

Fredriksson, Peter; Söderström, Martin

Working Paper

Do unemployment benefits increase unemployment? New evidence on an old question

Working Paper, No. 2008:15

Provided in Cooperation with:

IFAU - Institute for Evaluation of Labour Market and Education Policy, Uppsala

Suggested Citation: Fredriksson, Peter; Söderström, Martin (2008) : Do unemployment benefits increase unemployment? New evidence on an old question, Working Paper, No. 2008:15, Institute for Labour Market Policy Evaluation (IFAU), Uppsala

This Version is available at:

<https://hdl.handle.net/10419/45713>

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.



IFAU – INSTITUTE FOR
LABOUR MARKET POLICY
EVALUATION

Do unemployment benefits increase unemployment? New evidence on an old question

Peter Fredriksson
Martin Söderström

WORKING PAPER 2008:15

The Institute for Labour Market Policy Evaluation (IFAU) is a research institute under the Swedish Ministry of Employment, situated in Uppsala. IFAU's objective is to promote, support and carry out scientific evaluations. The assignment includes: the effects of labour market policies, studies of the functioning of the labour market, the labour market effects of educational policies and the labour market effects of social insurance policies. IFAU shall also disseminate its results so that they become accessible to different interested parties in Sweden and abroad.

IFAU also provides funding for research projects within its areas of interest. The deadline for applications is October 1 each year. Since the researchers at IFAU are mainly economists, researchers from other disciplines are encouraged to apply for funding.

IFAU is run by a Director-General. The institute has a scientific council, consisting of a chairman, the Director-General and five other members. Among other things, the scientific council proposes a decision for the allocation of research grants. A reference group including representatives for employer organizations and trade unions, as well as the ministries and authorities concerned is also connected to the institute.

Postal address: P.O. Box 513, 751 20 Uppsala

Visiting address: Kyrkogårdsgatan 6, Uppsala

Phone: +46 18 471 70 70

Fax: +46 18 471 70 71

ifau@ifau.uu.se

www.ifau.se

Papers published in the Working Paper Series should, according to the IFAU policy, have been discussed at seminars held at IFAU and at least one other academic forum, and have been read by one external and one internal referee. They need not, however, have undergone the standard scrutiny for publication in a scientific journal. The purpose of the Working Paper Series is to provide a factual basis for public policy and the public policy discussion.

ISSN 1651-1166

Do unemployment benefits increase unemployment? New evidence on an old question[#]

by

Peter Fredriksson^{*} and Martin Söderström^{**}

May 20, 2008

Abstract

We examine the relationship between unemployment benefits and unemployment using Swedish regional data. To estimate the effect of an increase in unemployment insurance (UI) on unemployment we exploit the ceiling on UI benefits. The benefit ceiling, coupled with the fact that there are regional wage differentials, implies that the generosity of UI varies regionally. More importantly, the *actual generosity of UI* varies within region over time due to variations in the benefit ceiling. We find fairly robust evidence suggesting that the actual generosity of UI does matter for regional unemployment. Increases in the actual replacement rate contribute to higher unemployment as suggested by theory. We also show that removing the wage cap in UI benefit receipt would reduce the dispersion of regional unemployment. This result is due to the fact that low unemployment regions tend to be high wage regions where the benefit ceiling has a greater bite. Removing the benefit ceiling thus implies that the actual generosity of UI increases more in low unemployment regions.

Keywords: Unemployment, Unemployment insurance, Unemployment dispersion
JEL-codes: J64, J65

[#] We thank Bertil Holmlund, Eva Mörk, Oskar Nordström Skans, Peter Skogman Thoursie, as well as seminar participants at the University of Stockholm, Uppsala University, and IAB (Nuremberg, Germany) for very helpful comments. Financial support from the Swedish council for Working Life and Social Research is gratefully acknowledged.

^{*} Institute for Labour Market Policy Evaluation (IFAU) and Department of Economics, Uppsala University. Address for correspondence: IFAU, P.O. Box 513, SE-751 20 Uppsala, Sweden. E-mail: peter.fredriksson@ifau.uu.se.

^{**} Ministry of Finance, Government of Sweden. The paper was written while the author was working at the Department of Economics, Uppsala University.

Table of contents

1	Introduction	3
2	A sketch of the identification strategy.....	6
3	The Swedish institutional setting	8
3.1	Unemployment benefits in Sweden.....	9
4	Data and construction of key variables	12
4.1	Construction of key independent variables	12
4.2	Supplementary unemployment benefits	14
4.3	Data	15
5	The variation in the actual replacement rate	19
6	The effect of the actual replacement rate	22
7	Policy interventions.....	26
8	Conclusions	30
	References	31
	Appendix A: Creating regional panel data	33
	Appendix B: Coefficient estimates.....	38

1 Introduction

Whether the provision and generosity of unemployment insurance (UI) increase unemployment has been the subject of much research.¹ Theory generally predicts that unemployment will rise in response to an increase in UI generosity. However, the empirical evidence is not as unequivocal as the theory suggests.

There are a number of studies using micro data to identify the effects of UI generosity for those already unemployed; Meyer (1995) surveys the most convincing experimental evidence. But the provision of UI affects other margins than individual unemployment duration. In addition to affecting search behavior, UI may affect, e.g., wage-setting and quitting behavior. In other words, we are most interested in the general equilibrium effects of variations in the generosity of UI.

Aggregate time series data have the potential of capturing general equilibrium effects of benefit generosity. However, the use of aggregate data creates severe identification problems. This may be part of the explanation for the fact that the estimated effects are much smaller than one would think based on theory.² The use of data over countries or regions, observed at different points in time, is presumably a more promising way to estimate the equilibrium effects of variations in UI benefit generosity. The prototypical US study in this vein (e.g. Katz & Meyer, 1990) uses policy changes at the state level to identify the effects. However, this approach can be criticized because policy changes at the state level are endogenous with respect to the local cycle; see Card & Levine (2000) and Lalive & Zweimüller (2004).

We also use regional panel data. However, the approach to identification is different and, to our knowledge, novel. The source of variation comes from a nationally determined policy. We exploit the fact that in most real-world UI systems there are ceilings on the amount of benefits received.³ The ceiling stems from the fact that there

¹ See Holmlund (1998), Krueger & Meyer (2002), and Fredriksson & Holmlund (2006) for recent reviews of the literature.

² What “one would think based on theory” is usually based on models where UI is equivalent to the “wage” during unemployment. Most empirical specifications are also derived from this simple model. Of course, real-world UI systems are much more complex and modeling their institutional features may yield different conclusions, a point forcefully made by Atkinson & Micklewright (1991).

³ In the US, the maximum benefit amount even varies by state (Krueger & Meyer, 2002).

is a cap on income which is used to calculate the actual benefit received; increases in income above the cap produce no increase in the actual benefit.⁴ Coupled with the fact that there are well-known regional wage differentials within countries, this implies that the *actual generosity* of UI varies regionally. More importantly, it will vary within region over time because changes in the ceiling produce regional variations in generosity depending on whether the region is above and/or below the ceiling before and after the policy change; moreover, differences in regional wage growth yield regional variation in actual generosity for a given national ceiling.

The fact that the level and changes in the regional wage may produce changes in the actual generosity of UI is, as such, not that useful. Regional wages and wage growth are endogenous with respect to regional unemployment. The challenge is therefore to find a strategy for constructing measures of predicted wages which are plausibly exogenous to local unemployment. Given an exogenous predicted wage, variations in the ceiling will produce differential changes in the actual generosity of UI depending on whether the region is predicted to be above or below the wage cap.

This empirical strategy is implemented using Swedish data during 1974–2002. To generate predicted wages we exploit individual data. For each individual and time point we estimate what the wage would be if his or her characteristics were priced on the national labor market. We then calculate the UI benefit and the actual replacement rate (given the estimated wage) should this individual become unemployed. Finally, the measures of UI generosity are aggregated to the regional level and related to regional unemployment. Notice that the non-linearity of the benefit schedule – induced by the benefit ceiling – implies that the unemployment effect of changes in the actual generosity of UI is identified even if we hold predicted wages and other labor force characteristics constant.⁵

Whether unemployment responds to changes in UI benefit generosity is one of the classic questions in labor economics that dates back to, e.g., Pigou (1932). The policy

⁴ Carling *et al.* (2001) use a similar approach, albeit applied to micro data, when examining whether unemployment duration is affected by variations in UI generosity. They use the fact that because of the benefit ceiling some benefit recipients are treated with a policy change while others are not.

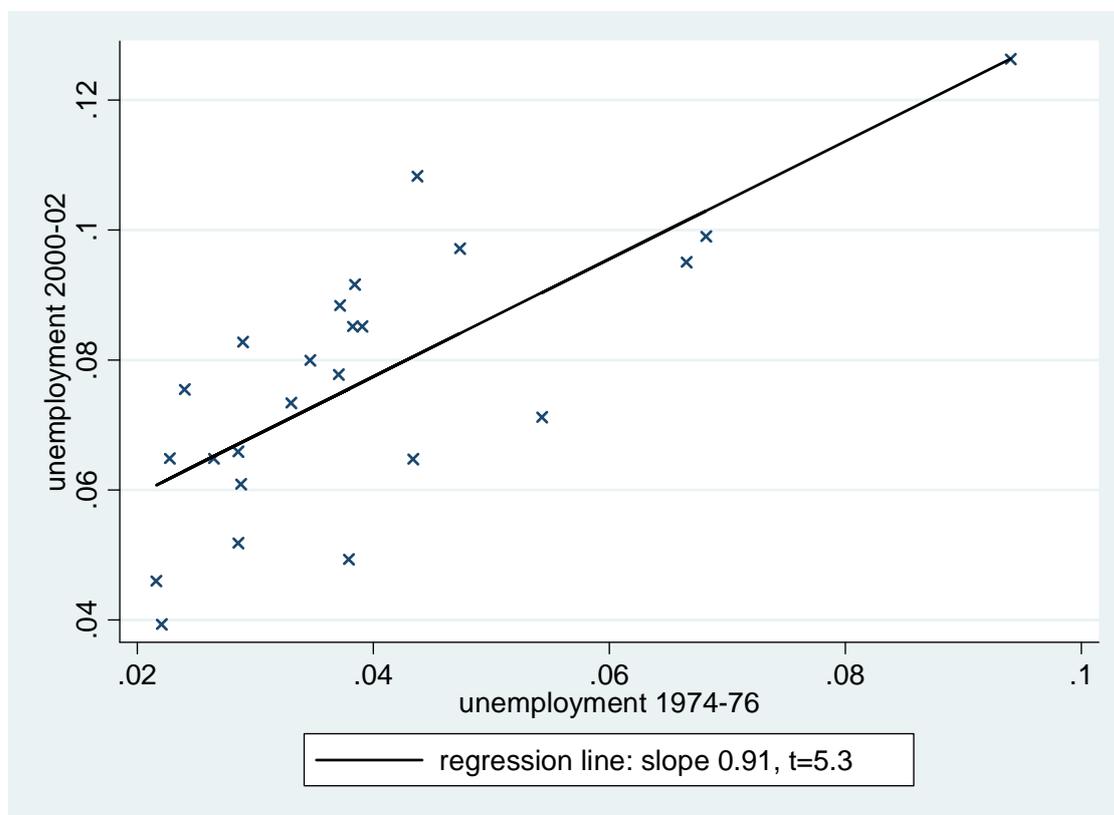
relevance of this question should thus be clear. But there is an additional reason to re-examine the issue: the design of the national unemployment insurance system has implications for the regional unemployment distribution because of the ceiling in benefit receipt.

It is an empirical fact that regional unemployment differentials are very stable in Europe. Figure 1 illustrates this for regional labor markets in Sweden. It is clear that regions which were high unemployment regions in the mid 1970s are also high unemployment regions in the beginning of the 2000s, and vice versa; the regression line has a slope of 0.91 with a t-value of 5.3. The benefit ceiling implies that UI is more generous in high unemployment/low wage regions, a fact that may further increase the spread of regional unemployment differentials. Therefore, it is interesting to examine whether (and how much) a more “neutral” design of the UI system – one that has no benefit ceiling – would reduce the dispersion of unemployment across regions.

The remainder of the paper is outlined as follows. Section 2 sketches our identification strategy. Section 3 describes the Swedish institutional setting. Section 4 presents the data and our empirical strategy. In Section 5 we ask the question: What variation identifies the actual replacement rate? Section 6 presents the estimation results. In Section 7 we conduct two policy experiments to simulate the effects of UI policies on aggregate unemployment and the distribution of unemployment across regions. First we remove the benefit ceiling while holding the nominal replacement rate fixed. Then we raise the nominal replacement rate with the wage cap still in place. Section 8 concludes.

⁵ Later on we will illustrate that the aggregate movements in the benefit ceiling are more or less idiosyncratic.

Figure 1 Regional unemployment persistence



Notes: “Unemployment” is defined as the sum of the openly unemployed and participants in active labor market programs as a share of the labor force.

Sources: Labor Force Surveys and National Labor Market Board.

2 A sketch of the identification strategy

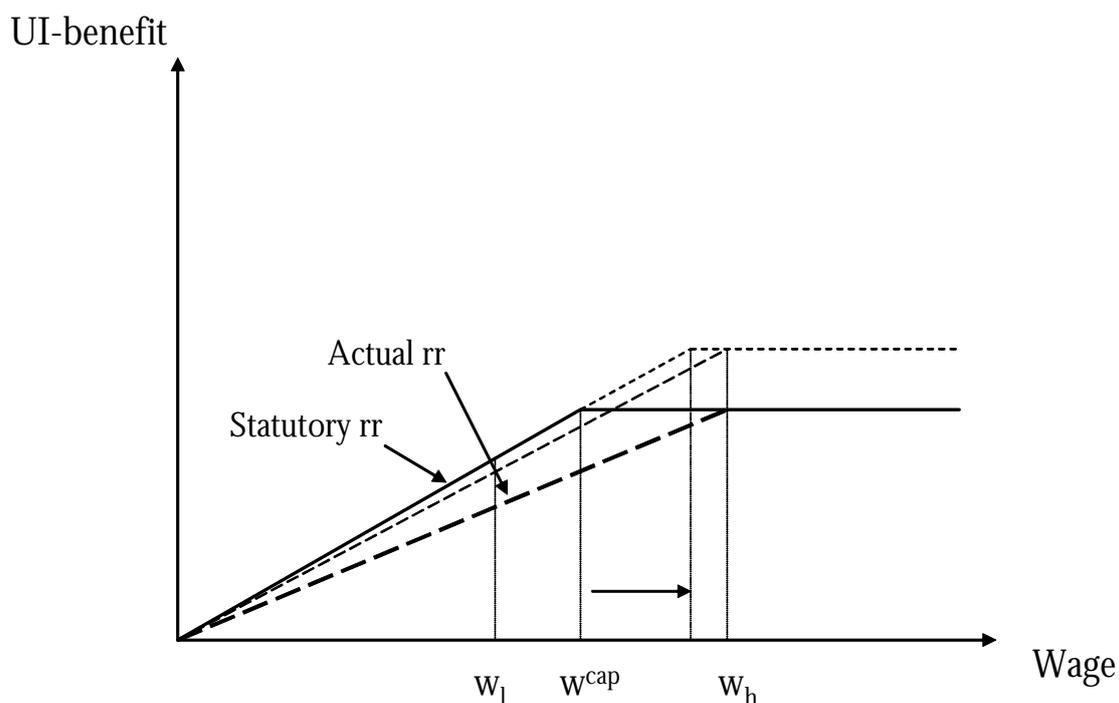
Unemployment benefits matter for the local wage determination since they influence outside opportunities.⁶ More generous benefits, thus, increase wage pressure and as a result regional unemployment rises. The non-linearity of the benefit schedule is useful for identification as we illustrate in Figure 2. The bold (solid) line depicts the benefit schedule. According to this schedule, benefits increase linearly with wages for all wages below the cap (w^{cap}); the rate of increase in benefits is given by the nominal

⁶ This claim is based on a model of local wage bargaining where the wage equation for a worker with given characteristics is given by a weighted average of opportunities inside and outside the firm. In Fredriksson and Söderström (2006), we specify such a model in detail; here, we only provide the gist of the identification strategy.

replacement rate (statutory rr). For wages above the cap there is no increase in benefits as indicated by the flat segment of the benefit schedule.

Suppose, for simplicity, that there is no dispersion of wages within region. Then the actual replacement rate (actual rr) in the high-wage (w_h) region is given by the slope of the dashed line, while the statutory and actual replacement rates coincide in the low-wage region (w_l). Now, suppose that the benefit ceiling increases. Then such a change has no effect in the low-wage region – unemployment insurance replaces previous earnings at the same rate as prior to the change. But there is a substantial effect in the high-wage region, as indicated by the thinner dashed line in Figure 2.

Figure 2 The effects of variations in the benefit ceiling



It is easy to see that if there is wage growth in the high-wage region – i.e., w_h is pushed further to the right in the figure – then this will lower the actual replacement rate. It is also straightforward to verify that if the nominal replacement rate increases this will have the biggest effect on the generosity of UI in the low-wage region.

Other possible sources of variation are more subtle, however. Consider wage dispersion within regions. Let us focus on two regions where mean wages are the same and coinciding with w^{cap} . Suppose, further, that in the two regions the wages are symmetrically distributed around the mean. Then, in the region with the greater variation in wages, the top half of the distribution will have a lower actual replacement rate on average. Thus, the standard deviation of the wage distribution should be negatively associated with the actual replacement rate.

The gist of our identification strategy is the differential effects of changes in the wage cap and the statutory replacement rate. Increases in the wage-cap raises the actual replacement rate more in regions where wages are expected to be higher, while increases in the statutory rate raises the actual replacement rate more in regions where wages are expected to be lower.

To make full use of these predictions we must, of course, take account of the fact that wages (and hence the actual replacement rate) are endogenous to unemployment. More specifically, the concern is that the region-specific shocks will spill over onto unemployment as well as wages. In section 4, we outline how we try to eliminate this simultaneity problem.

3 The Swedish institutional setting

The “Swedish model” is a frequently used term for describing institutions in the Swedish labor market. The Swedish model featured centralized collective wage bargaining and extensive use of active labor market policy.

Given the (historical) reliance on centralized bargaining one might ask if local wage-determination is a relevant feature in the Swedish labor market. However, even during the heydays of the Swedish model, there was bargaining at different layers. There has always been additional wage drift at the local level, which constitutes a substantial fraction of the aggregate wage increase. Historically, wage drift at the local level

accounted for 45 percent of total wage increases (Nilsson, 1993); between 1997 and 2002 wage drift amounted to 31 percent of the total increase.⁷

Wage-setting institutions have changed rather drastically over the past couple of decades.⁸ Centralized bargaining started to crumble in the beginning of the 1980s (Edin & Holmlund, 1995). During the 1990s, there was also a substantial move towards decentralization of wage negotiations. This started in the beginning of the 1990s, when some central agreements for white-collar workers in the private sector neither contained total wage increases nor minimum wage increases. It was entirely up to the employer and the employee to determine the wage; see Lindgren (2005). This trend towards decentralization has resulted in only 7 percent of the employed having their wages completely determined by the central industry bargain in 2004; moreover, the norm in the public sector is individualistic wage determination (Fredriksson & Topel, 2006).⁹

3.1 Unemployment benefits in Sweden

Receipt of unemployment insurance benefits requires the fulfilment of an employment requirement and a membership requirement.¹⁰ The duration of UI receipt is formally 60 weeks. UI benefits replace a fixed fraction (currently 80 percent) of previous earnings up to a ceiling.

For those who do not fulfil the membership requirement there is an Unemployment Assistance (UA) system. Compensation on UA is unrelated to previous earnings and the

⁷ This figure comes from the business cycle statistics reported by Statistics Sweden. Incidentally, it is not obvious how one should define wage drift since the early 1990s. During the 1990s, decentralized or individualistic bargaining has become increasingly common; see below.

⁸ Despite these changes, unions figure as prominently in the Swedish labor market as they did during the beginning of the 1980s. The unionization rate in Sweden has hovered around 80 percent over the past couple of decades (OECD, 2004).

⁹ At the same time as there has been decentralization of the wage bargain, a new coordination regime has emerged. In 1997, the so-called Industrial Agreement (IA) was struck between unions and employers in the manufacturing sector. This agreement involves a set of procedural rules, similar in many ways to the laws governing collective bargaining in the US. It stipulates, inter alia, time-tables for negotiations, rules for conflict resolution, and gives a prominent role for mediators. The IA-model may have delivered incentives for wage restraint at the aggregate level. But it is reasonable to think that it has had a minor influence on the regional wage structure, since the main function of the IA is to establish a set of procedural rules of the game.

¹⁰ The information on the UI rules in this section comes from Olli Segendorf (2003) and pertain to the time period studied in this paper. The employment requirement stipulates that the individual must have worked for a certain number of days during the year immediately preceding unemployment. Sweden is

generosity of UA is much lower than UI; on average it replaces roughly 40 percent of previous earnings. Since the key aspect of our model is the effect of unemployment insurance on wage-setting, we will simply ignore the UA-system in the sequel. To us, this seems like an innocuous omission: the relevant issue in a wage bargaining framework is the level of benefit entitlement for an average *employed worker* upon unemployment entry.¹¹

Another feature of unemployment benefits in Sweden is more relevant. All collective agreements provide additional compensation for (some) workers in the case of redundancies. Despite their relevance, it is very hard to get the full picture of the conditions and payments involved (Sebardt, 2005, provides very useful information, however).¹²

The redundancy payments regulated by collective agreement may come in two forms: either as a lump-sum severance payment or as a supplementary unemployment benefit. Although lump-sum severance payments may be non-negligible and should affect incentives in the wage bargain, we choose to ignore them here. The main reason for this omission is that eligibility is a function of tenure – which is information that we do not have. Furthermore, for the biggest group having a lump-sum severance payment – public sector workers – the lump-sum is proportional to the previous wage with no ceiling imposed. With this construction, the severance payment does not contribute to identification.¹³

one of the few countries where UI is voluntary, hence the receipt of UI also requires the membership in a UI fund for at least 12 months and the payment of a small fee.

¹¹ This is partly the reason for also ignoring the duration of benefit receipt. More importantly, however, benefit duration is unrelated to previous wages and hence do not contribute to identification.

¹² Indeed, Wadensjö (1993) adequately refers to the additional compensation provided by collective agreement as the “unknown part of the social insurance system”. The information in the rest of this section relies heavily on Sebardt (2005).

¹³ The main agreements providing lump-sum severance pay concern public sector employees and private sector blue-collar workers. For local public sector employees, such constructions have existed since 1984. The severance pay is proportional to the previous wage (with no ceiling). At most the employee can be paid half of their annual earnings. This happens in the case of employment for 18 years in the local public sector. For each year of “tenure” less than 18 years there is a proportional reduction in the lump-sum payment. For blue-collar workers, the severance payment is only a function of tenure and age. A rough description is that only individuals above age 50 qualify; in addition, the worker should have at least 10 years of tenure. The payment is proportional to tenure, but increasing with age for given tenure; see Sebardt (2005). Of course, the existence of severance payments raises the nominal replacement rates for

For our whole study period, 1974–2002, there are no supplementary unemployment benefits for the vast majority of workers. Thus, in terms of the periodic unemployment benefit payments, the rules of the public unemployment insurance system apply. There are some notable exceptions, however. Starting in 1990, all central government employees got additional insurance via a collective agreement. Given that the employee has an open-ended contract, or has been on fixed-term contracts for at least three years, there is no benefit ceiling. That is, the employee gets the statutory replacement rate independent of the previous wage.

The oldest collective agreement offering supplementary benefits applies to white-collar workers in the private sector. This has been in place for the entirety of our study period. The supplementary benefit structure is more complex than for government employees. Supplementary benefits are only offered for workers above age 40 who have at least 5 years of tenure. The basic structure is that workers should be offered an actual replacement rate which is no less than 70 percent. A simple way to think about these payments is thus that they kick-in at a wage equaling the benefit ceiling divided by 0.7.¹⁴

The final collective agreement offering supplementary unemployment benefits refers to local public sector employees. This agreement was struck in 1984. It features a strict eligibility requirement. It is given only to redundant employees over 45 satisfying a “tenure” requirement. For 45 year-olds, the tenure requirement is that they should have worked in the public sector for 17.5 years.¹⁵ Should they qualify for supplementary benefits, they are given a benefit equaling the nominal replacement rate times the previous wage with no ceiling imposed.¹⁶

the workers affected by them. Notice that we can to some extent control for the incidence of severance payments by controlling for age and industry composition.

¹⁴ This is almost how the system worked in 2005; the complication that we have not mentioned is that for wages above 20 price base amounts (SEK 788,000 in 2005), the slope of the benefit-wage schedule becomes 0.25. Further, relative to the system that existed during 1974–2002, it is a slight simplification at the bottom end. Those below the wage cap implied by a replacement rate of 70 percent were given a relatively small nominal amount as well; this nominal amount raises the nominal wage replacement rate for those below the wage cap in the public UI system.

¹⁵ The tenure requirement decreases with age: at age 60, 10 years of tenure is required.

¹⁶ On top of all this, some UI funds offer their members the option of purchasing private unemployment insurance. However, this possibility is very recent and hence does not concern us.

4 Data and construction of key variables

We use three principal data sources: (i) LINDA – an individual (register) data base (see Edin & Fredriksson, 2000, for a description); (ii) regional (open) unemployment from the Labor Force Surveys; and (iii) regional active labor market program rates from the Labor Market Board.

The LINDA data set is based on a combination of income tax registers, population censuses, wage data, and other sources. Unfortunately, the wage data are not available for our entire study period. We only have access to wages for a representative sample of workers from 1998 and onwards. Apart from wages, the individual data we use in our analysis are based on register information. Earnings and some other characteristics (gender, age, education, marital status, and industry affiliation) are obtained from the income tax registers, which also contain information on region of residence and country of birth from the population registers. The earnings information and most of the other individual characteristics are available throughout the time period; see appendix for more information on data availability.

The individual data are used to calculate measures of the composition of the regional labor force and to run individual earnings regressions. The estimated parameters from the earnings regressions are used to generate expected wages had the characteristics of the individual worker been priced at the national labor market. We use this strategy to free the estimates from the simultaneity bias caused by local shocks affecting both regional unemployment and wages. Having generated these expected wages we calculate the average of these wages at the regional level and the actual generosity of UI at the regional level.

4.1 Construction of key independent variables

We start by estimating individual earnings regressions separately by year. These equations have the following structure

$$\ln y_{ijt} = \alpha_t + \alpha_{jt} + \beta_t X_{it} + \varepsilon_{ijt} \quad (1)$$

where i indexes individuals, j regions, and t time. In equation (1), y denotes earnings, α_{jt} is a region-fixed effect – normalized such that $\sum_j \alpha_{jt} = 0$ – and X denotes the vector of covariates. We control flexibly for region at the estimation stage to avoid sorting bias in the coefficient vector, β_t ; such a bias might arise if high-skilled individuals cluster in regions hit by positive wage shocks. The covariate vector includes information on gender, age (separate dummies for each five-year age category) educational attainment, marital status, country of birth, and industry. We run these equations for each year between 1970 and 1998 including only individuals who are 16–59 years of age.¹⁷

Using the estimates of the parameters in (1) we want to generate an expected wage – the wage that each individual would obtain if his/her characteristics were priced on the national labor market. Our main strategy to compute such a wage is as follows

$$w_{it}^e = \delta_{it} - \bar{\delta}_t + \bar{w}_t \quad (2)$$

where $\delta_{it} = \exp(\hat{\beta}_{t-4} X_{it})$, $\bar{\delta}_t$ denotes the mean of δ_{it} , and \bar{w}_t denotes the average wage in the country.¹⁸ Thus, the individual gets assigned the same wage independently of where s(he) is located. We lag the “national price vector”, $\hat{\beta}$, four years in order to ensure that the expected wage is independent of any region-specific shocks. Big regions, such as Stockholm, are likely to be very influential in the estimation of $\hat{\beta}_t$. If we would have used $\hat{\beta}_t$ rather than $\hat{\beta}_{t-4}$ a potential worry is that the wage predictions would not have been independent of shocks to unemployment in Stockholm. Another reason for not using $\hat{\beta}_t$ concerns skilled-biased technical change. Suppose there is skilled-bias technical change. This will presumably raise the return to education and will represent a favorable employment shock in regions rich on observed and

¹⁷ The upper age limit is due to the fact that the information on education is only consistently available for individuals less than 60 years-of-age. When estimating these equations we exclude the lowest quintile of the earnings distribution. The rationale for this is that we want the parameter estimates to resemble what one gets when estimating traditional wage equations; see Antelius & Björklund (2000).

¹⁸ According to equation (2) we adjust the predictions such that they are mean zero and center them on the mean national wage.

unobserved human capital. This scenario will induce a negative correlation between the wage prediction and the error-term in the unemployment equation.

Given a measure of the expected wage, we proceed to define an individual indicator variable for having predicted wages above the wage cap. Moreover, we calculate the actual replacement rate at the individual level as

$$\rho_{it}^a = \rho_t^n \left[I(w_{it}^e \leq w_t^{cap}) + (w_t^{cap} / w_{it}^e) I(w_{it}^e > w_t^{cap}) \right] \quad (3)$$

where ρ_t^n denotes the statutory replacement rate, w_t^{cap} the wage-cap and $I(\cdot)$ the indicator function. We then average over all individuals residing in the region which gives us

$$\rho_{jt}^a = \frac{\sum_{i \in j} \rho_{it}^a}{N_{jt}}, \quad w_{jt}^e = \frac{\sum_{i \in j} w_{it}^e}{N_{jt}} \quad (4)$$

where N_{jt} is the number of individuals residing in region j at time t . ρ_{jt}^a is the key independent variable in the empirical analysis.

In equations (3) and (4) we have calculated the actual replacement rates as if only the public UI system is relevant. Obviously, we would also like to take the existence of supplementary unemployment benefits into account. In the next section, we outline how we try to accommodate this feature.

4.2 Supplementary unemployment benefits

Since we do not have adequate information in the data, taking supplementary unemployment benefits into account is bound to involve some approximations. In the data, we observe in what sector the individual works but we do not observe whether the individual is a blue-collar or a white-collar worker. Further, we do not observe tenure for the individual worker.

The supplementary unemployment benefit in the central government sector is fairly straightforward to approximate. Historically, the vast majority of workers in the public sector were on open-ended contracts. Therefore, we simply assume that all workers are eligible for this system from 1990 and onwards. Since this agreement implies that there

is no benefit ceiling, we set the actual replacement rate equal to the nominal one from 1990 and onwards for central government workers.

White-collar worker status in the private sector is proxied with workers in the private sector having at least three years of (theoretical) upper-secondary education. The supplementary benefit was paid to individuals who were at least 40 years-of-age with at least 5 years of tenure in the firm. The question then is: What does the tenure structure look like for white-collar workers in the private sector above 40? To examine this question we used survey data from the Swedish Level of Livings Survey (LNU) in 2000; Erikson & Åberg (1987) describe the LNU data. It turned out that 75 percent of workers in the private sector with at least 3 years of upper-secondary education had tenure of at least 5 years. Therefore, as an approximation, we assume that all workers that we classify as private sector white-collar workers are eligible for supplementary benefits if they satisfy the age constraint. The workers that qualify for this supplementary benefit are given the benefit structure outlined in section 3.1, i.e., the actual replacement rate never falls below 0.7.

The final supplementary benefit agreement concerns local government employees. In this case the age constraint is 45 and the “tenure” requirement is almost 18 years. Since this requirement appears very stringent, we have chosen to ignore this agreement altogether.¹⁹

4.3 Data

There are many steps involved in creating these regional panel data. The full detail of our data collection effort is presented in Appendix A. Here we describe the main steps and present the main characteristics of the data.

We begin by creating a data set involving individual characteristics and earnings from 1970 to 2002. The included individual characteristics are fairly standard. We have

¹⁹ Also, in this case we had a brief look at the LNU data. The “tenure” requirement in the agreement pertains to the total number of years worked in the local public sector. This is not observed in the LNU data. If we look at tenure with the current employer – a reasonable approximation of the number of years of continuous employment in the local public sector – we find that a quarter of those aged 45 are eligible. Eligibility increases with age. At age 50, half of the relevant population is eligible and at age 59 around three quarters are eligible. If we take into consideration that only 24 percent of the workforce is employed

information on gender, age, marital status, region of residence (at the county level), educational attainment, industry affiliation (2-digit ISIC), and country of birth. With respect to country of birth we distinguish between individuals of native, Nordic, OECD, or non-OECD origin.²⁰ In terms of education, we distinguish between compulsory school (or less), upper secondary school, and tertiary education.

We first utilize these data for estimating individual earnings regressions. On the basis of the estimated equations we generate an expected “wage” for each individual as described above. The mean of the predictions is adjusted such that it corresponds to the national average wage for each point in time.²¹

Then we also need information on the relevant parameters of the UI system: the benefit ceiling and the nominal replacement rate. The benefit ceiling is specified in nominal terms, so it comes as no surprise that it has been changed frequently. On 20 occasions the ceiling was changed during the time period. During the entire time period, the ceiling is changed on the discretion of the legislator and there is a good deal of haphazardness introduced by these discretionary changes. The nominal replacement rates have been changed more infrequently. There have been four changes in the nominal replacement rate between 1974 and 2002.

In Figure 3 we plot the evolution of the nominal replacement rate and the wage cap (divided by mean wages) over time at the national level. Along with these two series, we also plot the evolution of the actual replacement rate – unadjusted as well as adjusted for the incidence of supplementary unemployment benefits.

Figure 3 shows that there is a good deal of idiosyncratic variation in the wage cap and that this variation contributes to most of the variation in the actual replacement rate (we substantiate this claim in the next section). Figure 3 also shows that benefit

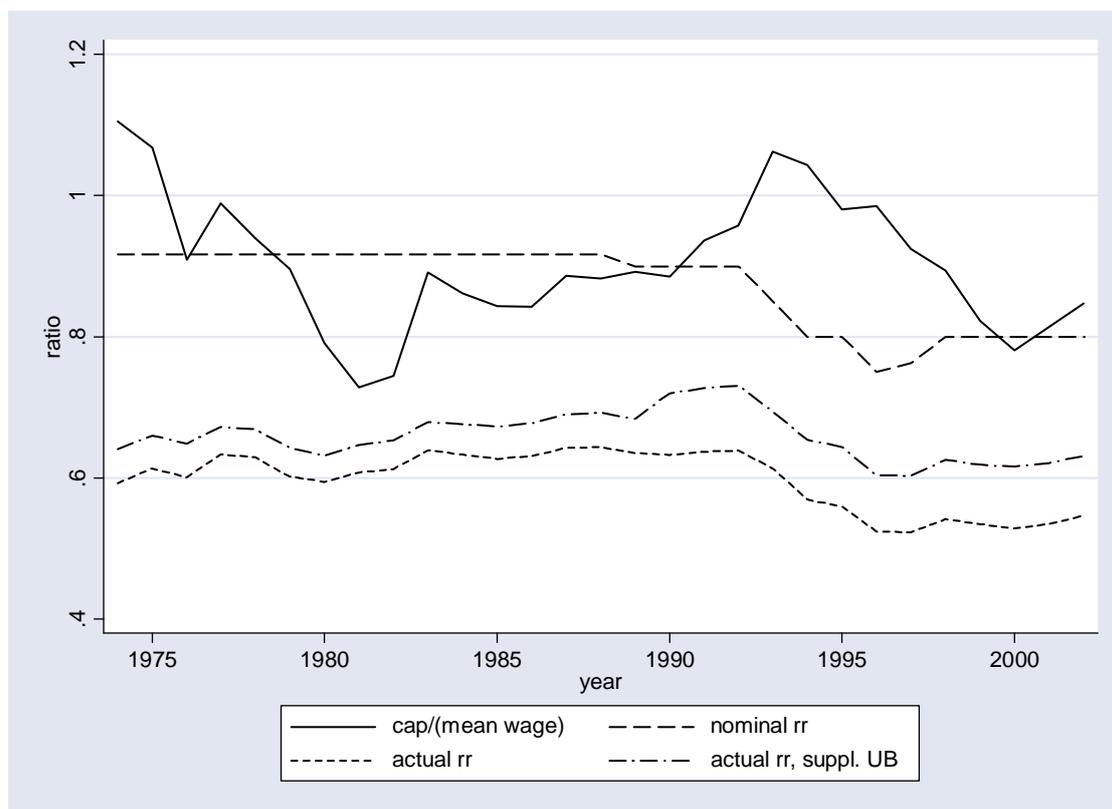
in the local public sector, a small share of the population is eligible for this supplementary unemployment benefit and, therefore, we simply ignore it.

²⁰ Individuals are generally classified as being of OECD origin if they were born in a country which was a member of the OECD in 1985. The only exceptions from this rule are Turkey – which is included among the non-OECD countries – and the Nordic countries.

²¹ We are implicitly assuming that the estimates of the slope parameters in the earnings regressions are the same as they would be in the wage regressions. This may be a questionable assumption since earnings variations are also due to variations in hours worked. But notice that we trim the lower tail of the earnings distribution to minimize this problem.

generosity was scaled back following the unemployment crisis in the beginning of the 1990s.

Figure 3 The nominal/actual replacement rate and the wage cap, 1974–2002

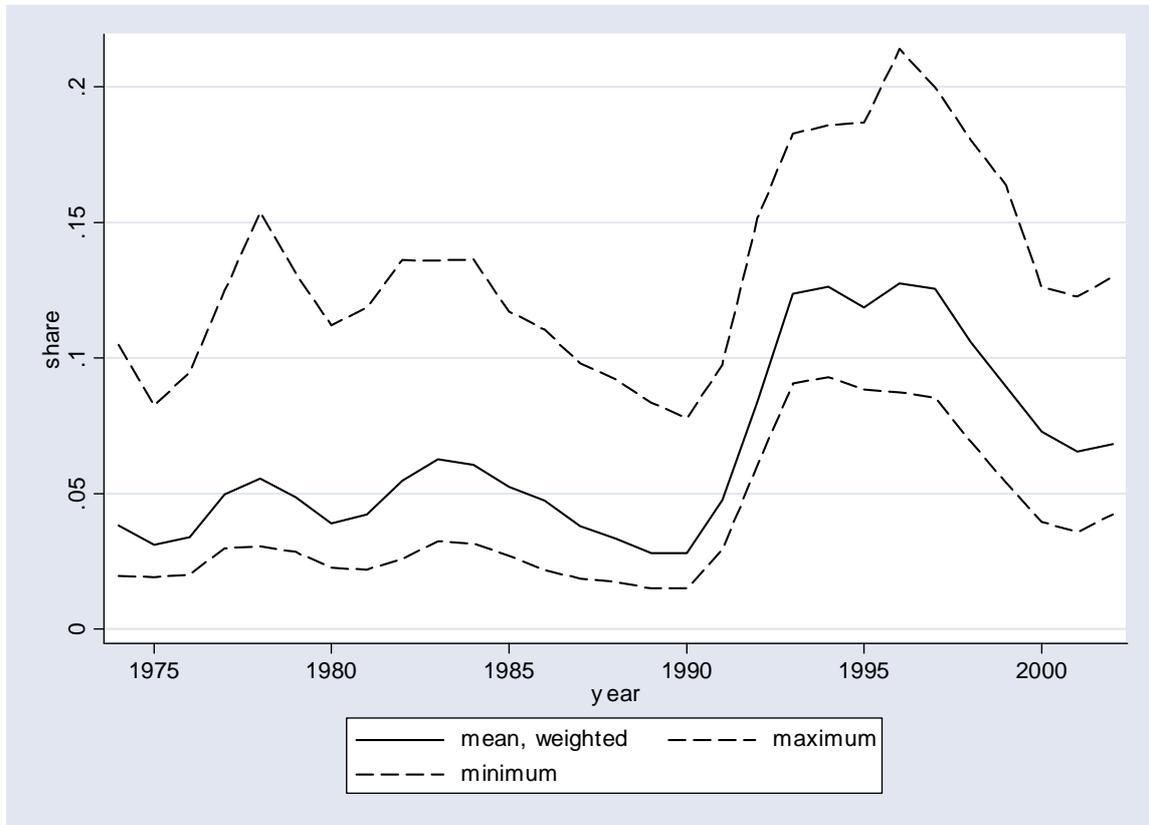


Sources: See data appendix

Our key outcome measure is defined as the sum of open unemployment and participants in labor market programs as a share of the labor force. With some abuse of language we refer to this sum as “unemployment” in the sequel.

Figure 4 shows the development of mean unemployment along with the evolution of the extremes in the distribution to give a sense about the regional variation in the data. The most striking event in this figure is the adverse shock that hit Sweden in the beginning of the 1990s. In just three years unemployment shot up from around three percent in 1990 to roughly 13 percent in 1993. The aggregate unemployment rate was stable at this high level until 1997. In some regions, however, unemployment continued to rise to reach 22 percent in 1997. The period since then has seen substantial fall in unemployment.

Figure 4 Unemployment, mean and spread, 1974–2002

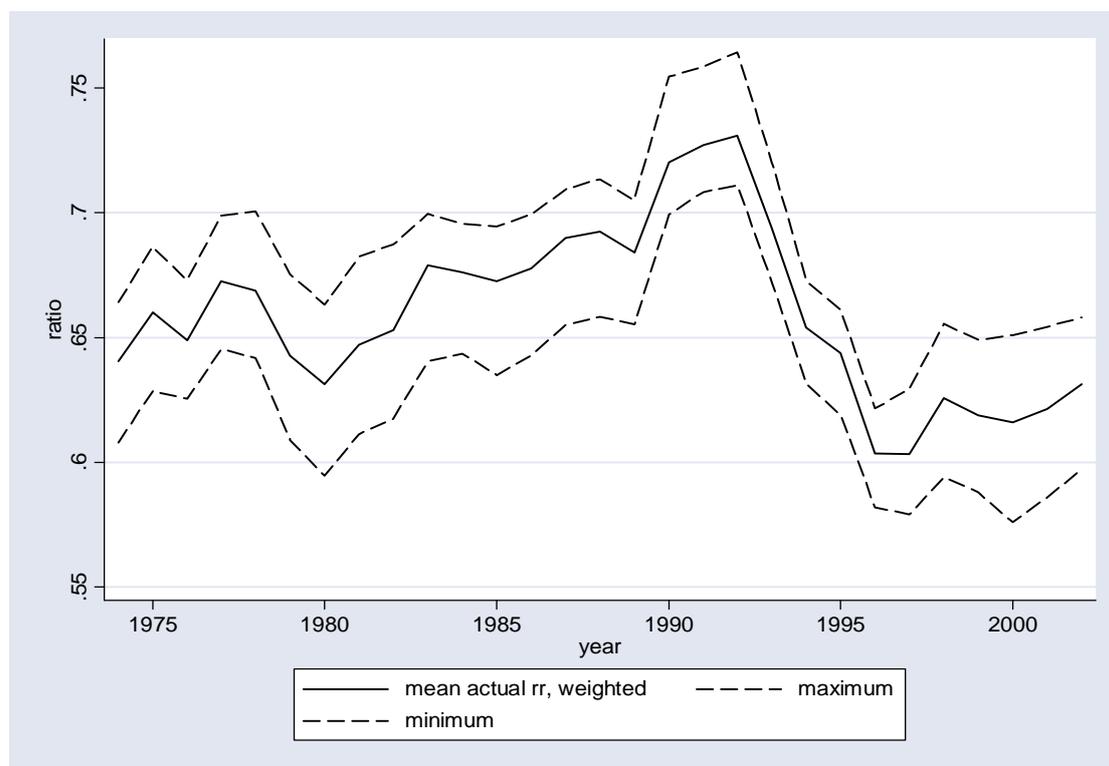


Notes: Unemployment is defined as the sum of the openly unemployment and participants in active labor market programs as a share of the labor force.

Sources: Labor Force Surveys and the Labor Market Board.

Figure 5 gives a sense about the regional variation in our key measure of the generosity of the UI system. It shows the variation in the actual replacement rates over time and across regions when supplementary unemployment benefits have been taken into account. The actual replacement rate stood at a high in the early 1990s when it equaled 73 percent. Since then it has fallen quite rapidly to 63 percent in 2002. The variation across regions was particularly high around 2000. It is evident that there is a good deal of variation across regions as well as time, which we utilize in the following sections.

Figure 5 The actual replacement rate, mean and spread, 1974–2002



Notes: The actual replacement rate have been generated using coefficients estimated on earnings data from 1970–98. The actual replacement rate takes supplementary unemployment benefits into account.

5 The variation in the actual replacement rate

In section 2 we argued that the variations in expected wages, the benefit ceiling, the nominal replacement rate, as well as the variation in the spread of the expected wage distribution all contribute to the variation in the actual replacement rate. Here we illustrate the empirical importance of each source of variation. Table 1 presents the results.

To facilitate the interpretation of the independent variables we standardize these variables with their standard deviations. In panel A) we show the results when not taking the existence of supplementary unemployment benefits into account. All the estimates have signs which are consistent with the discussion in section 2. So, for instance, if expected wages increases by a standard deviation this yields a reduction of the actual replacement rate by half a percentage point; see column (2). It is also

interesting to note that the variation in the wage cap is such a powerful predictor of the actual replacement rate; this confirms the impression given already in Figure 3. An increase in the cap has the effect of increasing the actual generosity of UI more in regions which are expected to be high-wage.²²

Table 1 What explains the variation in the actual replacement rate?

	(1)	(2)	(3)
A)			
No account for supplementary UB			
Expected wage	-0.392** (0.033)	-0.502** (0.177)	-0.701** (0.178)
Expected wage interacted with wage cap		0.466** (0.056)	0.550** (0.064)
Expected wage interacted with nominal replacement rate		-0.321** (0.132)	-0.197 (0.130)
Standard deviation of expected wage			-0.062** (0.014)
# observations	696	696	696
Within R ²	0.63	0.80	0.81
B)			
With account for supplementary UB			
Expected wage	-0.281** (0.042)	-1.30** (0.437)	-1.32** (0.442)
Expected wage interacted with cap		0.601** (0.103)	0.611** (0.113)
Expected wage interacted with nominal replacement rate		0.463 (0.362)	0.477 (0.361)
Standard deviation of expected wage			-0.007 (0.022)
# observations	696	696	696
Within R ²	0.29	0.43	0.43

Notes: Dependent variable in percent. The table reports standardized coefficients. An individual coefficient has the interpretation of percentage point change in response to a standard deviation increase in one of the independent variables. The regressions also control for regional fixed effects, region-specific trends, fixed time effects, and exogenous labor force characteristics. Within R² reports the share of the variance explained by the four variables in the table after having controlled for other covariates, region-specific FEs and trends, as well as time effects. Other covariates include age, education, immigrant status, gender, industry affiliation, and the share covered by supplementary UB (only panel B). Regressions are weighted by population. Standard errors, reported in parentheses, allow for clustering at the county level. Significance levels: * = 10%, ** = 5%.

²² Notice that it is only the interaction effect which is identified. The main effect of the wage cap is “swamped” by time fixed effects.

In panel B) we consider the variation in the generosity of UI when supplementary unemployment benefits are taken into account. The evidence presented in panel B) is not as clean as the estimates presented in the previous panel. For instance, the statutory replacement rate no longer has a greater effect in regions that are predicted to be low-wage (which should be the case according to the simple benefit formula). And the standard deviation of the expected wage distribution ceases to be a significant predictor of the actual replacement rate. Nevertheless, the estimates (again) suggest that the wage cap is the most significant contributor to the explained variance of the actual replacement rate.

Another aspect of the results in Table 1 is also worth noting. With the four variables we do not account fully for the variation in the actual replacement rate. In other words, there is residual variation, since the explained variance does not equal unity. There are a number of reasons for this. At the individual level, the benefit schedule depicted in Figure 2 is deterministic. This is not the case at the aggregate regional level. To explain the variation in the actual replacement rate fully at the regional level, we would have to include all moments of the expected wage distribution; obviously, this is not feasible. Further, supplementary unemployment benefits introduce additional noise, which is evidenced by the fact that explained variance is lower in panel B) than in panel A).²³

In summary, the most important finding in this section is that a substantial fraction of the variation in the actual replacement rate at the regional level is due to variations in the national wage cap. An increase in the wage cap has a greater positive effect on UI generosity in regions which are expected to be high-wage. Thus it should be possible to identify the effect of the actual replacement rate on regional unemployment using only the variation in the wage cap. This identification strategy is the one that we will mainly pursue in the next section.

²³ A final reason is that we are not using the functional form implied by Figure 2. Since this is not the right function at the regional level, we have no reason to impose it.

6 The effect of the actual replacement rate

With the exercise in section 5 as a background we now proceed to examine the relevance of the UI system for regional unemployment. We begin with a very basic question. Do the parameters of the UI system (the benefit ceiling, w^{cap} , and the statutory replacement rate, ρ^n) have any impact on regional unemployment? This is a relevant question given that many collective agreements supplement unemployment benefits. To investigate this issue, we first estimate the equation

$$\ln u_{jt} = \lambda_w w_{jt}^e + \lambda_{cap} (w_t^{cap} \times w_{jt}^e) + \lambda_\rho (\rho_t^n \times w_{jt}^e) + \lambda_\sigma \sigma_{jt} + \lambda_X X_{jt} + \mu_j^1 + \mu_t^1 + \delta_{jt}^1 + \omega_{jt}^1 \quad (5)$$

where λ denotes parameters to be estimated, w_{jt}^e the expected wage and σ_{jt} the standard deviation of the expected wage distribution. The vector of characteristics, X , includes the same components as in the individual earnings regressions since any exclusion restriction with respect to the components of X is bound to be arbitrary. Furthermore, X includes a control for supplementary unemployment benefits. The specification of equation (5) also takes region-specific effects (μ_j^1), time fixed effects (μ_t^1), as well as region-specific trends (δ_{jt}^1) into account.

The idea behind equation (5) is that the first four components conceptually drive the variation in the actual replacement rate at the regional level. One can potentially make the argument that the expected wage and the standard deviation capture omitted variables in the unemployment equation. But it is very hard to see that this is a relevant argument for the interaction terms. In particular, if we find that $\lambda_{cap} > 0$ this strongly suggests that the design of the national UI system has implications for regional unemployment. The same line of argument goes for the interaction with the nominal replacement rate where we would expect $\lambda_\rho < 0$.

Table 2 reports the results. We standardize the key independent variables to facilitate the interpretation of the coefficients of these variables.

Table 2 Basic estimates

	ln(unemployment)
Expected wage	-0.069 (0.054)
Expected wage interacted with wage cap	0.036** (0.015)
Expected wage interacted with nominal replacement rate	0.044 (0.046)
Standard deviation of expected wage distribution	0.008 [*] (0.005)
Other covariates	Yes
Region-specific FEs	Yes
Region-specific trends	Yes
Time effects	Yes
Overall R ²	0.981
Within R ²	0.045
# observations	696

Notes: Key independent variables are standardized and have the interpretation of the effect on the dependent variable in response to a standard deviation increase in the independent variable. All regressions are estimated using a within-estimator and include controls for gender, age, marital status, educational attainment, immigrant status, industry affiliation, and the share of individuals covered by collective agreements with supplementary unemployment benefits. Within R² reports the share of the variance explained by the four variables in the table after having controlled for other covariates, region-specific FEs and trends, as well as time effects. Regressions are weighted by population. Standard errors, reported in parentheses, are clustered by county. Significance levels: ^{*} = 10%, ^{**} = 5%.

The interaction between the expected wage and the wage cap enters significantly with a positive sign. Thus, changes in the wage cap produce a greater increase in unemployment in regions which are expected to have a high wage. A standard deviation increase in this interaction term raises unemployment by almost 4 percent. The remaining interaction variable is not significant and does not have the predicted negative sign; this result is consistent with the estimates reported in panel B) of Table 1.

The most important result contained in Table 2 is that the parameters of the national UI system do affect regional unemployment. Having established this we proceed to estimating equations imposing more structure.

The specification in equation (6) imposes more structure. In this case we relate unemployment directly to the actual replacement rate (ρ^a). Thus

$$\ln u_{jt} = \varphi_\rho \rho_{jt}^a + \varphi_w w_{jt}^e + \varphi_\sigma \sigma_{jt} + \varphi_X X_{jt} + \mu_j^2 + \mu_t^2 + \delta_j^2 t + \omega_{jt} \quad (6)$$

where φ denotes parameters to be estimated.

There are two potential ways to estimate (6). The first is akin to a control function approach. Controlling for expected wages and the standard deviation of the expected wage distribution, the remaining variation in the actual replacement rate has two components: one source of variation is due to the interaction terms between the expected wage and the wage cap as well as the nominal replacement rate respectively; the other source of variation is the residual variation in the actual replacement rate. Given the substantial difficulties involved in measuring the actual generosity of UI, the residual variation is likely to contain a lot of noise; this implies that the control function approach will generate estimates that are biased downwards due to attenuation. Therefore, we will not pursue this strategy.

The other approach to estimating (6) is to just utilize the predicted variation in the actual generosity of UI stemming from the variation in the interaction terms – the most important of these interactions being the variation stemming from the changes in the wage cap. Implementing this strategy is straightforward; it amounts to estimating equation (6) using standard IV (i.e. two-stage least squares) methods.

Table 3 reports estimates of equation (6). The dependent variable is the log of unemployment. In column (1), the actual replacement rate does not account for supplementary unemployment benefits; in column (2) it does. The equations are based on using an IV approach and are all estimated using a traditional within-estimator.

Table 3 suggests that the actual replacement rate has a positive and significant effect on unemployment. The preferred estimate, shown in column (2), suggests that unemployment rises by 5 percent (i.e. the unemployment rate increases from, say, 6 to 6.3 percent) in response to increase in the actual replacement rate of 1 percentage point.²⁴ The elasticity of unemployment with respect to benefit generosity implied by

²⁴ This change in benefit generosity roughly corresponds to the weighted standard deviation of the actual replacement rate within regions and time.

this estimate is remarkably high. Evaluated at the mean actual replacement rate in 2002 (63 percent), the elasticity equals 3.4.

Table 3 The effect of the actual replacement rate on unemployment

	ln(unemployment)	
	(1)	(2)
Actual replacement rate (percent) (No account for supplementary UB)	0.045** (0.023)	
Actual replacement rate (percent) (Account for supplementary UB)		0.054** (0.023)
Expected wage/1000	0.040** (0.016)	0.036** (0.013)
Standard deviation of expected wage/1000	0.030* (0.012)	0.023 (0.012)
Other covariates	Yes	Yes
Region-specific FEs	Yes	Yes
Region-specific trends	Yes	Yes
Time effects	Yes	Yes
Overall R ²	0.981	0.980
Estimation approach	2SLS	2SLS
# observations	696	696

Notes: The interactions between the expected wage and the wage cap as well as the nominal replacement rate respectively are used to identify the coefficient on the actual replacement rate. All regressions are estimated using a within-estimator and include controls for gender, age, marital status, educational attainment, immigrant status, industry affiliation, and the share of individuals covered by collective agreements with supplementary unemployment benefits. Table B1 in Appendix B reports the coefficient estimates on the majority of the remaining covariates for the specification reported in column (2). Regressions are weighted by population. Standard errors, reported in parentheses, are clustered by county. Significance levels: * = 10%, ** = 5%.

The estimates reported in Table 3 are higher than we have found elsewhere in the literature. They are roughly four times higher than Nickell (1998) obtained in his study of a cross-section of OECD countries. Krueger & Meyer (2002) report a benefit elasticity of one when taking the effect on the incidence as well as duration of unemployment into account.

Of course, it is hard to pinpoint why we get higher estimates than those available elsewhere in the literature. Relative to Nickell (1998), we would argue that effects we estimate are more credibly identified than in his cross-country regression. The estimate

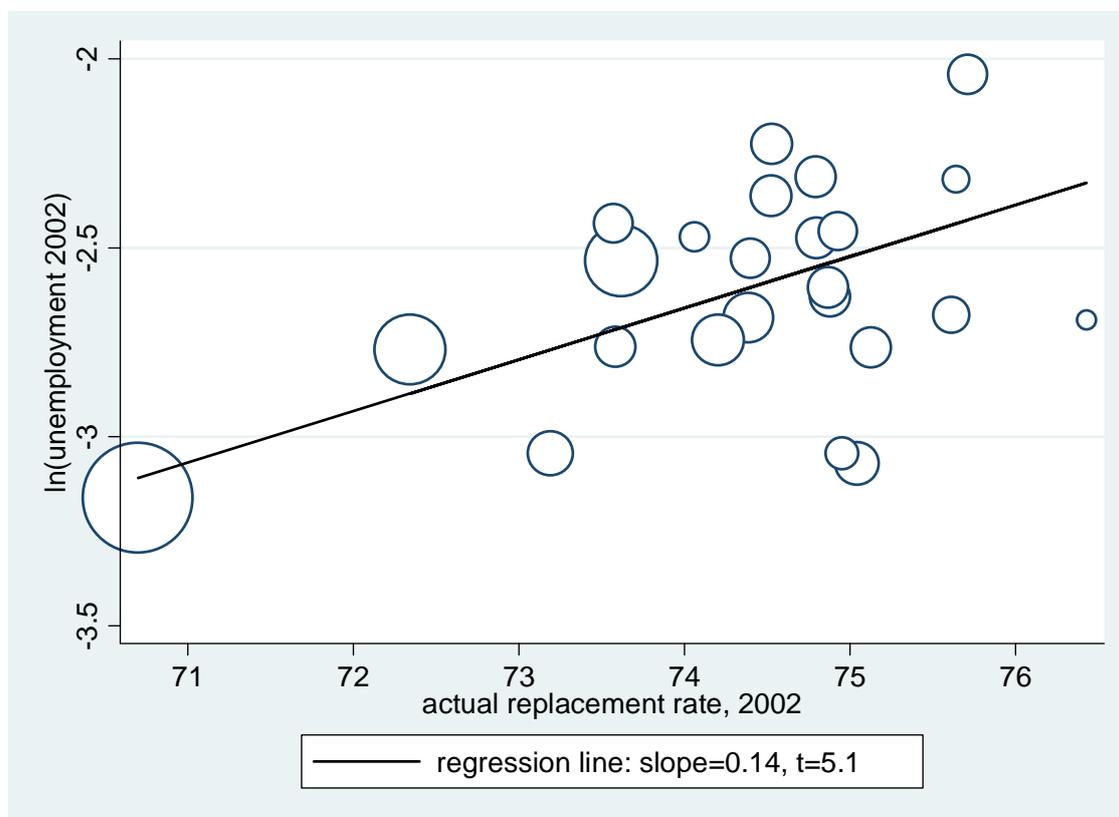
reported in the Krueger & Meyer (2002) is obtained by surveying micro studies mostly pertaining to the US. Here we cannot argue that our estimate is more credibly identified. But clearly the estimated parameter is different in the sense that it takes equilibrium adjustments into account to a greater extent than in micro studies. Also, we obtain this estimate using Swedish data where unemployment benefits are substantially more generous than in the US. This is a relevant issue since, e.g., standard matching models suggest that the general equilibrium effect on unemployment of a given variation in UI generosity is greater the higher is UI benefits from the outset; some illustrative simulations on this theme are reported in Holmlund (1998) and Hornstein *et al.* (2005).

We have subjected the specification in column (2) to some specification checks. First we used the unemployment rate as the dependent variable. The estimate is substantially weaker. A percentage point increase in UI generosity causes unemployment to rise by 0.090 percentage points; the standard error of this estimate is 0.062. Second, we introduced a lag of the actual replacement rate. This virtually had no effect on the estimate and the coefficient on the lag was not significant. Third, we transformed the model by taking first differences. This reduced the size as well as the precision of the estimate. The coefficient on the actual replacement rate was reduced to 0.024 with a standard error of 0.015. Despite the fact that the estimates sometimes become less precise, we view them as fairly robust to specification changes.

7 Policy interventions

The purpose of this section is to conduct two policy simulations. In particular we are interested in the effect of removing the benefit ceiling and the effect of increasing the nominal replacement rate. These two policy changes have obvious implications for aggregate unemployment – i.e. aggregate unemployment increases. The more interesting effects are those on the regional distribution of unemployment. In almost all countries, regional unemployment differentials are very stable over time; see Figure 1 and, e.g., Fredriksson (1999) for a collection of evidence. Perhaps the design of the social insurance system contributes to this feature?

Figure 6 The correlation between unemployment and the actual replacement rate

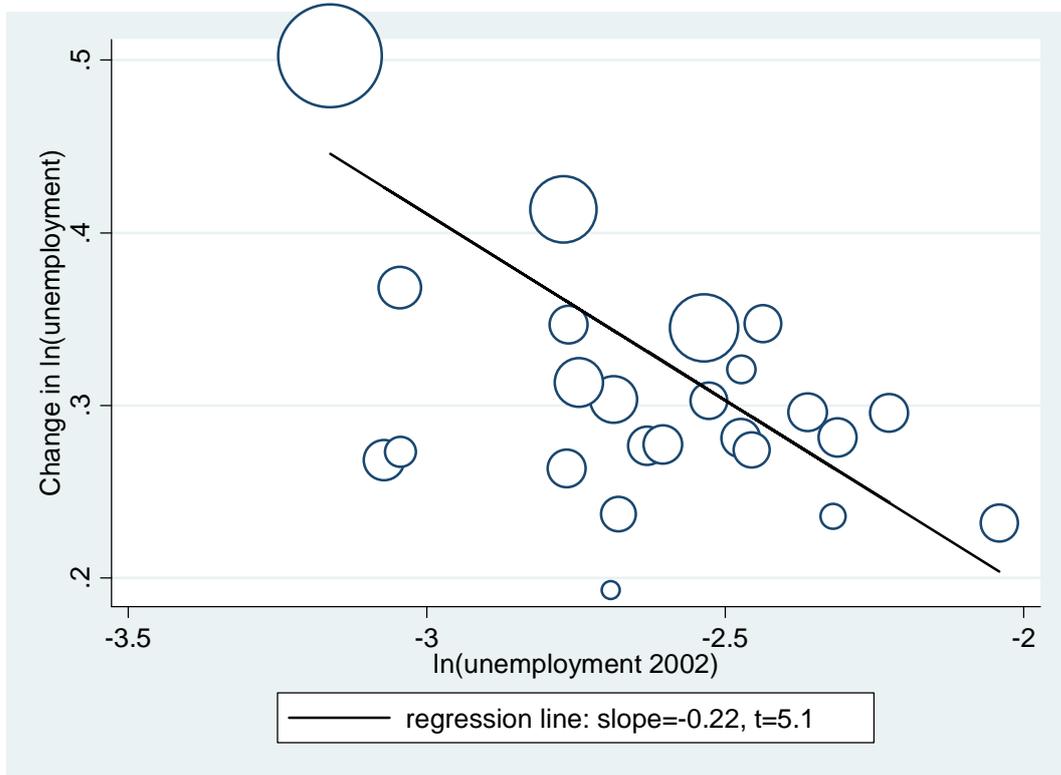


The starting point for these experiments is the regional distribution of unemployment and actual replacement rates in 2002. In contrast to the previous analysis we actually have wage data for 2002 and hence we have a better estimate of the “true” actual replacement rate. In Figure 6 we show the correlation between regional unemployment and the actual replacement rate accounting for supplementary unemployment benefits. As shown by the slope of the regression line, a percentage point increase in the actual replacement rate is associated with 14 percent higher unemployment. This just illustrates that high-wage regions tend to be low-unemployment regions. Since unemployment benefits replace a lower fraction of previous wages in high-wage regions, they also tend to have a lower actual replacement rate. This simultaneity bias thus inflates the estimate of the relationship between benefit generosity and unemployment.

In 2002, the aggregate unemployment rate stood at 6.8 percent. To generate the situation after a policy change we use the estimate on the actual replacement rate

reported in column (3) of Table 3, i.e., the coefficient on the actual replacement rate is set to 0.054.

Figure 7 Policy simulation: Removing the cap



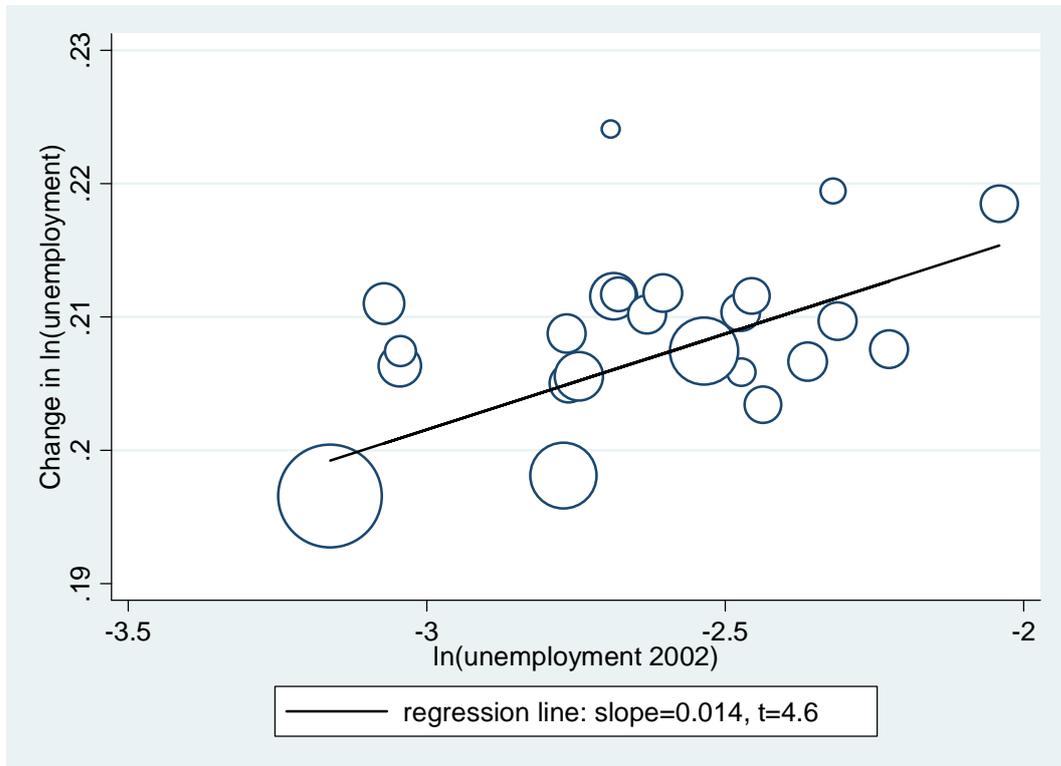
Notes: This graph is based on a hypothetical policy experiment where the benefit ceiling is removed. The implied change in unemployment is calculated using a coefficient on the actual replacement of 0.054. The regression line is based on a weighted regression using regional population as weights.

What happens if we remove the benefit ceiling? Obviously this has the effect of making the system more generous – the actual replacement rate rises by 6.5 percentage points, on average. As a consequence, there is an increase in overall unemployment from 6.8 percent to 9.6 percent. What is more the spread of the regional unemployment distribution is reduced. This is illustrated in Figure 7, which relates the change in log unemployment – induced by the reform – to the log of the unemployment rate prior to the change. As the graph shows, there is a greater change in regions where unemployment was low initially; the slope of the regression line is negative with a t -ratio of 5. The intuition for this result is that the proposed policy change has a bigger

effect in high-wage regions, which also tend to be low-unemployment regions. Thus, the policy change contributes to reducing unemployment differentials.

Now, what about raising the nominal replacement rate to 85 percent? Again, this makes the system more generous and the actual replacement rate rises by 3.8 percentage points. Consequently, the aggregate unemployment rate rises to almost 8.4 percent. What is more, this policy change has the opposite effect on the regional distribution of unemployment in comparison to the change in the benefit ceiling. As Figure 8 shows, the unemployment rate increases more in regions which were high-unemployment locations initially. The intuition is analogous to the previous case. High-unemployment regions tend to be low-wage regions. Consequently, changes in the statutory replacement rate have a bigger impact on the actual generosity of the UI system in these locations. Comparing the slope of the regression lines in Figure 7 and Figure 8, we see that removing the benefit ceiling has a stronger differential impact across regions.

Figure 8 Policy simulation: Increasing the nominal replacement rate



Notes: This graph is based on a hypothetical policy experiment where the nominal replacement rate is raised to 85 %. The implied change in unemployment is calculated using a coefficient on the actual replacement of 0.054. The regression line is based on a weighted regression using regional population as weights.

To sum up, the results of these policy simulations show that the design of the national UI system has repercussions on the regional labor market. The impact on the regional distribution of unemployment differs across the policy experiments. If UI is made more generous by raising the benefit ceiling this will compress unemployment differentials, while if generosity increases because of an increase in the statutory replacement rate this will exacerbate regional unemployment differences.

8 Conclusions

We have presented new evidence on the unemployment effects of increasing UI benefit generosity. The empirical strategy has been to utilize the fact that the nationally imposed benefit ceiling causes actual UI generosity to vary regionally. This paper has thus used variations in the national UI rules to estimate the effects at the regional level. Hence, the estimates should not suffer from the potential policy endogeneity hampering studies using regional policy changes for identification.

The evidence suggests that benefit generosity increases unemployment. We view this evidence as fairly robust since the estimates are similar across alternative specifications. The magnitudes involved are rather substantial and appear to be relatively high compared to estimates available elsewhere in the literature. The estimates suggest that an increase in the (actual) replacement rate of 5 percentage points contributes to increasing unemployment by 25 percent.

We have also shown that the benefit ceiling may contribute to exacerbating regional unemployment differentials. Lowering the ceiling reduces benefit generosity more in high-wage regions. Since high-wage regions also tend to be low-unemployment regions, the result follows. Moreover, a reduction in the statutory replacement rate has the opposite effect. Given that a benefit ceiling exists, a reduction in the statutory rate will reduce benefit generosity more in high-unemployment regions. Thus, these simple policy experiments illustrate that national rules in social protection systems can have (perhaps unintended) repercussions at the regional level.

References

- Antelius, J. & A. Björklund (2000), How Reliable are Register Data for Studies of the Return on Schooling? An examination of Swedish Data, *Scandinavian Journal of Educational Research*, 44, 341-355.
- Atkinson, A. & J. Micklewright (1991), Unemployment Compensation and Labor Market Transitions: A Critical Review, *Journal of Economic Literature*, 29, 1679-1727.
- Card, D. & P. Levine (2000), Extended Benefits and the Duration of UI Spells: Evidence from the New Jersey Extended Benefit Program, *Journal of Public Economics*, 78, 107-138.
- Carling, K., B. Holmlund, & A. Vejsiu (2001), Do Benefit Cuts Boost Job Finding? Swedish Evidence from the 1990s, *Economic Journal*, 111, 766-790.
- Edin, P-A. & P. Fredriksson (2000), LINDA – Longitudinal INDividual DAta for Sweden, Working Paper 2000:19, Department of Economics, Uppsala University.
- Edin, P-A. & B. Holmlund (1995), The Rise and Fall of Solidarity Wage Policy? in R Freeman & L Katz (eds) *Differences and Changes in Wage Structures*, University of Chicago Press.
- Erikson, R. & R. Åberg (1987), *Welfare in Transition*, Clarendon Press, Oxford.
- Fredriksson, P. (1999), The Dynamics of Regional Labor Markets and Active Labor Market Policy: Swedish Evidence, *Oxford Economic Papers*, Vol. 51.
- Fredriksson, P. & B. Holmlund (2006), Improving Incentives in Unemployment Insurance: A Review of Recent Research, *Journal of Economic Surveys*, 21, 357-386.
- Fredriksson, P. & M. Söderström (2006), Do Unemployment Benefits Increase Unemployment – New Evidence on an Old Question, in M Söderström, *Evaluating Institutional Changes in Education and Wage Policy*, Economic Studies 95, Department of Economics, Uppsala University.
- Fredriksson, P. & R. Topel (2006), “Wage Determination and Employment in Sweden since the early 1990s”, manuscript, Department of Economics, Uppsala University.
- Holmlund, B. (1998), Unemployment Insurance in Theory and in Practice, *Scandinavian Journal of Economics*, 100, 113-141.

- Hornstein, A., P. Krusell, & G. Violante (2005), Unemployment and Vacancy Fluctuations in the Matching Model: Inspecting the Mechanism, *Federal Reserve Bank of Richmond Economic Quarterly*, 91, 19-50.
- Katz, L. & B. Meyer (1990), The Impact of the Potential Duration of Unemployment Benefits on the Duration of Unemployment, *Journal of Public Economics*, 41, 45-72.
- Krueger, A. & B. Meyer (2002), The Labor Supply Effects of Social Insurance”, in A. Auerbach & M. Feldstein (eds) *Handbook of Public Economics*, Vol. 4, North-Holland, Amsterdam, 2327-2392.
- Lalive, R. & J. Zweimüller (2004), Benefit Entitlement and Unemployment Duration: The Role of Policy Endogeneity, *Journal of Public Economics*, 88, 2587-2616.
- Lindgren, B. (2005), Lönespridning i näringslivet 2004, Svenskt Näringsliv
- Meyer, B. (1995), Lessons from the U.S. Unemployment Insurance Experiments, *Journal of Economic Literature*, 33, 91-131.
- Nickell, S. (1998), Unemployment: Questions and some Answers, *Economic Journal*, 108, 802-816.
- Nilsson, C. (1993), The Swedish Model: Labour Market Institutions and Contracts, in J. Hartoog & J. Theeuwes (eds) *Labor Market Contracts and Institutions*, Elsevier, Amsterdam.
- OECD (2004), *Employment Outlook*, Organization of Economic Cooperation and Development, Paris.
- Olli Segendorf, Å. (2003), Arbetsmarknadspolitiskt kalendarium II, Rapport 2003:9, Institute for Labour Market Policy Evaluation, Uppsala.
- Pigou, A. (1932), *The Theory of Unemployment*, MacMillan, London.
- Sebardt, G. (2005), *Redundancy and the Swedish Model – Swedish Collective Agreements on Employment Security in a National and International Context*, Iustus Förlag, Uppsala.
- Wadensjö, E. (1993), Socialförsäkringssystemets okända delar, in A. Lindbeck, T. Persson, A. Sandmo, & B. Swedenborg (eds) *Nya villkor för ekonomi och politik*, SOU 1993:16, Ministry of Finance, Stockholm.

Appendix A: Creating regional panel data

This appendix describes the construction of the regional panel data. Regional labor force composition, predicted wages and replacement rates are calculated from individual data. We use LINDA, a 3.35 percent representative sample of the Swedish population; see Edin & Fredriksson (2000). From this register, we select all individuals between the ages of 16–59, from 1970 to 2002.²⁵ In the early 1970s data contain roughly 130,000 individuals per year; in 2002 about 150,000. LINDA has a panel dimension which is very useful when constructing the data. If information is missing in one year, we can check if this information is available at another time point. This panel structure of the data is extremely valuable when comes to impute missing information on educational attainment as discussed below.

The regions correspond to the counties of Sweden. Between 1970 and 1996 there were 24 counties in Sweden. In 1996 two counties were merged, and in 1997 another three counties were merged.²⁶ Hence, from 1997 and onwards, there are 21 counties in Sweden. Since we also have data at the municipality level we can reconstruct the original 24 counties. We have used the municipality data to split the merging counties – thus creating 24 regions for the full time period.

The individual characteristics used in this paper are standard. Gender is identified by a female dummy. We define a set of age-group dummies for each five-year interval; the youngest group thus contains individuals aged 16 to 20, and the oldest group contains the ages 56 to 59. Marital status identifies married individuals. Immigrants are divided into three groups depending on the country of birth. We identify three groups: Nordic, Oecd, and non-Oecd immigrants. The definition of Nordic ancestry is obvious; the categorization into Oecd and non-Oecd immigrants is perhaps less obvious. We have used the following rule: individuals are classified as being of OECD origin if they were born in a country which was a member of the OECD in 1985. The only exceptions from

²⁵ We have to restrict the analysis to individuals younger than 60, since educational information is not available for those older than 59 for the full time period.

²⁶ In 1996, the county of Skåne was created by merging the counties of Malmöhus and Kristianstad. In 1997, the county of Västra Götaland was created by a merger of the counties of Älvsborg, Göteborg och Bohuslän, and Skaraborg.

this rule are Turkey – which is included among the non-OECD countries – and, of course, the Nordic countries.

Industry affiliation is defined by two-digit ISIC-codes, generating 33 industry dummies. The coding changed in 1993, but at the two-digit level it is possible to link the two coding systems. However, information on industry affiliation is missing for four years: 1974, 1976, 1977, and 1979. To deal with this issue we use the following simple rule: the information observed in 1975 is used for the individual also in 1974 and 1976; analogously, the information observed in 1978 is used also in 1977 and 1979.

Educational attainment is divided into three categories: compulsory, secondary, and tertiary schooling. Starting in 1991 educational information is available each year. Prior to 1991 we only observe educational attainment at two time points: in 1970 and 1990. We have used the following procedure to attach educational information to the individuals during 1971–89. If an individual is at least 25 years-of-age in 1970, education is assumed to be completed and the observation from 1970 is used to fill out the missing information during 1971–89. If the individual is younger than 25 in 1970, we use data from 1990. Different rules are used depending on educational attainment in 1990 and age at the time point of observation. For an individual who has completed tertiary education, we assign the level of attainment should this individual turn 25 during 1971–89. Should this individual turn 21 during this time period he or she assigned secondary schooling and when the individual is below age 21 he or she is assigned compulsory schooling. For an individual who has completed secondary schooling in 1990, we use this attainment level from the point when the individual turns 21 and onwards. Prior to turning 21, compulsory schooling is used as the highest attainment level. An individual who had completed compulsory schooling in 1990 is classified as having attained compulsory schooling from the time point when he or she enters our data.

Our key measures (expected wages and actual replacement rates) are constructed using earnings and wage information as described in the text, as well as the UI rules described below.

Finally, the regional panel is constructed by averaging over all individuals residing in a particular region. This gives us annual information on the composition of the regional

population as well as the key explanatory variables of interest. Ideally, we would have liked to calculate the characteristics of the regional labor force. But this was not possible since there was no indicator of labor force status in our data. However, the measurement error involved is likely to be small.

To these regional panel data we match information on unemployment. Regional unemployment data are defined for the age-category 16–64; they are collected from the Labor Force Surveys and the Labor Market Board. As the measure of unemployment we use the sum of open unemployment and participants in labor market programs as a share of the labor force.

Unemployment insurance

The design of the public unemployment insurance system has varied somewhat over time. There are two distinct time-periods – the first covers the period from 1974 to 1988, and the second the period 1989–2002.

Between 1974 and 1988, individuals were sorted into different benefit levels depending primarily on how much they earned. The various UI funds used different benefit ceilings. There was a national benefit ceiling, however, and the replacement rate could never exceed 91.7 percent of previous income. This implies that the maximum benefit level varied between individuals, depending on which particular UI fund the individuals were members of. Since we cannot observe membership in a particular UI fund, we use the “average maximum benefit level” as a proxy for the maximum level. This measure is reported in the Annual Financial Report of the Labor Market Board; it is calculated as a weighted average over individuals, where the weights are based on the number of members in a particular UI fund.

From 1989 and onwards, the construction of the UI system is more straightforward. An unemployed individual then receives a certain amount (in percent) of the previous wage, up to a maximum level.

Even though the design of the system has varied somewhat over time, we implement the rules in essentially the same way. An individual receives a benefit equal to the nominal replacement rate multiplied by foregone income, but the benefit can never exceed the ceiling. The ceiling is here defined from 1974–88 by the “average maximum benefit level” and from 1989–2002 as the “maximum benefit”.

Table A1 displays the benefit levels (in SEK per day), and the nominal replacement rates from 1974 to 2002 as observed on December 31st each year.²⁷ Column (1) displays the national benefit ceiling. Remember that this variable is only used as a measure of the benefit ceiling from 1989 to 2002.²⁸ As described above, the average maximum benefit level in column (2) is used between 1974 and 1988. Note that prior to 1977, the difference between the average maximum benefit level and the national benefit ceiling is substantial. But from then on the differences across UI funds become smaller, to eventually disappear completely.

Column (3) reports the date when the benefit ceiling was changed. Typically, this was not at the beginning of a calendar year. Hence, the annual benefit ceiling used in this paper is calculated in column (4) using the information in columns (1) to (3). For example, there is a change in the maximum benefit on July 1st 1979, and then the calendar year benefit ceiling (179.39), is simply calculated as the mean of the average benefit values observed on December 31st 1978 (171.16) and December 31st 1979 (187.62).

Column (5) shows the nominal replacement rate, column (6) reports when it was changed, and column (7) the annual average of the nominal replacement rate used in the analysis. Hence, the bold figures in column (4) and (7) are the primary input in our analysis. From these data, we calculate the wage cap by dividing the benefit ceiling with the nominal replacement rate. The wage cap and the nominal replacement rate are shown in Figure 3.

²⁷ Information is taken from the annual reports of the Labor Market Board.

²⁸ From 2001 and onwards, the benefit ceiling decreases after 100 days of unemployment, as shown by the figures in the brackets. In this paper, we use only the maximum during the first 100 days.

Table A1 The unemployment insurance system, 1974–2002

Year	National benefit ceiling SEK/day (Dec 31) (1)	Average benefit ceiling SEK/day (Dec 31) (2)	Date of change in ceiling (3)	Benefit ceiling SEK/day (calendar year) (4)	Nominal repl. rate (Dec 31) (5)	Date of change in nominal repl. rate (6)	Nominal repl. rate (calendar year) (7)
1974	130	98,07		98.07	0.917		0.917
1975	130	116,58		116.58	0.917		0.917
1976	160	122,22	July 1 st	119.4	0.917		0.917
1977	160	151,76		151.76	0.917		0.917
1978	180	171,16	July 1 st	161.46	0.917		0.917
1979	195	187,62	July 1 st	179.39	0.917		0.917
1980	195	192,19		192.19	0.917		0.917
1981	210	206,80	April 1 st	203.1475	0.917		0.917
1982	230	227,66	July 1 st	217.23	0.917		0.917
1983	280	278,80	Jan 1 st	278.8	0.917		0.917
1984	300	298,87	July 1 st	288.835	0.917		0.917
1985	315	314,48	July 1 st	306.675	0.917		0.917
1986	360	359,20	July 1 st	336.84	0.917		0.917
1987	400	400	July 1 st	379.6	0.917		0.917
1988	425	425	July 4 th	412.5	0.917		0.917
1989	450		Jan 2 nd	450	0.9	Jan 2 nd ; 90%	0.9
1990	495		Jan 1 st	495	0.9		0.9
1991	543		Jan 7 th	543	0.9		0.9
1992	564		Jan 6 th	564	0.9		0.9
1993	564		Jan 4 th ; 598 July 5 th ; 564	581	0.8	July 5 th ; 80%	0.85
1994	564			564	0.8		0.8
1995	564			564	0.8		0.8
1996	564			564	0.75	Jan 1 st ; 75%	0.75
1997	580		Dec 29 th	564	0.8	Sep 29 th ; 80%	0.7625
1998	580			580	0.8		0.8
1999	580			580	0.8		0.8
2000	580			580	0.8		0.8
2001	680 (580)		July 2 nd	630	0.8		0.8
2002	730 (680)		July 1 st	705	0.8		0.8

Notes: Column (4) is based on cols. (2) and (3) during 1974–88, and cols. (1) and (3) during 1989–2002. Column (7) is based on columns (5) and (6) throughout the time period.

Appendix B: Coefficient estimates

Table B1 shows coefficient estimates corresponding to our preferred specification, i.e., column (2) in Table 3.

Table B1 Estimates on a selection of observed regional control variables

	Coefficient estimate (standard error)
Covered by collective agreement with supplementary UB	1.74** (0.637)
Female	2.58 (2.01)
Married	-1.99* (1.12)
Nordic	-0.784 (3.45)
Oecd	-3.57 (7.51)
non-Oecd	-4.79 (3.20)
Secondary schooling	-5.06** (1.84)
Tertiary schooling	-3.85* (2.13)
Age 21-25	3.26 (2.24)
Age 26-30	4.26 (2.84)
Age 31-35	6.89** (3.16)
Age 36-40	8.03** (3.16)
Age 41-45	8.68** (2.58)
Age 46-50	9.05** (3.02)
Age 51-55	11.63* (3.31)
Age 56-59	10.51* (3.04)
Region-specific fixed effects	Yes
Region-specific trends	Yes
Time effects	Yes
Overall R ²	0.981
# observations	696

Notes: The regressions also include a constant and industry employment shares. Regressions are weighted by population. Standard errors, reported in parentheses, are clustered by county. Significance levels: * = 10%, and ** = 5%.

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

Rapporter/Reports

- 2008:1** de Luna Xavier, Anders Forslund and Linus Liljeberg "Effekter av yrkesinriktad arbetsmarknadsutbildning för deltagare under perioden 2002–04"
- 2008:2** Johansson Per and Sophie Langenskiöld "Ett alternativt program för äldre långtidsarbetslösa – utvärdering av Arbetstorget för erfarna"
- 2008:3** Hallberg Daniel "Hur påverkar konjunktursvängningar förtida tjänstepensionering?"
- 2008:4** Dahlberg Matz and Eva Mörk "Valår och den kommunala politiken"
- 2008:5** Engström Per, Patrik Hesselius, Bertil Holmlund and Patric Tirmén "Hur fungerar arbetsförmedlingens anvisningar av lediga platser?"
- 2008:6** Nilsson J Peter "De långsiktiga konsekvenserna av alkoholkonsumtion under graviditeten"
- 2008:7** Alexius Annika and Bertil Holmlund "Penningpolitiken och den svenska arbetslösheten"
- 2008:8** Anderzén Ingrid, Ingrid Demmelmaier, Ann-Sophie Hansson, Per Johansson, Erica Lindahl and Ulrika Winblad "Samverkan i Resursteam: effekter på organisation, hälsa och sjukskrivning"
- 2008:9** Lundin Daniela and Linus Liljeberg "Arbetsförmedlingens arbete med nystartsjobben"
- 2008:10** Hytti Helka and Laura Hartman "Integration vs kompensation – välfärdsstrategier kring arbetsoförmåga i Sverige och Finland"
- 2008:11** Hesselius Patrik, Per Johansson and Johan Vikström "Påverkas individen av omgivningens sjukfrånvaro?"
- 2008:12** Fredriksson Peter and Martin Söderström "Vilken effekt har arbetslöshetsersättningen på regional arbetslöshet?"

Working papers

- 2008:1** Albrecht James, Gerard van den Berg and Susan Vroman "The aggregate labor market effects of the Swedish knowledge lift programme"
- 2008:2** Hallberg Daniel "Economic fluctuations and retirement of older employees"
- 2008:3** Dahlberg Matz and Eva Mörk "Is there an election cycle in public employment? Separating time effects from election year effects"
- 2008:4** Nilsson J Peter "Does a pint a day affect your child's pay? The effect of prenatal alcohol exposure on adult outcomes"
- 2008:5** Alexius Annika and Bertil Holmlund "Monetary policy and Swedish unemployment fluctuations"
- 2008:6** Costa Dias Monica, Hidehiko Ichimura and Gerard van den Berg "The matching method for treatment evaluation with selective participation and ineligible"

- 2008:7** Richardson Katarina and Gerard J. van den Berg “Duration dependence versus unobserved heterogeneity in treatment effects: Swedish labor market training and the transition rate to employment”
- 2008:8** Hesselius Patrik, Per Johansson and Johan Vikström “Monitoring and norms in sickness insurance: empirical evidence from a natural experiment”
- 2008:9** Verho Jouko, “Scars of recession: the long-term costs of the Finnish economic crisis”
- 2008:10** Andersen Torben M. and Lars Haagen Pedersen “Distribution and labour market incentives in the welfare state – Danish experiences”
- 2008:11** Waldfogel Jane “Welfare reforms and child well-being in the US and UK”
- 2008:12** Brewer Mike “Welfare reform in the UK: 1997–2007”
- 2008:13** Moffitt Robert “Welfare reform: the US experience”
- 2008:14** Meyer Bruce D. “The US earned income tax credit, its effects, and possible reforms”
- 2008:15** Fredriksson Peter and Martin Söderström “Do unemployment benefits increase unemployment? New evidence on an old question”

Dissertation series

- 2007:1** Lundin Martin “The conditions for multi-level governance: implementation, politics and cooperation in Swedish active labor market policy”
- 2007:2** Edmark Karin “Interactions among Swedish local governments”
- 2008:1** Andersson Christian “Teachers and Student outcomes: evidence using Swedish data”