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Three essays on unemployment insurance

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Contents

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Introduction and summary

Unemployment insurance insures individuals against income loss resulting from loss of employment. Among risk adverse individuals, spread of risk through an insurance system is, all other things equal, socially beneficial. (see e.g. Laffont & Martimort, 2002).

The generosity of a system of unemployment insurance can be described along three lines: the requirements for eligibility, the replacement rate, and the potential benefit duration. The requirements for eligibility define the conditions, often in terms of job durations or earlier work income, that the individual must satisfy to be entitled to unemployment insurance at all. The replacement rate is the share of the wage that will be replaced by UI benefits in case of unemployment, and the potential benefit duration describes for how long the benefits can last.

Unemployment insurance will alter the economic incentives, and the scope of action of the individuals covered by it. This thesis investigates how the various features of the unemployment insurance system affect the individuals in the labor market, and their transitions into and out of unemployment. The first chapter focuses on potential benefit duration and eligibility criteria, and their effects both on entry into- and exit from unemployment. The second chapter studies the effect of eligibility criteria on unemployment entry, by using an identification strategy different from the one applied in the first chapter. In the first chapter, a difference in differences strategy is applied, while a regression discontinuity approach is used in the second. Chapter 3 focuses on the effect of the replacement rate on entry into unemployment. The main findings indicate that the risk of entering registered unemployment increases considerably with eligibility to unemployment insurance, but that this effect primarily reflects higher registration propensity (conditional on job loss) and only a slight increase in the probability of job loss. The risk of entering unemployment is unaffected by changes in potential benefit duration within the 12 – 36 months range. If a person is eligible to UI, variation in the replacement rate also seems to be of less importance in explaining unemployment entry. An exception might be among workers in small firms. *Exit* from unemployment is sensitive both to changes in eligibility status and potential benefit durations.

Unemployment insurance will affect unemployment duration through two distinct channels, one implying a social cost, and the other a social benefit (Chetty, 2008). First, the fact that many

aspects of the effort induced to find and keep a job are unobservable introduces a distortion of the relative price of leisure and consumption. This is the source of moral hazard. Unemployment insurance creates a wedge between private and social marginal costs of leisure. This causes individuals to spend too much time in the unemployment state. Secondly, due to credit market failures, many of the actors in the labor market are liquidity constrained. They have no savings, and are not able to lend money to cover necessary expenditures in case of a temporary fall in income i.e.: transitory income shocks cannot be smoothed relative to permanent income. Unemployment insurance benefits provide cash in hand among individuals hit by unemployment, which makes it possible to keep searching for a productive job match a little longer, instead of accepting the first offer that emerges. To the extent that unemployment insurance brings the behavior of non-liquid individuals to be more similar to what they would have done if they were able to finance their job search by the use of their own savings or loans, the effect of unemployment insurance on individual behavior is actually socially beneficial. Hence, when the findings in this thesis suggest that modifying the parameters in the UI system in a more generous direction will increase unemployment inflow and reduce unemployment exit, it is not necessarily a sign of distorted incentives causing social inefficiencies. Sometimes quitting ones job to search for a better one or to wait a little longer for the perfect job offer can be socially beneficial. The liquidity channel might be more relevant for explaining the effect of UI features on unemployment exit than unemployment entry though. Employed individuals are probably less liquidity constrained than unemployed individuals.

Another feature that the chapters in this thesis have in common is the application of quasi-experimental methods for identification of causal relationships. A quasi-experiment is a research design that lacks the key ingredient of a true experiment: random assignment of treatment. If assignment of treatment is not random, a pure comparison of averages between the treatment group and the control group will mix up the effect of treatment and the effect of selection. If the selection process is driven by unobservable variables, which is quite often the case, controlling for observable variables will still leave us with a selection bias. A quasi experiment mimics the randomized trial by applying a source of variation in the explanatory variable that, by assumption, keeps other variables balanced (Angrist & Pischke, 2009).

The suitable choice of quasi-experimental design will depend on the available source of manipulation of treatment, and the validity of the resulting estimate will depend on the validity of a set of identifying assumptions. In the first chapter in this thesis, a difference-in-differences approach is applied to study an unemployment reform that affected different income groups at different times, to estimate the effect of the changes in the institutional settings. In the second chapter, the minimum income threshold for unemployment insurance eligibility is exploited in a regression discontinuity setting. The third chapter builds on the fact that the income basis for the unemployment insurance benefits is limited to income earned prior to the last turn of the year. This time limit, set to ease the calculation of UI entitlements among UI applicants since income data can easily be collected from last year's tax registers, creates variation in the replacement rate that is not random in the strict sense, but probably not correlated with unobservable variables.

The quasi-experimental approach has many drawbacks. First, the identifying assumptions are often not testable, and hence, the internal validity of the results might be questioned. Secondly, since the researcher is not able to manipulate the explanatory variable himself, he is left with the variation created by the specific institutional setting. The sample affected by this quasi-random variation might also not be representative for the population of interest, and therefore external validity might be limited. The advantage of a quasi-experimental approach to the identification of causal relationships is that the alternative, a randomized trial, is likely to be impossible, costly, and/or unethical to conduct. In addition, if we are interested in long term effects of a treatment, we will have to wait for the long term results to be realized before the data can be analysed. Within social research, relying on quasi experimental approach is often our best shot.

This thesis contributes to the literature by providing a comprehensive investigation of the effect of unemployment insurance on unemployment entry. The literature on this topic is growing, but still small. As much of this literature is based on survey data, another contribution is the use of administrative register data for this purpose. The way the unemployment insurance system affects exit from unemployment is more thoroughly examined in the economics literature, but given limitations to external validity, analysis applying new ranges of variation in the explanatory variables, based on data form another institutional context will always contribute to a better understanding of the phenomenon.

The rest of this introduction summarizes the three main chapters of my thesis.

Analyses of an unemployment insurance reform

Based on administrative register data from Norway, this chapter studies the effects of unemployment insurance (UI) eligibility and potential benefit duration (PBD) on entry into unemployment, and exit from unemployment. Reforms in the Norwegian unemployment insurance system that took place in 2003 and 2004 are applied for identification. The reforms involved an unambiguous reduction in the generosity of the UI system. Depending on income history, the policy changes affect individuals differently, and at different points in time. This allows for a difference in differences analysis.

When analysing the effects of the reforms on entry into unemployment, I estimate a logit model. The results indicate that changes in PBD within a 12 – 36 months range do not affect entry into unemployment. Loss of eligibility affects the entry into registered unemployment quite strongly, but to some extent, this may reflect that given loss of employment, the incentives to register at the unemployment office is stronger for persons entitled to UI. The effect is weaker and less robust when a broader, activity based definition of unemployment, not derived from registration, is used. When studying the transitions out of unemployment, I estimate a mixed proportional hazards rate model with competing risk. Unemployment spells may terminate with a transition to re-employment, or to health related benefits or education. The results suggest that loss of eligibility increases the hazard back to employment quite strongly the first months of the unemployment spell, and that the reduction in PBD increases the hazard to employment at the new timing of benefit exhaustion. Simulations of the model indicate that each months reduction in PBD reduces the duration until reemployment by 2-6 days.

I also use the simulations to study how the destination state after unemployment is affected by the reforms, but find no significant effects on the share of the unemployment spells that ends with a transition to employment eventually.

Tempted into joblessness?

In this chapter, the effect of unemployment insurance (UI) eligibility on entry into unemployment is investigated. The Norwegian unemployment insurance system is universal, and covers everyone with a certain attachment to the labor market, defined in terms of earlier work income. Eligibility for unemployment insurance is conditional on income previous calendar years exceeding certain levels. This is a setting well suited for a regression discontinuity design.

The analysis is based on individual register data, and two definitions of entry into unemployment are applied. The first definition is entry into the unemployment registers. The results from a sharp regression discontinuity analysis indicate that the monthly risk of transiting from employment the unemployment register increases by 0.30 percentage points (pp) as an effect of UI eligibility. To test whether this effect only represents an increase in the probability of entering the unemployment register given that a job match is terminated, or if UI eligibility also cause separation from employment, a wider activity based definition, not involving information from the unemployment registers, is also applied. The monthly risk of entering unemployment defined this way increases by around 0.15 pp, and larger bandwidths are needed to obtain statistically significant effects.

A fuzzy design, as well as various limitations to the sample is also applied, for the purpose of robustness testing. The results seem to be quite robust.

The unemployment insurance replacement rate and job durations

This chapter investigates the effect of the unemployment insurance (UI) replacement rate on entry into unemployment. In Norway UI benefits are proportional to earlier income, but only income from the three previous calendar years are included in the income basis for UI. This institutional setting implies that two individuals with the same wage level and job duration, who entered the labor market within the last two years, will face different UI replacement rate in case of unemployment, dependent on their exact within-year timing of entry into the labor market.

This random assignment-like variation is exploited to analyse the causal effect of the UI replacement rate on the employment duration among newcomers in the labor market. A mixed proportional hazard rates model is applied to individual register data. Two different samples of labor market entrants are used: Immigrants and newly educated.

The findings suggest that variation in the replacement rate on the extensive margin (having a replacement equal to zero vs. facing a positive one, i.e. eligibility for UI) is statistically significant in explaining unemployment entry, while marginal variation in the replacement rate given eligibility is not. When allowing for heterogeneous responses to variation in the replacement rate, workers in very small companies might seem to be sensitive also to continuous

variation in the replacement rate. This may indicate that the ties between the employers and the employees are tighter in very small firms.

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Chapter I:

Analyses of an unemployment insurance reform

Abstract

This chapter studies the effects of unemployment insurance (UI) eligibility and potential benefit duration (PBD) on entry into unemployment, and exit from unemployment. Reforms in the Norwegian unemployment insurance system that took place in 2003 and 2004 are applied for identification. The reforms involves an unambiguous reduction in the generosity of the UI system. Depending on income history, individuals are affected differently, and at different points in time. This allows for a difference in differences analysis.

The results indicate that changes in PBD within the range from 12 to 36 months do not affect entry into unemployment. Loss of eligibility affects the entry into registered unemployment quite strongly. The effect is weaker and less robust when a broader, activity based definition of unemployment is used. The analyses of exit from unemployment indicate that each month's reduction in PBD reduces the duration until reemployment by 2-6 days.

1. Introduction

In 2003 and 2004, substantial reforms in the Norwegian system of unemployment insurance (UI) were implemented. The income requirement for eligibility was increased, and the potential benefit duration (PBD) was reduced by one third. The reforms involve an unambiguous reduction in the generosity of the UI system. The purpose of this chapter is to analyse the effects of these changes on transitions in the labor market.

There are two research questions in this chapter. The first one deals with the process of unemployment entry. Do the reforms affect the transition rate from employment and into insured or registered unemployment? If they do, can we find an effect on job durations, or is only the balance between insured and uninsured unemployment changed?

The second research question relates to the process of exit from unemployment. Do the reforms affect the transition rate back to employment, and what are the effects on average duration until re-employment?

I will approach these questions by analysing individual administrative register data. The reforms affect the workers differently, and at different points in time, depending on work income over the past three calendar years. This will be exploited by using a difference in differences framework, with alternative control groups. The method is quasi-experimental.

The chapter contributes to the existing literature by offering a comprehensive evaluation of UI eligibility and PBD, both on unemployment entry and exit, based on the same identification sources. In addition to registered and insured unemployment, it focuses on the distinction between employment and non-employment. Entry into non-employment is studied, in addition to entry into registered/insured unemployment, and individuals that become unemployed are followed until they are observed as employed, even if they exit the unemployment register. It also addresses the question of destination state: changes in the institutional setting might affect not only the timing of a transition, but also the type of transition.

My main findings are that reduction in the potential benefit duration within a 12 – 36 months range does not reduce the probability of making a transition from employment to unemployment insurance take-up, but reduces the average duration from UI take-up to employment with 2 to 6 days per month of reduction in PBD. Eligibility loss reduces the hazard rate from employment into registered unemployment by about 20 %. Half of this effect seems to be driven by a reduced tendency to leave employment, while the other half by a decrease in the registration propensity given that unemployment has taken place. Among those that do register as unemployed, loss of eligibility reduces the average duration to re-employment by 1.6 months.

The structure of the chapter is as follows: Section 2 provides an overview of the existing literature on how features of the unemployment insurance system can affect entry into unemployment, and the job finding rates. Section 3 describes the Norwegian system of unemployment insurance, and the reforms studied in this chapter, in detail. Section 4 presents the data and descriptive statistics. Section 5 and 6 presents my results on unemployment entry and exit respectively, and a simulation of the model, to present the effects of the reforms in more policy relevant ways. Section 7 concludes.

2. Related literature

I will start this section by discussing potential mechanisms that can be relevant in explaining an effect of UI generosity on unemployment entry and follow up by discussing some of the empirical contributions on the topic. I will then discuss the theoretical and empirical contributions on the topic of exit from unemployment

2.1. Unemployment insurance and entry into unemployment

The earliest contributions on the relationship between UI and separation from employment are related to the experience rating system in the US: Firm taxes are related to the previous incidences and durations of the employees of the firm, but to an insufficient degree. The maximum and minimum level of taxes internalizes only parts of the costs related to the use of UI benefits. Feldstein (1976) develops a theoretical model that shows that this subsidizing of UI make firms respond to drops in product demand by laying off workers temporarily, under an implicit contract between the employer and the employee saying that the worker will be recalled before their benefits are exhausted. Norwegian firms face no experience rating. Employers bears no financial costs related to unemployment among former employees, so if this mechanism is relevant in the US, it is likely to be so also in Norway.

If existing job matches are hit by idiosyncratic shocks to productivity or product demand, UI generosity will affect the probability of a job match to be terminated through the outside options of the employee. If wages can be renegotiated, reduced UI generosity will increase the bargaining space, and in case of a negative shock to productivity, the employee will be more willing to lower his wages to insure that the job match is still beneficial for both parties (Pissarides, 2000). Transitions out of the labor market as a response to a positive shock to the value of non-market activity (home production, leisure etc.), in line with (Flinn and Heckman, 1982), might also occur more often among well insured person. Job search is hard to observe and document. Hence, seemingly unemployed individuals who receive UI benefits might be spending a considerable amount of time in the non-market state. If this is the case, entitlement to more generous UI entitlements might be seen as a shift to the right in the distribution of the instantaneous value of non-market activity.

The working environment act requires that a note of dismissal is given some time before the actual destruction of the job match takes place (*The Working Environment Act, §15-3*, 2005). We could therefore also expect to see an effect of UI generosity on unemployment entry simply from the fact that individuals with no or less generous UI entitlements will search more intensively for a new job and/or reduce their reservation wage during their time of notice. Hence, workers with less generous UI might be more likely to have a new job at hand before the old employment relationship ends. Gutierrez (2012) shows that *any perceived risk* of a layoff will make on-the-job-search activity sensitive to PBD. Hence workers will engage in more on-the-job search if they are entitled to less generous UI benefit, either in terms of replacement rate or PBD as long as they are at some risk of losing their job.

Given strong employment protection, a generous UI system can also be a way of "buying off" less productive workers. The Norwegian Working Environment Act (Arbeidsmiljøloven) states that "Employees may not be dismissed unless this is objectively justified on the basis of circumstances related to the undertaking, the employer or the employee" (*The Working Environment Act (Official english translation of Arbeidsmiljøloven) §15-7*, 2005). A severe breach of the written contract of employment may thus be a legitimate reason for a layoff, but these actions might be hard to document, and the limits between acceptable and non-acceptable breaches may always be questioned. Laying off an unproductive worker might be less hazardous if this person is entitled to (generous) UI benefits than if not, as the risk of legal conflict is smaller, and the firm might be spared from a reputation as an uncooperative employer (Rebollo-Sanz, 2012).

The existing empirical contributions on the question of whether PBD affects entry into unemployment are based on studies from Germany, Austria, and France, where PBD was changed in some age groups, and left unchanged in others.

Fitzenberger and Wilke (2009) study how the increases in PBD in Germany during the 1980s affect entry into non-employment, and entry into periods between jobs[1]. They find that the oldest workers respond to the changes by entering non-employment more frequently, but they find no effects on entry into periods between jobs, neither among older or younger workers. The same

[1] Periods between jobs are defined as non-employment periods conditioned on the person returning to employment within two years.

pattern is found by Dlugosz, Stephan, and Wilke (2013), who study the reductions in PBD that took place in Germany in 2006. Baguelin and Remillon (2014) study the effects of a sharp reduction in PBD that took place among older workers in France in 2003, and by Tuit and van Ours (2010) study the removal of a PBD discontinuity at the age of 57.5 in the Dutch UI system. Both find that only the oldest workers are affected by the changes. Winter-Ebmer (2003) study a reform that increased PBD from 52 to 209 weeks for workers aged above 50, who lived in specific Austrian regions. The author does not explicitly address the question of age dependent effects, but as the treated group is relatively old, the findings strengthen the impression that the most important effect of an increase in PBD is to give more workers the possibility of exiting the labor market earlier.

There is also evidence that PBD has an effect on younger workers. Lalive, Van Ours, and Zweimuller (2011) find effects of increases in PBD also among workers in their 40s. In 1989, PBD increased from 30 to 39 weeks among workers in this age group. Workers aged above 50 experienced an increase from 30 to 52, while younger workers, which are used as a control group in a difference in differences setting, kept their PBD unchanged. The authors analyse quarterly data, and find that the risk of entering the unemployment register from one quarter to the next increases by 0.1 percentage points as a result of the increase in PBD from 30 to 39 weeks. As the average quarterly risk of entering unemployment is 1 %, the results indicate that an increase in PBD of about one third, from 30 to 39 weeks increases the unemployment inflow rate by 10 %.

The empirical literature on the effect of PBD on unemployment entry thus suggests that PBD affects the timing of exit from the labor market. UI seems to be used as a bridge between employment and retirement. There is disagreement though, on whether or not the effects are also relevant among younger workers. My analysis contributes to this discussion by focusing exclusively on workers that are too young to bridge employment and retirement with a UI spell. Another contribution is that I study variation in PBD on another margin than Lalive et al. (2011) Both post- and pre-reform PBD in Norway is longer than the post reform PBD in the reform studied by Lalive et al. (2011). The risk of actually facing benefit exhaustion is lower when PBD is longer, and this may explain why the effects of the changes in PBD on unemployment entry seem to be important in Lalive et.al (2011), but absent in my study.

In contrast to the literature focusing on the effect of PBD on unemployment entry, the papers that have investigated the effect of UI eligibility on unemployment entry are typically based on survey data. Eligibility is normally conditioned on duration of job spells prior to unemployment, with a clearly defined threshold. In Canada, eligibility to UI is conditional on the job duration having lasted for a certain number of weeks. Prior to 1990, this limit varied between 10 and 14 weeks, depending on region. Christofides and McKenna (1995) use longitudinal survey data from 1988 and 1989, and their estimates indicate that the hazard out of employment increases with 47% the week that eligibility is obtained.

In 1990 this Canadian job duration requirement was set to 14 weeks over the entire country. Green and Riddell (1997) study the regions that changed from 10 to 14 weeks. They find that the concentration of job durations reflects the new job duration requirement in the region. Green and Sargent (1998) find that the tailoring of job lengths to meet requirements for UI eligibility is mostly a phenomenon in the seasonal sector.

Rebollo-Sanz (2012) compares the hazard rates out of employment for workers who, due to earlier employment periods, become eligible for UI after 6 months, to workers who become eligible for UI after 12 months. Both groups have exit spikes around 6 and 12 months, but the spike at 12 months is much higher for the individuals that needed to stay employed for 12 months to become eligible. The effect is much stronger for temporary contracts than for permanent contracts and for women than for men. The effect is only present on the lay-off hazard. The author finds no effect on the quit hazard.

All in all, eligibility to UI seems to be important for understanding entry into unemployment. As the contributions mentioned above base their identification on requirements for eligibility in terms of job duration prior to unemployment, one might suspect though, that requirements for eligibility affect the *timing* of a job separation, more than the *existence* of it. If the effect of eligibility is mainly caused by timing of unemployment entries that would have happened anyway, the adaptation imposes less incentive costs on the UI system, than if eligibility actually causes employment relations to terminate.

This chapter contributes to the literature on UI eligibility and unemployment entry by studying variation in eligibility caused by work income in the *previous calendar year.* In most cases, this factor cannot be manipulated by minor timing adjustments of the ongoing employment spell.

Because of this, the estimate of the effect obtained is more relevant for the discussion of disincentive costs of UI eligibility, than the previous studies mentioned.

2.2. Unemployment insurance and job finding

There is a large theoretical and empirical literature on how features of the unemployment insurance system affect exit from unemployment and the job finding rate. A partial equilibrium search model with a non-stationary environment (meaning that the PBD is limited) predicts that the hazard from unemployment will increase until the time of benefit exhaustion, and stay constant thereafter. If the PBD is reduced, the hazard rate is going to increase faster than before, but stabilize earlier, at the new time of benefit exhaustion, and at a lower level, due to the entitlement effect (Mortensen, 1977). An unemployed individual without entitlements will have a constant hazard throughout the unemployment spell, at the level corresponding to that of entitled individuals at the time of benefit exhaustion. From these predictions, we should expect to see that loosing UI eligibility affects the hazard rate most strongly in the beginning of the unemployment spell, and that the reduction in PBD has the strongest effect on the hazard out of employment at the new time of benefit exhaustion.

The general findings in the empirical literature are that exit from unemployment increases around the time of benefit exhaustion, but the strength of the effects depends on how exit from unemployment is defined. If unemployment duration is defined as time on UI, or time in the unemployment register, the effects are larger than if unemployment duration is measured as time until reemployment (Card, Chetty, and Weber, 2007).

Van Ours and Vodopivec (2006) analyse a Slovenian reform of the UI system that took place in October 1998. PBD, dependent on tenure in the labor market, was substantially reduced for almost everyone. They find that for a 30 year old male worker in good health, the reduction in PBD from 12 to 6 months reduced median unemployment with 1.1 months. This corresponds to an effect per month of PBD reduction of 5-6 days. The female counterpart is more strongly affected: The median duration of unemployment is reduced with 17 days for every month's reduction in PBD.

Schmieder and Bender (2012) study the German age thresholds for PBD exhaustions using a regression discontinuity design. They define the duration until reemployment as the time from the

beginning of a UI spell to the date of a new registered employment spell, capped at 36 months. They use unemployment spell from the period July 1987 to 1999, during which the German UI system was stable. Duration is regressed locally on an indicator for being above the age threshold, and linear functions of age are allowed to have different parameters on each side of the threshold. They find than in increase in PBD of one month increases the expected duration until reemployment by 3-4 days.

The twist in the present chapter, in contrast to the contributions mentioned, is the competing risk setup combined with the fact that individuals stay in the risk set even if they leave the unemployment register. This is motivated by the wish to also investigate whether the reforms affect the destination state. Shortening the period of subsidized search could both increase and decrease the share of the unemployment spells that do exit to employment eventually. Modelling exit from the unemployment register as an absorbing destination state, like van Ours and Vodopivec (2006) do, implies that people are no longer in the risk set of finding a new job if they leave the register. As the shortening of PBD is very likely to accelerate the exit from the register, and there is no reason to expect register-leavers to be unavailable for employment, this modelling procedure would underestimate the share of the spells that end up in employment after the reform, and the framework would not be suited for a discussion of destination state.

3. The Norwegian system of unemployment insurance.

The Norwegian system of unemployment insurance is compulsory and regulated by the National Insurance Act[2]. It is financed through the general tax system and administered by the Norwegian Labor and Welfare Administration. It is intended to "...partly cover income loss caused by unemployment" (*The National Insurance Act (Folketrygdloven) §4-1*, 1997).

The main condition for UI eligibility is that work is involuntarily lost, either entirely or partly. Recipients of UI have to be able and willing to accept any job offer, independent of location (within Norway), or whether it is part time or not, as long as it is decently paid (on tariff)[3]. They have to register with the Job Centre (the Norwegian Labor Administration), and confirm every

[2] My translation of «Folketrygdloven».

[3] If job offers that, by a UI administrator are considered as suitable are turned down are, benefits will be put in quarantine for a period of time.

second week that they are still searching for a job. Eligibility is also conditioned on work income[4] over the previous calendar year(s) exceeding certain levels. These levels are defined in terms of the "basic amount", an amount regulated annually as a function of wage growth. In January 2014 the value of one basic amount was NOK 85 245, or around 10 000 €.[5] If work income in the previous calendar year (NB: Not the previous 12 months) exceeds 1.5 basic amounts (Prior to January the 1st 2003, this limit was 1.25 basic amounts) *or* the sum of work income from the past three calendar years exceeds 3 basic amounts, one is covered by unemployment insurance (*The National Insurance Act (Folketrygdloven) §§4-3 - 4-8*, 1997).

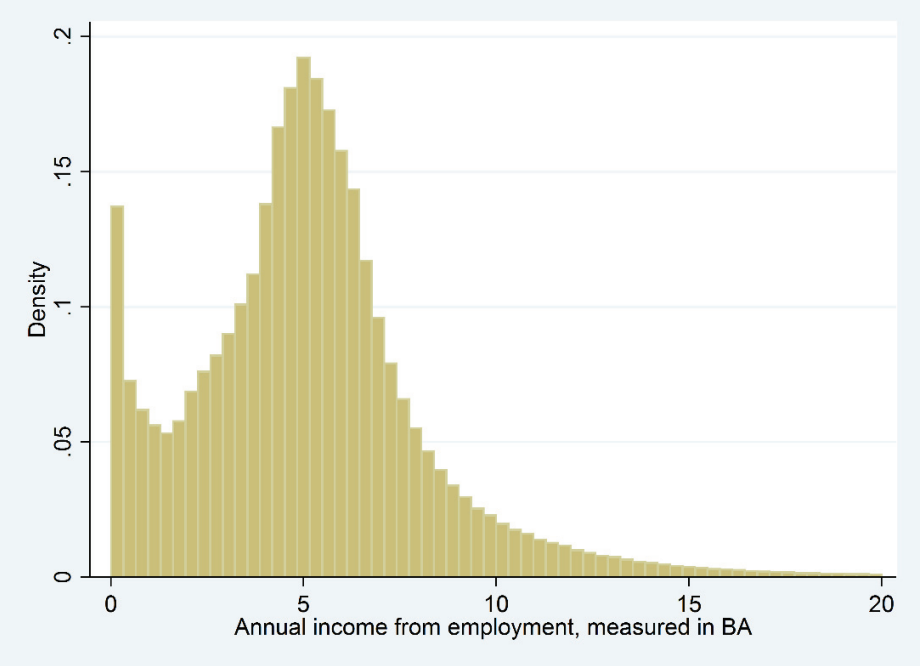
The yearly amount of UI is 62,4 % of the income basis[6]. Conditional on the eligibility income criteria being met, the income basis is the *maximum* of income[7] previous calendar year (conditional on this amount being above 1.5 basic amounts), and the average income over the past three calendar years (conditional on this amount being above 1 basic amounts, up to a ceiling at 6 basic amounts. Income above 6 basic amounts is not included in the income basis (*The National Insurance Act (Folketrygdloven) §§4-11 - 4-12*, 1997).

Potential benefit duration (PBD) also depends on the income basis. If the income criterion for UI eligibility is satisfied, but the income basis is below 2 BA, PBD is 52 weeks. (Prior to January 1st, 2004 PDB was 78 weeks for this group.) If the income basis is above 2 basic amounts then PBD is 104 weeks (156 weeks prior to January 1st, 2003) (*The National Insurance Act (Folketrygdloven) §4-15*, 1997).

Special rules apply to repeat users. First of all, if a person starts a UI spell in year t, work income from year $t-1$, $t-2$ and $t-3$ can not be used as income basis for any subsequent UI period (*The National Insurance Act (Folketrygdloven) §4-4*, 1997). Secondly, if a UI period is interrupted by a break (net of weeks spent participating on labor market measures, of less than 52 weeks) one can automatically return to the old UI spell, with the same degree of exhaustion as

[4] From January 1st, 2011, sickness allowance from pregnancy related illness and parental benefits (given to parents on parental leave) are given the same status as work income.
[5] Figure 11 in Appendix A displays the yearly growth in BA compared to growth in wages and Consumer price index.
[6] Unemployed persons with dependent children are also entitled to additional child support. The size of this supplement is NOK 85 per child per week, but UI included child support cannot constitute more than 90 % of the income basis.
[7] Additional to income from work, the income basis also includes sickness allowance, parental benefits, and unemployment insurance. From July 1st 2005, benefits given when caring for close relatives are also included.

Figure 1: The distribution of work income in 2006



Note: A histogram over the work income of individuals aged between 25 and 59, with income from employment between 0 BA and 20 BA in 2006. N=1 911 876.

before the break. However, in case of a job period lasting at least twelve weeks, one can choose to start a new UI spell, with income not erased at the beginning of the previous UI spell as income basis (*The National Insurance Act (Folketrygdloven) §4-16*, 1997). Around the turn of the year, this rule can be beneficial for the unemployed, even in the case of very short and low paid temporary job spells.

Figure 1 displays the distribution of income from employment in 2006, measured in basic amounts (BA)[8]. The sample on which this figure is based includes every person aged between 25 and 59 in 2006, with an income from employment that is positive, but below 20 BA. 11.7 % of the sample have work income below 1.5 BA. Since eligibility to UI can be obtained also from having average income over the past three years above 1 BA, Figure 1 indicate that the share of workers covered by UI in 2007 is above 88.3 %. 32.1 % have income from employment above 6 BA, the maximal benefit basis.

[8] We see that the density is very high at the smallest levels of income. When studying the histogram of total income, including also transfers from the government and capital income (this figure is not included in this paper), this pattern is not present. This indicates that the relatively large share of individuals with very low income from employment tend to have additional income sources.

Many job searchers register, and stay registered, as unemployed at the Job Centre even if they are not eligible for unemployment insurance. Table 1, which is based on the unemployment register from 2000 – 2008, offers a closer look at the subsample that are registered as unemployed. Monthly observations of registered unemployed are grouped by registration status (full time unemployed, part time unemployed or participant on active labor market measures (ALMM)) and the benefits they receive (Unemployment insurance, other benefits related to their status as job searcher, or nothing). It shows that people tend to be registered as unemployed even if they do not take up unemployment insurance. As many as 32.80 % of the person-months with full time unemployed are not associated with any job search related financial support. There might be several reasons for this pattern. First, registering as unemployed can be done quickly and easily, while application for UI requires more effort, possibly in addition to being attached to stigma costs (Moffitt, 1983). Parts of the fraction of uninsured registered unemployed can probably also be explained by the fact that the application process for UI might take some time. Secondly, there are incentives to register, and to stay within the register, as unemployed even if you are not entitled to UI. Everyone who contacts the Job Centre have the right to have his need for job search assistance evaluated. If necessary, the Job Centre and the unemployed will collaborate on developing a plan of action to increase the person's job market prospects (*The Labor- and Welfare Administration act §§14-15*, 2006).

Table 1: Monthly observations of registered unemployed, grouped by type of registration, and type of financial support related to the status as registered unemployed.

		Income related to the status as registered unemployed			
		Unemployment insurance	**Other benefits**	**No financial support**	**Total**
Type of registration	**Full time unemployed**	3 092 813 (65.88%)	61 840 (1.32%)	1 539 755 (32.80%)	4 694 408 (100%)
	Part time unemployed	1 139 275 (53.07%)	12 045 (0.56%)	995 397 (46.37%)	2 146 717 (100%)
	Active labor market measure	216 820 (30.97%)	194 806 (27.83%)	288 358 (41.19%)	699 984 (100%)
	Total	4 448 908	268 691	2 823 510	7 541 109

Note: Table is based on observations of individuals in the unemployment register in the period from 2000-2008. Row percentages are reported in the parentheses.

3.1. The reforms in 2003 and 2004

In January 2003, the eligibility threshold for income previous calendar year increased from 1.25 basic amounts (BA) to 1.5 BA. The motivation for this change was to strengthen the principle

that UI was intended to cover individuals with work income as their main source of income (*Ot.prp. (Law proposal) nr. 15 (2002–2003)*). Thus, the purpose of the change in the eligibility criteria was to affect persons with a non-permanent and weak attachment to the labor market.

Another change that was applicable from January 1st 2003 was a reduction in PBD for persons with income basis above 2 BA, from 156 weeks to 104 weeks. The government expected increased labor demand in the coming years, and wanted to prevent long term unemployed individuals from passively receiving unemployment benefits in a situation with high demand for labor. The focus in the economics literature and in the recommendations from the OECD at this time was changing, from recommending qualification programs and active labor market policies, to focusing on the incentives of the unemployed, so the shortening of PBD was viewed as an effective way to face the expected economic recovery (*Ot.prp. (Law proposal) nr. 15 (2002–2003)*).

On January 1st 2004 the PBD for unemployed with income basis above the minimum threshold, but bellow 2 BA, the PBD was reduced from 78 to 52 weeks. This was done to re-establish the principle that persons with lower attachment to the labor market should be granted considerable shorter UI periods than persons more strongly attached to employment. After the change in 2003, the difference in the PBDs was considered too small (*Ot.prp. (Law proposal) nr. 12 (2003–2004)*).

None of these changes had retroactive force, so every ongoing UI spell was unaffected by the reform. Table 2 displays the way different income groups were affected by the changes, and when, depending on income in the last calendar year and over the last three years. The fact that cells 5,6, 7 and 3 were unaffected by the reforms in 2003, and that the rest of the cells were unaffected by the changes in 2004, makes it possible to use close lying cells as control groups for each other in a difference in differences setting.

Table 2: The content of the reform, by income history

Average income, three previous calendar years			Income previous calendar year: 1-1,25 BA Eligible	1-1,25 BA PBD	1,25-1,5 BA Eligible	1,25-1,5 BA PBD	1,5 – 2 BA Eligible	1,5 – 2 BA PBD	2 → BA Eligible	2 → BA PBD
0-1 BA			Cell 1		Cell 2		Cell 3		Cell 4	
		2002	-	0	✓	18	✓	18	✓	36
		2003	-	0	-	0	✓	18	✓	24
		2004	-	0	-	0	✓	12	✓	24
1-2 BA			Cell 5		Cell 6		Cell 7		Cell 8	
		2002	✓	18	✓	18	✓	18	✓	36
		2003	✓	18	✓	18	✓	18	✓	24
		2004	✓	12	✓	12	✓	12	✓	24
2 → BA			Cell 9		Cell 10		Cell 11		Cell 12	
		2002	✓	36	✓	36	✓	36	✓	36
		2003	✓	24	✓	24	✓	24	✓	24
		2004	✓	24	✓	24	✓	24	✓	24

Note: The dark grey shaded cells are affected in January 2003, while the light gray shaded cells are affected in January 2004. Source: *Ot.prp. (Law proposal) nr. 15 (2002–2003)*; *Ot.prp. (Law proposal) nr. 12 (2003–2004)*

Four other minor changes also came into force in January 2003[9]:

i. Requirement for loss in working time increased from 40 % to 50 %.
ii. The labor administration's authority to require activity from UI recipients is pointed out explicitly in the law text.
iii. Longer waiting time at the beginning of the unemployment spell before UI is payed out: from 3 to 5 days.
iv. Take away holiday allowance (9,5 % of UI for persons that have received UI for more than 8 weeks).

All eligible individuals face changes i-iv simultaneously. It is thus hard to find a suitable control group to study the effects of them Since I in general apply UI eligible control groups in my analysis, I will not discuss these changes in this chapter.

4. Data

I use administrative register data from Statistics Norway: Annual income from 1986 to 2007 is used to determine UI entitlements for the years 1989 to 2008. The employment registers from 1992 to 2008 provide information on periods of employment. The unemployment registers include data on registered unemployment (both full time and part time), participation on labor

[9] The two last points on the list were reversed later. The holiday allowance was reintroduced from July 1st 2006, and the number of waiting days was reduced from five to four at July 1st 2007, and further down to three from January 1st 2008.

market measures, and claims of unemployment. These data are available from 1989. In combination with data on periods on education, disability, medical rehabilitation, and pensions, these data are used to construct a monthly panel data set of states in the labor market from January 2000 to December 2008, for all individuals in Norway aged between 25 and 60 years.

Five labor market states are defined[10]:

- Employed
- Registered unemployed on UI (RUI)
- Registered unemployed without UI (R)
- On health related benefits (not including sick leave) or education (HE)
- Non-activity (meaning that they cannot be found in any of the registers)

Figure 2 displays the distribution over the different states over the time period for individuals that are employed at least one month during the period from 2000 to 2008, i.e. the sample applied in the analysis. The graph is capped downwards at 1 million. Over the 108 months included in the data set, the average number of individuals included is 2 046 926.[11] We see that the share of uninsured registered unemployed is substantial. There are, as discussed above, incentives to register, and to stay within the register as unemployed, even if you are not entitled to UI.

To be able to discuss how transitions between these states are affected by the reforms, I need to determine who are affected, in what way, and when. This is done by grouping the monthly observations into the correct cell in Table 2.[12] For any monthly observation of a person who did not receive UI earlier in the present calendar year, and not in the two preceding calendar years, placing in the correct cell in Table 2 can be done quite easily, based on data on income from the three previous calendar years only. For other observations, hereby defined as "repeat users",

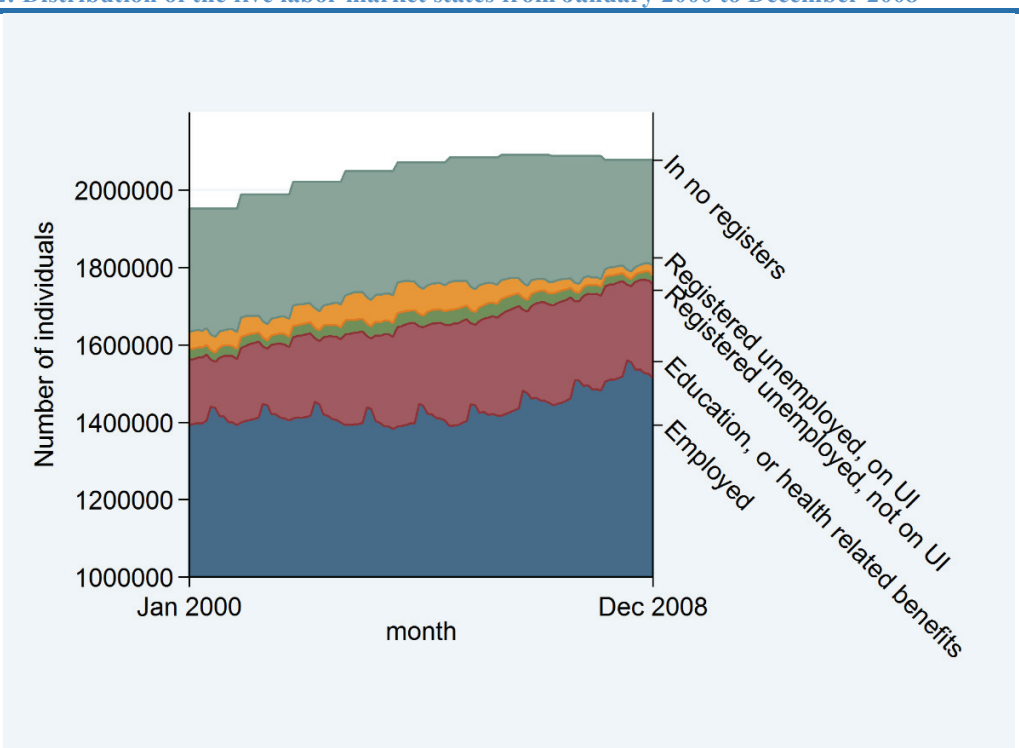
[10] The quality of the data regarding timing is better in the unemployment, social security- and education registers than in the work registers. Thus, the states are defined the following way: If someone, at the end of the month, is in the work register, but not in any of the other registers, he is considered as employed. If someone is on HE, but not in the unemployment register, he is considered as out of the labor force. Everyone in the unemployment registers is defined as unemployed. Some are registered as full time unemployed, some are part time unemployed, and some are participation on active labor market measures. The group of insured unemployed is a sub sample of the registered unemployed.

[11] A striking pattern in Figure 2 is the jagged shape of the employed pool. It increases sharply every July. This is counterbalanced by a reduction in the AHE mass at the same time. When studying the sum of the employees and the AHE participants, it has a fairly smooth shape over time. The jumps in employment around July are therefore probably caused by newly educated individuals entering the labor market at the same time.

[12] For any monthly observation of an employed person, potential UI entitlements will be determined by then income history at the month of observation. For an observation of an unemployed individual, entitlements will in general be determined by the income history at the time he entered unemployment.

determination of UI entitlements is handled by considering the entire history of income, employment, unemployment and UI uptake from 1989 and until the present calendar month. The procedure is described in detail in appendix B.

Figure 2: Distribution of the five labor market states from January 2000 to December 2008



Note: Based on the main sample in the analysis: Persons aged between 25 and 60 year, who are employed at least one month during the observation period from January 2000 to December 2008. The graph is capped downwards at 1 000 000.

5. Analyses of entry into unemployment

This section investigates how the changes in the UI system affected entry into unemployment. We start by analysing the effect of the reduction in PBD on the transitions from employment to insured unemployment[13], and continue by studying the effect of eligibility loss on entry into the

[13] We see from the transition matrix in Table 9 in appendix A that not all transitions to insured unemployment are made directly from employment. This could be due to some delay in the processing of the UI application and/or the use of waiting periods as sanctions for quits, and emphasizes the need for a proper definition of the risk set of the transition to UI. When studying entry into insured unemployment, I allow for short periods, up to two months, with registered (but uninsured) unemployment or inactivity between employment and insured unemployment.

unemployment registers[14]. I estimate monthly transition rates from employment to unemployment by applying a logit model with a difference in differences identification strategy.

In the analysis in this section, I include only observations of individuals with income previous calendar year between 1 BA and 2.5 BA. The advantage with this choice is that the treatment- and control group are comparable to each other. The treatment- and control group are more likely to be subject to the same calendar time effects, than if I included observations of individuals with higher incomes as well. This is, as discussed later, a central identifying assumption when the difference in differences method is applies. A disadvantage is that the results might suffer from a lack of external validity. The results are not necessarily representative to the general population.

In each analysis, I also limit my sample to observations within one year before- or after the reform. This is done because the group serving as a control group is subject to a reform either one year before- or one year after the treatment group. To be able to isolate the effect of the reform, I limit the analysis to years where the control group stays untreated.

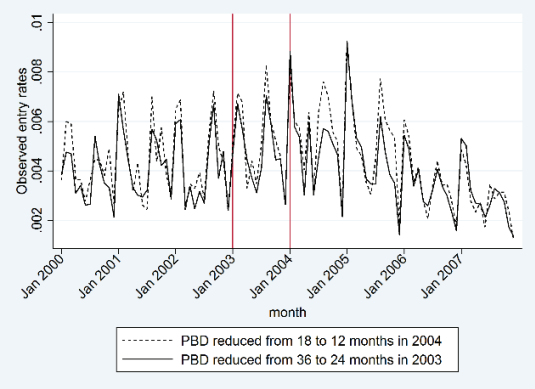
Figure 3 shows the trends in the risks of entering UI and registered unemployment, for different income groups. Figure 3 A1 displays the monthly risk of entering insured unemployment, by calendar year, for the groups that have PBD reduced from 36 to 24 months in 2003, and the group whose PBD is reduced from 18 to 12 months in 2004. Figure 3 A2 displays the same concept, but with seasonable patterns in the probabilities of entering UI removed.[15] The seasonal patterns seem to be unstable, and hard to remove, so to present a smoother pattern, Figure 3A3 displays annual averages over monthly probabilities of UI entries. If the workers respond to the changes by reducing their transition into UI, we should expect the solid line to increase less than the dotted line from 2002 to 2003, and the dotted line to increase less than the solid line from 2003 to 2004. This does however not seem to be the case.

Figure 3 B1-B3 shows how the probabilities of entry into registered unemployment change over time, for those that lose eligibility in 2003 and similar income groups, slightly below or above the intervals affected by the 2003 reform. We see from Figure 3 B3 that there is a clear drop in the

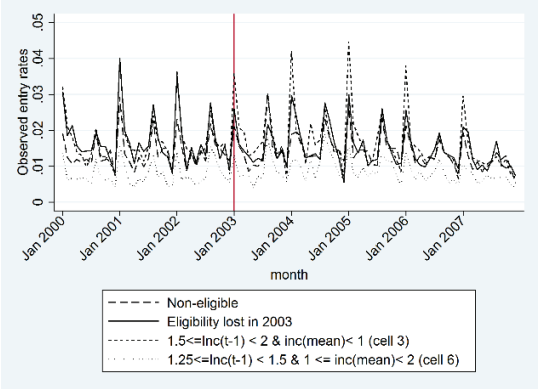
[14] Analysing the effect of eligibility loss on entry into insured unemployment is of less purpose.
[15] This is done by means of a regression with within-year monthly dummies

Figure 3:

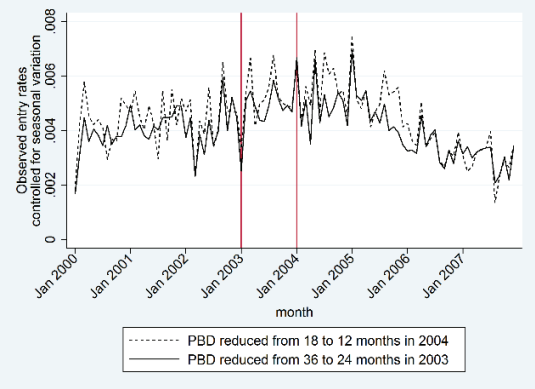
A1) Monthly probabilities of entering insuredd unemployment for the groups that experiences reductions in PBD in 2003 and 2004.



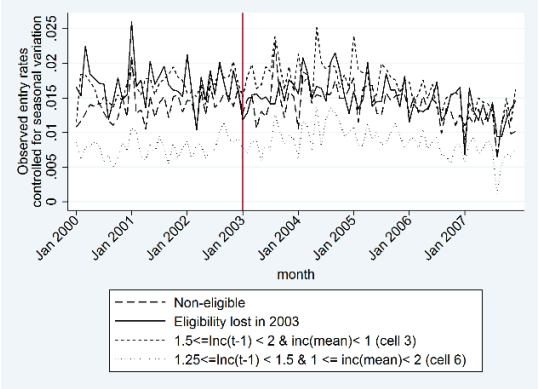
B1) Monthly probabilites of entering registered unemployment, for the group that loses eligibility in 2003, and potential control groups.



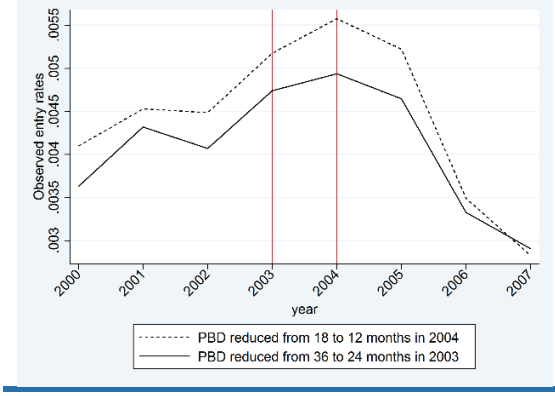
A2) Monthly probabilities of entering insured unemployment, with seasonality removed, for the groups that experiences reductions in PBD in 2003 and 2004.



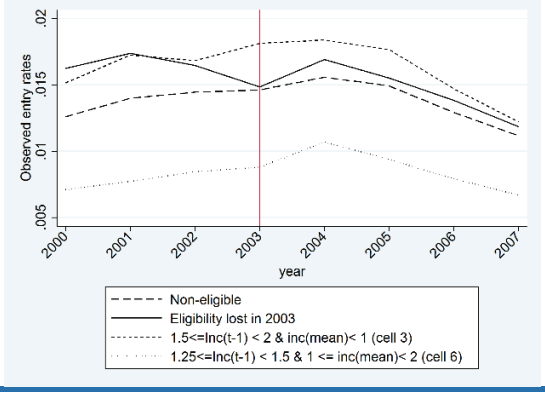
B2) Monthly probabilities of entering registered unemployment, with seasonality removed, for the group that loses eligibility in 2003, and potential control groups.



A3) Annual averages over monthly probabilities of entering insured unemployment, for the groups that experiences reductions in PBD in 2003 and 2004.



B3) Annual averages over monthly probabilities of entering registered unemployment, for the group that loses eligibility in 2003, and potential control groups.



Note: Income capped at 2.5 BA for the sake of comparability between the income groups.

Note: Income in the non-eligible group is capped downwards at 1 BA for the sake of comparability between the groups

group that loses eligibility from 2002 to 2003, a pattern not shared with the other income groups[16].

I estimate a logit model given by equation (1):

$$Prob(y_i = 1|g_i, p_i, x_i) = \frac{1}{1 + \exp(-(\alpha + \delta g_i + \mu p_i + \gamma(g_i \times p_i) + \theta a_i + \eta(g_i \times a_i) + \beta x_i))} \quad (1)$$

i indicates observations on a monthly level. y_i is a variable taking the value 1 if a transition to registered/insured unemployment occurs during the month and 0 otherwise. α is a constant term assumed common for every observation, g_i takes the value 1 if observation i belongs to the treatment group and 0 otherwise. p_i indicates that observation i is a post reform observation, and $g_i \times p_i$ is an interaction between g_i and p_i. a_i is a dummy variable taking the value 1 if the observation is within the anticipation period, three months prior to the reform, and $(a_i \times g_i)$, the interaction between treatment group and anticipation period, will catch the effect of the anticipation of the reform. The anticipation variables are only included in some of the specifications. x_i is a vector of demographic dummies, containing information on age, tenure in the labor market, duration of this specific employment spell, previous experience from unemployment insurance, county of residence, family situation, immigrant status and gender[17]. Descriptive statistics of the observations included in the regressions are reported in Table 10 in Appendix A. The average treatment effect for the treated at the time of treatment, conditional on x is defined as

[16] The group of "non-eligible" is capped downwards at 1 BA, to make this group more comparable to the treated group.

[17] Age is split up in five categories: Below 30, from 30 to 34, from 35 to 39, from 40 to 49, and above 50. The group from 35 to 39 is the reference group. Education is split up in five categories: no education, elementary education, high school, higher education, lower degree, and higher education higher degree. Completed high school is the reference value. Family situation is categorized depending on whether the person is cohabiting or married, or living alone, and on whether or not he is living together with children under the age of 18. Singles without children is the reference value. Experience from unemployment is captured by the number of months they was registered as unemployed during the five years before they start their employment spell. I split up in three categories: 0, 1-6 months, and over 7 months. I do the same with the number of months on unemployment insurance. Immigrant status is captured by two dummies: One for western immigrant (Western Europe, Australia and Northern America), and one for non-western immigrants. Tenure in the labor market is defined as the number of years prior to entry into my data set, with work income above 2 BA. It is split up I the following categories: 0, 1-5, 6-10, 11-20 and above 20. 1-5 years is the reference value. Duration of the employment spell is included as dummies: One for each quarter the first two years of the spell, and one dummy indicating that the spell has lasted for more than two years.

$$\tau(g_i = 1, p_i = 1, x_i) = f(\alpha + \delta + \mu + \gamma + \beta x_i) - f(\alpha + \delta + \mu + \beta x_i) \quad (2)$$

(Puhani, 2012). The average of this quantity is approximated by the "margins" post estimation function in Stata[18].

Identification when a difference in differences identification strategy is used, hinges on parallel trends in the (relevant transformation) of the dependent variable(Angrist and Pischke, 2009). When applying the logit model, this means that the development over time in the log odds ratio of entering unemployment in the treatment group and control group, given x, would have been parallel, if the reform did not take place. By studying pre-reform trends, we can shed light on the validity of this assumption. If pre reform trends are not parallel, the assumption that the trends from the pre reform year to the post reform year would have been parallel if the reform did not take place, is hard to justify. From Figure 3 A3 we see that the development in the risk of entering unemployment seem to be quite similar from 2000 to 2002, in the groups that experienced reductions in PBD in 2003 and 2004 respectively. From Figure 3 B3 we see that the eligibility losers have approximately the same pre-reform trend in rates of entry into unemployment as the group belonging to cell 3 in Table 2. Therefore, I use cell 3 as control group when the effects of eligibility loss is analysed.

Anticipation of the reform before it took place could affect the validity of the estimates. The reform was first mentioned in the national budget for 2003. The budget was accepted by the government on September the 20th 2002, and presented for the parliament on October the 3rd, 2003. A search in the media data base retriever from 2002 on the term "dagpenger" (The Norwegian term for UI) results in 244 hits. None of the articles published before October the 3rd mention anything about changes in the existing system, but after October the 3rd there are many references to changes in UI. A person that would lose eligibility to UI, or have PBD reduced, would most likely be able to anticipate this, and possibly respond to it before the reform. One response could be to enter unemployment prior to the turn of the year, as a last chance to enjoy a (longer) period on UI. This would increase the average pre reform risk of unemployment entry in the treatment group, and the estimated treatment effect would be downwards biased: Treatment

[18] The code used to generate the effect is margins, dydx($g \times p$) at($g == 1, p == 1$)

would seem to reduce entry into unemployment more than it actually does. This possibility is handled by adding a dummy indicating the anticipation period and an interaction between treatment group and anticipation period to the regression. This ensures that post reform outcomes are compared to pre anticipation outcomes. Another possible response to the anticipation of a forthcoming eligibility loss could be to spend the last months in 2002 working extra hours to make sure that the new income requirement for eligibility is met. In this way the person self-selects out of the treatment group as an effect of the reform, and the estimates would compound the changes in the incentives of the individuals, with the compositional changes in the treatment- and control group. The examination of the income distribution around the threshold at 1.5 BA, reported in chapter 2 in this dissertation, indicates though, that the adjustment of income to ensure eligibility is not a prominent phenomenon.

I start by investigating the effect of having PBD reduced from 36 to 24 months. I estimate equation (1) on the monthly observations of employment spells in 2002 and 2003. The dependent variable y_i indicates transition to insured unemployment. The group that reduces PBD from 36 to 24 months in 2003 is the treatment group, while those that have potential benefit duration reduced in January 2004 is the control group. I also limit the observations to the ones with incomes previous years and on average over the past three years below 2.5 BA, to ensure that the treatment group and control group are comparable.

Table 3 below reports the marginal effects, with 95 % confidence intervals. Full sets of estimated coefficients and standard errors are reported in Appendix A.

We see from Regression 1 in Table 3 that the effect of the reform measured in percentage points is not significantly different from 0. The average monthly risk of entering insured unemployment among the treatment group after the reform is 0.45 %. Thus, these results indicate that by 97.5 % certainty, the effect of the reform does not reduce the inflow rate into unemployment by more than 7.54 %. This is weaker than the 10 % effects found by Lalive et al. (2011) on workers aged between 40 and 49, who had PBD increased from 30 to 39 weeks.

Table 3: Results when estimating the effect of reductions in PBD on entry into insured unemployment

	Regression 1: Effects of PBD reduced from 36 to 24 months	Regression 2: Effects of PBD reduced from 18 to 12 months
Treatment group×post reform	0.0129 [-0.03396, 0.05976]	0.02397 [-0.02161, 0.06955]
N	1 510 441	1 459 202

Note: Marginal effects, defined as dy/dx at $g = 1$ and $p = 1$ (the post reform treatment group), measured as percentage points. 95 % confidence intervals based on standard errors clustered at the individual level are reported in the brackets. Monthly observations of employment from one year prior to- and one year after the reform are included in the regression. Full set of estimates is reported in Table 12 in Appendix A.

Regression 2 in Table 3 reports the marginal treatment effect of reducing PBD from 18 to 12 months. In this regression, observations from 2003 and 2004 are used, and the group that had PBD reduced in January 2003 are used as control group. There is no significant effect of this change either, and since the baseline risk is higher (0.55 %) and the lower level of the confidence interval is higher than in the other reform, the relative effect on the inflow rate is even weaker than the effect of the PBD reduction from 36 to 24 months.

The results thus indicate that the changes in PBD within a range of 12 to 36 months do not reduce the inflow into unemployment. The effect on the inflow rate is significantly weaker than what is found in Lalive et al. (2011).

We proceed to investigate the effects of eligibility loss on unemployment entry. The dependent variable now is transition into registered unemployment, and equation (1) is estimated on monthly observations of the sample of employment spells in 2002 and 2003. Due to the similar pre reform development in the risk of entering registered unemployment, those that earned less than 1 BA on average over the past three calendar year, but between 1.5 and 2 BA previous year, (Cell 3 in Table 2), represented by the short-dashed line in Figure 3 B3 is used as control group. The marginal treatment- and anticipation effects for the treated at the time of treatment are reported in Table 4.

We see from Regression 3 in Table 4 that the risk of entering registered unemployment decreases significantly as an effect of the reform. The point estimate indicate a reduction in the monthly risk of about 0.33 pp, which given the baseline monthly level for this group of about 1.5 %

corresponds to a reduction in the inflow rate of about 20 %[19]. The anticipation effect is estimated to be negative, contrary to what one should expect, but the confidence interval is very large, so the anticipation effect might just as well be positive.

Table 4: Results from the estimations of the effect of eligibility loss on entry into registered unemployment and health related benefits or education (HE).

	Regression 3: Transition from employment to registered unemployment Effect of eligibility loss	Regression 4: Transition from employment to HE Effect of eligibility loss
Treatment group×post reform	-0.330* [-0.5608,-0.0475]	-0.041 [-0.214, 0.132]
Treatment group×anticip.period	-0.111 [-0.461, 0.239]	0.041 [-0.133, 0.216]
N	213 428	213 428

Note: Marginal effects, defined as dy/dx at $g = 1$ and $p = 1$ (the post reform treatment group), measured as percentage points. 95 % confidence intervals based on standard errors clustered at the individual level are reported in the brackets. Monthly observations employed individuals in 2002 and 2003 are included in the regression.
* indicate $p < 0.05$. Full set of estimates is reported in Table 13 in Appendix A.

A strong effect of eligibility loss on entry into the unemployment register is not surprising, as the incentives to register are considerably reduced when UI eligibility disappear. More interesting is to study their alternative behavior. In regression 4 in Table 4 the marginal effects of the reform on the transition into health related benefits or education (HE) is reported. One could suspect that persons who lose their job substitute to other benefits when UI eligibility disappear. The results however indicate no significant increase in the transition into HE. The point estimate is actually negative.

I also want to test whether the effect on entry into the unemployment register is only an effect on the tendency to register *given that* the job is lost/left, and hence indicates nothing but a change in the balance between registered and unregistered unemployment, or whether it actually also reflects a reduction in the risk of separation from employment. I therefore continue by studying entry into a broader definition of unemployment: Non-employment, no education and no health related benefits. Figure 5 illustrates the relationship between this definition and the two other definitions.[20] The trends in monthly probabilities of entering this broad definition of

[19] The point estimate of the treatment effect is reduced to 0.304 if the terms related to anticipation is not included in the regression.
[20] An issue with the employment register is that the termination of a job spell is sometimes registered with a delay. Therefore, people may seem to still be employed even if they enter the unemployment register as full time employed.

unemployment are displayed in Figure 4. The seasonal patterns are still strong and hard to remove, but from Figure 4 3) the probability of entering this broad measure of unemployment seems to increase less in the treatment group than in the neighbouring income groups.

An obvious disadvantage with this definition is that it includes too many observations, also those that leave the labor force, and hence it is characterized by a high degree of measurement error. The advantage is that this measurement error does not vary systematically with UI eligibility. The rest of this section will focus on the effect of eligibility loss on this transition.

To be able to compare these results to the effects found of eligibility loss on entry into registered unemployment, we start by examining the results obtained from applying the same control group as in Table 4 (Cell 3 in Table 2).

We see from Regression 5 in Table 5, that the estimated marginal effect on entry into this broad definition of unemployment is -0.177 or -0.198 percentage points, depending on whether or not anticipation is taken into account. This indicates that slightly above half of the effect on entry into the unemployment register is driven by a reduction in the tendency to separate from employment when UI eligibility is lost. Neither the treatment effects nor the anticipation effect in Regression 5 are significantly different from 0, though.

The results are sensitive to the choice of control group. Regression 6 in Table 5 reports the results when all the alternatives, both cell 3, 6 and the non-eligible (cell 1) is applied. The estimated effect, when controlling for anticipation is -0.205, but the effect is still not significantly different from 0. To increase statistical power, Regression 7 in Table 5 reports the results when observations from 2000 and 2001 are also added[21]. The resulting effect when controlling for anticipation is -0.226. The coefficient attached to the interaction term has a z value of 1.75, and the result is therefore statistically significant at a 90 % level. All the estimates of the effect of the reform reported in Table 5, lie around -0.20[22]. Given that the monthly risk in the treatment group in 2003 of entering this broad definition of unemployment is 3.5 %, the estimate corresponds to a

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Since the broadest definition of unemployment does not apply information from the unemployment register at all, this definition does not fully encircle the two more narrow definitions.

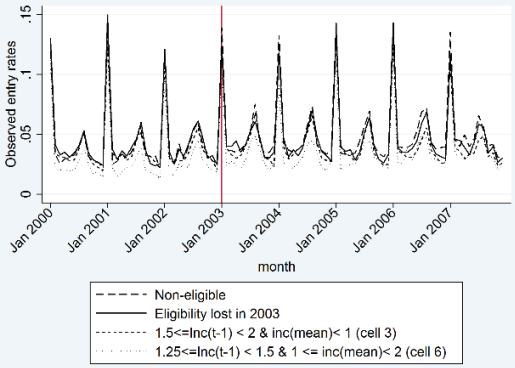
[21] More post reform years cannot be included, as the control group experiences a change in PBD in January 2004.

[22] It If applying only cell 3, the estimated marginal effect is -0.044 pp, i.e. practically close to 0 if observatins from 2000 to 2003 are include. If using only the non eligible, the estimated marginal effect is much stronger: -0.424, and if using cell 6, it is -0.322. Both results are significantly different from zero.

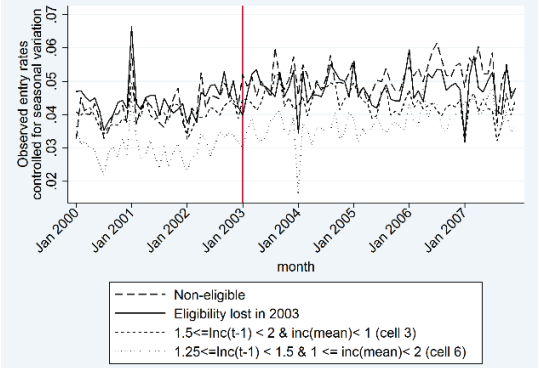
fall in the inflow rate of about 5.7 %. As expected, the anticipation effect is estimated to be positive, but the effects are not significantly different from zero.

Figure 4:

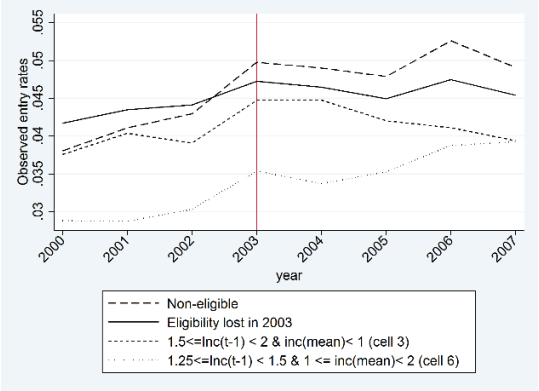
1) Monthly probabilites of entering Non-employment and no HE, for the group that loses eligibility in 2003, and potential control groups.



2) Monthly probabilities of entering Non-employment and no HE, with seasonality removed, for the group that loses eligibility in 2003, and potential control groups.

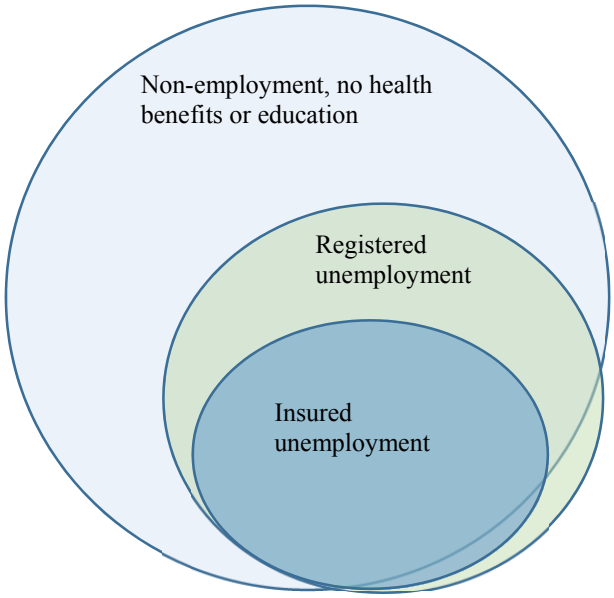


3) Annual averages over monthly probabilities of entering Non-employment and no HE, for the group that loses eligibility in 2003, and potential control groups, by year.



Note: Income in the non-eligible group is capped downwards at 1 BA for the sake of comparability between the groups

Figure 5: Three different definitions of unemployment



Note: A display of the three measures of unemployment used to estimate the effect of changes in the UI system on unemployment entry.

Table 5: Results from the estimations of the effect of eligibility loss on transitions to Non-employment, no education, and no health benefits.

	Regression 5: (observations from 2002-2003) Cell 3 is Control group		Regression 6: (observations from 2002-2003) Cells 3, 6 and 1 are control group		Regression 7: (observations from 2000-2003) Cells 3, 6 and 1 are control group	
	a)No anticipation	b)Allowing for anticipation	a)No anticipation	b)Allowing for anticipation	a)No anticipation	b)Allowing for anticipation
Treatment group×post reform	-0.177 [-0.484, 0.129]	-0.198 [-0.592, 0.197]	-0.289 [-0.625 0.047]	-0.205 [-0.531, 0.121]	-0.251 [-0.520 0.018]	-0.226 [-0.485, 0.034]
Treatment group×anticip.period		0.053 [-0.363, 0.470]		0.175 [-0.145, 0.497]		0.090 [-0.156, 0.338]
N	198 381		384 821		786 498	

Note: Marginal effects, defined as dy/dx at $g = 1$ and $p = 1$ (the post reform treatment group), measured in percentage points. 95 % confidence intervals based on standard errors clustered at the individual level are reported in the brackets. Full set of estimates is reported in Table 14 in Appendix A. Regressions are based on monthly observations of employed individuals in 2002 and 2003.

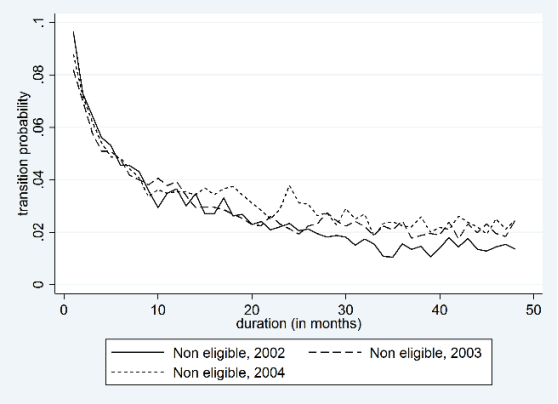
To summarize the results in this section, reduction in PBD from 36 to 24 months, or from 18 to 12 months does not have any significant effect on entry into unemployment, and the effects are significantly weaker than the effects found by Lalive et.al. (2011). Loss of eligibility reduces entry into registered unemployment by around 0.3 percentage points, which corresponds to a relative effect on the inflow rate of around 20 %. When modelling entry into a broader definition of unemployment to investigate whether this effect is partly caused by a reduced exit from employment, the point estimates indicate that entry into this broad definition of unemployment is reduced when UI eligibility is lost. In some of the specifications, the effects are also statistically significant. I find no effects of eligibility loss on substitution into other welfare benefits.

6. Analyses of exit from unemployment.

This section investigates the effects of the reforms on the transition from unemployment and back to employment. Figure 6 displays descriptive figures of transition rates from unemployment to employment for the groups subject to the various reforms, before and after the reform. We observe in Figure 6 B, that the group entitled to 18 months of PBD in 2002, but that lost eligibility to UI completely in 2003, the transition rate of unemployment spells starting in 2002 increase from month 14 to month 22, and falls thereafter. Spells within this group starting in 2003 and 2004 shows no such pattern, but instead we see a clear increase in the transition to employment during the first two months of the spell. The loss of eligibility therefore seems to affect the job-finding rate in the beginning of the spell strongly. Among those that have PBD reduced from 36 to 24 months in 2003 (displayed in Figure 6 C) spells starting in 2002 have a sharp increase in the transition rate back to employment from month 35 to month 38. For spells starting in 2003 and 2004 this increase seems to have been moved to the period from month 22 to 26. For the group whose PBD decreases from 18 to 12 months in 2004, (displayed in Figure 6 D) the transition rates for spells starting in 2002 and 2003 are quite similar, showing a small increase in the transition to employment from month 17 to month 19/20. Unemployment spells starting in 2004 have a clear rise in the transition rate from month 11 to month 14. These graphical patterns are as expected from the way the reforms affected the incentives.

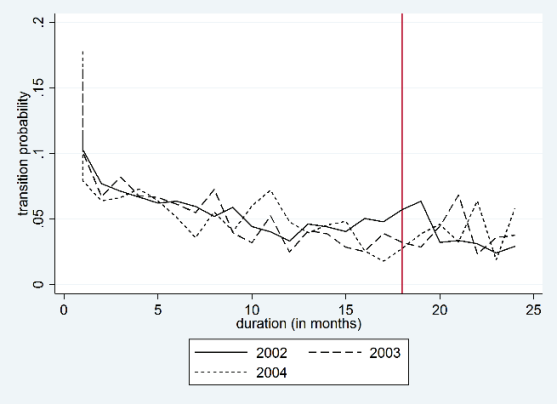
Figure 6: Monthly rates of exit from unemployment to employment, by unemployment spell duration and year of unemployment entry,

A) Non-eligible



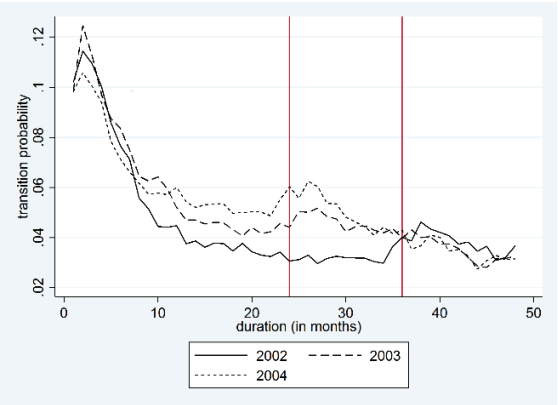
Note: The start of the spell is defined as time of entry into the unemployment register.

B) The income group that loses eligibility to UI in 2003



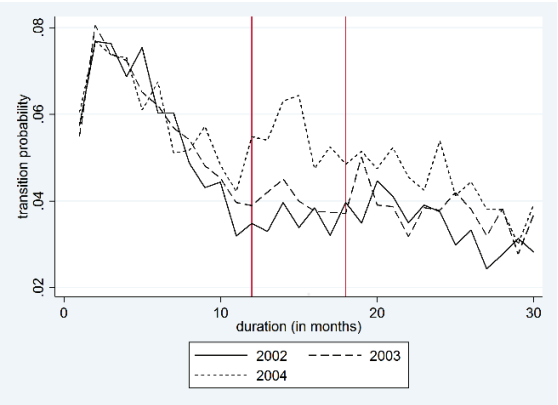
Note: The start of the spell is defined as time of entry into the unemployment register.

C) The income group whose PBD falls from 36 to 24 months in 2003



Note: The start of the spell is defined as time of entry into unemployment insurance.

D) The income group whose PBD falls from 18 to 12 months in 2004



Note: The start of the spell is defined as time of entry into unemployment insurance.

In the following econometric analysis, each reform (eligibility loss, PBD reduced from 36 to 24 months, and PBD reduced from 18 to 12 months) will be treated separately. I estimate a competing risk hazard rate model with unobserved heterogeneity. Unemployment spells that started within one year from the point of time of the reform will be included in the regressions. As in the analyses of entry into unemployment, this is done to make sure that the income group serving as control group in the respective analyses are not subject to any reform in the

observation period. As opposed to the analyses of unemployment entry, in the analyses of unemployment exit, I do not limit the sample to individuals with income below 2.5 BA. This is done to obtain statistical power, since the number of unemployment spells is much lower than the number of employment spells. Robustness analysis (not reported in this paper) indicate however that while the confidence intervals are strongly reduced, the point estimates related to the effect of the reform are more or less unaltered from the inclusion of larger parts of the income span in the sample.

I censor spells after 48 months, by the time the individual turns 60 years, and by the time of death. I keep the spells in the risk set, even if they disappear from the unemployment register or enter active labor market programs. This is in contrast to van Ours and Vodopivec (2006), and is done to be able to discuss the potential effects of the reforms on the destination state.

Let us assume an underlying hazard function of the following form:

$$\theta_{idk} = \exp(\alpha_{dk} + \delta_{dk} g_i + \mu_{dk} p_i + \gamma_{dk}(g_i \times p_i) + \beta_k(x_i) + v_{ik}) \qquad (3)$$

g_i is a dummy indicating that the observation is in the treatment group. p_i indicate that the spell started after the reform, and $g_i \times p_i$ is the interaction between the two. x_i is a vector of demographic- and background variables previously defined in footnote 17. The ds attached to the parameters indicate duration, and the parameters therefore are vectors. For a given duration d, γ_{dk}can be viewed as the treatment effect of the reform on the hazard rate of the treated at the time of treatment, to destination k. I assume that the hazards are constant within periods, but vary flexibly between them[23]. v_{ik} reflects individual unobserved heterogeneity in the hazard to destination state k.[24]

[23] The periods are split up on a monthly basis the first six months of the unemployment spell. The classification later in the spell depends on which reform I look at. Due to shorter unemployment spells and less observation, a more crude classification of duration is needed when studying the effects of eligibility loss than the effects of reduced PBD.

[24] My identification strategy depends on differences between transition profiles. The downward slope of the empirical transition profiles displayed in Figure 6 are results both of duration dependence (a given unemployed individual is less likely to find a job as time goes by due to depreciation of human capital, signaling effects to potential employers discouragement), and selection (the well qualified and motivated individuals leave unemployment earlier than unqualified and unmotivated individuals). This selection is not a problem for my estimates unless the reform also changes the selection process differently in the treatment and control groups. There are, however, reasons to believe that the selection process is affected. Exhaustion of UI pushes many, but not all, of the unemployed back to work. We can expect those that are not able to find a new job before, or shortly after benefit exhaustion, on average, to be less qualified than those that do. Hence those that are still unemployed between the old

The model is proportional in the sense that the relative shift of the hazard with respect to a dummy variable is assumed to be independent of the value of any other variable. The identification of the distribution of unobservable individual heterogeneity hinges on this proportionality assumption (Gaure, Røed, and Zhang, 2007). Given the distribution of individual heterogeneity, the assumptions needed for identification of the treatment effect is that the hazards of the treatment group and the control groups, given duration and x, would have had parallel trends if the reform did not take place.

The monthly probability of transition to destination k is given by the following equation:

$$\begin{aligned} & h_{idk} \\ & = 1 - exp\left(-\sum_{k=1}^{2} \exp(\alpha_{dk} + \delta_{dk} g_i + \mu_{dk} p_i + \gamma_{dk}(g_i \times p_i) + \beta_k(x_i) + v_{ik})\right) \\ & \times \frac{\exp(\alpha_{dk} + \delta_{dk} g_i + \mu_{dk} p_i + \gamma_{dk}(g_i \times p_i) + \beta_k(x_i) + v_{ik})}{\sum_{k=1}^{2} \exp(\alpha_{dk} + \delta_{dk} g_i + \mu_{dk} p_i + \gamma_{dk}(g_i \times p_i) + \beta_k(x_i) + v_{ik})} \end{aligned} \tag{4}$$

The contribution to the likelihood for spell i of duration D_i for a given value of v_{1i} and v_{2i} are given by:

$$L_i(v_i) = \prod_{d=1}^{D_i} h_{id1}^{y_{i1d}} h_{id2}^{y_{i2d}} (1 - h_{id1} - h_{id2})^{1 - y_{i1d} - y_{i2d}} \tag{5}$$

I approach the distribution of unobserved heterogeneity as a discrete one, with an a priori unknown number of support points. If W is the number of support points, and $\{v_l, p_l\}$ the associated values and probabilities, then the likelihood function is given by

---

and the new timing of benefit exhaustion are likely to be better qualified if they became unemployed prior to the reform, than if they became unemployed after. This will bias my estimates of the reform effect downwards in the period between the old and new time of benefit exhaustion, unless unobserved heterogeneity is controlled for properly.

$$L = \prod_{i}^{N} E[L_i(v_i)] = \prod_{i}^{N} \sum_{l=1}^{W} p_l L_i(v_l), \quad \sum_{l=1}^{W} p_l = 1\,, \tag{6}$$

With $L_i(v_i)$ inserted from equation 5[25].

The procedure continues to add support points until it maximizes the likelihood function, with a punishment for the number of parameters included subtracted. The punishment applied here is simply the number of parameters estimated in the model. The reason for this choice is documented in (Zhang, 2003). Further details of the estimation procedure are described in (Gaure, Røed, and Zhang, 2007).

Table 11 in Appendix A presents descriptive statistics of the unemployment spells included in the analysis.

Complete sets of estimates are reported in Table 15 (The effect of eligibility loss), Table 16 (The effect of PBD reduction from 36 to 24 months) and Table 17 (The effect of PBD reduction from 18 to 12 months) in Appendix A.

We start by studying the effects of eligibility loss. A total of 7529 unemployment spells starting in 2002 and 2003 are included in the regression. Cell 3 in Table 2 is used as control group. Figure 7 displays the vector of coefficients attached to the duration vector interacted with treatment group and post reform. We see, as expected from the descriptive picture in Figure 6 B, that eligibility loss lifts the hazard for the first two months of the spell. A point estimate of 1.5 in duration month 2 indicates that the reform increases the hazard back to employment with 50 % at that specific duration. The hazards would have been higher around the time of the old benefit exhaustion (18 months) without the reform. This leads to negative point estimates from month 13 to 24. Only the effect the second duration month is significantly different from 0, though.

We continue by investigating the effects of the reduction in PBD from 36 to 24 months. 181 448 unemployment spells, starting in 2002 and 2003 are included in the sample. The control group consists of those experiencing a reduction in PBD from 18 to 12 months in 2004. Figure 8 displays the estimated effects of this reform, by spell duration. The point estimates increase

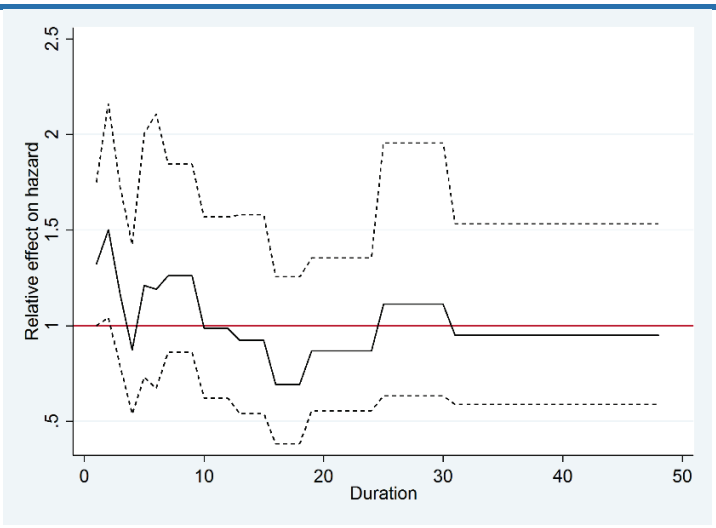
[25] The likelihood function is programmed and maximized by Research fellow Simen Gaure at the Frisch Centre for Economic Research.

gradually from the beginning of the spell, and then stabilizes around duration month 22, and is significantly above 0 until duration month 33.

Figure 9 shows the effects of the reductions in PBD from 36 to 24 months on the transition into education, or health related benefits. We see that the risk of this transition also increases at the new timing of benefit exhaustion. In contrast to the effect on the hazard into employment, the point estimates of the effect on this transition are in general negative in the beginning of the spell.

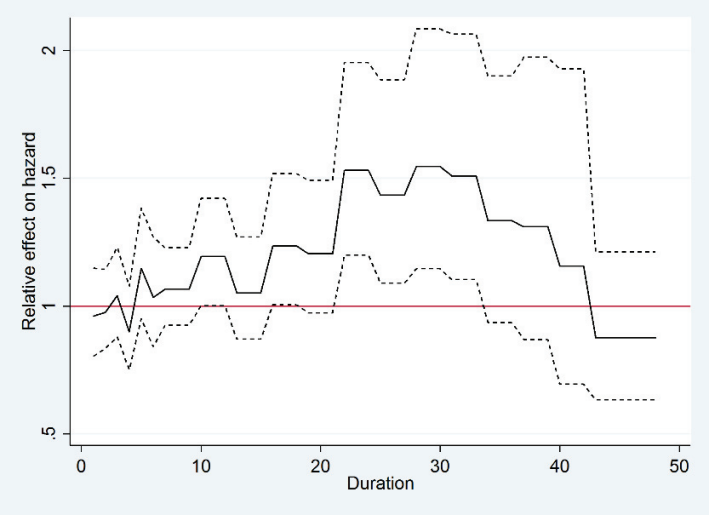
Figure 10 shows that the results of the reduction in PBD from 18 to 12 months, on the transition back to employment. This analysis is based on 180 241 unemployment spells that started during 2003 and 3004. The income group whose PBD was reduced from 36 to 24 months at the beginning of 2003 is used as a control group. The increase in the hazard is largest, and statistically different from 0, around the new timing of benefit exhaustion, also in this case.

Figure 7: The treatment effects of eligibility loss on the hazard from unemployment to employment, by unemployment duration



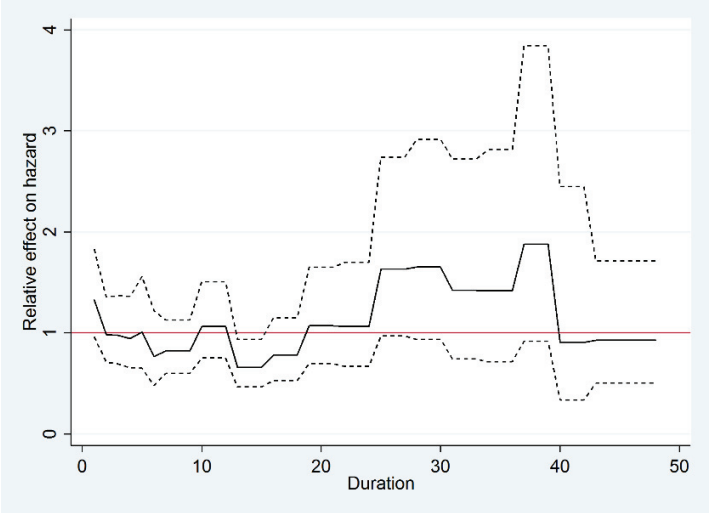
Note: Controll group consists of persons with income previous calendar year slightly above the new income requirement for eligibility. Unemployment spells starting om 2002 and 2003 are included in the analysis. The estimation procedure results in 2 support points on the distribution of unobserved heterogeneity. Full set of estimated coefficients is reported in Table 15 in Appendix A

Figure 8: The treatment effects of reduction in PBD from 36 to 24 months on the hazard from unemployment to employment, by unemployment duration



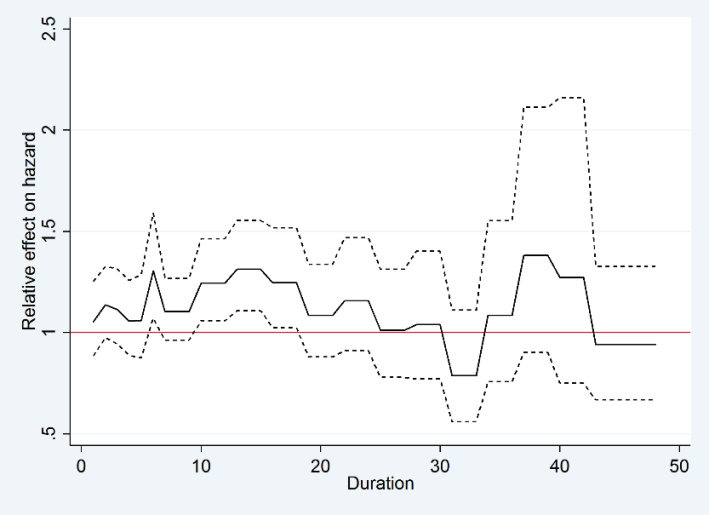
Note: Controll group consists of persons subject to reduction in PBD from 18 to 12 months in 2004. Unemployment spells starting in 2002 and 2003 are included in the analysis. The estimation procedure results in 8 support points on the distribution of unobserved heterogeneity. Full set of estimated coefficients is reported in Table 16 in Appendix A.

Figure 9: The treatment effects of reduction in PBD from 36 to 24 months on the hazard from unemployment to education or health related welfare benefits, by unemployment duration



Note: Controll group consists of persons subject to reduction in PBD from 36 to 24 months in january 2003. Unemployment spells starting in 2002 and 2003 are included in the analysis. The estimation procedure results in 8 support points on the distribution of unobserved heterogeneity. Full set of estimated coefficients is reported in Table 16 in Appendix A.

Figure 10: The treatment effects of reduction in PBD from 18 to 12 months on the hazard from unemployment to employment, by unemployment duration



Note: Controll group consists of persons subject to reduction in PBD from 36 to 24 months in january 2003. Unemployment spells starting in 2002 and 2003 are included in the analysis. The estimation procedure results in 4 support points on the distribution of unobserved heterogeneity. Full set of estimated coefficients is reported in Table 17 in Appendix A

6.1. Simulation results of unemployment durations and destinations

We see in general from the results so far that the reforms lift the hazard rates to employment, and also affect the transitions to education, and health benefits. The estimates are characterized by a high degree of uncertainty, though. The aggregated impacts of the reforms will also depend on the number of unemployment spells that lasts long enough for the new limits of PBD to actually bind. To translate the estimated coefficients and their standard errors into more policy relevant measures of treatment effects, I conduct simulations of the estimated models.

10000 artificial unemployment spells, with the same composition on covariates as the treatment group spells that begun the year after the reform, are started. For each spell, terms of unobserved individual heterogeneity are drawn from its estimated distribution, and based on the full set of estimated coefficients monthly transition probabilities to HE and employment are calculated. The unemployment spells are followed for up to 100 months, or until all of the simulated spells have left the risk set. This is the "with reform"-case. The contra factual "without reform" case is obtained by setting all the coefficients related to the treatment, the vector γ_{dk} in equation (4), equal to zero. The intention with the counterfactual scenario is to describe how the individuals

from the treatment group that became unemployed the year after the reform would have behaved, if the reform did not take place. The results from the "with reform" simulation are compared to the "without reform" simulation, and point estimates of the treatment effect on the treated at the time of treatment defined as the following quantities;

i. the share that re-enter employment eventually,
ii. the average duration until re-employment and
iii. the average duration until HE entry,

are calculated.

To report the uncertainty of these measures, we also need to take the uncertainty of the estimated coefficients into account. Let Σ be the $n \times n$ covariance matrix of the coefficients in the model, and μ the $n \times 1$ vector of expected values. A $n \times 1$ vector Y, randomly drawn from the multi-normal distribution $N(\mu, \Sigma)$ is obtained by the following transformation: $Y = \mu + \Omega X$, where $X \sim N(0,1)$ is a vector of i.i.d. unit normal, and Ω is the Cholesky decomposition of the covariance matrix: $\Sigma = \Omega\Omega'$ (Judd, 1998, p 59-60). By replacing Σ and μ with their estimated counterparts, I draw sets of coefficients from its estimated multi normal distribution[26], and repeat the procedure described above 500 times. This results in a distribution of measures of treatment effects, from which we can derive confidence intervals, and thereby conduct statistical inference.

We see from Table 6 to Table 8 that neither eligibility loss nor reduction in PBD has significant effects on the share of the spells that actually do end up employed. The point estimate of the effect of eligibility loss is negative, but has a very large confidence interval, while the point estimates of the effects of PBD reductions are positive.

The changes in PBD reduce the duration until reemployment significantly. The point estimate of the reduction from 36 to 24 months is 0.7 months, which corresponds to an effect of 0.06 months per month reduction in PBD. The effect of the reduction from 18 to 12 months seems to be stronger, and corresponds to a relative effect per month reduction in PBD of 0.18. When studying the treatment effects on the hazard functions to employment of the changes in PBD, displayed in Figure 8 and 10, the treatment effect on the hazard at the new time of benefit exhaustion is stronger in the 36 to 24 months reform, than in the 18 to 12 months reform. The 36 to 24 months reform increases the hazard around 24 months duration by approximately 50 %, while the 18 to

[26] The estimated probabilities and values of the unobserved individual heterogeneity terms are not redrawn in each simulation of the model.

12 months reform increases the hazard around 12 months duration by about 30 %. The fact that the reduction from 18 to 12 months affects the average duration of time to employment more strongly than the reduction from 36 to 24 month can probably be explained by the difference in margins. More unemployment spells last 12 months than 24 months, and the share of the unemployed individuals that face the new timing of benefit exhaustion is higher in the 18 to 12 months reduction than in the 36 to 24 months reduction. This generates stronger effects on the aggregated level.

The average duration until education and health benefits is not significantly affected by any of the reforms. The confidence intervals are quite large, and symmetrically around 0

Table 6: Simulated results of eligibility loss	With reform	Without reform	Difference
Share of the spells exiting to employment	71.3%	73.2%	-1.9 pp [-4.6, 4.1]
Average duration to employment, given that employment is the destination (months)	10.7	12.0	-1.3 [-2.9, 0.28]
Average duration to HE, given that HE is the destination (months)	8.9	9.3	-0.4 [-2.2, 1.7]

Note: 95 % confidence intervals obtained from the simulation are reported in the brackets.
* indicate p<0.05.

Table 7: Simulated results of the reduction in PBD from 36 to 24 months	With reform	Without reform	Difference
Share of the spells exiting to employment (percentage points)	86.3%	85.7%	0.6 pp [-0.06, 1.81]
Average duration to employment, given that employment is the destination (months)	11.9	12.6	-0.7 * [-1.51, -0.1]
Average duration to HE, given that HE is the destination (months)	18.6	18.3	0.3 [-2.31, 1.81]

Note: 95 % confidence intervals obtained from the simulation are reported in the brackets
* p<0.05

Table 8: Simulated results of the reduction in PBD from 18 to 12 months	With reform	Without reform	Difference
Share of the spells exiting to employment	78.1 %	77.0 %	1.1 pp [-0.58, 2.87]
Average duration to employment, given that employment is the destination (months)	13.6	14.5	-1.1* [-1.76, -0.24]
Average duration to HE, given that HE is the destination (months)	15.9	16.4	-0.4 [-2.09, 0.99]

Note: 95 % confidence intervals obtained from the simulation are reported in the brackets
* p<0.05

7. Conclusion

In 2003 and 2004, the Norwegian unemployment insurance (UI) system was reformed. The income requirement for UI eligibility was increased, and the potential benefit period (PBD) was reduced by one third. The motivation for the change in the eligibility criteria was to strengthen the principle that unemployment insurance was supposed to cover individuals with a real attachment to the labor market. The intention with the reduction in PBD was to prevent individuals from staying passive on long-term unemployment insurance benefits in an economic situation with expected increased demand for labor. According to the law proposal, the ministry of labor expected the changes in the system to lead to a better functioning labor market, without significantly weakening the safety net of the individual job seekers (*Ot.prp. (Law proposal) nr. 15 (2002–2003)*, p. 6).

This chapter offers a comprehensive evaluation of the impacts of the reforms, both on entry into unemployment, and exit from unemployment to employment and to alternative states.

Changes in potential benefit duration (PBD) do not seem to affect the transition into unemployment noticeably, but it increases the hazards back to employment, especially around the new timing of benefit exhaustion. This indicates that the law change succeeded in activating long term unemployed: given that a person has been unemployed for nearly 2 years, the reduction in PBD from 36 to 24 months significantly increases his prospects of finding a job within a short period of time. The same counts for a person at a one-year duration subject to the reduction in PBD from 18 to 12 months. The reforms did not cause significant changes in the hazard rates earlier in the spells, however. The effects on the mean duration of the unemployment spells thus critically hinge on the share of the unemployment spells that last long enough for the new hazard rate spikes to become relevant. This is mirrored in the facts that the effect on average unemployment duration is quite small compared to the reduction in PBD, and that the reduction from 18 to 12 months of PBD has larger impact than the 36 to 24 months reduction.

Loss of UI eligibility might seem to reduce the tendency to separate from employment. In addition to reducing the financial costs related to the UI system mechanically, by reducing the share of the labor force covered by UI, the behavioral response in the treated group implies even lower benefit expenditures and increased tax income to the government.

The chapter also studies how the reforms affect transitions to health related benefits and education. As health related benefits and student loans might be alternative income sources to UI we could expect to see an increase in the inflow into these states, both from employment and from unemployment. I find no effects on entry into these states from employment. When studying the transition into these states from unemployment, the hazards might seem to increase around the new timing of benefit exhaustion, but the changes are not significantly different from 0. There is no effect on the share that ends up on these alternative, possibly quite absorbing, income sources eventually either. The reforms thus seem to affect the balance between unemployment and employment, more than the balance between unemployment and alternative sources of income.

The fact that the share ending up on employment eventually, after a period of unemployment, seems to be unaffected by the reforms, also holds a message related to the productivity of long search periods. If long search periods were productive, this share would have been reduced, as the liquidity constraint from benefit exhaustion would have pushed more job seekers into alternative and absorbing income sources. The results in this chapter indicate that the productivity of job search conducted during the last third of the UI spell prior to the reform was not high enough to outweigh the effects of negative duration dependence on the prospects of returning to work.

In the reforms studied here, pushing the individual job seekers out of UI earlier does not seem to be done at the expense of pushing them into the "wrong" destination state.

Appendix A: descriptive statistics and full sets of estimated coefficients

Figure 11: Yearly growth in the Basic Amount, compared to wage growth and CPI growth

Source: Statistics Norway and the Inland Revenue Service (skatteetaten)

Table 9: Transition matrix

		State month t+1					
		No registers	Employed	HE	Reg., no UI	UI	Total
State, month t	No registers	31640252	1629362	253122	384740	145278	34052754
	Employed	1578620	152066640	630328	382411	216526	154874525
	HE	262876	621811	22777624	134610	25050	23821971
	Reg., no UI	293526	356486	120047	2015688	407239	3192986
	UI	167289	338470	44069	267606	4308295	5125729

Note: Counts of person months. The term "reg., no UI" refers registered as unemployed, but not receiving UI. "HE" refers to health related benefits, or education. Source: The main sample of the analyses: Monthly observations of labor market states from 2000 to 2008.

Table 10: Descriptive statistics of job spell observations. Mean values

	Eligibility losers		Control group for eligibility losers (cell 3 in Table 2)		Group with PBD reduced from 36 to 24 months in 2003			Group with PBD reduced from 18 to 12 months in 2003		
	2002	2003	2002	2003	2002	2003	2004	2002	2003	2004
Age	36.9	36.8	36.4	36.6	40.6	40.8	40.8	39.5	39.6	39.3
Share, females	0.73	0.73	0.73	0.71	0.84	0.84	0.83	0.80	0.79	0.77
Employment tenure (in years)	2.5	2.4	2.4	2.5	3.1	3.2	3.4	2.7	2.8	2.9
Share, singles with children	16.7 %	18.4 %	17.2 %	18.3 %	14.0 %	14.5 %	16.6 %	16.1 %	17.2 %	20.5 %
Share, couples without children	27.2 %	27.4 %	28.4 %	27.8 %	22.7 %	22.4 %	22.1 %	23.8 %	23.5 %	23.2 %
Share, couples with children	18.1 %	16.4 %	15.1 %	14.8 %	30.5 %	29.4 %	28.3 %	26.6 %	25.8 %	24.3 %
Months on UI previous 5 years	2.2	2.1	2.3	2.1	2.9	2.8	2.8	2.7	2.5	2.5
Months as registered unemployed the past 5 years	5.4	5.1	5.6	5.2	5.0	4.8	4.8	5.1	4.8	4.7
Share from Southern Norway	5.7 %	6.0 %	4.9 %	5.4 %	7.4 %	7.4 %	7.7 %	7.1 %	7.3 %	7.0 %
Share from Western Norway	24.4 %	27.3 %	26.0 %	26.7 %	29.1 %	29.0 %	29.4 %	30.0 %	29.7 %	29.9 %
Share from Mid-Norway	8.5 %	8.9 %	8.8 %	8.5 %	8.6 %	8.8 %	8.7 %	8.6 %	8.6 %	8.6 %
Share from Northern Norway	9.2 %	8.4 %	9.5 %	9.1 %	.	8.3 %	8.1 %	8.1 %	8.1 %	8.0 %
Share with no education	0.8 %	0.6 %	1.0 %	0.7 %	0.3 %	0.3 %	0.3 %	0.4 %	0.3 %	0.4 %
Share with compulsory schooling	30.2 %	27.9 %	26.1 %	26.8 %	29.4 %	28.6 %	27.5 %	29.8 %	28.9 %	26.8 %
Share with higher education	26.1 %	27.8 %	30.7 %	30.0 %	19.3 %	19.7 %	20.9 %	22.4 %	23.4 %	27.1 %
Share with unknown education	3.2 %	3.9 %	3.3 %	4.4 %	0.9 %	1.0 %	1.1 %	1.1 %	1.4 %	1.6 %
Monthly risk of entering registered unemployment	1.64 %	1.49 %	1.67 %	1.82 %	-	-	-	-	-	-
Monthly risk of entering unemployment insurance	-	-	-	-	0.40 %	0.47 %	0.49 %	0.45 %	0.52 %	0.56 %
Monthly risk of entering HE	0.68 %	0.78 %	0.66 %	0.80 %	0.61 %	0.66 %	1.06 %	0.65 %	0.72 %	1.63 %
***N* (Number of person-months)**	42286	39869	67862	60583	444864	421525	410905	328034	308639	307414

Note: Descriptive statistics of the sample used to estimate the effect of eligibility loss and reductions in the potential benefit duration on entry into unemployment

Table 11: Descriptive statistics of unemployment spells observations. Mean values

	Group loosing eligibility in 2003		Control group for eligibility losers (cell 3 in Table 2)		Group having PBD reduced from 36 to 24 months in 2003			Group having PBD reduced from 18 to 12 months in 2004		
	2002	2003	2002	2003	2002	2003	2004	2002	2003	2004
Age	34.0	34.1	34.5	34.8	37.8	38.0	38.0	34.4	34.3	34.8
Share, females	58.8 %	59.9 %	59.9 %	64.0 %	42.6 %	39.2 %	47.1 %	61.7 %	60.7 %	63.2 %
Employment tenure (in years)	1.7	1.7	1.9	2.0	6.1	6.8	6.9	2.1	2.3	2.6
Share, singles with children	4.3 %	3.7 %	5.7 %	5.8 %	30.2 %	31.2 %	31.9 %	31.1 %	35.8 %	37.3 %
Share, couples without children	33.7 %	35.8 %	31.7 %	32.5 %	31.0 %	29.5 %	29.3 %	32.8 %	30.6 %	27.8 %
Share, couples with children	7.8 %	7.8 %	8.4 %	8.9 %	17.2 %	17.2 %	17.1 %	14.4 %	13.6 %	14.4 %
Months on UI previous 5 years	3.7	2.8	4.4	4.6	4.8	4.2	4.3	5.3	4.8	4.7
Months as registered unemployed the past 5 years	9.6	8.4	10.6	10.0	8.1	7.0	7.4	11.0	9.8	9.9
Share from Southern Norway	6.1 %	6.6 %	5.6 %	6.5 %	6.6 %	6.5 %	6.0 %	6.5 %	6.7 %	7.1 %
Share from Western Norway	24.5 %	27.6 %	23.9 %	24.6 %	25.9 %	28.6 %	26.0 %	26.2 %	26.2 %	26.8 %
Share from Mid-Norway	10.9 %	10.0 %	12.3 %	10.6 %	9.8 %	9.3 %	9.7 %	10.9 %	10.3 %	11.1 %
Share from Northern Norway	12.5 %	12.0 %	15.6 %	14.3 %	12.8 %	11.7 %	12.7 %	13.7 %	11.6 %	12.5 %
Share with no education	0.6 %	0.3 %	0.8 %	1.0 %	0.3 %	0.3 %	0.3 %	0.4 %	0.5 %	0.4 %
Share with compulsory schooling	34.9 %	31.9 %	32.9 %	32.7 %	27.3 %	26.1 %	26.6 %	30.0 %	29.6 %	28.7 %
Share with higher education	26.5 %	28.4 %	23.0 %	26.4 %	21.1 %	21.2 %	23.0 %	27.3 %	32.1 %	31.4 %
Share with unknown education	4.1 %	5.0 %	4.3 %	6.2 %	1.1 %	1.4 %	1.7 %	1.9 %	2.2 %	2.7 %
Share exiting to employment	66.2 %	67.2 %	68.1 %	70.4 %	77.9 %	82.4 %	83.4 %	66.4 %	68.9 %	73.1 %
Share exiting to HE	24.1 %	26.3 %	20.8 %	21.3 %	11.3 %	9.7 %	9.2 %	21.4 %	20.4 %	17.8 %
Months per spell observed	13.4	11.3	14.9	13.1	15.7	13.5	13.1	17.3	16.0	14.3
N (number of unemp. Spells)	1546	1196	2403	2384	78630	91577	71214	5407	5834	5616

Note: Descriptive statistics of the sample used to estimate the effect of eligibility loss and reductions in the potential benefit duration on exit from unemployment The statistics reflect the values at the beginning of the spells.

Table 12: Full set of coefficients from logit estimation of the effects of PBD reduction on the entry into insured unemployment.

	Regression 1		Regression 2	
Treatment group	0.0375	(0.0353)	-0.0484	(0.0338)
Post reform	0.153***	(0.0363)	0.0422	(0.0318)
Treatment group×Post reform	0.0259	(0.0487)	0.0480	(0.0476)
4<=duration<=6	-0.118**	(0.0443)	-0.0257	(0.0420)
7<=duration<=9	-0.0241	(0.0447)	-0.0186	(0.0432)
10<=duration<=12	-0.0998*	(0.0457)	-0.0814	(0.0443)
13<=duration<=15	-0.217***	(0.0496)	-0.220***	(0.0482)
16<=duration<=18	-0.320***	(0.0572)	-0.352***	(0.0566)
19<=duration<=21	-0.417***	(0.0681)	-0.439***	(0.0676)
22<=duration<=23	-0.683***	(0.0818)	-0.603***	(0.0779)
25<=duration	-1.180***	(0.0450)	-1.141***	(0.0436)
No education	0.580***	(0.137)	0.587***	(0.142)
Compulsory education	0.248***	(0.0285)	0.278***	(0.0280)
Higher education	-0.203***	(0.0336)	-0.188***	(0.0321)
25<=age<=29	0.0386	(0.0364)	0.00287	(0.0353)
35<=age<=39	-0.0636	(0.0402)	-0.126**	(0.0399)
40<=age<=49	-0.196***	(0.0400)	-0.162***	(0.0387)
50<=age<=59	-0.309***	(0.0595)	-0.352***	(0.0588)
Western immigrant	0.000880	(0.0484)	0.0651	(0.0446)
Non-western immigrant	0.640***	(0.0371)	0.482***	(0.0376)
Female	-0.177***	(0.0289)	-0.0499	(0.0283)
1<=UI experience <=6	0.277***	(0.0484)	0.361***	(0.0458)
7<=UI experience	0.709***	(0.0302)	0.736***	(0.0297)
Tenure=0	-0.190***	(0.0424)	0.0122	(0.0374)
6<=Tenure<=10	-0.246***	(0.0689)	-0.270***	(0.0588)
11<=Tenure<=20	-0.345***	(0.0776)	-0.313***	(0.0644)
21<=Tenure	-0.191	(0.160)	-0.356**	(0.130)
Single, with child(ren)	0.0954	(0.0959)	0.0690	(0.0919)
Couple, no child(ren)	0.0658	(0.0943)	0.0782	(0.0904)
Couple, with child(ren)	-0.194*	(0.0935)	-0.266**	(0.0901)
Constant term	-4.790***	(0.114)	-4.824***	(0.109)
N	1510441		1459202	

Note: Standard errors in parentheses, clustered at the individual level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 13: Full set of coefficients from logit estimation of the effects of UI eligibility loss on entry into registered unemployment and HE

	Regression 3		Regression 4	
Treatment	-0.0449	(0.0562)	0.0401	(0.0839)
Post reform	0.0592	(0.0471)	0.0919	(0.0695)
Treatment x post reform	-0.202**	(0.0774)	-0.0524	(0.111)
Anticipation period	-0.117	(0.0736)	-0.477***	(0.124)
TG ×anticip.	-0.0770	(0.120)	0.0848	(0.190)
4≤duration≤ 6	-0.691***	(0.0507)	-0.406***	(0.0904)
7≤duration≤ 9	-0.814***	(0.0536)	-0.0961	(0.0867)
10 ≤duration≤ 12	-0.978***	(0.0573)	-0.0727	(0.0878)
13 ≤duration≤ 15	-1.176***	(0.0661)	-0.127	(0.0972)
16 ≤duration≤ 18	-1.229***	(0.0812)	-0.559***	(0.131)
19 ≤duration≤ 21	-1.379***	(0.106)	-0.249	(0.144)
22≤duration≤ 24	-1.416***	(0.129)	0.133	(0.145)
25≤duration	-1.652***	(0.110)	-0.701***	(0.155)
No educ	0.269	(0.153)	-2.429*	(0.998)
Compulsort educ	0.250***	(0.0430)	-0.170*	(0.0662)
Higher educ	-0.317***	(0.0494)	-0.165*	(0.0647)
25<=age<=29	0.00120	(0.0492)	0.270***	(0.0721)
35<=age<=39	-0.0791	(0.0556)	0.0120	(0.0861)
40<=age<=49	-0.222***	(0.0571)	-0.0157	(0.0853)
50<=age<=59	-0.290**	(0.0996)	-0.407**	(0.141)
Western immigrant	0.0695	(0.0706)	-0.0490	(0.0941)
Non-western immigrant	0.756***	(0.0448)	-0.384***	(0.0855)
Female	-0.0970*	(0.0396)	0.129*	(0.0615)
1<=UI experience <=6	0.134*	(0.0608)	-0.0284	(0.0870)
7<=UI experience	0.348***	(0.0545)	-0.0263	(0.0870)
Tenure=0	-0.511***	(0.0664)	0.354**	(0.121)
6<=Tenure<=10	-0.257*	(0.113)	-0.551*	(0.241)
11<=Tenure<=20	-0.376**	(0.135)	-0.213	(0.237)
21<=Tenure	-0.721	(0.392)	-1.345	(1.015)
Single, with child(ren)	0.132	(0.157)	-0.0880	(0.204)
Couple, no child(ren)	0.251	(0.155)	-0.282	(0.203)
Couple, with child(ren)	-0.0909	(0.169)	-0.318	(0.220)
Constant term	-2.921***	(0.178)	-4.662***	(0.251)
N	213428		213428	

Standard errors in parentheses, clustered at the individual level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 14: Full set of coefficients from logit estimation of the effects of UI eligibility loss on entry into wide definition of unemployment: Non-employment, no education and no health related benefits.

	Regression 5		Regression 6		Regression 7	
	Controlgroup = Cell 3, obs 2002-2003		Controlgroup = Cell 1,3 and 6 obs 2002-2003		Controlgroup = Cell 1, 3 and 6obs 2000-2003	
Treatment group (TG)	0.138***	(0.0406)	0.0612	(0.0353)	0.0881***	(0.0207)
Year=2000					-0.170***	(0.0246)
Year=2001					-0.119***	(0.0204)
Year=2003	0.0440	(0.0363)	0.0539*	(0.0231)	0.0131	(0.0224)
TG x post reform	-0.0568	(0.0563)	-0.0619	(0.0488)	-0.0707	(0.0405)
Anticip. period	-0.495***	(0.0633)	-0.544***	(0.0401)	-0.662***	(0.0389)
TG ×anticip.	0.0235	(0.0951)	0.0856	(0.0819)	0.0548	(0.0773)
4≤duration≤ 6	-0.253***	(0.0397)	-0.368***	(0.0283)		
7≤duration≤ 9	-0.509***	(0.0431)	-0.602***	(0.0317)		
10 ≤duration≤ 12	-0.423***	(0.0431)	-0.523***	(0.0321)		
13 ≤duration≤ 15	-0.740***	(0.0513)	-0.893***	(0.0399)		
16 ≤duration≤ 18	-0.813***	(0.0617)	-0.902***	(0.0473)		
19 ≤duration≤ 21	-0.935***	(0.0772)	-1.029***	(0.0592)		
22≤duration≤ 24	-0.841***	(0.0860)	-0.820***	(0.0622)		
25≤duration	-1.240***	(0.0749)	-1.401***	(0.0414)		
No educ	0.214	(0.136)	0.221*	(0.102)	0.134	(0.0853)
Compulsory educ	0.210***	(0.0343)	0.164***	(0.0249)	0.152***	(0.0183)
Higher educ	-0.0347	(0.0347)	-0.0779**	(0.0257)	-0.0275	(0.0191)
25<=age<=29	0.0567	(0.0375)	0.0235	(0.0285)	0.105***	(0.0210)
35<=age<=39	-0.151***	(0.0436)	-0.127***	(0.0326)	-0.150***	(0.0243)
40<=age<=49	-0.123**	(0.0422)	-0.0508	(0.0316)	-0.151***	(0.0236)
50<=age<=59	-0.198**	(0.0734)	-0.0913	(0.0505)	-0.252***	(0.0374)
Western immigrant	0.189***	(0.0463)	0.169***	(0.0344)	0.196***	(0.0258)
Non-western immigrant	0.223***	(0.0384)	0.167***	(0.0296)	0.203***	(0.0224)
Female	-0.0994**	(0.0305)	-0.0983***	(0.0226)	-0.173***	(0.0168)
1<=UI experience <=6	0.0322	(0.0594)	0.0500	(0.0432)	-0.0174	(0.0319)
7<=UI experience	0.138**	(0.0444)	0.123***	(0.0322)	0.0631**	(0.0223)
Tenure=0	-0.167**	(0.0551)	-0.0738	(0.0384)	-0.0270	(0.0299)
6<=Tenure<=10	0.113	(0.0886)	0.0490	(0.0629)	-0.0117	(0.0491)
11<=Tenure<=20	0.0348	(0.101)	0.0400	(0.0681)	-0.0234	(0.0544)
21<=Tenure	-0.117	(0.249)	-0.200	(0.154)	-0.258	(0.133)
Single, with child	0.168	(0.123)	0.168*	(0.0854)	0.0628	(0.0617)
Couple, no child	0.0914	(0.122)	0.0976	(0.0844)	0.00243	(0.0608)
Couple, with child	0.0808	(0.126)	0.00782	(0.0865)	-0.172**	(0.0622)
_cons	-2.851***	(0.140)	-2.756***	(0.0976)	-3.111***	(0.0716)
N	198381		384821		786498	

Standard errors in parentheses, clustered at the individual level. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 15: Full set of coefficients from a duration model estimated to identify the effect on eligibility loss on the hazard from unemployment to employment.

	Transition to employment		Transition to HE	
	Coeff	**Std.error**	**Coeff**	**Std.error**
Non-western immigrant	-0.09564*	0.048135	-0.27697*	0.081935
Female	0.19271*	0.039033	0.010633	0.060292
Western immigrant	-0.07941	0.067112	0.02538	0.099483
25<=age<=29	0.073672	0.046083	0.119039	0.076225
35<=age<=39	-0.02038	0.053242	-0.06077	0.093961
40<=age<=49	-0.00218	0.060454	-0.02675	0.105163
50<=age<=59	-0.20787*	0.092012	-0.12438	0.151861
1<=UI experience <=6	0.035048	0.078533	-0.02335	0.119816
7<=UI experience	0.038116	0.05721	-0.01043	0.092735
Single, with child(ren)	0.023224	0.092771	-0.49538*	0.141607
Couple, no child(ren)	-0.03964	0.098126	-0.50276*	0.149181
Couple, with child(ren)	0.005979	0.094463	-0.75431*	0.146441
Tenure=0	-0.07847	0.040861	0.088946	0.067326
6<=Tenure<=10	-0.09577	0.082793	-0.06527	0.134838
11<=Tenure<=20	-0.23396*	0.102413	-0.14813	0.155473
21<=Tenure	0.25202	0.235498	0.33453	0.368662
No education	-0.16188	0.192083	-0.66759	0.435983
Compulsory schooling	-0.18433*	0.041761	-0.32038*	0.07307
Higher education	0.052353	0.047405	0.40802*	0.069887
Duration=2	-0.29816*	0.102576	-0.29385	0.175358
Duration=3	-0.05607	0.103373	-0.37196	0.195761
Duration=4	-0.30946*	0.121237	-0.70254*	0.23994
Duration=5	-0.3364*	0.130555	-0.21225	0.216258
Duration=6	-0.43644*	0.142965	-0.58785*	0.265093
7<=Duration<=9	-0.26663*	0.113033	-0.30538	0.192751
10<=Duration<=12	-0.58631*	0.138424	-0.43931	0.229147
13<=Duration<=15	-0.6263*	0.150632	-0.15765	0.23262
16<=Duration<=18	-0.52737*	0.153707	-0.63515*	0.295583
19<=Duration<=24	-0.4944*	0.142963	-0.29833	0.242367
25<=Duration<=30	-0.72065*	0.171066	-0.47479	0.28925
31<=Duration<=48	-0.73754*	0.156196	-0.92846*	0.300999
Duration=1×PR	0.10319	0.088402	-0.04659	0.152997
Duration=2×PR	-0.19514	0.123477	0.160025	0.191148
Duration=3×PR	-0.0735	0.117967	0.000325	0.230051
Duration=4×PR	0.079808	0.140313	0.079264	0.294381
Duration=5×PR	0.06754	0.15304	-0.25041	0.273231
Duration=6×PR	-0.01772	0.173971	-0.17359	0.345871
7<=Duration<=9×PR	-0.08931	0.111616	0.168645	0.188634
10<=Duration<=12×PR	0.088976	0.14116	-0.22611	0.260963
13<=Duration<=15×PR	0.200719	0.15386	-0.25577	0.254386
16<=Duration<=18×PR	0.11162	0.162576	0.400906	0.305374
19<=Duration<=24×PR	0.110151	0.129053	-0.16575	0.236791
25<=Duration<=30×PR	0.3072	0.163527	-0.08299	0.302096
31<=Duration<=48×PR	0.126418	0.126648	0.569878*	0.255417
Duration=1×TG	-0.1035	0.104827	0.247796	0.157981
Duration=2×TG	0.204603	0.126269	0.439235*	0.199904
Duration=3×TG	0.00244	0.133028	0.219126	0.246693
Duration=4×TG	0.041187	0.163177	0.859806*	0.27651
Duration=5×TG	0.065371	0.177832	0.302981	0.271485
Duration=6×TG	0.15087	0.194429	-0.02268	0.386708
7<=Duration<=9×TG	0.003552	0.127698	-0.09193	0.233697

10<=Duration<=12×TG	0.27207	0.156347	0.50015*	0.249625	
13<=Duration<=15×TG	0.113756	0.18402	-0.02785	0.285851	
16<=Duration<=18×TG	0.165456	0.187192	0.402387	0.349393	
19<=Duration<=24×TG	0.202502	0.147949	-0.59849	0.331432	
25<=Duration<=30×TG	0.018181	0.204217	-0.28317	0.381769	
31<=Duration<=48×TG	-0.08075	0.153832	-0.19457	0.347123	
Duration=1×PR×TG	0.279439	0.143157	0.373409	0.219828	
Duration=2×PR×TG	0.407105*	0.185776	-0.0851	0.286216	
Duration=3×PR×TG	0.152732	0.199737	0.409684	0.347014	
Duration=4×PR×TG	-0.13458	0.248359	-0.72399	0.451844	
Duration=5×PR×TG	0.191514	0.25749	0.548203	0.402978	
Duration=6×PR×TG	0.173884	0.291704	1.232537*	0.522463	
7<=Duration<=9×PR×TG	0.231184	0.194683	0.022824	0.348328	
10<=Duration<=12×PR×TG	-0.01279	0.236191	-0.79439	0.496777	
13<=Duration<=15×PR×TG	-0.08073	0.274521	0.470762	0.431411	
16<=Duration<=18×PR×TG	-0.36757	0.303167	-1.04711	0.60944	
19<=Duration<=24×PR×TG	-0.14302	0.227594	0.871471	0.458912	
25<=Duration<=30×PR×TG	0.106623	0.287721	1.238929*	0.504825	
31<=Duration<=48×PR×TG	-0.05278	0.24426	-0.15542	0.491597	
Unobserved heterogeneity:					Prob:
Intercept 1	-2.79537	0.159127	-3.67462	0.256694	0.541433
Intercept 2	-1.25839	0.136687	-1.81253	0.212792	0.458567
N (spells)	7529				

Note: Stars indicate $p < 0.05$, PR is Post Reform, TG is Treatment Group

Table 16: Full set of coefficients from a duration model estimated to identify the effect on PBD reduction from 36 to 24 months on the hazard from unemployment to employment.

	Transition to employment		Transition to HE	
	Coeff	**Std.error**	**Coeff**	**Std.error**
Non-western immigrant	-0.53482*	0.014788	-0.2704*	0.040096
Female	-0.31673*	0.008953	0.387872*	0.028949
Western immigrant	-0.15938*	0.014521	-0.05289	0.039312
25<=age<=29	0.006971	0.010746	0.452712*	0.030502
35<=age<=39	-0.04099*	0.011494	-0.20187*	0.032408
40<=age<=49	-0.08633*	0.013488	-0.24647*	0.037533
50<=age<=59	-0.28133*	0.017917	-0.35573*	0.050238
1<=UI experience <=6	0.495407*	0.012513	0.033004	0.036368
7<=UI experience	0.18722*	0.013855	-0.216*	0.038885
Single, with child(ren)	0.114454*	0.010496	-0.29721*	0.029876
Couple, no child(ren)	0.204667*	0.013281	-0.18401*	0.038869
Couple, with child(ren)	0.187432*	0.011844	-0.47148*	0.036504
Tenure=0	0.086069*	0.0263	0.225175*	0.064358
6<=Tenure<=10	0.138518*	0.01248	-0.09195*	0.035944
11<=Tenure<=20	0.205255*	0.01529	-0.24352*	0.045761
21<=Tenure	0.395096*	0.023025	-0.16117*	0.072148
No education	-0.04381	0.068473	-0.75088*	0.225032
Compulsory schooling	-0.20914*	0.009458	-0.24018*	0.027243
Higher education	-0.1576*	0.011059	0.886241*	0.037787
Duration=2	0.30835*	0.08542	0.187393	0.140659
Duration=3	0.37258*	0.086483	0.32347*	0.150632
Duration=4	0.290721*	0.090701	0.435253*	0.159553
Duration=5	0.425806*	0.09012	0.083022	0.186098
Duration=6	0.232558*	0.097585	-0.00096	0.198937
7<=Duration<=9	0.13375	0.081363	0.155106	0.155191
10<=Duration<=12	-0.12163	0.090567	0.385886*	0.160122
13<=Duration<=15	-0.09041	0.09452	0.47105*	0.171651
16<=Duration<=18	0.002805	0.097483	0.583471*	0.180772
19<=Duration<=21	0.196982*	0.099383	0.759903*	0.189754
22<=Duration<=24	0.203308	0.10694	0.771882*	0.210552
25<=Duration<=27	0.029102	0.121079	0.914395*	0.218655
28<=Duration<=30	0.115919	0.127193	0.960539*	0.233048
31<=Duration<=33	0.24516	0.13112	0.768518*	0.26487
34<=Duration<=36	0.205628	0.143298	0.866665*	0.274941
37<=Duration<=39	0.166986	0.15762	1.245096*	0.268091
40<=Duration<=42	0.226837	0.191768	0.809163*	0.399731
43<=Duration<=48	0.244367	0.137676	1.084145*	0.26309
Duration=1×PR	-0.04328	0.089661	-0.34431*	0.153017
Duration=2×PR	0.070805	0.078807	-0.09782	0.154239
Duration=3×PR	-0.04802	0.083259	-0.07363	0.162568
Duration=4×PR	0.030069	0.089715	-0.14031	0.173681
Duration=5×PR	-0.15647	0.092971	-0.01873	0.211647
Duration=6×PR	0.019416	0.102625	0.134209	0.225546
7<=Duration<=9×PR	0.042334	0.070108	0.001903	0.152663
10<=Duration<=12×PR	0.132339	0.08676	-0.2995	0.168061
13<=Duration<=15×PR	0.214134*	0.093177	0.172194	0.168427
16<=Duration<=18×PR	0.071051	0.101753	0.004427	0.18751
19<=Duration<=21×PR	0.099164	0.10471	-0.26718	0.208401
22<=Duration<=24×PR	0.020604	0.12014	-0.04199	0.226429
25<=Duration<=27×PR	0.354343*	0.135669	-0.30202	0.253307
28<=Duration<=30×PR	0.236097	0.148246	-0.32254	0.275755
31<=Duration<=33×PR	0.289005	0.154812	-0.16731	0.319021

34<=Duration<=36×PR	0.286375	0.175622	-0.11357	0.336693	
37<=Duration<=39×PR	0.101642	0.204288	-0.58085	0.350977	
40<=Duration<=42×PR	0.145193	0.2544	0.064573	0.490249	
43<=Duration<=48×PR	0.41428*	0.159771	-0.18313	0.299104	
Duration=1×TG	0.423433*	0.066948	-0.69055*	0.117099	
Duration=2×TG	0.309779*	0.06092	-0.54227*	0.125461	
Duration=3×TG	0.274918*	0.062583	-0.69945*	0.132358	
Duration=4×TG	0.342331*	0.068225	-0.87989*	0.14069	
Duration=5×TG	0.110052	0.068438	-0.50281*	0.169518	
Duration=6×TG	0.252276*	0.077929	-0.38542*	0.182963	
7<=Duration<=9×TG	0.194609*	0.054353	-0.64325*	0.125547	
10<=Duration<=12×TG	0.254296*	0.067557	-0.75351*	0.12859	
13<=Duration<=15×TG	0.140065	0.073173	-0.7366*	0.139183	
16<=Duration<=18×TG	0.105743	0.076874	-0.87782*	0.149424	
19<=Duration<=21×TG	-0.05665	0.079841	-1.04796*	0.157457	
22<=Duration<=24×TG	-0.06397	0.089361	-1.11484*	0.181333	
25<=Duration<=27×TG	0.136517	0.106523	-1.20238*	0.190603	
28<=Duration<=30×TG	0.150181	0.113024	-1.35466*	0.206903	
31<=Duration<=33×TG	0.088717	0.117683	-0.97429*	0.240235	
34<=Duration<=36×TG	0.343899*	0.130945	-1.0041*	0.251736	
37<=Duration<=39×TG	0.714235*	0.146148	-1.19302*	0.241829	
40<=Duration<=42×TG	0.750693*	0.183721	-0.67898	0.386394	
43<=Duration<=48×TG	0.752635*	0.123243	-0.74072*	0.228761	
Duration=1×PR×TG	-0.03999	0.091364	0.282799	0.163945	
Duration=2×PR×TG	-0.02429	0.080622	-0.01753	0.164089	
Duration=3×PR×TG	0.039815	0.085323	-0.02461	0.173615	
Duration=4×PR×TG	-0.10629	0.092085	-0.05665	0.186813	
Duration=5×PR×TG	0.137292	0.09579	0.009024	0.22266	
Duration=6×PR×TG	0.033439	0.105617	-0.26676	0.237347	
7<=Duration<=9×TG	0.063442	0.072405	-0.19573	0.161226	
10<=Duration<=12×PR×TG	0.176656*	0.089473	0.062591	0.177053	
13<=Duration<=15×PR×TG	0.049864	0.096606	-0.41682*	0.178316	
16<=Duration<=18×PR×TG	0.211381*	0.105376	-0.24995	0.198456	
19<=Duration<=21×PR×TG	0.186579	0.108877	0.069086	0.219675	
22<=Duration<=24×PR×TG	0.425915*	0.124367	0.063487	0.238188	
25<=Duration<=27×PR×TG	0.359851*	0.139847	0.488854	0.264733	
28<=Duration<=30×PR×TG	0.435414*	0.152722	0.501925	0.289818	
31<=Duration<=33×PR×TG	0.411357*	0.159934	0.351599	0.331275	
34<=Duration<=36×PR×TG	0.288032	0.180703	0.349334	0.349806	
37<=Duration<=39×PR×TG	0.269899	0.209122	0.629989	0.365311	
40<=Duration<=42×PR×TG	0.145289	0.260897	-0.0997	0.507856	
43<=Duration<=48×PR×TG	-0.13302	0.165651	-0.07429	0.311896	
Unobserved heterogeneity:					Prob:
intercept 1	-1.62711	0.085235	-19.0051	0	0.37977
intercept 2	-3.11929	0.157228	-4.50876	0.239438	0.206425
intercept 3	-2.5457	0.262765	-8.82523	4.698239	0.160114
intercept 4	-4.02694	0.27045	-57.9102	0	0.090039
intercept 5	-2.22661	0.222643	-2.48661	0.337699	0.075643
intercept 6	-5.10456	0.514304	-2.80709	0.169705	0.047799
intercept 7	-31.0554	0	-5.47886	0.437108	0.027364
intercept 8	-2.28642	0.693371	-0.848	0.511985	0.012846
N (spells)	181493				

Note: Stars indicate p < 0.05, , PR is Post Reform, TG is Treatment Group

Table 17: Full set of coefficients from a duration model estimated to identify the effect on PBD reduction from 18 to 12 months on the hazard from unemployment to employment.

	Transition to employment		Transition to HE	
	Coeff	**Std.error**	**Coeff**	**Std.error**
Non-western immigrant	-0.53402*	0.014855	-0.19063*	0.036312
Female	-0.29685*	0.008721	0.343372*	0.022782
Western immigrant	-0.15158*	0.014531	-0.04228	0.03601
25<=age<=29	0.020701	0.010887	0.384053*	0.027851
35<=age<=39	-0.04935*	0.011514	-0.15204*	0.031186
40<=age<=49	-0.09632*	0.013348	-0.18463*	0.034969
50<=age<=59	-0.29039*	0.01777	-0.21082*	0.046957
1<=UI experience <=6	0.493864*	0.012621	0.074913*	0.033209
7<=UI experience	0.1703*	0.013988	-0.12698*	0.034572
Single, with child(ren)	0.065859*	0.010415	-0.27182*	0.026139
Couple, no child(ren)	0.200554*	0.013193	-0.10319*	0.035338
Couple, with child(ren)	0.14639*	0.011705	-0.39686*	0.03069
Tenure=0	0.180409*	0.025439	-0.03446	0.054996
6<=Tenure<=10	0.12191*	0.011839	-0.1189*	0.03179
11<=Tenure<=20	0.184339*	0.01518	-0.25231*	0.041938
21<=Tenure	0.348631*	0.022996	-0.23635*	0.068498
No education	-0.06321	0.069306	-0.82173*	0.220932
Compulsory schooling	-0.18171*	0.009527	-0.26771*	0.026249
Higher education	-0.09454*	0.010532	0.707186*	0.025697
Duration=2	0.323391*	0.016124	0.192865*	0.053319
Duration=3	0.296783*	0.01747	0.12671*	0.056349
Duration=4	0.21344*	0.019283	-0.08065	0.061956
Duration=5	0.172733*	0.020926	0.102145	0.061124
Duration=6	0.193105*	0.022268	-0.01145	0.065348
7<=Duration<=9	0.087537*	0.020341	-0.20192*	0.055927
10<=Duration<=12	0.090759*	0.023061	-0.15304*	0.060288
13<=Duration<=15	-0.04236	0.02586	-0.07681	0.064008
16<=Duration<=18	0.028244	0.027615	-0.11249	0.069208
19<=Duration<=21	0.0555	0.029556	-0.06131	0.072901
22<=Duration<=24	0.206021*	0.031235	0.100889	0.076462
25<=Duration<=27	0.488272*	0.032719	0.326461*	0.080318
28<=Duration<=30	0.533829*	0.035957	0.224169*	0.090271
31<=Duration<=33	0.620754*	0.039304	0.430469*	0.09484
34<=Duration<=36	0.703981*	0.042842	0.56852*	0.101867
37<=Duration<=39	0.830746*	0.046858	0.595081*	0.113184
40<=Duration<=42	0.846971*	0.056677	0.609889*	0.134194
43<=Duration<=48	0.861679*	0.052178	0.637913*	0.119088
Duration=1×PR	0.041569*	0.018049	-0.31492*	0.063128
Duration=2×PR	-0.13412*	0.017667	-0.24538*	0.059238
Duration=3×PR	-0.08846*	0.019319	-0.3186*	0.066016
Duration=4×PR	-0.00443	0.021307	-0.16148*	0.07349
Duration=5×PR	-0.09053*	0.023852	-0.40578*	0.075507
Duration=6×PR	-0.15127*	0.025644	-0.13877	0.077161
7<=Duration<=9×PR	-0.09366*	0.018116	-0.11422*	0.05329
10<=Duration<=12×PR	-0.02467	0.020699	-0.11436*	0.058037
13<=Duration<=15×PR	0.14561*	0.024032	-0.06891	0.06158
16<=Duration<=18×PR	0.18493*	0.026114	0.010169	0.067955
19<=Duration<=21×PR	0.232027*	0.028798	0.141801*	0.070971
22<=Duration<=24×PR	0.306305*	0.03048	0.044466	0.075394
25<=Duration<=27×PR	0.275781*	0.031906	0.012987	0.078489
28<=Duration<=30×PR	0.25746*	0.036955	0.322077*	0.087962

31<=Duration<=33×PR	0.15236*	0.04272	0.018103	0.098442	
34<=Duration<=36×PR	0.18808*	0.047619	0.140938	0.102454	
37<=Duration<=39×PR	0.072525	0.054041	0.159753	0.115843	
40<=Duration<=42×PR	0.177738*	0.069685	0.011612	0.158756	
43<=Duration<=48×PR	0.19425*	0.052075	-0.02472	0.1103	
Duration=1×TG	-0.45513*	0.064708	0.510496*	0.111351	
Duration=2×TG	-0.3486*	0.055873	0.550184*	0.106346	
Duration=3×TG	-0.36903*	0.060532	0.658132*	0.112537	
Duration=4×TG	-0.28162*	0.064003	0.815749*	0.124374	
Duration=5×TG	-0.2863*	0.069033	0.354663*	0.145431	
Duration=6×TG	-0.31857*	0.073146	0.505459*	0.152077	
7<=Duration<=9×TG	-0.27888*	0.050587	0.6718*	0.104854	
10<=Duration<=12×TG	-0.43539*	0.060883	0.532202*	0.124421	
13<=Duration<=15×TG	-0.17632*	0.064899	1.011415*	0.115735	
16<=Duration<=18×TG	-0.28776*	0.073427	1.002604*	0.136563	
19<=Duration<=21×TG	-0.08612	0.075225	0.877378*	0.158676	
22<=Duration<=24×TG	-0.30761*	0.087066	0.990355*	0.166228	
25<=Duration<=27×TG	-0.43598*	0.090593	0.684548*	0.193266	
28<=Duration<=30×TG	-0.52504*	0.102545	0.839149*	0.217837	
31<=Duration<=33×TG	-0.44036*	0.107556	0.6282*	0.243305	
34<=Duration<=36×TG	-0.5838*	0.123722	0.659032*	0.25515	
37<=Duration<=39×TG	-0.9516*	0.149461	0.552321	0.289972	
40<=Duration<=42×TG	-0.87541*	0.185655	0.744327*	0.338164	
43<=Duration<=48×TG	-0.63208*	0.112749	0.719847*	0.233001	
Duration=1×PR×TG	0.051428	0.088762	-0.05557	0.169738	
Duration=2×PR×TG	0.128744	0.078278	0.228143	0.149977	
Duration=3×PR×TG	0.108499	0.084646	0.111345	0.165661	
Duration=4×PR×TG	0.055434	0.089047	-0.26902	0.191267	
Duration=5×PR×TG	0.058415	0.098047	0.072494	0.220589	
Duration=6×PR×TG	0.266673*	0.100701	-0.28963	0.232912	
7<=Duration<=9×TG	0.09856	0.070495	0.051101	0.144332	
10<=Duration<=12×PR×TG	0.218427*	0.082577	0.223491	0.167564	
13<=Duration<=15×PR×TG	0.271269*	0.086438	0.117855	0.159554	
16<=Duration<=18×PR×TG	0.21963*	0.100601	-0.30102	0.20464	
19<=Duration<=21×PR×TG	0.081233	0.106808	-0.1727	0.230435	
22<=Duration<=24×PR×TG	0.145372	0.121817	-0.64762*	0.27311	
25<=Duration<=27×PR×TG	0.011156	0.132883	0.037383	0.276401	
28<=Duration<=30×PR×TG	0.038329	0.152511	-0.17793	0.305316	
31<=Duration<=33×PR×TG	-0.23882	0.175221	0.523172	0.320687	
34<=Duration<=36×PR×TG	0.081249	0.183101	0.046235	0.363863	
37<=Duration<=39×PR×TG	0.322179	0.217255	-0.20878	0.432425	
40<=Duration<=42×PR×TG	0.240838	0.269919	-1.02352	0.68655	
43<=Duration<=48×PR×TG	-0.06134	0.17531	-0.14782	0.34449	
Unobs. heter.:					Prob
intercept 1	-1.34483	0.026583	-4.79034	0.104918	0.450779
intercept 2	-2.51955	0.039489	-7.2175	0.368891	0.35481
intercept 3	-3.61048	0.089263	-3.28365	0.068366	0.107604
intercept 4	-4.69095	0.08579	-6.33787	0.150888	0.086807
N (spells)	174294				

Note: Stars indicate p < 0.05, PR is Post Reform, TG is Treatment Group

Appendix B: The routine used to determine UI entitlements among repeat users.

The procedure used to determine eligibility and entitlements for repeat users is described in this appendix. With the term "repeat users", I mean everyone who has started a UI spell during the present calendar year, or during the two previous calendar years *or* who has had a break from their old UI spell that net of participation on labor market measures is shorter than 12 months. The first group is repeat users in the sense that if they were to enter unemployment again, previous UI spells might affect their income basis, since work income from the three calendar years prior to the beginning of a new UI spell is "zeroed out". The second group will be able to return to their previous UI spell, without having to apply again. Hence, it is harder determine their entitlements precisely. The procedure conducted to do so is based on detailed knowledge about the regulations of the Norwegian UI system, income- and labor market history from 1989 to 2007, and some assumptions on the choices made by the UI applicants.

The first step is to find a month for every individual, between 1989 and 1999, where the person has not received UI during the present calendar year, or during the two previous calendar years. We know that if the person starts a UI spell sometime after this month, this spell will be fresh, and entitlements will be calculated based on the observed work incomes from the three previous calendar years. Then I follow the individual along the time line. At some time, let us say in calendar year t, he becomes unemployed. If he starts receiving UI, the procedure for calculating entitlements described in section 3 is applied. Work income from the years $t-1$, $t-2$ and $t-3$ is cancelled, meaning that they cannot be included in any later income basis. At some point in time, he stops receiving UI, and possibly returns to work. Until he has worked[27] at least three months since his last month on UI benefits, it is known for sure that if he makes a transition to unemployment again, he will return to his old UI spell, with the same benefits as before, and he will continue exhausting the potential benefit duration from the previous spell. If his break from UI, net of months spent participating in labor market measures, is longer than 12 months, we also know for sure, that he will have to start a new UI spell, with income not cancelled. After three

[27] Employed is more widely defined here than the state "employed" in the main data set. This is done to make months as part time employed count, even if the person is also, for example, part time unemployed, or on some graded health related benefit. This is in line with the regulations in the UI system.

months as employed, he can choose to start a new spell, with work income earned from year t and onwards potentially included in the income basis. If he has worked three months or more since his last month on UI benefits, but had a break from UI lasting less than a year, he can choose whether he wants to start a new spell, or to continue on the old one. If the old spell is exhausted, meaning that the months on UI since his last start of a new UI spell equals or exceeds the PBD he was entitled to, a new fresh spell will be started. So far in the description of the procedure, the regulations and the data on income and labor market history has been determining the entitlements of the individuals, and no additional assumptions have been necessary. The assumptions on the behavior of the UI applicants are first needed when facing individuals that can choose to start a new fresh UI-spell, or to continue on the previous one. I assume that persons with one or two months left of their old UI, would want to start a new spell, given that the benefits in the new spell are positive. If the entitlements left in the old UI spell has a duration of three months or more, he will make the choice that gives him the highest monthly benefits. The regulations and the assumptions are summarized in Table 18.

The routine is based on assumptions on how the individual prioritizes between high levels- and long durations- of benefits. These assumptions might represent the decisions made by the unemployed individuals quite poorly. In worst case the result might be that eligibility is determined more precisely without the routine (i.e. based on income data only, without considering earlier UI- and employment history at all.)

We can approach the question of the preciseness of the eligibility determination by studying the relationship between determined eligibility and reception of UI, given that a person is unemployed.

I do that by studying registered (insured or uninsured) unemployment spells that lasted for three months or longer, and I split them up by whether or not UI was received during those first three months, and whether or not they are measured to be eligible for UI at the beginning of the unemployment spell.

Table 19 presents the cross tabulation of determined eligibility and UI on all registered UI spells lasting longer than 6 months.

Table 18: Description of how entitlements for repeat users are calculated

	Less than 12 months since previous UI		12 months or more since previous UI
	Employed 0-2 months since previous UI	**Employed 3-11 months since previous UI**	
Previous UI spell is exhausted	The person will not be eligible for UI (1.31 %)	A new UI spell will be started , but income included in the income basis for earlier UI spells is excluded (0.69 %)	A new UI spell will be started. (52.76 %)
1-2 months are left of previous UI spell	The person will continue on his old spell (16.19 %)	If starting a new UI spell provides positive UI benefits, a new UI spell will be started. (0.09 %) Otherwise, the previous spell will be continued. (0.20 %)	
3 months or more are left of previous UI spell		If starting a new spell results in higher benefits than continuing on the old spell, a new UI spell will be started. (4.31 %) Otherwise, the old spell will be continued. (24.46 %)	

Note: A repeat user is defined as a person that at a given point in time started a UI spell during the present calendar year or during the two previous calendar years or who has had a break from their old UI spell that net of participation on labor market measures is shorter than 12 months. The term "a new UI spell will be started" still means that income included in the income basis for earlier UI spells will be eliminated. The percentages in the parenthesis indicate the share of monthly observations of repeat user observations that are placed in each cell.

Table 19: Registered unemployment spells tabulated by determined UI eligibility and whether UI is paid out or not.

	Not receiving UI	**Receiving UI**	**Total**
Not eligible for UI	138258 (94,0 %)	8780 (6,0 %)	147038
Eligible for UI	159422 (26,8 %)	436345 (73,2 %)	595767
Total	297680	445125	742805

Note: Unemployment spells lasting 3 months or longer are counted. UI eligibility is determined at the beginning of the spell, and receiving UI indicates that UI was paid out at least one month during the first three months of the unemployment spell.

We see that we are able to explain actual UI payments quite well. By those measured as non-eligible, only 6 % receive UI, compared to 73.2 % among those measured as eligible. We see that the share of non-receivers among those measured as eligible is 26.8 %, but given the literature on take up-rates, this is not surprising. (see e.g. Anderson and Meyer, 1997). Unemployed

individuals could always choose not to take up UI benefits, for example because of stigma costs. Having observations in the upper off-diagonal cell is harder to explain. Non-eligible individuals should not be seen receiving UI. This indicates that the procedure applied is not able to measure eligibility without errors

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Chapter II:

Tempted into joblessness?

Abstract

Based on administrative register data from Norway, this chapter investigates the effect of unemployment insurance (UI) eligibility on entry into unemployment. The Norwegian unemployment insurance system is universal, and covers everyone with a certain attachment to the labor market, defined in terms of earlier income from employment. Eligibility to unemployment insurance is conditional on income previous calendar years exceeding certain thresholds. This is a setting well suited for a regression discontinuity design.

I apply two alternative definitions of entry into unemployment. The first defines a person as unemployed when he starts registering at the unemployment office. The main results indicate that UI eligibility raises the monthly risk of transiting from employment to the unemployment register by 0.30 percentage points. My alternative definition interprets a person as unemployed if he leaves a current employment without moving to another job, education or any health- or age related benefit. For this wider activity based definition, I find that UI eligibility raises the risk of entering unemployment by around 0.15 percentage points. Sensitivity analysis show, however, that this result is less robust than when the definition based on the unemployment register is used.

1. Introduction

Unemployment Insurance (UI) systems are often described along three lines. The first one is the criteria for eligibility. What is required, in terms of earlier work income and/or employment duration for the individual to be entitled to UI at all? The second one is the benefit size: How much of the presumed labor income does the unemployment insurance replace. The third dimension is the potential benefit duration: When are UI benefits exhausted?

The focus of the economics literature on UI has mainly been on how these dimensions of unemployment insurance systems affect *exit* from unemployment. The main findings suggest that both the replacement rate and the maximum duration of unemployment benefits have considerable effects on the duration of unemployment spells (See e.g. Røed and Zhang, 2005;

Katz and Meyer, 1990). The knowledge about how the unemployment insurance system affects the level of unemployment in an economy will be limited though, unless the mechanisms related to entry into unemployment are also well understood, since the steady state level of unemployment is affected both by entry and exit into unemployment (see e.g. Mortensen and Pissarides, 1994).[1] A non-negligible effect of UI generosity on job separation rates can possibly constitute a disincentive cost that should be taken into account when the UI system is designed.

A small, but growing empirical literature studies the effects of these three dimensions of UI generosity on entry into unemployment. This chapter contributes to this knowledge by focusing on the role of the eligibility criteria. As I explain in the next section, due to the source of variation in eligibility used to identify the effect, it is better suited than the other contributions to disentangle the effect of UI on the *timing* of unemployment entry, from the effect on *incidence* of unemployment as such. Because of the richness of the data available to me, I also contribute to the register data based studies on this question, by introducing a definition of unemployment entry that makes it possible to separate the effect of UI eligibility on unemployment entry from the effect on reclassification of non-employment time (Card, Chetty, and Weber, 2007).

The Norwegian unemployment insurance system is universal, and covers everyone with a certain attachment to the labor market, defined in terms of earlier work income. Thus, individuals eligible to UI in general have a history of higher income compared to non-eligible individuals. If the risk of unemployment entry is correlated with income for other reasons than through eligibility, a simple comparison of eligible and non-eligible individuals would lead to a bias in the estimates of the effect. In this paper, this issue is solved by applying a regression discontinuity design. I exploit the fact that entitlement to unemployment insurance is conditional on income previous calendar years exceeding certain levels. A major advantage of this source of identification is that at the point in time when the outcome is realized, the factor that generates entitlement (income *previous calendar year*) can no longer be manipulated. Hence, given the

[1] Let us denote the size of the total labor force as L, the number of unemployed as U, and the number of employed as E. Hence, $L = U + E$. The rate of entry into unemployment is λ, and the rate of exit from unemployment is θ. Steady state unemployment implies, in case of a constant labor force, that the number of individuals exiting unemployment is equal to the number of individuals entering unemployment, i.e that $\lambda E = \theta U$. Solving this equation for $u = \frac{U}{L}$ results in the following expression for the steady state unemployment rate: $u = \frac{\lambda}{\lambda+\theta}$. The steady state unemployment rate will increase if rate of entry into unemployment increases.

institutional settings in Norway, a sharp regression discontinuity design seems suitable (Angrist and Pischke, 2009).

What do I mean by a causal relationship between UI eligibility and unemployment entry? Let us first think in terms of a policy change. An economy introduces or expands the coverage of UI. First, this will probably change the incentives and scope of action for both employers and employees. This will be discussed more thoroughly in the next section.

Secondly, the fact that the individuals change their behavior might also affect the general equilibrium in the economy. For example, wage pressure may increase, which in turn yields lower job creation and higher job destruction. Higher job destruction rates imply even higher risk of unemployment entry (Mortensen and Pissarides, 1994 ; Hagedorn, Karahan, Manovskii, and Mitman, 2013).

Whether one is able to separate the two types of effects from each other, will depend on the source of identification applied. If identification is based on a time discontinuity around entry or change of a UI program, separating the two is probably challenging. In the present analysis, identification is based on a discontinuity in income. This implies that some individuals are eligible for UI *at the expense of the UI eligibility of others*. This makes it possible to isolate the first order effects from potential equilibrium effects.

If crossing the income threshold changes the probability of UI eligibility discontinuously, but leads to no, or only small continuous changes in the distribution of other factors that affect the risk of unemployment entry, this design is suitable for the identification of the causal effect of UI eligibility on the risk of unemployment entry at the threshold.

The results of the analysis suggest that eligibility strongly affect the probability of entering registered unemployment. Approximately half of this effect seems to be driven by an increase in the tendency to separate from employment. The other half is driven by an increased propensity to register as unemployed given that separation from employment has occurred.

The outline of the rest of the chapter is as follows: Section 2 provides a presentation of related literature. It starts by discussing potential mechanisms that could explain an effect of UI eligibility on unemployment entry and continues with an overview of the existing empirical literature on this topic. Section 3 describes the relevant features of the Norwegian UI system.

Section 4 describes the data applied, discusses the measures of unemployment entry and presents descriptive statistics. Section 5 presents the econometric identification strategy, section 5 presents the main results and sensitivity analyses, and section 7 concludes.

2. Related literature

Several mechanisms can potentially be involved in a causal effect of UI on unemployment entry at the microeconomic level. If existing job matches are hit by idiosyncratic shocks to productivity or product demand, UI generosity will affect the probability that a job match is terminated through the outside options of the employee. If wages can be renegotiated, increased UI generosity will decrease the bargaining space, and in case of a negative shock to productivity, the employee will be less willing to lower his wages to insure that the job match is still beneficial for both parties. Thus a person eligible for UI is more likely to separate from an existing job match if hit by a negative productivity shock, than a person not entitled to UI (Pissarides, 2000). Transitions out of the labor market as a response to a positive shock to the value of non-market activity (home production, leisure etc.) , in line with (Flinn & Heckman, 1982), might also occur more often among well insured person. Job search is hard to observe and document. Hence, seemingly unemployed individuals who receive UI benefits might be spending a considerable amount of time in the non-market state. If this is the case, entitlement to more generous UI entitlements might be seen as a shift to the right in the distribution of the instantaneous value of non-market activity.

Another possible mechanism results from the fact that UI eligibility will affect the incentives to exert on-the-job search. A note of dismissal is normally given some time before the actual destruction of the job match takes place. We could therefore also expect to see an effect of UI generosity on unemployment entry simply from the fact that individuals with no or less generous UI entitlements will search more intensively for a new job and/or reduce their reservation wage during their time of notice. If this is the case, displaced workers will be more likely to have a new job at hand before the old employment relationship ends. Gutierrez (2012) shows that *any perceived risk* of a layoff will make on-the-job search efforts sensitive to UI generosity. If workers apprehend some risk of job loss, they will engage in less on-the-job search if they are entitled to (more generous) UI benefits.

Given strong employment protection, a UI system can also be a way of "buying off" less productive workers. The Norwegian Working Environment Act (Arbeidsmiljøloven) states that "Employees may not be dismissed unless this is objectively justified on the basis of circumstances related to the undertaking, the employer or the employee" (*The Working Environment Act (Official english translation of Arbeidsmiljøloven) §§15-1 - 15-17*, 2005). A severe breach of the written contract of employment may thus be a legitimate reasons for a layoff, but these actions might be hard to document, and the limits between acceptable and non-acceptable breaches may always be questioned. Laying of an unproductive worker might thus be less hazardous if this person is entitled to (generous) UI benefits, as the risk of legal conflict is smaller, and the firm might be spared from a reputation as an uncooperative employer (Rebollo-Sanz, 2012).

The question of whether or not these behavioral responses to UI eligibility represent social costs is still unanswered. Since job search effort and the reasons for a job loss is often unobservable, unemployment insurance creates a wedge between private and social marginal costs of leisure. This is the source of moral hazard, which leads eligible workers to enter unemployment too often from a social perspective. On the other hand, if credit market failures exist, the responses might also represent liquidity effects: Many individuals will not be able to finance a job search period with savings or loans. UI eligibility provides them with the ability to quit a less productive job to search for better one. In some cases this might actually be socially beneficial (Chetty, 2008). The present study does not attempt to disentangle between moral hazard- and liquidity effect of unemployment insurance.

A number of existing papers study the way the incidence of unemployment is affected by the potential benefit duration (Lalive, Van Ours, and Zweimuller, 2011 ; Fitzenberger and Wilke, 2009 ; Dlugosz, Stephan, and Wilke, 2013 ; Winter-Ebmer, 2003 ; Tuit and van Ours (2010) ; Hägglund, 2009) , as well as the benefit level (Topel, 1983 ; Light and Omori, 2004). In this section, I will describe the contributions that focus on the role of eligibility.

Jurajda (2002) studies American retrospective survey data from 1974 to 1979. Various programs create variation in eligibility, replacement rate and potential benefit duration. The author models employment spells, and finds no effect of replacement rate and PBD, but a positive effect of eligibility on the probability of layoff. Quits are not affected.

In Canada, eligibility to UI is conditional on the job tenure exceeding a certain number of weeks. Prior to 1990, this limit varied between 10 and 14 weeks, depending on the region of residence. Christofides and McKenna (1995) use longitudinal survey data from 1988 and 1989, and estimate a hazard rate out of employment. Their estimate indicates that the hazard increases strongly the week that eligibility is obtained.

In 1990, this Canadian job duration requirement was set to 14 weeks over the entire country. Green and Riddell (1997) use survey data and study the regions that changed from 10 to 14 weeks. They find that the concentration of job durations is moved according to the new job duration requirement. When estimating a competing risk model, where a job spell could end in either a quit or a layoff, they find no effects on quits, but significant effects on layoffs. This is despite the fact that quitters faced no sanctions in the UI system during their observation period. Green and Sargent (1998) find that the tailoring of job lengths to meet requirements for UI eligibility is mostly a phenomenon in the seasonal sector.

Rebollo-Sanz (2012) compares the hazard rates out of employment in Spain for workers who differ in their job duration requirements due to earlier employment spells. Some become eligible to UI after 6 months, while others become eligible after 12 months. Both groups have exit spikes around both 6 and 12 months, but the spike at 12 months is much higher for the individuals that needed to stay employed for 12 months to become eligible. The effect is stronger for temporary contracts than for permanent contracts and stronger for women than for men. She finds no effect on the quit hazard.

Hägglund (2009) studies the effects on employment duration of changes in the employment duration requirements for UI that took place in Sweden during the 1990s. He bases his analysis on data from the unemployment register and defines employment duration as the time between two succeeding unemployment spells, and finds that the 5 week prolonging of the employment requirements in 1997 results in an approximate 2.3 weeks extension in the expected employment duration.

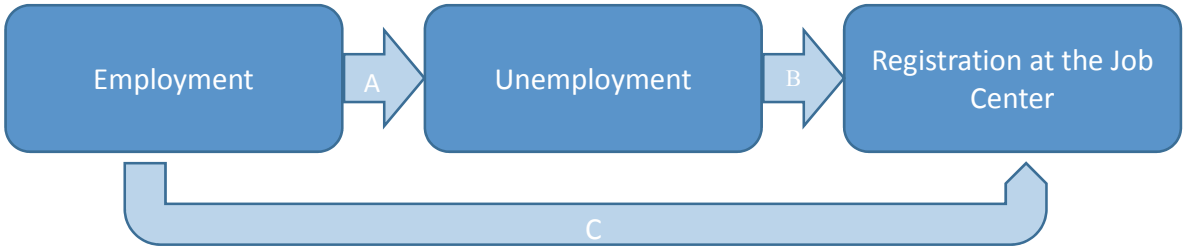
While Jurajda (2002) and the Canadian studies are based on survey data, Rebollo-Sanz (2012) and Hägglund (2009) and the present paper use register data. In many cases, register data might be superior to survey data. First, it allows a high degree of coverage, and thus, statistical power, at low cost. As presence in administrative registers is in general not a subject of choice, selection

bias into the data set will also be less of a problem when register data is used. Due to memory loss and desirability bias on sensitive topics, like e.g. unemployment and welfare dependence, survey data might also suffer from systematic measurement errors, to a greater extent than register data (Krumpal, 2013). Bound, Brown, and Mathiowetz (2001) review studies that by means of re-interviews, and by comparing retrospective interviews to contemporaneous interviews, investigates the degree of measurement errors of labor market states in survey data. A substantial degree of misclassifications is found.

Even if register data solve some problems, they create others. Since they are not collected for the purpose of research, measuring the variables of interest might be challenging. Operationalization of the term unemployment, usually defined as being without, and searching for a job, by means of register data is not straightforward, especially among non-eligible individuals. Rebollo-Sanz (2012) Hägglund (2009) base their definitions of unemployment entry on entry into social security records. However, UI eligibility will probably affect entry into social security records or the unemployment entry in two ways. First, it is likely to increase the probability of actually becoming unemployed (illustrated by arrow A in Figure 1). Secondly, UI eligibility also increase the incentives to contact the Job Centre *given that unemployment has occurred* strongly (illustrated by arrow B in Figure 1). The latter is a reclassification effect, most likely to be positive (Card et al., 2007). The effect on entry into the unemployment registers (illustrated by arrow C in Figure 1) will be an increasing function of effect A and B, and will be positive, even if effect A is 0. The studies based on survey data might be more suited to isolate effect A, than the existing studies based on register data.

The present paper contributes to the register data based research on the effect of UI eligibility on unemployment entry by introducing an alternative definition of unemployment entry. This definition might be more suited to identify effect A, than measures based on the unemployment registers. Based on rich data on employment, education, and age- and health related benefits I attempt to measure unemployment in a way that reduces systematic measurement errors among non-eligible individuals. I discuss this alternative definition more thoroughly in section 4.

Figure 1: Illustration of the processes involved in unemployment entry and unemployment registration.



Note: UI eligibility is likely to increase both the transition rate from employment to unemployment (Effect A), and entry into the unemployment register given that unemployment has taken place (Effect B). The effect of UI eligibility on the transition rate from employment to the unemployment register (Effect C) will be an increasing function of both effect A and effect B.

Another contribution of the present paper is that my source of identification allows me to separate the effect of UI eligibility on the *incidence* of unemployment, from the effect of UI eligibility on the *timing* of unemployment. The papers that study the role of requirements for eligibility on unemployment have one feature in common: The requirements for UI eligibility analysed are defined in terms of job durations. The fact that layoffs are timed according to the requirements for UI eligibility does not necessarily indicate that they would not have happened if not for the UI eligibility. It is likely, at least partly, to be an effect on the *timing* of a layoff, more than an effect on the *occurrence* of a layoff. A layoff can be postponed a few weeks or days. This would probably not induce severe costs to the company, but the financial gains for the employee might be significant. The gains for the employer, from a small adjustment of timing, will probably include smaller risk of a legal conflict with the former employee, and a reputation as a cooperative establishment. The source of variation in eligibility applied in the present chapter differs from the existing contributions in that the variable determining eligibility is considerably more difficult to manipulate. If the question of a destruction of a job match comes up during calendar year t, nothing can be done to affect eligibility, which is determined by income from year $t-1$. The parties could postpone the layoff until the turn of the year, to obtain eligibility based on income from year t, but this is in general more costly than to wait until the requirement of e.g. 14 weeks of job duration is fulfilled (as in the Canadian case). Limitations to the sample are also made in the sensitivity analysis, to ensure further that the effects found are not related to the timing of an unavoidable job separation.

3. Institutional settings

To be entitled to unemployment insurance in Norway, the person has to fulfil the following requirements:

- Have work hours unintentionally reduced by at least 50 %.
- Be searching for a job
- Register as unemployed at the Job Centre, and confirm every second week to be still unemployed, and searching for a job
- Wage income previous calendar year has to exceed 1.5 Basic Amounts (BA) *or* wage income over the three previous calendar years has to add up to more than 3 BA[2].

One Basic amount corresponds in 2012 to approximately 80 000 NOK, or 10 000 €. The amount is regulated annually, as a function of Norwegian price- and wage growth.

The last point on this list of requirements will be exploited in this chapter.

If the job applicant is qualified for unemployment benefits, the annual benefit amount will be 62.4 % of the Income Basis (IB), which is either the labor income for the previous calendar year, or the average income over the past three calendar years, depending on what is most beneficial for the unemployed person (*The National Insurance Act §§4-1 - 4-28*, 1997). IB cannot exceed 6 BA. Letting $\overline{w}$ express average income over the past three years, and w_{t-1} income previous calendar year, both measured in basic amounts, and $\boldsymbol{I}(\cdot)$ be an indicator function taking the value 1 if the condition in the parenthesis is satisfied and zero otherwise, the annual amount of UI benefits (UIB) is given by the following formula:

$$UIB = 0.624 \times \min\left(6, \max\left(\overline{w} \times \boldsymbol{I}(\overline{w} \geq 1\ BA),\ w_{t-1} \times \boldsymbol{I}(w_{t-1} \geq 1.5\ BA)\right)\right)$$

The monthly amount received in unemployment benefits, for a person just satisfying the income requirements, is 6240 NOK, or approximately 780 € before tax.[3] For the income group studied in this analysis, the potential benefit duration is 12 months.

[2] Special rules apply for repeat users. Earlier unemployment insurance spells can affect eligibility in two ways: If a person exited a UI spell less than 12 months ago, he can automatically reenter his old spell in case of unemployment and continue benefit exhaustion at the point he left, and income earned three years prior to the beginning of a fresh UI spell cannot be included in any income basis again. To obtain precise information on the relevant measures of income for repeat users, the entire history of employment- and unemployment spells from 1989 is considered. The details regarding this procedure can be found in Appendix B of the first chapter in this dissertation: "Analyses of an Unemployment Insurance reform".

Welfare assistance (WA) is payments intended to cover life expenses when other income is not sufficient. These benefits are means tested, and entitlement is not conditional on attachment to the labor market (*The Social Services Act,* §18 2009). Since the effect identified by a regression discontinuity design is local and in this case located at a very low income level, some of the persons in our sample are likely to be candidates for these benefits if they were to become unemployed. If WA can easily replace UI, the size of the estimates will be reduced, not because UI in general does not affect job separation, but because at our local point of identification, treatment is irrelevant in any economic meaningful way.

However, any other source of support, for example the income of a spouse, or savings, disqualifies a person from WA. Additionally, WA is probably associated with a higher degree of stigma than other benefits. Hence, receiving UI instead of receiving WA probably makes a difference. The average monthly payments of WA during spells of registered unemployment, is 117 € for UI entitled persons, and 251 € for persons without entitlement. There is, as one should expect, a clear difference in WA payments, but still it is small compared to the monthly UI payments granted by UI entitlement (780 €). Hence, treatment does in fact seem relevant, also among the low-income sample studied in this chapter.

4. Data and definitions

The analysis in this chapter builds on administrative register data on income, social welfare, unemployment spells, employment spells and demographics.

The starting point for the analysis is every individual that in at least one of the years between 2003 and 2008 had income from work between 1 and 2 BA. I add information on employment status and construct monthly observations for all of these individuals from 2004 to 2009.[4] A monthly observation is excluded if the individual was not employed in the previous month, as this person is not at risk of becoming unemployed. Observations are also excluded if the

[3] According to the tax calculator at the web pages of the Inland Revenue Service (skatteetaten.no), the annual tax on an income on this level is 63 € per month. The tax will be even less if the person lives with children, or pays mortgage interests.

[4] A person is considered as employed in month t if he is registered in the employment register at the end of that month, and at the same time is not registered as under education, or on any health- or age related benefits, or on parental leave.

individual had a wage income below 1 BA or above 2 BA previous calendar year, as only income between 1 BA and 2 BA is considered close enough to the threshold to be of interest[5].

A person is also excluded from the main sample if the sum of income over the past three years exceeds 3 BA, as these are eligible to UI through the three year criterion (see section 3), and thus are not affected by the threshold at 1.5 BA.

Some sample limitations are made to improve the accuracy and relevance of my broad activity-based definition of unemployment entry: Women who gave birth during the present calendar year are excluded. Individuals within the income span studied here are in general not entitled to parental benefits that can be tracked in our data, and to avoid defining their birth-related withdrawal from the labor force as unemployment entry, I choose not to include this group in my analysis.

I also limit the analysis to individuals belonging to age groups that are likely to be in the core of the labor force: those aged between 25 and 55. Older individuals are more likely than others are, to leave employment for non-activity, and thus, erroneously be defined as unemployed in my broad activity based definitions of unemployment.

The resulting sample consists of 1 126 416 monthly employment observations and 138 119 individuals. Descriptive statistics are presented in Table 1. The sample contains a larger share of women, immigrants, low educated and youths than the Norwegian employed population at large. For example, when studying the total population of employees in 2010, the share of women were 49 % against 16 % in my sample. Average age was 41 years while in my sample it is 35 years. The share of western and non-western immigrants constitute 7.3 and 9.3 % respectively against 27 % and 13 % in my sample, and the share with only compulsory education 19 % against 24 % in my sample. These differences, caused by the intention to include only individuals around the income threshold, indicate that my sample is not representative for the general Norwegian employed population. One can say that external validity is sacrificed for the ability to identify the causal effect in the sample included; i.e. for the sake of internal validity.

It seems reasonable to assume that my sample is less attached to the labor market than the general working population. The same might be true, also for the other studies referred to above. Jurajda (2002) studies workers who used to be unemployed. Rebollo-Sanz (2012) studies individuals that were in contact with the social security system at least once between 2005 and 2008. The sample studied by Green and Riddell (1997) is less educated than the average Canadian population, and as Hägglund(2009) study the duration of employment spells succeeding an unemployment spell, his sample is also likely to be less attached to the labor market, compared to the average Swedish employee. This indicates that though the sample in my study differs quite a lot from the Norwegian working population, it shares some features with samples used in the related literature.

I apply two definitions of entry into unemployment. The first one is based on the unemployment register. An individual that was in the employment register at the end of previous month, and register as full time unemployed during the present month is defined as an unemployment entrant.

Every job searcher is allowed to register as unemployed, irrespective of eligibility to UI. Registering as unemployed does not require personal attendance at the Job Centre; it can be done quickly and easily online. Registration entitles the individual to job search assistance, and if necessary: participation in active labor market measures (*The Labor- and Welfare Administration act §§14-15*, 2006). Thus, unemployment registration can be accomplished with negligible time- and stigma costs, and irrespective of UI eligibility, the incentives to do so are present. Still, non-eligible job seekers have weaker incentives to register as unemployed than eligible job seekers have, since UI is not available. Since the costs related to registration are not completely absent, the differences in the benefits from registration might lead to a higher tendency to enter the unemployment register among UI eligible job searchers compared to non-eligible job searchers. This implies that the number of non-eligible unemployment entrants might be under estimated when unemployment is defined in terms of presence in the unemployment registers, i.e. a measurement error in the dependent variable correlated with the independent variable. This will bias the estimated effect of eligibility on unemployment entry upwards. One might therefore claim that the use of this definition is quite uninteresting. Presence in administrative unemployment registers is however quite frequently applied as a measure of unemployment in

the economics literature. Since it is also an interesting basis for comparison, I chose to apply it, in addition to the second definition.

The second definition is applied to try to answer the question of whether or not eligibility to UI actually make separation from employment more likely, or if it just leads to a reclassification of non-employment time based on registration activity (Card et al., 2007). According to this definition, which does not apply information from the unemployment registers at all, a person becomes unemployed if he leaves the employment register without entering any education, health- or age related benefits or parental leave. This definition thus includes persons seemingly without any activity or individual income, except potentially for job search and unemployment insurance. With this definition, I include everyone that leaves the labor force voluntarily, without any activity or benefit that is traceable from the register, and hence, it is probably too wide. However, given the limitations to the sample that is made with respect to age and maternity, the high Norwegian labor market participation rate, and the rich data applied to narrow the definition to include only those that seemingly have no other income or activity, it may still be fruitful. If UI eligibility is important in determining unemployment entry (and not only registration propensity), the use of this broader definition should reflect that. We see from Table 1 that the monthly risk of entering unemployment according to this definition is 3.5 %, more than three times as high as the risk of registering at a Job Centre.

Figure 2 displays histograms over yearly income from employment measured in Basic Amounts. Figure 2 A displays the distribution of income from employment in 2003-2008, capped at 20 BA. The observation unit is person-years. It shows that in the observation period on which this analysis is based, the vast majority of Norwegian employees have income from employment that strongly exceeds 1.5 BA per year. The distribution reaches its maximum around 5 BA, which is quite far away from the UI eligibility threshold. [6] Since the treatment effect identified by applying a regression discontinuity design is local and in this case, located at a very low level, the external validity of the findings might be questioned. The findings in this chapter are not necessarily representative for the average Norwegian worker.

[6] We see that the density is very high at the smallest levels of income. When studying the histogram of total income, including also transfers from the government and capital income (this figure is not included in this paper), this pattern is not present. This indicates that the relatively large share of individuals with very low income from employment tend to have other additional income sources.

Table 1: Descriptive Statistics of the mean sample

Number of monthly observations	1 126 416
Number of individuals	138 119
Selected means over monthly observations	
Definitions of unemployment:	
Monthly risk of register based unemployment	1.0 %
Monthly risk of activity based unemployment	3.5 %
Share of women	60.6 %
Age	35.5 years
Share of non-western immigrants	26.9 %
Share of western immigrants	12.8 %
Share with only compulsory schooling	24.0 %
Average months in the unemployment register the last 5 years	5.4
Average months in the unemployment register the last 5 years	2.5
Average tenure in employment (Measured as the number of years the individual has earned above 2 BA)	5.4

Note: Monthly observations of employed individuals, with UI income basis between 1 and 2 BA, from 2004 to 2009

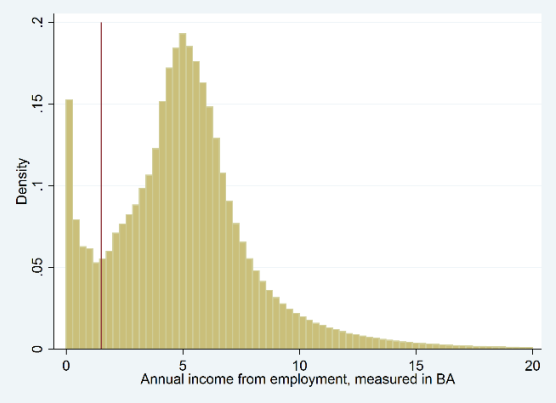
Figure 2 B displays the distribution of incomes previous calendar year, given that the sum of income over the past three years is below 3 BA. In this group, UI eligibility is fully determined by whether they earned above 1.5 BA previous calendar year. The downhill slope of the distribution is created mechanically by the exclusion of individuals that earned above 3 BA in sum over the three past years: The more a person earned in the previous year, the more likely he is to be excluded from the sample. Figure 2 C displays the distribution of income in year t-1, given that sum of income over tear t-2 and year t-3 is below 1 BA. In this sample, having income close to 2 BA in the previous calendar year does not make it more likely to be excluded from the sample than if earning close to 1 BA, and as we see, we avoid the downhill slope in the distribution. The main analysis in this sample is conducted on the sample displayed in Figure 2 B, but the results when using the sample displayed in Figure 2 C are also reported.

Figure 3 displays the monthly probabilities of unemployment, for the two definitions of the term, by income, based on the analysis sample described in Table 1 and in Figure 2 B. We see a clear sign of a jump in the probability of registering as unemployed around the threshold value, when using registration as definition of unemployment. This pattern is not as visible when applying the wider definition of unemployment. The declining pattern in Figure 3 B and on the right side of the threshold in Figure 3 A probably reflects the fact that the more an individual earned in the previous calendar year, the more attached he is likely to be to employment. The lack of a falling trend on the left side of the threshold in Figure 3 A might be caused by measurement error, i.e.

that some individuals close to the threshold are eligible for UI, despite that my data suggests otherwise.

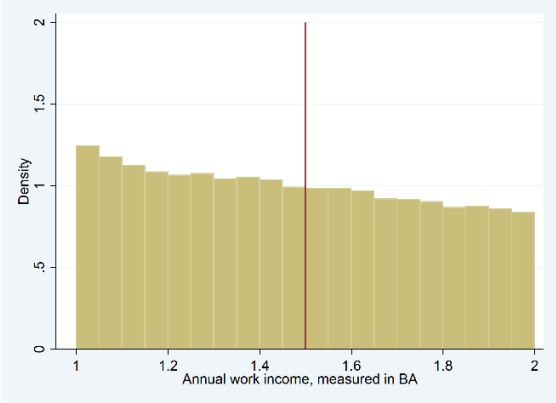
Figure 2: Income distributions

A) Distribution of yearly income from employment 2003-2008



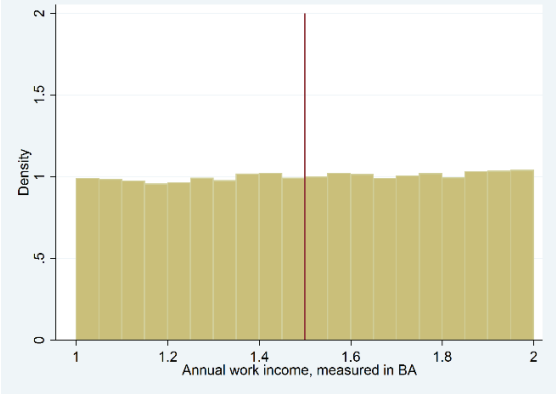
Note: Source: The Norwegian income register. Observation unit is person-years. Observations are included if $0 < income < 20\ BA$, and if age is between 25 and 59 years. N=11 515 086.

B) Distribution of income previous calendar year in the main sample in the analysis



Note: Observation unit is person-months. Observations are included if income year t-1 >= 1 and income year t-1<= 2, and the sum of income year t-1, t-2 and t-3 is below 3 BA. N=1 126 416.

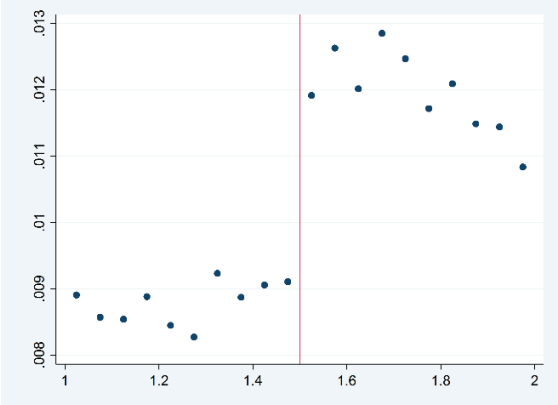
C) Distribution of income previous calendar year in sample used for robustness analysis



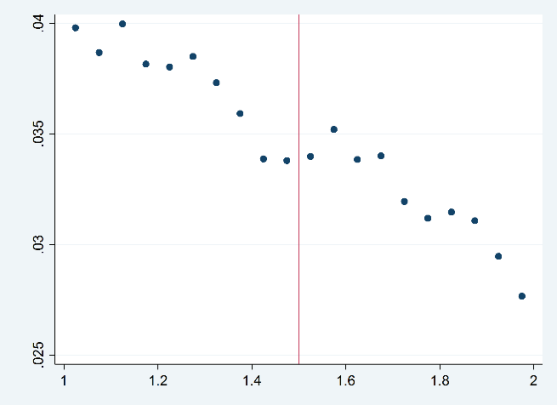
Note: Observation unit is person-months. Observations are included if income year t-1 >= 1 and income year t-1<= 2, and the sum of income year t-1 and t-2 is below 1 BA. N=896 553.

Figure 3: Monthly risk of entering register based unemployment, and activity based definition, by income previous calendar year.

A) Register based unemployment



B) Activity based definition of unemployment



Note: The average probabilities of unemployment entry, grouped by income previous calendar year, at intervals of 0.05 BA

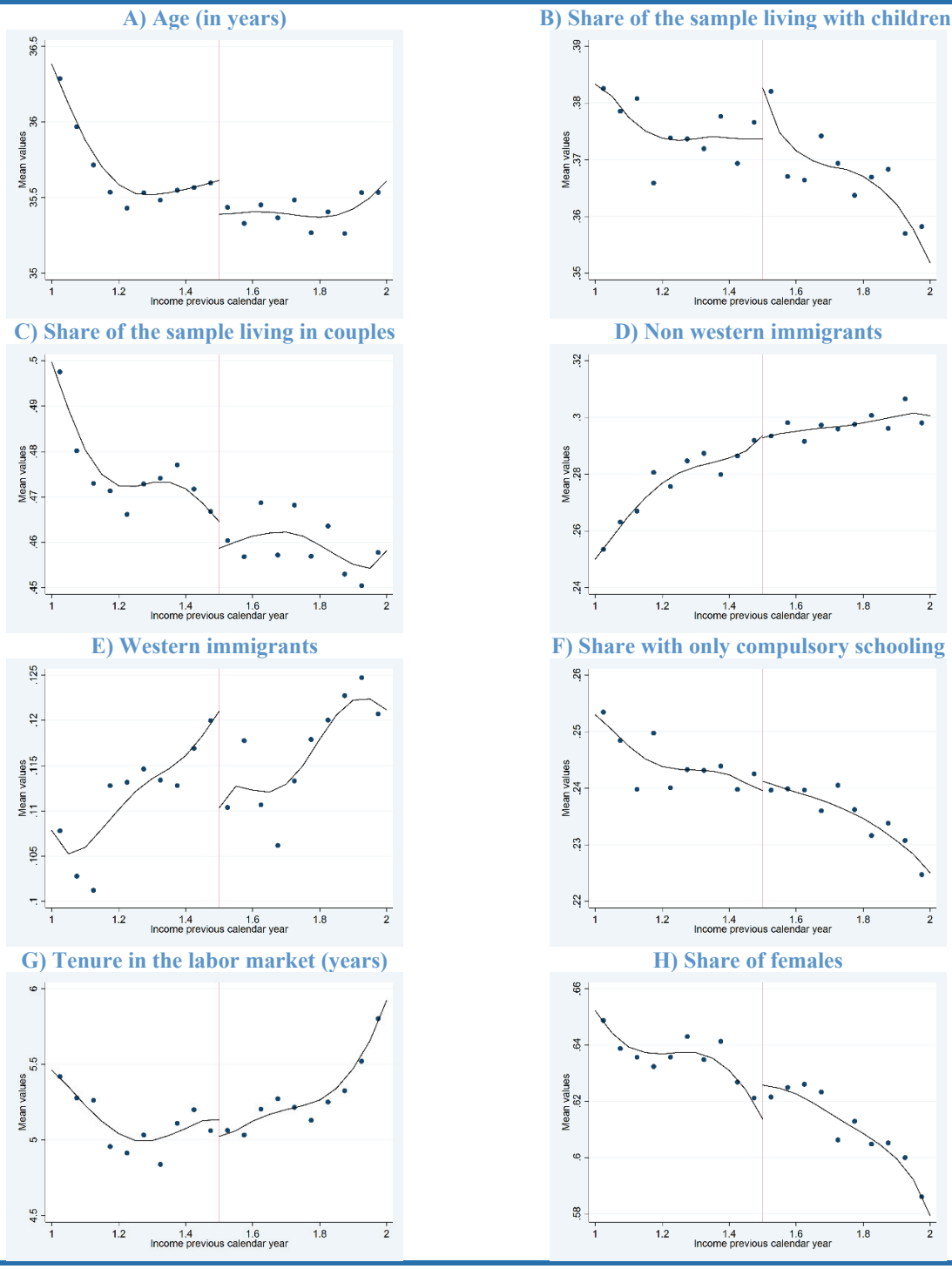
5. Methods and models

I intend to identify how the probability of unemployment entry changes, as a person becomes entitled to UI. I frame the research question in terms of a Rubin causal model with potential outcomes.

$Y_i(0)$ and $Y_i(1)$ indicate the outcomes for observation i in the case of non-eligibility and eligibility, respectively. They take the value 1 if job separation occurs, and 0 if not.

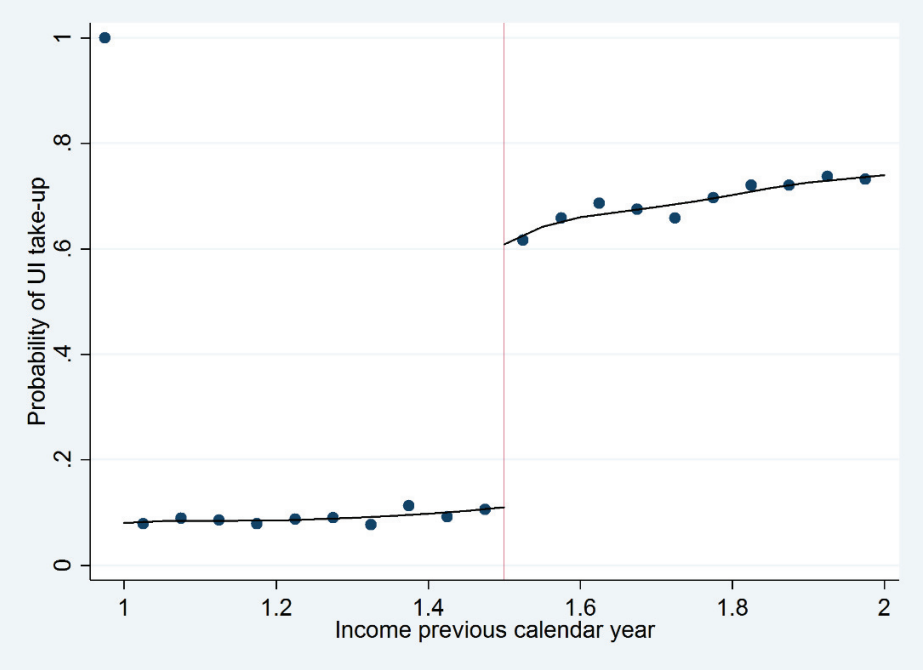
Let W_i denote the UI eligibility status for observation i. The value 1 indicates eligibility, and 0 non-eligibility. X_i is the running variable (income previous calendar year), and c is the threshold value. The treatment effect of entitlement on unemployment entry for observation i is $Y_i(1) - Y_i(0)$. If both $Y_i(0)$ and $Y_i(1)$ are 0, observation i would never experience entry into unemployment, irrespective of eligibility. If they both are 1, entry into unemployment is inevitable. In both cases, the treatment effect is 0. The cases where treatment effect is different from zero, is where $Y_i(1)$ is 1 and $Y_i(0)$ is zero, or the less likely case where entitlement works the opposite way: $Y_i(0) = 1$ and $Y_i(1) = 0$.

Figure 4: Mean values of demographic- and background variables, by income previous calendar year.



Note: Plots of average values of potentially confounding variables grouped by income previous calendar year, at intervals of 0.05 BA, combined with local linear estimates of the non-parametric regression function, estimated separately on each side of the threshold. The regression function is evaluated at 11 points on each side of the threshold, with bandwidth of 0.2 BA.

Figure 5: UI take-up among individuals in the unemployment register, by income previous calendar year measured in BA



Note: Estimates of the probabilities of UI uptake among 14 034 spells of registered unemployment between 2004 and 2010 lasting 3 months or more. Only spells of individuals with average income over the past three calendar years below 1 BA is included in the sample. Uptake is measured as uptake at least one month during the 3 first months of the registered unemployment spell. Income previous calendar year (benefit basis) is measured in basic amounts. Plots of average values of UI uptake grouped by income previous calendar year at intervals of 0.05 BA are combined with local linear estimates of the non-parametric regression function, estimated separately on each side of the threshold. The regression function is estimatet at 11 points on each side of the threshold, with a bandwidth of 0.2 BA.

We never observe both $Y_i(0)$ and $Y_i(1)$. We only observe the realized outcome Y_i, a function of the potential outcomes and treatment status. If we denote treatment status, in this case eligibility, this function would be

$$Y_i = Y_i(0) + W_i(Y_i(1) - Y_i(0)) \quad (1)$$

Following Hahn, Todd and Van der Klaauw (2001), we can estimate the average treatment effect of eligibility at the income threshold by studying observations close to the threshold. Let us denote the income threshold as c, and ϵ as a small number. The difference in the probability of unemployment entry for individuals with income slightly above- and below the threshold is

$$E(Y_i|X_i = c + \epsilon) - E(Y_i|X_i = c - \epsilon) \quad (2)$$

By inserting equation 1, this expression can be rewritten to:

$$E\left[Y_i(0) - W_i\left(Y_i(1) - Y_i(0)\right)\middle|X_i = c + \epsilon\right] - E\left[Y_i(0) - W_i\left(Y_i(1) - Y_i(0)\right)\middle|X_i = c - \epsilon\right] \quad (3)$$

If the following assumption is done:

A1 $E[Y_i(0)|X_i = x]$ is continuous at $x = c$,

which implies that $\lim_{(x \to c^+)} E(Y_i(0)|X_i = x) = \lim_{(x \to c^-)} E(Y_i(0)|X_i = x)$

This expression can further be simplified to:

$$E\left[W_i\left(Y_i(1) - Y_i(0)\right)\middle|X_i = c + \epsilon\right] - E\left[W_i\left(Y_i(1) - Y_i(0)\right)\middle|X_i = c - \epsilon\right] \quad (4)$$

as ϵ goes to 0.

Assumption 1 (A1) requires that if eligibility did not change around the threshold, we would not see a discontinuity in the probability of unemployment entry at the threshold. This implies that nothing else than obtaining UI eligibility should affect the probability of unemployment entry discontinuously at the threshold. The validity of this assumption can be discussed by studying the distribution of other variables that possibly could affect entry into unemployment, around the threshold value. If some of these distributions changes discontinuously at the threshold, one might suspect that the jump (or lack thereof) in the probability of unemployment entry is caused by a jump in the distribution of other explanatory variables, and thereby biasing the estimate of the effect. Figure 4 displays average values of variables that might be important in predicting unemployment entry, grouped by income, combined with a local linear estimator of the non-parametric relationship between the confounders and income. Judged by the estimated functions, discontinuities seem to be present at the threshold, at least for some of the variables. This especially counts for the variables age, the rate that lives with children, and share of western immigrants. The question of whether or not the jump is significantly different from zero is, as with my main results, addressed by bootstrapping (see the end of this section). I find a statistically significant jump at the threshold only for the share of western immigrants. Since I test the distribution of 8 variables, the finding of one discontinuity significant at a 5 % level

might be a mere coincidence. As the distribution of non-western immigrants seem to be continuous at the threshold, I continue the analysis without considering confounding variables.

Another violation of A1 is related to monotonic manipulation of the running variable (McCrary, 2008). Workers with the slightest risk of experiencing unemployment next calendar year, have an incentive to ensure entitlement by adjusting their income according to the income requirements. If individuals with a higher underlying risk of unemployment, due to for example abilities or industry, are more likely to act upon these incentives than persons with a low underlying risk of unemployment are, we would observe a jump in the probability of unemployment entry that is caused by a discontinuous change in the distribution of underlying unobservable variables, and not by eligibility. This adjustment of income would manifest itself as an increase in the income distribution just above the threshold. Figure 2 B and C show however that this seems not to be the case. Adjustment of income to ensure eligibility does not seem to be relevant, and therefore, a formal test, e.g. the one suggested by McCrary (2008) is not conducted.

If we impose the assumption of conditional independence between treatment and treatment effect (A2),

A2 $$E\big[W_i\big(Y_i(1)-Y_i(0)\big)\big|X_i=x\pm\epsilon\big]=E[W_i|X_i=x\pm\epsilon]\times E[(Y_i(1)-Y_i(0))|X_i=x\pm\epsilon],$$

Expression 4 can be simplified further to

$$E\big[W_i|X_i=c+\epsilon\big]\times E\big[\big(Y_i(1)-Y_i(0)\big)\big|X_i=c+\epsilon\big] - E\big[W_i|X_i=c+\epsilon\big]\times E\big[\big(Y_i(1)-Y_i(0)\big)\big|X_i=c-\epsilon\big] \tag{5}$$

Imposing a third assumption, of continuity of the treatment effect regarded as a function of income at the threshold:

A3 $E[Y_i(1)-Y_i(0)|X_i=x]$ is continuous at $x=c$,

which means that $\lim_{x\to c^+}E(Y_i(1)-Y_i(0)|X_i=x)=\lim_{x\to c^-}E(Y_i(1)-Y_i(0)|X_i=x)$, we will have the following estimator for the average treatment effect at the threshold:

$$E\big[\big(Y_i(1)-Y_i(0)\big)\big|X_i=c\big]=\frac{y^+-y^-}{w^+-w^-}\,, \tag{6}$$

where

$$
\begin{aligned}
y^{+} &= \lim_{(x \to c^{+})} E(Y_i | X_i = x) \\
y^{-} &= \lim_{x \to c^{-}} E(Y_i | X_i = x) \\
w^{+} &= \lim_{(x \to c^{+})} E(W_i | X_i = x) \\
w^{-} &= \lim_{x \to c^{-}} E(W_i | X_i = x)
\end{aligned}
\tag{7}
$$

A2 and A3 entail no selection based on the treatment effect, into treatment, and into the state of having income above the threshold value, respectively. If eligibility is completely determined by the treatment effect, (in)validity of one implies (in)validity of the other.

A high treatment effect in this case mean that $Y_i(1) - Y_i(0) = 1$. Individuals with high treatment effects are persons who do enter unemployment if eligible, but otherwise not, in other words: that they are more sensitive to economic incentives than others are. A3 would be violated if individuals highly sensitive to the economic incentives ensure eligibility by earning just above the threshold, to a larger extent than others. It is reasonable that individuals inclined to "exploit" the UI system in case of eligibility, also make sure to be in a position to do so. A precise manipulation of the running variable of this kind would imply that the expected treatment effect is higher among individuals just above the threshold, than just below. This would, if we assume that the treatment effect can never be negative, and that A1 still holds, lead to a positive bias in the estimates of the treatment effect.

In contrast to A1, which, as discussed above, excludes manipulation based on $Y(0)$, A2 and A3 treat manipulation based on $Y(1) - Y(0)$. However, since also this type of manipulation is most likely to be monotonic (no one has incentives to make an effort *not* to be eligible), it should lead to a jump in the income distribution on the right side the threshold. The lack of such a jump in the distributions in Figure 2 A and B suggests that A3 might be valid. If income determines eligibility completely, this means that A2 also holds.

We see from Figure 5, that the UI take-up rate among registered unemployed is positive, also among individuals with income below the threshold value. This suggests that eligibility is not completely determined by the observed income used to identify eligibility in this paper. Depending on the source of this measurement error, A2 might be violated even if A3 holds. In

case of unsystematic measurement errors in the income data, or unsystematic imperfections the procedure applied to measure the eligibility among repeat users of unemployment insurance[7], A2 is probably not violated. However, the UI uptake among seemingly non-eligible individuals could be caused by some eligibility generating income not observable in my data that requires additional documentation from the unemployed to be included in the income bases for UI. If individuals with high treatment effects are more eager to document income not present in the administrative income registers, treatment and treatment effect might not be independent at the threshold, conditional on observed income.

The National Insurance Act states clearly which incomes that can possibly be included in the income on which UI eligibility are based. A strong attempt is made in this analysis, to include any eligibility generated income, and exclude all other. It is therefore more likely that the positive UI uptake among seemingly non-eligible individuals is caused by unsystematic errors in my algorithm used to determine UI status among repeat users (who probably are quite strongly represented in the income group studied here), than by unregistered income which can be included in the income basis if documented by the job searcher. In that case, A2 is also likely to hold.

A local linear estimator will be applied to estimate the elements of equation 6. This is an estimator that quite effectively deals with boundary problems in regression discontinuity estimation (Fan, 1992).

The local linear estimator for y^+ is given by $\hat{a}$, where

$$\hat{a}, \hat{b} = argmin_{a,b} \sum_{i=1}^{n} \left(Y_i - a - b(X_i - c)\right)^2 k_i \qquad (8)$$

k_i is the quartic Kernel function: $k_i = \frac{15}{16}\left(1 - \left(\frac{X_i - c}{h}\right)^2\right)^2 \boldsymbol{I}(\frac{\boldsymbol{X_i - c}}{\boldsymbol{h}} \leq 1)$ if $X_i \geq c$, and 0 otherwise. $\boldsymbol{I}(\cdot)$ is an indicator variable taking the value 1 if the condition inside the parenthesis is true, and 0 otherwise. h is the bandwidth: If $|X_i - c\ | > h$ then $k_i = 0$.

[7] See Appendix B, Chapter 1

y^- is estimated equivalently.

$(w^+ - w^-)$ is the change in the probability of entitlement from crossing the threshold. If the sharp design is suitable, the nominator in equation 6 is 1 by definition. If the fuzzy design is appropriate, we need to estimate it. The robustness tests conducted below involve an approximation to a fuzzy design, and this topic will be discussed further later on in the paper.

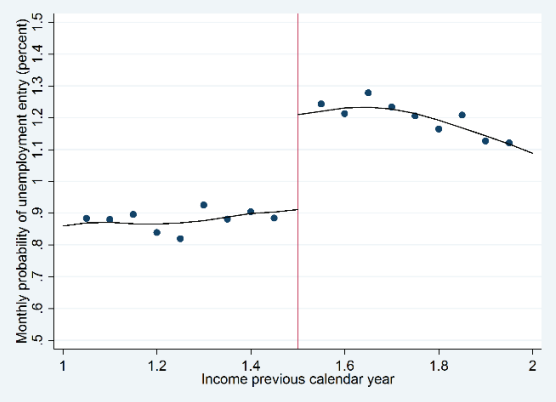
The local linear regression is also evaluated at other points than the threshold show the fit of the estimation to the conditional expectation.

6. Results

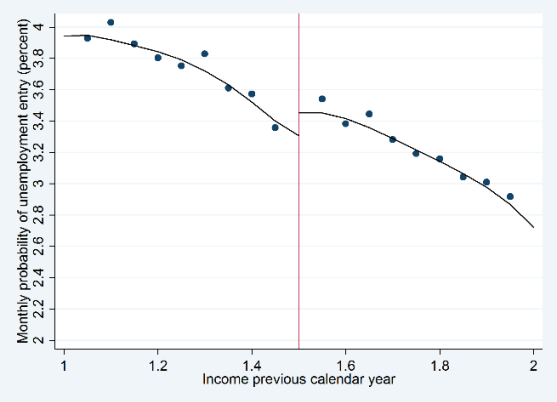
Figure 6 displays the results when a local linear regression is conducted on each side of the threshold separately. The bandwidth applied is 0.2 basic amounts, corresponding to around 2000 €. The results when alternative bandwidths and alternative specifications of the model and the sample are applied will follow. All results are also reported in Table 2.

Figure 6: Local linear regression results of the estimation of the probability of entering unemployment.

A)Estimating the monthly probability of entry into registered unemployment



B) Estimating the monthly probability of entry into activity based unemployment.



Note: Plots of rates of entry into unemployment grouped by income previous calendar year at intervals of 0.05 BA, combined with local linear estimates of the non-parametric regression function, estimated separately on each side of the threshold. The regression function is estimatet at 11 points on each side of the threshold, and a bandwidth of 0.2 BA is applied.

As described above, if we assume that the probability of UI eligibility changes from 0 to 1 when the income threshold is crossed, meaning that a sharp RD design is suitable, the difference

between the intersection points of the two regression lines through the vertical line at the income threshold, can be interpreted as the causal effect of eligibility, on the probability of entering unemployment.

The point estimate of the effect of eligibility on entry into registered unemployment is an increase of 0.30 percentage points per months, from a risk level close to 0.91 %. This corresponds to a relative increase in the probability of entering registered unemployment of 33 %. Eligibility to UI thus seem to affect the probability of entering the unemployment register quite strongly.

The effect of eligibility on entry into the activity-based measure of unemployment is, as expected, weaker both in absolute and in relative terms. The point estimate is an increase slightly below 0.15 percentage points, from a baseline level of 3.35 %, i.e. a relative increase of 4,5 %.

The question of whether or not the results are significantly different from 0 is approached by bootstrapping (See e.g. Efron and Tibshirani, 1986). I conduct 500 repetitions of resampling with replacement on the individual level. Then I run the estimation procedure on each of these samples, and thereby obtain 500 estimates of the treatment effect: $y^+ - y^-$. The 2.5 percentile and the 97.5 percentile in the distribution of estimates are reported as the lower and higher levels respectively, in a 95 % confidence interval of the treatment effect.

I also want to test whether or not the estimated effect is sensitive to the choice of bandwidth. Figure 7 reports the results of the estimated treatment effects for a variety of bandwidths, ranging from 0.5 BA to 0.01 BA[8]. The solid line illustrate the point estimates of the effect, and the dashed lines indicate the limits of the confidence interval.

For bandwidths between 0.1 BA and 0.5 BA, the estimated effect of eligibility on entry into the unemployment registers lies quite stably around 0.30 percentage points. It is significantly different from zero for bandwidths wider than 0.06 BA. This finding confirms Hägglund (2009) and Rebollo-Sanz (2012) in that UI eligibility increases the probability of entering the unemployment registers.

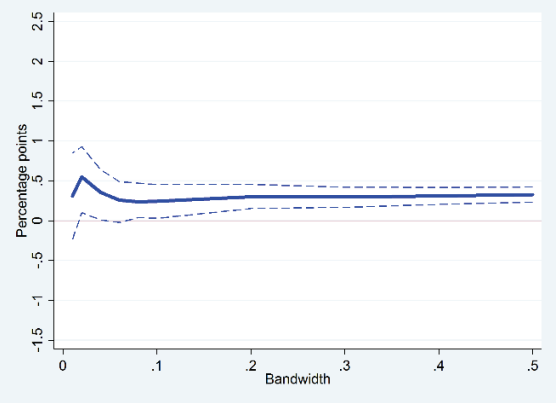
Figure 7 B shows that the effect on entry into the activity-based measure of unemployment is not significantly different from 0 when a bandwidth equal to 0.2 BA is used. It is borderline

[8] As 1 BA is approximately 10 000 €, the smallest bandwidth implies that only observations with a yearly income between 14900 € and 15100 € are given positive weight in the estimation procedure.

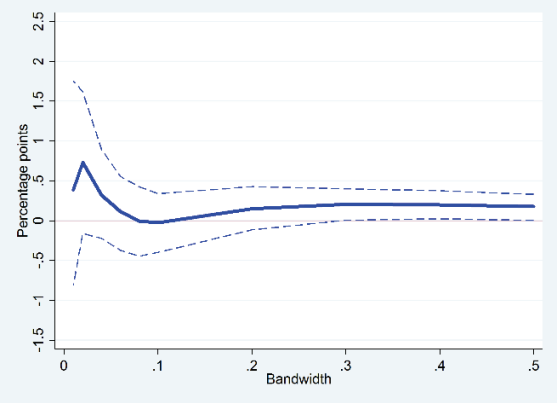
significant when bandwidths from 0.3 to 0.5 BA is used. The point estimates of the effects are also slightly negative for bandwidths close to 0.1 BA. The effect of UI eligibility on entry into unemployment measured this way is therefore not very robust.

Figure 7: Results of the estimation of the effect of eligibility to UI on the main sample. Sharp RD design.

A) The effects of eligibility on the risk of entering registered unemployment, for different choices of bandwidth, when a sharp RD design is applied



B) The effects of eligibility on the risk of entering activity based unemployment, for different choices of bandwidth, when a sharp RD design is applied



Note: A display of the estimated local average treatment effect of UI eligibility on entry into unemployment, for several choices of bandwidths, ranging from 0.01 BA to 0.5 BA. The solid line represent the point estimate, and the dotted lines represent the limits of a 95 % confidence interval.

The lack of statistical significance in the main analysis might indicate that the effects of UI eligibility on unemployment entry found in the survey based literature is driven more by the effect of UI eligibility on the *timing* of unemployment entry, than by the effect on the *incidence* of unemployment. Another interpretation of the results is that the activity-based definition of unemployment is too wide. The high degree of measurement error, although, unsystematic, reduces the statistical power in my analyses, and covers the causal relationship that might be present.

For bandwidths from 0.2 BA and above, the ratio of the point estimates of the effect on register-based and activity-based unemployment seem to be quite roust. Even if statistical significance might be lacking, the point estimates indicate that around half of the effect of eligibility on entry into registered unemployment is driven by a higher tendency to leave employment. The other half is driven by a reclassification of non-employment time: eligibility increases the incentives to register as unemployed given that employment is lost or left.

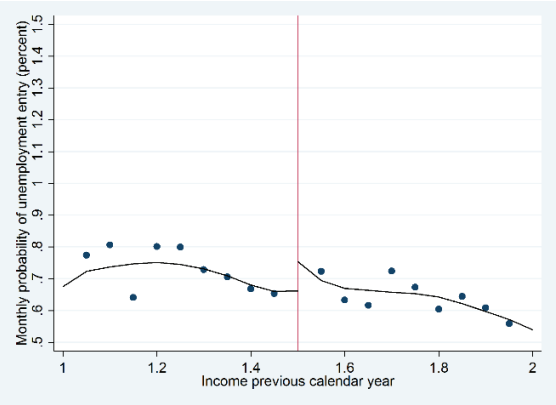
6.1. Sensitivity analysis

To investigate further the robustness of my results, I continue by running the analysis on alternative samples, and by applying an approximation to a fuzzy design.

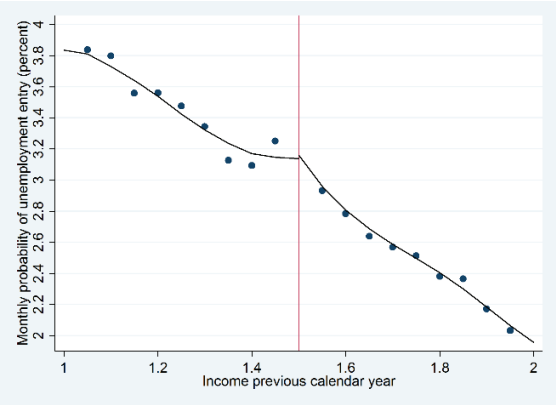
6.1.1. Placebo sample

Figure 8: Local linear regression results of the estimation of the probability of entering unemployment.

A)Estimating the monthly probability of entry into registered unemployment



B) Estimating the monthly probability of entry into activity based unemployment.



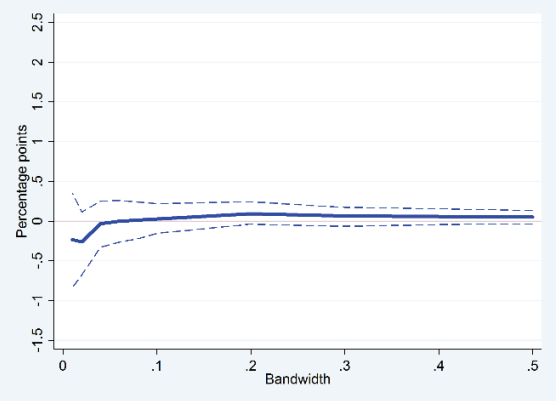
Note: Plots of rates of entry into unemploymnt grouped by income previous calendar year at intervals of 0.05 BA, combined with local linear estimates of the non-parametric regression function, estimated separately on each side of the threshold. The regression function is estimatet at 11 points on each side of the threshold, and a bandwidth of 0.2 BA is applied.

The estimation procedure is conducted on a "placebo" sample: Individuals that earned close to 1.5 BA previous calendar year, but above 4.5 BA in sum over the past three calendar years. These individuals gain nothing in terms of UI eligibility, or UI replacement rate from crossing the income threshold at 1.5 BA. 8 displays the conditional expectations of UI entry by income, combined with the estimated non-parametric polynomial. The effect of crossing the income threshold on entry into registered unemployment is only 0.09 percentage point, which is less than a third of the effect found in the main sample. The estimated effect on entry into activity based unemployment is 0.02 percentage points. Figure 9 displays the sensitivity of the estimated effect with respect to the choice of bandwidth, including confidence intervals. Most of the bandwidth choices lead to effects on entry into registered unemployment quite precisely estimated to 0. This counts for bandwidths from 0.2 BA and upwards when the activity-based definition of unemployment is used (see Figure 9 B)). None of the estimates are significantly different from 0.

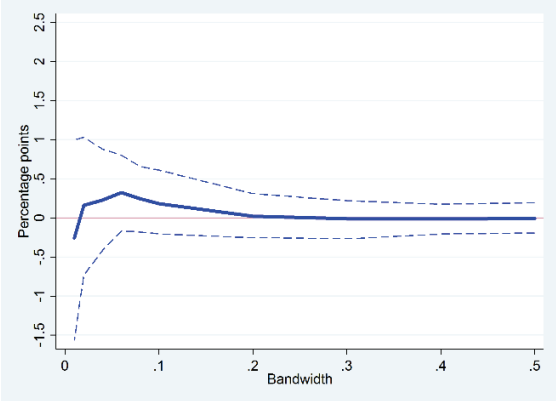
This strengthens the view that the results on the main sample, reported in Figure 6 and Figure 7 are in fact caused by UI eligibility.

Figure 9: Results of the estimation of the effect of eligibility to UI on the placebo sample. Sharp RD design

A) The effects of eligibility on the risk of entering registered unemployment, for different choices of bandwidth, when a sharp RD design is applied



B) The effects of eligibility on the risk of entering activity based unemployment, for different choices of bandwidth, when a sharp RD design is applied

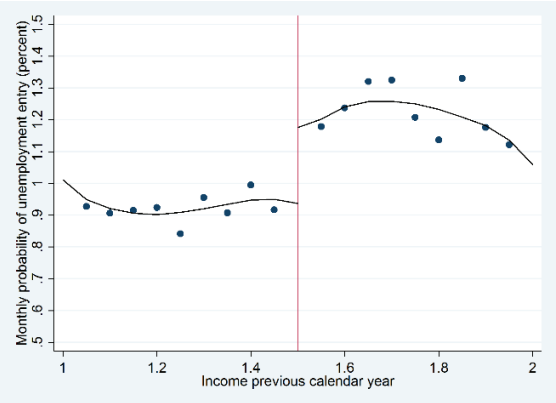


Note: A display of the estimated local average treatment effect of UI eligibility on entry into unemployment, for several choices of bandwidths, ranging from 0.01 BA to 0.5 BA. The solid line represent the point estimate, and the dotted lines represent the limits of a 95 % confidence interval.

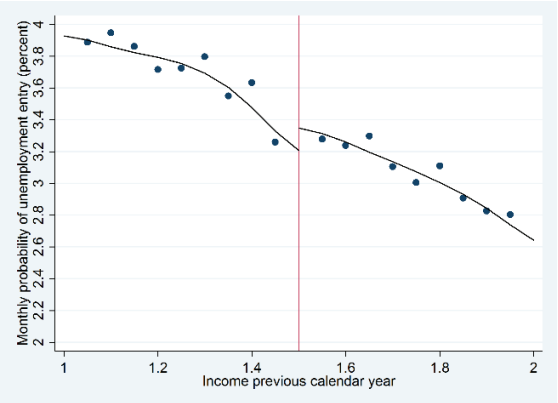
6.1.2. Excluding observations close to the New Year

Figure 10: Local linear regression results of the estimation of the probability of entering unemployment.

A)Estimating the monthly probability of entry into registered unemployment



B) Estimating the monthly probability of entry into activity based unemployment.



Note: Plots of rates of entry into unemploymnt grouped by income previous calendar year at intervals of 0.05 BA, combined with local linear estimates of the non-parametric regression function, estimated separately on each side of the threshold. The regression function is estimatet at 11 points on each side of the threshold, and a bandwidth of 0.2 BA is applied.

Close to the New Year, the association between UI eligibility and risk of entering unemployment is likely to be stronger than the rest of the year. There are two reasons for this:

In case of a layoff, unless severe misconduct is the reason, the dismissal notice is usually given some weeks or months prior to the job separation. According to the working environment act *(§§15-1 - 15-17*), the minimum term of notice is between one and three months, depending on the employment duration. After the age of 50, the term of notice is between four and six months. Workers that receive their note of dismissal at the end of a calendar year can put extra effort into securing entitlement the preceding calendar year, for example by working extra shifts. This is an example of reversed causality: Instead of eligibility affecting the risk of unemployment, the incidence of unemployment affects eligibility. In addition, around the turn of the year, a small adjustment of timing of a lay off might ensure the employee entitlement to UI. Awaiting the job separation until January, or accelerating it to December, might, depending on the wage history of the employee, affect his entitlement status, without imposing significant costs on the employer. In these situations, job separation would happen independently of the UI status of the employee, but the timing of it is likely to influence the entitlement status.

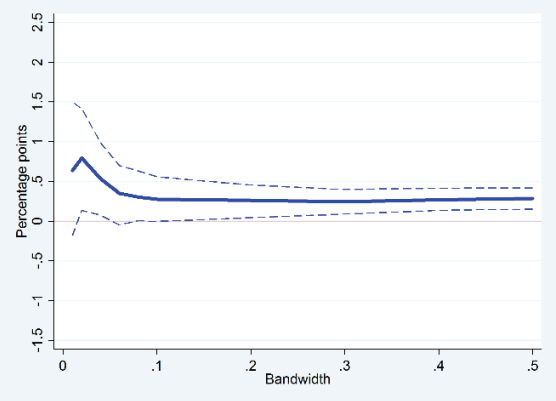
To avoid reversed causality, and to reduce the possibility of our results being driven by timing adjustment even further, I run the analysis on the sample of observations from April to September. The results are reported in Figure 10. The estimated effects are reduced only marginally: From 0.30 to 0.29 percentage points in entry into the unemployment register, and from 0.15 to 0.14 on entry into activity based unemployment.

Figure 11 displays the dependency of the estimated causal effects, and their confidence interval, with respect to bandwidth choices. The main difference from the results on the main sample reported in Figure 7 is that the confidence intervals increase from the exclusion of half of the sample. The point estimates are higher on the half-year sample when using the smallest bandwidths, but lower for the widest bandwidths, both on entry into registered unemployment and when the activity based definition of unemployment is applied. There is thus no consistent pattern showing weaker point estimates when observation months close to New Year are excluded from the sample. This exclusion nevertheless reduce the average estimated effect for bandwidths between 0.1 and 0.5 BA from 0.30 to 0.27 pp when we study entry into registered unemployment, and from 0.14 to 0.12 pp when entry into the activity based definition of

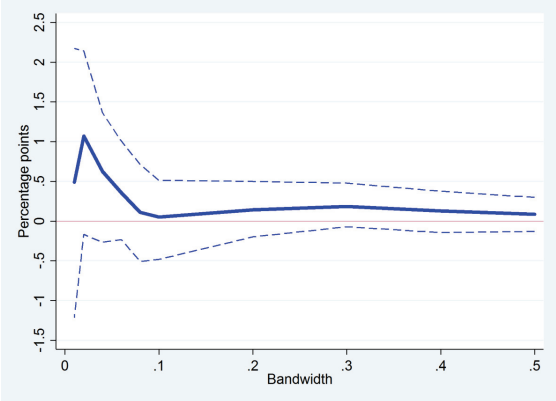
unemployment is analysed. This might indicate that timing of job separations around New Year is of some importance. The increase of the confidence intervals, in combination with the slightly weaker estimated effects for the widest bandwidths, results in only non-significant effects on entry into the activity based definition of unemployment.

Figure 11: Results of the estimation of the effect of eligibility to UI on the treated sample from April to September sample. Sharp RD design.

A) The effects of eligibility on the risk of entering registered unemployment, for different choices of bandwidth, when a sharp RD design is applied



B) The effects of eligibility on the risk of entering activity based unemployment, for different choices of bandwidth, when a sharp RD design is applied



Note: A display of the estimated local average treatment effect of UI eligibility on entry into unemployment, for several choices of bandwidths, ranging from 0.01 BA to 0.5 BA. The solid line represent the point estimate, and the dotted lines represent the limits of a 95 % confidence interval.

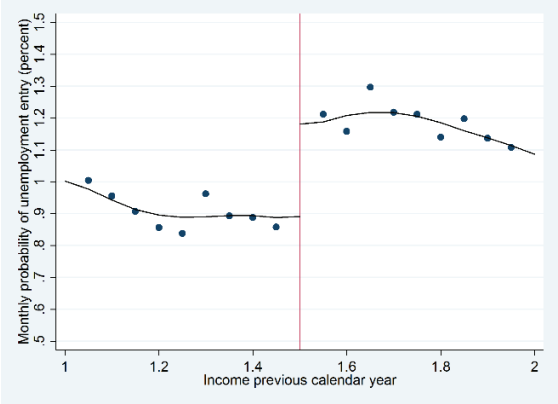
6.1.3. Applying a stricter sampling rule

The sampling rule, intended to include everyone whose eligibility is determined by the income threshold and exclude everyone else, might also create some selection problems caused by employment history. The main sample consists of individual earning between 1 and 2 BA previous calendar year, but below 3 BA in sum over the previous three calendar years. This implies that in the sample, individuals with income previous year above the threshold on average earned less during year t-2 and t-3, than the individuals with income previous calendar year below the threshold did. My estimator controls linearly for income previous calendar years, allowing the slope to be different on each side of the threshold, but does not control for employment history prior to that. It is therefore interesting to see how the results are affected when a sampling rule that makes sure that income in year t-1 is not correlated to earlier employment history through the incomes in year t-2 and t-3 is applied. When conditioning on the

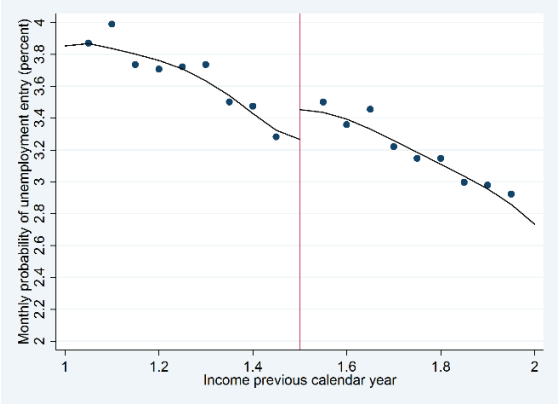
sum of income in year t-2 and year t-3 being less than 1 BA, the probability of being included in the sample does not depend on income in year t-1 (as long as income in year t-1 is between 1 and 2 BA). The distribution income year t-1 of this sample is displayed in Figure 2 C. The results of the estimation on this sample are displayed in Figure 12. Compared to the results on the main sample, the effect of UI eligibility on entry into registered unemployment is reduced from 0.30 to 0.29 percentage points, whereas the effect on entry into activity based unemployment increases from 0.15 to 0.19 percentage points.

Figure 12: Local linear regression results of the estimation of the probability of entering unemployment.

A)Estimating the monthly probability of entry into registered unemployment



B) Estimating the monthly probability of entry into activity based unemployment.

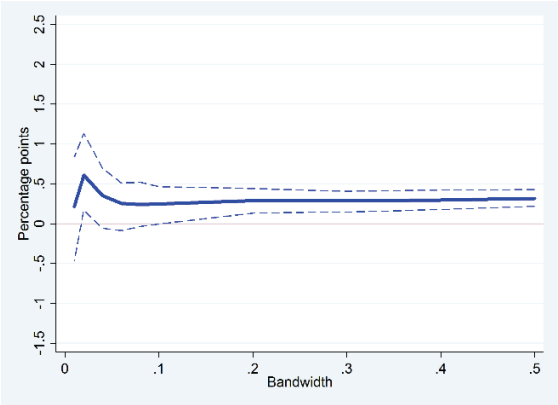


Note: Plots of rates of entry into unemploymnt grouped by income previous calendar year at intervals of 0.05 BA, combined with local linear estimates of the non-parametric regression function, estimated separately on each side of the threshold. The regression function is estimatet at 11 points on each side of the threshold, and a bandwidth of 0.2 BA is applied.

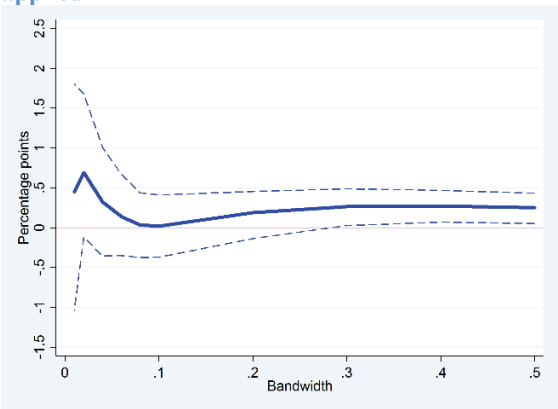
Point estimates and confidence intervals for a range of bandwidths are presented in Figure 13. Due to a decrease in the sample size compared to the main sample, statistical power is lost, and larger bandwidths are needed for the effects on entry into registered unemployment to be significant. The differences in the point estimates on this sample and the main sample are diminishingly small when it comes to the effect on registered unemployment. It is reduced from 0.30 to 0.29 pp. The estimated effects on entry into activity based definition of unemployment increase slightly from this sample exclusion, and the average point estimate for bandwidths between 0.1 and 0.5 BA is 0.20 pp.

Figure 13: Results of the estimation of the effect of eligibility to UI on the main sample. Sharp RD design. Including only observations with sum income year t-2 and year t-3 < 1 BA.

A) The effects of eligibility on the risk of entering registered unemployment, for different choices of bandwidth, when a fuzzy RD design is applied



B) The effects of eligibility on the risk of entering activity based unemployment, for different choices of bandwidth, when a fuzzy RD design is applied



Note: A display of the estimated local average treatment effect of UI eligibility on entry into unemployment, for several choices of bandwidths, ranging from 0.01 BA to 0.5 BA. The solid line represent the point estimate, and the dotted lines represent the limits of a 95 % confidence interval.

6.1.4. An approximation to a fuzzy RD design

As discussed above, Figure 5 suggests that individuals who seem not to satisfy the income requirement are still eligible for UI. This indicates that a fuzzy design is more appropriate.

A fuzzy design requires that treatment is observable. If income does not perfectly predict eligibility, this is not the case in this analysis. Instead, a fuzzy analysis is *approximated* by estimating $w^+ - w^-$ with the help of UI take-up rates among person in the unemployment register, and some additional assumptions.

Let Z_i indicate UI take-up of the unemployed individual i. It takes the value 0 or 1. Let $Z_i(0)$ and $Z_i(1)$ denote two potential outcomes: UI take-up in case of non-eligibility ($W_i = 0$), and UI take-up in case of eligibility ($W_i = 1$). The observed Z_i will then be a function of eligibility and potential outcomes:

$$Z_i = W_i \times Z_i(1) + (1 - W_i) \times Z_i(0) \quad (9)$$

Since W_i indicate "true eligibility", $Z_i(0) = 0$, and thus, $Z_i = W_i \times Z_i(1)$.

Let X_i stil denote income previous calendar year. If we consider observations close to the income threshold, the difference in the take-up rates among unemployed persons on each side of the threshold is $E(Z_i|X_i = c + \epsilon) - E(Z_i|X_i = c - \epsilon)$. By inserting equation (8) this expression can be rewritten to

$E(W_i \times Z_i(1)|X_i = c + \epsilon) - E(W_i \times Z_i(1)|X_i = c - \epsilon)$. By imposing the following assumption:

A4 Conditional Independence between W_i and $Z_i(1)$

We can state that:

$$\begin{aligned} &E(Z_i|X_i = c + \epsilon) - E(Z_i|X_i = c - \epsilon) = \\ &E(W_i|X_i = c + \epsilon) \times E(Z_i(1)|X_i = c + \epsilon) - \\ &E(W_i|X_i = c - \epsilon) \times E(Z_i(1)|X_i = c - \epsilon). \end{aligned} \tag{10}$$

Let us next impose an assumption of continuity of $E\big(Z_i(1)\big)$, regarded as a function of income:

A5 $$\lim_{(x \to c^+)} E(Z_i(1)|X_i = x) = \lim_{(x \to c^-)} E(Z_i(1)|X_i = x)$$

It can be interpreted in line with A1: Take-up rates would not change discontinuously at the threshold in the absence of a jump in the probability of UI eligibility at the threshold.

By applying the following notation, in addition to notation previously defined:

$$z^+ = \lim_{x \to c^+} E(Z_i|X_i = x)$$

$$z^- = \lim_{x \to c^-} E(Z_i|X_i = x)$$

equation (10) can, at the limit, be expressed as:

$$z^+ - z^- = (w^+ - w^-) \times E(Z_i(1)|X_i = c) \tag{11}$$

Consequently, $w^+ - w^-$ will thus be proportional to the difference in take-up rates:

$$w^+ - w^- = \frac{z^+ - z^-}{E(Z_i(1)|X_i = c)} \tag{12}$$

Given A4 and A5 the numerator in equation (12) can be estimated by available data. Without further assumptions imposed, this does not count for the nominator.

We see from equation (6) that the smaller $w^+ - w^-$ is, the stronger will the estimate of the treatment effect be. A conservative approach would thus be to find a lower bound to $E(Z_i(1)|X_i = c)$. This could be done by assuming that everyone with income above the threshold is eligible. In that case z^+, the take-up rate among individuals with income just above the threshold, would be the estimate of $E(Z_i(1)|X_i = c)$.

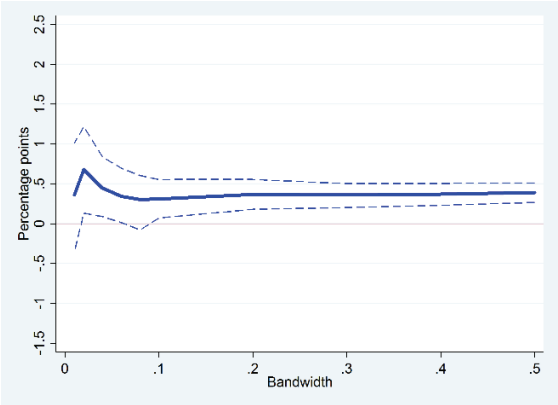
A6 $$w^+ = 1$$

The fact that uptake is not 1 to the right of the threshold in Figure 5 does not contradict A6. Mofitt (1983) explains low take-up rates with "stigma cost". According to Kroft (2008) estimates of the take-up rate of unemployment benefits among eligible individuals varies between 0.4 and 0.7, and the rate is increasing in the size of the benefits. Hence, our estimated take-up rate at about 0.7 for persons just entitled does seem reasonable. This assumption will, on the other hand contradict my discussion related to assumption A2 above. I argue there, that UI uptake among seemingly non-eligible individuals most likely is caused by unsystematic errors in the algorithm applied to determine UI status among repeat users. Unsystematic errors in the algorithm will lead also to errors above the threshold. Thus, we might question the validity of the last assumption. The consequence of still applying it is that the treatment effect is understated, and therefore the use of it might be justified by its moderating effect.

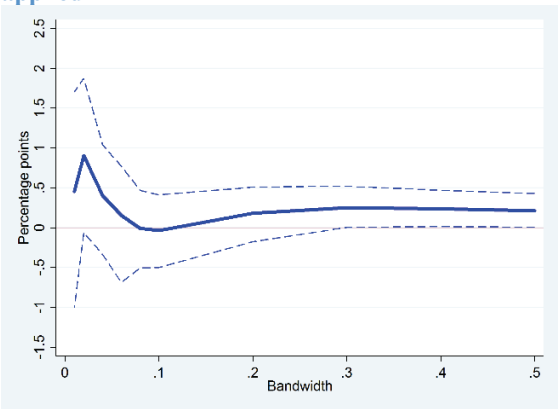
The fuzzy design also reduces the external validity of the results even further. The estimated treatment effect will be the local average treatment effect for compliers: the sub sample that, as opposed to always-takers, has their eligibility status determined by the running variable (Angrist and Pischke, 2009). Assuming that the observations of UI recipients among seemingly non-eligible individuals reflect non-systematic measurement errors in the income data, the sample of compliers will be representative also for the always-takers though.

Figure 14: Results of the estimation of the effect of eligibility to UI, based on an approximation to a fuzzy RD design.

A) The effects of eligibility on the risk of entering registered unemployment, for different choices of bandwidth, when a fuzzy RD design is applied



B) The effects of eligibility on the risk of entering activity based unemployment, for different choices of bandwidth, when a fuzzy RD design is applied



Note: A display of the estimated local average treatment effect of UI eligibility on entry into unemployment, for several choices of bandwidths, ranging from 0.01 BA to 0.5 BA. The solid line represent the point estimate, and the dotted lines represent the limits of a 95 % confidence interval.

Figure 14 reports the estimates that results from the fuzzy regression discontinuity design. The estimates of the treatment effect increase slightly from this exercise. When a bandwidth of 0.2 BA is used, the estimated effects on entry into registered unemployment is 0.39 pp, and the estimated effect on entry into activity-based unemployment is 0.21 pp. The average estimate for bandwidths between 0.1 and 0.5 BA is 0.36 pp on entry into registered unemployment, and 0.18 pp on entry into activity based unemployment.

7. Conclusion

This chapter studies the effect of eligibility to unemployment insurance (UI) on the risk of unemployment entry, by exploiting a setting in the Norwegian UI system that is suited for a regression discontinuity design. Register data is used for the purpose, and the term "unemployment" is operationalized in two different ways: one narrow definition based on entry into the unemployment register, and one wide, based on measures of activity and other welfare benefits.

The main results indicate that UI eligibility increases the monthly probability of entry into the unemployment register by 0.30 percentage points, or in relative terms, by 33 percent. This result

is quite robust to the choice of bandwidth, sample- and model specification. The estimated effects are weaker and less robust when the activity-based measure of unemployment is used. The results indicate that the monthly probability of unemployment measured this way increases by 0.15 percentage points as an effect of UI eligibility. Although the statistical significance of his estimate is sensitive to the choice of bandwidth, the point estimate might indicate that about half of the effect of UI eligibility on entry into registered unemployment may be caused by an effect on job separations. The other half is a re-classification effect on non-employment time: Unemployed individuals entitled to UI are more likely than unemployed individuals without UI entitlements are, to register at the Job Centre.

The present analysis might therefore be said to confirm that UI eligibility affects entry into the unemployment registers, and into unemployment. Although the effects on entry into the broader measure of unemployment is weaker, and not significantly different from 0 when observations close to the New Year is excluded from the sample, the findings of this chapter might indicate that the effects found in the related literature is not only driven by *timing* of unemployment, but also by the *incidence* of unemployment.

Table 2: Effects of eligibility (measured in percentage points) on the risk of entering register based and activity based definition of unemployment, and upper and lower limits of the 95 % confidence interval, by bandwidth choice

		Register based definition			Activity based definition			
	Bandwidth	Effect(pp)	Upper limit	Lower limit	Effect(pp)	Upper limit	Lower limit	Observations within one BW from the threshold
Sharp design, sample from January to December	0.01	0.30692	0.848371	-0.24026	0.387756	1.754407	-0.8119	22 325
	0.02	0.547803	0.929121	0.101254	0.729343	1.616446	-0.16012	44 333
	0.04	0.35731	0.641021	0.008819	0.322169	0.89398	-0.22264	89 059
	0.06	0.259474	0.490531	-0.02317	0.115683	0.555683	-0.37152	133 287
	0.08	0.236308	0.471158	0.038731	-0.00657	0.425513	-0.44579	179 237
	0.1	0.242944	0.452235	0.028707	-0.02779	0.338989	-0.39845	224 784
	0.2	0.298647	0.455282	0.151577	0.148975	0.426552	-0.11507	449 353
	0.3	0.29714	0.420404	0.1673	0.205893	0.398014	0.005056	672 721
	0.4	0.308583	0.415273	0.204353	0.197794	0.375733	0.022056	895 131
	0.5	0.326643	0.421895	0.232802	0.178307	0.330486	0.00364	1 126416
Sharp design, pseudo sample	0.01	-0.23299	0.35485	-0.83428	-0.25645	0.996487	-1.56503	18 904
	0.02	-0.26024	0.114457	-0.67733	0.161814	1.031631	-0.73228	37 875
	0.04	-0.03285	0.250803	-0.32943	0.228127	0.880508	-0.41576	75 508
	0.06	-0.00177	0.260266	-0.26307	0.325582	0.800638	-0.16648	114 532
	0.08	0.012526	0.239811	-0.22119	0.24686	0.658297	-0.17931	153 331
	0.1	0.026146	0.219057	-0.1538	0.181289	0.611491	-0.20483	194 542
	0.2	0.091988	0.242774	-0.03821	0.020394	0.311333	-0.25336	403 266
	0.3	0.065769	0.172396	-0.06581	-0.01126	0.221309	-0.26772	627 053
	0.4	0.058018	0.154258	-0.04431	-0.01232	0.174716	-0.20602	874 157
	0.5	0.052013	0.132185	-0.03506	-0.00545	0.193799	-0.19309	1 161042
Fuzzy design, main sample	0.01	0.360372	1.011115	-0.36335	0.455286	1.704641	-1.00932	22 325
	0.02	0.679912	1.217182	0.131725	0.905234	1.876306	-0.06041	44 333
	0.04	0.447586	0.838932	0.088784	0.403565	1.041851	-0.33411	89 059
	0.06	0.340744	0.69429	0.014007	0.151917	0.770644	-0.68776	133 287
	0.08	0.302123	0.60414	-0.07914	-0.00839	0.467619	-0.50529	179 237
	0.1	0.308998	0.553556	0.070556	-0.03534	0.412858	-0.50029	224 784
	0.2	0.364667	0.55417	0.179089	0.181908	0.508282	-0.17656	449 353
	0.3	0.35947	0.502803	0.202275	0.249082	0.520336	0.003385	672 721
	0.4	0.371014	0.504274	0.230559	0.237811	0.470078	0.011831	895 131
	0.5	0.390899	0.510531	0.267398	0.213383	0.42908	0.004414	1 126416
Sharp design, sample from April to September	0.01	0.634762	1.503086	-0.18605	0.488616	2.174229	-1.21676	11208
	0.02	0.792019	1.413443	0.135831	1.068875	2.141049	-0.16782	22 354
	0.04	0.535979	0.986293	0.075074	0.625634	1.364033	-0.26561	44 804
	0.06	0.348024	0.697739	-0.04758	0.355296	1.005804	-0.23286	67 165
	0.08	0.304894	0.632389	0.004647	0.1089	0.71413	-0.50795	90 548
	0.1	0.274553	0.557604	-0.004	0.050637	0.512183	-0.4817	113 607
	0.2	0.260961	0.454499	0.042924	0.142929	0.49806	-0.19851	227 657
	0.3	0.243181	0.397741	0.088852	0.184843	0.477526	-0.07117	341 115
	0.4	0.26787	0.413147	0.135178	0.12923	0.376045	-0.14468	453 800
	0.5	0.285843	0.416458	0.148402	0.08427	0.298546	-0.12979	570 742
Sharp design, sample with sum income year t-3 and t-2 < 1	0.01	0.216577	0.833953	-0.46648	0.448837	1.801757	-1.04319	17 769
	0.02	0.609668	1.130308	0.170365	0.691954	1.679405	-0.11797	35 412
	0.04	0.352146	0.697079	-0.05744	0.324316	1.007422	-0.35628	71 110
	0.06	0.253749	0.510127	-0.08694	0.138099	0.667336	-0.35041	106 439
	0.08	0.239474	0.520176	-0.03496	0.03321	0.436712	-0.37636	142 976
	0.1	0.24678	0.46475	-0.00431	0.019766	0.410066	-0.37207	179 800
	0.2	0.289662	0.440317	0.133005	0.186774	0.45459	-0.13713	358 691
	0.3	0.285175	0.404878	0.146108	0.265261	0.486553	0.025556	537 390
	0.4	0.297597	0.422148	0.176829	0.270915	0.468216	0.069549	714 823
	0.5	0.318875	0.42521	0.218837	0.252384	0.432282	0.050638	896 553

Note: bandwidth is measured in basic amounts (BA). 1 BA ≈10 000 €.

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Chapter III:

The unemployment insurance replacement rate and job durations

Abstract

Based on administrative register data, this chapter investigates the effect of the unemployment insurance (UI) replacement rate on entry into unemployment. In Norway, UI benefits are proportional to earlier income, but income from the prevailing calendar year at the timing of unemployment entry is excluded from the income basis for UI. This institutional characteristic implies that two individuals with the same wage level and job duration may will face different UI replacement rate in case of unemployment, dependent on their timing of entry into the labor market.

This random-assignment-like variation is exploited to analyse the causal effect of the UI replacement rate on the employment duration among newcomers in the labor market. A mixed proportional hazard rates model is applied to individual register data. Two different samples of labor market entrants are used: Immigrants and newly educated.

The findings suggest that while variation in the replacement rate on the extensive margin (having a replacement equal to zero vs. facing a positive one, i.e. eligibility for UI) is statistically significant in explaining unemployment entry, variation on the intensive margin, given eligibility, is not. When allowing for heterogeneous responses to variation in the replacement rate, workers in very small companies appear to be sensitive also to continuous variation in the replacement rate.

1. Introduction

This chapter investigates how the transition from employment to unemployment is affected by the replacement rate in the unemployment insurance system. The focus in the economics literature has mainly been on how unemployment insurance generosity affects *exit* from unemployment, and the main findings suggest that both the replacement rate and the potential

benefit duration affect the duration of unemployment spells positively; see e.g., Tatsiramos and van Ours (2012) for an overview. The knowledge about how the unemployment insurance system affects the overall unemployment level in an economy will be limited though, unless also the mechanisms related to entry into unemployment are well understood, since the steady state unemployment level is affected both by the transition rate from employment to unemployment, and the transition rate from unemployment to employment. With constant transition rates between employment and unemployment, the steady state rate of unemployment is equal to the transition rate from employment to unemployment divided by the sum of the the two transition rates (Mortensen and Pissarides 1994). The steady state unemployment rate will increase if rate of entry into unemployment increases.

Mortensen and Pissarides (1994) also discuss factors that can affect the rate of entry into unemployment. Existing job matches are hit by idiosyncratic shocks to productivity. If the productivity level of a match falls below a certain reservation level, the firm will destroy the match. The reservation productivity level is increasing in, among other factors, the replacement rate in the prevailing UI system. Therefore, more generous UI benefits might increase the transition rate into unemployment.

An interpretation of this model is that employees entitled to generous UI benefits have better outside options than employees with less generous entitlements. Thus, negative shocks to productivity are less likely to result in a new agreement on a lower wage, the more generous the UI entitlements of the individual are.

Following this argument, workers' response to shocks to the reservation wage, or disutility from work might also depend on their degree of unemployment insurance. For example, if the working environment at the workplace deteriorates, workers with generous entitlements will be more likely to quit their job to search for a new one (Blanchard and Tirole 2008).

On-the-job search might also explain why poorly insured individuals separate from employment less often than well insured. The working environment act requires that a note of dismissal is given some time before the actual destruction of the job match takes place. (*The Working Environment Act, § 15-3*) We could therefore also expect to see an effect of UI generosity on unemployment entry simply from the fact that individuals with less generous UI entitlements will search more intensively for a new job and/or reduce their reservation wage during their time of

notice. If this is the case, displaced workers entitled to small UI benefits will be more likely to have a new job at hand before the old employment relationship ends. Gutierrez (2012) shows that *any perceived risk* of a layoff will make on-the-job-search activity sensitive to UI generosity. Workers will engage in less on-the-job search if they are entitled to (more generous) UI benefit, as long as they apprehend some risk of job loss.

Given strong employment protection, a generous UI system can also be used to "buy off" less productive workers. The Norwegian Working Environment Act (Arbeidsmiljøloven) states that "Employees may not be dismissed unless this is objectively justified on the basis of circumstances related to the undertaking, the employer or the employee" (*The Working Environment Act (Official English Translation of Arbeidsmiljøloven) §§15-1 - 15-17* 2005). A severe breach of the written contract of employment may thus be a legitimate reasons for a layoff, but these actions might be hard to document, and the limits between acceptable and non-acceptable breaches may always be questioned. Laying off an unproductive worker might thus be less hazardous if this person is entitled to generous UI benefits than if not, as the risk of legal conflict is smaller, and the firm might be spared from a reputation as an uncooperative employer (Rebollo-Sanz 2012).

The Norwegian unemployment insurance system is universal, and covers everyone with an annual income above approximately 15 000 €. The benefits are proportional to labor income in the *previous calendar year*. Income earned in the year of unemployment entry is not included in the income basis for UI. This institutional detail creates variation in the replacement rate among newcomers in the labor market. During the first 24 months of a job spell, two employees with the same employment tenure will face different replacement rates in case of unemployment, depending on the timing of entry into the labor market. This random-assignment-like variation will be exploited in this chapter, to extract the causal relationship between the degree of unemployment insurance, and entry into unemployment.

The effect identified in this paper can be interpreted as the expected change in the hazard from employment to unemployment following from a marginal increase in a worker's UI replacement rate. This interpretation follows from the choice of random-assignment-like variation used for identification. Due to the source of identification applied, this paper thus focuses on the

microeconomic effect, and abstracts from general equilibrium effects resulting from e.g. changed wage dynamics.

The main findings of the present analysis are that the replacement rate affects the transition to registered unemployment, but that most of the effect is driven by variation at the extensive margin (having, versus not having a positive potential replacement rate). An exception seems to be among workers in the smallest enterprises. The effect of variation at the intensive margin is stronger in this group than for employees in larger firms.

2. Related literature

This chapter relates to the small, but growing literature on the effects of UI generosity on unemployment entry. Some contributions have studied how the potential benefit duration (PBD) affects entry into unemployment. Others focus on the effects of eligibility to UI, and yet others have focused on the topic of the present paper: the effect of the UI replacement rate on entry into unemployment.

Winter-Ebmer (2003) analyse an increase in PBD from 52 to 209 weeks among older workers in specific regions in Austria. Lalive et al. (2011) study smaller increases in PBD in Austria, from 30 to 39 or 52 weeks. Fitzenberger and Wilke (2009) study the PBD expansions in Germany during the 1980s. Dlugosz, Stephan, and Wilke (2013) study the shortenings of PBD that took place in Germany in 2006. Baguelin and Remillon (2014) study the effects of a sharp reduction in PBD that took place among older workers in France in 2003. Tuit and van Ours (2010) study the removal of a PBD discontinuity at the age of 57.5 in the Dutch UI system. The main conclusion from these studies is that UI is used to bridge employment and early retirement. Thus, changes in PBD mainly affect workers close to retirement age. The Austrian analyses however, find effects also among younger workers.

The contributions on the effect of eligibility on unemployment entry typically study job duration requirement for eligibility to UI, and how the timing of unemployment entry is adjusted accordingly. Christofides and McKenna (1995), Green and Riddell (1997), and Green and Sargent (1998) study survey data from Canada, and find that job durations seem to concentrate around the job duration requirements. Based on register data, Rebollo-Sanz (2012) shows the

same for Spain, and Hägglund (2009) finds similar results when analysing the effects of prolonged employment requirements in Sweden during the 90s.

The contributions most closely related to the present paper use American survey data to investigate the effect of the UI benefit size on entry into unemployment. Topel (1983) study the march 1975 file of the current population survey, including retrospective data on earnings and employment. Based on state- and industry variation in the level of UI benefits, he finds that the risk of experiencing a layoff increases with the replacement ratio.

Jurajda (2002) studies 808 males from the trade adjustment assistance (TAA) survey; data resulting from retrospective interviews with persons that experienced unemployment during the 1970s, merged with data on UI claims. Due to differences between states, and various programs, the data contain variation on both eligibility, benefit size and potential benefit duration. He estimates a competing risks hazard rate model from employment to quit and layoff, and finds that while the hazard to quits is unaffected by UI parameters, layoffs are affected by eligibility, but not by benefit size or potential benefit duration.

Light and Omori (2004) investigate the males in the 1979 cohort of the National Longitudinal Survey of Youth, and study how job to job transitions, quits to non-employment and layoffs depend on the weekly unemployment benefit amount (measured in dollars). They find that the benefit size affects the transition to new jobs negatively, which indicates that the benefit size influences on-the-job search. An implication of this is that the risk of actually becoming unemployed increases if the benefit size increases. Layoffs and quits to non-activity are also affected positively by an increase in the benefit amount.

I contribute to this literature by applying register data, and a random-assignment-like variation in the replacement rate, which allows for identification of a causal effect. I estimate a single risk hazard rate motel on employment spells. I only model the transition to unemployment. Employment spells are censored if they enter education, start claiming health- or age related benefit (not sickness pay), take parental leave, if they leave the employment register for more than two months without registering as unemployed, or by the end of 2010

Among the contributions above, only Jurajda (2002) distinguishes variation in the replacement rate on the extensive margin (being, vs. not being eligible to UI) from variation on the intensive

margin (small changes in the replacement rate). In some versions of the econometric model applied in the present paper, I test whether or not Jurajda's findings can be confirmed in the Norwegian data.

Some of the literature on the effects of features of the UI system shows gender differences in the responsiveness to economic incentives (See e.g. Røed and Zhang, 2003). As men and women seem to have different patterns of transitions in the labor market, analyses are quite often conducted on male samples only. This is the case with Light and Omori (2004) and Jurajda (2002). Topel (1983) pools men and women, but does not attempt to investigate whether or not women and men are affected differently by the replacement rate. The present paper also pools men and women, but in some of the model specifications, an interaction term between the replacement rate and gender is included, to study whether or not women's entry into unemployment is more motivated by the UI replacement rate than are men's entry into unemployment.

It is also plausible that earlier experience with unemployment might alter the responsiveness to variation in the UI replacement rate. To act upon economic incentives requires some awareness of them. Prior experience with unemployment probably provides knowledge to the institutional details, and may therefore lead to a stronger response to variation in the replacement rate. We might also suspect that welfare stigma costs, in line with Moffitt (1983), are affected by unemployment experience. It is not clear, though, whether stigma costs decrease or increase from experience. According to Lucas, Clark, Georgellis, and Diener (2004) who study German longitudinal data on life satisfaction, people who had been unemployed earlier did not react differently to a new incidence of unemployment than did persons who had not been unemployed before. This might indicate that the stigma costs related to unemployment are not reduced because of prior experience. To investigate whether earlier unemployment experience change the responsiveness to variation in the replacement rate, I introduce an interaction variable between the replacement rate and an indicator for high experience with unemployment in some of the model specifications.[1]

[1] I define high unemployment experience as having been in the unemployment register more than 6 of the previous 60 months.

To be able to isolate random-assignment-like variation in the replacement rate, the sample applied in the present paper is limited to newcomers in the labor market: Youths and immigrants. Despite the fact that newcomers are not of particular interest per se, knowledge on the patterns of labor market transitions in this group might be helpful when the results are to be interpreted. In general, despite possessing higher education levels on average, youths are more exposed to unemployment, and also to more long term unemployment, than the general population (see e.g. Brada, Marelli, and Signorelli, 2014). The youth unemployment rate is also more sensitive to business cycles than the total unemployment rate (Hutengs and Stadtmann 2014). There also exists evidence of scarring effects of unemployment among young people (see e.g. Nilsen and Reiso 2011). The fact that the levels, determinants, and effects of youth unemployment and general unemployment differ, might suggest that the results in this analysis cannot be generalized to the working population at large. For example, given scarring effects, avoiding unemployment might be a long term investment among young people, to a larger extent than among workers more established in the labor market. This might imply that, although entry rates into unemployment is higher among young workers, it is less driven by short term economic incentives, since the long term incentives to avoid unemployment and scarring effects is stronger. On the other hand, young people might have smaller stigma costs related to welfare participation (Moffitt 1983). Interaction over time between economic incentives and social norms might also lead younger generations to view UI as an insurance they are not only entitled to, but also entitled to exploit (Lindbeck, Nyberg, and Weibull 1999). If this is the case, we should expect the effects identified in the sample of this study to be stronger than the effects in the general population. These distinctive qualities of the samples applied limit the external validity of the analyses.

3. Institutional settings

The Norwegian unemployment insurance system is universal, and covers every job applicant with some attachment to the labor market. To be entitled to unemployment insurance, the following requirements have to be fulfilled:

- Have work hours unintentionally reduced by at least 50 %.
- Be searching for a job
- Register as unemployed at the Norwegian Labor Administration, and confirm every second week to be still unemployed, and searching for a job
- Wage income in the previous calendar year has to exceed 1.5 Basic Amounts (BA) *or* wage income over the three previous calendar years has to sum up to more than 3 BA *(The National insurance act, §§ 4-3 – 4-8)*.

One Basic Amount corresponds in 2012 to approximately 80 000 NOK, or 10 000 €. The amount is regulated annually, as a function of Norwegian price- and wage growth.

If the job applicant is qualified for unemployment benefits, the income basis (IB) will be calculated as the maximum of work income previous calendar year and the average of work income over the three previous calendar years, up to a ceiling of 6 BA. The yearly payments will be 62.4 % of IB *(The National Insurance Act, §§ 4-11 – 4-12)*.

If IB exceeds 2 BA, the potential duration of benefit payments is 24 months, and if IB is between 1,5 BA and 2 BA, it is 12 months (*The National Insurance Act, § 4-15*).

The fact that unemployment benefits are calculated based on income in the previous calendar years creates variation in the degree of unemployment insurance among newcomers in the labor market. Imagine two labor market entrants without prior work experience. One starts a full time job on January the 1st, and the other enters employment on July the 1st. They are hired at the same yearly wage. After 12 months, they both become unemployed. Due to timing of entry and exit, the January-starter had his entire work period located in previous calendar year, and hence, he will have his entire yearly wage included in the benefit basis. The July-starter on the other hand, will have only half of his yearly wage included in the benefit basis, since only six of the 12 months in his job spell was placed in the previous calendar year.

My identification strategy is to exploit the fact that for a given job spell duration within the 24 first months after employment entry; the benefit basis will depend on the timing of labor market entry. The share of the yearly wage that is included in the income basis, given job duration and starting month, is illustrated in Figure 1. The solid line illustrates a person entering employment in January. The first twelve months of employment, he will not be entitled to UI at all. During these months, all of the wage earned will be earned within the current calendar year, and thus, his

income basis is zero. The moment he enters his second year of employment, everything earned so far, an entire annual salary, will be placed in the previous calendar year, and are therefore included in the income basis for UI. The development over time in UI entitlements for a July entrant, in Figure 1 displayed by the long-dashed line, will be different. The July starter crosses his first turn of the year already after 6 months of employment. He then becomes eligible for UI, but only the wage earned during these 6 months, corresponding to half a yearly salary, will be included in the income basis for UI. Not until the time when the July starter enters his third calendar year of employment, will his income basis consist of an entire yearly salary.

In most UI systems, there is a penalty related to voluntary quits and layoffs resulting from severe misconduct. In Norway, this involves a quarantine of 8 weeks before benefits are paid out. This clearly reduces the incentives for a job quit, and might indicate that some of the mechanisms described in the introduction, which is based on the employee taking an active part in the job separation process, are irrelevant. 8 weeks is a short period of time compared to the potential benefit duration period, which for most workers is two years, and thus the prospects of receiving UI benefits after 8 weeks can still be expected to affect the worker's behavior, but the legal setting clearly encourage layoffs more than quits.

The empirical literature referred to in the previous section confirms that the distinction between quits and layoffs is vital: Quits appear not to be affected by UI benefit size, while layoffs are. I am not able to separate quits from layoffs in my data, which is a weakness of the present analysis. Pooling one transition that is likely to be affected by the replacement rate together with one transition that is not will weaken, and potentially hide the effect of interest. On the other hand, in most cases employers and employees have the possibility to cooperate on mislabelling quits as layoffs (Blanchard and Tirole 2008). The incentives to do so are present in all UI systems with sanctions to quits. This suggests that a distinction between quits and layoffs might be more or less feigned, and that pooling quits and layoffs into one transition into unemployment still has relevance.

The mislabelling of quits as layoffs is not directly favorable to the employer. However, he might be willing to do so in the interests of the employee, at least if the job separation is beneficial to both parties. These actions are probably more prominent in case of tight employer-employee

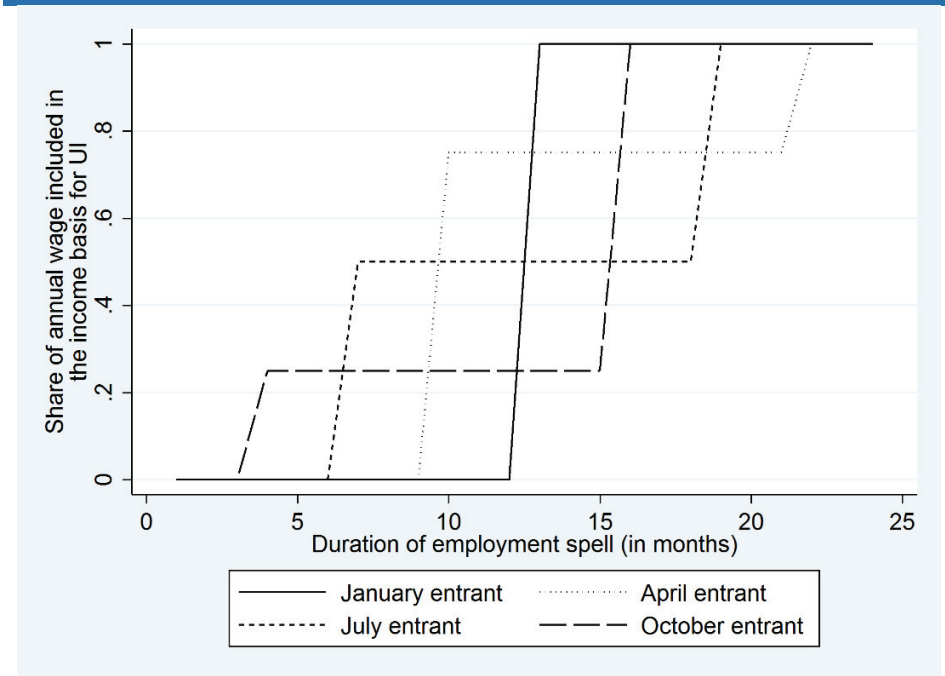
relationships. For this reason, the paper also investigates treatment effect heterogeneity among workers in small and large firms.

4. Data

The analysis is based on Norwegian individual register data from Statistics Norway. Registers on employment- , education- and welfare spells, registered unemployment and demographics are applied to create a panel data set, between January 2004 and December 2010. Individuals that can be characterized as newcomers in the labor market are followed on a monthly basis from the time they enter their first job[2]. In this case, the term "newcomers" points to persons that at the time when I start observing them, have no, or negligible entitlements in the unemployment insurance system. In this analysis, I study two groups for which this is likely to be the case.

1. Individuals that have completed an education lasting at least three years.
2. Immigrants from outside of EU.

Figure 1: The dependency of the income basis for UI on timing of employment entry



Note: Share of annual wage included in the income basis for UI benefit calculation, by employment spell duration, for employment spells starting in January, April, July and October

[2] The timing of entry is defined as the first month of presence in the employment register combined with no record of ongoing education, or any other benefits. In addition the employment has to be full time employment.

Individuals under education are likely to have part time jobs and summer jobs, and hence they are not newcomers in a strict sense. The income earned during education, however, is likely to be small compared to the wage obtained from a full time job. The time available to paid work will be limited. In addition to this, regulations in the student financing system imposes a high real tax on students earning too much: Unless labor income is above a certain threshold, which in 2013 was set to 1.77 BA, students get a substantial share of their student loans transferred to grant if they follow normal progression (Lånekassen.no). To ensure even further that the newly educated do not have a history of high income when they enter the labor market, I limit my sample to persons earning less than 2 BA per year during their education period.

People entering the country for the first time are more likely to be newcomers in a strict sense, meaning that they have no earlier income on which UI benefits can be based. Immigrants from the European Union can however have UI based on income from their native country, and therefore EU immigrants are excluded from this sample (Instructions to §§4-1 – 4-28 in the National Insurance Act (Dagpengeforskriften), § 13-5).

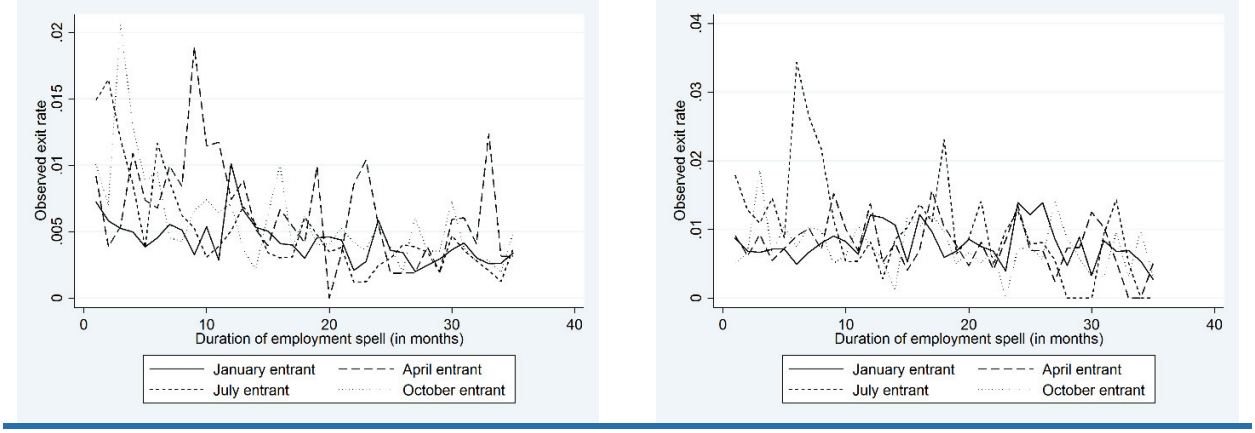
In spite of having the newcomer condition in common, the samples are likely to differ from each other in many ways. The econometric analysis will be conducted on each sample separately.

Table 1 reports the number of employment spells belonging to each newcomer category, and descriptive statistic for the first duration month of the spells are provided in Table 2. We see that persons that have been in education typically enter employment during the summer months, but there is some variation on starting month also within this sample. A substantial share of the employment spells start in January. Also among immigrants and individuals with exhausted UI, January is a frequent starting month.

The individuals are observed for up to 36 months, or until they register as unemployed. I right censor when they enter education, start claiming health- or age related benefit (not sickness pay), take parental leave, if they leave the employment register for more than two months without registering as unemployed, or by the end of 2010.

Figure 2: Observed monthly rates of exit to unemployment, by employment spell duration, for persons entering employment in January, April, July and October

A) Sample of newly educated **B) Sample of immigrants**



Note: The figure is based on the sample described in Table 2

Figure 2 displays the monthly probabilities of exit to unemployment, by employment spell duration, for workers entering employment at different times during the year. We see in general that the sample of immigrants have a higher risk of entering unemployment than the sample of newly educated. We also see a clear seasonal pattern: Transition into unemployment increases sharply in January, irrespective of entry month[3].

Table 1: Numbers of full time employment spells, by starting month and newcomer condition

	Education	Immigrant
Jan	10 216	4 574
Feb	2 705	1 611
March	1 532	1 309
April	1 306	1 314
May	2 386	1 481
June	30 838	1 564
July	10 861	1 506
August	19 536	1 874
September	7 423	1 902
October	4 058	1 537
November	2 803	1 547
December	2 285	693

Note: Source: Duration data on education, employment, and information on timing of arrival to Norway, from the years 2004 to 2010

[3] For January entrants the 13th and the 25th duration month is a January calendar month. For July entrants the 7th, 19th and 31st duration month is a January calendar month, etc.

Table 2: Descriptives of the employment entrants, measured at the time of employment entry

	Education	Immigrant
Age	24,7	32,8
Female	50 %	26 %
Tenure (in years)	1,56	0,32
Single no children	81,8 %	10,7 %
Couple, no children	9,2 %	0,8 %
Couple, with children	2,6 %	8,8 %
Experience, UI	0,3	0,0
Experience, unemployment	1,0	0,3
Living in Southern Norway	5,8 %	0,9 %
Living in Western Norway	26,7 %	6,7 %
Living in Mid Norway	9,1 %	1,7 %
Living in North Norway	9,9 %	1,4 %
No education	0,0 %	17,1 %
Compulsory schooling	14,3 %	6,2 %
Higher education	55,3 %	19,9 %
Education unknown	0,1 %	39,0 %
Public sector	35,4 %	10,9 %
Number of employees (median)	390	51
N	95 949	20 912

Based on the institutional details we can, if the replacement rate is a central factor in the processes that leads workers into unemployment, expect to see that the development over time, in the exit rate to unemployment, will differ between persons entering employment in different starting months. A January entrant is not eligible for UI the second month of his employment spell. Neither is the July starter. One year later however, in duration month 14, the January starter is entitled to a replacement rate of 62.4, while the July starter will only receive half of this in case of unemployment. In duration month 26, both the January- and the July entrant are entitled to a replacement rate of 62.4 %. Based on these system features, we can state the following hypothesis: The exit rate among January starters will increase more (fall less) from duration month 2 to duration month 14 than the exit rate among July starter. In addition: The exit rate among January starters will increase less (fall more) from duration month 14 to duration month 26 than the exit rate among July starters. Stated more generally;

A. The change in the exit rate from duration month 2 to 14 will be a decreasing function of the within-year month of entry into employment.
B. The change in the exit rate from duration month 14 to duration month 26 will be an increasing function of the within-year month of entry into employment.

In a preliminary descriptive analysis, I evaluate these hypotheses. By within-year month of employment entry, I calculate the conditional exit rate to unemployment in duration month 2, 14 and 26 respectively. Table 3 reports the number of ongoing employment spells at the beginning of, and the number of exits during, these months.

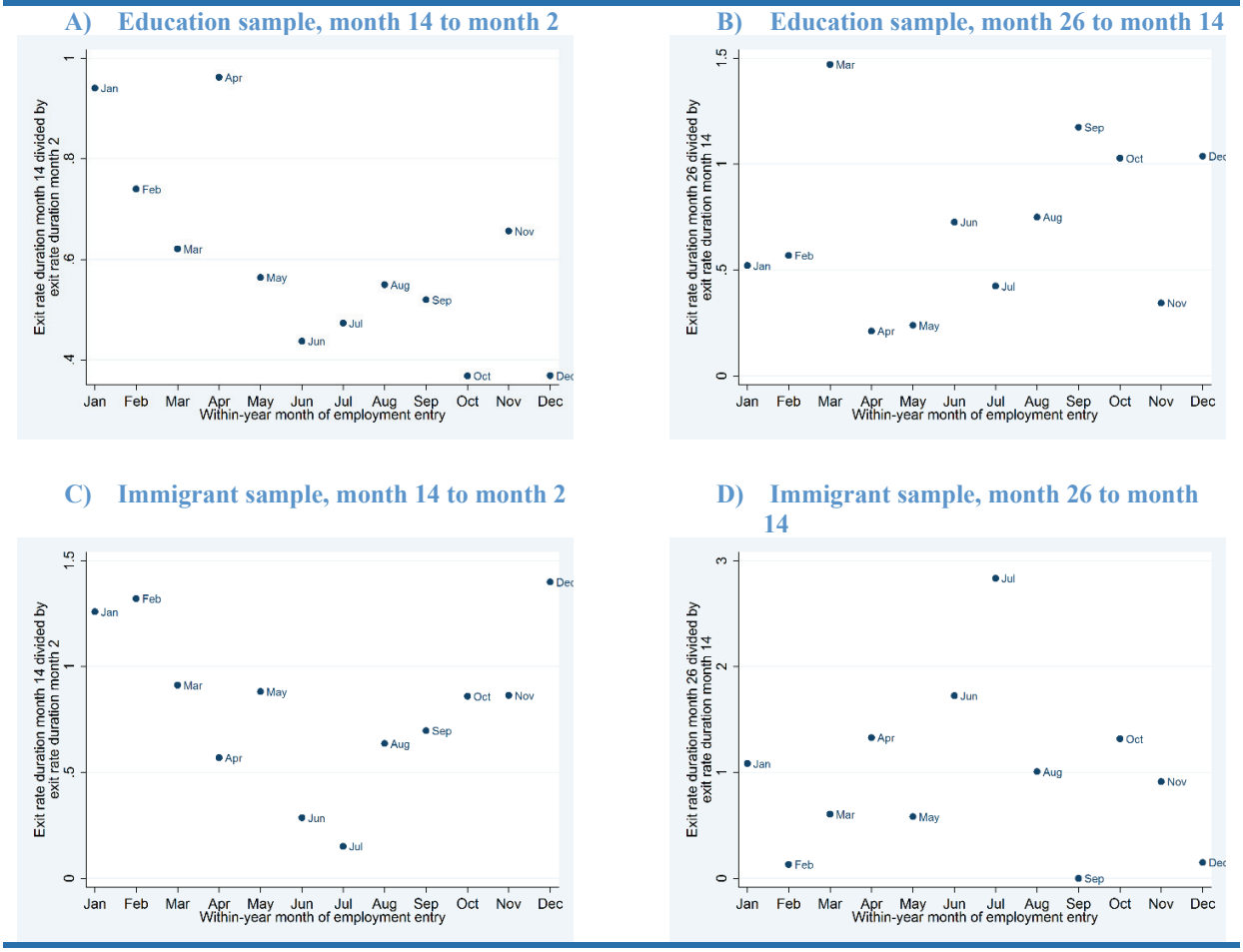
Table 3: Number of ongoing spells and rate of exit to unemployment, by duration month

	Education sample		**Immigrant sample**	
	Number of ongoing spells	Share that exits to unemployment	Number of ongoing spells	Share that exits to unemployment
Duration month 2	96225	1.02 %	21849	1.02 %
Duration month 14	60250	0.54 %	12803	0.87 %
Duration month 26	38865	0.36 %	7011	0.66 %

Note: Based on the sample described in Table 1 and 2.

The ratio of the exit rate in duration month 14(26) to duration month 2(14) is used as a measure of the change in the exit rate from month 2(14) to month 14(26). These ratios are plotted in Figure 3, sprit by within-year month of entry. Figure 3 A displays the ratios of exit rate in month 14 to exit rates in month 2, in the sample of newly educated. From hypothesis A, we should expect to see a falling pattern in this figure. We see that the conditional exit rate among January entrants from the education sample nearly does not change from duration month 2 to duration month 14. The exit rate among July entrants is more than halved during the same period. Although there are large fluctuations from entry month to entry month, the general picture in Figure 3A might be claimed to confirm hypothesis A. The pattern is not equally evident in the immigrant sample, displayed in panel C of Figure 3, though. Panel B in Figure 3 display the ratios of the conditional exit rates in duration month 26 to duration month 14 in the sample of newly educated, and panel D does so for the immigrant sample. From hypothesis B, we should expect to see an increasing pattern in these panels. Such a pattern is not visible in any of the panels.

Figure 3: Ratios of the conditional exit rates to unemployment in duration month 14(26) to the conditional exit rate to unemployment in duration month 2(14), by within-year month of employment entry



Note: Figure is based on the sample described in Table 1 and 2. From hypothesis A we should expect to see a falling pattern in panel A and C. From hupothesis B we should expect to see an increasing pattern in panel B and D.

The lack of correspondence between the hypotheses and the empirical patterns could reflect that there is no effect of the replacement rate on the entry to unemployment, or it could indicate that the individuals most responsive to the economic incentives have left employment already prior to month 14(26). To be able to discuss the topic further, a duration analysis of employment spells is conducted.

5. Econometric analysis

To investigate the causal relationship between the replacement rate and the transition to registered unemployment, I estimate a mixed proportional hazard rate model. I study newcomers

in the labor market from the timing of entry (given that the first job they enter is a full time job), and for 3 years, until the end of 2010, or until a transition to unemployment occurs, on a monthly basis.

The model is defined in terms of a continuous underlying hazard rate function. The hazard rate for individual i at duration time d, given that the worker is still employed at time d is given by $\theta_i(d)$:

$$\theta_i\,(d) = \frac{\lim_{\Delta\, d \to 0} P(d \leq\ D \leq\ d\ + \Delta\, d | D \geq\ d, i\,)}{\Delta\, d} \qquad (1)$$

I assume that the underlying continuous hazard function is piecewise constant, and proportional. The hazard rate, $\theta_i(d)$, is assumed to be constant within every duration month m, but to vary freely between months.

$$\theta_i(m) = \exp\begin{bmatrix}\sigma_m I(M = m) + \lambda_e I(E = e) + \eta_t I(T = t) \\ +\psi rr_{im} + \beta x_i + v_i\end{bmatrix}, \qquad (2)$$

$I(M = m)$ is an indicator variable taking the value 1 if duration variable M takes the value m. M varies between 2 and 36. $I(E = e)$ indicates that month of entry, E, which varies between 1 and 12, equals e. Equivalently $I(T = t)$ indicates calendar month. T takes a separate value for each calendar time month between March 2004 and December 2010. rr_{im} is the replacement rate that individual i faces if unemployment entry were to happen during duration month m, measured in percentage of past full-year-equivalent earnings. ψ is thus the parameter of interest. It measures the relative effect on the hazard from a 1 percentage point increase in the potential replacement rate. x_i is a vector of dummy variables related to gender, age, education, family situation, immigrant status, region of residence, firm size and sector, all measured at the first month of the employment spell[4]. I also include information on earlier experience with unemployment. The

[4] Age is split up in 5 categories. Below 30 years, between 30 and 34, between 35 and 39, between 40 and 49, and above 50. Education is divided into 5 groups: No education, compulsory schooling, high school, higher education and unknown education. Family types are grouped by couple/single, and by children/no children. Immigrant status is split up in "not immigrant", "western immigrant" and "non-western immigrant", regions or residence are eastern-, southern-, western-, mid- and north- Norway. Earlier experience with unemployment is based on counting the months in the unemployment register the 60 months prior to employment entry. It is split up in three categories: 0, 1-6 ("medium") and above 6 ("high"). Tenure in the labor market is based on a count of the years prior to entry into the labor market with a yearly work income above 2 BA. It is split up in the following groups: 0, 1-5, 6-10, 11-20 and above 20. Firm size is based on counting the number of individuals in the enterprise in April at the year of

immigrants may have been registered as unemployed in the period after arrival to Norway, but prior to their first job. Newly educated might have been registered as unemployed both prior to their education spell and in the period succeeding their employment spell and preceding their first job. The newly educated might also have work experience prior to their education spell, and in the regressions on this sample, I include information on earlier work experience. v_i is a term of unobserved heterogeneity.

The probability of exit to registered unemployment during calendar month m is

$$h_{im} = 1 - \exp[-\exp\begin{bmatrix}\sigma_m I(M=m) + \lambda_e I(E=e) + \eta_t I(T=t) \\ +\psi rr_{im} + \beta x_i + v_i\end{bmatrix}] \tag{3}$$

and the contribution to the likelihood function for a person i who enters unemployment during duration month m, is, conditional on the unobserved individual specific term v_i equal to:

$$L_i(v_i) = \prod_{d=1}^{m-1}\{[1-h_{id}]\} \times h_{im} \tag{4}$$

The distribution of unobserved heterogeneity is unknown. It is approximated as a discrete distribution. Support points are added until the likelihood function with the number of estimated parameters subtracted, is maximized. With $L_i(v_i)$ inserted from equation (4), the likelihood function is given as[5]

$$L = \prod_{i=1}^{N} E_{v_i}[L_i(v_i)] = \prod_{i=1}^{N}\sum_{l=1}^{W} p_l L_i(v_i), \qquad \sum_{l=1}^{W} p_l = 1 \tag{5}$$

N is the number of employment spells included in the regression, and W is the number of support points for the distribution of unobserved heterogeneity.

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employment entry. It is divided into 5 groups: below 10, 10-49, 50-199, 200-499 and above 500. Sector indicates whether the enterprise was public or private.

[5] The likelihood function is programmed and maximized by Research fellow Simen Gaure at the Ragnar Frisch Centre for Economic Research.

The replacement rate of individual i, rr_i, is determined as a function of timing of entry into the labor market and calendar year changes:

$$rr_i = \begin{cases} \blacksquare & 0 \text{ if calendar year = 1 } \textit{or} \text{ if calendar year = 2 and entry month } \geq 10 \\ \blacksquare & \frac{13-E_i}{12} \times 62.4 \text{ if calendar year =2 and entry month } \leq 9 \\ \blacksquare & 62.4 \text{ if calendar year =3 or 4.} \end{cases} \quad (6)$$

The mean levels of this variable, over all the monthly observations are 33.5 % in the education sample and 30.4 % in the immigrant sample.

Individuals that enter employment during the last months of the year are likely not to earn above the minimum income threshold for UI eligibility, equal to 1.5 BA, during their first calendar year. If this is the case, their potential replacement rate in calendar year 2 is equal to zero. To deal with this, I set the replacement rate in calendar 2 equal to 0 for individuals that enter in October, November and December.

Since the replacement rate during the second calendar year is a function of entry month, and individuals starting work in different months might differ in their probability of becoming unemployed, dummies for entry month are included in the regression. This vector will than capture differences in the probabilities of becoming unemployed that are constant during the employment spell, between individuals starting in different months. The replacement rate is also increasing in spell duration. The vector of duration month indicators controls for effects of duration that are common for every spell, irrespective of starting month. The vector of calendar time controls for time effects common to every spell, on the hazard into unemployment. The individual experiences an increase in the replacement rate the first two Januaries of employment[6]. Temporary contracts probably also tend to last until the end of the calendar year, and hence, January is associated with an increased inflow into unemployment for reasons not necessarily related to the increase in the replacement rate. Controlling for calendar time, and especially the January months, is thus not only done to increase precision of the estimated model, but also to be able to distinguish the effect of the increased replacement rate from a general January effect.

[6] At the first turn of the year the replacement rata changes from zero to $\frac{13-E_i}{12} \times 62.4$ %, and at the second turn of the year it increases further to 62.4 %. At the third turn of the year the replacement rate is kept constant.

The model is assumed to be proportional. Every explanatory variable has the same relative effect on the hazard function, irrespective of the values of the other explanatory variables, including duration. This assumption is crucial for the distribution of unobserved heterogeneity to be identified (Gaure, Røed, and Zhang, 2007). It is also vital for the causal interpretation of the parameter attached to the replacement rate variable. Identification is based on the dependence between the development of the hazard rate and the development in the replacement rate. If the exit rate into unemployment increases when the replacement rate increases, keeping other variables constant, this will be interpreted as a causal effect from the replacement rate on the hazard into unemployment. The identifying assumption is that the underlying duration dependence is independent of starting month. The shapes of the hazard functions would have been equal for employment spells starting in different months, if their levels of replacement rates were equal.

The distribution of the replacement rate variable, rr_i, is double peaked. Around 34 % of the monthly observations have a replacement rate equal to zero, and approximately the same share has a rate equal to the maximum; 62.4 %. The smallest positive replacement rate is 20.8 %[7]. Much of the sample variation in the replacement rate is thus on the extensive margin: being eligible to UI benefits, vs. not being eligible to UI benefits. The fact that eligibility affects entry into the unemployment register is shown in the two other chapters in this dissertation. It seems to be strong, partly because UI eligibility strongly increases the incentives to register as unemployed *given unemployment*, i.e. a pure registration effect. Variation in the replacement rate on the intensive margin probably also has an effect on registration, but to a smaller extent than variation on the extensive margin. To isolate the intensive margin from the extensive margin, in some specifications of the model a dummy taking the value 1 if $rr_i = 0$ is included in the regression

As discussed above, there are also reasons to believe that the effects of the replacement rate on the hazard into unemployment might be heterogeneous. In some versions of the model, this is allowed for by including an interaction between replacement rate and other explanatory variables.

[7] This is caused by the fact that individuals that start during October, November and December are assumed not to have fulfilled the minimum income requirements for eligibility during their first year as unemployed.

6. Results

Let us start by studying the sample of newly educated. Individuals that enter employment during May, June and July have very high probabilities of entering unemployment during the first 4 months of their employment spell compared to the rest of the sample. This might be caused by a high frequency of seasonal first jobs in this group. A person whose first job is a seasonal job is likely to have an underlying duration dependence in the hazard to unemployment that is different from a person entering a non-seasonal job, and the identifying assumptions of the model no longer hold. For this reason, May, June and July entrants are excluded from the sample of newly educated.

Regression 1 in Table 4 reports the key results from the estimation of equation (3). I report the relative effect on the hazard from employment to unemployment by a 1 percentage point increase in the replacement rate, measured in percent[8]. The results thus indicate that in increase in the replacement rate of 1 pp increases the hazard rate to unemployment by 0.537 %. The result is significant at a 99 % level.

Table 4: Results of the estimation of equation 3 on the sample of newly educated

	Regression 1		Regression 2		Regression 3		Regression 4	
	Effect	z-value	Effect	z-value	Effect	z-value	Effect	z-value
Replacement rate (RR)	0.537***	2.948	0.454**	2.375	0.361	1.493	0.310	1.147
RR×female			-0.043	-0.378			-0.064	-0.295
RR×small company			0.732***	4.911			0.345	1.156
RR×high unempl. experience			-0.207	-1.129			-0.099	-0.261
RR=0					-12.603	-1.406	-8.015	-0.721
RR=0×female							-1.588	-0.132
RR=0×small company							-22.254	-1.484
RR= 0 ×high unempl. experience							7.247	0.330
Number of spells	51864							

Note: Effects of a 1 percentage point increase in the replacement rate, on the continuous underlying hazard, measured in percent. Controlling for calendar time, starting month, employment spell duration, demographic variables, and background variables. Full sets of coefficients for regression 4 are reported in Table 6 in Appendix A.
* p<=0.1 , **p<=0.05, ***p<=0.01

In regression 2, the replacement rate is interacted with dummy variables for gender, firm size and previous experience with unemployment. This implies that the coefficient on RR now measure

[8] Let ψ be the coefficient attached to the replacement rate variable. The numbers in Table 4 and Table 5 are obtained by the following formula: $100 \times (\exp(\psi) - 1)$

the impact estimate for a male working in a firm with more than 10 employees, who in the last five years prior to employment entry has spent less than 7 months in the unemployment register. We see that the effect estimated for this reference person is slightly reduced, but it is still significantly different from zero. Workers in the smallest firms seem to respond more strongly to changes in the replacement rate than other workers. This is consistent with tight employer-employee ties making it easier to exploit the system of unemployment insurance. It could also indicate that tight bonds between employers and workers make employers more reluctant to lay off workers with no or low replacement rates. There does not seem to be any difference between the genders in the effect of the replacement rate. This is quite surprising given the existing literature on the field, which do indicate that the genders differ on their response to economic incentives in the welfare system. The standard error of the estimate is, however quite large, and thus this finding might simply be resulting from lack of statistical power. There is no sign of workers with high unemployment experience being more sensitive to the incentives. The point estimate of the interaction term is in fact negative. The hypothesis that experience with the unemployment system leads to knowledge to the institutional details, and hence ability to act upon their inherent economic incentives, is therefore not confirmed. On the other hand, selection mechanisms might be involved. Prior experiences in the unemployment register, possibly with no, or very low UI entitlements, might be correlated with strong motivation to work. This selection effect probably hides the potential effect of learning the details of the UI system.

In regression 3, we go back to assuming homogeneous effects of the replacement rate, but expand the model by introducing a dummy indicating that the replacement rate is equal to zero. This means that the replacement rate variable now capture only variation on the intensive margin. We see, as we would expect, that the point estimate of the effect of not being eligible to UI is negative, and that the point estimate of the effect of the replacement rate is reduced. None of the variables related to UI benefits are significant in this specification though. In regression 4, I interact dummy variables for gender, firm size and unemployment experience with both the replacement rate and the dummy indicating that the replacement rate is zero. Thus heterogeneous responses to variation in the replacement rate both on the extensive and the intensive margin is allowed for. None of the estimates related to UI variables are statistically significant in this specification either. The point estimates could indicate though, that workers in small firms

respond more strongly to changes in the replacement rate than others, especially on the extensive margin.

The results of estimating the same models on the sample of immigrants are reported in Table 5. Regression 5 shows approximately the same point estimate as when the same model is estimated on the sample of newly educated persons. (See Regression 1 in Table 4) A 1 percentage point increase in the replacement rate increases the hazard into registered unemployment by 0.582 %.

Table 5: Results of the estimation of variations of equation 3 on the sample of immigrants

	Regression 5		Regression 6		Regression 7		Regression 8	
	Effect	z-value	Effect	z-value	Effect	z-value	Effect	z-value
Replacement rate (RR)	0.582**	2.531	0.350	1.425	-0.488	-1.134	-0.388	-0.952
RR×female			0.023	0.142			-0.074	-0.176
RR×small company			0.906***	4.875			1.267**	2.439
RR×high unempl.experience			-0.702**	-2.137			0.167	0.187
RR=0					-46.992***	-3.252	-31.996**	-2.051
RR=0×female							-10.778	-0.480
RR=0×small company							11.936	0.385
RR= 0 ×high unempl. experience							99.111	1.427
Number of spells	20912							

Note: Effects of a 1 percentage point increase in the replacement rate, on the continuous underlying hazard, measured in percent. Controlling for calendar time, starting month, employment spell duration, demographic variables, and background variables. Full sets of coefficients for regression 8 are reported in Table 6 in Appendix A.
* p<=0.1 , **p<=0.05, ***p<=0.01

When I allow for heterogeneous responses to changes in the replacement rate, in Regression 6, we see as in the newly educated sample, that workers in small firms respond more strongly also in this sample. Workers with high unemployment experience have a significantly weaker response than the rest of the sample. That finding might, as in the previous sample, be driven by selection. In fact, the selection effect is likely to be stronger in the immigrant sample than in the sample of newly educated, and that might explain why the effect is significantly negative in this sample. As explained above, in this group, prior experience in the unemployment registers must have occurred after their time of entry into Norway, and prior to their first employment spell, i.e. the spell analysed her. This means that the unemployment spell was uninsured. This is in contrast to the sample of newly educated, where unemployment spells might have been insured if they occurred prior to their education spell. Experience with registered unemployment is therefore more strongly associated with motivation for work in the immigrant sample than in the education

sample. All in all, due to strong selection effects, this analysis is probably not able to identify any effect of learning of institutional details and or changed stigma costs.

When the intensive and extensive margin is separated (see regression 7), the point estimate of the effect of the replacement rate actually turns out to be negative. The standard error is large though. The point estimate of the effect of having no UI eligibility is negative, and significant. Thus, the significant result in Regression 5 seems to be driven merely by variation at the extensive margin. When allowing for heterogeneity in treatment effects of variation in the replacement rate both on the extensive and intensive margin, we get the results reported in Regression 8. Not being eligible to UI still reduces inflow into the unemployment register significantly in the immigrant sample. The effect of not being eligible for UI seems to be homogeneous among the subgroups studied. No significant effect of variation in the replacement rate on the intensive margin is found in the reference group, but workers in small firms' responds significantly stronger than others to a small increase in the replacement rate. This could indicate that there is actually an effect of continuous variation in the replacement rate among workers in small firms. However, the sum of the coefficients attached to the variables RR and RRxSmallfirm is not statistically significant at conventional levels. This sum of coefficients has a z-value equal to 1.5224.[9] It is statistically significant on a 13 percent level.

7. Conclusion

This chapter investigates the causal relationship between the unemployment insurance replacement rate and unemployment entry, using register data. A particular Norwegian institutional feature creates a quasi-experimental setting. Variation in the replacement rate that is likely to be unrelated to other unobserved factors also affecting job duration and unemployment entry, exist among newcomers in the labor market.

Samples of newly educated persons and immigrants from outside of the European Union are studied to test the effect of the replacement rate on entry into unemployment.

[9] We see from Table 6 in Appendix A that the coefficient attached to RR is -0.0039. The coefficient attached to RRxSmallfirm is 0.0126. The sum of them equals 0.0087. The standard error of this sum of coefficient is obtained by using the formula for the variance of sum of variables: $var(x+y) = var(x) + var(y) + 2cov(x,y)$. The estimate of the covariance of the coefficients is -5.29× 10^{-6} (not reported elsewhere in this paper). This implies a standard error of the sum of coefficients equal to 0.0057.

In the simplest models, I find significant and positive effects of the replacement rate on the transition rate to unemployment in both samples. This confirms the findings in the literature on this topic that is based on American survey data. I also distinguish between the effects of variation in the replacement rate on the intensive margin (changes in replacement rate *given* that the replacement rate is positive) and variation on the extensive margin (having, versus not having a positive replacement rate) by including a dummy variable indicating eligibility. The result form this expansion of the model is that variation on the intensive margin in general no longer affects the hazard into unemployment significantly. The fact that variation in the replacement rate on the extensive margin still has a significant effect in the immigrant sample confirms the findings of Jurajda (2002): UI Eligibility matters, benefit level is less important, at least within the range of variation observed in the data used in paper (from around 0.20 to 0.624)

Employees in the smallest enterprises might seem to be more sensitive to the replacement rate. The ties between the employers and the employees are likely to be tighter in very small firms, and the decisions involved in the destruction of a job match might take the form of a dialogue. The employer in a small firm might be more willing to confirm that a job separation is a layoff, and thereby help the worker to avoid the 8 weeks sanctions that he will face without this confirmation. He might also know (and care) more about the financial situations of his employees, and for altruistic reasons avoid laying off workers with no or very low UI entitlements. The fact that unemployment entries labelled as layoffs seem, according to the literature, to be affected by UI features, while entries labelled as quits are not, might thus just reflect that the quit-versus-layoff-decisions are made through dialogues between employers and employees. It is important, though, to keep in mind that since the levels, causes, and effects of unemployment seem to be different among newcomers than among workers with more tenure and experience, we might not be able to generalized the results to the general working population.

Appendix A

Table 6: Full sets of coefficients of Regression 4 and 8

	Regression 4 Education sample		Regression8 Immigrant sample	
	Coefficient	Standard error	Coefficient	Standard error
Replacement rate (RR)	0.0031	0.0027	-0.0039	0.0041
RR=0	-0.0835	0.1159	-0.3856**	0.1880
RR×Female	-0.0006	0.0022	-0.0007	0.0042
RR×Small firm (L≤10)	0.0034	0.0030	0.0126**	0.0052
RR×High unep.experi.	-0.0010	0.0038	0.0017	0.0089
RR=0×Female	-0.0160	0.1210	-0.1140	0.2375
RR=0×Small firm	-0.2517	0.1696	0.1128	0.2930
RR=0×High unemp.exp	0.0700	0.2117	0.6887	0.4825
Public sector	-0.2047***	0.0544	-1.1916***	0.1412
Western immigrant	0.0795	0.0585		
Non-western immigrant	0.1830***	0.0666	1.8267***	0.1946
Female	-0.0184	0.1134	-0.3354	0.2286
Age<30	-0.4537***	0.0688	0.0461	0.0790
35≤Age≤39	0.2860**	0.1177	0.2463***	0.0937
40≤Age≤49	0.5780***	0.1417	0.3820***	0.1015
50≤Age≤60	0.9283***	0.2357	0.4634***	0.1721
Firm size:				
L≤10	0.2069	0.1626	-0.1866	0.2885
$11 \leq L \leq 49$	0.0580	0.0569	0.4757***	0.1043
$50 \leq L \leq 199$	0.0791	0.0630	0.0565	0.1078
$200 \leq L \leq 499$	-0.0066	0.0733	0.1468	0.1370
Family situation:				
Single, children	-0.0694	0.0650	0.9137***	0.2852
Couple, no children	-0.0609	0.1211	0.0335	0.1266
Couple, children	-0.5231***	0.1002	0.2513*	0.1376
Calendar month:				
2004-3	2.4967***	0.9410	4.4854***	0.9596
2004-4	1.1479	1.3303	2.0390*	1.1159
2004-5	0.5710	1.0876	2.4621***	0.8540
2004-6	0.7323	1.1949	1.2375	0.9949
2004-7	0.0000	0.0000	0.0000	0.0000
2004-8	1.6686**	0.7678	1.1026*	0.6309
2004-9	0.8930***	0.2293	1.3160**	0.5325
2004-10	0.5455**	0.2181	0.9721*	0.5333
2004-11	0.1344	0.2366	0.9156*	0.4826
2004-12	-0.3470	0.2671	0.1730	0.5958
2005-1	0.8471***	0.1794	1.3122***	0.3513
2005-2	0.3494*	0.1951	0.0776	0.4811
2005-3	-0.3941	0.2489	-0.4729	0.5532
2005-4	-0.2287	0.2357	0.0549	0.4143
2005-5	-0.4130	0.2557	0.0925	0.4342
2005-6	-0.9570***	0.3250	-0.2625	0.4807
2005-7	-0.2971	0.2453	0.2383	0.3576
2005-8	0.6641***	0.1776	0.3269	0.3317
2005-9	0.3581**	0.1664	-0.2916	0.3949
2005-10	-0.3349*	0.1903	-0.6194	0.4443
2005-11	-0.5452***	0.1949	0.4029	0.3074
2005-12	-0.6212***	0.1922	-0.0619	0.3590
2006-1	0.2271	0.1501	0.7886***	0.2596

2006-2	-0.4166**	0.1695	-0.0841	0.3146
2006-3	-0.8372***	0.1917	0.2881	0.2792
2006-4	-1.1549***	0.2199	0.4036	0.2774
2006-5	-0.8765***	0.1981	0.0599	0.3060
2006-6	-1.2314***	0.2306	-0.6844*	0.3855
2006-7	-0.7827***	0.1938	-0.3787	0.3199
2006-8	-0.2027	0.1619	-0.4737	0.3255
2006-9	-0.6757***	0.1740	-0.0283	0.2722
2006-10	-0.7994***	0.1734	0.1329	0.2544
2006-11	-1.0871***	0.1856	-0.0017	0.2689
2006-12	-1.1466***	0.1865	-0.8286*	0.3591
2007-1	-0.4134***	0.1489	0.2169	0.2388
2007-2	-0.7590***	0.1552	0.1433	0.2372
2007-3	-1.2104***	0.1775	-0.9862***	0.3147
2007-4	-1.0200***	0.1707	-0.4223	0.2638
2007-5	-1.5210***	0.2035	-0.3504	0.2545
2007-6	-1.4088***	0.2013	-0.5365**	0.2627
2007-7	-1.0111***	0.1739	-0.8803***	0.2919
2007-8	-0.6845***	0.1579	-0.6439**	0.2508
2007-9	-0.7646***	0.1563	-0.2867	0.2303
2007-10	-0.8144***	0.1566	-0.5163**	0.2397
2007-11	-1.4191***	0.1853	-0.9376***	0.2718
2007-12	-1.8725***	0.2146	-1.2201***	0.2909
2008-1	-0.6967***	0.1483	-0.2617	0.2155
2008-2	-1.0292***	0.1537	-0.2921	0.2132
2008-3	-1.2184***	0.1624	-0.9047***	0.2427
2008-4	-1.3278***	0.1735	-0.4175*	0.2149
2008-5	-1.6323***	0.1909	-0.9534***	0.2410
2008-6	-1.5754***	0.1917	-1.1441***	0.2504
2008-7	-1.5438***	0.1867	-0.8533***	0.2270
2008-8	-0.9544***	0.1583	-0.9251***	0.2252
2008-9	-0.8501***	0.1497	-0.4441**	0.2029
2008-10	-0.9996***	0.1552	-0.4463**	0.2063
2008-11	-0.6084***	0.1422	-0.1567	0.1936
2008-12	-0.8980***	0.1514	-0.2778	0.2001
2009-1	0.0724	0.1259	0.3321*	0.1781
2009-2	-0.2635**	0.1309	0.3620**	0.1769
2009-3	-0.0562	0.1266	0.5705***	0.1690
2009-4	-0.2872**	0.1347	0.1772	0.1800
2009-5	-0.4182***	0.1395	0.1856	0.1794
2009-6	-0.4494***	0.1438	0.1623	0.1795
2009-7	-0.6255***	0.1480	-0.1063	0.1912
2009-8	-0.2908**	0.1368	-0.2269	0.1932
2009-9	-0.1598	0.1334	0.4290**	0.1700
2009-10	-0.4365***	0.1427	0.2786	0.1772
2009-11	-0.6316***	0.1529	0.2893	0.1793
2009-12	-0.7595***	0.1563	-0.3854*	0.2104
2010-1	0.1375	0.1270	0.4366**	0.1714
2010-2	-0.2184	0.1344	0.3931**	0.1733
2010-3	-0.3897***	0.1407	0.3362*	0.1737
2010-4	-0.2452*	0.1399	-0.0481	0.1902
2010-5	-0.8424***	0.1658	-0.1097	0.1944
2010-6	-0.5371***	0.1546	0.1609	0.1780
2010-7	-0.8126***	0.1638	-0.3412*	0.2013
2010-9	-0.2846**	0.1411	-0.2337	0.1943
2010-10	-0.6144***	0.1565	-0.1149	0.1911
2010-11	-0.7404***	0.1623	-0.1718	0.1945
2010-12	-1.0142***	0.1731	-0.2782	0.2028
Region of residence				

Southern	0.3006***	0.0778	0.3845	0.2480
Western	0.0586	0.0447	0.1458	0.1205
Mid	0.3492***	0.0650	0.4150**	0.2094
North	0.4075***	0.0610	1.2154***	0.1794
Unemployment experience				
Medium	0.7723***	0.0541	1.8287***	0.1514
High	1.1115***	0.2089	1.1871**	0.4842
Entry month:				
Jan			-0.8286***	0.1516
Feb	0.1945**	0.0914	-0.6593***	0.1756
Mar	0.3286***	0.1069	-0.5225***	0.1797
Apr	0.3048***	0.1097	-0.4421**	0.1786
May			-0.1624	0.1664
Jun				
Jul			0.1084	0.1635
Aug	0.1646***	0.0613	-0.4762***	0.1671
Sep	0.1958***	0.0737	-0.5370***	0.1686
Oct	0.4177***	0.0764	-0.2490	0.1895
Nov	0.3197***	0.0867	-0.2208	0.1941
Dec	0.3612***	0.0922	0.3958*	0.2113
Previous tenure in labor market:				
1 to 6 years	-0.1707***	0.0469		
6 to 10 years	-0.4642***	0.1160		
11 to 20 years	-0.6790***	0.1435		
More than 20 years	-0.6641***	0.2169		
Education:				
No education			-0.0690	0.1604
Compulsory schooling	0.6928***	0.0583	0.7315***	0.1361
Higher education	-1.1901***	0.0684	-0.5661***	0.1237
Education unknown	-0.1159	0.6465	0.0349	0.1104
Spell duration month:				
Duration=3	0.1589**	0.0809	0.5788***	0.1782
Duration=4	0.1370	0.0932	0.9005***	0.2172
Duration=5	0.1762*	0.1023	1.1660***	0.2406
Duration=6	0.0134	0.1110	1.3042***	0.2665
Duration=7	0.1833	0.1175	1.4937***	0.2874
Duration=8	-0.0317	0.1288	1.6677***	0.3045
Duration=9	-0.0103	0.1338	1.6722***	0.3213
Duration=10	-0.0431	0.1436	1.7002***	0.3359
Duration=11	0.0887	0.1468	1.5236***	0.3477
Duration=12	0.1182	0.1535	1.7356***	0.3545
Duration=13	0.2084	0.1569	1.8228***	0.3657
Duration=14	0.0097	0.1647	1.7070***	0.3795
Duration=15	-0.1425	0.1800	1.9168***	0.3893
Duration=16	-0.1774	0.1882	1.7678***	0.3969
Duration=17	0.0527	0.1912	2.1314***	0.4046
Duration=18	-0.2268	0.1968	2.1665***	0.4153
Duration=19	0.0383	0.1988	2.0748***	0.4263
Duration=20	0.0080	0.2033	2.0553***	0.4362
Duration=21	-0.1933	0.2149	1.9967***	0.4501
Duration=22	-0.1835	0.2249	2.1227***	0.4499
Duration=23	-0.4892**	0.2334	2.1382***	0.4602
Duration=24	-0.1041	0.2238	2.0532***	0.4665
Duration=25	-0.2375	0.2203	2.4110***	0.4606
Duration=26	-0.2196	0.2240	2.0197***	0.4787
Duration=27	-0.5132**	0.2349	2.4024***	0.4768
Duration=28	-0.5792**	0.2449	2.4348***	0.4860
Duration=29	-0.5096**	0.2459	1.9373***	0.5070

Duration=30	-0.4898**	0.2328	2.4046***	0.4957
Duration=31	-0.1776	0.2322	2.1117***	0.5138
Duration=32	-0.5208**	0.2471	2.6026***	0.5027
Duration=33	-0.4973*	0.2541	2.4682***	0.5111
Duration=34	-0.4304*	0.2597	2.1232***	0.5355
Duration=35	-0.5491**	0.2638	2.2494***	0.5338
Duration=36	-0.3380	0.2595	2.2811***	0.5399
Support points for the distribution of unobserved heterogeneity				
	Value	Prob.	Value	Prob.
	-5.519	0.723	-10.969	0.538
	-3.375	0.241	-7.724	0.313
	-1.283	0.036	-5.542	0.125
			-2.819	0.022
			0.928	0.003
Number of spells	20 912		51 864	

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