less selection. In regular downsizing, employers have incentives to displace the least productive workers first. When the whole plant is closed down, all workers are displaced. Gibbons & Katz (1991) construct a theoretical model based on this idea.

There are, however, potential flaws in using plant closure information. Firstly, it is possible that workers are sorted by some unobserved characteristics to firms that are going to be closed down. For example, plant closure firms could have systematically hired less productive workers than other firms that are similar in observed characteristics. This may be a relevant factor especially when the economic environment is stable. Nevertheless during the Finnish recession, by far the most important reason for plant closures was the excess risk taking of firms and the large demand shocks.

The second problem is that plant closure can be a time-consuming process and its starting point is difficult to define. Firms may have tried to improve the average productivity before the plant is finally closed. This would cause the treatment group to contain too many high type workers. Alternatively, it may be easier for high type workers to leave the firm before closure which is often referred to as the early leaver problem. The advance notification is the only documented way to provide information on the forthcoming closure. At least in case of bankruptcy, the required two weeks notification period is short in Finland.

Potential sorting is the main reason why only plant closures are used here instead of a broader definition of displacement. The recession period is good as it provides sufficient number of cases. Moreover, the fact that the Finnish economy turned unexpectedly and quickly from boom to recession reduces the potential sorting problem. It was difficult for firms to anticipate the events and hence to adjust the number of workers beforehand. During a normal economic fluctuation, there is more time to restructure organisation and it is also easier to get extra funding.

Displacement analysis is often done using linked employer-employee data with relative inaccurate information on firm exits and displacements. A false firm death refers to a classification error where individuals have been incorrectly defined as displaced. In this analysis setup, plant closure cases are identified when individuals register at a labour office which means that the problem of false firm deaths is avoided. The early leaver problem is common to all studies but in this setup it is further required that individuals register as unemployed. If individuals face unemployment, the information in the data is likely to be correct as the registering is needed for the benefits and the reason for unemployment is checked by the case worker.

Without a firm identifier it is difficult to assess the potential sorting problem. However, a comparison of job-to-job worker flows gives some descriptive evidence. Flows are computed from a separate employer-employee dataset on workers in the Finnish manufacturing industry from 1991 to 1999. These figures show that the share of job changers remained almost constant over the recession period while the share of exits and entries varied strongly.[8] This may indicate that it was difficult to change a job during the recession, although it is possible that aggregate flows hide a shift from higher share of forced job changes to higher share of voluntary job changes.

5.2 Comparison of groups

The composition and outcomes of three groups are compared in the following. The groups are defined by their employment status between the beginning of 1991 and the end of 1993. The employed group consists of those who did not become unemployed. The unemployed group consists of those who had at least some unemployment. The plant closure group consists of a subset of all unemployed individuals who became unemployed because of plant closure.

As Table 2 shows, the groups are quite similar in terms of the key background variables. Descriptive statistics for the other control variables are provided in the Appendix. The pre-recession earnings in the employed group has over 10% higher mean and larger standard deviation than the two other groups. However, the earnings distributions are still quite similar and the difference is mainly due to more frequent high earnings among the employed group. This difference is also reflected in other variables. Employment before the recession does not show much

[8]The manufacturing industry employed 22% of Finnish workers in 1999. The following figures cover all manufacturing industry excluding paper industry. The employment is observed annually at the end of year. The share of firm changers remains 5–6% in 1991–1995. The share of exits decreases almost 10 percentage points when the recovery starts and the share of entries doubles.

Table 2: Means of pre-treatment outcomes, individual characteristics and unemployment during the treatment period in three groups.

		Employed	Plant closure	Unemployed
Pre-treatment period 1989–90				
	Employment (mo)	23.97	23.96	23.92
		(0.21)	(0.22)	(0.34)
	annual earnings (1000 mk)	136.83	122.21	118.09
		(58.32)	(50.37)	(43.83)
Covariates observed at the end of 1990				
	Age	35.65	34.71	34.94
		(5.86)	(5.79)	(5.97)
Education	base	0.27	0.28	0.31
	high school	0.04	0.04	0.03
	vocational	0.38	0.43	0.45
	lower tertiary	0.16	0.16	0.13
	higher tertiary	0.15	0.1	0.08
Socio-economic status	blue collar	0.52	0.6	0.67
	white collar low	0.25	0.22	0.2
	white collar high	0.23	0.18	0.13
Area type	urban	0.76	0.71	0.71
	semi-urban	0.12	0.12	0.14
	rural	0.12	0.17	0.15
Industry	other	0.02	0.01	0.02
	primary prod	0.02	0.02	0.02
	mfg consump prod	0.06	0.04	0.03
	mfg wood prod	0.09	0.07	0.05
	mfg metal prod	0.05	0.09	0.06
	mfg machinery	0.08	0.08	0.07
	mfg technical prod	0.07	0.06	0.06
	mfg other	0.07	0.1	0.08
	house construction	0.03	0.05	0.15
	other constrion	0.04	0.08	0.11
	wholesale trade	0.1	0.09	0.07
	other trade	0.1	0.12	0.11
	transportation	0.06	0.04	0.05
	communications	0.06	0.05	0.02
	financial services	0.04	0.01	0.01
	business services	0.06	0.08	0.07
	other services	0.05	0.02	0.02
Treatment period 1991–1993				
	Unemployment (mo)	0	10.72	10.54
	N	16162	371	6253

Note: standard deviations in parenthesis (calculated after taking averages over years). Broad industry categories are derived based on Statistic Finland's two-digit classification. Plant closure group is a subset of unemployed.

Figure 4: Average nominal annual earnings (1000 mk) in the groups.

variation because it is one of the sample selection criteria.

The employed group is on average slightly older, more educated and more often in white collar work. Their employer in 1990 was more often located in urban areas. The plant closure group is educated better on average than all unemployed which reflects partly the industry composition. The employed individuals worked more often in the manufacturing of consumption good and wood industries as well as in financial and other service industry. The construction industries were more common among the unemployed individuals.

Figure 4 shows the average nominal earnings for the three groups between 1987 and 1999. As the same individuals are followed, cohort effects and time trend are present. In addition, inflation causes an increasing trend, especially in the late 1980. However, these confounding factors do not affect the differences between the groups. Before the recession period, the differences between the groups are stable. The mean earnings in the two groups of unemployed individuals drop during the recession but recover at a stable rate afterwards. However, the gap between the unemployed and the employed group remains large even 6 years after recession in 1999. The magnitude of the difference is large which suggests scarring or stigmatisation.

The patterns of other outcomes after the recession are shown in Figure 5. The average months in employment is decreasing for those who did not experience

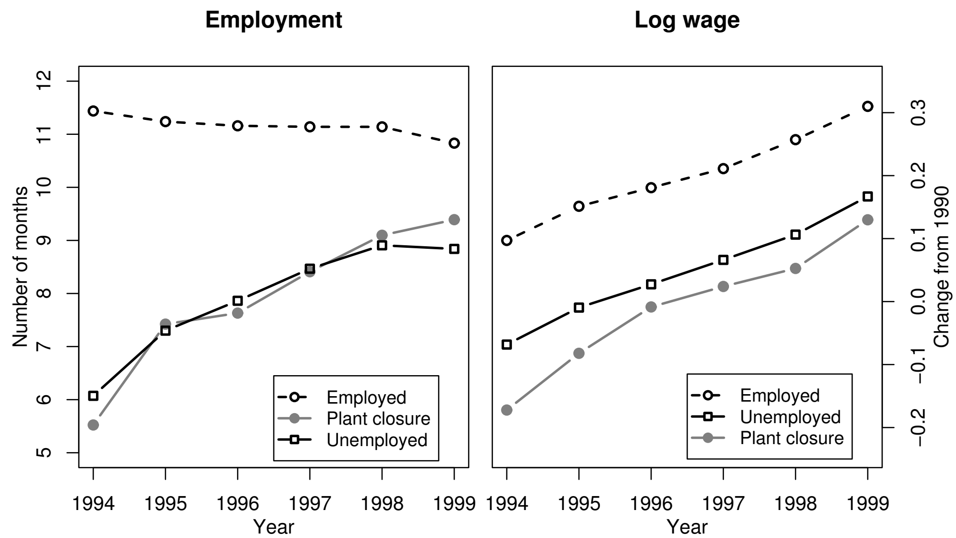

Figure 5: Average number of months in employment and change in log nominal wages since 1990 for the groups. Wages are only observed for those who work.

unemployment during the recession.[9] This is due to natural flow to unemployment and out of labour force. Unemployed individuals experience on average 3.5 months of unemployment per year during the recession. They start with very low employment in 1994 and their employment increases roughly by half a month every year until 1999.

As noted earlier, wages are observed only for those who work. In 1994, wages are missing for 2% of the employed group, 28% of the unemployed group and 35% of the plant closure group. At the end of the period in 1999, the numbers are 6%, 15% and 12%, respectively. As expected, those who did not experience unemployment have the nominal wage growth since 1990. Both the unemployed and plant closure groups have lower average nominal wages in 1994 than in 1990. The growth between 1994 and 1999 is quite steady in all groups.

The comparison of the individual characteristics between the groups shows that they are surprisingly similar. Interestingly, the means of the control variables in the plant closure group are typically between the means of the employed and unemployed groups. Similarity of the groups reflects the fact that the recession affected the whole economy. There are also no notable differences in the other variables shown in the Appendix. Even the regional compositions of the groups

[9] Alternatively it is possible to use binary employment classifications instead of months per year. Using the binary definitions of 12 months and 6 months per year in employment do not change the profiles.

are nearly identical. However, there is an obvious difference between the groups in the average pre-recession earnings. To construct a valid comparison group for the plant closure group, the compositional difference of the employed individuals is adjusted by matching.

5.3 Matching estimator

A single outcome variable Y is used in the notation for simplicity, although there are three outcomes of interest: annual earnings, months in employment and log wages. The treatment indicator D takes value 1 if an individual has experienced unemployment during the recession period and 0 otherwise. The potential outcome Y_1 denotes the individual outcome if unemployed and Y_0 denotes the outcome if employed. As only either Y_1 or Y_0 is observed for every individual, it is only possible to compare the difference in average outcomes. The vector X describing the characteristics of the individuals and the employing firms is observed before the recession.

The effect of interest is the mean difference in outcomes when experiencing unemployment relative to remaining employed for the unemployed group. The estimated parameter is the average treatment effect on the treated. If the effect is heterogeneous, the estimated parameter differs from the average effect for all private sector workers. This could be the case, for example, if the industry composition of the firms displacing workers is not representative. Formally the parameter of interest is:

$$\Delta^{TT} = E(Y_1 - Y_0 | D = 1).$$

The average treatment effect on the treated is estimated by a matching estimator. The basic idea is that each unemployed individual is compared with employed individual with similar background characteristics.

The key assumption required for the identification of the parameter is the Conditional Independence Assumption (CIA) which can be formally denoted by $(Y_0 \perp D) \mid X$. It states that, conditional on the observed individual and firm characteristics, the treatment status is independent of the outcome if employed. Thus, there can be no selection on unobserved characteristics. The variation in employment status due to plant closures is often thought to provide an exogenous source of variation. Further, the fact that the analysis period is a severe recession probably increases the random component in the layoffs.

Yet it may well be that the conditioning set X does not include all relevant variables that determine treatment status and outcomes. It is also possible that only a selected group of plant closure cases have registered at the employment office. Because the annual earnings in $D = 0$ state are observed for all in the pre-recession period, it is possible to indirectly assess the validity of the CIA on that period. An informal test is done by matching on earlier covariate information that is not used in the information set of the main analysis. If there is no difference in the pre-recession earnings between individuals who later experience unemployment and those who remain employed, it provides support for the CIA. Note that the pre-recession earnings are included in the information set of the main analysis.

The second important requirement for matching is the common support assumption. It states that a counterpart must be found for each unemployed individual among the employed individuals, formally $\Pr(D = 1|X) < 1 \; \forall \; X$. This condition is not restricting in this case because the employed group is far bigger than the unemployed group. However, when the conditioning set X has high dimensionality, the number of subgroups grows quickly.

The propensity score matching provides a simple method to reduce the dimension of the conditioning set. The idea is to estimate a balancing score $\Pr(D = 1|X)$ which gives each individual the probability of experiencing unemployment. Rosenbaum & Rubin (1983) show that it is sufficient to balance on the propensity score instead of X. The common support assumption for propensity score can be checked by comparing the probability distributions of scores between unemployed and employed individuals.

The key conditioning variable in the analysis is the pre-recession earnings. To ensure that individuals in the same income category are compared, exact matching is done with respect this variable. This also makes it possible to study the heterogeneity of the treatment effect across the earnings categories. For other covariates, a propensity score is estimated by logistic regression where all variables are categorical except age which is included with a quadratic term. To avoid compression of values around zero, matching is done on the linear predictor $X\hat{\beta}$.

With respect to the propensity score, individuals are matched by the nearest neighbour method. To increase the efficiency of the estimator, several controls are used if they all match a treated individual. A tolerance value 10^{-5} is used to determine the acceptable distance. The heteroskedasticity-consistent standard errors are estimated following Abadie & Imbens (2006).

5.4 Matching and covariate balance

Exact matching is done by the 15-quantiles of 1989–90 earnings from the plant closure group.[10] As the objective is to find a counterpart for the unemployed individuals, those who worked in an industry with no plant closures are excluded from the comparison group.

The propensity score is estimated by a logit-model for the other control variables measured in 1990. The complete model output is presented in the Appendix.

[10] 15-quantiles were chosen because they provide sufficiently accurate matching and make it possible to study heterogeneity by earnings quintile.

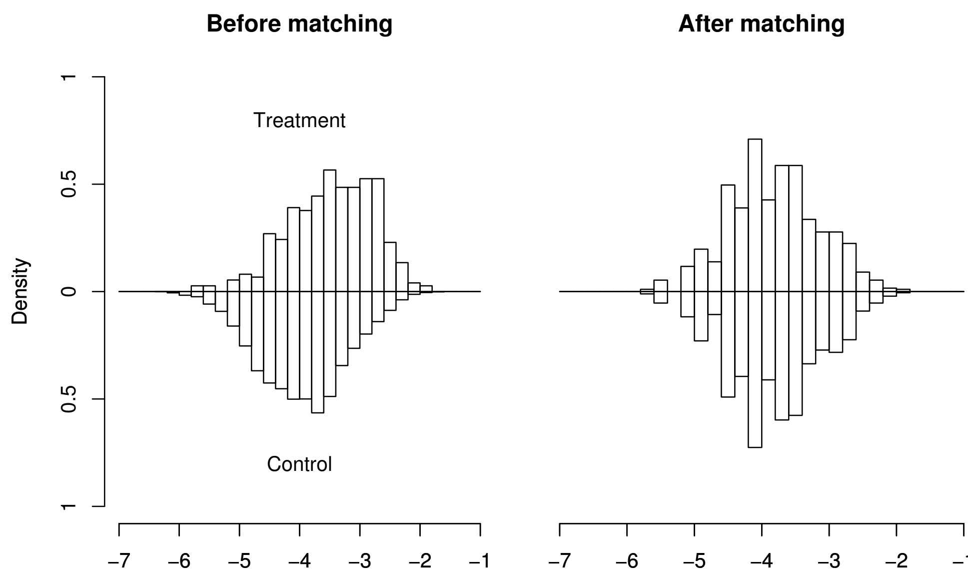

Figure 6: Distributions of estimated propensity score before and after matching in the treatment and the control groups. Matching is done on the linear predictor from a logistic regression (i.e. on log-odds ratio).

Then the Conditional Independence Assumption (CIA) and the common support assumption are assessed. The CIA can be informally tested by comparing the outcomes of the groups in the pre-recession period. The common support can be evaluated by comparing the balancing score across the groups.

Table 2 indicates that the employed individuals have, on average, higher 1989–90 earnings than others. A t-test shows that the employed group is significantly ($t = 6.01$, $p < 0.01$) different from the plant closure group. An indirect test of the CIA is done by matching individuals using 1988 covariate information. If the difference in 1989–90 earnings remains between the treatment and the control group after matching, it suggests that the CIA does not hold. Again exact matching is done using the 15-quantiles of earnings and the same balancing score specification is used as in the main analysis to the extent that the covariate information is available. After matching, the difference between the plant closure and the employed group reduces from -14.62 to -1.07 with p-value 0.50. Thus, this gives support for the assumption that the data include sufficiently rich set of conditioning covariates for the CIA to hold.

According to Table 2 the analysis groups have relatively similar composition. Also the fact that the employed group is much larger than the plant closure group suggests that limited common support is not a problem in this study. Figure 6

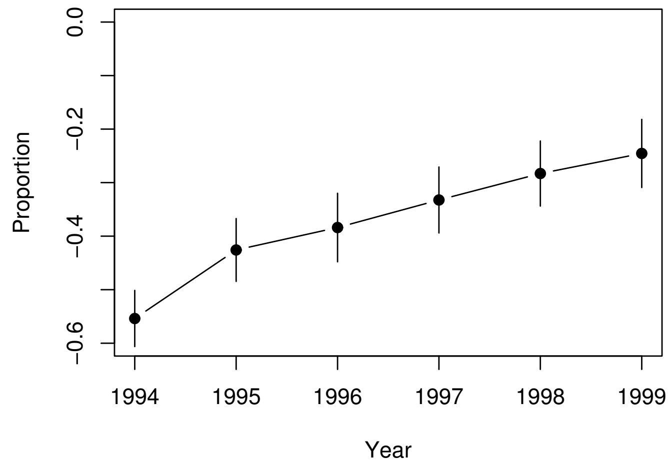

Figure 7: Effect on annual earnings (ATT) as a proportion of earnings in the control group by year. Vertical bars denote 95% confidence intervals.

shows the distributions of the linear predictions from logistic regression before and after matching. The distribution of the control group covers the range of treated individuals and matching creates practically identical distributions for both groups.

6 Results

6.1 Main results

The effect of experiencing unemployment during the recession period 1991–1993 is studied on annual earnings, months in employment and wages. The two latter outcomes are simply obtained by decomposing annual earnings. However, they provide some insight on whether the earnings difference is due to persistent unemployment or wage scarring. The complete matching estimates are shown in the Appendix.

Figure 7 presents the treatment effect on annual earnings from 1994 to 1999. The earnings losses are shown as a proportion of the average earnings in the matched comparison group. The initial losses are large, around 50% in 1994. In the following years, the earnings difference is reduced at a slow pace. Six years after the recession in 1999, the earnings loss is still 25%. This pattern is not entirely similar to the raw means shown in Figure 4 where the gap is more persistent. The result exceeds the losses estimated in most of the previous studies which is not surprising as they do not focus on a deep recession. However, a similar estimate is obtained by Jacobson et al. (1993) who report a 25% earnings

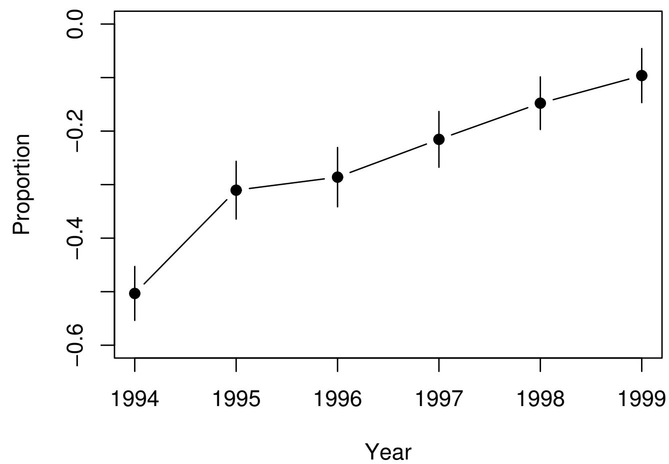

Figure 8: Effect on months in employment (ATT) as a proportion of employment in the control group by year. Vertical bars denote 95% confidence intervals.

loss 6 years after displacement for Pennsylvania which was heavily dependent on a declining manufacturing industry at the time.

The large earnings losses in the years following the recession are mostly explained by the high level of unemployment in the treatment group. The effect of unemployment on future months in employment shown in Figure 8 is relatively similar to the difference in average employment. In 1994 the difference is again 50% but in 1999 it has reduced to 10%. The reduction of employment difference takes years and implies strong unemployment persistence. Indeed, almost a half of individuals not working in 1994 remain without work in 1995.

Also some previous European studies have found strong persistence. For example, Bender et al. (2002) report that more than 20% of those who are unemployed due to displacement remain unemployed after 5 years. A Swedish study by Eliason & Storrie (2006) also finds a long-lasting effect on employment as it is visible up to 12 years. They estimate around 4 percentage points effect for all displaced of whom not all experience unemployment.

The differences in wages are illustrated in Figure 9. Here the aim is to measure wage scarring. However, this outcome is observed only for employed individuals who are likely to be a selected group of the analysis sample. The employment increases in the plant closure group from 1994 to 1999 which reduces the comparability of the estimates since the population of the treated varies over time.

This limitation in mind, it is interesting to note that the estimates change less over time than in the case of employment. Wages are 23% lower in 1994 and 14% lower in 1999 than in the comparison group. At the same time, the percentage of treated individuals with more than one month in employment grows from 65

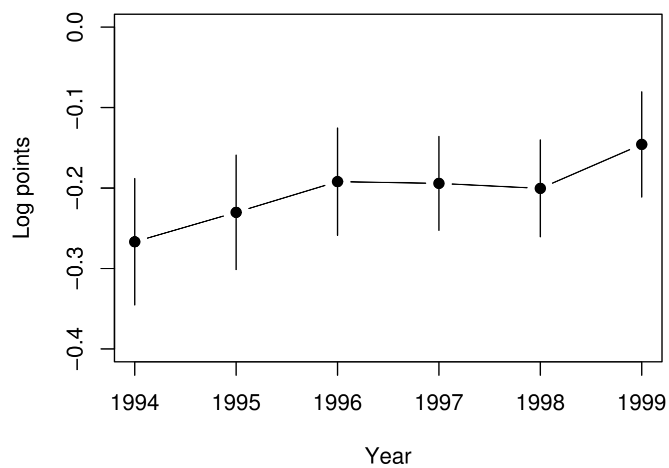

Figure 9: Effect on log wages (ATT) by year. Vertical bars denote 95% confidence intervals.

to 87. This indicates that there is notable wage scarring which recovers relatively slowly over time.

The estimated wage scar in 1999 is relatively similar to what has been found in other studies. The British studies focusing on the effect of unemployment provide the closest point of comparison. Arulampalam (2001) estimates a 14% loss after three years. Gregg & Tominey (2005) find a very long-lasting wage scar from youth unemployment which is in the order of 13–21% at the age 42. Also some displacement studies estimate similar numbers. For example, using U.S. data, Stevens (1997) estimates a 12% loss in hourly wages in the first year after displacement and observes this loss to diminish only slightly over a ten-year period. On the other hand, Bender et al. (2002) finds the opposite as French and German workers face a negligible wage loss. However, they note that those who remained unemployed over year after displacement faced a penalty when re-employed.

6.2 Heterogeneous responses

It is likely that some individuals are more prone to scarring. For example, in some industries firm-specific human capital is more important than in others. Alternatively, some individuals may suffer less from long-term unemployment than others. This creates variation in the treatment effect given the time in unemployment. There is also variation in the duration of unemployment. The longer the unemployment continues the larger losses are expected. However, it is important to note that the duration of unemployment is observed in the post-treatment period and the variable suffers from selection bias as less employable individuals

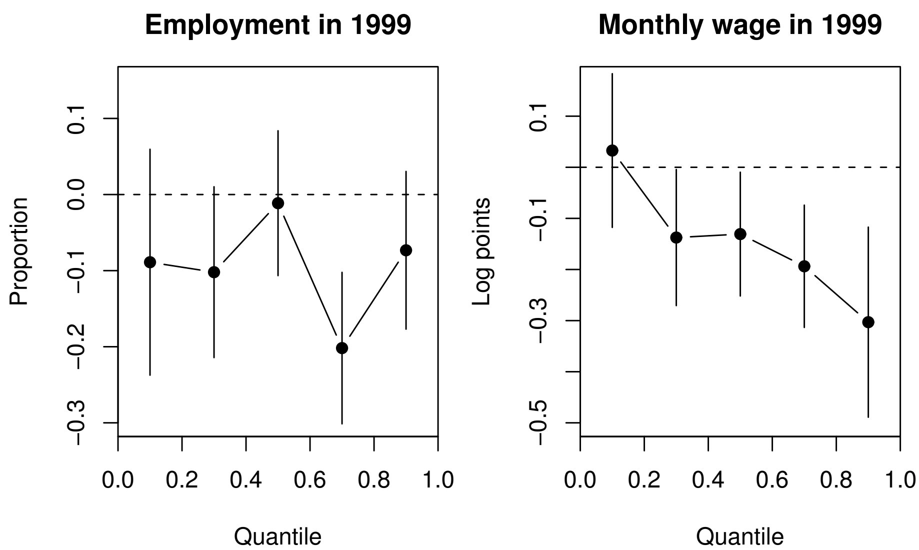

Figure 10: Treatment effects by pre-treatment earnings (1989–90) quantile. Vertical bars denote 95% confidence intervals.

are probably also more prone to scarring.

Figure 10 illustrates the effect of treatment by pre-treatment earnings in 1989–90.[11] The matching estimates are computed separately for individuals in each 5-quantiles of the data. Only the results for 1999 outcomes are shown as the profiles are very similar for earlier years. As the number of treated individuals is relatively low when split between the quantiles, the standard errors are large. The effect on employment in 1999 does not show any systematic trend.

For wages there seems to be a negative relation between losses and pre-treatment earnings even though the confidence intervals are again wide. In the lowest quantile, there is no wage loss whereas in the quantile the point estimate corresponds to a 26% loss. It is likely that the job specific minimum wages prevent wage losses in the lowest quantile. Because the same propensity scores are used as in the main analysis, the estimates by quantile approximately add up to the pooled estimates.

Previous studies have explored several sources of treatment heterogeneity. Gregory & Jukes (2001) and Burda & Mertens (2001) report a similar negative relation between earnings losses and earnings quantile. Eliason & Storrie (2006) study heterogeneity by age. They find some indication that older individuals

[11] Effect on annual earnings in 1999 by pre-treatment earnings is not shown because it only combines the effects seen on employment and monthly wage. Point estimates are more negative for those with the two pre-treatment earnings quintiles.

have less favourable labour market outcomes. When outcomes are studied by age, similar, although not significant, results for employment are found in this sample (results not shown).

7 Conclusions

The recession in 1991–1993 generated a severe unemployment problem that has affected the Finnish economy over ten years. This study has estimated the long-term costs of unemployment that began during the recession. The identification relies on the variation in unemployment created by plant closures. The idea behind this identification strategy is that the deep recession was caused by unexpected shocks, like mistakes in monetary policy and the collapse of the Soviet Union, which caused many firms to close down that would have survived the normal economic fluctuation. The analysed sample consists of men between 25 and 45 years of age in 1990 who had a stable work history in the private sector and no unemployment before the recession. They are expected to stay in the labour force even when the labour market situation is weak.

The key assumption in the analysis is that individuals who remained employed during the recession can be used as a comparison group for the plant closure group. Employed individuals were matched by a rich set of covariates. Because the reason for unemployment is available only for individuals who register as unemployed, there is a risk of selection on unobservables. The difference in outcomes on the pre-recession period was tested to address this problem. The matched plant closure group passed this informal test. The estimated parameter is the average treatment effect on the treated.

Annual earnings and months in employment of the individuals were observed until the end of 1999. Monthly wages are obtained by dividing annual earnings by months in employment. The plant closure group suffered large and long-lasting losses in annual earnings. Although annual earnings recover, the effect of working in a plant that closed down is a 25% reduction in annual earnings compared with not working in this plant during the recession. The low level of employment in the plant closure group explains most of the initial earnings losses. After the recession, the effect on employment months is 50% but the difference reduces to 10% in 1999.

The wage estimates change less, from a 23% penalty to a 14% penalty between 1994 and 1999. Although, wage estimates must be interpreted carefully, especially in the beginning of the period, as they are observed only for employed individuals. When the heterogeneity of treatment effect is studied, the wage loss is strongly related to pre-recession earnings level as high earners suffer more.

The losses in annual earnings are large when compared to most of the previous studies analysing the cost of unemployment or displacement. This is not surprising as they do not analyse recession periods. Nevertheless, similar strong unemployment persistence has been observed also in other European studies. The estimated wage losses are roughly in line with previous studies. The centralised wage bargaining creates rigid wages, especially for low income earners, which probably reduces wage losses. This is also consistent with the observation that individuals with the lowest pre-recession earnings have no wage scar. Individuals with high pre-recession earnings have large wage scars which is possibly due to lost human capital.

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Appendix

Table 3: Employer's location, native language, family type and housing type at the end of 1990 in three groups.

		Employed	Closure	Unemployed
Region	Uusimaa	0.35	0.34	0.32
	Varsinais-Suomi	0.10	0.08	0.10
	Satakunta	0.06	0.06	0.05
	Häme	0.08	0.10	0.08
	Pirkanmaa	0.10	0.09	0.10
	Kaakkois-Suomi	0.08	0.07	0.07
	Etelä-Savo	0.02	0.02	0.02
	Pohjois-Savo	0.04	0.04	0.04
	Pohjois-Karjala	0.02	0.02	0.03
	Keski-Suomi	0.02	0.02	0.02
	Etelä-Pohjanmaa	0.03	0.05	0.04
	Pohjanmaa	0.05	0.04	0.04
	Pohjois-Pohjanmaa	0.04	0.04	0.06
	Kainuu	0.01	0.02	0.01
	Lappi	0.02	0.02	0.02
Language	Finnish	0.93	0.95	0.96
	Swedish	0.06	0.04	0.04
	other	0.00	0.01	0.00
Family type	other	0.23	0.28	0.28
	married	0.07	0.08	0.06
	married and children	0.60	0.53	0.54
	not married and children	0.06	0.08	0.07
	single parent	0.04	0.04	0.05
Housing	other	0.21	0.25	0.28
	owns flat	0.41	0.40	0.33
	owns house	0.38	0.35	0.39
	N	16162	371	6253

Table 4: Variable description.

Area type	statistical grouping of municipality where the employing firm is located: urban, semi-urban and rural.
Region	where the employing firm is located. 15 levels: Uusimaa, Varsinais-Suomi, Satakunta, Häme, Pirkanmaa, Kaakkois-Suomi, Etelä-Savo, Pohjois-Savo, Pohjois-Karjala, Keski-Suomi, Etelä-Pohjanmaa, Pohjanmaa, Pohjois-Pohjanmaa, Kainuu, Lappi.
Industry	two-digit classification of the employing firm (Statistics Finland 1988). The following broad classification is used in tables: other (two-digit codes 81–87, 92–99), primary production (01–04), manufacturing consumption products (11–13), wood products (14–15), metal products (07–09, 23–24), machinery, technical products (27–29), other manufacturing (21–22, 29), house construction (35), other construction (36–38), wholesale trade (41–42), other trade (43–48), transportation (51–56), communications (16, 57–58), financial services (61–62, 77), business services (71–76), other services (31–34, 65–67, 88, 91).
Age	in years.
Earnings	nominal annual wage and entrepreneur earnings in a given year or average over two years (in 1000 mk).
Education	primary, high school, vocational, lower tertiary, higher tertiary.
Socio-economic status	blue collar (manual workers), white collar low (lower level employees), white collar high (upper level employees).
Language	Finnish, Swedish, other native language.
Family type	other, married, married couple with children (under 18 years), not married couple with children, single parent.
Housing status	other, own house, own flat.

Table 5: Propensity score estimates from logistic regression (continues on next page).

	Estimate	Std. Error	z value	Pr(>|z|)
(Intercept)	−3.8560	2.1534	−1.79	0.0734
semi-urban area	−0.0267	0.1761	−0.15	0.8795
rural area	0.2677	0.1679	1.59	0.1109
Varsinais-Suomi	−0.3194	0.2139	−1.49	0.1355
Satakunta	−0.0026	0.2548	−0.01	0.9920
Häme	0.0889	0.2085	0.43	0.6700
Pirkanmaa	−0.1081	0.2121	−0.51	0.6101
Kaakkois-Suomi	−0.0350	0.2354	−0.15	0.8819
Etelä-Savo	−0.1683	0.4093	−0.41	0.6809
Pohjois-Savo	0.0150	0.2800	0.05	0.9574
Pohjois-Karjala	−0.3363	0.4039	−0.83	0.4050
Keski-Suomi	0.0558	0.3797	0.15	0.8833
Etelä-Pohjanmaa	0.3826	0.2780	1.38	0.1688
Pohjanmaa	−0.2653	0.3042	−0.87	0.3833
Pohjois-Pohjanmaa	0.0516	0.2788	0.19	0.8531
Kainuu	0.4709	0.4461	1.06	0.2912
Lappi	0.2836	0.3846	0.74	0.4609
industry 04	−1.7535	0.8790	−1.99	0.0461
industry 09	−0.7243	1.1464	−0.63	0.5275
industry 11	−1.7829	0.6419	−2.78	0.0055
industry 12	−0.6412	0.7828	−0.82	0.4127
industry 13	0.1707	0.7336	0.23	0.8160
industry 14	−0.6630	0.5762	−1.15	0.2499
industry 15	−1.4184	0.6127	−2.32	0.0206
industry 16	−0.5443	0.5730	−0.95	0.3422
industry 17	0.2792	0.5761	0.48	0.6279
industry 18	−1.4723	0.8789	−1.68	0.0939
industry 21	−1.3272	0.7228	−1.84	0.0663
industry 22	−0.4429	0.5997	−0.74	0.4602
industry 23	−0.7220	0.7835	−0.92	0.3568
industry 24	−0.1324	0.5503	−0.24	0.8099
industry 25	−0.6971	0.5510	−1.27	0.2058
industry 26	−1.4946	0.6306	−2.37	0.0178
industry 27	−0.2176	0.5885	−0.37	0.7116
industry 29	−1.4568	1.1322	−1.29	0.1982
industry 35	−0.1501	0.5679	−0.26	0.7915
industry 36	0.2509	0.5559	0.45	0.6517
industry 37	−1.8428	1.1297	−1.63	0.1028
industry 38	0.6112	0.7987	0.77	0.4441

Continued from previous page

	Estimate	Std. Error	z value	Pr(>|z|)
industry 41	−0.7437	0.5491	−1.35	0.1756
industry 43	−0.7863	0.5907	−1.33	0.1831
industry 44	−0.9898	0.8843	−1.12	0.2630
industry 45	−0.5270	0.5616	−0.94	0.3481
industry 47	−0.3281	0.7888	−0.42	0.6774
industry 48	0.0395	0.6763	0.06	0.9535
industry 52	−1.3631	0.6081	−2.24	0.0250
industry 53	0.2249	1.1566	0.19	0.8458
industry 56	−1.5352	0.7804	−1.97	0.0492
industry 61	−2.1740	0.8826	−2.46	0.0138
industry 65	−1.4708	0.8808	−1.67	0.0950
industry 66	−1.6696	1.1327	−1.47	0.1405
industry 67	0.2262	0.8966	0.25	0.8008
industry 71	0.4539	0.5756	0.79	0.4304
industry 72	−1.0567	0.6983	−1.51	0.1302
industry 75	−1.9396	1.1335	−1.71	0.0870
industry 76	−1.4830	0.8821	−1.68	0.0927
industry 77	−0.0488	1.1469	−0.04	0.9660
industry 85	−0.9808	1.1385	−0.86	0.3890
industry 91	−1.1376	0.8856	−1.28	0.1990
industry 99	0.1164	0.7298	0.16	0.8732
age	0.0848	0.1217	0.70	0.4860
age^2	−0.0015	0.0017	−0.85	0.3944
high school educ.	−0.1352	0.3154	−0.43	0.6682
vocational educ.	−0.0584	0.1355	−0.43	0.6663
lower tertiary educ.	0.0847	0.2022	0.42	0.6754
higher tertiary educ.	−0.3872	0.2852	−1.36	0.1746
white collar low	−0.2366	0.1678	−1.41	0.1586
white collar high	−0.0961	0.2314	−0.42	0.6778
Swedish language	−0.3306	0.2866	−1.15	0.2486
other language	0.5670	0.6291	0.90	0.3674
married	−0.1147	0.2202	−0.52	0.6023
married and children	−0.2371	0.1379	−1.72	0.0854
not married and children	0.1392	0.2185	0.64	0.5240
single parent	−0.1317	0.2959	−0.45	0.6562
owns flat	0.0648	0.1420	0.46	0.6484
owns house	−0.0769	0.1554	−0.49	0.6206

Table 6: Matching estimates.

	1994	1995	1996	1997	1998	1999
Annual earnings						
Proportion	-0.554	-0.426	-0.384	-0.332	-0.283	-0.245
95% CI	[-0.61,-0.5]	[-0.48,-0.37]	[-0.45,-0.32]	[-0.39,-0.27]	[-0.34,-0.22]	[-0.31,-0.18]
Estimate	-73.323	-58.545	-54.448	-48.848	-43.405	-37.982
S.E.	3.545	4.123	4.638	4.624	4.759	5.032
N controls	937	937	937	937	937	937
Months in employment						
Proportion	-0.503	-0.31	-0.286	-0.215	-0.148	-0.096
95% CI	[-0.55,-0.45]	[-0.36,-0.26]	[-0.34,-0.23]	[-0.27,-0.16]	[-0.2,-0.1]	[-0.15,-0.05]
Estimate	-5.596	-3.341	-3.057	-2.31	-1.579	-1
S.E.	0.286	0.297	0.302	0.285	0.268	0.268
N controls	937	937	937	937	937	937
Log wage						
Estimate	-0.267	-0.23	-0.192	-0.194	-0.2	-0.146
S.E.	0.04	0.036	0.034	0.03	0.031	0.033
95% CI	[-0.35,-0.19]	[-0.3,-0.16]	[-0.26,-0.13]	[-0.25,-0.14]	[-0.26,-0.14]	[-0.21,-0.08]
N controls	594	693	681	700	748	757

Note: Exact matching is done by the 15-quantiles of the mean 1989–90 earnings and propensity score matching with the nearest neighbourhood method is used for the other covariates: area type, region, industry, age, education, socio-economic classification, native language, family type, housing status (observed at the end of 1990). The proportions are calculated from the mean of matched employed group. Robust standard errors are estimated following Abadie & Imbens (2006).

Chapter IV

Determinants of unemployment duration over the business cycle in Finland

Abstract

The recession of the early 1990s caused a serious unemployment problem in Finland. This study analyses the determinants of unemployment duration using individual data from 1987 to 2000. Duration until employment is modelled using a proportional hazard model with piecewise constant baseline hazard. The main focus is on the relative contribution of compositional variation and macroeconomic conditions to unemployment duration. According to the results, the aggregate outflow effect dominates and the observed compositional variation implies only a small increasing trend in the average duration during the recession period.

1 Introduction

This study investigates the cyclical variation in unemployment duration in Finland using individual data from 1987 to 2000. The Finnish economy experienced exceptional changes in the analysis period. After a boom in the late 1980's, the economy turned into a very deep recession. Between 1991 and 1993, GDP fell over 10% and the unemployment rate increased fivefold. The late 1990's was a period of recovery and stable growth but the unemployment problem remained.

The cyclical variation in unemployment duration follows the same pattern as the aggregate unemployment. Figure 1 illustrates the mean and the median durations of the unemployment spells in the analysis data. For spells that began before the recession, the mean duration was below 100 days. When the recession started at the end of 1990, the mean duration increased quickly. The peak is reached in 1992 and after that the mean duration declines steadily. The main question in this study is whether compositional variation contributed to these changes in duration, especially during the recession period.

A recession period usually causes an increase in displacements and reduction in hirings as firms adjust to lower demand. As it is more difficult to find a job, unemployment durations become longer. An indirect effect of recession is that the composition of individuals becoming unemployed may change. It is often assumed that an increase in displacements leads to a lower average employability of unemployed individuals (e.g. Baker, 1992). This happens if firms choose to lay off the least productive workers first. However, the high number of mass layoffs during the recession may have an opposite effect as firms closing down do not sort displaced workers.

In the empirical model, two main sources of the variation in unemployment duration are identified. The outflow effect of the macroeconomic conditions is captured by the unemployment rate. The compositional effect of inflow changes is modelled by using an extensive set of individual characteristics. Annual and quarterly dummies are used to capture the residual variation. The relative influence of the different sources of variation are compared by predicting unemployment durations using a duration model. Similar strategy has been previously used by Rosholm (2001).

Generally the main motivation in understanding cyclical variation in unemployment is to design more efficient labour market policies. In particular, if compositional variation plays a major role, it indicates that active labour market programmes should be adjusted according to the cycle. It should be noted that only the impact of observed individual heterogeneity is studied. However, this is the relevant part of heterogeneity as the same information is also observed by the policy makers.

Most of the earlier studies on the cyclicality of unemployment duration and compositional variation have analysed macrodata because large panel datasets

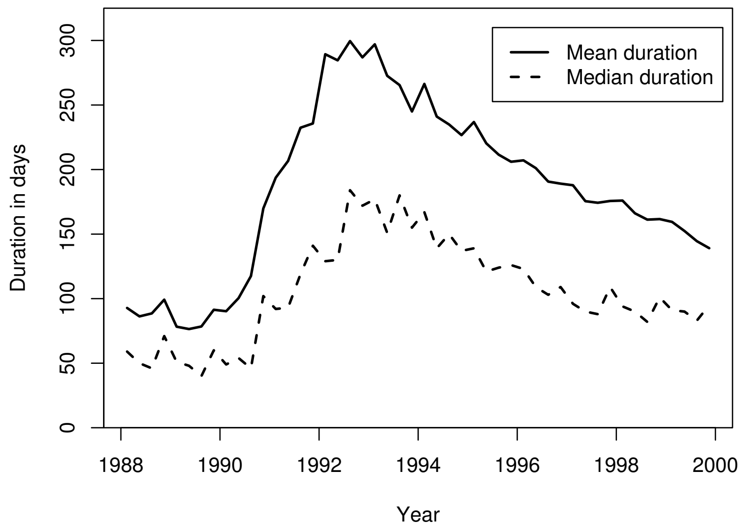

Figure 1: Mean and median duration of unemployment spells by the quarter of entry (source: analysis data).

have become available only recently. One of the main questions in the macro level analysis is how to identify the effect of heterogeneity (for demographic group level analysis, see Baker, 1992; Abbring, van den Berg & van Ours, 2001). The method introduced by van den Berg & van Ours (1994) allows the estimation of mixed proportional hazard model with discrete aggregate data on outflow from unemployment. The main advantage of the method is that this type of data are more commonly available than microlevel data, especially for long time periods. Abbring, van den Berg & van Ours (2002) apply this method to study cyclical variation in French unemployment. The same approach has been used in other studies (e.g. Turon 2003; Burgess & Turon 2005; Cockx & Dejemeppe 2005; Dejemeppe 2005).

The first studies analysing cyclical variation in unemployment duration using microdata suffered from relative small sample sizes and short follow-up periods (e.g. Dynarski & Sheffrin 1990). Rosholm (2001) addresses this topic using register data with large sample size and long time period. He analyses Danish data from 1981 to 1990 and finds that compositional variation is important in explaining unemployment duration and that the average quality of those becoming unemployed improves during booms. Other microdata studies that emphasise business cycle variation include Imbens & Lynch (2006) who analyse unemployed youth and Bover, Arellano & Bentolila (2002) who, however, do not focus on the compositional variation.[1] It should be noted that the macrodata studies model exit rate from unemployment while the microdata studies are able to distinguish between the exit states and focus on employment rate.

The analysis dataset used in this study is a 10% representative sample of the Finnish workforce containing information from several administrative registers from 1987 to 2000. Most importantly the data include the dates of transitions to and out of unemployment. The unemployment spells are followed until the end of 2001. In addition, information is provided on transitions to employment and active labour market programmes. A rich set of variables describing individual characteristics are available on annual level. These data are used to create a set of labour market history variables for each individual.

Unemployment duration until employment is modelled using a proportional hazard model with a piecewise constant baseline hazard. All unemployment spells starting between the beginning of 1988 and the end of 1999 are included in the model. The key variable in the model is the seasonally adjusted regional unemployment rate. It is included as a time-varying covariate that changes value quarterly. Annual and regional fixed effects are used to control for general regional differences and calendar time effects. Thus, the main source for identifying variation is obtained from within region variation in the unemployment rate. The time-varying quarterly dummies capture seasonal variation in employment. Individual characteristics are included as fixed covariates.

The model is estimated separately for genders and four time periods because the parameter values of the model change over the business cycle. The results show that the inflow composition changes during the recession as unemployed individuals become older and better educated on average. The structural change in the economy is also reflected in the occupational distribution. However, the outflow effect dominates and the observed compositional variation implies only a relatively small increasing trend in the predicted average duration between 1988 and 1993. This means that the characteristics of those entering unemployment became slightly less favourable for employment. The seasonality in unemployment duration, that is predicted using inflow variation, is strong and its pattern changes after the recession.

The remaining paper is organised as follows. Section 2 briefly discusses the economic development and the labour market policy in Finland. The analysis data are described and descriptive statistics are shown in Section 3. Section 4 discusses econometric methods. Results are presented in Section 5 and Section 6 concludes.

[1] Compositional variation has not been analysed explicitly using Finnish data but the effect of business cycle on unemployment duration has been studied to some extent by Holm, Kyyrä & Rantala (1999) and Koskela & Uusitalo (2006).

2 Institutional setting

2.1 Finnish economy

The Finnish economy was very volatile during the analysis period. Variation in unemployment and GDP growth is illustrated in Figure 2. The late 1980s was characterised by high economic growth and low unemployment. Especially the proportion of long-term unemployment[2] decreased which was mostly due to the government's policy to use active labour market programmes (ALMP) to prevent people from falling into this category. The boom turned into an economic crisis in 1990 and the unemployment rate started to rise dramatically.[3] During the following years, the proportion of long-term unemployed grew quickly because of the large number of layoffs at the time when re-employment possibilities were weak. The economy started to recover in 1993 and the unemployment rate stabilised. During the next years, the GDP grew and the unemployment rate declined. However, the proportion of long-term unemployment did not decrease. This can be seen as a result of a structural change in the economy: economic recovery took place only on some sectors of the economy and there was a large number of people who had poor employment possibilities. In the late 1990s, the economy was booming again. The unemployment rate decreased steadily but the high long-term unemployment was persistent.

2.2 Finnish labour market policy

Institutional features have a strong effect on individuals behaviour during unemployment. The unemployment benefit system affects the incentives to search and to accept a job. The strong emphasis on ALMP in Finland is the main reason for individuals exiting other state than employment.

The unemployment benefit system is a combination of a basic daily allowance and an earnings-related allowance with limited duration.[4] The basic allowance is 23 euros per day and it is paid for 5 days per week. Those with children get an increase from 4 to 8 euros. The duration of the basic allowance is unlimited but it is required that the unemployed person is willing to accept a job offer. The benefit is lost for 30 to 90 days if the person has quit a job, refuses to accept a job or refuses to participate in ALMP.

To be entitled for the earnings-related allowance, a membership in an unemployment fund and a 10 months employment history during the last two years

[2]The long-term unemployment rate is the main macroeconomic indicator that is related to unemployment duration. Individuals are defined as long-term unemployed after 12 months of unemployment.

[3]For more detailed discussion on Finnish economic development and unemployment, see Koskela & Uusitalo (2006).

[4]The figures are for the year 2003.

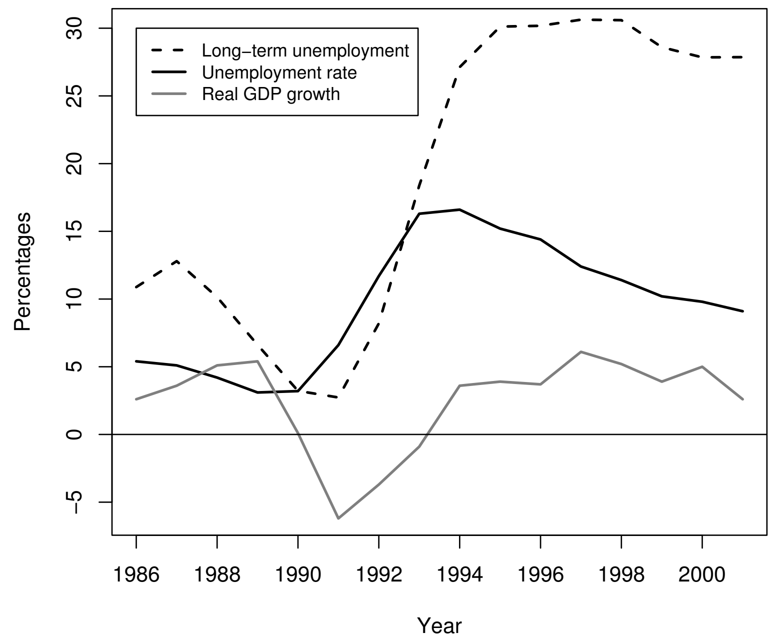

Figure 2: Unemployment rate, proportion of long-term unemployed and GDP growth in Finland (Statistics Finland; Labour Force Survey).

are required.[5] The replacement rate decreases with earnings. It varies from almost 80% to below 40% with monthly earnings from 1000 euros to 4000 euros, respectively. For the median income earner, the net replacement ratio is 64%. The duration of the earnings-related benefit is 500 days and the benefits are paid for 5 days per week, i.e. the maximum duration is close to two years.

There are some special rules considering young and elderly people. An unemployed person under 25 years of age is obliged to seek and participate in vocational education.[6] Otherwise a young person is not eligible for the basic allowance. Before 1997 people over 53 years of age were entitled for the earnings-related allowance until the retirement age. In 1997 the age limit was raised to 55 years.

Since the 1970s, the activation of unemployed individuals has played an important role in the Finnish labour market policy. The main objective has been to reduce frictions in the market by offering education and guidance in job search. Participation in labour market training increases the length of the earnings-related allowance by 4 months. The share of the labour force in training has varied from

[5]The required number of months in work was raised from 6 to 10 months in 1997. The requirements were changed again in 2003.

[6]This rule came into effect first in 1996 for those under 20 years of age but it was extended for those under 25 in 1997.

1% to 2% in the 1990s.

Another form of ALMP is to offer subsidised jobs for individuals who have difficulties in finding a job. At the end of the 1980s, government had an aim of full employment and since 1988 there was a commitment to offer a subsidised job for all individuals in long-term unemployment. For those under 20 years of age, the time limit was 6 months. As a result of this policy, the proportion of long-term unemployment was very low before the recession. However, soon after the dramatic rise in unemployment, it became impossible to offer a job for all and the commitment was abandoned gradually by 1993. The share of the labour force in subsidised jobs rose from 1% to 2.5% between 1990 and 1997.

Wages in Finland are determined to a large extent by collective agreements between trade unions and employer organisations. During the analysis period, the coverage of agreements was around 95% of workers. There is no minimum wage legislation but collective contracts contain job-complexity and education specific minimum wages.

3 Data

3.1 Analysis data

The analysis data are based on the Employment Statistics database of Statistics Finland. The dataset is a representative sample of 350,000 individuals between 12 to 75 years of age living in Finland in 1997. The information in the data is combined from several administrative registers from 1987 to 2000. The labour administration provides most of the important information for this study, including dates of individual labour market transitions. The information on job spells comes from the pension institutes.

The analysis data are constructed as an inflow sample by including unemployment spells starting between the beginning of 1987 and the end of 1999. The follow-up ends at the end of 2001 which means that the ongoing spells are censored at that time. Spells starting after 1999 are excluded to allow at least two years follow-up and because some background variables are not available for 2000. The background variables include demographic and socio-economic characteristics of individuals.

There are some drawbacks in the dataset. Only one employment spell and one ALMP spell of each type is recorded per year. In addition, only four unemployment spells are included annually. However, the share of individuals with four spells in one year is very low in the analysis data.

The registers of labour administration are not complete. Approximately 6% of the unemployment spell end dates and 20% of the information on the exit state are missing in the original dataset. It is possible to fix a major proportion of the missing data by using other information in the dataset. However, the overall share of missing information remains above 10% because the exit state is often encoded as 'other state or unknown'.[7]

The major institutional changes should be taken into account when unemployment is analysed over a long time period. Especially the reform in 1997 concerning elderly people had a major impact on the employment probability (Kyyrä & Wilke, 2007). This is addressed by limiting analysis data to individuals from 20 to 49 years of age. In addition, 2906 individuals are removed from the data because of missing covariate information. This leaves a dataset of 111,764 individuals having 423,126 unemployment spells between 1988 and 1999.

3.2 Variables

The key variable in this analysis is the indicator of macroeconomic conditions or the business cycle. The previous studies have used several different measures. Popular choices include the unemployment rate and GDP or some transformation of these. The regional unemployment rate is used in this study as it is directly linked to the changes in labour demand. It is available as a quarterly series for 13 labour force districts. Regional series has two advantages over national series. Firstly, it takes into account the regional differences that are relatively large in Finland. Secondly, it brings more variation and strengthens the identification. To remove variation that is not related to the business cycle, seasonally adjusted unemployment series is used (see the Appendix).

Quarterly dummies are used to capture the strong seasonal variation in employment probability. Annual dummies denoting the year unemployment begins are included to capture time trends that are not captured by the unemployment rate. The region of residence is included to take into account fixed regional differences.

Individual background information is observed either at the end of the year preceding unemployment or when individuals register as unemployed. The variables are: gender, age (6 categories), education (4), broad occupation (9), family type (6), native language (3), the statistical classification of the residence area (3) and a disability indicator. In addition, the following variables were constructed using the information on labour market history available in the data: time in unemployment during previous 12 months (4 categories), previous labour market state (4 categories) and indicator for repeated unemployment (over two spells during the past 12 months). A detailed variable description is provided in the Appendix.

Figure 3: Smoothed quarterly inflow to unemployment and outflows from unemployment. Both number of exits to any state and number of exits to employment are shown.

3.3 Descriptive statistics

The changes in the number of unemployed individuals can be illustrated using inflow and outflow series. Figure 3 presents quarterly flow series computed from the analysis data. Because of strong seasonality, Loess smoothing is used.[8] When the recession starts in 1990, the gap between inflow and outflow starts to grow. The number of unemployed individuals increases quickly until 1994 when the outflow finally exceeds the inflow. After that, the outflow remains higher than inflow and the unemployment rate decreases slowly but steadily. It is interesting that flows remain on much higher level after the recession. This reflects the fact that repeated unemployment increases during the recession. The large impact of ALMP is seen in the outflow to employment which grows slowly compared with other flows.[9]

Table 1 presents the exits from unemployment by the exit state and the year unemployment has started. The shares of exit reasons vary substantially between years. In the late 1980's, around 60% of the individuals are known to exit to

[7]The details of the procedures that were used to fix missing information are presented in Verho (2005).

[8]Loess is a local regression method proposed by Cleveland (1979).

[9]The same definition of employment is used here as in the duration model. Employed include recalls and exits to unknown state.

Table 1: Exit states from unemployment in percentages and the total number of unemployment spells by the starting year of unemployment.

	Employed	Recall	Unknown	ALMP	Out of LF	Total
1988	61.6	8.9	13.9	8.1	7.5	20123
1989	61.2	8.5	14.3	8.0	8.0	19090
1990	47.3	8.5	19.5	18.2	6.5	21471
1991	28.5	3.8	31.8	29.4	6.4	37117
1992	29.1	5.5	25.6	29.9	10.0	42780
1993	31.9	10.9	14.3	31.2	11.8	44619
1994	42.3	6.0	8.1	31.1	12.5	42607
1995	43.0	5.6	7.7	32.2	11.5	42371
1996	43.7	5.1	7.9	32.1	11.1	43949
1997	43.8	5.1	7.5	32.3	11.3	37869
1998	44.7	4.9	10.4	29.5	10.5	36195
1999	45.1	5.4	14.3	24.8	10.4	34935
Total	41.5	6.3	14.3	27.7	10.2	423126

Note: Employed = exit to employment can be identified from the data, Recall = recalled by the previous employer, Unknown = exit state cannot be identified from the data, ALMP = labour market training or subsidised work, Out of LF = exit from labour force.

employment. When the recession starts this share drops quickly while the number of individuals exiting to active labour market programmes increases. Also the number of individuals who leave the labour force grows. In recalls, there is a large peak in 1993. When the recovery in the economy starts around 1994, there is no large change in the share of individuals exiting to active labour market programmes or out of the labour force.

The unknown state in Table 1 consists of individuals for whom the exit state could not be determined from the data. If individuals find a new job without using the public employment services, the labour administration is often not informed. To some extent it is possible to identify exits to employment by using the information on labour market history that is available in the data. Yet a relative high share of individuals exit to unknown state. The share of unknown exits increases especially during the recession.

The changes in the composition of individuals who flow into unemployment may contribute to the cyclical variation of the average unemployment duration. Figure 4 shows the annual inflow composition by age, education and occupation. In 1987, half of the individuals entering unemployment are under 30 years. Their share drops and the share of over 40 years old grows gradually by 10 percentage points. At the same time, the proportion of individuals with basic education declines while tertiary education becomes more common among the unemployed individuals. These trends are roughly similar for men and women.

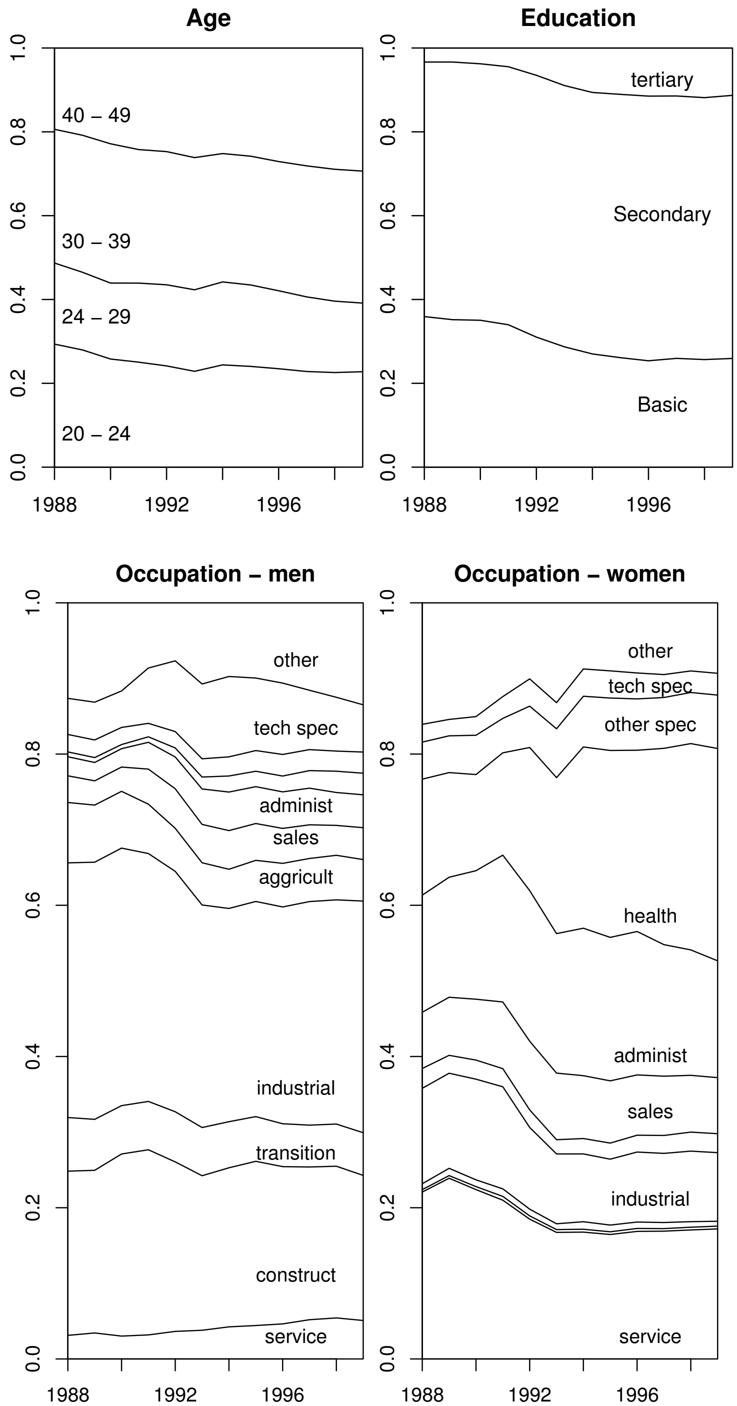

Figure 4: Variation in the composition of inflow for age, education and occupation.

The occupational distributions of unemployed individuals are given in the lower panels of Figure 4 by gender since there are large differences. For unemployed men, the common occupations are in industrial and construction work. During the analysis period, the share of the other occupations increases slightly. Between 1990 and 1992, the proportion of technical specialists grows while especially the share of industrial occupation diminishes. The common occupations for unemployed women are in health care, service and administrative work. During the period, the share of health care and other specialist occupations grows and the share of service and industrial occupations decreases. The distribution changes one year later than for men. The detailed characteristics of unemployed individuals are presented in the Appendix.

4 Econometric methods

4.1 Model

Unemployment durations are conveniently modelled by specifying a model for the hazard function. An unemployment duration T is censored when the exit state is other than employment or when the duration is longer than the follow-up period. Also spells that end to recall or to exit into unknown state are considered as exits to employment. The exits to unknown state are more likely exits to employment than exits out of the labour force in the analysed age groups.[10] The follow-up period is limited to three years.

The model is used to study the determinants of unemployment duration over time. This is done by predicting the impact of inflow composition and the business cycle variables. A proportional hazard model with piecewise constant baseline hazard is chosen because it provides a flexible specification that is useful for prediction purposes. The model for hazard θ at duration t can be denoted

$$\theta(t) = \lambda(t) \exp(x(t)\beta),$$

where $\lambda > 0$ is the baseline hazard and $\exp(x(t)\beta)$ is the systematic part including the explanatory variables x. The piecewise constant baseline hazard is specified using 14 interval parameters α_j. The first two intervals are 30 days to capture the quickly deceasing hazard at the beginning of the spell. The next 11 intervals are 60 days and the last interval is a residual piece from 720 to 1095 days. If $\alpha_j > \alpha_{j+1}$, it implies a negative duration dependence between intervals j and $j+1$. This gives a step function

$$\lambda(t) = \exp(\alpha_j), \quad c_{j-1} \leq t < c_j, \quad j = 1, \ldots, 14.$$

[10] The exits to unknown state are not strongly related to the duration of spell. The main results of compositional analysis are robust to changing the event definition by treating the exits to unknown state as censored observations.

Three different type of explanatory variables are included in the model. The individual background variables x_1 are observed at the beginning of the spell and kept fixed. The regional unemployment rate $u_{\tau(t)}$ varies quarterly in calendar time τ and depends on the duration time t. To allow a non-linear effect of unemployment rate, also a second order term is included. The residual calendar time variation is captured by a vector of fixed annual dummies Y and time-varying quarter dummies $Q_{\tau(t)}$ which are taking into account seasonality in employment. For technical reasons, time-varying covariates change value only between intervals. Finally, regional differences are controlled by including a vector of dummies for the region of residence R. This specification gives a model

$$\theta(t) = \exp(\alpha_j) \cdot \exp(x_1\beta_1 + R\beta_2 + Y\beta_3 + \beta_4 Q_{\tau(t)} + \beta_5 u_{\tau(t)} + \beta_6 u^2_{\tau(t)}).$$

The model is extended by including interaction terms between the linear unemployment term $u_{\tau(t)}$ and individual characteristics x_1 as well as the baseline hazard α_j. The interaction terms allow the effect of individual characteristics and the duration dependence vary according to the level of unemployment. The unemployment rate $u_{\tau(t)}$ is the difference from the mean unemployment rate in the analysis period (10%).

When the region of residence and the year the unemployment begins are controlled for, the main source for identifying variation for the unemployment rate is obtained from the within region variation across the business cycle. Regional variation in Finland is large although many regions have similar trends (see the Appendix). A second source for identifying variation is obtained from the time-variation of the quarterly unemployment rate during unemployment spells. When a spell continues over a quarter, the value of the unemployment rate changes.

The proportional hazard model is a log-linear model. Thus, it is assumed that covariates have a constant multiplicative effect on the employment hazard. However, in reality effects can vary over the duration of spells, between time periods and sub-populations. Interacting the time-varying business cycle proxy with individual characteristics allows some dependence between covariates and the duration of spell. When a long time period with large macroeconomic fluctuation is analysed, as in this case, it is very likely that parameters vary in time. Indeed, it seems that there are different time periods that follow roughly the phases of the business cycle.[11]

To take into account the differences in parameter values between different periods, the model is estimated separately for the pre-recession period (spells

[11] The annual variation of the hazard rate can be studied non-parametrically using, for example, cumulative hazards. Time variation of the model parameters can be examined by estimating the model separately by the year unemployment begins. The cumulative hazards are presented in the Appendix. Also the yearly estimated models (not reported) point to the conclusion that the analysis period should be split as the baseline hazard and the other parameter values differ noticeable between the periods.

that begin in 1988–1989), the recession period (1990–1992), the recovery period (1993–1995) and the growth period (1996–1999). In fact, the baseline hazards are relatively similar between the last two periods but there are differences in other parameters. The model is also estimated separately for genders because there are evident differences in baseline hazards and other parameters.

Duration models suffer from downward biased estimates when there is unobserved heterogeneity, especially in case of baseline hazard and time-varying covariates. A possible solution would be to follow Heckman & Singer (1984) who suggest estimating the mixing distribution in a mixed proportional hazard model to correct the bias. However, the interest in the parameter estimates is limited in this case because the model is mainly used for predicting. Therefore, the explicit modelling of the unobserved heterogeneity is not very useful as it doesn't change the mean effects (Wooldridge, 2002, p. 706). In addition, there seems to be a trade-off between the flexibility of the baseline hazard and the number of the mass-points used in the non-parametric unobserved heterogeneity distribution (Baker & Melino, 2000).

The piecewise constant baseline hazard implies that single intervals are independent and follow an exponential regression model.[12] Many individuals experience multiple spells during single analysis periods (see the Appendix). This is typical for individuals in seasonal work or for those who have a loose attachment to the labour force. However, it is assumed in the analysis that after controlling an extensive set of individual covariates and detailed labour market history variables, the multiple spells can be considered as independent observations.

4.2 Identification of the sources of variation

The different sources of variation in unemployment duration until employment are identified following Rosholm (2001). The components are compositional variation, an outflow effect that affects all unemployed individuals and residual calendar-time variation. A similar approach has also been used with aggregated data (e.g. Abbring et al., 2001). The basic idea is to allow each component to take different values over time while keeping others fixed. Then the expected unemployment durations until employment $\mathrm{E}(T|x_1, R, Y, Q, u)$ are predicted quarterly for each year which will show the variation that the studied component creates.

The compositional variation gives the impact of the observed individual heterogeneity. The predictions are obtained for each cohort of individuals who enter unemployment in a given quarter and year. The variables taking different values are x_1 and R according to the inflow composition. The regional unemployment u is kept on the average level of the analysis period (10%). Also the annual and

[12] The model is a special case of a Weibull model or a Poisson model with an offset parameter which implies that the model can be conveniently estimated using the standard procedures available in statistical software packages.

quarterly dummies are kept on their average level ($\overline{Y}$,$\overline{Q}$). This measures, for example, the impact of change in the average age or education of individuals who enter unemployment between the first quarter of 1988 and last quarter of 1999.

The outflow effect is obtained using the aggregate unemployment rate as a proxy for the business cycle. The predictions are computed for the average person ($\overline{x_1, R}$) in the data and u takes the values of the seasonally adjusted quarterly aggregate unemployment rate. The calendar time dummies are kept again on their average level ($\overline{Y}$,$\overline{Q}$). This gives the direct influence of the business cycle on unemployment duration. Finally, the influence of the residual calendar-time variation is predicted using the annual and quarterly dummies (Y, Q) while keeping other variables at their expected level. The predictions are obtained for the average person ($\overline{x_1, R}$) and the unemployment rate is kept on 10% level.

5 Results

The results are presented first for a basic model without interactions terms. The marginal effects of the key covariates are presented to illustrate what determines unemployment durations and how large is the variation between the analysis periods. Then the model is extended by interacting the linear unemployment rate term with individual covariates and baseline hazard. This allows duration dependence and the effect individual characteristics to vary by the level of unemployment in the region. To motivate the extension of the model, significance of the interaction terms are tested. Then the impact of compositional, business cycle and residual-time variation on unemployment duration is studied.

5.1 Effect of covariates

The coefficients of the model give the marginal effect of the variables on the log hazard. The key covariate in the analysis is the regional unemployment rate. It is included as a second order polynomial in the model. Figure 5 shows the effects for the range of aggregate unemployment rates that are observed in each analysis period. The unemployment rate has a statistically significant effect in all cases except for women in 1988–89. Generally, an increase in the unemployment rate is related to a lower hazard rate and longer unemployment duration. However, for the low values of unemployment in 1988–89 and high values in 1990–92 the relation is reverse for men. The magnitude of coefficients is relatively small which means in practise that the regional unemployment rate works somewhat poorly as a proxy for the business cycle.

Figure 6 presents the coefficients for a set of interesting individual covariates. There are obvious changes in the parameters between the periods. This points to the conclusion that compositional variation contributes through both inflow variation and changes in the relative position of the different groups of unemployed

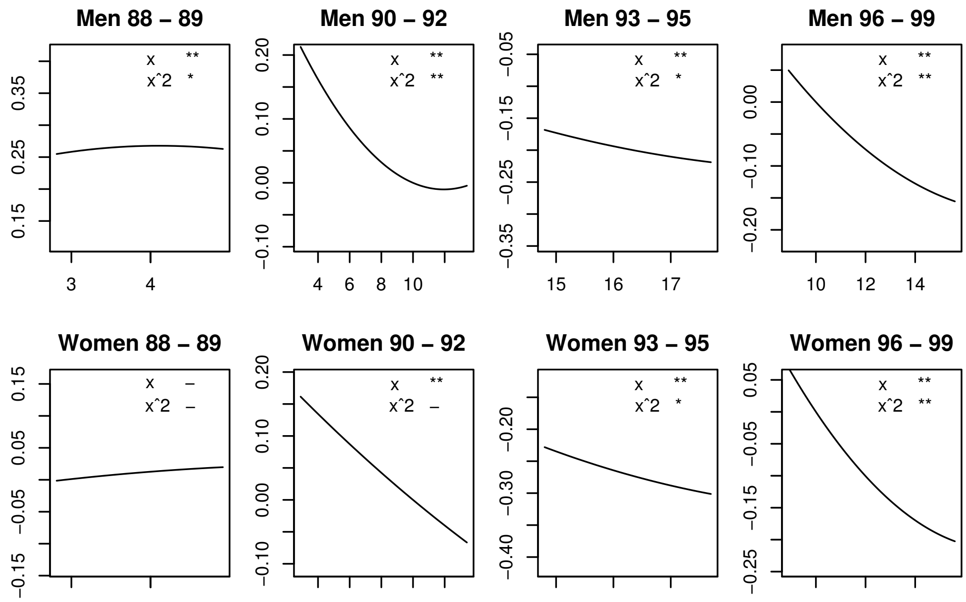

Figure 5: The effect of the regional unemployment rate on log hazard for a range of values in percentages. The significance of the coefficients on 5% level is denoted by * and on 1% level by **.

individuals. Interacting the regional unemployment rate with individual covariates provides some more flexibility in the model.

There are interesting patterns in the coefficients that are related to the changes in relative labour demand. The increase in the coefficients show that the relative position of 25–29 and 45–49 years old men becomes better during the analysis period. In case of education, the individuals with tertiary education perform worse after the recession, i.e. the last two coefficients are lower. The recession also changed demand for different skills which is reflected in the large time variation in the occupation coefficients. The full model output is presented in the Appendix.

The basic model is extended by interacting the regional unemployment rate $u_{\tau(t)}$ with baseline hazard α_j and individual covariates x_1. Table 2 shows the results of likelihood ratio tests between the basic model and models where a single interaction term is introduced at a time. The interaction with baseline hazard is significant in every model which indicates that duration dependence changes with the level of unemployment. Also all interactions with individual covariates are significant except in case of disability indicator for men and area type for women. For consistency, all interaction terms are included in the full model for both genders.

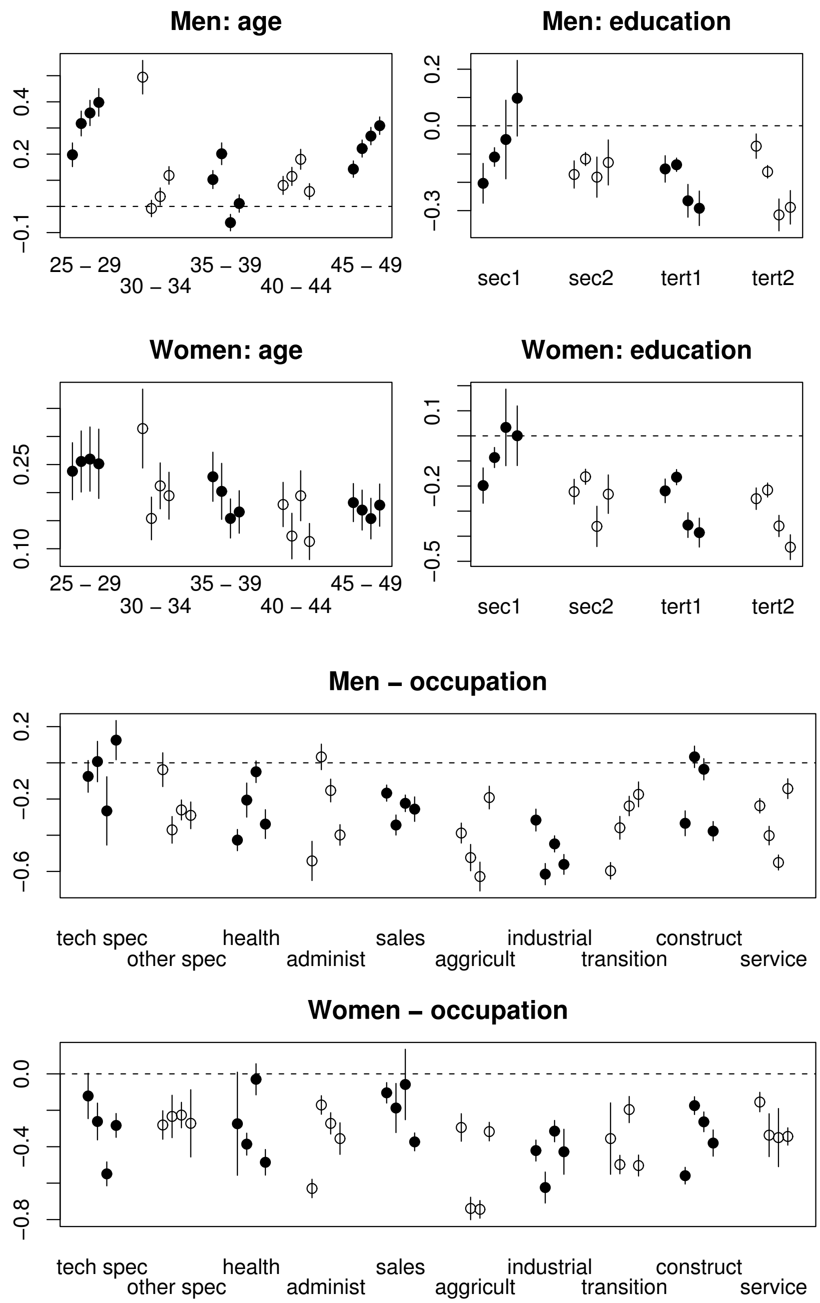

Figure 6: Coefficients for age, education and occupation. y-axis shows the marginal effect on log hazard. The baseline group is 20–24 years old with basic education and 'other' occupation. Successive points show the coefficients for the four analysis periods. Vertical bars denote 95% confidence intervals.

Table 2: Tests of interaction between the regional unemployment rate and individual covariates.

	88–89	90–92	93–95	96–99
Men				
baseline	**	**	**	**
age	-	**	**	**
education	**	**	**	**
occupation	**	**	**	**
family type	*	**	-	**
language	-	**	**	*
area type	*	**	**	-
disability	-	-	-	-
unemployment history	**	**	**	**
repeated unempl.	*	-	-	*
previous state	*	**	**	**
Women				
baseline	*	**	**	**
age	-	*	**	**
education	**	**	**	**
occupation	**	**	**	**
family type	-	**	-	*
language	*	**	-	-
area type	-	-	-	-
disability	**	-	*	*
unemployment history	-	**	**	**
repeated unempl.	-	-	-	**
previous state	-	**	**	**

Note: Likelihood ratio tests are done by including a single interaction term at a time. No significance is denoted by -, 5% level significance by * and 1% level by **.

5.2 Determinants of unemployment duration

The following analysis illustrates the relative contribution of compositional changes in the unemployment inflow, the outflow effect and the residual-time variation to predicted unemployment duration. The aggregate unemployment rate is shown in Figures 7 and 8 due to its role as a business cycle proxy. The predicted series are discontinuous because the predictions are obtained from separate models.

Figure 7 presents the role of compositional variation. The upper panel shows that the predicted compositional variation is relatively small compared with overall changes in the average unemployment durations. However, the lower panel with finer scale reveals that compositional variation includes trends and noticeable seasonal variation. Before 1993 there seems to be a mild increasing trend

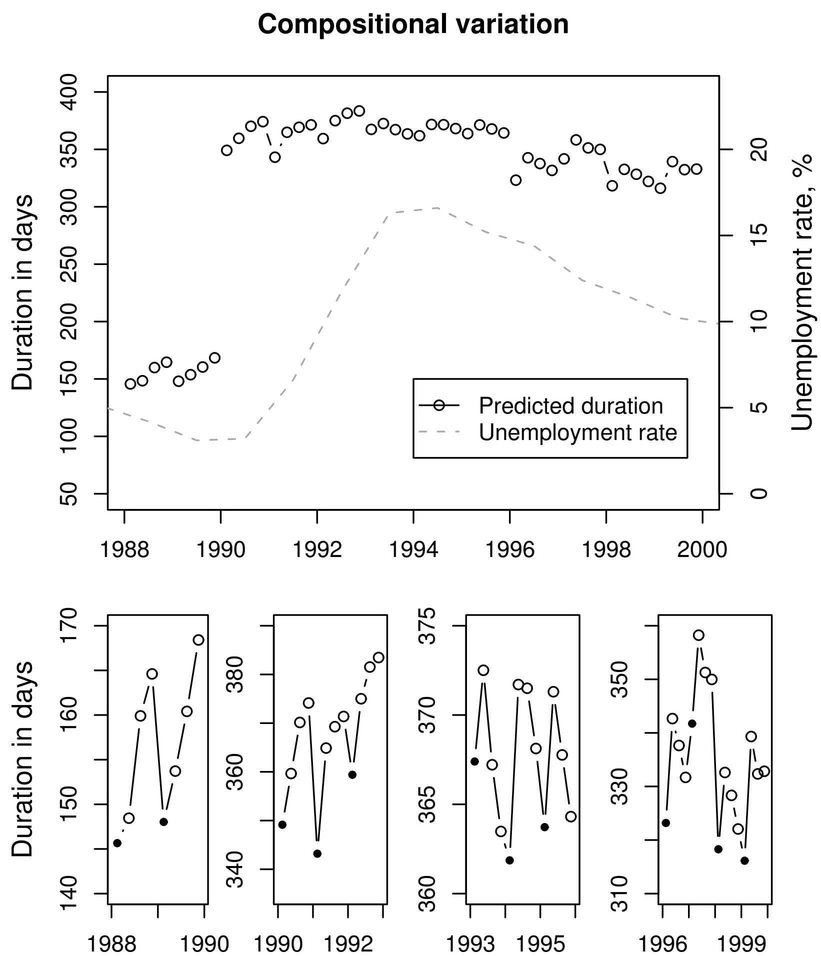

Figure 7: Effect of compositional variation on the predicted unemployment duration and the unemployment rate. The upper panel shows the predictions on the same scale and the lower panel shows the analysis periods on separate scales (black dot denotes the first quarter of each year).

which means that the average observed characteristics of individuals become less favourable for employment.[13] Between 1993–96 the magnitude of the variation is small. From 1996 onwards the variation in the predictions is larger but there is no evident trend.

The seasonality in the compositional variation is quite strong, especially in the early periods. In 1988–89, the within year variation is 13% of the predicted mean duration in the period. The respective share is half smaller in 1990–92 and becomes even smaller later. In the first two periods, the later quarter individuals enter unemployment, the worse characteristics they have. In the two last periods, the picture changes as the characteristics are worse for those who enter unemployment in the second quarter.

The magnitude of changes between the annual mean durations are smaller. In the first period, the increase is 1.9% and in the second period the largest change is 3.5% compared with the previous year. Between 1993 and 1996, the respective changes are very small but in the last period the change between 1997 and 1998 is relatively large, -7.1%. The previous studies have mixed results on the relevance of compositional variation. Rosholm (2001) finds noticeable procyclical compositional variation, i.e. the characteristics of individuals entering unemployment improve during booms. The results of this analysis are more in line with van den Berg & van den Klaauw (2001), Abbring et al. (2002) and Imbens & Lynch (2006) who find the influence of cyclical compositional effects to be small or negligible. Also Abbring et al. (2001, 2002) find seasonality in compositional variation to be important. However, Abbring et al. (2002) find the pattern to be quite different in France as those entering unemployment in the last two quarters have the highest exit rates.

The effect of unemployment rate and residual variation are shown in Figure 8. The predictions are done using the seasonally adjusted aggregate quarterly unemployment rate. It seems that the model is unable to contribute the business cycle variation to the unemployment rate and the majority of the variation is captured by the annual dummies. This is true especially in the recession period. The model performs better in 1996–99 where the unemployment rate captures the declining trend and the residual variation consists mainly of seasonal variation.

The predicted impact of quarterly dummies is very large. This is partly due to the fact that the seasonal dummies are kept constant during the predicted spells which overstates their effect. Interestingly, the quarterly dummies show a different type of seasonality than compositional variation. The summer season seems to be the best time for employment while the last quarter of the year is the worst.

The magnitude of compositional variation is relevant for the efficient design

[13] When compositional variation is studied without the labour market history variables, the pattern changes interestingly. The small increasing trend changes to a small decreasing trend.

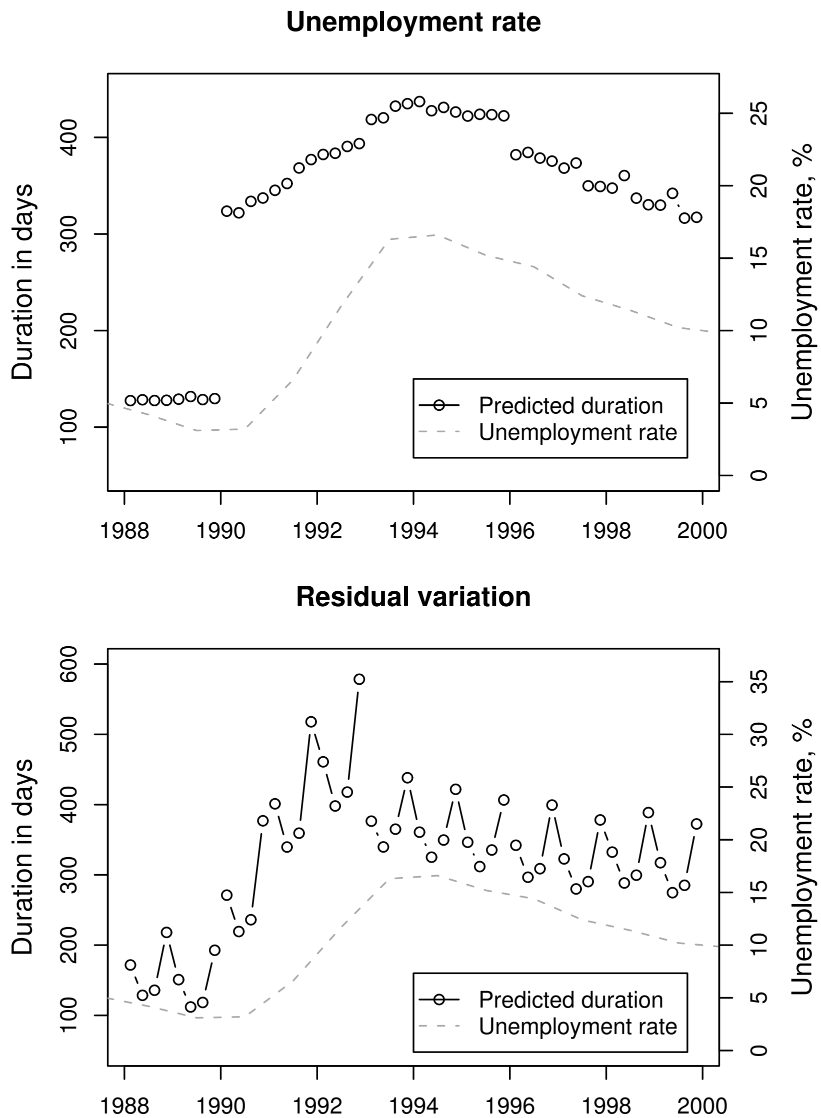

Figure 8: Effect of aggregate unemployment rate and residual variation on the predicted unemployment duration and the unemployment rate.

of labour market policy. When compositional variation is large, it may be useful, for example, to adjust ALMP over the business cycle by targeting programmes for those with long expected unemployment durations. However, according to the results the variation in the predicted duration is dominated by outflow effects, i.e. the aggregate unemployment and residual variation. Thus, from this perspective there does not seem to be large opportunities for more efficient targeting of ALMP. A possible interpretation of the result is that the ALMP practice used in this period was sufficient to smooth larger compositional variation. Based on the results, there could be more room for adjusting ALMP to account for the seasonality of compositional variation. In 1994–1999, those entering unemployment in the second quarter have the longest predicted durations. This could be related to school leavers although inspecting those entering unemployment does not reveal large shifts in the quarterly age distributions.

6 Conclusions

The unemployment rate in Finland increased dramatically during the recession in the early 1990s. The unemployment rate is influenced by both the number of inflow and the average duration of unemployment. This study analyses the determinants of unemployment duration in Finland using individual data from 1987 to 2000. The main question in the study is how much the changes in the composition of individuals contributed to the large increase in the average unemployment duration during the recession.

Three different components in the unemployment duration are identified following Rosholm (2001). The compositional effect is obtained by taking into account the changes in the observed heterogeneity of inflow. For example, when more individuals who are slowly employed enter unemployment, the average duration increases. The outflow effect is captured by using the regional unemployment rate as a proxy for macroeconomic conditions. Annual and quarterly dummies are used to capture residual calendar-time variation.

Eight separate duration models are estimated for genders and for the unemployment spells starting in the following time periods: 1987–1989, 1990–1992, 1993–1995 and 1996–1999. The analysis shows that there are large changes in the parameter values between the periods. This is not surprising given the large structural change that took place in the economy. The change is also reflected in the inflow composition as individuals entering unemployment become older and better educated on average. Also the occupational distribution changes.

The observed compositional variation implies only a relatively small increasing trend in the predicted unemployment duration in the recession period. This means that the change in the composition of new unemployed individuals is not a major component in the large increase in the unemployment duration. The characteristics of individuals became slightly less favourable for employment. The result can be contrasted to Rosholm (2001) who finds a noticeable effect of compositional variation. Unimportant cyclical inflow composition effects, that are more similar to this study, have been found by van den Berg & van den Klaauw (2001), Abbring et al. (2002) and Imbens & Lynch (2006). Interestingly, the seasonal variation predicted using compositional variation is relatively strong. The outflow effect contributes most of the variation in the predicted duration. This points to the conclusion that compositional variation is not a major concern from the policy point of view. However, compositional variation includes relatively strong seasonality which could be taken into account when adjusting labour market policy.

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Appendix

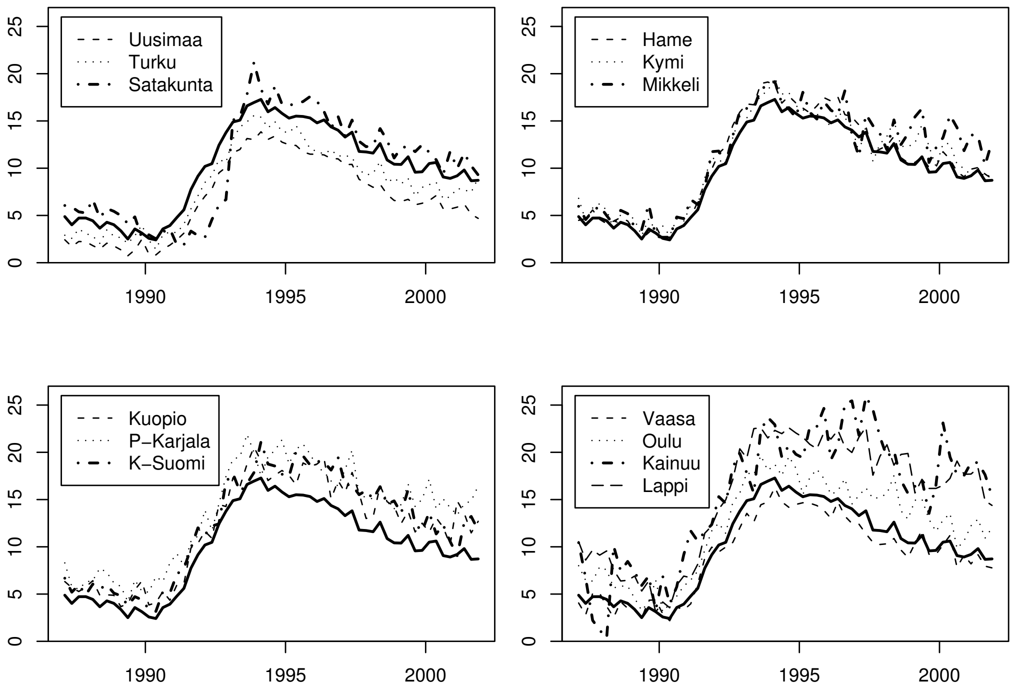

Figure 9: Seasonally adjusted regional and aggregate (solid line) unemployment rate series (source: Labour Force Survey). The definition of unemployment changed in 1997 due to EU standards. The series by the old definition is available for the period 1987–1996 and by the new definition for the period 1995–2001. The overlapping period was used to adjust 1987–1994 unemployment rates using a linear model (R-squared 0.97). Seasonal adjustment was done separately for each series using quarterly seasonal dummies.

Table 3: Descriptive statistics for men (%).

	1988–1989	1990–1992	1993–1995	1996–1999
Age: (19,24]	28.17	24.56	24.21	24.44
(24,29]	18.49	19.15	19.28	16.76
(29,34]	16.63	16.17	16.01	15.91
(34,39]	16.42	15.96	14.37	14.30
(39,44]	12.56	15.10	14.57	14.06
(44,49]	7.73	9.04	11.56	14.54
Education: primary	37.89	33.83	29.81	30.33
secondary 1	5.93	6.05	7.23	7.32
secondary 2	53.44	55.23	54.83	54.79
tertiary 1	1.34	2.95	4.60	4.05
tertiary 2	1.41	1.95	3.53	3.52
Occupation: other	12.88	8.87	10.16	11.93
tech spec	4.88	7.63	10.04	7.79
other spec	2.32	2.04	2.55	2.78
health	0.65	0.88	1.90	2.49
administ	2.48	3.58	4.88	4.62
sales	3.37	4.58	5.02	4.33
aggricult	7.78	6.39	5.41	5.72
industrial	33.84	32.65	28.73	29.56
transport	6.92	6.49	6.14	5.61
construct	21.62	23.55	21.03	20.12
service	3.27	3.34	4.14	5.06
Family type: other	34.88	36.51	35.60	43.09
married	4.05	4.67	5.04	4.44
married & children	43.88	44.49	45.21	37.03
unmarried & children	4.60	5.97	5.98	7.03
single parent	12.58	8.37	8.17	8.42
Language: Finnish	97.55	96.37	95.16	95.21
Swedish	1.98	2.72	3.29	2.70
other	0.47	0.91	1.55	2.09
Area type: urban	51.88	53.75	54.41	56.22
semi urban	14.98	16.03	16.50	16.58
rural	33.15	30.21	29.09	27.19
Disability	6.03	4.41	3.93	3.80
UE history: 0	45.52	47.11	29.07	28.16
(0,30]	11.27	9.96	7.21	7.92
(30,180]	33.75	31.35	35.85	37.69
(180,365]	9.45	11.58	27.87	26.22
Repeated UE	3.13	2.51	2.39	2.84
Previous state: other	72.53	72.15	64.99	65.41
subsidised empl	8.08	8.26	13.24	10.84
training	3.31	3.84	7.13	9.39
work	16.07	15.75	14.65	14.37
Region: Uusimaa	12.76	18.25	19.61	12.82
Turku	7.31	7.54	8.29	11.35
Satakunta	5.59	5.26	4.86	6.06
Hame	13.30	14.62	14.42	16.42
Kymi	7.44	6.86	6.70	6.29
Mikkeli	5.22	4.59	4.47	5.32
Kuopio	7.31	6.20	5.99	6.22
P-Karjala	5.45	4.64	4.43	5.03
K-Suomi	5.25	5.67	5.49	5.20
Vaasa	7.53	8.04	8.32	8.88
Oulu	9.89	8.55	8.30	7.72
Kainuu	4.37	3.35	2.98	3.77
Lappi	8.58	6.43	6.14	4.92
N obs	21696	59837	67974	73318

Table 4: Descriptive statistics for women (%).

	1988–1989	1990–1992	1993–1995	1996–1999
Age: (19,24]	29.30	25.20	23.20	21.60
(24,29]	19.50	18.60	19.90	18.20
(29,34]	16.70	16.20	16.30	16.90
(34,39]	14.70	15.90	15.30	15.30
(39,44]	11.30	14.50	14.00	14.70
(44,49]	8.60	9.70	11.30	13.30
Education: primary	32.70	31.70	24.50	21.50
secondary 1	8.10	9.20	9.40	8.80
secondary 2	55.10	53.50	53.60	54.60
tertiary 1	1.20	2.40	6.90	8.60
tertiary 2	2.80	3.20	5.60	6.50
Occupation: other	15.80	11.90	10.30	9.30
tech spec	2.30	3.10	3.60	3.10
other spec	4.90	5.10	6.70	6.80
health	14.60	15.70	23.10	26.20
administ	15.70	19.10	19.00	17.20
sales	7.60	8.80	8.40	7.70
aggricult	2.50	2.40	2.00	2.40
industrial	12.60	12.30	9.00	9.20
transport	0.90	0.90	0.90	0.80
construct	0.30	0.50	0.40	0.40
service	23.00	20.20	16.70	17.00
Family type: other	22.30	28.00	30.40	31.80
married	6.40	6.50	6.80	6.40
married & children	49.90	46.30	45.20	40.50
unmarried & children	5.10	7.80	5.60	7.40
single parent	16.30	11.30	12.00	13.90
Language: Finnish	97.30	96.20	95.10	95.00
Swedish	2.10	2.80	3.50	3.10
other	0.60	1.00	1.30	1.90
Area type: urban	54.40	57.10	58.60	59.40
semi urban	15.90	16.10	16.30	16.80
rural	29.70	26.90	25.10	23.80
Disability	7.90	7.60	5.50	5.00
UE history: 0	52.90	54.20	37.20	33.00
(0,30]	10.50	10.20	7.90	9.60
(30,180]	28.90	27.60	32.60	36.80
(180,365]	7.80	8.00	22.40	20.70
Repeated UE	2.90	2.40	2.30	2.60
Previous state: other	62.30	63.20	61.50	57.50
subsidised empl	10.10	8.50	13.70	14.20
training	3.50	4.50	6.70	10.30
work	24.10	23.80	18.10	17.90
Region: Uusimaa	9.60	16.50	19.40	12.60
Turku	6.80	7.90	8.60	11.30
Satakunta	7.20	6.10	5.10	6.30
Hame	16.70	15.60	15.40	18.60
Kymi	9.00	7.80	7.70	6.60
Mikkeli	5.00	4.30	4.30	5.10
Kuopio	5.90	5.80	5.60	6.00
P-Karjala	5.00	4.50	4.00	4.60
K-Suomi	6.20	6.20	5.50	5.20
Vaasa	9.10	8.80	9.00	9.20
Oulu	8.50	7.40	7.20	7.10
Kainuu	3.20	2.80	2.60	3.20
Lappi	7.90	6.40	5.60	4.20
N obs	17517	41531	61623	79630

Table 5: Variable description.

Variable	Description
Age	Age in years at the beginning of unemployment. Classified to 6 groups: 20–24, 25–29, 30–34, 35–39, 40–44, 45–49.
Education	The highest degree earned at the time unemployment starts according to Statistics Finland classification: basic (comprehensive school), secondary 1 (lower), secondary 2 (upper), tertiary 1 (lower) and tertiary 2 (upper).
Occupation	Occupational classification according to the labour administration, see Table 6.
Family type	Type of the family: other (single or unmarried couple), married couple, married couple with children, unmarried couple with children or single parent.
Language	Native language: Finnish, Swedish or other.
Area type	Statistical classification of the residence area (municipality): urban, semi urban area or rural.
Disability	Indicator for persons who have been defined mentally or physically disabled by the labour administration. The 1997 data is used for missing information in 1998–1999.
UE history	Length of unemployment during the previous 12 months. Time in unemployment is computed using the unemployment spell information in the data and classified into: 0, 1–30, 31–180, 181–365 days.
Repeated UE	Indicator for more than two unemployment spells during the previous 12 months. The number of spells is computed using the information in the data. This captures individuals who experience repeated unemployment.
Previous state	Previous labour market state before entry into unemployment. Derived using information in the data on employment and active labour market programmes for the previous two months. Levels are other, subsidised employment, labour market training and employment.
Region	Region of residence by labour force district (13 regions).
Quarter	Quarter of year. Included as a time-varying covariate.
Start year	The year unemployment spell begins.
Regional ur	Regional unemployment rate in percentages by labour force district. Included as a time-varying covariate.

Table 6: Description of occupation classification.

Class	Description
other	No occupation classification.
tech spec	Technical specialists (engineering, chemistry, physics, biology)
other spec	Other specialists (includes teaching, law, journalism, art and humanist research)
health	Health care and social workers.
administ	Administrative, clerical and IT workers.
sales	Commercial workers (marketing, property, finance and sales)
aggricult	Agriculture, forestry and fishing workers.
transport	Transportation and post workers.
construct	Construction and mining workers.
industrial	Industrial workers.
service	Service workers (includes security, hotels and restaurants, military).

Table 7: Number of unemployment spells per individual.

	88–89	90–92	93–95	96–99
1	15407	27240	31002	26185
2	6117	14998	20728	18126
3	2289	7263	10155	11384
4	830	3065	3952	6556
5 or more	263	1841	1997	5347
sum	24906	54407	67834	67598

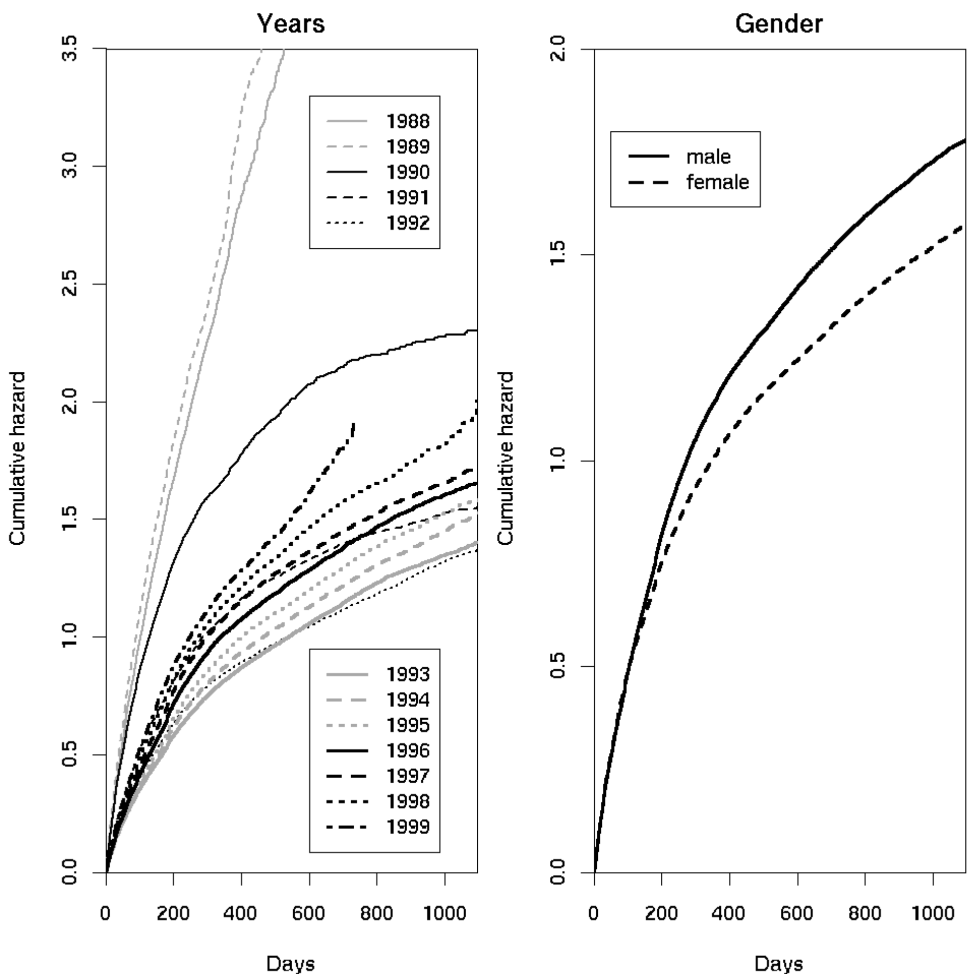

Figure 10: Cumulative hazard of employment by the year of unemployment begins and gender.

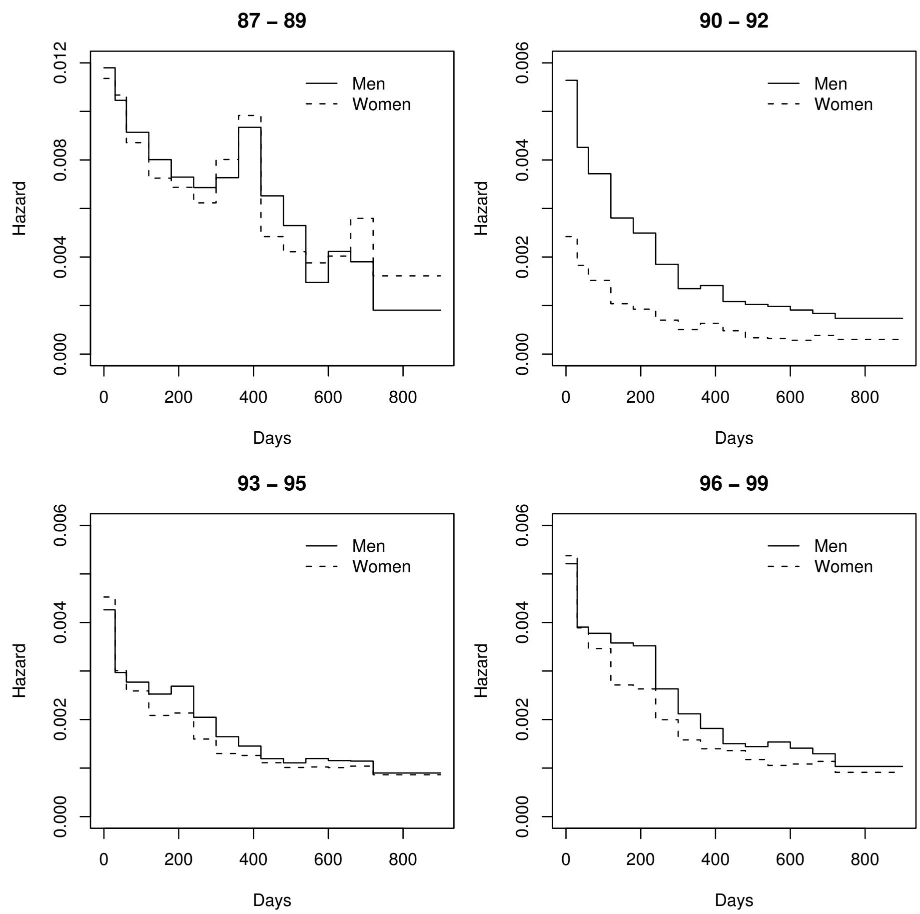

Figure 11: Estimated baseline hazards for men and women.

Table 8: Coefficients for men's model (continues in Table 10).

	88-89		90–92		93–95		96–99	
	Coef	S.E.	Coef	S.E.	Coef	S.E.	Coef	S.E.
(Intercept)	4.71	0.081	5.094	0.045	5.863	0.041	5.484	0.03
piece2	0.121	0.02	0.281	0.016	0.361	0.017	0.288	0.015
piece3	0.255	0.02	0.417	0.015	0.43	0.016	0.322	0.014
piece4	0.387	0.027	0.699	0.019	0.523	0.017	0.376	0.016
piece5	0.481	0.036	0.816	0.022	0.461	0.019	0.392	0.018
piece6	0.542	0.049	1.115	0.028	0.733	0.023	0.683	0.023
piece7	0.484	0.064	1.432	0.034	0.951	0.027	0.9	0.029
piece8	0.233	0.081	1.386	0.039	1.076	0.032	1.053	0.035
piece9	0.593	0.128	1.652	0.05	1.272	0.039	1.243	0.043
piece10	0.801	0.18	1.707	0.056	1.348	0.044	1.284	0.048
piece11	1.386	0.317	1.748	0.062	1.27	0.047	1.22	0.053
piece12	1.026	0.354	1.826	0.069	1.306	0.053	1.307	0.062
piece13	1.132	0.448	1.907	0.079	1.315	0.06	1.392	0.072
piece14	1.875	0.409	2.034	0.048	1.557	0.037	1.617	0.048
age (24,29]	0.198	0.023	-0.008	0.016	-0.062	0.016	0.057	0.015
age (29,34]	0.317	0.024	0.038	0.017	0.011	0.017	0.143	0.016
age (34,39]	0.358	0.025	0.119	0.017	0.08	0.018	0.221	0.017
age (39,44]	0.398	0.027	0.103	0.018	0.115	0.018	0.269	0.017
age (44,49]	0.494	0.033	0.201	0.021	0.18	0.019	0.309	0.017
education sec1	-0.204	0.036	-0.172	0.025	-0.153	0.024	-0.072	0.022
education sec2	-0.111	0.017	-0.118	0.012	-0.138	0.012	-0.163	0.011
education tert1	-0.049	0.071	-0.182	0.036	-0.265	0.03	-0.315	0.029
education tert2	0.097	0.068	-0.13	0.041	-0.292	0.031	-0.288	0.03
occupation tech spec	-0.075	0.045	-0.049	0.03	-0.388	0.028	-0.239	0.027
occupation other spec	0.007	0.057	-0.339	0.041	-0.524	0.037	-0.174	0.035
occupation health	-0.266	0.096	-0.542	0.055	-0.628	0.041	-0.334	0.035
occupation administ	0.125	0.055	0.033	0.036	-0.192	0.032	0.033	0.03
occupation sales	-0.038	0.048	-0.153	0.032	-0.316	0.031	-0.036	0.03
occupation aggricult	-0.37	0.038	-0.398	0.029	-0.615	0.03	-0.377	0.027
occupation industrial	-0.26	0.028	-0.167	0.023	-0.448	0.023	-0.237	0.02
occupation transition	-0.29	0.038	-0.344	0.028	-0.561	0.028	-0.402	0.026
occupation construct	-0.427	0.03	-0.224	0.023	-0.596	0.023	-0.551	0.021
occupation service	-0.206	0.048	-0.256	0.035	-0.359	0.032	-0.143	0.028
family married	-0.169	0.039	-0.275	0.025	-0.263	0.024	-0.298	0.023
family married & child	-0.21	0.018	-0.203	0.012	-0.27	0.012	-0.253	0.011
family unmarried & child	-0.143	0.037	-0.025	0.023	-0.187	0.022	-0.218	0.019
family single parent	-0.026	0.025	0.046	0.021	0.022	0.021	0.018	0.019
language Swedish	0.097	0.055	-0.1	0.031	-0.183	0.027	-0.177	0.028
language other	-0.113	0.108	0.071	0.059	0.37	0.05	0.5	0.04
area type semi urb	-0.053	0.022	-0.109	0.015	-0.073	0.014	-0.114	0.013
area type rural	-0.066	0.019	-0.056	0.013	-0.056	0.013	-0.072	0.012
disaibility	0.479	0.035	0.392	0.03	0.623	0.033	0.519	0.03
ue history (0,30]	-0.177	0.025	-0.201	0.017	-0.376	0.02	-0.275	0.018
ue history (30,180]	0.034	0.018	0.05	0.012	-0.139	0.013	-0.036	0.012
ue history (180,365]	0.259	0.028	0.339	0.02	0.186	0.015	0.427	0.014
repeated ue	-0.159	0.043	-0.25	0.032	-0.447	0.029	-0.323	0.027
prev state subs empl	0.686	0.033	1.22	0.027	1.096	0.02	0.941	0.019
prev state training	0.234	0.042	0.598	0.032	0.618	0.024	0.645	0.02
prev state work	0.191	0.021	0.51	0.016	0.306	0.015	0.289	0.014

Table 9: Coefficients for women's model (continues in Table 11).

	88-89		90–92		93–95		96–99	
	Coef	S.E.	Coef	S.E.	Coef	S.E.	Coef	S.E.
(Intercept)	4.41	0.1	4.682	0.056	5.529	0.048	5.462	0.033
piece2	0.062	0.023	0.281	0.018	0.408	0.018	0.323	0.015
piece3	0.265	0.024	0.466	0.018	0.558	0.017	0.44	0.014
piece4	0.449	0.032	0.847	0.024	0.775	0.02	0.684	0.018
piece5	0.502	0.042	0.961	0.029	0.751	0.022	0.714	0.02
piece6	0.6	0.058	1.241	0.036	1.041	0.027	0.991	0.026
piece7	0.348	0.07	1.568	0.045	1.247	0.033	1.224	0.033
piece8	0.144	0.099	1.34	0.051	1.278	0.038	1.347	0.04
piece9	0.853	0.197	1.617	0.069	1.405	0.046	1.374	0.048
piece10	0.991	0.268	1.976	0.091	1.497	0.055	1.519	0.06
piece11	1.106	0.354	2.025	0.102	1.487	0.063	1.628	0.074
piece12	1.034	0.409	2.14	0.117	1.5	0.072	1.602	0.084
piece13	0.708	0.448	1.844	0.112	1.47	0.081	1.552	0.094
piece14	1.26	0.379	2.086	0.071	1.658	0.052	1.773	0.066
age (24,29]	0.238	0.026	0.154	0.019	0.154	0.018	0.113	0.016
age (29,34]	0.255	0.028	0.212	0.021	0.166	0.019	0.182	0.017
age (34,39]	0.26	0.029	0.194	0.021	0.179	0.02	0.169	0.018
age (39,44]	0.251	0.032	0.228	0.022	0.123	0.021	0.154	0.018
age (44,49]	0.314	0.036	0.202	0.025	0.194	0.023	0.178	0.019
education sec1	-0.198	0.036	-0.223	0.025	-0.219	0.024	-0.25	0.022
education sec2	-0.086	0.02	-0.163	0.015	-0.165	0.015	-0.216	0.014
education tert1	0.034	0.078	-0.361	0.041	-0.356	0.025	-0.359	0.022
education tert2	0.001	0.061	-0.232	0.039	-0.386	0.029	-0.444	0.025
occupation tech spec	-0.121	0.064	-0.03	0.044	-0.294	0.039	-0.196	0.037
occupation other spec	-0.262	0.052	-0.486	0.036	-0.739	0.032	-0.503	0.029
occupation health	-0.549	0.034	-0.629	0.026	-0.744	0.025	-0.559	0.023
occupation administ	-0.283	0.033	-0.171	0.026	-0.317	0.026	-0.174	0.025
occupation sales	-0.281	0.04	-0.272	0.03	-0.421	0.029	-0.263	0.028
occupation aggricult	-0.234	0.059	-0.355	0.044	-0.624	0.043	-0.38	0.037
occupation industrial	-0.226	0.036	-0.104	0.029	-0.314	0.03	-0.155	0.027
occupation transition	-0.272	0.094	-0.188	0.069	-0.428	0.063	-0.337	0.06
occupation construct	-0.274	0.144	-0.058	0.099	-0.355	0.1	-0.35	0.081
occupation service	-0.386	0.031	-0.373	0.025	-0.498	0.026	-0.344	0.024
family married	0.08	0.04	0.057	0.028	0.006	0.024	-0.011	0.022
family married & child	0.014	0.023	0.05	0.016	0.088	0.014	0.014	0.013
family unmarried & child	0.181	0.043	0.234	0.026	0.378	0.028	0.245	0.022
family single parent	0.067	0.028	0.176	0.023	0.244	0.021	0.223	0.017
language Swedish	0.091	0.061	-0.083	0.038	-0.148	0.029	-0.138	0.026
language other	0.24	0.117	0.299	0.074	0.413	0.064	0.578	0.047
area type semi-urb	-0.055	0.025	-0.075	0.018	-0.052	0.016	-0.075	0.014
area type rural	-0.062	0.021	-0.067	0.016	-0.072	0.015	-0.09	0.013
disability	0.538	0.037	0.36	0.028	0.56	0.032	0.557	0.029
ue history (0,30]	-0.278	0.029	-0.317	0.02	-0.486	0.021	-0.488	0.017
ue history (30,180]	0.002	0.021	-0.049	0.016	-0.193	0.014	-0.159	0.012
ue history (180,365]	0.147	0.036	0.206	0.029	0.101	0.018	0.26	0.016
repeated	-0.3	0.05	-0.352	0.039	-0.507	0.034	-0.431	0.029
prev state subs empl	0.553	0.034	0.901	0.031	0.939	0.023	0.665	0.017
prev state train	0.014	0.047	0.305	0.034	0.576	0.029	0.592	0.021
prev state work	0.101	0.021	0.243	0.015	0.204	0.015	0.167	0.013

Table 10: Coefficients for men's model (continued).

	88-89		90–92		93–95		96–99	
	Coef	S.E.	Coef	S.E.	Coef	S.E.	Coef	S.E.
region Turku	0.038	0.04	-0.156	0.022	-0.142	0.023	-0.12	0.024
region Satakunta	0.268	0.055	0.008	0.027	-0.165	0.038	-0.22	0.038
region Hame	0.187	0.044	-0.076	0.022	-0.098	0.034	-0.161	0.033
region Kymi	0.158	0.051	-0.135	0.026	-0.189	0.035	-0.177	0.038
region Mikkeli	0.325	0.055	-0.091	0.031	-0.096	0.039	-0.158	0.041
region Kuopio	0.291	0.055	-0.178	0.028	-0.065	0.04	-0.119	0.042
region P-Karjala	0.388	0.062	-0.18	0.034	-0.089	0.046	-0.107	0.048
region K-Suomi	0.324	0.058	-0.129	0.029	-0.03	0.041	-0.128	0.046
region Vaasa	0.288	0.046	-0.105	0.024	-0.107	0.026	-0.14	0.029
region Oulu	0.197	0.053	-0.233	0.026	-0.179	0.037	-0.191	0.043
region Kainuu	0.34	0.062	-0.191	0.039	0.019	0.055	-0.106	0.058
region Lappi	0.203	0.057	-0.142	0.032	-0.069	0.05	-0.123	0.056
quarter II	-0.411	0.021	-0.332	0.014	-0.248	0.014	-0.328	0.013
quarter III	-0.347	0.022	-0.159	0.014	-0.04	0.014	-0.114	0.013
quarter IV	0.203	0.023	0.391	0.016	0.275	0.015	0.298	0.014
year 2	-0.1	0.017	0.395	0.019	-0.078	0.012	-0.1	0.015
year 3			0.492	0.03	-0.126	0.013	-0.056	0.018
year 4							-0.107	0.021
regional ur	0.091	0.027	0.011	0.003	0.046	0.013	0.042	0.006
regional ur^2	0.008	0.003	-0.003	0	-0.002	0.001	-0.003	0

Table 11: Coefficients for women's model (continued).

	88-89		90–92		93–95		96–99	
	Coef	S.E.	Coef	S.E.	Coef	S.E.	Coef	S.E.
region Turku	0.072	0.049	-0.121	0.027	-0.181	0.026	-0.123	0.024
region Satakunta	0.295	0.064	0.043	0.032	-0.101	0.045	-0.149	0.041
region Hame	0.124	0.052	-0.084	0.027	-0.134	0.039	-0.126	0.034
region Kymi	0.245	0.06	-0.117	0.032	-0.118	0.04	-0.094	0.04
region Mikkeli	0.191	0.065	-0.105	0.038	-0.078	0.045	-0.088	0.044
region Kuopio	0.193	0.067	-0.091	0.035	-0.081	0.047	-0.07	0.046
region P-Karjala	0.355	0.074	-0.113	0.043	-0.029	0.055	0.06	0.052
region K-Suomi	0.235	0.067	-0.103	0.035	-0.144	0.047	-0.076	0.049
region Vaasa	0.285	0.054	-0.011	0.029	-0.053	0.029	-0.005	0.03
region Oulu	0.269	0.064	-0.168	0.034	-0.145	0.044	-0.114	0.046
region Kainuu	0.218	0.077	-0.052	0.053	0.085	0.066	0.033	0.065
region Lappi	0.136	0.069	-0.142	0.04	-0.114	0.06	-0.047	0.062
quarter II	-0.091	0.025	-0.037	0.018	0.013	0.017	0.005	0.016
quarter III	-0.051	0.025	-0.099	0.017	-0.042	0.016	-0.113	0.014
quarter IV	0.281	0.026	0.313	0.019	0.108	0.017	0.067	0.015
year 2	-0.157	0.019	0.453	0.025	-0.045	0.014	-0.033	0.017
year 3			0.714	0.039	-0.07	0.016	-0.007	0.02
year 4							-0.059	0.024
regional ur	0.014	0.034	0.021	0.004	0.061	0.015	0.058	0.007
regional ur^2	0.002	0.003	0	0	-0.003	0.001	-0.004	0

Chapter V

Workplace accidents and business cycle: Evidence from Swedish health registers*

Abstract

In this paper, we study the effect of business cycle on the incidence of workplace accidents. We link individual data covering Swedish inhospital care 1997–2005 to the population database. These data allow us to study if changes in the composition of workers or strategic worker behaviour are driving the cyclicality of accidents. Our results show that the incidence of workplace accidents increases during economic upturns but only in specific subgroups. We find some evidence that compositional changes in labour force may contribute to cyclicality for women. In the male population, on the other hand, only the less severe accidents are cyclical which would be consistent with strategic worker behaviour.

*Joint work with Per Johansson and Tuomas Pekkarinen

1 Introduction

Workplace accidents are an important and poorly understood phenomenon. The International Labour Organization (ILO) estimates that globally approximately 2.2 million people die annually from work-related accidents and diseases. Furthermore, there are an estimated 270 million non-fatal workplace accidents every year. The economic costs of these accidents are considerable. According to the ILO, they amount to 4 % of the world's domestic product.

These global averages naturally mask considerable differences across countries and in time. The incidence of workplace accidents is typically lower in industrialised countries and many countries, especially in the European Union, have managed to steadily decrease the incidence rate since the early 1990's. However, the reasons for cross-country differences and time variation in workplace accidents are still an open question.

This paper studies the effect of business cycles on the incidence of workplace accidents. Recently Boone & van Ours (2006) have shown that workplace accidents are procyclical in the OECD countries. There are several competing explanations for the procyclicality of accident rates. First, the pace of work is often higher in booms and this can lead to hazardous working conditions and higher effort levels. In an influential paper Ruhm (2000) attributed the procyclicality of mortality to these effects. Second, economic fluctuations lead to changes in the composition of the workforce. If more accident-prone, inexperienced workers are hired during booms, this compositional variation may lead to procyclical variation in the incidence of accidents. Finally, Boone & van Ours (2006) attribute the procyclical variation to strategic reporting behaviour on behalf of the workers. If accidents increase the likelihood of being fired, workers will be more reluctant to report accidents during downturns, because the cost of job loss will be higher. These competing explanations have very different implications. According to the pace of work and composition explanations, the cyclical variation of accidents is genuine whereas according to the reporting explanations the incidence of actual accidents does not vary with the business cycle.

Our aim is to use data from Swedish patent registers to examine the explanatory power of these competing explanations for the procyclicality of workplace accidents. This is a unique data source that covers all the inhospital care spells in Sweden during 1997–2005. Furthermore, these data can be linked to register data on the demographics and labour market outcomes of the whole Swedish population. These data allow us to study the cyclical variation according to the type of accident, within different subgroups with varying attachment to the labour market, and by the timing of the accident. Hence, we can distinguish the genuine workplace accidents from accidents that take place out of work. We are also able to study the cyclical variation of workplace accidents among permanently and marginally employed individuals. This feature of the data makes it possible to disentangle whether the compositional changes in the workforce are driving the cyclical variation in accidents rates. Finally, we also study cyclical variation by the severity of the accidents. This enable us to assess whether the cyclicality is, at least partially, driven by strategic worker behaviour. If only less severe accidents follow a cyclical pattern, it suggests that workers may be reluctant to treat less severe accidents during a recession.

We regress the number of workplace accidents in Swedish provinces on the provincial unemployment rate controlling for provincial and annual fixed effects. This implies that we identify the effect of local unemployment on accidents using within-province variation across business cycles. Our results show that workplace accidents are procyclical in Sweden but only for some subgroups. For men, this cyclical variation is only present for those in stable employment with secondary degree. The composition effect does not seem to influence the male accidents rate. For women in non-stable employment, accidents are weakly procyclical which indicates that compositional variation may contribute to the cyclicality. We find procyclicality to be stronger in less severe accidents than in severe accidents among men. This suggests that the cyclicality of workplace accidents may be, at least partially, driven by strategic worker behaviour. For women, the cyclicality does not depend on the severity of accidents.

The paper is structured as follows. In the following section, we present an overview of the literature on the cyclical variation in health and accidents and discuss the different explanations for these variations. The third section presents the Swedish patient and population data and shows some descriptive evidence on the variation in accident rates. Then we discuss the econometric model that we use to study the cyclicality of the accidents and the fifth section presents the estimation results. The sixth section concludes.

2 Previous studies

There is a vast literature on the relationship of health and economic fluctuations. The earlier time series literature, such as the studies by Brenner (1973, 1975, 1979), pointed towards countercyclical variation in many health conditions and illnesses. Admissions to mental hospitals, infant mortality rates, and deaths due to various diseases seemed to decrease during economic booms. These results were called into question in the famous study by Ruhm (2000) that used cross-state variation in business cycles to identify the effect of local labour market conditions on mortality from different causes. Ruhm found that, apart from suicides, deaths from all causes increased during booms. This procyclical variation in mortality has been replicated with data from many other countries (Ruhm, 2006; Johansson, 2004; Gerdtham & Ruhm, 2006; Neumayer, 2004; Granados, 2005).

One of the most obvious channels linking economic fluctuations to health are workplace accidents. Why would workplace accidents then be procyclical? One of the mechanisms that Ruhm (2000) suggests is the fact that health is an input into the production of goods and services. The pace of work is increased and the number of job hours extended during economic expansions. This may lead to hazardous working conditions, increased levels of job-related stress, and higher effort levels. All these factors should contribute to higher incidence of workplace accidents. There is indeed some evidence of higher incidence of work-related accidents during economic expansions. Catalano (1979) shows that the incidence of disabling accidents in the Californian manufacturing industry increased during economic expansion and Brooker, Frank & Tarasuk (1997) show the same pattern in the case of acute back injuries in three industrial sectors in Ontario.

Yet, it is unclear whether this cyclical fluctuation really reflects the effect of the hazardous working conditions. Typically, the composition of the labour force varies with the business cycle. During booms, new workers are hired and if these workers are more accident-prone, potentially due to lack of experience, this change in the composition could alone explain the procyclical variation in the incidence of accidents. Booth, Francesconi & Frank (2002), for example, find that temporary employees in the UK are provided less on-the-job-training than permanent workers. If this training decreases the risk of accidents, then one would expect the incidence of accidents to increase with the share of temporary workers. Indeed, Guadalupe (2003) shows that in Spain the accidents rates of fixed term workers are higher than that of the permanent workers.

Altogether different possibility is that the procyclical variation of reported accidents simply reflects strategic behaviour on behalf of the workers. This is the mechanism that Boone & van Ours (2006) study in their theoretical model. If a reported accident is interpreted as a negative signal of the worker's productive ability, reporting an accident should increase the probability of being fired. Boone & van Ours assume that workers always have some discretion when it comes to reporting a minor accident. Since the cost of job loss is always higher during economic downturns, a rational worker should be less reluctant to report the accident when the unemployment is high. Thus, the procyclicality of accident rates may simply reflect reporting behaviour, while the incidence of actual accidents need not be cyclical at all. There is some evidence that this kind of strategic behaviour does play a role when it comes to absenteeism. Johansson & Palme (1996) have shown that work absence among Swedish blue-collar workers decreased when the cost of being absent increased and Askildsen, Bratberg & Nilsen (2005) show evidence on the procyclicality of absenteeism in Norway which also holds for the subsample of permanent workers. Boone & van Ours (2006) also point out that while they do find procyclicality in the incidence of all workplace accidents, they do not find cyclical variation in fatal accidents where reporting should not play a role.

While the previous literature has convincingly documented the procyclical nature of workplace accidents and showed that both the cyclical variation in the workforce as well as strategic reporting behaviour on behalf of the workers may contribute to this phenomenon, we have little evidence on the relative importance of these factors. The purpose of this analysis is to use unique Swedish data to examine how much of the cyclical variation in workplace accidents is due to changes in the composition of the workforce. While we are only able to study reported and treated workplace accidents, we can, nevertheless, separate the accidents by their severity. This allows to asses the role of strategic worker behaviour in the cyclicality of accidents. If only less severe accidents follow a cyclical pattern, then this would suggest that strategic worker behaviour can, at least partially, explain some of the observed cyclicality in workplace accidents.

3 Data

The analysis data are constructed by linking two data sources. The Inpatient Register of the National Board of Health and Welfare provides information on the accident rates. In principle, these data record all use of public institutional care since 1987 and since 1997 also non-institutional care is included in the data. The data contain information on the diagnosis, the type of injury, any operations that took place, dates of admission and dismissal and hospital identifiers. Since 1997, the data use ICD-10 (International Classification of Diseases) coding. The IFAU database is a panel that covers the entire Swedish working-age population from 1990 onwards. The data are register-based and contain a rich set of individual characteristics such as education and labour market outcomes.

The data used in this paper are constructed by merging information from these two datasets. For each year, the individual background characteristics are observed in the last November. Our analysis data covers the years 1997–2005. We exclude the earlier years because the ICD-10 codes are only available for these years. The data covers all Swedes between 25 and 64 years of age except individuals living in two southern provinces. Skåne is excluded because the ICD-10 coding was adopted there slower. Västra Götaland is excluded because two smaller provinces were merged into it in 1997 which seem to have changed practises in the registering of accidents.

3.1 Analysis populations

We define several different analysis populations using the information available in the IFAU database. First, males and females are analysed separately, since business cycles may have different effects on accident rates by gender. Second, we distinguish between individuals who are in stable employment and those who are in non-stable employment. The idea here is to define a population that would

Table 1: Characteristics of populations (%).

	All	Stable	Non-stable
Age			
25–34	25.69	12.78	21.14
35–44	25.96	29.04	28.17
45–54	26.26	36.04	24.01
55–64	22.09	22.14	26.69
Education			
basic	21.99	17.89	26.62
secondary	47.85	46.21	49.42
tertiary	30.16	35.90	23.96
N obs. / year	3,306,435	1,280,665	1,658,289

be free of compositional variation across the business cycle. Finally, we split the data by education levels to focus on workers in different tasks.

Our main analysis population covers all working-age individuals except those below 25 who are often still studying. This population is adjusted annually by including those who turn 25 and excluding those who turn 65, die or emigrate. The number of individuals grows from 3.23 million in 1997 to 3.35 million in 2005.

We define stable employment by including only individuals whose earnings are above a threshold level and who experience no unemployment in the analysis period. No entry is allowed which means that the initial population observed in 1997 ages and is affected by cohort effects. The exclusion criteria are that individuals (a) receive unemployment benefits (b) receive subsidy for studying (c) have taxable annual earnings below 50,000 SEK (in 2005 value). If any of these criteria is fulfilled in any year, individuals are defined to be in the non-stable employment group.

These three analysis populations are further divided to subgroups by their level of education. The basic education is 9 years in the compulsory school. Secondary education is 2–3 years of high school which can be either vocational or aim to academic schooling. Tertiary education is anything above high school. Table 1 presents the age and education distributions in the populations. Individuals in the stable employment are older and better educated on average than individuals in non-stable employment.

3.2 Outcomes

Accidents are observed when individuals are treated in inhospital care. In practise, the data cover only more severe accidents as minor accidents can typically be treated in out-patient care. We focus on the ICD-10 chapter W accidents, shown

Table 2: ICD-10 codes in chapter W (other external causes of accidental injury).

ICD-10	Description
W00–W19	Falls
W20–W49	Exposure to inanimate mechanical forces
W50–W64	Exposure to animate mechanical forces
W65–W74	Accidental drowning and submersion
W75–W84	Other accidental threats to breathing
W85–W99	Exposure to electric current, radiation and extreme ambient air temperature and pressure

in Table 2. The excluded accidents are in chapter V including traffic injuries and in chapter X including e.g. fire, heat, cold or poisoning related accidents.

Our outcome of interest is the total number of inhospitalisations per year. Since we want to distinguish between work-related and leisure accidents, we use the common holiday season in Sweden as proxy for leisure accidents. Thus we count the number of inhospitalisations where the admission date is outside the summer holiday period and where the admission date is in July. In addition, we utilise the information on the length of treatment as an indicator for the severity of accidents. In the analysis data, 63% of men's and 57% of women's inhospitalisations last less than three days. We use over two days care as a cutoff-value for severe accidents.

Figure 1 presents the annual accident rates by age for males and females. Accidents leading to inhospitalisation are relatively rare, 1000 individuals experience annually 2–10 inhospitalisations. Males have higher accident rate than females, especially at young age. Accident rates increase with age and the change is larger for females.

4 Model

Our empirical model specifies a relationship between the annual number of inhospitalisations due to accidents and the regional unemployment rate in a flexible way. To account for trends and changes in accident rates arising from improved workplace safety, more efficient treatment practises and variation in other environmental factors, we control for annual fixed effects. We also include province fixed effects to capture regional differences that affect accident rates. Within province variation in the unemployment rate is used to identify the parameter of interest.

We estimate the model for several different outcomes that are count variables. Count data takes only discrete non-negative values and are conveniently modelled using Poisson regression model. As our key covariate varies only on provincial level, we aggregate the number of accidents on that level. However, the acci-

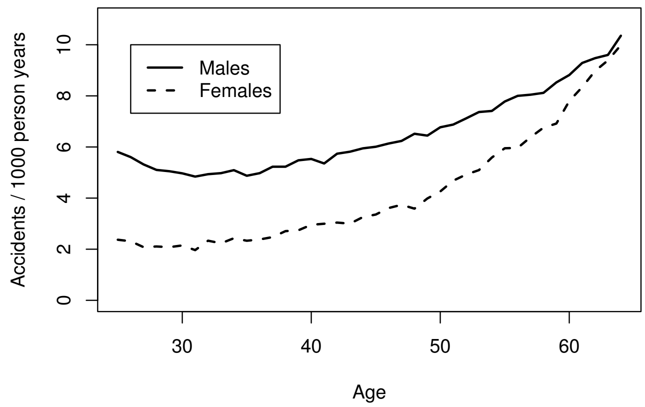

Figure 1: Accident rates for males and females.

dent rates vary noticeable by age and gender. We wish to utilise this variation and, therefore, we compute age, gender and province specific annual number of accidents. Separate models are estimated for genders to study the differences in behaviour.

Denote the number of accidents by Y_{jkt} where $j = 25, \ldots, 64$ is a subscript for age, $k = 1, \ldots, 19$ for province and $t = 1997, \ldots, 2005$ for year. The Poisson regression model is specified as

$$\mathrm{E}(Y_{jkt}) = \exp(age_j + province_k + year_t + UE_{kt}\beta),$$

where overdispersion is allowed, i.e. $\mathrm{Var}(Y_i) = \tau\mathrm{E}(Y_i)$. Covariates age, $province$ and $year$ are dummy variables. The only continuous variable UE is the level of unemployment rate. Poisson model is log-linear which means that β can be interpreted as semi-elasticity between the unemployment rate and the number of accidents.

The model is estimated for several outcomes and sub-groups. We distinguish between the time accidents occur and the different lengths of treatment. The holiday season in July is used as a proxy for leisure accidents. If we find cyclicality in accidents that take place outside summer holiday season but not in accidents taking place in July, it suggests that the cyclicality in accidents comes from workplace accidents. The length of hospital treatment is a proxy for the severity of accidents. Theoretical model by Boone & van Ours (2006) predicts that if reporting accidents affects the reputation of workers, minor accidents should be more cyclical than severe accidents. While we are naturally only dealing reported accidents in this study, studying the cyclical patterns of accidents by their severity can give some evidence on the role of this kind of strategic behaviour. If only less severe accidents follow a cyclical pattern, the strategic behaviour on behalf of the workers is likely to contribute to the observed cyclical pattern. In what follows, we define accidents to be severe if hospitalisation is longer than two days.

The main results are estimated for the Swedish population between 25 and 64 years of age. We also defined a population of individuals in stable employment and non-stable employment. Individuals in stable employment have earnings every year and no unemployment during the analysis period. Thus, they have a strong attachment to the labour market. Individuals in non-stable employment have either very low earnings or unemployment in some year and they present a group whose labour force attachment is more sensitive to the business cycle.

According to the composition hypothesis, cyclical variation in workplace accidents is caused by the entry of more accident prone individuals into the workforce during good times. It is reasonable to assume that individuals in non-stable employment are on average less experienced workers. Therefore, if we find stronger cyclicality in the non-stable group than in the stable group, it provides support for the composition hypothesis.

Finally, we split the analysis populations by their level of education. The level of education is strongly related to the occupational distribution and the skill level of workers. The stable employment group is on average better educated than the non-stable group. Estimating the model by the education level allows us to study if less educated individuals, who are more often blue collar workers, are more sensitive to the business cycle. In addition, it reveals whether potential differences between analysis populations remain after conditioning on the level of education.

5 Results

Tables 3 and 4 report the main results of the paper. Each reported coefficient refers to a separate model where accidents of the corresponding analysis population are regressed on the local unemployment rate and a full set of regional, time, and age dummies. As can be seen from the results on the first row of Table 3, the accident rate in the whole male analysis population does not show signs of cyclicality. The effect of unemployment on accidents is insignificantly different from zero. The result remains similar when we focus only on working months. Also decomposing the male population by employment status does not reveal significant differences, although the effect of unemployment on accidents is more negative for men in stable employment.[1] The analysis by education levels reveals that this

[1]Note that the decomposition is done for individuals observed in 1997 and the stable and non-stable groups are affected by cohort effects. Therefore estimates between the decomposed groups and the main analysis population are not directly comparable.

Table 3: Results for male populations by the time of occurrence.

	All			Working months			July		
	Coef	S.E.	p-val	Coef	S.E.	p-val	Coef	S.E.	p-val
All	0.65	0.81	0.42	0.78	0.83	0.35	−0.89	2.52	0.72
basic	1.20	1.41	0.39	1.19	1.43	0.41	1.55	4.47	0.73
secondary	−0.32	1.10	0.77	−0.04	1.14	0.97	−3.62	3.51	0.30
tertiary	0.51	1.79	0.77	0.25	1.86	0.89	3.32	5.80	0.57
Stable	−1.36	1.40	0.33	−1.39	1.45	0.34	−1.11	4.50	0.81
basic	3.47	2.73	0.20	3.71	2.83	0.19	1.13	9.06	0.90
secondary	−3.88	1.94	0.05	−3.65	2.02	0.07	−6.74	6.39	0.29
tertiary	−2.21	2.61	0.40	−3.03	2.72	0.27	6.58	8.31	0.43
Non-stable	−0.89	1.06	0.41	−0.66	1.08	0.54	−3.37	3.29	0.31
basic	−0.06	1.69	0.97	−0.15	1.70	0.93	1.08	5.36	0.84
secondary	−1.73	1.42	0.22	−1.50	1.47	0.31	−4.27	4.51	0.34
tertiary	−1.15	2.75	0.68	−0.43	2.85	0.88	−9.54	8.83	0.28

Table 4: Results for female populations by the time of occurrence.

	All			Working months			July		
	Coef	S.E.	p-val	Coef	S.E.	p-val	Coef	S.E.	p-val
All	0.33	0.95	0.73	0.37	0.99	0.71	−0.11	3.12	0.97
basic	0.12	1.76	0.94	0.08	1.80	0.96	0.72	5.84	0.90
secondary	0.43	1.37	0.75	0.41	1.42	0.77	0.87	4.41	0.84
tertiary	−2.48	1.90	0.19	−2.23	1.96	0.26	−5.53	6.69	0.41
Stable	−2.21	1.87	0.24	−1.94	1.93	0.31	−5.39	6.27	0.39
basic	−5.59	4.39	0.20	−6.50	4.55	0.15	4.74	12.82	0.71
secondary	−2.13	2.71	0.43	−1.51	2.80	0.59	−9.99	9.65	0.30
tertiary	−1.13	2.89	0.70	−0.68	3.00	0.82	−6.66	10.05	0.51
Non-stable	−2.06	1.18	0.08	−2.22	1.22	0.07	−0.07	3.73	0.99
basic	−1.28	1.96	0.51	−1.27	2.00	0.53	−1.33	6.44	0.84
secondary	−0.22	1.61	0.89	−0.52	1.68	0.76	3.20	5.15	0.53
tertiary	−5.87	2.68	0.03	−6.24	2.76	0.02	−1.40	9.34	0.88

Table 5: Results for male populations by treatment length.

	0–2 days			Over 2 days		
	Coef	S.E.	p-val	Coef	S.E.	p-val
All	0.21	0.92	0.82	1.43	1.25	0.25
basic	0.93	1.70	0.58	1.61	2.04	0.43
secondary	−1.60	1.22	0.19	2.01	1.80	0.26
tertiary	2.00	2.02	0.32	−2.59	3.10	0.40
Stable	−2.56	1.54	0.10	1.21	2.39	0.61
Non-stable	−1.42	1.25	0.26	−0.15	1.53	0.92

Table 6: Results for female populations by treatment length.

	0–2 days			Over 2 days		
	Coef	S.E.	p-val	Coef	S.E.	p-val
All	0.91	1.16	0.43	−0.48	1.42	0.73
basic	1.74	2.28	0.45	−1.55	2.44	0.53
secondary	0.24	1.65	0.88	0.71	2.08	0.73
tertiary	−1.30	2.25	0.56	−4.64	3.07	0.13
Stable	−1.40	2.21	0.53	−3.47	2.97	0.24
Non-stable	−1.84	1.48	0.21	−2.40	1.65	0.15

procyclicality is a phenomenon limited to male workers with a secondary degree. The point estimate shows that one percentage point increase in the unemployment rate implies 3.88% decrease in accidents. The result remains similar for accidents that take place during working months.[2] On the other hand, among the male workers in non-stable employment, there is no similar sign of cyclicality. Hence, the composition effect does not seem to influence the male accidents rate.

Table 4 reports the analogous results for women. The accident rate shows no cyclicality in the whole population of female workers. However, the accident rate of women in non-stable employment is weakly procyclical, also when focusing on working months. Interestingly, this cyclicality seems to be the strongest among women with tertiary education. Based on these results, it seems that compositional variation may contribute to procyclicality in the female workforce.

Does the cyclical pattern of workplace accidents vary by the severity of the accidents? If only less severe accidents follow a cyclical pattern, this would suggest that workers may simply be reluctant to have less severe accidents treated during a recession. In Tables 5–6, we report the effect of the local unemployment rate on accidents by their treatment length. We classify accidents as less severe if they require less than two days of treatment and as severe if the treatment takes longer.

The results in Table 5 indicate that the procyclicality of the accident rates

[2]The estimates for accidents occurring in July suffer from relative small sample sizes (i.e. low number of events, see the Appendix), especially for the smaller populations.

of men in stable employment is weakly present for less severe accidents. Severe accidents do not show any sign of cyclicality in any male subgroup. The decomposition by education group reveals no significant differences. Taken at face value, these results would suggest that at least some part of the procyclicality of the male accident rates could be due to strategic behaviour on behalf of workers and may not reflect genuine changes in accidents.

For women the story is very different. In Table 6, we do not observe any differences in the cyclical pattern of severe and less-severe accidents. This would suggest that the procyclicality of the female accident rates does reflect real changes in the actual accidents.

6 Conclusions

Workplace accidents are an important phenomenon in the labour market and they also imply significant social costs. The incidence of workplace accidents varies considerably across countries and in time. Recent research has also suggested that workplace accidents are procyclical: the incidence of accidents increases during economic booms and decreases during downturns. Various explanations have been suggested for this procyclical pattern. Some authors, inspired by Ruhm (2000), argue that faster pace of work during booms and compositional changes in the labour force are the sources of cyclical variations in the accident rate. These explanations imply genuine cyclical changes in the accident rate. On the other hand, Boone & van Ours (2006) have recently suggested that strategic reporting behaviour on the behalf of workers can explain the cyclical pattern. According to this explanation, the workers are simply more reluctant to report any accidents during recessions even if they take place with the same probability. Thus, there is only variation in the reported accidents, not in the actual accidents.

This paper is an attempt to study the cyclical variation in the incidence of workplace accidents in Sweden and its reasons using data from a unique source that cover all the inhospital care spells in the country. We regress the number of workplace accidents at the provincial level on the provincial unemployment controlling for annual and provincial fixed effects. We find some evidence of procyclical variation of workplace accidents in Sweden but only for specific subgroups. For these subgroups, the incidence of accidents increases during economic upturns.

For men, there is only evidence of cyclicality among those in stable employment with secondary degree. Compositional changes do not seem to be the source of cyclicality in the variation of accident rates in the male population. Among women procyclicality is weakly present only for those in non-stable employment. This indicates that compositional changes in the labour force may play some role in the accident rates of women.

When it comes to strategic behaviour, we show that only the less severe accidents increase weakly in economic booms in the male population with stable employment. We find no evidence for the procyclicality of severe accidents among men. This result can be interpreted as support for strategic behaviour. However, it is unclear how big a part of the cyclical variation is explained by this kind of behaviour. Further, in the female population, the incidence of less severe accidents is not cyclical. Thus, it seems that the results for males are partly consistent with the strategic reporting theory but it is unlikely to be the sole explanation.

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Appendix

Table 7: Swedish provinces.

Region	Name
01	Stockholms län
03	Uppsala län
04	Södermanlands län
05	Östergötlands län
06	Jönköpings län
07	Kronobergs län
08	Kalmar län
09	Gotlands län
10	Blekinge län
12	Skåne län
13	Hallands län
14	Västra Götalands län
17	Värmlands län
18	Örebro län
19	Västmanlands län
20	Dalarnas län
21	Gävleborgs län
22	Västernorrlands län
23	Jämtlands län
25	Norrbottens län

Table 8: Average annual number of accidents by treatment length and the time of occurrence.

	All	Working months	July
Males			
Any	10735	9879	856
0–2	6748	6185	563
over 2	3987	3694	293
mean, days	3.97	4.01	3.52
Females			
Any	6650	6130	520
0–2	3773	3467	306
over 2	2877	2663	214
mean, days	4.12	4.15	3.83

Note: mean = the total time in treatment divided by the total number of visits.

Table 9: Different model specifications.

	a	b	c	d
Males				
Coef.	−9.31	0.19	0.65	0.65
S.E.	0.73	0.35	0.90	0.81
p-value	0.00	0.59	0.47	0.42
Females				
Coef.	−10.81	0.96	0.33	0.33
S.E.	0.92	0.56	1.44	0.95
p-value	0.00	0.09	0.82	0.73
Controls				
Region	N	Y	Y	Y
Year	N	N	Y	Y
Age	N	N	N	Y