When designing labor market policy, policymakers are in need of empirical and causal evidence of the effectiveness of their policies. This thesis consists of four empirical studies to evaluate the causal effects of various types of labor market policies in the Netherlands. Chapter two studies the effects of the length of the Unemployment Insurance entitlement period on job finding and post-unemployment outcomes. The third chapter exploits the removal of Disability Insurance (DI) experience rating for small firms to estimate the effects of experience rating on the DI inflow and outflow. Next, the fourth chapter investigates whether DI experience rating causes financial distress amongst firms or triggered firms to reduce labor costs by increasing layoffs. Finally, chapter five uses a large-scaled randomized experiment to study the effects of a job search program for older unemployed workers.

Nynke de Groot (1984) graduated from the Master in Econometrics at the University of Amsterdam in 2008. After working at a research company for several years, Nynke returned to academia to write her PhD thesis at the Vrije Universiteit in Amsterdam. She is currently working as a researcher at the Vrije Universiteit.

Evaluating labor market policy in the Netherlands Nynke de Groot

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Evaluating labor market policy in the Netherlands

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promotoren: prof.dr. P.W.C. Koning
prof.dr. B. van der Klaauw

Acknowledgements

When I was a child, I wanted to become a doctor. Obviously, not a doctor in economics, but the type of doctor with a white coat who helps people get better. But by the time I had to decide which subjects to take in high school, I realized that being a doctor with a white coat not only meant helping people, but also entailed a lot of less attractive things like blood, long surgeries and patients who do not always get better. After losing my career plan, I had no idea which direction to go. So, I decided to just follow the subjects I liked best and see where I would end up. This has been my strategy ever since. And now, after years of just doing what I like, it seems that somehow I did accomplish my childhood goal of becoming a doctor and maybe even – although perhaps I am being too optimistic – helped some people.

Many people contributed to fulfilling my childhood dream and I would like to take the opportunity to thank all of you now. First and foremost, I would like to thank my supervisors Pierre Koning and Bas van der Klaauw for your advice and support when writing this thesis. I was lucky to have you both as a supervisor, together you made a great team. Pierre, thank you for considering me to become your first PhD-student ever. You were so supportive and always made time for me, I hope you will stay as involved with all your next PhD-students as you were with me. I learned a lot from you and your enthusiasm about social assistance was inspiring. And, perhaps equally important, I really enjoyed working with you.

It was a course in Applied Microeconometrics taught by Bas and Maarten Lindeboom which inspired me to start writing academic papers. Although I spent four years at the UvA learning everything about fancy econometric methods, including all the required assumptions and asymptotic properties, during the course I discovered a whole new world of simple, practical tools and the cool research applying these methods. After the course, I told Bas I was thinking about writing a PhD thesis, and he immediately agreed on writing a paper together. This turned out to be chapter two of this thesis. Bas, thank you for introducing this other – much more fun – side of econometrics to me and for everything you have taught me about econometrics ever since. I am grateful for the trust you put in me when organizing the experiment of chapter five, and I knew I could always count on you if needed. It was fun to work together and you were always available for stupid questions - your words - although most of my questions ended in conversations about running, your kids or my renovation.

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My interest in labor economics was sparked when I was working at APE. During the various research projects with Leo and Philip, I learned about the Dutch institutions and the issues policymakers are faced with. Although life at APE was busy and there were always approaching deadlines around the corner, I was encouraged to work on academic papers and developing skills which were not immediately relevant for my work as a consultant. I am grateful for the opportunities Leo, Peter, Philip and Rene have given me.

During my time at the VU I had the luck of having great office mates. On my first morning at the VU, Sandra was the only one around at the early hour so I took a desk in her office. This turned out to be a great decision, we have spend hours chatting about work-related and less work-related stuff. After the move I shared an open office with Xiaoming. I did not really know him at first, but I soon learned that he was a really friendly and funny guy. It was an honor to be a paranymph for both of you. I now share an office with Sándor, an obvious choice as an office mate considering we were already running buddies and have so much in common. You have been so helpful over the years, I am glad you agreed to be my paranymph. Besides my office mates, I would like to thank several colleagues at the VU for all the coffee breaks, lunches, running events and kitchen conversations. I especially want to thank Nadine, Lisette, Paul, Mathilde, Gosia and Jonneke, the VU was definitely less fun after you left. Pieter, Sabien, Maarten, Wim, Wouter and Diana, I am happy that you are still around! I would also like to thank the Work in Progress group for all their suggestions on the chapters in this thesis, and for the fun during drinks at TI and dinners.

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Nynke de Groot
Zaandijk, December 2017

Contents

CHAPTER 1

Introduction

Governments provide benefits to insure individuals against the risk of losing their income due to disability or unemployment. These benefits can amount to a substantial share of the governmental expenditures. In 2015, the Dutch government spent over 18 billion euro – or roughly 2,200 euro per employed worker – on unemployment and disability insurance benefits (CBS, 2017).

Policymakers can use several types of labor market policy instruments to decrease the costs of providing benefits. When deciding which of these instruments to implement, policymakers are in need of empirical and causal evidence of the effectiveness of their policies. Over the last decades, economic science experienced a shift from mostly theoretical studies towards empirical studies using large-scaled administrative datasets. At the same time, an increasing number of empirical studies - especially in the field of labor economics - uses (quasi-) experimental research designs which yield credible and causal results, also known as the "credibility revolution" (Angrist and Pischke (2010)). These two movements combined allows labor economists to answer long-standing research questions and to provide policymakers with empirical evidence of their policies.

This thesis consists of four empirical studies using natural and randomized experimental variation to evaluate the causal effects of labor market policy in the Netherlands. The second chapter discusses the design of the Unemployment Insurance (UI) benefit scheme. More specifically, it studies the effects of the length of the entitlement period to UI benefits. Policymakers face a trade-off when deciding on the length of the UI entitlement period. Providing benefits for a long period can cause moral hazard problems. Unemployed workers can become too selective in which job offers to accept or search less actively for a job. This could result in longer use of the benefits. On the other hand, shorter entitlement to benefits means that job seekers have less time to search for a suitable job. Therefore, the quality of the match between the job seeker and the new job might by worse.

To estimate the effects of the UI entitlement period on job finding and subsequent job quality, chapter two uses two identification strategies. First, a substantial reform in the Dutch UI law in October 2006 is exploited. The reform reduced the shortest entitlement period from six to three months, and the longest entitlement period from 60 to 38 months. Although the reform reduced the entitlement period for the majority of workers, for some workers the entitlement period did not change or even slightly increased. This variation in the size of the change in entitlement period allows the use of a difference-in-difference-analysis in which calendar time effects are separated from effects of the changed UI entitlement period. The second identification strategy is based on the calculation of the UI entitlement period before the reform. Before October 2006, the UI entitlement period was a step function of labor market history - which is a combination of age and employment history. Being born just before or after January 1 could change the UI entitlement period by up to 12 months. This allows the use of a regression discontinuity design in which the labor market outcomes of job seekers born just before the entitlement threshold are compared to job seekers born just after the threshold.

The estimation results from the difference-in-difference analysis and the regression discontinuity approach concur. The results show that reducing the entitlement period to UI benefits increases job finding rates. A shorter entitlement period decreases the cumulative UI payments, but this is more than fully compensated by the additional earnings from work due to the higher job finding rates. With respect to post-unemployment job quality, the estimates suggest that reducing the entitlement period has at most modest effects on post-unemployment wages, but also that workers leave unemployment more often because of accepting a temporary job. These results imply that unemployed workers lower their job demands when faced with a shorter UI entitlement period. Because reducing the entitlement period also increases job turnover, in the long run individuals are more likely to have a permanent contract if the UI entitlement period is reduced.

Policymakers can also use incentives targeted at employers to reduce benefits costs. An example of such an incentive is experience rating. Experience rating means that employers pay a premium that reflects the benefit costs of their (former) workers. In other words, if the workers of a firm face a relatively high risk of becoming unemployed or disabled, the firm pays a higher premium. The aim of the experience-rated DI premiums is to increase employer awareness of the costs of DI and as such stimulate employers to prevent DI inflow and incentivize DI outflow. Chapter three studies whether experience rating in the Netherlands has an effect on DI inflow and outflow. In order to estimate the causal effects of experience rating, it exploits the removal of experience rating for the group of small firms that took place in 2003. This allows for the use of a difference-in-difference analysis, with large firms as a control group for which the experience-rating incentive did not change.

The results from the study in chapter three show that the removal of DI experience rating for small firms in 2003 and 2004 increased the inflow into DI and reduced the outflow from DI of the workers from those firms. This is in line with economic predictions. As to DI outflow, the effects are confined to partially disabled workers only and occur in the first year of DI. However, results from additional analyses suggest that the removal of experience rating did not affect DI inflow or DI outflow in the years after 2005. Two important reforms took place in 2005 and 2006 which could explain this change in the effectiveness of DI experience rating. In 2005, the sickness period that precedes DI benefit receipt – and for which firms are financially responsible – was extended from one to two years. In addition two new types of DI benefits were introduced in 2006: one for workers who were permanently and fully disabled, and one for those who are partially and/or temporarily disabled. Experience rating only applied to new partially and/or temporarily disabled individuals. Although both reforms substantially reduced the inflow into DI and the coverage of experience rating, the study suggests that the decrease in the impact of experience rating after 2005 can be largely attributed to the extension of sick leave benefits from one to two years.

Despite the fact that several studies – including the study described in chapter three – find that DI experience rating can reduce the costs of public DI, so far the Netherlands and Finland are the only countries with experience-rated DI premiums. Policymakers argue that with DI experience rating, firms have to bear risks which are beyond their control, in particular benefit costs stemming from non-occupational injuries. In effect, financial risks arising from experience rating may increase the likelihood of financial distress.

Chapter four investigates whether large increases in the DI premium cause financial distress amongst firms or trigger firms to reduce labor costs by increasing layoffs. More specifically, it estimates the effect of a positive premium adjustment due to experience rating on the probability of a firm exit, the inflow into UI and the inflow into other non-experience-rated DI benefits.

The key challenge in this chapter is to separate the effects of the DI risk of a firm from the effects of the experience-rated premium. This is achieved by exploiting specific features of the Dutch DI experience-rating system which are exogenous to the firm. These features, such as the removal of experience rating for small firms between 2003 and 2007 and the existence of premium caps, create substantial year-to-year changes in the mapping of disability risks to premiums within and between firms.

The estimation results suggest that a positive premium adjustment increases the probability of a firm exit. This effect consists both of increases in firm bankruptcies and increases in mergers of firms. The latter finding points at strategic behavior amongst firms, as merging firms lose the benefit costs of former workers that were initially assigned to them. Firms also respond to increases in the experience-rated premium by increased lay-offs. This effect is largely confined to workers entering the UI scheme after a firm exit. There is no evidence of substitution effects of the non-experience-rated DI scheme for permanent and full disability.

Chapter five evaluates an active labor market policy in the Netherlands. Active labor market policies (ALMP) are governmental programs aimed at improving the employment probabilities of (disadvantaged) individuals. ALMP comprise a broad variety of programs, such as job seach assistance, hiring subsidies or job search monitoring. Chapter five studies the effects of a job search program for older unemployed workers in the Netherlands. Older job seekers have a relatively large risk of becoming long-term unemployed. Although they are not more likely to lose their job compared to their younger counterparts, once they become unemployed it is more difficult for them to find a new job and they are more likely to suffer wage losses in their new job. The Dutch job search assistance program called "Successfully To Employment Program" (STEP) aims to increase the job finding probabilities of job seekers aged 50 and older by improving job search and networking skills.

The effects of STEP are estimated using a large-scaled randomized controlled trial (RCT). Out of a sample of 50,000 older job seekers, 20% of the sample was randomly placed in the control group and should not be invited to STEP, while the job seekers in the treatment group were invited to STEP after three months of UI benefit receipt. Roughly half of the job seekers in the treatment group eventually participated in STEP, whereas about 8% of the control group participated. Since the only difference between the treatment and control group is the invitation to STEP, any differences in UI outflow, job finding rates or other labor market outcomes can be attributed to the difference in the participation rate of STEP.

The results in chapter five indicate that participating in STEP has positive effects on the outflow from UI and job finding. Participation in STEP increases the exit rate from UI in the first year of UI benefit receipt with 4.4 percentage points. Most of these additional exits from UI are to employment. Eighteen months after UI, the positive effects of STEP on UI outflow persist, while the effects on job finding disappear. This implies that STEP leads to faster job finding instead of additional job finding. Because participants are more likely to exit UI, their received cumulative UI benefits are also lower. This reduction in UI benefits increases over time and exceeds the costs of STEP after fourteen months since UI inflow. The total income of older unemployed workers is not affected by participation in STEP because the reduction in UI benefits is substituted by an increase in earned wage from employment. There is no evidence that STEP affects the quality of the first job after unemployment.

Finally, the sixth chapter provides a brief summary of the four chapters in this thesis and concludes.

CHAPTER 2

The Effects of Reducing the Entitlement Period to Unemployment Insurance Benefits.*

2.1 Introduction

In most continental European countries the welfare state expanded until the early 1990s. Since then the generosity of benefits schemes has been reduced gradually. But compared to other OECD countries, continental European countries still provide generous benefits (e.g. Immervoll and Richardson (2011)). The Netherlands is no exception. Until 2006, the entitlement period to unemployment insurance (UI) benefits could be up to five years and most workers received 70 percent of their last earned gross wage during this period.

Providing benefits for inactivity causes moral hazard problems. Unemployed workers may exert too little effort to find work or become more selective in which job offer to accept. Being selective is not always bad. For example, unemployment benefits act as a search subsidy, i.e. individuals can financially survive without work and are not forced to immediately start working in the first available job, which might be ill-suited for them. In a system with generous benefits, the quality of the match between worker and job may, therefore, be better.

*This chapter is based on De Groot and Van der Klaauw (2017a).

In this chapter we study the effects of the length of entitlement to UI benefits on the exit rate from unemployment and on subsequent labor market outcomes. We adopt two identification strategies. First, we exploit a substantial reform in the Dutch UI law in October 2006. Both before and after the reform the length of the entitlement period depended on the individual labor market history, which is a function of age and the number of years employed. The reform reduced the shortest entitlement period from six to three months, and the longest entitlement period from 60 to 38 months. For some workers the entitlement period did not change or even slightly increased. This allows us to separate calendar time effects from the effects of the changed UI entitlement period. Second, we use that before the reform of October 2006, the UI entitlement period was a step function of labor market history. In a regression discontinuity design we exploit that the actual age on January 1 1998 determines the age component in the labor market history. Being born just before or after January 1 can change the UI entitlement period by up to twelve months.

Job search theory predicts that the duration of unemployment increases when the benefits entitlement period is extended. Empirical evidence confirms this prediction. Van Ours and Vodopivec (2006) exploit changes in the Slovenian UI system and show that reducing the entitlement period increases the exit rate to work and to other destinations. Lalive (2008) finds that for Austria extending the entitlement to benefits for 50 years old from 39 to 209 weeks reduces the job finding rate. Card and Levine (2000) find mixed evidence of an extended benefits program. A state level comparison shows that exit rates from the UI benefits scheme remain largely unaffected, but individual data show a significant reduction in exit rates. Schmieder et al. (2012) use discontinuities at ages 42, 44 and 49 in UI entitlement to show that the effect of the entitlement period on the unemployment duration does not vary over the business cycle.

Reduced job finding rates due to longer benefits entitlement are not necessarily bad, if the quality of worker-job matches increases. This is the case if a longer benefits entitlement period allows workers to be more selective. But if the longer entitlement period causes workers to be unemployed longer, skills depreciate more and job opportunities decline. It is an empirical question which of these countervailing forces prevails and at which margins. Card et al. (2007), Lalive (2007) and Bennmarker et al. (2013) do not find any effect on post-unemployment wages, while Schmieder et al. (2016) find that increasing the UI entitlement period decreases post-unemployment wages. In contrast, Centeno and Novo (2009), Cockx and Picchio (2013) and Nekoei and Weber (2017) find a positive but small effect of extending UI benefits on post-unemployment wages.

We make two contributions to the literature. First, whereas earlier studies only consider a specific margin where exogenous variation in the benefits entitlement period is generated, our UI reform and the pre-reform discontinuities in UI entitlement provide more variation along the full distribution of individuals.[1] This allows us to study more thoroughly for which individuals and at which moment the UI benefits entitlement period has an impact. Second, we follow individuals for several years after leaving the benefits system and the data contain many post-unemployment outcomes such as earnings, working hours, type of contract and sector. Therefore, we provide a more extensive analysis on the effect of the UI entitlement period on the match quality between the worker and her job.

In the empirical analysis, we use administrative data on all UI benefits spells which started between January 2004 and December 2008 in the Netherlands. This includes in total over 500,000 spells. We combine these data with other administrative datasets provided by Statistics Netherlands to observe demographic and socioeconomic characteristics as well as post-unemployment labor market outcomes. For the period until 2010 we observe earnings, working hours and type of contract in all jobs after unemployment. In addition, we observe eligibility and receipt of other types of benefits.

The estimation results from our difference-in-difference analysis and regression discontinuity approach concur. The empirical results support the earlier literature that reducing the entitlement period to UI benefits increases job finding rates. The job finding rates are already higher from the start of unemployment, but peak just before the end of UI benefits entitlement. Cumulative UI benefits payments are significantly lower, but this is more than fully compensated by additional earnings from work. The wage effects are modest, but workers leave unemployment more often because of accepting a temporary job. These results suggest that unemployed workers lower their reservation wages and job demands when faced with a shorter UI entitlement period. The long-run effects show increased job turnover, increasing the probability of having a permanent contract three or five years after becoming unemployed. Previous empirical studies exploit age discontinuities between 40 and 55. We find that these age groups are much more responsive than workers under

[1]For example, Nekoei and Weber (2017), Lalive (2008), and Bennmarker et al. (2013) study unemployed workers at the 40, 50 and 55-year old threshold respectively, while Schmieder et al. (2012) and Schmieder et al. (2016) study unemployed workers at the 42, 44 and 49-year threshold. Furthermore, Card et al. (2007) consider both extended severance payments for lay-offed workers who have worked at least three years at their previous employer and the discontinuity in the entitlement period around the threshold of 36 months of employment in the previous five years.

age 35. Finally, our results show that effects are smaller for individuals with long entitlement periods, which is in line with job search theory.

This chapter proceeds as follows. In the next section we provide some theoretical background. In section 2.3 we describe the Dutch UI system and the reform of October 2006. In section 2.4 we present our data. We discuss the effects of reducing the entitlement period on job finding and other labor market outcomes in sections 2.5 and 2.6, where we discuss the difference-in-difference and regression discontinuity approaches, respectively. In section 2.7 we explore the underlying job finding mechanism. Our conclusions are presented in section 2.8.

2.2 Job search theory and expected effects

Job search models describe the behavior of unemployed workers (e.g. Mortensen (1986), Van den Berg (1990)). Each period the unemployed worker decides to which vacancies to send a job application. Each job application can result in a job offer. Whether or not such a job offer is acceptable for the unemployed worker depends on the characteristics of the job and the worker's labor market prospects. For ease of exposition theoretical models often impose that jobs are characterized by the wage. In our empirical analysis we also consider other job characteristics as measures for the quality of a job.

Job search theory assumes that unemployed workers maximize the present value of their lifetime utility, where utility is a function of income and leisure. When all jobs are full-time, the job offer acceptance decision is based on a reservation wage strategy. Each period the unemployed worker chooses a reservation wage and accepts a job offer if the associated wage exceeds the reservation wage in that period. Furthermore, in each period the unemployed worker determines the number of job applications such that the marginal costs of a job application equal the marginal returns.

The generosity of unemployment benefits plays a key role in job search decisions. If benefits are generous, either in level or length of the entitlement period, theory predicts that unemployed workers increase their reservation wage. Unemployed workers are thus more selective in which job offers to accept. If a worker accepts a job offer in a particular period, the wage or more general job quality will be higher if the job was found in a generous benefits system.

Increasing the reservation wage reduces the marginal benefits of search, which implies that the optimal number of job applications is lower. Therefore, a more generous unemployment benefits scheme reduces the job finding rate both because unemployed workers become more selective and because they search less intensively. These behavioral responses are referred to as moral hazard. Whereas being selective on job offers has the positive effect that the match between the worker and job improves which may have long-term consequences, the reduction in job applications only causes unemployment durations to become longer. At the same time, these longer unemployment durations can potentially offset the positive effect on job quality, because of the reduced arrival of job offers and depreciation of skills. It is not clear which of these two countervailing forces prevails.

Van den Berg (1990) discusses a job search model taking account of limited entitlement to UI benefits. In the model the present value of being unemployed decreases with the unemployment duration since the remaining entitlement period to UI benefits decreases. Therefore, the reservation wage declines and unemployed workers increase their search effort. When individuals are forward looking, shortening the UI entitlement period already increases job finding rates at the moment of entering unemployment.

The standard theory predicts a smooth increase in job finding rates until UI benefits exhaustion. Several empirical studies show the existence of spikes in exit rates towards UI benefits exhaustion (e.g. Moffitt (1985), Katz and Meyer (1990) and Meyer (1990)). Most studies also find that exit rates drop again after exhausting UI benefits. Various explanations for the existence of spikes are provided in the literature, such as former employers reemploying laid-off workers at the moment of UI benefits exhaustion (Katz and Meyer (1990)) or employers and unemployed workers agreeing to delay the starting date of a new job until UI benefits exhaustion (Boone and Van Ours (2012)). Focusing on such spikes provides insight in job search behavior, but does not answer the policy relevant question how the length of benefits entitlement affects exit rates from unemployment. Answering this question requires exogenous variation in the entitlement period.

2.3 Institutional setting

In this section we first describe the Dutch UI system before the reform in October 2006. Next, we describe the changes induced by the reform.

2.3.1 Dutch UI system before October 2006

The Dutch UI law insures all employees against the risk of unemployment.[2] If a worker becomes unemployed, she is entitled to UI benefits when she has a sufficiently employment history. Entitlement requires that the worker loses at least five working hours, or 50 percent of her working hours if she works less than ten hours. The worker also has to satisfy the so-called *weeks condition* and *years condition*.[3] The weeks condition requires a worker to have worked at least 26 of the previous 39 weeks. The years condition states that the worker should have been employed for at least four out of the last five calendar years.

Workers satisfying both the weeks and the years condition were entitled to wage-related benefits equal to 70 percent of the last wage (capped at a maximum) for at least six months. The maximum duration of collecting UI benefits depends on the worker's labor market history, which is a function of age and actual employment. Because the UI administration does not have employment records before 1998, the labor market history before 1998 is equal to the age of the worker on January 1, 1998 minus 18. For the years after 1998, the labor market history consists of actual employment. For those years, a calendar year counts as employed if the worker worked at least 52 days in that year. If the worker had a labor market history of between five and ten years, the maximum length of the UI entitlement period was nine months. And this maximum entitlement period increased with each interval of five additional years of labor market history up to five years. When a UI benefits recipient leaves unemployment because of an accepted job but enters unemployment again within six months, the old UI spell is continued.

Workers who are either not or no longer entitled to UI benefits, can apply for welfare benefits. Welfare is means tested and complements the household income to 50 percent of the minimum wage for a unlimited time period. Couples and single parents receive some additional benefits. Job search requirements are similar for all benefits programs, recipients have to make a job application at least once every week.[4]

[2]The law excludes self-employed workers and some civil servants who have special arrangements.

[3]Workers satisfying the weeks condition, but not the years condition were entitled to short term UI for six months equal to 70 percent of the minimum wage or 70 percent of the last wage, whichever was lower.

[4]Whereas welfare benefits recipients have to accept all jobs, during the first year UI benefits recipients only have to accept jobs that match their skill and wage level.

2.3.2 UI reform in October 2006

The UI reform in October 2006 entailed four changes, which are summarized in Table 2.1. First, the weeks condition was tightened from having worked 26 of the previous 39 weeks to having worked 26 of the previous 36 weeks. Second, workers not satisfying the years condition (worked at least four of the past five years) are now entitled to short term UI with a level of 70 percent of their last wage instead of 70 percent of the minimum wage. Third, the replacement rate in the first two months of both short term and long term UI is now 75 percent of the last wage and afterwards 70 percent (both capped at the same maximum). Fourth, the length of the entitlement period to long term UI is reduced for almost all benefits recipients. The length of the entitlement period is now a linear function of labor market history, each additional year increased the UI entitlement period by one month.

We focus on the effects of the change in the entitlement period. Therefore, we only consider individuals who are entitled to long term UI benefits. In the analysis of the effects of the reform, we only consider individuals who satisfy the slightly stricter weeks condition after the reform.

Figure 2.1 shows the entitlement to UI benefits before and after October 2006. The reform reduced the entitlement period most for individuals with long labor market histories. For workers with a labor market history of nine, 12, 18 and 24 years the entitlement period was unaffected by the reform, while workers with a labor market history of 13, 14 or 19 years were after the reform entitled to UI benefits for a longer period. In the empirical analysis of the reform we exclude individuals with a labor market history of 13 and 14 years. Before the reform, the entitlement period

Table 2.1: Entitlement and level of UI benefits before and after the reform.

	Before reform	**After reform**
Short term UI		
When entitled	Worked 26 of last 39 weeks	Worked 26 of last 36 weeks
Level UI	70% of minimum wage	70-75% of last earned wage
Duration	6 months	3 months
Long term UI		
When entitled	Worked 26 of last 39 weeks Worked 4 out of 5 years	Worked 26 of last 36 weeks Worked 4 out of 5 years
Level UI	70% of last earned wage	70-75% of last earned wage
Duration	6-60 months	4-38 months

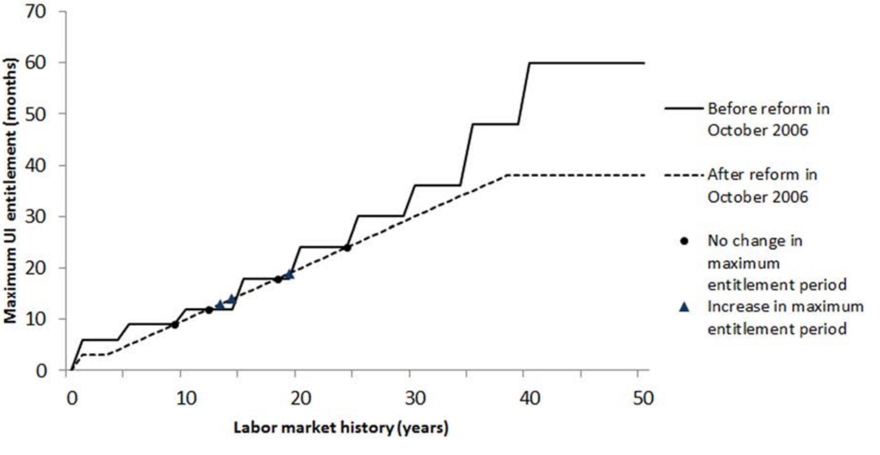
of these individuals did not exceed 12 months, and, therefore, these individuals did not receive extensive active labor market policies. After the reform, the entitlement period of these individuals exceeds 12 months and they became exposed to a regime with more extensive active labor market policies.[5]

2.4 Description of the data

We use data provided by Statistics Netherlands covering the period from 1999 to 2010. Statistics Netherlands combines information from various administrations using social security numbers. We use registrations at municipalities, the UI administration and the tax office. These include all payments of various types of benefits, but also information on jobs such as wages, working hours, type of contract (flexible or permanent), sector, etc. The data cover the full Dutch population.

We observe 1.8 million individuals who started collecting UI benefits at least once between 1999 and 2010 and they experience over 3 million UI spells. Figure 2.2 shows the number of individuals entering UI for every three months. The graph shows that the inflow follows the business cycle closely and that there is no substantial change in the inflow around the reform in October 2006. From the end of 2008 onwards the inflow into UI increased substantially. Therefore, in the empirical analysis exploiting the UI reform we only consider individuals entering UI between July 1, 2004 and

Figure 2.1: Entitlement to UI benefits before and after the reform.



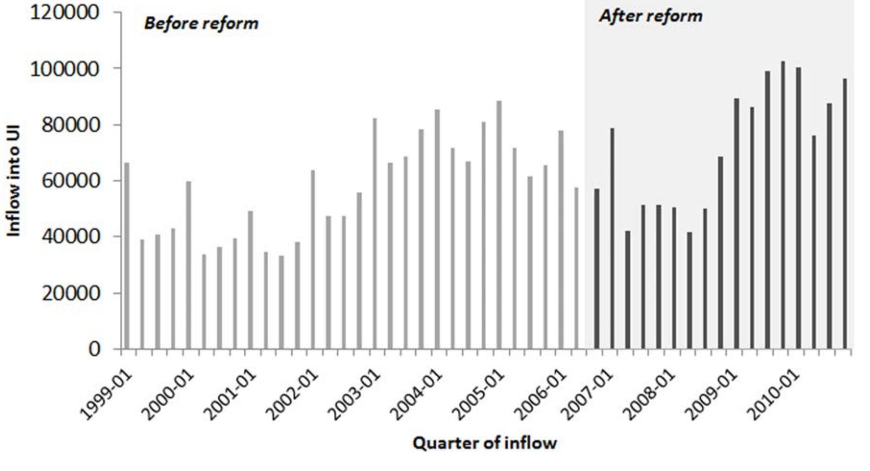
[5]Our empirical results are not sensitive to excluding these individuals.

December 31, 2008. This provides an interval of 27 months both before and after the reform. As the calculation of the labor market history was changed in January 2004, we use data from individuals entering between January 2004 and September 2006 for the regression discontinuity design exploiting the jumps in the entitlement period before the reform. We discuss how the jumps are exploited in section 2.6.

For each UI spell we observe daily information about the start and end date of collecting benefits, and the level of benefits. We use the age of the worker at January 1 1998 and the actual employment history since 1999 to construct the maximum entitlement period before and after October 2006.[6] We correct for reentering UI using the institutional rules.

From the registration of municipalities we obtain demographic variables, which we merge with the labor market information. The demographic variables contain, for example, date of birth, gender, household composition, etc. This allows us to identify the partner for which we observe labor market outcomes as well. Recall that the partners' earnings determine whether someone will become eligible for welfare after UI (see section 2.3). We construct a variable indicating potential eligibility for welfare benefits and a variable measuring the partners' earnings.

Figure 2.2: Quarterly inflow into UI between 1999 and 2010.



[6]We do not observe employment in 1998 while the UI administration does observe actual employment for this year. We count 1998 as an employed year if the worker was at least 18 years old in 1998 and employed in 1999.

Once an individual stops collecting UI benefits, we know if this was due to exhaustion of UI benefits. In that case we observe whether or not the individual starts collecting welfare benefits. Otherwise we observe subsequent labor market outcomes, e.g. earnings, working hours and type of contract. We observe this information for each job during and after unemployment.

Table 2.2 shows descriptive statistics for the total UI inflow before and after the reform.[7] Before the reform individuals had, on average, a labor market history of slightly less than 21 years. Individuals who entered UI after the reform had a labor market history of roughly 0.6 years longer. If the institutional rules from before the reform would apply the entitlement to UI benefits would, on average, be 20.6 months for both individuals before and after the reform. The reform reduced the average UI entitlement period with almost four months to 16.7 months. We observe some differences in the composition of the total group of workers entering UI before and after the reform. After the reform, we observe more women, a higher average age, more individuals who have a partner with an income above the welfare threshold, more immigrants and more single parents.

The third panel of Table 2.2 shows the mean UI duration and average job finding probabilities before and after the reform. Compared to individuals who entered before the reform, we observe a large reduction in both the mean UI duration and the median duration until finding work for individuals who entered after the reform. The percentage of unemployed workers who found work within one year after entering unemployment increased with, on average, 1.4 percentage points, while the percentage of workers who found work within three years decreased. Both individuals who entered before and after the reform have, on average, 4.1 jobs within three years after entering unemployment.

The fourth panel considers the cumulative income in the three years after inflow UI. Given the decrease in the median UI duration, it is not surprising that the average UI benefits decreased with over €2,000. The average cumulative earned wage is roughly €1,000 lower for individuals who entered after the reform. The characteristics of the first job after unemployment are given in the last panel. By construction, these statistics only include individuals who found a job within three years after UI inflow. Both the annual earnings and the daily wage have increased after the reform. After the reform, fewer unemployed workers accept a temporary contract while the number of working hours slightly increases.

[7] The pre-reform data largely coincide with the data used in the regression discontinuity analysis. Observed characteristics are balanced around the thresholds. Therefore, we do not provide separate summary statistics for this sample and postpone a more detailed discussion to section 2.6.

Table 2.2: Descriptive statistics of UI spells starting before and after the reform.[a]

	July 2004-September 2006	October 2006-December 2008
UI eligibility characteristics		
Labor market history (years)	20.6	21.2
Potential UI entitlement before reform (months)	20.6	20.6
Actual UI entitlement (months)	20.6	16.7
Personal characteristics		
Female (%)	46.7	48.1
Age at start unemployment (years)	39.0	40.2
Couple (%)	63.3	62.1
Partner with income (%)	37.7	39.1
Single parent (%)	6.0	7.5
Children (%)	38.6	38.8
Immigrant (%)	19.7	21.1
Annual earnings before UI[b] (€)	34,103	33,091
UI duration and job finding		
Median UI duration (days)	166	92
Median duration until work (days)	152	119
Found work within one year (%)	66.9	68.3
Found work within three years (%)	82.8	80.3
Number of jobs within three years	4.1	4.1
Cumulative income		
Total UI benefits in three years after inflow[b] (€)	14,836	12,673
Total earned wage in three years after inflow[b] (€)	55,997	54,888
Job quality first job		
Annual earnings first job[b] (€)	21,172	22,348
Daily wage first job[b] (€)	94.50	107.69
Temporary contract in first job (%)	36.4	33.1
Working hours in first job (per week)	27.6	27.8
Number of spells	356,566	225,168

[a] Statistics only include individuals entitled to long term UI benefits and satisfying the stricter weeks condition as it applies after the reform.
[b] Calculated in price level of 2010.

Figure 2.3 shows Kaplan-Meier estimates for exit to work for individuals who entered before the reform. In both graphs we distinguish between groups with a different entitlement period. In the upper panel we distinguish between individuals with a labor market history of nine and ten years – in other words individuals who are entitled to nine and twelve months of UI benefits. We observe a higher job finding rate for individuals entitled to nine months of UI benefits. The difference in job finding is persistent up to three years after entry into UI. In the lower panel we distinguish between individuals with a labor market history of 39 and 40 years, who are entitled to 48 and 60 months of UI benefits respectively. Again we observe a lower survival probability for individuals who are entitled to shorter UI benefits.

In the two panels of Figure 2.4 we compare the survival probabilities of individuals who entered before and after the reform. We distinguish between two groups with similar labor market histories for which the reform reduced the entitlement period. In the upper panel we observe a larger outflow to work for individuals entitled to ten months of UI benefits after the reform compared to their counterparts with an equal labor market history who entered before the reform and were thus entitled to twelve months of benefits. In the lower panel we compare individuals with a labor market history of 40 years before and after the reform and who were entitled to 60 and 38 months respectively. We observe a lower survival probability for those who entered after the reform for the entire period under consideration. The graphs suggest that potential effects of reducing the entitlement period on job finding exist during the original entitlement period, but slowly vanish afterwards.

Figure 2.5 shows annual earnings in the first job stratified by the unemployment duration. Individuals with a longer UI entitlement period have higher accepted earnings. This is not necessarily due to the entitlement period, but can also reflect the employment history or age. For individuals with long entitlement periods, we observe a declining pattern in accepted earnings. This may be due to a decreasing reservation wage, but can also be caused by heterogeneity among workers or non-stationarities in the job search process. For individuals with a shorter UI entitlement period, the decline of earnings in the first accepted job is less pronounced. This can suggest that the reservation wage of these individuals is not above the minimum wage and, therefore, all jobs are acceptable.

Figure 2.3: Kaplan-Meier estimates for the survival in unemployment for UI recipients who entered UI before the reform with different labor market histories and different lengths of the entitlement period. Upper panel: Labor market history of nine and ten years. Lower panel: Labor market history of 39 and 40 years.

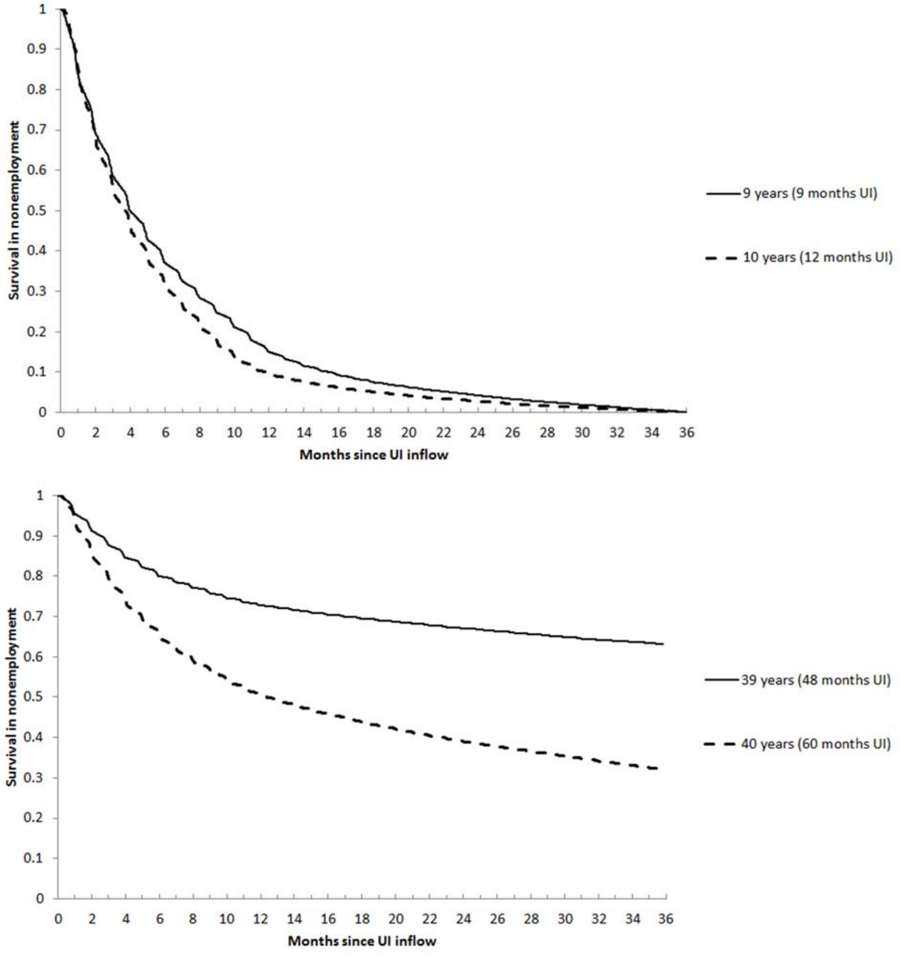


Figure 2.4: Kaplan-Meier estimates for the survival in unemployment for UI recipients with similar labor market histories who entered before and after the reform. Upper panel: Labor market history of 10 years. Lower panel: Labor market history of 40 years.

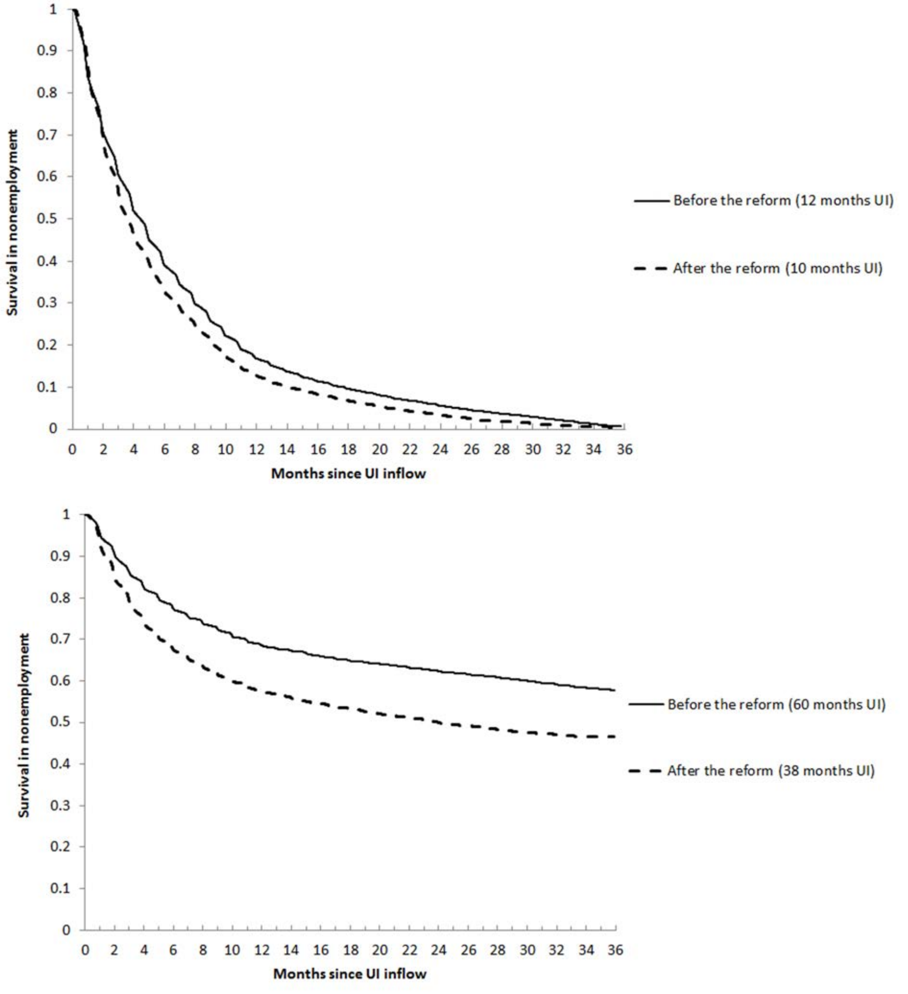
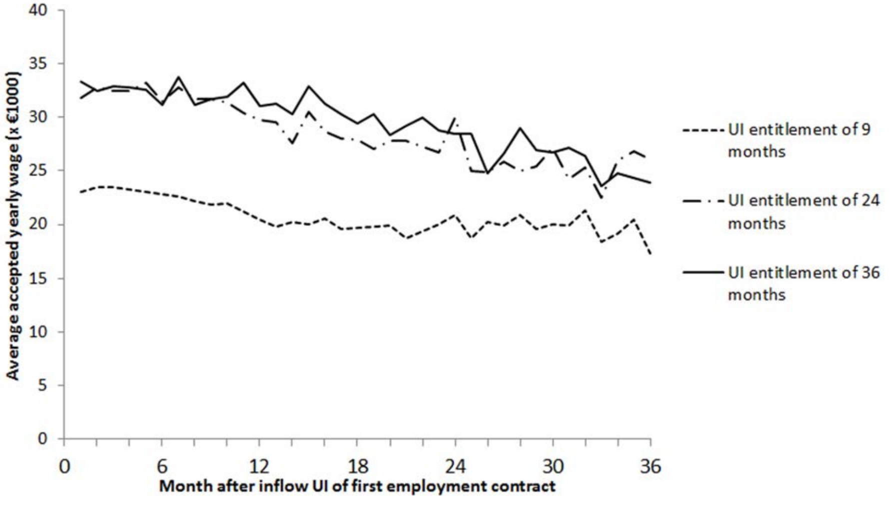


Figure 2.5: Annual earnings in the first job for individuals with different UI entitlement periods.



2.5 Estimated effects of the reform

Job search theory predicts that after reducing the generosity of a benefits scheme unemployed workers lower their reservation wage and increase their search effort. A reduction in the entitlement period to UI benefits then reduces the expected length of an unemployment spell and decreases the subsequent job quality. In this section, we test these hypotheses empirically by exploiting the reform in October 2006.

2.5.1 The model

We specify a regression model to estimate the effects of reducing the UI entitlement period on various labor market outcomes Y_{it} of individual i who started collecting UI benefits at calendar time t

$$Y_{it} = \alpha + \delta D_{it} + \sum_h \gamma_h \mathbb{1}\{H_{it} = h\} + X_{it}\beta + \mu_t + \epsilon_{it} \qquad (2.1)$$

The variable D describes the change (in months) in the UI entitlement period due to the reform. Prior to October 2006, the variable D always equals zero. We construct D such that if the reform reduces UI entitlement with six months, D takes value six. For about 88 percent of the entrants in UI after October 2006, the UI entitlement period is shorter than it would have been before the reform, thus for those individuals $D > 0$. The parameter of interest δ should be interpreted as, for example, the increase in exit probability due to reducing the entitlement period with one month.

The entitlement period to UI benefits is determined by the labor market history. We include fixed effects γ_h for all possible values of the labor market history H. As D depends on the labor market history, including these fixed effects controls for the endogeneity of D. The vector X contains several worker characteristics which capture both personal characteristics, UI history and characteristics of the last job before UI. With respect to the UI history, X contains a variable indicating if the worker returned to UI within six months and resumes the previous UI spell. In that case, X also contains a variable describing the previous elapsed UI duration, which takes the value zero in the absence of a previous UI spell. Other variables we include in X are gender, household composition, ethnicity, whether or not someone collected UI in the three years before, whether someone had a part-time job at the moment of UI inflow, earnings before entering UI, and sector.[8] The time trend μ is specified using dummy variables for each quarter of inflow in UI. This controls for calendar time variation in job finding probabilities, for example, due to business cycle variation or seasonality.

Our empirical model is a difference-in-difference model. Recall from Section 2.3 that for some employment histories the reform did not affect the maximum entitlement period to UI benefits. More specifically, individuals with a labor market history of nine, 12, 18 and 24 years were not affected by the reform and they form the control group which identifies the time trend μ. The treatment group consists of all individuals for whom the maximum entitlement period would have been affected by the reform, thus individuals with a labor market history different from nine, 12, 18 or 24 years, where for those entering after the reform $D \neq 0$. Within the treatment group D varies between -2 and 22. The fixed effects for the labor market histories control for differences in exit rates between individuals with different labor market histories. The identification of the effect of a change in the entitlement period D hinges on a common trend between individuals with different labor market histories.

We test the common trend assumption in two different ways. First, we graphically explore the trend in labor market outcomes Y before and after the reform, where we

[8] Including these control variables does not affect the estimated effects of the reduction in UI entitlement.

distinguish between the control group and treatment group. Figure 2.6 shows these trends for finding work within 12 months and cumulative earnings within three years after inflow UI (Appendix 2.A.1 contains the same figures for UI benefits receipt and temporary work). Although the level of both the job finding probability and the cumulative earnings is lower for the treatment group, the trends prior to the reform look similar, which is also the case for other outcome measures. For cohorts after the reform, the difference in job finding probabilities somewhat decreases, while we do not observe a change in the difference in cumulative earnings.

Next, we test this more formally by estimating the regression model specified in equation (2.1) for all labor market outcomes, but only using the subsample of individuals who entered UI one year before the reform. We include a placebo treatment variable, i.e. we suppose that the reform occurred in March 2006 instead of October 2006 and substitute D by the difference in months of UI entitlement if the individual would have been affected by the reform. D is zero for individuals who entered between October 2005 en March 2006.[9] Out of the 15 outcome variables, we find three significant placebo treatment effects, although two of these three outcome variables are strongly correlated. Based on the graphical analysis and the results from the placebo tests we conclude that there is a common trend in the outcome variables of the treatment and control group.

2.5.2 Effects of reducing UI entitlement

Table 2.3 presents the estimated effects of reducing the UI entitlement period on various outcome measures for finding work, earnings, benefit receipt and job characteristics. Each row in the table represents the estimated coefficient of the reduction in months for separate regressions. In each row, we also report the mean outcome before the reform.

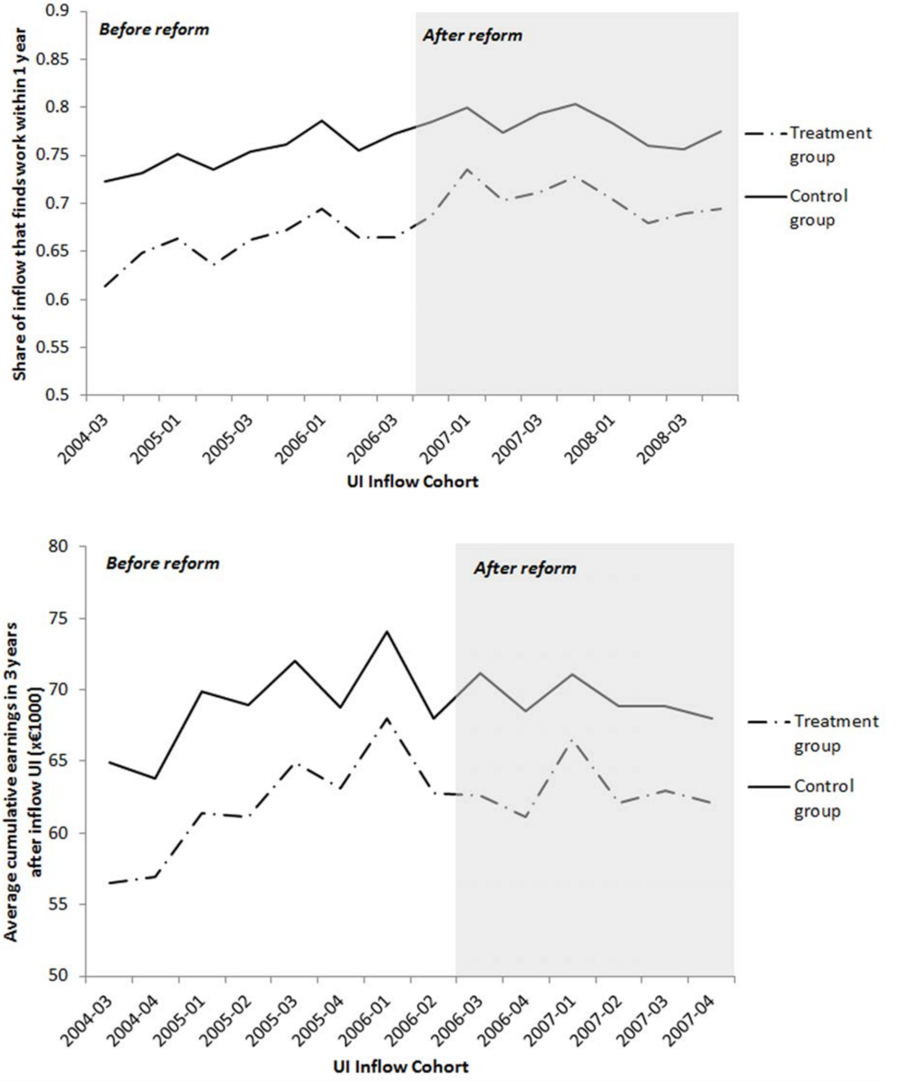
The first panel considers finding work. The outcome variables take value one if someone finds work within three, six, 12 or 24 or 36 months, respectively.[10] Reducing the UI entitlement period has no effect on the probability of finding work within three months, but the effects are significant for longer observation periods.[11] The

[9]The estimated placebo effects are shown in Table 2.A1 in Appendix 2.A.1.

[10]If an individual finds work after UI exhaustion, this is included in the outcome variables as well.

[11]Recall that in October 2006 the level of benefits in the first two months of UI increased from 70 to 75 percent of the last wage. This can affect the exit to work as well and can offset a potential positive effect of the UI reduction. To test for this we perform estimations using individuals who entered UI just before and just after the reform with a labor market history such that the UI

Figure 2.6: Job finding probability within one year and cumulative earnings within three years by inflow cohort.



Note: Individuals in the treatment group would experience a change in the UI entitlement period if they entered after the reform in October 2006. Individuals in the control group have the same entitlement period regardless of their moment of inflow (i.e. individuals with a labor market history of nine, 12, 18 of 24 years).

reform reduced the UI entitlement period with, on average, four months. Therefore, due to the reform the probability of finding work within six months after entering UI increased with about 0.005 percentage points. The effects of the reform increase as we consider a longer time window, the job finding probabilities within two and three years increase with about 0.016 percentage points. This is quite substantial if we take into account that before the reform 78 percent of the individuals found work within two years and during the third year after inflow only five percent of all individuals find work. So, the reform induced some individuals to find work, who would otherwise be at risk of being unemployed for a long period. However, our estimated effects are relatively small compared to, for example, the effects found by Van Ours and Vodopivec (2006), and the reduced UI entitlement period can not explain the total observed increase in job finding rates after the reform.

The second panel of Table 2.3 presents the effects of the UI entitlement period on the cumulative income three years after entering UI. We consider income from UI benefits, earned wages and the sum of UI benefits, earned wage and welfare benefits (defined as total income). Shortening the UI entitlement period with one month reduces, on average, total UI benefits payments within three years after inflow with about 107 euros. Expressed as a percentage of the average UI benefits payment these savings are 0.7 percent. At the same time, the reduction increases the cumulative earned wage with about 226 euros. The negative effect on UI benefits is offset by the positive effect on wages and a one month reduction increases the total income by 119 euros.

The effects of reducing the UI entitlement period on the number of jobs within three years are given in the third panel of Table 2.3. After reducing the UI entitlement period, workers tend to find more jobs within the first three years after entering UI. This could be due to increased job finding in general, but it could also be a signal for reduced job quality as workers move to better jobs.

Next, we consider job quality in more detail. Job search theory predicts that when the benefits system is less generous, unemployed workers reduce their job demands. Therefore, in the fourth panel we consider as outcomes characteristics of the first job after unemployment. These job characteristics are only observed for the possibly selective sample of workers who find work within the observation period. Such dynamic selection may bias the estimated effects, because the reform increases

entitlement period remains the same. The results of these estimations are described in Appendix 2.A.2. We do not find evidence that the change in the benefits level affects job search behavior and decreases job finding probabilities.

Table 2.3: Estimated effects of reducing the UI entitlement period.

	Coefficient	Standard error	Mean
Job finding probabilities			
Finding work within 3 months	-0.0005	(0.0004)	0.28
Finding work within 6 months	0.0013***	(0.0004)	0.49
Finding work within 12 months	0.0028***	(0.0004)	0.67
Finding work within 24 months	0.0040***	(0.0004)	0.78
Finding work within 36 months	0.0038***	(0.0004)	0.83
Cumulative income, three years			
Total UI benefits	-107.49***	(24.97)	14,836
Total earnings	226.48***	(50.10)	55,997
Total income	119.28***	(41.22)	72,052
Cumulative number of jobs, three years			
Number of jobs	0.0180***	(0.0023)	3.85
Job quality, first job			
Finding a job	0.0034**	(0.0004)	0.82
Daily wage	-0.161*	(0.084)	101.08
Working hours	0.0129	(0.0094)	27.55
Temporary contract	0.0022***	(0.0004)	0.36
Job quality, after 3 years			
Having a job	0.0036**	(0.0004)	0.65
Daily wage	-0.106	(0.104)	119.47
Working hours	0.0207*	(0.0120)	32.17
Temporary contract	-0.0011***	(0.0004)	0.17

Note: Each row represents a separate regression. All regressions include calendar time fixed effects, labor market history fixed effects and individual characteristics as controls. Standard errors are clustered at the level of region and quarter of inflow. * significant at a level of 10%, ** significant at a level of 5%, *** significant at a level of 1% .

job finding rates. To get some idea about the size of this potential bias, we estimate the effect of a one month reduction of the UI entitlement period on the probability of having any job at all in the observed time window. The estimated effect implies that the average four months reduction in UI entitlement due to the reform increased job finding from 82.3 to 83.6 percent. The effect is significant but given the high job finding rate, the selection bias will be modest. This is confirmed by the fact that pre-unemployment wages of those individuals who are observed to have found work are unaffected by the reform.

We find a small negative effect of a one month reduction on the daily wage of the first job. On average, individuals affected by the reform earn 64 cents per day less in their first job, which is about 0.63 percent of the average daily wage. This is in line with earlier literature that finds at most a small negative effect of a UI reduction on earnings. Centeno and Novo (2009), Cockx and Picchio (2013) and Nekoei and Weber (2017) find a small positive effect of an extension of the entitlement period while Lalive (2007) and Card et al. (2007) do not find any effect and Schmieder et al. (2016) find a negative effect. From our results we can draw two conclusions. First, the effect of reduced skill depreciation due to increased job finding on wages is smaller than the reduced reservation wage path. Second, the increased job finding dominates the small decline in wages when considering cumulative earnings. These results suggest that individuals experience some disutility from starting working. Individuals often have a temporary contract in their first job, which is considered as a negative signal about job quality and it provides less security than a permanent contract.[12]

In the final panel we present the effects on the quality of the job three years after inflow UI. Here we only consider workers who have a job three years after inflow, which is the case for about two-thirds of the individuals. The reform increased the probability of having a job after three years by, on average, 1.4 percentage points, which means that we can not rule out that our job quality estimates are subject to (dynamic) selection bias. In addition, we find that the pre-unemployment wage of post-reform workers who are employed after three years is, on average, 3% lower than the pre-unemployment wage of employed pre-reform workers, which indicates that those employed after the reform, on average, have less favorable characteristics.

Our results do not show a significant effect on wages and a small and only marginally significant effect on working hours. The estimated effect on having a job with a temporary contract is negative. Combined with the increased job finding this implies that the reform causes that after three years more workers have a permanent contract. Our preferred explanation is that the reform stimulates more individuals to find work quickly. First jobs are often temporary jobs and, therefore, average job turnover is high and a substantial share of the individuals lose their job again. However, the increased job finding causes that after three years more individuals have a job with a permanent contract. These jobs are associated with higher wages, which explains the average wage growth between the first job and the job after three years.

Above we provided the average effect of reducing the UI entitlement period. Next, we test whether responses differ by subgroup by estimating our models on different

[12]Boone and Van Ours (2012) find that the spikes around exhaustion of UI benefits are larger for permanent jobs than for temporary jobs and regard this as evidence that spikes occur because unemployed workers delay their starting date of a new job until the moment of exhaustion of UI benefits.

subsamples stratified by gender and age. In addition, we consider heterogeneous effects by level of benefits and entitlement to welfare benefits after exhausting UI. The group with low levels of UI benefits and potential entitlement to welfare benefits is of special interest because these workers do not face an income drop when exhausting UI benefits. Standard job search theory predicts that for this group the effects of a reduction of the UI entitlement period are limited, in particular since UI en welfare impose the same job search requirements on benefits recipients.

The estimation results are presented in the tables in Appendix 2.A.3. We do not find substantial differences in the effects between men and women and also the benefits level and potential entitlement to welfare benefits hardly affect the estimated effect. The latter does not coincide with job search theory, but can be explained by the low take-up rate of welfare benefits. We find that only 48 percent of the unemployed workers who become eligible for welfare actually start collecting these benefits.

Our estimation results indicate that young workers (under the age of 35) respond far less strongly to changes in the UI entitlement period. The previous literature often exploits age thresholds between age 40 and 55. According to our results this describes the population which is most responsive to the UI entitlement period. We do not find different effects for individuals between age 40 and 55 and individuals older than 55. The latter indicates that also older workers who may be close to retirement or leaving the labor market otherwise change their job search behavior in response to changes in the UI entitlement period.

2.6 Regression discontinuity design

Before the reform in October 2006, the length of the UI entitlement period was a stepwise function of labor market history (see Figure 2.1). We use a regression discontinuity design to exploit the jumps in the entitlement period to obtain additional estimates of the effect of the UI entitlement period.

2.6.1 The model

Recall that the employment history is equal to the number of years in which the worker worked at least 52 days. Since only few individuals work less than 52 days a year, the 52-days threshold does not give enough statistical power (and it can be manipulated by choosing the lay-off date). Because the UI administration does not have records of employment histories before 1998, the labor market history concerning years before 1998 equals the age on January 1998 minus 18. We observe the month of birth, which we can exploit to identify the effect of a longer entitlement period. More specifically, we compare, for example, two individuals who both entered UI in January 2005 and were both employed for all the years between 1998 and 2004, but individual A was born in January 1977 and individual B was born in December 1976. This implies individual A is entitled to nine months and individual B is entitled to 12 months. We specify the regression model which pools all thresholds indicated by s:

$$Y_{its} = \alpha_s + \delta D_{its} + \kappa M_i + \lambda M_i \mathbb{1}\{M_i \geq 0\} + \beta E_{it} + \mu_t + \epsilon_{its} \tag{2.2}$$

D indicates the drop in UI entitlement at the threshold. So this variable equals zero for individuals born in December or earlier and the difference (in months) in the UI entitlement period for individuals born in January or later. The parameter δ has the same interpretation as in the difference-in-difference model in the previous section and describes how a one month reduction in UI entitlement affects the outcome Y. The variable M describes the the number of months from a threshold where the entitlement is increased.[13] The regression model is specified as a local linear model and we prefer a bandwidth of twelve months around the thresholds. As a robustness check we consider bandwidths of six and 24 months around the threshold and a local quadratic model with a bandwidth of twelve months (see Tables 2.A7 and 2.A8 in Appendix 2.A.5). In all regressions, we control for the observed number of employed years since 1998 denoted by E and fixed effects for the quarter of inflow μ_t and the thresholds α_s. In the estimation we use data on workers entering UI between January 2004 and September 2006.[14] Unemployed workers with more than 12 or 24 months of entitlement are more likely to participate in more intensive re-integration programs. Therefore, these thresholds are not suitable for the regression discontinuity analysis.

Figure 2.7 shows the number of observations by labor market history in years and the corresponding UI entitlement in months. If unemployed workers strategically decide about the moment at which they enter UI, we should see a jump in the number of observations at the entitlement thresholds. Except for the threshold of 18 months

[13]Being born in January implies that $M = -1$, being born in December gives $M = 0$, being born in November implies $M = 1$, etc.

[14]We exclude immigrants from the analysis as the share of immigrants for which the exact month of birth is unknown is relatively large and Statistics Netherlands sets their month of birth to January.

– which is excluded from the analysis because of the discontinuity in re-integration programs – we do not observe an increase in the number of observations at the thresholds.[15]

2.6.2 Estimation results

The estimation results of the regression discontinuity analysis, where we pool the thresholds are given in Table 2.4. These results concur with the results reported in the previous section. Since we now consider inflow during the pre-reform period, the observation period is longer and we can also report effects up to five years after becoming unemployed.

Figure 2.7: Inflow by labor market history and UI entitlement in months.

Note: Pre-reform period, January 2004-September 2006. Labor market history where the UI entitlement period increases is denoted by a darker color.

[15]In appendix 2.A.4 we show the share of observations by birth month for all thresholds under consideration (Figures 2.A2 and 2.A3). We observe a peak in the month just *before* the thresholds of 48 months of entitlement, so for individuals who are just entitled to 36 months of benefits instead of 48 months. This occurs because Statistics Netherlands sets the month of birth to January if the actual birth month is unknown, we also observe a small peak in the month before the 12 and 36 months threshold. If anything, we observe more observations before the threshold than after, which implies that unemployed workers do not strategically time their entry into UI. In addition, we do not observe discontinuities in most other covariates such as age, gender and wage before UI around the different thresholds (Table 2.A6) in Appendix 2.A.4. We observe small discontinuities in the number of children and the indicator for having a partner with wage. The RDD estimation results are not sensitive to including these variables as additional controls.

Table 2.4: Estimation results of regression discontinuity design.

	Coefficient	Standard error	Mean
Effects on job finding			
Finds job within 6 months	0.0024***	(0.0007)	0.53
Finds job within 12 months	0.0039***	(0.0006)	0.74
Finds job within 24 months	0.0036***	(0.0006)	0.84
Finds job within 36 months	0.0044***	(0.0006)	0.88
Finds job within 60 months	0.0049***	(0.0006)	0.89
Effects on cumulative income, 3 years			
Total UI benefits	-255***	(31)	15,007
Total earnings	493***	(114)	72,665
Total income	235**	(109)	88,112
Effects on cumulative income, 5 years			
Total UI benefits	-421***	(48)	15,831
Total earnings	881***	(177)	125,925
Total income	460***	(177)	142,693
Effects on cumulative number of jobs			
Number of jobs, 3 years	0.0214***	(0.0033)	4.24
Number of jobs, 5 years	0.0374***	(0.0048)	6.38
Effects on job quality, first job			
Finding a job	0.0041***	(0.0006)	0.89
Daily wage	0.28**	(0.14)	113.09
Working hours	0.030	(0.019)	28.8
Temporary contract	-0.001	(0.001)	0.30
Effects on job quality, after 3 years			
Having a job	0.0049***	(0.0007)	0.70
Daily wage	-0.01	(0.15)	132.11
Working hours	0.012	(0.020)	32.9
Temporary contract	-0.001	(0.001)	0.13
Effects on job quality, after 5 years			
Having a job	0.0029***	(0.0007)	0.65
Daily wage	0.41**	(0.19)	137.25
Working hours	0.043	(0.027)	32.6
Temporary contract	-0.001	(0.001)	0.10

Note: Each cell in the table represents a separate regression. All regressions are estimated using a local linear specification with a bandwidth of 12 months around the thresholds and include calendar time fixed effects and actual employment history (after 1998) fixed effects as controls. Standard errors are clustered at the level of region and quarter of inflow. * significant at a level of 10%, ** significant at a level of 5%, *** significant at a level of 1%.

The first panel shows the effects on job finding. Reducing the UI entitlement period increases job finding significantly. This effect is present after six months and the effect size increases for longer observation periods. The second and third panel considers the cumulative income within three and five years. The results after three years are similar as reported earlier, and now show to become even stronger after five years. In particular, after five years each reduction in UI entitlement of one month increases, on average, cumulative earnings by €881 and income by €460. The same pattern is observed in the fourth panel, where the effect on the number of jobs is slightly higher after five years than after three years.

In the last three panels we show the results on the quality of the first job and the jobs after three and five years. Again, we should take into account that there can be (dynamic) selection bias. Similar to the results of the difference-in-difference analyses, the effects are quite small and do not provide evidence for substantial effects on the job quality.

In Table 2.A9 in Appendix 2.A.5 we show estimation results for each threshold separately. Since each threshold is related to a different UI entitlement period, the additional analysis indicates if reducing the entitlement period has larger effects for individuals with otherwise longer or shorter entitlement periods. Job search theory predicts that effects are largest for unemployed workers with short entitlement periods. This is confirmed by our empirical results which show that the results are largest when the UI entitlement period is reduced from 12 to nine months. For this threshold the effects largely coincide with the effects discussed above. The effects size decline when the size of the UI entitlement period threshold increases.

2.7 Modeling job finding

In the previous sections we showed that reducing the maximum UI entitlement period increases the probability of finding work. In this section we explore the underlying dynamics in job finding during the unemployment spell using a hazard rate model. This model describes exit to work after τ periods of unemployment for an individual who enters UI at calendar time t with an observed UI entitlement $\max UI$, labor market history H and other observed characteristics X,

$$\theta(\tau|t, H, X) = \lambda(\tau)\phi(\max UI - \tau)\varphi(\max UI, t, X, H) \tag{2.3}$$

where $\lambda(\tau)$ denotes duration dependence in job finding. Our function of interest, $\phi(\max UI - \tau)$, describes how the job finding rate is affected by the remaining entitlement period after τ days of unemployment. We specify $\phi(\max UI - \tau)$ as a piecewise constant function. In the function $\varphi(\max UI, t, H, X)$, we allow the length of UI entitlement to have a constant effect on job finding from the start of unemployment. To account for endogeneity of the UI entitlement period we include calendar time indicators, and fixed effects for the labor market history H. Finally, we include the same covariates X as in the estimations discussed in Section 2.5. We use Cox partial likelihood method to estimate the hazard rate, thereby leaving $\lambda(\tau)$ unspecified.

Given that we restrict the model to a proportional specification, the identification of the causal effects of the UI entitlement is similar as in the difference-in-difference model specified in Section 2.5. The control group of individuals who are not affected by the reform identifies the calendar time effects. The labor market histories H control for differences between individuals with different employment histories or age, and the effects of the UI entitlement period are identified from interactions between calendar time and labor market histories. This identification hinges again on a common trend in exit rates to work between individuals with different labor market histories.[16]

Table 2.5 shows the estimated effects of the UI benefits entitlement period on the job finding rate.[17] The estimation results show that increasing the UI entitlement period significantly reduces the job finding rate. Each additional month of UI entitlement reduces the job finding rate from the start of the spell by about 1 percent. There is a clear peak in the exit rate to work around the moment of exhaustion. The job finding rate is highest in the month before and the month after exhausting UI benefits. The exit rate increases as the moment of exhaustion is approaching and declines again in the six months after exhaustion to the reference level before exhaustion.

Standard job search models predict that job finding rates stay constant after exhaustion of UI benefits as reservation wages remain low and job search effort high. Like many previous empirical studies, we find that job finding rates actually fall after UI benefits exhaustion. This pattern is often found in the literature (e.g. Moffitt (1985), Katz and Meyer (1990) and Meyer (1990)) and suggests that not only the

[16] We artificially censor all unemployment spells after three years. This avoids that for the pre-reform data we have a longer observation period and also reduces the impact of the financial crisis which started to affect the Dutch labor market late 2011.

[17] We take the time interval three to six months before exhaustion as the reference category since the minimum entitlement period is four months. As such, for every possible length of the entitlement period there are individuals observed in the reference category.

level of benefits is important in explaining the transition rate from unemployment to employment.

Table 2.5: Estimated effects of the UI entitlement period from a hazard rate model for finding work.

	Coefficient	Standard error
UI entitlement (in months)	-0.010***	(0.002)
Time until/after exhaustion		
More than 6 months after exhaustion	-0.201***	(0.013)
3-6 months after exhaustion	0.021	(0.013)
1-3 months after exhaustion	0.113***	(0.012)
First month after exhaustion	0.240***	(0.013)
Last month until exhaustion	0.239***	(0.011)
1-3 months until exhaustion	0.140***	(0.008)
3-6 months until exhaustion, (reference category)	0	
6-12 months until exhaustion	-0.064***	(0.006)
12-24 months until exhaustion	-0.093***	(0.008)
More than 24 exhaustion until exhaustion	-0.100***	(0.013)
Number of observations	581,734	

Note: The model includes calendar time fixed effects, labor market history fixed effects and individual characteristics as controls.* significant at a level of 10%, ** significant at a level of 5%, *** significant at a level of 1%.

In Appendix 2.A.6 we present estimation results for different subgroups. Based on standard job search theory we expect larger peaks in the exit rate to work around the moment of exhaustion for individuals who face a larger income drop after UI exhaustion (e.g. Van den Berg (1990)). Our findings are in line with this, we find the largest peak for individuals with high UI benefits and entitlement to welfare and individuals without welfare entitlement. For individuals not facing an income drop, we observe a much smaller peak in the exit rate to work in the months before and after exhaustion. This indicates that the level of benefits is not the only element of the benefits program which is important for job finding. The moment of exhausting UI benefits may be considered as an implicit deadline to unemployed workers, for example, because there is a larger negative stigma associated to collecting welfare benefits than UI benefits. Alternatively, the transition from UI to welfare may be associated with uncertainty, which unemployed workers dislike. Finally, recall that the take-up rate of welfare benefits is only 48 percent, so some unemployed workers voluntarily accept a drop in income at the end of UI entitlement. The results also indicate that unemployed workers change their job search behavior earlier in anticipation of a benefits drop if this drop is more substantial. When we compare other subgroups, we observe larger peaks for individuals with an entitlement to more than 12 months, men and individuals older than 50.

2.8 Conclusion

In this chapter we use two identification strategies to study the effect of the entitlement period to UI benefits on the exit rate to work and post-unemployment job quality. First, we exploit a substantial reform in the Dutch UI system in October 2006. The reform reduced, on average, the entitlement period by about four months, but there are groups of workers for whom the entitlement period did not change or even increased. This allows us to use a difference-in-difference model. Second, we exploit that prior to the reform the entitlement period was a step function of the worker's labor market history. Based on month of birth, we identify workers with an identical employment history but a different UI entitlement period. The difference just below and above the thresholds is, on average, seven months and can be at most 12 months. It allows for a regression discontinuity approach.

The estimation results from both approaches concur, and are in agreement with earlier literature. We find that reducing the UI entitlement period increases the job finding rate, which indicates the presence of moral hazard. A 10-week extension of benefits increases the non-employment time with 6-9 days. According to Chetty (2013), "... studies have uniformly found that a 10-week extension in unemployment benefits raises the average amount of time people spend out of work by at most one week". We have used a hazard rate model to estimate the underlying dynamics in job finding. This model shows that reducing the entitlement period increases job finding rates from the start of unemployment, but job finding rates peak just before the moment of exhausting UI benefits and declines again afterwards.

Moral hazard in unemployment benefits programs is not necessarily bad if the benefits act as search subsidy improving the post-unemployment job quality. Our results show at most modest effects of the UI entitlement period on the quality of the first job. We find some indication that unemployed workers are slightly more inclined to accept a temporary job. Job turnover after starting working again is high, causing that in the long run the effect on the job finding rate is the dominating factor. Three or five years after becoming unemployed the increase in cumulative earnings due to a UI entitlement reduction substantially exceeds the lower total benefits payments. This can only be explained if unemployed workers derive substantial utility from leisure or if they have a high discount rate.

We find some heterogeneity in effects of the UI entitlement period. First, young workers are much less responsive than workers above age 35. Most previous studies exploit age thresholds in benefits entitlement, which lie between 40 and 55. Young workers have, therefore, not received much attention. Second, the effect of reducing the entitlement period is most substantial for unemployed workers with already relatively short entitlement periods. This is consistent with the spike in job finding rates just before exhausting benefits and the conclusion that unemployed workers have relatively high discount rates.

We use our empirical results to quantify the expenditures associated to the UI reform in 2006. We consider the cohort of workers entering UI the year before the reform and follow them for three years. Total expenditures on UI benefits within these three years equal 2756 million euros. Our empirical results show that the reform reduces this by 86 million euros, which is a reduction of 3.1 percent. At the same time, due to increased job finding the cumulative earnings within three years increases with 182 million euros and the cumulative income - the sum of UI benefits, wages and welfare benefits - increased with 96 million euros. If the reform would have been implemented one year earlier, the total income of individuals within this period increases with 0.6 percent.

Finally, the Dutch reform occurred during a period of low unemployment and substantial GDP growth. This also holds for the observation period of the data used in the regression discontinuity design. Schmieder et al. (2012) and Kroft and Notowidigdo (2016) show that moral hazard associated to UI is larger during economic booms than in recessions. We should thus observe relatively large behavioral responses, but our estimated effects are slightly smaller than reported in the literature.

2.A Appendix

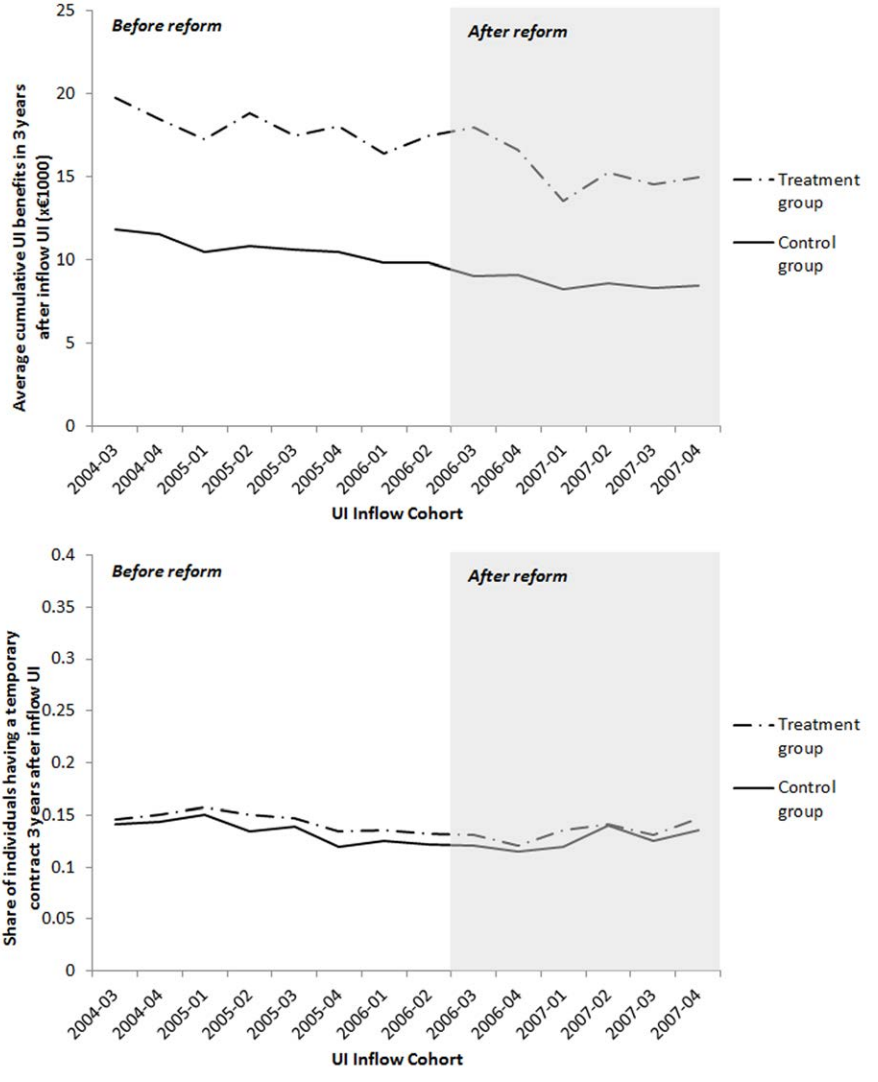
2.A.1 Results from placebo analysis

Table 2.A1: Estimated placebo effects before the actual reform to test for common trends.

	Coefficient	Standard error
Job finding		
Finding work within 3 months	-0.0007	(0.0007)
Finding work within 6 months	-0.0019***	(0.0007)
Finding work within 12 months	0.0001	(0.0005)
Finding work within 24 months	0.0010	(0.0007)
Finding work within 36 months	0.0013*	(0.0007)
Cumulative income, three years		
UI benefits within 3 years	-53.53	(38.96)
Earnings within 3 years	27.71	(76.45)
Income within 3 years	-18.25	(80.24)
Job quality, first job		
Daily wage	0.0284	(0.2039)
Working hours	-0.0057	(0.0182)
Temporary contract	0.0009	(0.0008)
Job quality, after 3 years		
Daily wage	0.1891	(0.1739)
Working hours	0.0253	(0.0184)
Temporary contract	0.0004	(0.0007)
Number of jobs within 3 years	0.0073**	(0.0036)

Note: Each row in the table represents a separate regression. All regressions include calendar time fixed effects, labor market history fixed effects and individual characteristics as controls. Standard errors are clustered at the level of region and quarter of inflow. * significant at a level of 10%, ** significant at a level of 5%, *** significant at a level of 1%.

Figure 2.A1: Cumulative UI benefits receipt and having a temporary contract within three years by inflow cohort.



Note: Individuals in the treatment group would experience a change in the UI entitlement period if they entered after the reform in October 2006. Individuals in the control group have the same entitlement period regardless of their moment of inflow (i.e. individuals with a labor market history of nine, 12, 18 of 24 years).

2.A.2 The effects of increasing of UI benefits level

The Dutch UI reform in October 2006 did not only change the length of the entitlement period, but also the level of UI benefits in the first two months of UI. Before October 2006 unemployed workers received 70 percent of the last earned wage. After October 2006 the benefits level was increased to 75 percent of the last earned wage during the first two months of UI. For the remaining UI months the benefits level was still 70 percent of the last earned wage. Røed and Zhang (2003), Moffitt (1985), and Meyer (1990) show that the level of benefits affects job search behavior. We test whether the small increase in UI benefits in the first two months affects job finding by considering workers who entered just before and just after the reform and for whom the UI entitlement period was unaffected. These are the individuals with a labor market history of either nine, 18 or 24 months. This has similarities with a regression discontinuity design and we estimate the effects of finding work within two, three, six, 12, 24 and 36 months.

Table 2.A2 shows the estimated coefficients for receiving high benefits during the first two months rather than 70 percent of the last wage. Because benefits are higher, job search theory predicts a negative effect on job finding. We do not find significant effects on the job finding probability for any of the observed time periods. Our estimates suggest that the increase in the benefits level after the reform did not afftect job search behavior or the job finding probabilities, which in turn suggests that our estimates in the main analysis are not biased.

Table 2.A2: Estimation effects of increasing the benefits level during the first two months of UI.

Job finding probability	Coefficient	Standard error
Finds job within 2 months	-0.085	(0.195)
Finds job within 3 months	-0.036	(0.193)
Finds job within 6 months	0.067	(0.196)
Finds job within 12 months	-0.023	(0.159)
Finds job within 24 months	0.070	(0.172)
Finds job within 36 months	0.094	(0.173)

Note: Estimations performed on a sample of unemployed workers who entered just before and after the reform and for which the entitlement period did not change. Each row in the table represents a separate regression. All regressions include calendar time fixed effects, labor market history fixed effects and individual characteristics as controls. Standard errors are clustered at the level of region and quarter of inflow. * significant at a level of 10%, ** significant at a level of 5%, *** significant at a level of 1%.

2.A.3 Estimation results of the difference-in-difference analysis for subgroups

Table 2.A3: Estimation results of difference-in-difference analysis using the 2006 UI reform, by gender.

	All		Men		Women	
Job finding probabilities						
Finds work, 3 months	-0.0005	(0.0004)	-0.0001	(0.0004)	-0.0008*	(0.0005)
Finds work, 6 months	0.0013***	(0.0004)	0.0018***	(0.0005)	0.0008	(0.0005)
Finds work, 12 months	0.0028***	(0.0004)	0.0031***	(0.0004)	0.0024***	(0.0005)
Finds work, 24 months	0.0040***	(0.0004)	0.0043***	(0.0005)	0.0037***	(0.0005)
Finds work, 36 months	0.0038***	(0.0004)	0.0040***	(0.0005)	0.0038***	(0.0005)
Cumulative income, three years						
Total UI benefits	-107.49***	(24.97)	-109.03***	(34.87)	-1.91	(24.85)
Total earnings	226.48***	(50.10)	230.52***	(65.48)	156.77***	(46.39)
Total income	119.28***	(41.23)	119.39**	(54.36)	158.57***	(42.01)
Cumulative number of jobs within three years						
Number of jobs	0.0180***	(0.0023)	0.0181***	(0.0024)	0.0194***	(0.0030)
Job quality, first job						
Finding a job	0.0034***	(0.0004)	0.0038***	(0.0005)	0.0031***	(0.0005)
Daily wage	-0.161*	(0.084)	-0.164	(0.119)	-0.037	(0.087)
Working hours	0.0129	(0.0094)	0.0359***	(0.0115)	-0.0021	(0.0124)
Temporary contract	0.0022***	(0.0004)	0.0020***	(0.0004)	0.0023***	(0.0005)
Job quality, after 3 years						
Having a job	0.0036***	(0.0004)	0.0044***	(0.0005)	0.0037***	(0.0006)
Daily wage	-0.106	(0.104)	-0.097	(0.145)	0.017	(0.100)
Working hours	0.0207*	(0.0120)	0.0252*	(0.0147)	0.0346**	(0.0175)
Temporary contract	-0.0011***	(0.0004)	-0.0018***	(0.0005)	-0.0004	(0.0006)

Note: Only the coefficient of reduction of entitlement period is shown. Each cell in the table represents a separate regression. All regressions include calendar time fixed effects, labor market history fixed effects and individual characteristics as controls. Standard errors are clustered at the level of region and quarter of inflow. * significant at a level of 10%, ** significant at a level of 5%, *** significant at a level of 1%.

Table 2.A4: Estimation results of difference-in-difference analysis using the 2006 UI reform, by age.

	Younger than 35		35-50 years		Older than 50	
Job finding probabilities						
Finds work, 3 months	0.0002	(0.0017)	0.0004	(0.0010)	0.0000	(0.0004)
Finds work, 6 months	-0.0002	(0.0015)	0.0019**	(0.0009)	0.0005	(0.0004)
Finds work, 12 months	-0.0012	(0.0013)	0.0028***	(0.0008)	0.0010**	(0.0005)
Finds work, 24 months	-0.0003	(0.0010)	0.0027***	(0.0007)	0.0023***	(0.0005)
Finds work, 36 months	0.0001	(0.0010)	0.0037***	(0.0005)	0.0037***	(0.0005)
Cumulative income, three years						
Total UI benefits	55.70	(39.81)	-117.96**	(51.39)	-42.56	(37.29)
Total earnings	-12.90	(236.98)	129.55	(159.50)	201.21***	(70.60)
Total income	93.80	(231.44)	35.65	(178.92)	156.56**	(66.70)
Number of jobs, three years						
Number of jobs	0.0114	(0.0094)	0.0143***	(0.0053)	0.0113***	(0.0026)
Job quality, first job						
Daily wage	-0.517**	(0.222)	0.078	(0.167)	-0.114	(0.152)
Working hours	-0.0239	(0.0410)	-0.0038	(0.0205)	0.0310**	(0.0135)
Temporary contract	0.0018	(0.0019)	0.0023**	(0.0009)	0.0011**	(0.0004)
Job quality, after 3 years						
Daily wage	-0.105	(0.288)	0.161	(0.222)	-0.178	(0.136)
Working hours	-0.0885**	(0.0388)	0.0182	(0.0234)	0.0273	(0.0170)
Temporary contract	0.0006	(0.0018)	-0.0013	(0.0011)	0.0001	(0.0007)

Note: Only the coefficient of reduction of entitlement period is shown. Each cell in the table represents a separate regression. All regressions include calendar time fixed effects, labor market history fixed effects and individual characteristics as controls. Standard errors are clustered at the level of region and quarter of inflow. * significant at a level of 10%, ** significant at a level of 5%, *** significant at a level of 1%.

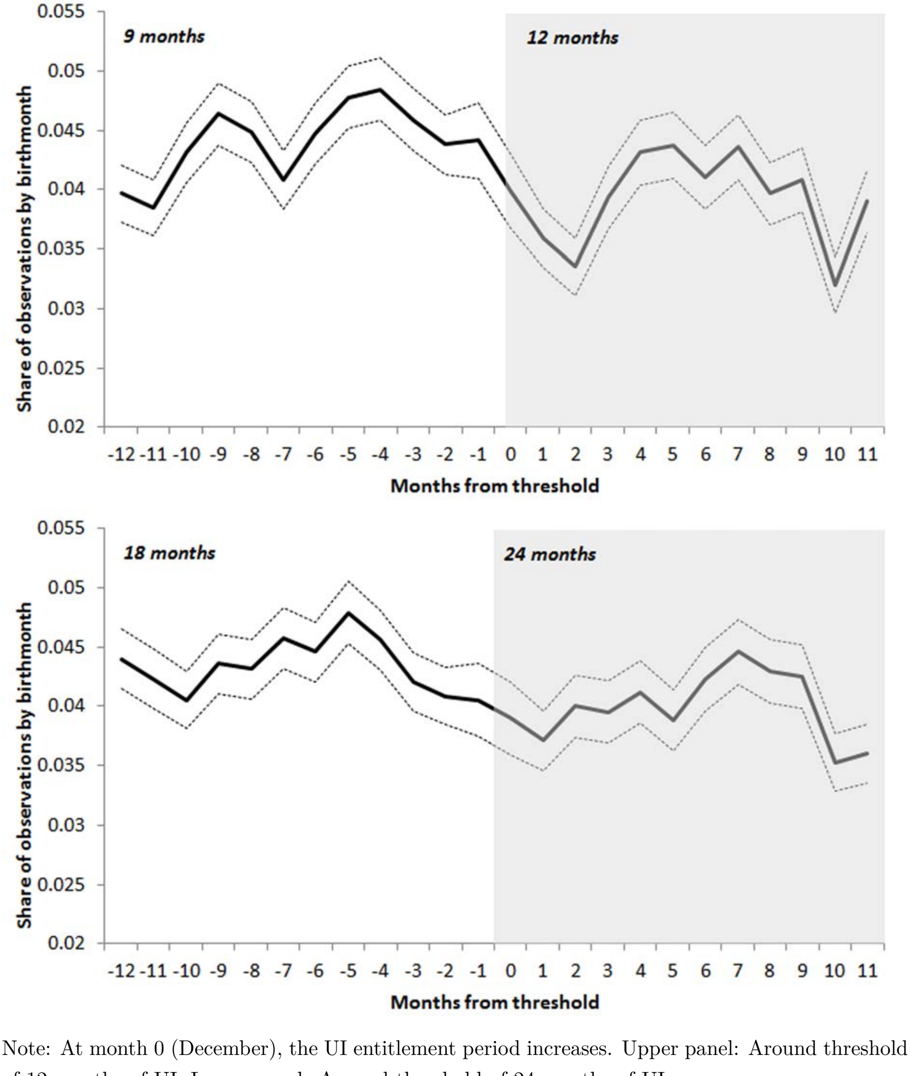
Table 2.A5: Estimation results of difference-in-difference analysis using the 2006 UI reform, by welfare entitlement and UI benefit level.

	Low UI with welfare		High UI with welfare		No welfare entitlement	
Job finding probabilities						
Finds work, 3 months	0.0006	(0.0008)	0.0001	(0.0004)	-0.0005	(0.0005)
Finds work, 6 months	0.0032***	(0.0008)	0.0017***	(0.0005)	0.0014**	(0.0005)
Finds work, 12 months	0.0037***	(0.0007)	0.0029***	(0.0005)	0.0034***	(0.0005)
Finds work, 24 months	0.0041***	(0.0007)	0.0040***	(0.0005)	0.0045***	(0.0005)
Finds work, 36 months	0.0044***	(0.0007)	0.0039***	(0.0005)	0.0038***	(0.0005)
Cumulative income, three years						
Total UI benefits	-23.97	(19.09)	-69.55**	(32.66)	-177.32***	(30.90)
Total earnings	204.91***	(76.45)	202.35***	(61.07)	224.85***	(77.24)
Total income	179.84**	(75.42)	130.00**	(53.47)	47.22	(64.49)
Number of jobs, three years						
Number of jobs	0.0175***	(0.0043)	0.0184***	(0.0025)	0.0220***	(0.0032)
Job quality, first job						
Daily wage	-0.280	(0.267)	-0.172	(0.141)	-0.027	(0.092)
Working hours	-0.0044	(0.0185)	0.0048	(0.0131)	0.0266**	(0.0124)
Temporary contract	0.0020**	(0.0009)	0.0026***	(0.0004)	0.0020***	(0.0005)
Job quality, after 3 years						
Daily wage	0.086	(0.206)	-0.183	(0.136)	0.010	(0.165)
Working hours	0.0423	(0.0284)	0.0151	(0.0149)	0.0243	(0.0158)
Temporary contract	-0.0006	(0.0011)	-0.0013**	(0.0006)	-0.0008	(0.0006)

Note: Only the coefficient of reduction of entitlement period is shown. Each cell in the table represents a separate regression. All regressions include calendar time fixed effects, labor market history fixed effects and individual characteristics as controls. Standard errors are clustered at the level of region and quarter of inflow. * significant at a level of 10%, ** significant at a level of 5%, *** significant at a level of 1%.

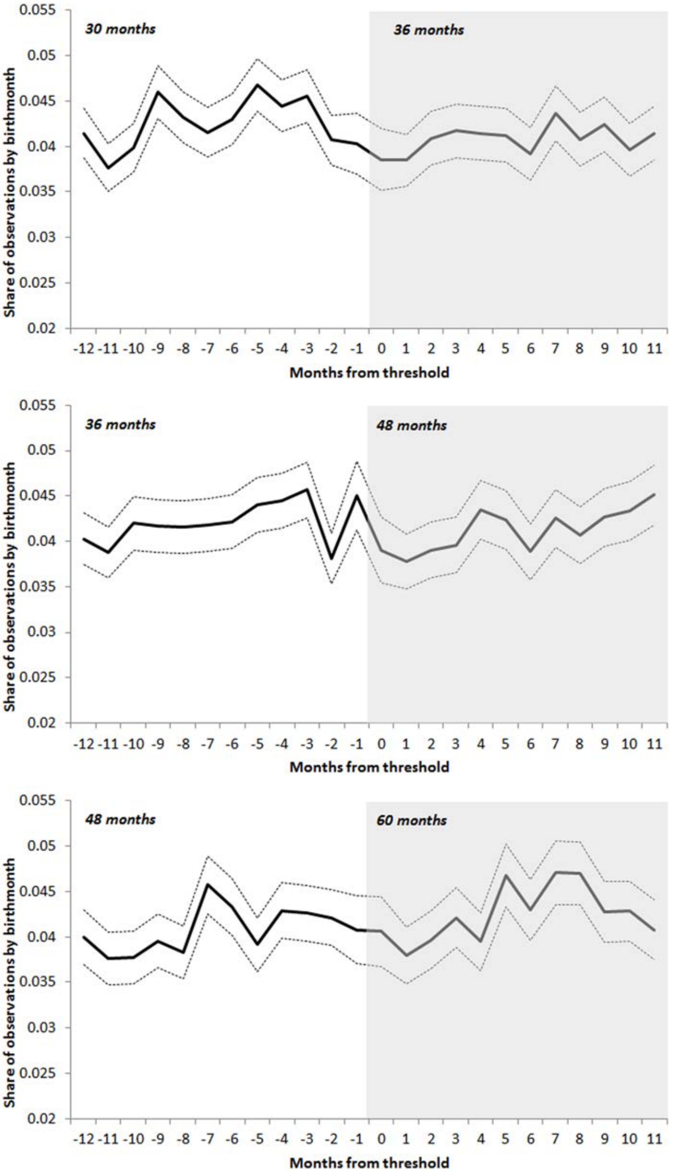
2.A.4 Test for discontinuities around UI entitlement threshold

Figure 2.A2: Share of observations by birth month around UI entitlement threshold of 12 and 24 months.



Note: At month 0 (December), the UI entitlement period increases. Upper panel: Around threshold of 12 months of UI. Lower panel: Around threshold of 24 months of UI.

Figure 2.A3: Share of observations by birth month around UI entitlement threshold of 36, 48 and 60 months.



Note: At month 0 (December), the UI entitlement period increases. Upper panel: Around threshold of 36 months of UI. Middle panel: Around threshold of 48 months of UI. Lower panel: Around threshold of 60 months of UI

Table 2.A6: Estimation results for test for discontinuity in controls around the threshold (pooled for all thresholds).

	Coefficient	Standard error
Estimated discontinuity in controls at threshold		
Men	-0.0001	(0.0007)
Age at entry UI	0.0001	(0.0002)
Wage before UI	-51.54	(87.46)
Couple	-0.0001	(0.0006)
Has children	-0.0030***	(0.0005)
Has partner with wage	-0.0018**	(0.0010)

Note: Estimations are obtained using a regression discontinuity design with a local linear specification and a bandwidth of 12 months around the threshold. Each row in the table represents a separate regression. All regressions include calendar time fixed effects and labor market history fixed effects as controls. Standard errors are clustered at the level of region and quarter of inflow. * significant at a level of 10%, ** significant at a level of 5%, *** significant at a level of 1%.

2.A.5 Estimation results of RDD, robustness checks and estimations by threshold

Table 2.A7: Estimation results of regression discontinuity design.

	Linear		Quadratic	
Effects on job finding				
Finds job within 6 months	0.0024***	(0.0007)	0.0024***	(0.0008)
Finds job within 12 months	0.0039***	(0.0006)	0.0041***	(0.0007)
Finds job within 36 months	0.0044***	(0.0006)	0.0053***	(0.0007)
Finds job within 60 months	0.0049***	(0.0006)	0.0057***	(0.0007)
Effects on cumulative income, 3 years				
Total UI benefits	-255***	(31)	-308***	(33)
Total earnings	493***	(114)	567***	(140)
Total income	235**	(109)	251*	(133)
Effects on cumulative income, 5 years				
Total UI benefits	-421***	(48)	-497***	(48)
Total earnings	881***	(177)	1023***	(233)
Total income	460***	(177)	517**	(217)
Effects on cumulative number of jobs				
Number of jobs, 3 years	0.0214***	(0.0033)	0.0223***	(0.0036)
Number of jobs, 5 years	0.0374***	(0.0048)	0.0442***	(0.0052)
Effects on job quality, first job				
Finding a job	0.0041***	(0.0006)	0.0049***	(0.0007)
Daily wage first job	0.28**	(0.14)	0.24	(0.15)
Working hours first job	0.030	(0.019)	0.046**	(0.024)
Temporary contract first job	0.001	(0.001)	0.001	(0.001)
Effects on job quality, after 3 years				
Having a job	-0.0049***	(0.0007)	-0.0056***	(0.0008)
Daily wage	0.01	(0.15)	0.10	(0.17)
Temporary contract	0.001	(0.001)	0.001	(0.001)
Effects on job quality, after 5 years				
Having a job	-0.0029***	(0.0007)	-0.0033***	(0.0008)
Daily wage	-0.41**	(0.19)	-0.43*	(0.23)
Temporary contract	0.001	(0.001)	0.000	(0.001)

Note: Local linear and local quadratic models with bandwidth of 12 months around the threshold. Each cell in the table represents a separate regression. All regressions include calendar time fixed effects and employment history fixed effects as controls. Standard errors are clustered at the level of region and quarter of inflow.

Table 2.A8: Estimation results of regression discontinuity design, different bandwidths.

	12 months		6 months		24 months	
Effects on job finding						
Finds work, 6 months	0.002***	(0.001)	0.003***	(0.0008)	0.003***	(0.001)
Finds work, 12 months	0.004***	(0.001)	0.004***	(0.0007)	0.004***	(0.000)
Finds work, 24 months	0.004***	(0.001)	0.003***	(0.0006)	0.005***	(0.000)
Finds work, 36 months	0.004***	(0.001)	0.004***	(0.0006)	0.006***	(0.000)
Finds work, 60 months	0.005***	(0.001)	0.005***	(0.0006)	0.006***	(0.000)
Effects on cumulative income, 3 years						
Total UI benefits	-255***	(31)	-242***	(35)	-321***	(24)
Total earnings	493***	(114)	472***	(128)	552***	(87)
Total income	235**	(109)	228*	(122)	228***	(82)
Effects on cumulative income, 5 years						
Total UI benefits	-421***	(48)	-413***	(53)	-515***	(35)
Total earnings	881***	(177)	789***	(180)	1003***	(150)
Total income	460***	(177)	373**	(177)	486***	(147)
Effects on cumulative number of jobs						
Number of jobs, 3 years	0.021***	(0.003)	0.022***	(0.004)	0.024***	(0.002)
Number of jobs, 5 years	0.037***	(0.005)	0.036***	(0.005)	0.042***	(0.004)
Effects on job quality, first job						
Finding a job	0.004***	(0.001)	0.004***	(0.001)	0.006***	(0.000)
Daily wage	0.28**	(0.14)	0.30*	(0.16)	0.17*	(0.10)
Working hours	0.030	(0.019)	0.046**	(0.021)	0.031**	(0.014)
Temporary contract	0.001	(0.001)	0.000	(0.001)	-0.001**	(0.001)
Effects on job quality, after 3 years						
Having a job	-0.005***	(0.001)	-0.005***	(0.001)	-0.005***	(0.001)
Daily wage	0.01	(0.15)	0.05	(0.18)	-0.10	(0.13)
Working hours	-0.012	(0.020)	-0.035	(0.021)	-0.010	(0.014)
Temporary contract	0.001	(0.001)	0.001	(0.001)	0.000	(0.001)
Effects on job quality, after 5 years						
Having a job	-0.003***	(0.001)	-0.003***	(0.001)	-0.003***	(0.001)
Daily wage	-0.41**	(0.19)	-0.31	(0.19)	-0.40***	(0.14)
Working hours	-0.043	(0.027)	-0.060**	(0.029)	-0.034*	(0.018)
Temporary contract	0.001	(0.001)	0.000	(0.001)	0.000	(0.001)

Note: Estimates presented here were obtained using a local linear model. Each cell in the table represents a separate regression. All regressions include calendar time fixed effects and employment history fixed effects as controls. Standard errors are clustered at the level of region and quarter of inflow. * significant at a level of 10%, ** significant at a level of 5%, *** significant at a level of 1%.

Table 2.A9: Estimation results of regression discontinuity design for every threshold separately.

	9-12 months		18-24 months		30-36 months		36-48 months		48-60 months	
Effects on job finding										
Finding work within 6 months	0.0079*	(0.0045)	-0.0001	(0.0023)	0.0017	(0.0028)	0.0024	(0.0016)	0.0007	(0.0017)
Finding work within 12 months	0.0095***	(0.0036)	0.0013	(0.0022)	0.0026	(0.0025)	0.0021	(0.0015)	0.0003	(0.0018)
Finding work within 24 months	0.0054**	(0.0024)	0.0011	(0.0016)	0.0012	(0.019)	0.0009	(0.0014)	0.0017	(0.0018)
Finding work within 36 months	0.0038*	(0.0019)	0.0025*	(0.0014)	0.0001	(0.0016)	0.0019	(0.014)	0.0014	(0.0018)
Finding work within 60 months	0.0052**	(0.0020)	0.0023	(0.0016)	0.0029	(0.0017)	0.0016	(0.0015)	0.0028	(0.0017)
Effects on cumulative income										
Total UI benefits within 3 years	-110**	(52)	-76	(56)	-96	(101)	-110*	(64)	-125	(123)
Total earnings within 3 years	848	(555)	1189***	(423)	-666	(452)	19	(287)	149	(291)
Total income within 3 years	749	(538)	1112***	(422)	-744	(453)	-87	(273)	25	(274)
Cumulative number of jobs										
Number of jobs within 3 years	0.039**	(0.020)	0.028**	(0.012)	0.001	(0.013)	0.007	(0.009)	0.011	(0.008)
Number of jobs within 5 years	0.064**	(0.028)	0.038**	(0.018)	-0.018	(0.020)	0.023	(0.014)	0.013	(0.013)
Effects on job quality										
Finding a job	0.0038*	(0.0019)	0.0026*	(0.0014)	0.0002	(0.0016)	0.0008	(0.0013)	0.0023	(0.0017)
Daily wage first job	0.52	(0.52)	1.17***	(0.40)	-0.04	(0.43)	0.39	(0.26)	0.54	(0.52)
Working hours first job	0.017	(0.113)	0.080	(0.065)	-0.048	(0.066)	-0.023	(0.037)	0.023	(0.050)
Temporary contract first job	-0.002	(0.005)	-0.003	(0.002)	0.003	(0.003)	-0.001	(0.002)	0.001	(0.002)
Having a job after 3 years	0.008**	(0.004)	0.005**	(0.002)	-0.002	(0.002)	0.004***	(0.001)	0.001	(0.002)
Daily wage after 3 years	0.08	(0.52)	0.55	(0.42)	-0.89	(0.56)	0.12	(0.27)	-0.05	(0.63)
Working hours after 3 years	0.055	(0.100)	-0.015	(0.048)	-0.013	(0.059)	-0.049	(0.034)	0.130*	(0.075)
Temporary contract after 3 years	-0.003	(0.004)	-0.003*	(0.002)	-0.000	(0.002)	-0.001	(0.001)	0.002	(0.002)

Note: Estimates presented here were obtained using a local linear model with a bandwidth of twelve months. Each cell in the table represents a separate regression. All regressions include calendar time fixed effects and employment history fixed effects as controls. Standard errors are clustered at the level of region and quarter of inflow. * significant at a level of 10%, ** significant at a level of 5%, *** significant at a level of 1%.

2.A.6 Estimation results of for job finding duration for subgroups

Table 2.A10: Estimated effects of the UI entitlement period from a hazard rate model for finding work, by welfare entitlement and level of UI benefits.

	All	Low UI with welfare	High UI with welfare	No welfare entitlement
UI entitlement (months)	-0.010***	-0.013***	-0.010***	-0.009***
	(0.001)	(0.002)	(0.001)	(0.001)
More than 6 months after exhaustion	-0.201***	0.152***	-0.143***	-0.324***
	(0.013)	(0.042)	(0.018)	(0.024)
3-6 months after exhaustion	0.021	0.157***	0.083***	-0.092***
	(0.013)	(0.039)	(0.018)	(0.023)
1-3 months after exhaustion	0.113***	0.101**	0.195**	0.025
	(0.012)	(0.040)	(0.017)	(0.022)
First month after exhaustion	0.240***	0.078	0.345***	0.167***
	(0.013)	(0.040)	(0.019)	(0.025)
Last month until exhaustion	0.239***	0.104**	0.308***	0.189***
	(0.011)	(0.033)	(0.016)	(0.020)
1-3 months until exhaustion	0.140***	0.091***	0.184***	0.117***
	(0.008)	(0.023)	(0.012)	(0.015)
3-6 months until exhaustion, (reference category)	-	-	-	-
6-12 months until exhaustion	-0.064***	-0.112**	-0.062***	-0.052***
	(0.006)	(0.018)	(0.009)	(0.012)
12-24 months until exhaustion	-0.093***	-0.085**	-0.086***	-0.068***
	(0.008)	(0.028)	(0.012))	(0.015)
More than 24 exhaustion until exhaustion	-0.100***	-0.030	-0.113***	-0.057*
	(0.013)	(0.042)	(0.018)	(0.022)
Number of observations	581,734	54,234	293,376	234,124

Note: Each column in the table represents a separate regression. All regressions include calendar time fixed effects, labor market history fixed effects and individual characteristics as controls. * significant at a level of 10%, ** significant at a level of 5%, *** significant at a level of 1%.

Table 2.A11: Estimated effects of the UI entitlement period from a hazard rate model for finding work, for immigrants and by length of entitlement period.

	Immigrants	Max. entitlement below 12 months	Max. entitlement above 12 months
UI entitlement (months)	-0.002	0.019***	-0.024***
	(0.001)	(0.005)	(0.001)
More than 6 months after exhaustion	-0.044	-0.118	-0.188***
	(0.030)	(0.066)	(0.022)
3-6 months after exhaustion	0.075*	-0.027	-0.050*
	(0.032)	(0.047)	(0.023)
1-3 months after exhaustion	0.117***	0.016	0.057*
	(0.031)	(0.035)	(0.023)
First month after exhaustion	0.227***	0.106***	0.292***
	(0.036)	(0.032)	(0.027)
Last month until exhaustion	0.241***	0.105***	0.408***
	(0.030)	(0.024)	(0.022)
1-3 months until exhaustion	0.111***	0.103***	0.091***
	(0.024)	(0.016)	(0.013)
3-6 months until exhaustion, (reference category)	-	-	-
6-12 months until exhaustion	-0.070***	-0.113***	-0.100***
	(0.018)	(0.016)	(0.013)
12-24 months until exhaustion	-0.078***	-	-0.195***
	(0.021)		(0.014)
More than 24 exhaustion until exhaustion	-0.054	-	-0.262***
	(0.030)		(0.018)
Number of observations	72,831	114,648	325,311

Note: Each column in the table represents a separate regression. All regressions include calendar time fixed effects, labor market history fixed effects and individual characteristics as controls. * significant at a level of 10%, ** significant at a level of 5%, *** significant at a level of 1%.

Table 2.A12: Estimated effects of the UI entitlement period from a hazard rate model for finding work, separately by gender and age.

	Men	Women	Younger than 35	Older than 50
UI entitlement (months)	-0.010***	-0.009***	0.003	-0.014***
	(0.001)	(0.001)	(0.003)	(0.001)
More than 6 months after exhaustion	-0.198***	-0.230***	-0.323***	-0.126
	(0.021)	(0.021)	(0.041)	(0.114)
3-6 months after exhaustion	0.100***	-0.061**	-0.087**	0.445***
	(0.020)	(0.020)	(0.029)	(0.106)
1-3 months after exhaustion	0.159***	0.039*	-0.012	0.330**
	(0.019)	(0.019)	(0.024)	(0.101)
First month after exhaustion	0.302***	0.163***	0.126***	0.280*
	(0.021)	(0.021)	(0.023)	(0.120)
Last month until exhaustion	0.289***	0.166***	0.125***	0.277**
	(0.018)	(0.018)	(0.018)	(0.009)
1-3 months until exhaustion	0.167***	0.107***	0.091***	0.133
	(0.013)	(0.013)	(0.013)	(0.007)
3-6 months until exhaustion, (reference category)	-	-	-	-
6-12 months until exhaustion	-0.062***	-0.078***	-0.074***	-0.002
	(0.010)	(0.010)	(0.011)	(0.006)
12-24 months until exhaustion	-0.054***	-0.123***	-0.044	0.240***
	(0.013)	(0.014)	(0.026))	(0.050)
More than 24 exhaustion until exhaustion	-0.075***	-0.086***	-	0.270***
	(0.019)	(0.022)		(0.005)
Number of observations	212,626	170,763	126,299	59,618

Note: Each column in the table represents a separate regression. All regressions include calendar time fixed effects, labor market history fixed effects and individual characteristics as controls. * significant at a level of 10%, ** significant at a level of 5%, *** significant at a level of 1%.

CHAPTER 3

Assessing the Effects of Disability Insurance Experience Rating. The Case of the Netherlands.*

3.1 Introduction

According to the literature, one of the most important conditions for preventing work disability is that workers should receive timely interventions and work adaptations (OECD (2010)). In this respect, firms can play a key role by facilitating the return to work from sickness (Autor and Duggan (2010)). Setting Disability Insurance (DI) premiums that are experience rated may therefore be effectively increase the awareness of firms concerning DI benefit costs, which eventually will reduce the number of DI beneficiaries. Even so, the literature on the effects of experience rating is limited (Tompa et al. (2012)).

In this context, the Netherlands provides an interesting setting in which to study the effects of experience rating. After DI enrollment peaked at 12 percent of the labor force in the mid-nineties, the Dutch government implemented several reforms to reduce the number of DI beneficiaries. One of these measures was the introduction of firm experience rating in 1998. While most countries that provide Workers' Compensation use experience rating to finance disability benefits, the Netherlands and Finland are the only countries with experience rating for public DI benefits.

*This chapter has been published as De Groot and Koning (2016).

In the Netherlands, the DI premium for both firms and governmental agencies is based on the DI costs of the (former) workers of the particular firm or agency. In the period investigated here, annual firm disability risks were defined as the disability costs of DI benefit recipients who entered into the program over a time frame of five preceding years, divided by the average wage sum over the same time frame. Next, the DI risk was translated into the DI premium that was paid by firms over their current wage sum. This premium was capped by both a maximum and a minimum premium. Over the years, the maximum DI premium peaked in 2004 at about 9% of the wage sum for firms classified as large. For the remaining group of small firms, DI maximum premium rates were proportionally lower.

To study the effects of experience rating, this chapter exploits the removal of experience rating for the group of small firms that took place in 2003. This removal of experience rating allows us to use a difference-in-difference (DiD) design, with large firms as a control group for which the experience-rating incentive did not change. We study whether the removal of experience rating increased the DI inflow and decreased DI outflow rates, using 2001 and 2002 as pre-treatment years and 2003 and 2004 as successive years in which the reform was enacted and may have affected DI inflow and DI outflow. In the empirical analysis, we use matched administrative data from Statistics Netherlands on firms and (former) workers between 1999 and 2011. We enrich these data with DI spells as well as other demographic and labor market characteristics. This resulted in a data set with over 250,000 unique firms and almost ten million workers who are eligible for DI benefits.

The choice of these particular years for the analysis had to do with two important reforms that took place in 2005 and 2006. These reforms probably affected small and large firms in different ways, which led us to limit the time period we use for our DiD design. In particular, the reform in 2005 extended the sickness period that precedes DI benefit receipt – and for which firms are financially responsible – from one to two years. In 2006, a substantial reform of the DI system introduced the distinction between two types of DI benefits: one for workers who were permanently and fully disabled, and one for those who are partially and/or temporarily disabled. Experience rating did not apply to the new scheme for permanently and fully disabled individuals, thus restricting the experience-rating incentive to new partially and/or temporarily disabled individuals. Overall, both reforms substantially reduced the inflow into DI and the coverage of experience rating.

Although our preferred model focuses on the pre-2005 period, we also present DiD analyses that exploit the re-introduction in 2008 of experience rating for small firms; this yields estimates of the effect of experience rating on DI inflow and DI outflow. Moreover, we re-estimate the pre-2005 analysis on a sample of individuals that excludes workers who would not have been entitled to DI benefits if they had applied for DI benefits after the reforms in 2005 and 2006. This provides us with more insight into the specific ways the reforms may have altered the potential impact of experience rating.

Generally, our findings are in line with economic predictions. In the time period under investigation, experience rating reduced inflow into DI and increased outflow from DI. These results are robust with respect to sensitivity analyses on the setup of our data and the specification of common trends. As to DI outflow, we find effects to be confined to partially disabled workers only. There is no evidence of experience-rating effects in the post-2005 period. We argue that this decrease in the impact can largely be attributed to the extension of the sick period before DI benefits commence – from one year to two years.

This chapter adds to a literature on experience rating that is still limited. For the Netherlands, Koning (2009) studies the unanticipated effects of experience rating of firms who experienced an increase in their DI premium. Van Sonsbeek and Gradus (2013) estimate the effect of experience rating in the Netherlands, using aggregated sector data. Both studies find that experience rating reduced the inflow into DI by about 15%. Korkeamäki and Kyyrä (2012) exploit a pension reform in Finland to study the effect of experience rating. They find significant effects of experience rating for older workers on both the inflow into sick leave and the transition from sick leave into disability retirement.[1]

Experience rating is more widespread in private Workers Compensation (WC) schemes than in DI schemes that are provided as a public scheme. Most studies on WC focus on such outcome measures as fatality- and injury rates. The picture that emerges from these studies is that experience rating reduces disability claim costs (see Hyatt and Thomason (1998) or Ruser and Butler (2010) for survey studies).[2] At the same time, evidence points to certain unintended effects of experience rating, such as increased claims control and increased pressure not to report injuries (Ison (1986), Lippel (1999), Strunin and Boden (2004)).

This chapter proceeds as follows. In the next section we describe the Dutch DI system and in Section 3.3 we discuss the method of experience rating. Section

[1]There is a related literature that studies the effect of experience rating in the context of sickness benefits; see e.g. Fevang et al. (2014) and Böheim and Leoni (2011).

[2]For the US, we refer to Ruser (1985, 1991), Seabury et al. (2012) and Bruce and Atkins (1993) as studies on experience rating. In addition, Campolieti et al. (2006) presents evidence for Canada and Lengagne (2014) for France.

3.4 presents our data. We discuss the empirical implementation in Section 3.5 and present the results from the estimations in Section 3.6. Section 3.7 concludes.

3.2 Institutional setting

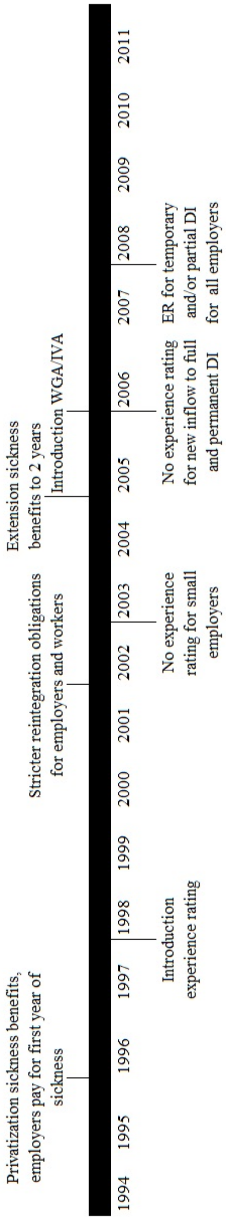
Until recently, the Dutch DI system could be characterized as one of the most generous schemes of all OECD countries (OECD (2010)). Although several reforms have been introduced to make the scheme less susceptible to moral hazard problems, the Dutch DI scheme still differs from most DI schemes in other countries in some important aspects. The level of the benefits is based on the difference between the pre-disability (covered) earnings and the residual earnings capacity, where the residual earnings capacity is the income the individual could earn conditional on his or her disability. This means that disability is measured as a percentage, rather than as an all-or-nothing condition. Moreover, the Netherlands is one of the few countries where the DI program covers all workers against all income losses that result from both occupational and non-occupational injuries (LaDou (2011)). DI claims are assessed by the public benefit administration called UWV (Uitvoeringsinstituut Werknemersverzekeringen, roughly translated as Employee Insurance Agency).

Since the introduction in 1967 of the generous DI scheme known as the WAO, the Dutch DI stock has steadily increased and the DI inflow has remained persistently high (Figure 3.2). The generosity of the system made it susceptible to moral hazard problems; for both firms and workers the scheme functioned as an attractive alternative pathway into unemployment (Koning and van Vuuren (2007) and Koning and van Vuuren (2010)). Starting from 1996, the Dutch government implemented various reforms to increase the incentive of both employers and workers to decrease DI enrollment (see Figure 3.1).

First, the sickness benefit program was privatized in 1996, making employers fully financially responsible for the first year of sickness benefits of their workers. Employer incentives were further enhanced by the system of DI experience rating that started in 1998.[3] Since then, the DI premium for Dutch firms has been based on the actual DI benefit costs of their (former) workers. The calculation of the DI premiums will be explained in the next section. The ability of firms to deter DI claims was (and still is) limited, as claims follow automatically once the sickness period ends.

[3]The incentives of sickness benefits and DI experience rating both applied to all employers, including governmental agencies. For ease of exposition, in the remainder of the chapter we refer to all employers – also including governmental agencies – as ‘firms’.

Figure 3.1: Changes in Disability Insurance employer incentives in the Netherlands, 1994-2011



In 2002, another reform increased the responsibility of firms by means of a more stringent system of gatekeeping; see De Jong et al. (2011) for a detailed description of the gatekeeper protocol. Firms have thus become responsible for the work resumption of sick workers, with the obligation to draft a rehabilitation plan together with the sick worker. In 2005, the sickness period for which firms are responsible, was further extended from one to two years. This measure, which effectively increased employer incentives to prevent sickness, also implies that (as of 2005) individuals entered disability benefits after two years of sick leave instead of after one year. This caused a substantial drop in DI inflow in 2005 (see Figure 3.2).

Finally, the most recent reform in 2006 entailed the start of two different types of DI benefits: the IVA (Income scheme for Fully Disabled) benefit for individuals who are fully and permanently disabled and the WGA (Act for Partially Disabled workers) benefit for those with partial, or temporarily full, disability.

Figure 3.2 shows that there are strong reasons to believe that the DI reforms have been largely successful in curbing DI inflow since the start of this century. Koning and Lindeboom (2015) argue that the key to this success has been the intensified role of firms in preventing long-term sickness absence and subsequent disability, with a strong emphasis on early interventions. Substantial economic incentives increased the urgency among firms to increase their efforts to prevent sickness and accidents and to help reintegrate disabled workers, while the Gatekeeper protocol facilitated employer awareness and guided firms in their new role. That said, the extent to which the experience rating system has contributed to this process remains unclear.

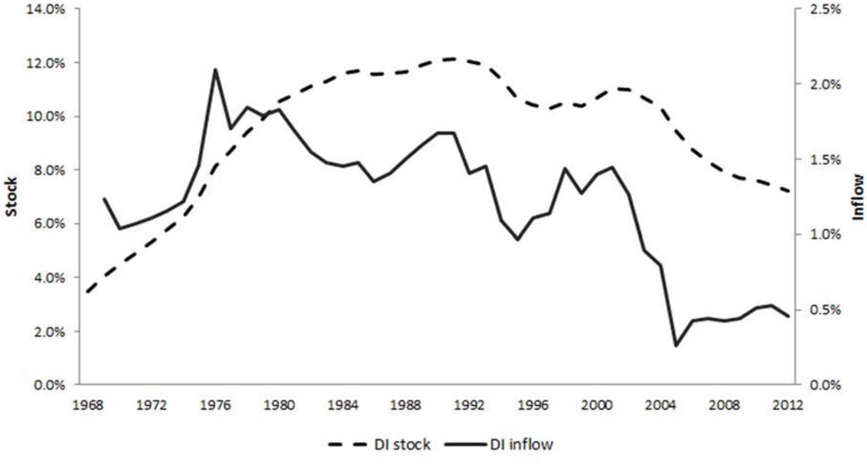
3.3 Experience rating in the Netherlands

In this section we explain the calculation of the experience-rated DI premium of Dutch firms. We first discuss the general method of calculating DI premiums in 1998 and then present an overview of changes in the calculation of the premiums over the years. To shed some light on the consequences of these changes, we also assess yearly variation in the size of DI experience-rated premiums, which is measured as a percentage of the annual wage costs of a firm.

3.3.1 Setting of experience rating

The experience-rated DI premium of Dutch firms is based on the individual disability risk of a firm. The disability risk is defined as

Figure 3.2: Dutch stock and inflow of workers in Disability Insurance, 1967-2012



Note: Measured as a percentage of the insured population. Source: Employee Insurance Agency Netherlands

$$d_{it} = \frac{\sum_{s=0}^{T} S_{t-2,t-2-s}}{\sum_{s=0}^{T} W_{t-2-s}/(T+1)} \ , \tag{3.1}$$

where $S_{t,\tau}$ are the disability costs of firm i in year t for recipients that entered into the program at time τ ($t \geq \tau$). As the equation shows, disability costs are divided by the insured wage costs W_t at time t, so as to obtain the disability risk d_t. Both the DI benefit costs and the wage sum are registered with a delay of two years and are summed over several successive cohorts of workers. In 1998, the time window for the disability risk was five years, so $T = 4$. Particularly for starting firms, the information that is needed to calculate the disability risk is incomplete. The disability cost percentage is then calculated over the longest available time window, and subsequently rescaled to a time window of five years. Although this way of rescaling (artificially) increases the spread of DI risks, the effective impact in actual premiums that are paid is limited; in almost all cases rescaling applies to small firms that either have no disability costs or would have paid maximum premiums also in the absence of rescaling.

Next, the firm DI premium p_{it} that follows the individual disability risk is capped by the minimum premium p_{min} and the maximum premium p_{max}:

$$p_{it} = min\left(p_{min} + d_{it},\, p_{max}\right)\ . \tag{3.2}$$

This means that every firm pays at least a uniform minimum premium. Moreover, the premium cap implies that the experience-rating system is 'incomplete' to some extent: higher disability costs result in proportionate increases in the DI premium up to the maximum premium, but over-users do not pay the additional costs they impose on the system. Next to DI benefit costs that originate from firm start-ups and firm bankruptcies, the costs of over-users are financed by the minimum premiums.

In the time period under investigation, the values of the minimum and maximum premiums vary with respect to firm size, the argument being that small firms are more susceptible to exogenous variation in their DI cost percentage. Initially, small firms were defined as having total wage costs that are smaller than the average wage costs per worker in the Netherlands, multiplied by 15 (workers). Maximum premiums are set equal to four times the average premium for large firms and to three times the average premium for small firms. Then, using an iterative algorithm, the minimum premiums are set at the level that balances the total disability costs with the collected premiums. As DI cost percentages of small firms are more likely to be bounded by the maximum, the minimum premium is higher for small firms.

For ease of exposition, equation 3.2 abstracts from any differences in DI benefits that stem from the two-year delay in the experience-rating system. That is, if the current average DI risk exceeds (is smaller than) the DI risk at $t-2$, the premiums are increased (decreased) proportionally. In the years before 2005, the DI risks were downscaled by at most 17%, but after 2005 upscaling of around 30% was applied.

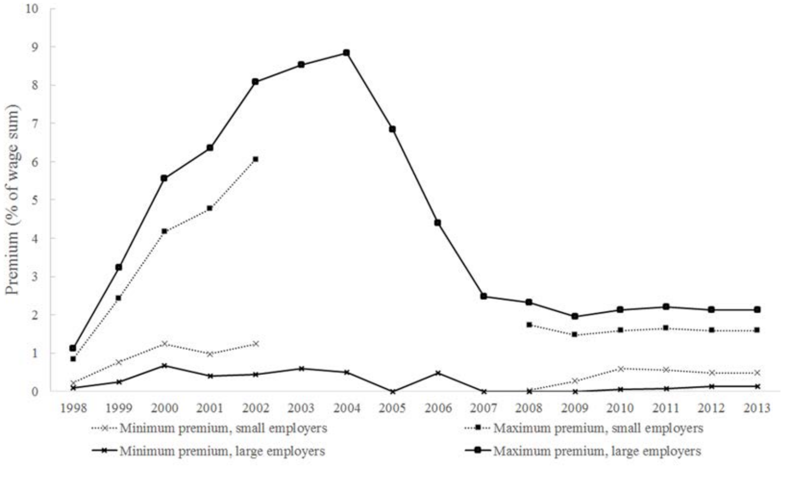
As a final remark, the introduction of experience rating was combined with the possibility for firms to opt out of the public system to private insurance providers. Between 2001 and 2004, at most 3.8% of the firms opted out of the public system (Deelen (2005)). Also, Hassink et al. (2015), who investigate the years 2007-2011 wherein the share of privately insured firms equaled about 30%, show that opting out had no effect on DI inflow rates. We thus do not expect opting out to substantially change the incentive of DI experience rating.

3.3.2 Experience rating over the years

Over the years, the calculation method of DI experience rating has not changed fundamentally. This does not mean, however, that the effective impact of experience rating on individual DI premiums has remained constant over time. In 2003, experience rating was abolished for firms that were classified as 'small', and was replaced by a system of sectoral premium rates. In 2004, the coverage of experience rating across firms was further reduced, as the group of 'small' firms was extended from 15 to 25 times the average wage costs in the Netherlands. Firms with wage costs between 15 to 25 times the average wage were thus still experience rated in 2003. In 2008, however, experience rating was re-introduced for smaller firms. The scheme now covers the DI benefit costs of the old WAO scheme and the new WGA scheme for temporary and/or partial disability. As the total costs of these two new benefit schemes together are gradually decreasing over time, the total sum of DI costs that are experience rated decreases over time as well.

Due to the above-mentioned changes, we observe substantial variation in the potential range of the experience-rated premiums across years (see Figure 3.3). With additional DI benefit cohorts that were annually added to the individual disability risk, the spread of experience-rated premiums increased in the first years of DI experience rating between 1998 and 2003. However, lower experience-rated DI costs caused by the extension of sick leave benefits in 2005 and the new DI scheme in 2006 have effectively reduced the spread of DI premiums to levels that have been fairly constant since 2007.

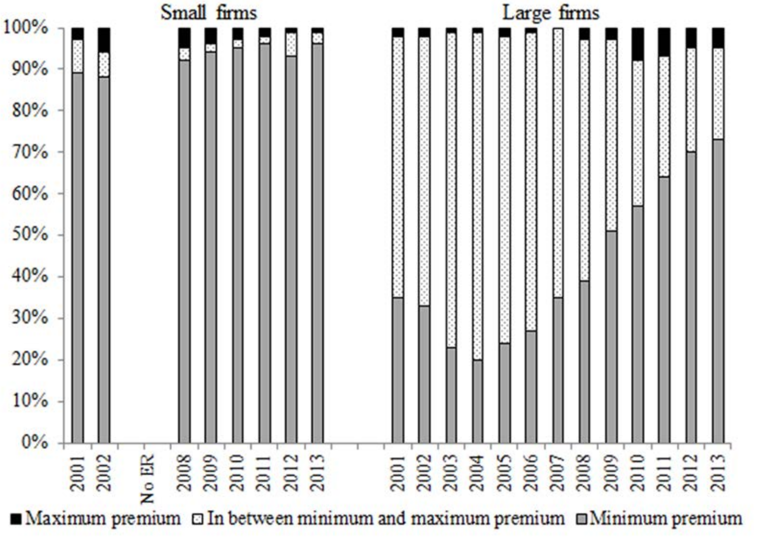
Figure 3.3: Range of experience-rated DI premiums stratified by firm size, 1998-2013



Note: DI premiums are measured as a percentage of wage costs. Firm size is based on the total wage costs of the firm. Source: Own calculations, based on UWV data

To shed more light on the importance of the minimum and maximum DI premiums, Figure 3.4 presents the distribution of the premiums for all firms, using administrative data from UWV. Clearly, the vast majority of small firms – without disabled workers that were assigned to them – pay the minimum premium. In the years 1999-2002, around 5% of the small firms paid the maximum premium; in 2008-2011 this percentage decreased to around 3%. While most small firms pay either the minimum or the maximum premium, the majority of the firms that are classified as 'large' pay a premium somewhere between the minimum and maximum premium.

Figure 3.4: Distribution of experience-rated DI premiums of firms, 1999-2013.



Source: Own calculations, based on data from UWV

3.4 Data

In our analysis, we use various administrative data sets from Statistics Netherlands that contain information on DI benefits and employment spells that are observed between 1999 and 2011. Data sets from Statistics Netherlands can be linked with unique firm and worker identifiers. As to firms, we also observe the administrative information from UWV that is needed to calculate their DI risks, including their status as 'small' or 'large'.

Unfortunately, until 2009, firms in the UWV data do not have similar identifiers as those of Statistics Netherlands. This means that the classification of firms into 'small' or 'large' can only be derived from the information of wage sum costs in the data of Statistics Netherlands. In this context, care should be taken in two respects.

First, the exact calculation of wage costs in the data of Statistics Netherlands may differ from calculations from UWV due, for instance, to differences in the reference date and the inclusion or exclusion of additional income such as leased cars or compensation for travel costs. This in turn implies the presence of measurement errors in the data from Statistics Netherlands, causing some employers to be wrongly classified as small or large. To shed more light on the potential impact of measurement errors, we can, however, merge the firm data for 2009-2011. We then find that about 0.5% of the small firms have been wrongly classified as large; the percentage of large firms that have been wrongly classified as small then decreases from 6.4% in 2009 to 4.6% in 2011. In light of these small fractions, we do not expect a large estimation bias. If anything, we would underestimate the potential effects of the removal of experience rating for small firms because some of the classified small firms are actually experience rated and vice versa.

Second, firms in the data from Statistics Netherlands may consist of different plants, each paying distinct experience-rated premiums. An example is a large chain of supermarkets in the Netherlands. Statistics Netherlands merges these supermarkets to one large firm, while UWV regards them as separate entities with different risk premiums. To solve this matter, we restrict our analysis to firms with single plants.[4] This results in a loss of around 20% of the firms and 30% of the workers in our sample. These are predominantly larger firms.

Table 3.1 summarizes the main characteristics of the combined data sets from Statistics Netherlands. We only present the statistics for the selected sample of firms with a single plant. Recall that the data also include governmental agencies, as DI experience rating also applies to these employers. Also, note that the statistics on DI recipients represent only the benefits of individuals who were assigned to a firm because of experience rating. As a result, we observe a decrease in the percentage of individuals with DI benefits, especially since the extension of the sick leave benefits in 2005 and the introduction of the new WGA and IVA schemes in 2006 (see Van Sonsbeek and Gradus (2013) and Koning and Lindeboom (2015)). Accordingly, the

[4]For example, in 2009 91% of the firms in the UWV data correspond to exactly one firm in the data of Statistics Netherlands, 7% to two firms, 2% to three or more firms. As a robustness test, we present model outcomes that also employ data from firms with multiple plants, assuming that plants all have similar experience-rating incentives.

average premium has decreased substantially after these reforms. Finally, we observe a decrease in the number of firms in our sample after 2005. This is due to a change in the source of employment contracts in 2006 in the data of Statistics Netherlands.

3.5 Empirical implementation

3.5.1 General estimation strategy

Obviously, the experience-rating system in the Netherlands aims at an increase in preventative and reintegration activities. In line with this, one would expect a decrease in the inflow into DI and an increase of the outflow out of DI of those disabled workers that were assigned to firms.[5] We now test whether experience rating had these intended effects on DI, using a difference-in-difference approach that exploits the removal of experience rating for small firms in 2003.[6]

Recall from Section 3.2 that several DI reforms took place after the introduction of experience rating in 1998. These reforms may have altered the effectiveness of DI experience rating. Specifically, in 2005 the sickness benefits period was extended to two years, and in 2006 the new DI scheme with two distinct schemes was enacted. It is likely that the reform in 2005 led to a lower DI inflow rate, with DI recipients having more severe impairments compared to the period when the assessment of claims was performed after one year of sickness benefit receipt, and the eligibility standards were less stringent. In addition, the introduction of a graduated DI system may have triggered complex behavioral responses among individuals – see e.g. Autor and Duggan (2007) and Marie and Vall Castello (2012).

Since the reforms in 2005 and 2006 changed the size and composition of the DI inflow substantially and may have affected small and large firms in different ways, the primary focus of our analysis will be on the time period from 1999 to 2004.[7] In these years, our treatment group consists of small firms for which experience rating was

[5]Experience rating could also have unintended effects, such as substitution to Unemployment Insurance (UI) benefits, changes in hiring policies or an increase of firm exits. These effects are, however, beyond the scope of the current study.

[6]Although there are two distinct experience-rating systems for small and large firms, the use of regression discontinuity designs to estimate the impact of experience rating is not straightforward in the current context. In particular, firms in a close interval around the threshold can switch from being classified as small to large, or the reverse.

[7]To clarify this point, consider the 2005 extension of the sick leave period. According to Kok et al. (2013), small firms responded to this change by increasing private insurance, whereas larger firms did not. This renders it likely that the decrease of DI inflow due to the extension of sick pay was higher for large firms than for small firms.

Table 3.1: Descriptive statistics of the firms, 2001-2011.

	2001	2003	2005	2007	2009	2011
Number of firms (x1000)	252	216	204	123	157	152
Number of workers (x1,000)	6,803	5,908	5,582	3,214	4,108	3,534
Average firm size (workers)	27.0	27.3	27.4	26.2	26.1	23.3
% of large firms	8.4	9.4	9.4	8.2	8.9	5.6
% Pays the minimum premium	94.4	86.5	83.6	87.7	90.9	93.7
% Pays the maximum premium	2.4	4.9	7.7	8.7	6.7	4.8
Average premium (% wage sum)	1.73	2.30	1.87	0.79	0.76	0.87
Average risk percentage	0.6	2.2	2.8	2.3	2.1	1.9
Sector (%)						
- Trade	23.1	23.0	23.2	26.7	25.2	22.9
- Industrial	13.7	14.4	14.5	15.8	14.1	10.7
- Business	10.9	10.8	11.5	11.7	12.7	10.7
- Health	11.0	11.3	11.1	13.1	11.4	11.6
- Food	9.1	9.1	8.8	10.0	9.3	9.5
Worker characteristics						
Average age	36.8	37.8	38.5	38.3	38.9	39.8
Male (%)	53.1	52.4	51.6	51.2	50.3	48.1
Immigrant (%)	16.7	16.5	16.4	16.8	17.9	18.4
Permanent contract (%)	-	-	-	72.0	68.9	69.5
Pre-disability earnings (€)	19,955	21,513	22,253	23,284	26,023	27,475
Characteristics DI recipients[a]						
Number of DI recipients (x1000)	196	220	187	81	81	69
DI, % of workers	3.6	4.5	4.0	2.9	2.3	2.3
- % WAO	100	100	100	84.6	60.8	41.3
- % WGA	-	-	-	12.3	30.4	43.7
- % IVA	-	-	-	3.1	8.8	15.0
- % Fully disabled	48.8	50.2	49.0	52.0	55.9	59.1
Inflow into disability	65,861	40,828	14,267	11,043	11,381	9,559
Inflow, % of workers	1.2	0.8	0.7	0.4	0.3	0.3
Outflow from disability	22,417	22,345	22,886	5,691	4,913	4,021
Outflow, % of workers	0.4	0.4	0.5	0.2	0.1	0.1
Average annual DI benefits (€)	6,714	9,150	10,567	12,328	13,469	14,321

Note: Based on all firms with one plant, for the years 2001 to 2011 (only odd years are shown)
[a] DI statistics only include the DI spells of individuals that could be linked to a firm. If an individual has not been employed for the last five years, the DI spell is not included either. This explains why the number of worker observations is considerably smaller than the total DI inflow.

removed in 2003-2004. As an additional analysis, we also present model outcomes for the period between 2006 and 2011. With experience rating being re-introduced for small firms in 2008, this means that the treatment group in this period consists of small firms that were not experience rated in the years 2006 and 2007.

3.5.2 Identification issues

The research design for both the inflow model and the outflow model essentially relies on three identifying assumptions. First, the DiD setup assumes that the outcome measures of the treatment group and the control group share a common time trend. Second, firms should not anticipate the wage costs threshold that determines the experience-rating incentive. Finally, there should be no firms that switch over time between the treatment group en the control group.

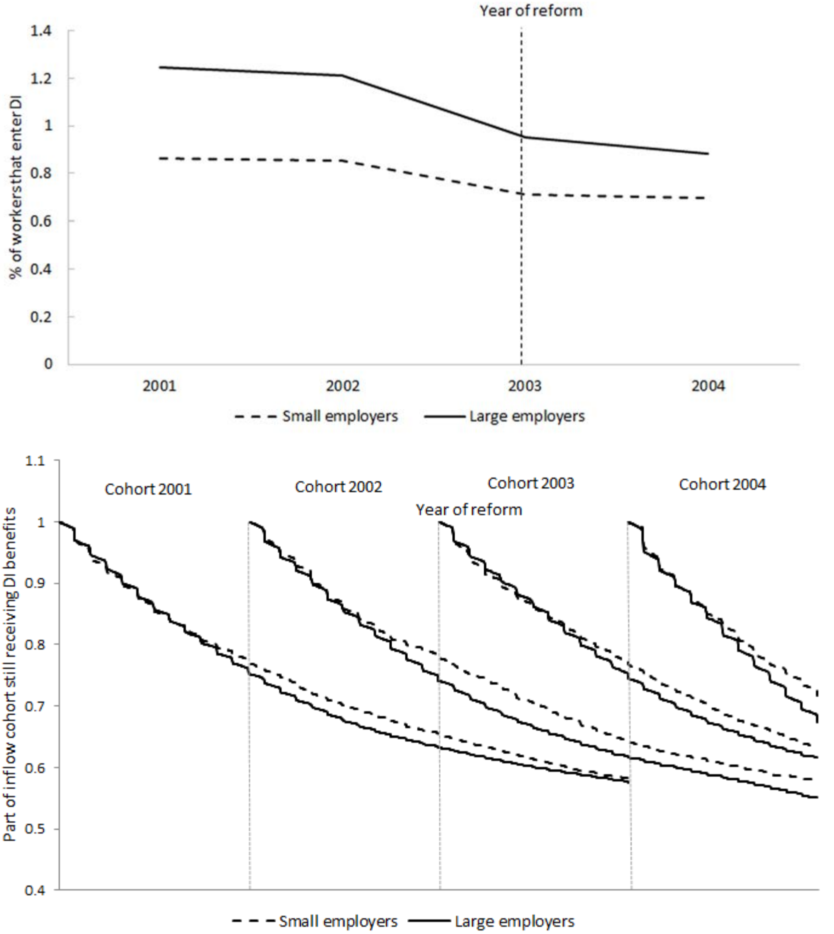
To start with, the common trends assumption implies that sick or disabled individuals who were employed at a small firm respond similarly to calendar time effects as their counterparts employed at large firms. As an eyeball test on this assumption, Figure 3.5 explores the evolution of DI inflow and DI outflow as pre-treatment trends. The upper panel portrays the inflow into DI as a percentage of the total numbers of workers for small and large firms in the years 2001-2004. Before the reform, we observe similar trends in inflow between 2001 and 2002.[8]

Similarly, the lower panel of Figure 3.5 shows the survival curves of those receiving DI by year of inflow into DI and size of the firm. We do not observe a difference in the survival curves of individuals from small and large firms between 2001 and 2002. The survival rates of individuals who worked at small firms are similar to their counterparts from large firms until the end of the first year of DI. After the first year, the survival probability of individuals who worked at a large firm drops below that of individuals who worked at a small firm. Nevertheless, more formal robustness tests are needed on time trends in DI inflow and outflow. Toward that end, we will formulate a placebo test and use samples of the treatment and control groups with more similar employer sizes.

Our second assumption is that firms do not anticipate the wage costs threshold that determines the size of the experience-rating incentive. Anticipation effects would occur if firms keep the wage costs just below the threshold to avoid experience

[8]We repeated the explanatory analysis for the small sample of firms that could be matched to UWV data, as we then observe the years 1999 and 2000. Again, we observe similar trends in inflow for small and large firms in the pre-treatment period. This figure is available from the authors upon request.

Figure 3.5: Inflow into DI (upper panel) and survival curves of DI receipt by year of DI inflow (lower panel), stratified by firm size

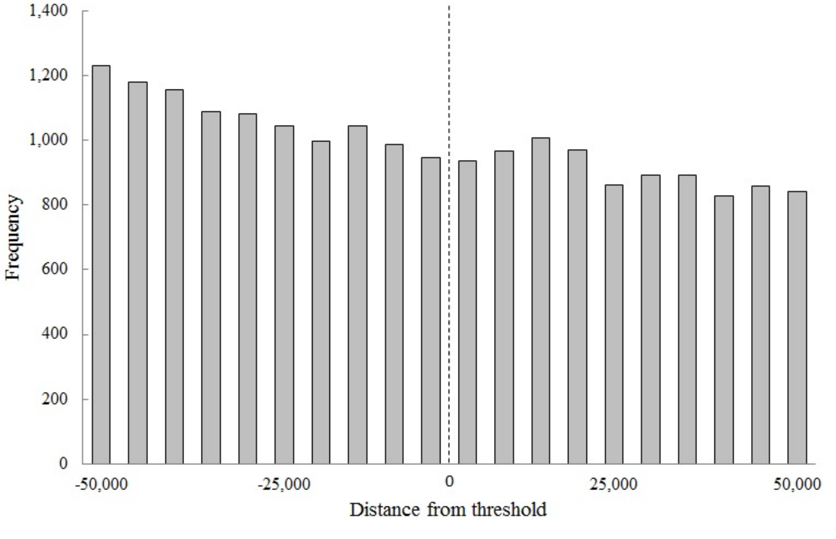


Note: Size of the firm is based on the wage costs of the firm rating, or the reverse. We argue that such effects are unlikely to exist, since the threshold, which is set in the year before the actual year of experience rating, applies to the wage costs of the two years beforehand. Moreover, the removal of experience rating for small firms in 2003 was announced in July 2002. Large firms were thus not able to decrease their wage costs to escape from experience rating.

This is confirmed by Figure 3.6, which displays the distribution of firms with total wage costs around the threshold of experience rating. In particular, there is no evidence that the wage costs of firms concentrate just below the threshold value. We also tested this formally with the discontinuity test suggested by McCrary (2008). The null hypothesis of a continuous wage sum around the threshold could not be rejected for any year between 2001 and 2011, except for 2007.[9]

Third, our estimation strategy assumes that firms are classified as small or large over a longer stretch of time. In practice, however, firms may switch from small to large in the next year, or the reverse. In this respect, recall that the thresholds for experience rating are set with a time delay. Consequently, the ex-ante incentive

Figure 3.6: Wage cost distribution of employers, stratified with intervals of €5,000 around the experience-rating threshold, aggregated over 2003-2007



[9]The McCrary test yielded a p-value of the null hypothesis of continuity in the density around the experience-rating threshold that was equal to 0.02 for the year 2007. For all other years, the p-value was well above 0.10.

effect of experience rating will be almost equal for firms with wage costs that are just below and just above the threshold. As there are many firms close to the threshold that switch between experience-rating statuses, one therefore may expect the effect estimates of experience rating to be biased towards zero. This effect applies particularly to firms with wage costs that are close to the threshold, as firms just below the experience-rating threshold are likely to be subject to experience rating in the following year (and vice versa).

To assess the size of a potential attenuation bias close to the threshold, Table 3.2 shows the percentage of firms that switched from one classification to another classification in the following year. The first two rows show the percentage of small and large firms classified as the opposite size in the following year. For small firms, this percentage is relatively small, at most 1%. We do observe a more substantial percentage of large firms that drop in the next year below the experience-rating threshold, with 7.0% of large firms at the most. When calculating the number of switches per firm, we find that the vast majority of firms never switches classification. Only 3.5% of the firms change from small to large or the other way around, and most of those firms only switch once (2.3%). We therefore expect that the bias of switching of firms is relatively small. If small firms take into account that they might be subject to experience rating the next year (or the reverse), this would cause a small underestimation of the effect of experience rating.

Table 3.2: Percentage of firms that switch from small to large (or the reverse), 2002-2011.

Actual size	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011
All firms										
Small to large	0.7	0.7	1.0	0.6	0.3	0.3	0.5	0.6	0.2	0.2
Large to small	4.8	4.3	7.0	5.6	4.8	5.0	5.6	1.6	5.6	2.9
Wage sum close to threshold[a]										
Small to large	15.2	21.4	28.6	22.1	17.8	17.4	24.2	22.9	18.5	21.7
Large to small	25.5	16.2	36.9	27.2	25.0	29.6	26.8	15.5	38.6	26.0
All	19.9	20.8	37.2	24.6	21.7	24.2	27.2	22.3	30.5	23.9

Note: The size of the firm is based on the experience-rating threshold of the wage costs. These wage costs are measured with a delay of two years. Before 2004, the experience-rating threshold was equal to 15 times the average wage; after 2004 it was equal to 25 times the average wage.

[a] Only firms with a wage sum that differs less than €100,000 from the threshold

Table 3.2 also shows that yearly switches between firm statuses are much more prominent if we zoom into wage sums that differ less than €100,000 from the threshold value. About 20% of the small firms close to the threshold are classified as a large firm in the following year, whereas the opposite holds for about 27% of the large firms. This suggests that a Regression Discontinuity design will probably underestimate the effect of DI experience rating.

3.5.3 DI inflow model

Thus far, we have discussed the assumptions that are necessary for our difference-in-difference design. We present next the empirical specification that is used to implement this design, using DI inflow and DI outflow as outcome variables of interest.

As the experience-rating incentive is directed to individual firms, we aggregate the individual data on DI inflow at the level of individual firms. An alternative would be to estimate an individual duration model for the time until inflow into DI. The main disadvantage of this approach is that we do not observe employment before 1999. We thus would have to estimate the model on a stock sample, which could lead to biased estimates.[10]

We define the inflow y_{jt}^{inflow} as the fraction of workers who worked for firm j in the year of risk ($t-1$ before 2005, $t-2$ after 2005), entering DI in year t. With the dependent variable that is expressed as a fraction of the workers per firm, we propose the fractional probit estimator described in Papke and Wooldridge (2008) that incorporates the longitudinal nature of the data. This essentially implies that the effect of the removal of experience rating is identified from 'within-firm' variation. We estimate the model using the pooled Bernoulli quasi maximum likelihood estimator, as described in Papke and Wooldridge (2008). This estimator assumes a conditional mean of the following form:

$$E(y_{jt}^{inflow}|S_{jt}^s, D_{jt}, X_{jt}, \rho_j) = \Phi(\alpha + \kappa^s S_{jt}^s + \bar{\kappa}^s \overline{S_j^s} + \delta D_{jt} + \bar{\delta}\overline{D_j} + \beta X_{jt} + \bar{\beta}\overline{X_j} + \mu_t + \rho_j) \tag{3.3}$$

where Φ is the standard normal cumulative distribution function and ρ_j is a firm effect that is assumed to follow a normal distribution, conditional on the regressors

[10]Although one may argue that biases due to stock sampling apply to both large and small firms, we cannot rule out that these biases are different. In particular, job turnover is likely to be larger for small firms. Even so, we ran a logit specification for DI inflow with individual data. We briefly discuss these results in the robustness checks in Section 3.6.

S^s_{jt}, D_{jt}, X_{jt} and μ_t.[11] α is a constant and the variable D is our treatment dummy: this variable is equal to 0 if the firm is classified as large in all years, as well as for firms that are classified as small in the years from 1999 to 2002 (before the removal of experience rating). Note that in the additional analyses for the period after 2005, the treatment variable is set to 0 from 2008 to 2011 (after the re-introduction of experience rating). Consequently, D_{jt} is set equal to one if the firm is classified as small between 2003 and 2007 and was not subject to experience rating.

Vector X_{jt} contains both firm characteristics (dummies for sector, average wage) and characteristics of the workers of the firm (average age, percentage of men, percentage of immigrants). Recall from Section 3.3 that in 2004 the threshold value of wage sums for small versus large firms was increased from 15 to 25 times the average wage per worker. In our analysis we therefore define 'medium-sized firms' as those that have a wage sum between 15 times and 25 times the average wage. For both small firms with a wage sum that is smaller than 15 times the average wage and medium-sized firms, we estimate control dummies S^1 and S^2. The time trend μ_t is specified using dummy variables for every year. This vector controls for calendar time variation in inflow probabilities and is identified by the control group of large firms. $\overline{S^s_j}$, $\overline{D_j}$ and $\overline{X_j}$ are the time-averages of S^s_{jt}, D_{jt} and X_{jt} for firm j.

In our regression we cluster the standard errors at the level of the firm and obtain them using 500 bootstrap replications. Unfortunately, there is no validated method existing at present to estimate the fractional probit model on an unbalanced sample. We therefore estimate the model on a balanced sample of firms.[12]

3.5.4 DI outflow model

To estimate the effect of experience rating on DI outflow, we use data on the level of the individual workers instead of firms. We thus avoid losing individual information on DI durations that would occur if we aggregate the outflow to the level of firms. We model the duration of DI benefits on a flow sample of individuals entering DI by using a hazard rate model with a Cox proportional hazard specification that can be estimated with standard Maximum Likelihood techniques:

[11] See Papke and Wooldridge (2008) for a derivation of this conditional mean.

[12] We did estimate the fractional probit model on the unbalanced panel following the method proposed in Wooldridge (2010). The main conclusions do not change when using these estimation results.

$$y_{ij\tau,t}^{outflow} = \lambda(t)exp(\kappa^s S_{jt}^s + \delta^{1st}D_{jt}^{1st} + \delta^{2nd}D_{jt}^{2nd} + \beta X_{ijt} + \mu_\tau) \quad (3.4)$$

where $y_{ij\tau,t}^{outflow}$ denotes the outflow hazard on day t for an individual i who entered DI at calendar time τ and worked for firm j before entering DI. $\lambda(t)$ represents the duration dependence in outflow from DI benefits. Again we include two firm-size dummies S_{jt}^s to control for the size of the firm (based on the total wage costs), as well as dummies for the year of inflow μ_τ. X_{ijt} includes both firm characteristics (i.e., sector and average wage of the firm) as well as worker characteristics (i.e., gender, immigrant, wage category, regional and household status). We allow the potential effect of experience rating to vary with respect to the DI duration, allowing for distinct treatment effects in the first (D_{jt}^{1st}) and second year of DI benefit receipt (D_{jt}^{2nd}).

3.6 Estimation results

3.6.1 Baseline specification

Table 3.3 shows the main estimation results for the fractional probit model for DI inflow, which is measured as a percentage of the workers at the firm (see columns two and three, respectively). A table with all coefficient estimates can be found in the appendix to this chapter.

Our key finding is that the removal of experience rating increased DI inflow in the period prior to 2005. The implied average partial effect of experience rating for small firms in this period is equal to an increase of 0.00051 in the annual DI inflow rate. As the average annual DI inflow rate for small firms equaled 0.0074 before the removal of experience rating, this implies a relative increase of 7%. This effect corresponds to about half of the size of the effect found by Koning (2009) and Van Sonsbeek and Gradus (2013). One explanation for this difference may be that the effects of experience rating are smaller for the treatment group of small firms than for the control group of large firms. Like Koning (2009), one may also argue that firms typically responded to unanticipated increases in premiums, rather than being fully informed and this able to anticipate the incentives.[13]

[13] The study of Korkeamäki and Kyyrä (2012) supports this hypothesis. They estimate the effect of a lump-sum payment by employers at the moment of DI entry. This effect is markedly larger than the effect of conventional experience-rating systems.

In a broader perspective, our results are comparable to those obtained by Campolieti et al. (2006) and Hyatt and Thomason (1998) for Workers' Compensation in Canada. Moreover, the coefficient estimates of the control variables are in line with expectations (see appendix 3.A: firms with older workers, a lower average wage and operating in the sectors construction and transport have a higher inflow into DI).

As to the estimation of effects on DI outflow, recall that we use data on individuals who entered the DI scheme between 2001 and 2004 and who can be assigned to a particular firm, and then estimate the DI duration using a Cox proportional hazard specification. The resulting coefficient estimates are given in columns four and five of Table 3.3. Loosely speaking, the coefficient values that are presented in the fourth column can be interpreted as a percentage increase or decrease in the exit rate out of DI. Again, a full table that includes all estimated coefficients can be found in the appendix to this chapter.

In line with expectations, the coefficient values of the removal of experience rating on DI outflow are negative. This implies that the removal of DI experience rating decreases the probability of an exit from DI, and thus increases the DI duration. Still, we find a significant impact only for the first year of DI benefit receipt. Our impact estimates correspond with a drop in the DI exit probability of 3.0 percentage points after one year (from 24.7% to 21.7%) and of 4.7 percentage points after two years (from 34.1% to 28.4%). These results correspond roughly to those of Van Sonsbeek and Gradus (2013), who find a positive, borderline significant effect of experience rating on DI outflow.

Our estimates indicate that individuals who worked for small firms are less likely to exit DI. Arguably, small firms may have fewer possibilities to arrange work adaptations or to offer job opportunities elsewhere. Conditional on work resumption, the probability of employment at the previous employer is about 50%. Finally, the remaining control variables of the DI outflow model are again in line with expectations: older individuals, women, immigrants, individuals with a low previous wage, single parents and individuals without children are less likely to exit DI.

The estimated effects of the removal of experience rating for small firms on both DI inflow and outflow can be translated into an effect on the total DI stock. In particular, the estimates imply that the total DI stock in 2004 was 0.4% larger because of the removal of experience rating for small firms. About two-thirds of this effect can be attributed to the effect on DI inflow, and one-third to the DI outflow effect. Assuming that the effects of experience rating on DI inflow and outflow are similar for large firms, the DI stock in 2004 would have been 1.7% larger if the removal of experience rating had been implemented for all firms.

Equipped with the individual information of employed workers and DI recipients, we are able to stratify the effect of experience rating with respect to various worker characteristics. Table 3.4 shows the coefficient estimates of the removal of experience rating for individuals with different degrees of disability and for different levels of DI benefits. The estimation results of the DI inflow model show no significant differences in effects between worker groups, which is probably due to the fact that (share) variables are calculated per firm. As to DI outflow, we find the experience-rating effect to be confined to partially disabled workers only. This suggests that the effects of experience rating are strongest for individuals with some job possibilities. Also, DI outflow effects are larger for workers with low pre-disability wages.[14]

3.6.2 Robustness analyses

In this subsection, we assess in greater detail our estimation strategy for both DI inflow and DI outflow effects. The results of the corresponding robustness analyses are presented in Table 3.5.

Table 3.3: Estimates of the effect of the removal of experience rating on DI inflow and DI outflow: Baseline specification

	Inflow		Outflow	
Removal of ER	0.027**	(0.009)	-	-
Removal of ER, first year after inflow	-	-	-0.154**	(0.022)
Removal of ER, second year after inflow	-	-	-0.039	(0.024)
Small firm	0.041	(0.040)	-0.037**	(0.014)
Middle-sized firm	0.040	(0.024)	0.029	(0.019)
Year effects	Yes		Yes	
Worker characteristics	No		Yes	
Firm characteristics	Yes		No	
Sector dummies	Yes		Yes	
Regional dummies	No		Yes	
Observations	183,665		119,631	

Note: Fractional probit estimates (quasi-MLE) for the fraction of workers per firm that is awarded with DI benefits (2001-2004) and Cox proportional hazard estimates (no hazard ratios) of outflow from DI for individuals who entered DI between 2001 and 2004. Standard errors between parentheses, for inflow estimations obtained using bootstrap with 500 replications. * significant at a level of 10%, ** significant at a level of 5%.

[14] Note that the coefficient estimates of the removal of experience rating do not differ across gender, age or sector that corresponds to the last job before the start of a DI spell.

Table 3.4: Estimates of the effect of the removal of experience rating on DI inflow and DI outflow: Heterogeneity

	DI inflow		DI outflow			
			First year		Second year	
Baseline specification	0.027**	(0.009)	-0.154**	(0.022)	-0.039	(0.024)
By degree of DI						
DI <=35 %	-0.075	(0.077)	-0.270**	(0.056)	0.023	(0.056)
DI 35-80 %	0.012	(0.040)	-0.297**	(0.069)	0.035	(0.069)
DI > 80%	0.034	(0.053)	-0.048	(0.040)	-0.002	(0.041)
By level of DI						
Below the median	-0.031	(0.027)	-0.191**	(0.036)	0.028	(0.036)
Above the median	0.140	(0.148)	-0.103**	(0.052)	0.058	(0.053)

Note: Every cell represents a separate analysis.Estimations include the same control variables as in the main analysis. Standard errors between parentheses, for inflow estimations obtained using bootstrap with 500 replications. * significant at a level of 10%, ** significant at a level of 5%.

First, we focus on the selection of firms that is used in our analyses. So far, we have restricted our sample to firms with one plant only, so as to exclude firms for which it was impossible to ascertain whether or not they were experience rated. As a robustness check on the DI inflow and DI outflow models, we therefore expanded our sample with firms that have multiple plants. We thus aggregated the wage costs for firms with multiple plants. We next assumed that the total wage costs determine whether or not the plants of these firms are experience rated. The first lines of Table 3.5 show that adding firms with multiple plants to our data in this way does not substantially change our estimation results for either model.

Second, our estimation strategy relies on the assumption that small firms (i.e. those without experience rating in 2003 and 2004) share a common trend with large firms. Although our graphical analyses in the previous section did not reveal substantial differences in the trends between small and large firms, we can also perform formal analyses by adapting our sample of firms and adjusting model specifications. One simple test on the common trends assumption is to exclude firms with wage costs extending far beyond the experience-rating threshold. We do so by including only firms with more than five and less than 250 workers. We thus relax the common trends assumption, since firms in the treatment and control group become more comparable. Table 3.5 shows that this causes coefficient estimates to decrease somewhat, while the coefficient estimates for the DI outflow model do not change significantly.

Adding another robustness check on the common trends assumption, we also performed a placebo test on the experience-rating incentive. By pretending that the removal of experience rating for small firms occurred in 2001 instead of 2003, we thus created a placebo dummy that is equal to one if the firm is small in the years 2001 or 2002.[15] We substituted the treatment variable by the placebo variable and re-estimated our model for the years 1999-2002. For both outcome measures, Table 3.5 shows that this yields insignificant estimates for the placebo variables.

Third, one may argue that the impact estimate of experience rating on DI outflow can be considered as a lower bound. Higher DI inflow rates for the treatment group of smaller firms may have affected the composition of DI recipients, with the additional inflow consisting of individuals with better job prospects and, consequently, higher DI exit probabilities. We test for the potential importance of these compositional effects by concentrating on a stock sample of individuals who entered DI before 2003, which is the year the reform took place. As the fourth panel of Table 3.5 shows, this yields substantially stronger impact estimates of experience rating on DI outflow. From this we conclude that compositional effects do attenuate the impact of experience rating on DI outflow levels.

Fourth, we also investigated the pattern of DI outflow effects by adopting a more refined specification of incentive effects, using intervals of six months instead of one year of DI benefit receipt. We then find significant and similar effects on outflow for the first one and a half years after DI inflow. Experience-rating effects become insignificant in the second half year of the second year, suggesting that, over time, the impact is hump-shaped.

Finally, we re-estimated the DI inflow model with individual instead of firm data, while using a logit specification. When interpreting these findings, one should bear in mind that we do not control for the employment duration of workers. The lower part of Table 3.5 shows the coefficient estimate of the removal of experience rating that follows from this strategy. In particular, we then find that the removal of experience rating increased DI inflow by roughly 15%. This is more than two times larger than the fractional probit estimate. One explanation may involve the oversampling of individuals from (very) large firms, which may violate the common trends assumption. We therefore repeated the estimation omitting individuals from very small firms (fewer than five workers) and large firms (with more than 250

[15]Since we need information on the years before 2001, we use data from UWV to measure the size of the firm for all outcome measures. The downside to this data set is that we can only account for the firms that still existed in 2009. For this reason we do not use this data set in the main analyses.

workers). As a result, the estimated effect significantly reduces in size and no longer differs significantly from the estimate based on firm-level data.

Table 3.5: Estimates of the effect of the removal of experience rating on DI inflow and DI outflow: Robustness tests

	DI inflow		DI outflow			
			First year		Second year	
Baseline specification	0.027**	(0.009)	-0.154**	(0.022)	-0.039	(0.024)
Selection of firms						
All firms (multiple plants)	0.028**	(0.008)	-0.140**	(0.017)	-0.059**	(0.021)
Test common trend, firm selection						
Without very small firms [a]	0.020**	(0.007)	-0.166**	(0.031)	0.037	(0.031)
Without very large firms [b]	0.026**	(0.026)	-0.136**	(0.032)	0.033	(0.033)
Without very small and large firms	0.014**	(0.007)	-0.152**	(0.034)	0.049	(0.035)
Test common trend, placebo test [c]						
Placebo variable	-0.011	(0.049)	-0.033	(0.061)	0.112	(0.076)
Selection of inflow						
Stock sample before 2003	-	-	-0.342**	(0.047)	-0.060*	(0.033)
Separate effects for first and second half of the year						
First half	-	-	-0.104**	(0.027)	-0.096**	(0.031)
Second half	-	-	-0.219**	(0.030)	0.037	(0.034)
Individual data						
Logit (coefficient)	0.1530**	(0.0137)	-		-	
Logit, without small and large firms	0.0993**	(0.0175)	-		-	

Note: Every cell represents a separate analysis. Estimations include the same control variables as in the main analysis. Standard errors between parentheses, for inflow estimations obtained using bootstrap with 500 replications. * significant at a level of 10%, ** significant at a level of 5%.
[a] Fewer than five workers; [b] More than 250 workers; [c] based on data UWV, 1999-2002

3.6.3 Additional analyses

The effect of premium caps

So far we have assumed that the effect of experience rating does not depend on the level of the experience-rated DI premium, but applies to all firms in the control group equally. However, we explained earlier that premia are capped at minimum and maximum rates, causing experience-rating incentives along the premium distribution to differ at the margin. In particular, firms with premiums that are capped at the maximum premium have no incentive to curb new DI inflow.

To estimate the importance of adverse effects of the maximum premium, we calculated the experience-rated DI premium rates for firms in our sample.[16] This sample does not include the treatment group of small firms that were not experience rated in 2003 and 2004; for this group, we estimate a separate dummy. If firms are aware they are paying the maximum premium, one would expect experience-rated firms paying the maximum premium to have higher DI inflow rates and lower DI outflow rates than those firms paying premiums below the maximum.

Clearly, the effect of paying the maximum premium on DI inflow and DI outflow is subject to endogeneity bias. Firms with few prevention and reintegration activities have higher DI risks and higher corresponding DI premiums – and thus exhibit a higher likelihood of paying the maximum premium. To avoid this endogeneity problem, we estimate model specifications for DI inflow and DI outflow conditioning the initial DI risk of a firm. More specifically, we extend our model by including a (third order) polynomial of DI risks. The impact of the maximum premium can thus be identified as a Regression Discontinuity effect at a certain level of the DI risk.

Table 3.6 shows the estimation results that follow from this estimation approach for both the DI inflow model and the DI outflow model. For the DI inflow model we find a strong discontinuity effect for experience-rated firms with maximum premiums. This impact is substantial when compared to other estimates. However, account should be taken of the fact that only a minority of firms pays the maximum premium, which implies also that local treatment effects will apply only to a specific group of firms as well. In line with our earlier results, we also find DI inflow rates to be higher for the group of firms that is not experience rated. As to DI outflow, Table 3.6 also shows disincentive effects of the maximum premium. These effects are comparable in size to the effect of the removal of experience rating.

Experience-rating effects after 2005

We argued earlier that the reforms taking place after 2004 have changed the size as well as the composition of the cohort of (new) DI recipients in ways that may well have been different for the treatment and control group of firms. This is the reason why we restricted our analysis from 2001 to 2004. Still, we are able to perform a similar DiD analysis for the period between 2006 and 2011, which includes the re-introduction of experience rating for small firms in 2008. In this context, the treatment is defined as the absence of experience rating in 2006 and 2007. As the

[16] Because we do not observe exactly the same information as UWV had when they calculated the premiums, the constructed DI risk and DI premium may be subject to measurement error.

Table 3.6: Estimates of the effect of the removal of experience rating on DI inflow and DI outflow: Premium caps

	DI inflow		DI outflow			
			First year		Second year	
Baseline specification	0.027**	(0.009)	-0.154**	(0.022)	-0.039	(0.024)
Estimation with interaction terms and risk premium						
Reference: pays premium below max	-	-	-	-	-	-
Pays the maximum premium	0.111**	(0.023)	-0.128**	(0.025)		
Removal of ER	0.030**	(0.005)	-0.166**	(0.022)	-0.051**	(0.024)
Risk percentage	0.081**	(0.039)	-0.054	(0.034)		
Risk percentage^2	-0.002	(0.005)	0.0004*	(0.0002)		
Risk percentage^3 (x10)	0.001	(0.001)	-0.0001*	(0.00003)		

Note: Estimations include the same control variables as in the main analysis. Standard errors between parentheses, for inflow estimations obtained using bootstrap with 500 replications. * significant at a level of 10%, ** significant at a level of 5%.

common trends assumption may well be more restrictive in the period after 2005, estimation results should be taken with caution (see Section 3.5.1).

Table 3.7 presents the coefficient estimate of the removal of experience rating that follows from this research design for 2006-2011, compared to the coefficient estimate that was obtained for the period before 2005. For both the DI inflow and DI outflow models, we find the effects of the removal of experience rating to be insignificant for the period after 2005. This suggests that firms became unresponsive to the experience-rating incentive. When interpreting this finding, recall that the DI scheme and the incentive of DI experience rating differ between the periods before and after 2005 at least in three ways. First, in the new DI scheme that started in 2006, experience rating no longer applies to individuals with a disability degree of less than 35% − as these are excluded from DI benefits in the new scheme. It is likely that this change increased the share of workers in DI with bad job prospects. Second, in 2005, the period of continued wage payments during sickness was extended from one to two years. This reform may have decreased the (additional) effect of experience rating as well, as re-employment probabilities usually decrease over time. Third, both the range of the experience-rating premiums as well as the level of the maximum premiums decreased substantially after 2005 (see Figure 3.3), causing the effective impact of the experience-rated premium on the employers wage costs to decrease accordingly.

With this in mind, the pertinent question is how changes in the size and composition of the DI inflow since 2005 have affected the impact of experience rating. To shed light on this question, we re-estimated our benchmark model for the pre-2005 period for the sample of workers that would still be entitled to DI benefits in the post-2005 period. Stated differently, we exclude from our sample those workers who would no longer have been entitled to DI benefits in the post-2005 period.

When following this strategy, we obtain coefficient estimates for the DI inflow and DI outflow models that are presented in the lower panel of Table 3.7. According to the table, the exclusion of workers with disability degrees below 35% does not significantly affect our model estimates for the DI inflow and DI outflow models. When excluding workers with DI spells that are shorter than one year, however, the effect estimates for the pre-2005 period become significantly smaller. The average partial effect on DI inflow drops from 0.0005 to 0.0003, whereas the effect on DI outflow in the first year becomes insignificant. This suggests that the lower impact of DI experience in the post-2005 period is partially due to the extension of the sickness period that precedes DI.[17]

Table 3.7: Estimates of the effect of the removal of experience rating on DI inflow and DI outflow: Before and after 2005

	DI inflow		DI outflow			
			First year		Second year	
Before 2005	0.0005**	(0.0002)	-0.154**	(0.022)	-0.039	(0.024)
After 2005	0.0001	(0.0001)	0.068	(0.079)	0.053	(0.137)
Before 2005, different samples:						
Exclusion DI spells <=35%	0.0005**	(0.0001)	-0.106**	(0.034)	0.016	(0.034)
Expansion sick leave period, >35%	0.0003**	(0.0001)	-0.047	(0.034)	0.084**	(0.040)

Note: Average partial effects for DI inflow. Every cell represents a different estimation. Estimations include the same control variables as in the main analysis. * significant at a level of 10%, ** significant at a level of 5%.

3.7 Conclusion

This chapter studies the effect of firm experience rating on DI inflow and DI outflow in the Netherlands, using matched firm- and worker data. We exploit the removal of experience rating for small firms in 2003, which allows us to use a difference-in-difference design. Our focus is on the period until 2005, as there were other reforms

[17]At the same time, there are reasons to believe that the impact of the extension may be underestimated. In particular, it is likely that financial incentives due to wage continuation in the sickness period are perceived by employers as more direct than the delayed impact of experience rating.

in 2005 in 2006 that may well have affected small and large firms in different ways. In particular, the 2005 reform extended the sickness benefit period that precedes DI claims from one to two years, and the 2006 reform split the disability scheme into separate schemes for permanently and fully disabled individuals and for temporarily and/or partially disabled individuals.

Our main finding is that the removal of experience rating in 2003 increased the DI inflow for small firms by about 7%, whereas DI outflow of individuals from small firms decreased by about 12%. We estimate that the DI stock in 2004 was 0.4% larger because of the reform. As to DI inflow, our results are about half the size of the effects on inflow found by Koning (2009) and Van Sonsbeek and Gradus (2013). Moreover, there is strong evidence that the decrease in DI outflow for the treatment group of small firms is confined to partially disabled workers and workers with relatively high DI benefits. Interestingly, we also find evidence that the cap that was used for experience-rated premiums had substantial disincentive effects. That is, firms paying the maximum premium had higher DI inflow rates and lower DI exit rates, suggesting that they responded to the absence of prevention and reintegration incentives (at the margin).

We also broadened our perspective by assessing the specific context that may or may not have contributed to the effectiveness of experience rating. We thus estimated our model for the period after 2005, exploiting the re-introduction of experience rating for small firms in 2008. We then found no evidence of experience-rating effects, on either DI inflow or DI outflow. To investigate the potential role of post-2005 reforms in explaining these outcomes, we re-estimated our benchmark model for the pre-2005 period omitting the workers that would no longer have been entitled to DI benefits in the post-2005 period. Based on this analysis, we argue that particularly the extension of the sickness benefit period from one to two years has lowered the potential impact of experience rating on both DI inflow and DI outflow.

3.A Appendix: Full estimation results of baseline specifications

Table 3.A1: Fractional probit estimations for the fraction of workers per firm that receives with DI benefits (2001-2004) and Cox proportional hazard estimates of outflow from DI, for individuals who entered DI between 2001 and 2004.

	DI Inflow		DI Outflow	
Effects Experience Rating				
Removal of ER	0.027**	(0.009)	-	-
Removal of ER, first year after inflow	-	-	-0.154**	(0.022)
Removal of ER, second year after inflow	-	-	-0.039	(0.024)
Firm characteristics				
Small firm	0.041	(0.040)	-0.037**	(0.014)
Middle-sized firm	0.040	(0.024)	0.029	(0.019)
Average age	0.007**	(0.001)	-	-
Percentage of men	-0.031	(0.047)	-	-
Percentage of immigrants	0.063	(0.056)	-	-
Percentage of single households	0.054	(0.040)	-	-
Percentage of single parents	0.031	(0.048)	-	-
Percentage of parents	0.089**	(0.019)	-	-
Annual wage below € 7,500	0.372**	(0.047)	-	-
Annual wage € 7,500-15,000	0.333**	(0.044)	-	-
Annual wage € 15,000-25,000	0.255**	(0.042)	-	-
Annual wage € 25,000-40,000	0.164**	(0.040)	-	-
Sector				
- Agriculture	0.089**	(0.019)	-0.029	(0.031)
- Industry	0.180**	(0.014)	-0.104**	(0.032)
- Government	0.131**	(0.013)	-0.025	(0.033)
- Construction	0.375**	(0.015)	-0.183**	(0.038)
- Trade	0.130**	(0.013)	0.013	(0.032)
- Food	0.033**	(0.017)	-0.019	(0.035)
- Transport	0.222**	(0.019)	0.133**	(0.035)
- Financial	0.255**	(0.061)	0.253**	(0.057)
- Business	0.116**	(0.015)	-0.055*	(0.033)
- Education	0.095**	(0.017)	-0.065*	(0.034)
- Health care	0.110**	(0.015)	-0.008	(0.031)

	DI Inflow		DI Outflow	
Worker characteristics				
Age, 25-35	-	-	-0.086**	(0.024)
Age, 35-45	-	-	-0.291**	(0.024)
Age, 45-55	-	-	-0.592**	(0.024)
Age, 55-65	-	-	-0.771**	(0.025)
Male	-	-	0.005	(0.010)
Single household	-	-	0.026	(0.033)
Couple	-	-	-0.029	(0.032)
Single parent	-	-	0.050	(0.035)
Has children	-	-	0.152**	(0.010)
Wage, € 10,000-20,000	-	-	0.052**	(0.011)
Wage, € 20,000-30,000	-	-	0.114**	(0.012)
Wage, € 30,000-40,000	-	-	0.226**	(0.016)
Wage, € 40,000-50,000	-	-	0.249**	(0.025)
Wage, > € 50,000	-	-	0.189**	(0.022)
Year effects	Yes		Yes	
Regional dummies	Yes		Yes	
Observations	183,665		119,631	
Log pseudolikelihood	-30,352		-689,144	

CHAPTER 4

A burden too big to bear? The effects of Disability Insurance experience rating on firm exits and layoffs.*

4.1 Introduction

In the literature, there is a general consensus that employers may play a key role in facilitating the return to work from sickness in order to prevent a deterioration of health conditions (Autor (2015), Burkhauser and Daly (2011)). Firms may implement work adaptations, arrange flexible working hours, provide assistive technologies or facilitate vocational rehabilitation. Moreover, firms may monitor sickness and reduce occupational risks. The evidence also suggests that firms do respond to experience rating by reducing the inflow in the Disability Insurance (DI) scheme – see recent evidence by e.g. De Groot and Koning (2016), Van Sonsbeek and Gradus (2013) and Korkeamäki and Kyyrä (2012). This provides an argument for the use of employer incentives to reduce sick pay and DI costs, with experience rating – i.e. the setting of firm premiums that reflect their benefit costs – as the most common one that is used in practice.

In light of these arguments, it may come as a surprise that the actual implementation of experience rating in DI schemes is limited, with the Netherlands and

*This chapter is based on De Groot and Koning (2017).

Finland as the only two countries which apply experience rating for public DI schemes. Experience rating is more common for Unemployment Insurance (UI) schemes or Workers' Compensation (WC) schemes. For public DI schemes, however, policymakers argue that firms may have to bear financial risks that are beyond their scope of control, particularly benefit costs that stem from non-occupational diseases or worker moral hazard (Autor (2015)). In effect, financial risks arising from experience rating may become disproportionately large, causing a higher likelihood of financial distress. Another argument against DI experience rating is that it may increase health screening activities by firms when hiring workers. This in turn would harm the employment opportunities of individuals with bad health.

To our knowledge, this study is the first to investigate whether DI experience rating affects the probability of firm exits and worker layoffs. We focus on the DI experience-rating system that was used between 1999 and 2013 in the Netherlands; during this period, firms paid experience-rated DI premiums that could amount to 9% of all labor costs at maximum. We test whether large increases in DI premiums have caused financial distress amongst firms, or triggered firms to reduce labor costs by increasing layoffs. More specifically, we estimate the effect of a positive premium adjustment due to experience rating on the probability of a bankruptcy, a merger or a restart as a new firm. In a similar way, we investigate the effect of changes in DI premiums on the inflow into UI and other non-experience-rated DI benefits.

To identify the effect of a premium adjustment due to experience rating, we exploit specific features of the Dutch DI experience-rating system that may be considered as exogenous to the firm. Most importantly, we use the removal of experience rating for firms between 2003 and 2007 for firms that were classified as 'small' by the Employee Insurance Agency that sets DI premiums. This reform allows us to use a difference-in-difference design, wherein disability risks of large firms continued to be experience rated and 'treated' small firms paid uniform premiums between 2003 and 2007. In addition, the identification of the effect of premium adjustments follows from other year-to-year changes in the mapping of disability risks to experience-rated premiums. For our analysis, we use matched administrative data from Statistics Netherlands on Dutch firms and workers. We enrich this data with detailed information on firm exits, DI spells of former workers, as well as other demographic and labor market characteristics.

We find that a premium adjustment of one percentage point of the wage sum increases the probability of a firm exit with 0.8 percentage point.[1] This effect consists

[1] In the period under investigation, the observed standard deviation of the experience-rated premium is 0.86 percentage point.

both of increases in firm bankruptcies and increases in mergers of firms. As mergers implied the loss of benefit costs of former workers that were initially assigned to the firms, the increase in mergers may point at strategic behavior amongst firms. At the same time, however, we do not find premium effects on the likelihood of exiting and restarting as a new firm, which would have been an alternative response of firms to avoid future DI costs.

We also find evidence that firms responded to positive premium adjustments by increased layoffs. In particular, an upward adjustment of one percentage point in the experience-rated DI premium, increases the inflow into UI with 0.12 percentage point. This effect is largely confined to workers entering the UI scheme after a firm exit; the effect on separations of firms that continued to exist is considerably smaller. Finally, we find no evidence of substitution effects to the non-experience-rated DI scheme for permanent and full disability.

Our analysis relates to two strands of literature. To start with, we add to studies on the effects of DI experience rating, with DI inflow and outflow as the most prominent outcome measures. For the Netherlands, De Groot and Koning (2016) find that experience rating reduces the inflow into DI by about 7% and increases the outflow from DI by about 12%. These effects are confined to the period where the sickness benefit period preceding DI benefits was one year; after the extension of the sickness benefit period from one to two years in 2006, incentive effects are no longer significant. In earlier papers, Koning (2009) and Van Sonsbeek and Gradus (2013) both estimate the effect of experience rating on Dutch DI inflow to be about 15%. For Finland, Korkeamäki and Kyyrä (2012) find significant negative effects of experience rating for older workers on both the inflow into sick leave and the transition from sick leave to disability retirement.[2]

This study is also related to a broader strand of literature that addresses the adverse effects of experience rating in UI schemes and WC schemes, such as increased firm exits or reduced hiring of workers with health problems. Similar to our analysis, Johnston (2016) investigates the direct impact of experience rating on firm exits – but then in the context of UI schemes. Using data from the US, Johnston (2016) finds that a one percentage point increase in the UI tax rate increases the probability of firm exits by 0.3 percentage point. In addition, there is evidence that UI experience rating or dismissal costs may reduce the entry rate of firms (see e.g. Kugler and Pica (2008)) or may reduce hiring probabilities of affected workers (Behaghel et al. (2008)).

[2]In the literature that addresses experience rating in WC schemes, there is evidence of reduced DI benefit costs (see Hyatt and Thomason (1998) or Ruser and Butler (2010) for survey studies).

In the context of WC experience rating, studies have found evidence indicating that firms may increase claims control or increase pressure on workers not to report injuries (Kralj (1994), Ison (1986), Lippel (1999), Strunin and Boden (2004)).

The remainder of this chapter proceeds as follows. In the next section we describe the Dutch DI and the experience-rating system. Section 4.3 describes the data. In Section 4.4 we present the model and results of the firm exits estimations, while we show the results of the analyses for layoffs and the inflow in other non-experience-rated schemes in Section 4.5. Section 4.6 concludes.

4.2 Institutional setting

This section provides insight in the key features of the Dutch DI system and the system of DI experience rating. We refer to De Groot and Koning (2016) and Koning and Lindeboom (2015) for a more detailed description.

4.2.1 Disability Insurance and experience rating

In the Netherlands, the DI program covers all workers against all injuries, so also including those resulting from non-occupational risks. Prior to DI benefit receipt, workers receive sick pay benefits for one year. Employers are financially responsible for the continuation of wages during this period. In 2005, the maximum sick pay period was extended from one to two years, with the aim of further increasing employer incentives to prevent long term sickness and inflow into the DI scheme.

If a worker is not able to work at the end of the sick leave period, he or she can apply for DI benefits. DI claims are assessed by the public benefit administration called UWV (In Dutch: Uitvoeringsinstituut Werknemersverzekeringen), which can be roughly translated as the Employee Insurance Agency. Disability is measured as a percentage of pre-disability earnings, rather than an all-or-nothing condition. Until 2006, workers were eligible to (partial) DI benefits if their degree of disability was at least 15% of their pre-disability wages. As of 2006, the threshold for DI receipt has been raised to 35%. Moreover, the DI program distinguishes two schemes since 2006: one scheme for individuals who are fully and permanently disabled called IVA ("Income scheme for Fully Disabled") and the other scheme for workers that are classified as partially or temporary disabled called WGA ("Act for Partially Disabled workers").

Since 1998, DI benefit costs in the Netherlands are experience rated.[3] Experience rating only applies to (former) workers who had a permanent contract, whereas the DI costs of workers with a temporary contract are covered by non-experience-rated premiums. Although the coverage of experience rating changed over the years that are under investigation in this chapter, the setup of calculating of DI premiums for employers has remained unaffected. That is, firms' disability risks are translated into DI premiums. The firm's disability risk is defined as the sum of DI benefit costs of workers that have enrolled in the scheme, measured as a percentage of the average wage sum of workers over a time window:

$$d_{it} = \frac{\sum_{s=0}^{T} S_{t-2,t-2-s}}{\sum_{s=0}^{T} W_{t-2-s}/(T+1)} \ , \tag{4.1}$$

where d_{it} is the DI risk of firm i in year t, $S_{t-2,\tau}$ are the disability costs in year $t-2$ of individuals that entered into the program at time τ and W_{t-2} is the wage sum of workers in year $t-2$. The equation makes apparent that DI benefit costs and wage sums are used with a delay of two years. In addition, there is a time window of $T+1$ years for cohorts of DI benefit costs of workers that are enrolled; workers that entered into DI earlier than that are not covered in the experience rating scheme. This time window is also used to calculate the average wage sum of the firm in the denominator of equation 4.1. In 1998, the time window was set equal to five years.[4]

With the firm's disability risk d_{it}, we can first calculate DI premium rates that would result in the absence of minimum and maximum premium rates. This unconstrained experience-rated premium follows from the comparison of the individual DI risk and the average DI risk D_t over all employers:

$$p_{it}^{u} = \overline{p}_t + f_t(d_{it} - D_t) \tag{4.2}$$

[3]The start of the experience rating scheme in 1998 was accompanied by the possibility for firms of opting-out of the public DI system to private insurance providers. We do not observe if a firm opted out, so firms which opted-out of the public DI system are still included in the analyses. As private insurers bear the financial responsibility for employers that opted out, the effect of experience-rated premiums may thus have been cushioned and we might underestimate the true effect of the experience-rated premium. Still, we expect the bias to be small given that the vast majority of firms in our sample did not opt out in the time period under investigation (Deelen (2005)).

[4]Particularly for firms that were established recently, information on DI benefit costs or wage sums may be missing for some years in the disability risk formula. In those cases, DI benefit costs and wage sum are calculated over the longest available time window, and rescaled to a time window of five years.

In this expression, $\overline{p_t}$ equals the average yearly premium that follows from DI benefit costs of enrollees in the relevant time window of cohorts. This average premium also covers benefit costs that are not experience rated, either due to firm exits or due to incomplete rating for firms paying maximum premiums. On top of the average premium $\overline{p_{it}}$, firms pay a variable premium rate that equals the the difference between the individual DI risk and the average DI risk in a particular year. This difference, known as the 'premium adjustment', is multiplied by f_t, so as to correct for differences in actual DI benefit costs and DI benefit costs of $t-2$.[5]

As a final step in the calculation, the DI premiums are constrained by minimum and maximum premiums. With minimum and maximum premiums depending on the total wage sum of firms, this means we have five possible outcomes for the DI premium:

$$p_{it} = \begin{cases} P_{t,S}^{min}, & \text{if } p_{it}^u < P_{t,S}^{min} \text{ and } \ W_{it-2} < \overline{W}_{t-2} \\ P_{t,L}^{min}, & \text{if } p_{it}^u < P_{t,L}^{min} \text{ and } \ W_{it-2} \geq \overline{W}_{t-2} \\ p_{it}^u, & \text{if } p_{it}^u \geq P_{t,S}^{min} \text{ and } \ p_{it}^u < P_S^{max} \text{ and } W_{it-2} < \overline{W}_{t-2} \\ p_{it}^u, & \text{if } p_{it}^u \geq P_{t,L}^{min} \text{ and } \ p_{it}^u < P_L^{max} \text{ and } W_{it-2} \geq \overline{W}_{t-2} \\ P_{t,S}^{max}, & \text{if } p_{it}^u \geq P_{t,S}^{max} \text{ and } \ W_{it-2} < \overline{W}_{t-2} \\ P_{t,L}^{max}, & \text{if } p_{it}^u \geq P_{t,L}^{max} \text{ and } \ W_{it-2} \geq \overline{W}_{t-2} \end{cases}$$

where $P_{t,S}^{min}$ and $P_{t,S}^{max}$ are the minimum and maximum premium in year t for small firms, and $P_{t,L}^{min}$ and $P_{t,L}^{max}$ are the minimum and maximum premium for large firms, respectively. A firm is defined as 'small' if the total wage costs W_{t-2} are less than the threshold $\overline{W}_{t-2}$. This threshold is set at the average wage costs per worker in the Netherlands, multiplied by 15 (workers). The maximum premium for small firms is set at three times the average premium, while the maximum premium for large firms P_t^{max} is set at four times the average premium. The idea behind these differences is that larger firms are expected to be more able to bear the financial consequences of variation in premium rates. Minimum premiums of small and large firms are set using an iterative algorithm, such that the collected DI premiums equal the total DI costs.

[5] With constant average annual DI risks, f_t would be equal to one. But since the DI risk decreased over the years in our sample, f_t was mostly smaller than one.

4.2.2 Experience rating over the years

Although the calculation of the experience-rated premium has not been modified fundamentally, there have been several changes in the DI system that affected the coverage and, thus, the potential impact of the experience-rated premium. One of the most significant reforms was the removal of the experience-rated premium for small firms in 2003. Between 2003 and 2007, small firms paid a sectoral premium instead of an experience-rated premium. In 2004 the threshold value for large firms was increased from 15 to 25 times the average national wage, thus further decreasing the coverage of experience rating across firms. In effect, this implies that for firms with a wage sum between 15 and 25 the average wage, the abolishment of experience rating occurred one year later than for smaller firms. Experience rating was re-introduced for (all) small firms in 2008.

Since the introduction of the two new types of DI benefits in 2006, the experience-rated premiums only cover the DI benefit costs of the old WAO scheme and the scheme for temporary or partially disabled (the 'WGA'-scheme). This implies that the DI benefit costs of the scheme for permanently and fully disabled (the IVA scheme) are not included in the calculation of the employer DI risk. As the share in the total DI costs of the new scheme for permanently and fully disabled is slowly increasing over time, the coverage of the experience-rated premiums decreases over time as well.

To provide more insight into the changes in the experience-rated premium over the years, Table 4.1 shows the parameters of the DI premium calculation between 1999 and 2013. Not surprisingly, the table shows that the average DI risk follows a similar pattern as the average DI premium, with yearly averages that vary substantially. As experience rating was introduced gradually since 1998, the average DI premium increased between 1999 and 2004. This trend was reversed in 2005, when the extension of the sick leave period from one to two years came into force. With individuals that started collecting DI benefits after two years of sick leave instead of one year, the total DI costs – and thus the DI premium – declined substantially since 2005. The introduction of the WGA and IVA benefits in 2006 contributed to a further decline of DI premium rates, as individuals with a DI percentage of less than 35% are no longer entitled to DI benefits and the costs of the IVA scheme are not covered by the experience-rated premiums.

Table 4.1 also makes apparent that the wage sum threshold that classifies firms as small or large has increased over the years. Obviously, the largest change was in 2004, when the threshold was no longer based upon a firm size of 15 workers and raised to 25 workers. For small firms, the minimum premium varied between 0.27% and 1.24% of the wage sum in the time period under consideration. As a result, firms without disability benefits that were assigned to them still had to pay a positive premium in all years. From the table, we also infer that the maximum premium of small firms ranged between 1.47% of the wage sum in 2009 and 6.06% of the wage sum in 2002. Between 2003 and 2007, however, small firms paid sectoral DI premiums. This contrasts to DI premium variation of large firms, which shows a more pronounced pattern. In particular, the minimum premium of large firms varied between 0% and 0.67% of the wage sum and the maximum premium varied between 1.96% and 8.84% of the wage sum.

To illustrate the functioning of the minimum and maximum premium, recall Figure 3.4 in the previous chapter which shows the distribution of the experience-rated DI premiums for small and large firms. The vast majority of small firms pays the minimum premium, while firms classified as large most often pay a premium that is unconstrained by the minimum and maximum premium. Since 2005, the proportion of large firms paying the minimum premium is increasing. This implies that the share of large firms with a DI risk of 0% is increasing as well. In 2013 the majority of the large firms paid the minimum premium.

4.3 Data

4.3.1 Matched worker-firm data

In our analysis, we use various administrative data sets from Statistics Netherlands, with information on employment and DI spells, worker characteristics and firm characteristics – all observed between 1999 and 2013.[6] Using unique firm identifiers, worker data can be matched to firms, and reverse. One limitation in the data is that firm statistics are not measured at the level of individual plants but at the level of legal entities instead. As DI premiums are usually set at the level of plants, we thus cannot reconstruct disability risks and DI premiums if a firm consists of more plants.[7] We therefore restrict our sample to firms with single plants, thus loosing about 20% of the firms in our sample.

[6]The data we discuss in this section are to a large extent comparable to those that are used in Chapter 3. As for the current analysis, we therefore devote most attention to the reconstruction of experience-rated DI premium rates that is needed to estimate premium rate effects.

[7]To shed some light on the size of this issue, we have merged firms in the data from Statistics Netherlands to firms in data from UWV which were used in the actual calculation of the experience-rated premiums. In 2009 91% of the firms in the UWV data correspond to exactly one firm in the data of Statistics Netherlands, 7% to two firms, 2% to three or more firms.

Table 4.1: Parameters of DI premium calculation between 1999 and 2013

	1999	2000	2001	2002	2003	2004	2005	2006	2007	2008	2009	2010	2011	2012	2013
Average premium[a]	0.85	1.54	1.66	2.11	2.38	2.35	1.67	0.98	0.48	0.57	0.47	0.59	0.62	0.55	0.54
Average DI risk	1.35	1.43	1.51	1.75	1.79	1.86	1.75	1.67	1.39	0.85	0.71	0.36	0.28	0.22	0.23
Threshold wage costs of small firm (€, x1000)	313	321	330	340	361	625	643	650	675	683	705	730	748	755	758
Minimum premium, small	0.77	1.24	0.98	1.24	-	-	-	-	-	0.30	0.27	0.59	0.56	0.48	0.48
Maximum premium, small	2.43	4.17	4.76	6.06	-	-	-	-	-	1.74	1.47	1.59	1.65	1.59	1.59
Minimum premium, large	0.25	0.67	0.41	0.45	0.59	0.49	0.00	0.00	0.00	0.00	0.00	0.06	0.07	0.13	0.13
Maximum premium, large	3.24	5.50	6.36	8.08	8.52	8.84	6.84	4.40	2.48	2.32	1.96	2.12	2.20	2.12	2.12
Scale factor DI risk	0.45	0.61	0.83	0.95	1.00	1.00	1.00	0.59	0.35	0.68	0.69	1.47	1.96	1.90	1.78

[a] All parameters are measured as a percentage of the wage costs, except for the wage costs threshold and the scale factor.

Table 4.2 shows the main characteristics of the matched firm-worker data for firms which we observe for at least three years in a row and which consist of a single plant.[8] In 2001, our sample consists of about 254,000 firms with on average 26.7 workers. We observe a strong drop in the number of firms in 2005, which is due to a change in the source of employment contracts in 2006 in the data of Statistics Netherlands. Over the years, there is an increase in the average wage costs per worker – from €16,600 in 2001 to €25,700 in 2013 – and an increase in the share of temporary workers of the workforce. Finally, in line with expectations, the fraction of DI inflow of the employed workers per firm decreases over the years.

Based on the data of Statistics Netherlands, we categorize firm exits as bankruptcies, mergers or 'other'. As our interest lies in firms exits that lead to substantial layoffs of workers, we redefined firm exits as 're-starters' if (i) at least 60% of their workforce can be retrieved in a new firm in the next year; (ii) the new firm is no more than three times smaller or larger than the old firm; (iii) the old firm had at least five workers. As a result, the incidence of firm exits due to bankruptcies and mergers becomes somewhat lower than in the original data – with percentages of firm exits due to bankruptcies and mergers ranging between 1.8 % and 3.2% and 0.2% and 0.5%, respectively. Also, it should be noted that there are peaks in the incidence of re-starters in 2005 and 2009, but these can largely be attributed to changes in the data setup of Statistics Netherlands.

To provide further insight in firm dynamics and differences between less and more successful firms, Table 4.3 shows the descriptive statistics for the firms' first observed year and the year in which the firm exits, if relevant. The average characteristics of the firms in their first year are given in columns two and three. In these columns, we distinguish between firms that will exit the market at some point in time in our period under consideration, and firms that continue to exist in the entire observed period. When comparing these, we observe some distinct differences. Firms which will exit the market on average are larger, pay a lower average wage, have a larger share of permanent contracts and a have larger share of inflow into DI. For this group of firms we also observe a higher inflow into UI.

In columns four to six of Table 4.3 we show the descriptive statistics of the last observed year for firms which exit the market at some point in time between 2001 and 2012. In these columns, we distinguish between firms by type of exit. When

[8] Recall that UWV uses data from the year $t-2$ to calculate the wage sum and risk percentage. Therefore we restrict the sample to the firms which exist sufficiently long enough to calculate the experience-rating characteristics.

Table 4.2: Descriptive statistics of the Statistics Netherlands data.

	2001	2003	2005	2007	2009	2011	2013
Firm characteristics							
Number of firms (x1000)	254	217	205	123	158	152	160
Average nr. of workers per firm	26.7	27.1	27.1	26.0	25.8	23.0	21.9
% of small firms	87.7	86.3	90.8	91.9	91.4	94.6	94.9
Sector (%)							
Trade	23.1	23.0	23.3	26.7	25.3	22.9	21.9
Business	11.0	11.1	11.5	11.6	12.7	10.8	10.9
Industrial	13.7	14.1	14.5	15.8	14.1	10.8	10.2
Health	11	11.0	11.0	13.0	11.4	11.5	11.4
Food	9.1	9.1	8.8	9.9	9.4	9.5	9.5
Agriculture	7.2	7.1	7.3	7.2	5.8	5.9	5.2
Construction	4.1	5.0	5.4	5.4	4.6	3.5	2.9
Transport	3.4	3.4	3.4	3.4	3.3	2.6	2.4
Firm exits [a]							
% of firm exits	2.8	3.9	5.1	4.6	6.0	3.8	-
- Bankruptcy	1.8	2.5	2.4	3.1	3.2	2.7	-
- Merge	0.2	0.3	0.2	0.3	0.4	0.5	-
- Start as new firm	0.3	0.3	2.2	0.8	2.3	0.6	-
- Other	0.3	0.8	0.3	0.3	0.1	0.1	-
Worker characteristics							
Average wage (x €1000)	16.6	17.6	18.6	19.1	22.5	25.5	25.7
Average age	36.2	37.3	38.2	39.0	39.1	40.2	40.7
Men (%)	55.8	55.9	56.0	54.4	55.1	55.7	56.1
Immigrants (%)	14.0	13.9	13.7	13.6	15.0	15.3	15.6
Temporary workers (%)	6.1	6.0	5.2	6.5	7.2	8.0	9.5
Permanent contracts [b] (%)	-	-	-	72.2	67.4	59.6	-
DI and UI inflow [c] *(%)*							
Inflow DI	0.96	0.75	0.22	0.31	0.37	0.35	0.32
- Inflow DI, WGA	-	-	-	0.25	0.30	0.28	0.26
- Inflow DI, IVA	-	-	-	0.06	0.07	0.07	0.06
Inflow UI	2.2	3.3	3.3	2.1	4.0	3.4	5.1

Only for firms with one plant (only odd years are shown). [a] Information on firm exits is not available for the year 2013. [b] Information on permanent contracts is only available for the years 2006-2012. [c] The statistics of DI and UI inflow only include the spells of individuals who could be linked to a firm.

comparing the last observed year of firms which exit because of a bankruptcy to the last year of firms exiting because of a merger or restart, we observe several differences between the firms. Compared to merging or restarting firms, firms which go bankrupt are smaller, pay a lower average wage, have a higher share of immigrants and have a larger share of inflow into DI and UI. Merging firms pay a higher wage, have a higher share of men and a lower share of permanent contracts compared to firms which go bankrupt or restart as a new firm, whereas restarting firms are relatively large and have a high share of permanent contracts.

In column seven we show the statistics of the last observed year for all firms which exit the market between 2001 and 2012 for all types of exits together. If we compare these statistics to column three – which shows the statistics of the first observed year for the same sample of firms – we see that in the period between the first and last observed year the firms somewhat decreased in size and reduced the average wage. Compared to the first observed year, the inflow into DI and the inflow into UI are both substantially larger in the last observed year of the firms. This may point at the firm being in financial distress, but could also stem from a rise of the experience-rated DI premium.

4.3.2 Disability risks and experience-rated DI premiums

As we have discussed earlier, disability risks and DI premiums of firms follow from information on wage sums, DI benefit costs and annual experience-rating parameters. In principle, all this information is observed in the data from Statistics Netherlands, with the exception of whether DI benefits are received by workers that had permanent or temporary contracts. Accordingly, we cannot distinguish DI benefit costs of permanent workers that were experience rated from DI benefit costs of temporary workers that were not experience rated. For firms with former temporary workers receiving DI benefits, we thus overestimate the disability risks and DI premiums that we reconstruct from the data.[9] In principle, these measurement errors can be reduced by selecting firms with a low fraction of temporary workers. We return to this issue when we test for the robustness of our model estimates.

Table 4.4 shows statistics of the disability risks and DI premiums that we have reconstructed from the data. As becomes apparent from the table, the average

[9]Another, less important potential source of measurement errors is due to differences in the definition of wages of Statistics Netherlands and the Employee Insurance Agency. These differences may stem from the inclusion or exclusion of additional income, such as compensation for travel costs or leased cars.

Table 4.3: Descriptive statistics of the first observed year of the firm by later observed exit and for firms in the last observed year by type of exit.

	First year of observation[a]		Last year of observation			
	No exit	Exit[b]	Bankruptcy	Merger	Restart	All exits
Firm characteristics						
Number of firms	114,477	15,792	10,887	1,757	2,702	15,792
Average workers per firm	17.6	25.6	5.9	21.9	47.6	23.3
Age of firm						
3-4 years	100	100	64.4	73.0	69.2	66.2
5-7 years	0	0	30.6	24.0	27.8	29.3
More than 8 years	0	0	4.9	3.0	3.0	4.4
Sector (%)						
Trade	20.2	21.8	22.6	11.7	18.7	20.6
Business	15.3	17.4	13.8	23.4	24.6	17.1
Industrial	9.9	12.3	9.2	7.4	18.5	10.7
Health	7.9	9.2	10.7	4.3	12.9	10.1
Food	7.7	9.5	13.5	2.6	3.0	10.3
Agriculture	3.9	2.8	3.5	0.7	3.1	3.1
Construction	2.8	3.1	3.5	1.8	3.4	3.2
Transport	3.0	4.2	3.8	3.0	5.1	4.0
Worker characteristics						
Average wage (x €1000)	28.5	21.5	13.5	39.0	27.8	19.5
Average age	39.0	39.3	40.8	43.0	39.0	40.8
Men (%)	61.7	59.1	54.9	67.8	60.1	57.5
Immigrants (%)	16.2	20.3	24.0	13.8	16.1	21.4
Temporary workers (%)	7.3	7.4	9.4	3.4	4.7	7.8
Permanent contracts [c] (%)	51.7	62.3	64.9	44.7	70.3	63.2
DI and UI inflow [d] (%)						
Inflow DI	0.27	1.09	1.97	0.96	0.39	1.55
Inflow DI, WAO	0.09	0.34	0.26	0.11	0.09	0.21
Inflow DI, WGA	0.15	0.64	1.51	0.62	0.26	1.17
Inflow DI, IVA	0.03	0.11	0.20	0.23	0.04	0.17
Inflow UI	3.1	7.6	15.5	5.7	3.7	12.1

Based on firms in data of Statistics Netherlands with one plant, 2001-2012.
[a] Given that the experience-rating statistics are based on the year t-2, the first observed year is the third year of a firm. Firms which exist for less than three years are not included in the sample.
[b] If a firm exits in any year before 2013, it is defined as "later exit'.
[c] Only available for firms in the year 2006 and later.
[d] The statistics of DI and UI inflow only include the spells of individuals who could be linked to a firm.

disability risks and DI premiums follow a similar pattern as those reported by the Employee Insurance Agency (see Table 4.1). That is, the average risk percentage increases until 2005 and decreases after the extension of the sick leave period in 2005 and the DI reform in 2006. We also observe average DI premium rates that are substantially lower in the period after the reforms. Recall also that experience rating was removed for small firms between 2003 and 2007, so there is no variation in DI premiums in those years for this group; in the other years, the vast majority of firms paid the minimum premium.

Based on the experience-rating settings, firms' DI premium rates can be characterized as a mapping of firms' disability risks. In this mapping, DI premiums increase with respect to disability risks until the maximum premium is reached. This however does not mean that correlations between disability risks and DI premiums are fairly constant over the years. From year to year, there are changes in the experience-rating parameters, with the complete removal of experience rating for small firms between 2003 and 2007 as the most extreme change. Accordingly, the mapping from disability risks to DI premium rates changed substantially over the years, particularly for firms that were classified as small by the Employee Insurance Agency.

To illustrate this point, Figure 4.1 pictures the evolution of the DI premium for (fictitious) firms with constant disability risks. We focus on firms with constant disability risks of 0%, 1% or 10% respectively, all measured between 1999 and 2013. The left (right) panel shows the DI premiums of a firm that is classified as small (large) in the experience-rating system. For large firms, we observe yearly variation in DI premiums for all risk profiles. This variation is driven by changes in the minimum and maximum premium rates and by changes in the correction factor in the experience-rating formula. Obviously, changes in the minimum premiums are

Table 4.4: Constructed experience-rating characteristics.

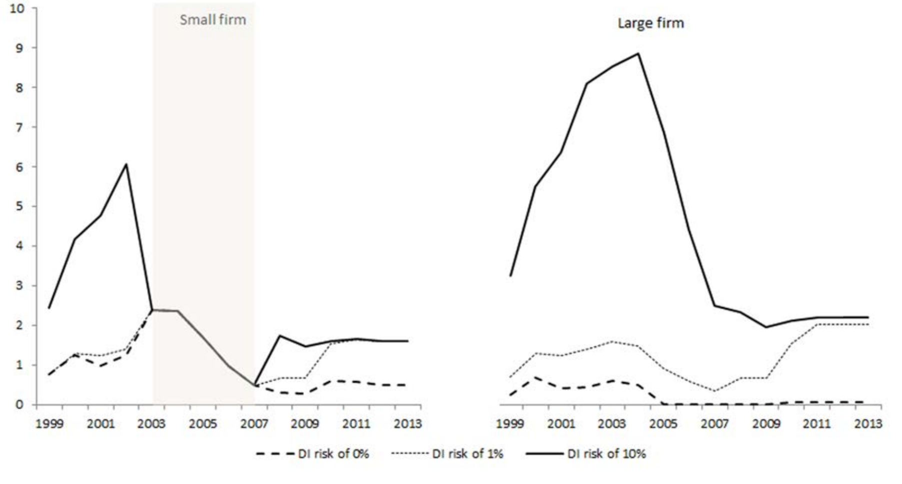
	2001	2003	2005	2007	2009	2011	2013
Average risk percentage	0.38	1.50	2.15	1.51	1.00	0.71	0.67
Average premium	1.13	2.02	1.51	0.51	0.35	0.59	0.52
Difference premium because of ER, small	-0.57	0.00	0.00	0.00	-0.13	-0.03	-0.03
Difference premium because of ER, large	-0.11	-0.18	0.05	0.14	-0.04	-0.07	0.02
% minimum premium	88.4	81.4	78.7	82.3	89.4	94.8	95.1
% maximum premium	2.5	5.4	8.4	6.2	5.5	3.5	3.3
% of small firms	87.7	86.3	85.9	87.5	86.9	92.4	92.9

Based on the data of the Statistics Netherlands data for all firms with one plant, for the years 2001 to 2013 (only odd years are shown) most relevant for firms with a disability risk of 0%, whereas changes in the maximum are most relevant for firms with a disability risk of 10%.

Compared to large firms, the level of the DI premium of small firms is less strongly linked to the level of disability risks. Most importantly, this is due to the removal of experience rating for small firms between 2003 and 2007. As all small firms paid a uniform premium in this period, we observe a substantial reduction in the DI premium of the firm with the disability risk of 10% and a DI premium increase for the firms with disability risks of 0% and 1%. Also, note that small firms were more likely to have DI premium rates that were capped by the maximum premium, which in turn further reduces the correlation between disability risks and DI premium rates (see Table 4.1 and Figure 3.4).

Figure 4.2 shows the implications of changes in the experience-rating settings for the actual firms that are observed in the data. In this figure, we show the annual correlation between changes in disability risks and changes in DI premiums, both for small firms and for large firms. In the absence of premium caps, yearly correlation between disability risks and DI premiums would be equal to one. For large firms, however, the yearly correlation between the disability risk ranges between 0.42 and 0.87, which is almost fully due to the constraining effect of maximum premiums. For small firms, the risk percentage and the DI premium are not correlated between

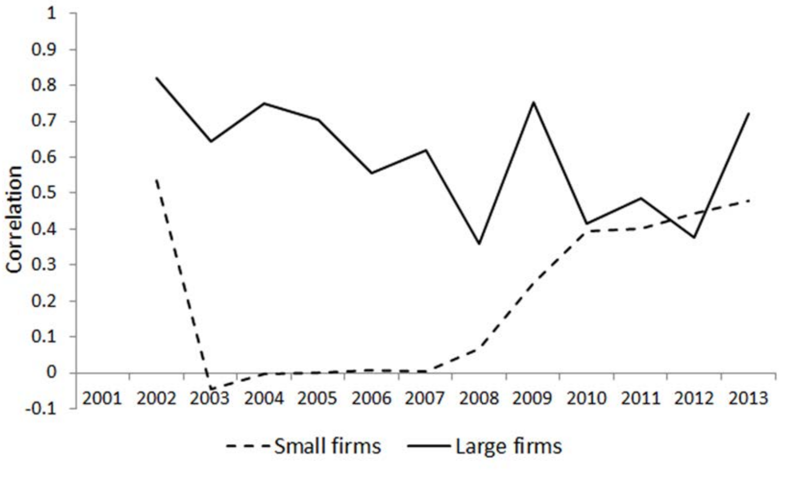
Figure 4.1: Illustration of the evolution of the DI premium for (fictitious) small and large firms with a constant DI risk, following from the experience rating settings.



2003 and 2007, as experience rating was absent in those years. In the other years the correlation between disability risks and DI premium rates is at most 0.57.

Taken together, we observe substantial variation in DI premium rates that can be attributed to changes in the experience-rating scheme and nonlinearities in the mapping of disability risks to DI premiums. This particularly holds for the sample of firms that are classified as small by the Employee Insurance Agency. Particularly for this group, the removal of experience rating for these firms provides us with a powerful tool to identify the isolated impact of changes in DI premium rates – so for given levels of disability risks. In what follows, our strategy is thus to control for these disability risks in the model specifications.

Figure 4.2: Correlation between the yearly change in the risk percentage and the yearly change in the experience-rated DI premium



Note: Yearly change in the risk percentage is equal to DI Risk$_t$ - DI Risk$_{t-1}$ and yearly change in the experience-rated DI premium to (DI premium$_t$ - DI premium$_{t-1}$)

4.4 Empirical analysis: firm exits

4.4.1 Model specification

With firm exits as an outcome measure, it is likely that effect estimates of DI premium rates are subject to endogeneity bias: firms with higher disability risks may be in financial distress for other reasons than high DI premiums, for instance because they devote less effort to preventative activities or have low-quality managers. As such, we may well overestimate the effect of DI premiums on firm exits and other behavioral responses of the firm. In our analysis, we therefore pursue a difference-in-differences estimation where we exploit changes in the experience-rating settings. More specifically, we condition on the firms' level of disability risks, while assuming that the remaining variation in DI premium rates is exogenous to the firm. In effect, this means that the effect of DI premiums is identified from changes in the experience-rating parameters, together with the cutoff effect of the maximum premium. We argue that most of the variation we exploit stems from the removal of experience rating for small firms between 2003 and 2007.[10]

We formalize this approach by specifying the duration until a firm exit as hazard rates, for a flow sample of firms. As we observe multiple types of firm exits, we adopt a competing risks framework with four risks: bankruptcies, mergers, re-starts, or other types. We thus model the risk type c ($c = 1, ..4$) with elapsed duration τ as follows

$$\theta^c_{it}(\tau|X_{it}, r_{it}, a_{it}, \mu^c_{t,s}) = \lambda^c(\tau)exp(\beta^c X_{it} + \gamma^c r_{it} + \delta^c a_{it} + \mu^c_{t,s}) \qquad (4.3)$$

The exit rate in year t of firm i with size s is a function of time-varying firm characteristics X – which includes the change in job openings per sector – year fixed effects μ separately for small and large employers and the employer DI risk r.[11] Duration dependence in firm exits is denoted by the baseline risk $\lambda^c(\tau)$ for the exit of type c.[12]

In our model, a can be considered as the main variable interest, as it describes the impact of deviations from uniform (average) premiums due to the experience-rating system. We refer to this variable as the "premium adjustment". By controlling for employer disability risks r, we separate direct risk effects from the indirect effect of the experience-rated DI premium. For small firms in the years without experience rating, a is set to 0. In the baseline specification, we start by specifying a linear direct effect of the disability risk; later on, we test the sensitivity of our results by adding more polynomials.

[10] In a difference-in-difference setting, small firms can be considered as the treatment group for which the link between the DI premium and the DI risk is removed. Large firms are the control group for which the DI premium depends on the DI risk in all years.

[11] Note that the size of the firm and the DI risk are based on data of $t - 2$.

[12] For the total firm exit rate, we specify a similar model which we estimate using a Cox Proportional Hazard specification where λ is the baseline hazard for all firm exits together.

4.4.2 Main estimation results

Table 4.5 shows the estimation results for the Cox proportional hazard model for all firm exits (column 2) and the competing risks models for the different exit types. Most notably, we find a significant and positive coefficient estimate of the premium adjustment due to experience rating. It implies that an increase of the DI premium with one percentage point leads to an increase in the annual firm exit rate by 0.8 percentage point (from 12.6% to 13.4%).[13] We also find evidence for a direct and positive effect of disability risks on the likelihood of firm exits. Compared to the indirect effect that runs through increases in experience-rated DI premiums, the 'direct' effect of the disability risk on firms exits is somewhat smaller in size.[14] We used the estimated coefficients to make a back-of-the-envelope calculation to estimate the net effect of the experience-rated premium between 2001 and 2011. In this calculation, we compare the actual number of firms exits in our sample to a scenario without experience rating, meaning firms paying an average premium. In this alternative scenario we also account for the negative effect of DI experience rating on the DI costs of chapter 3. Based on these calculations, we find that experience rating increased the number of firm exits between 2001 and 2011 with 15,500 exits, an increase of 0.8 percentage point.

As to the other covariates that may explain firm exits, we find most coefficient estimates to be in line with expectations. If the number of vacancies in a particular sector is increasing, the firm is less likely to exit. Also, the probability of a firm exit is decreasing with firm size and with respect to the average wage in the firm. Firms with older workers on average and firms with a larger share of men are more likely to exit. Firms in the sectors construction, transport and business have a higher probability to exit the market (not shown in the table).

Column three to five of Table 4.5 show our estimation results by the type of firm exit. To start with, we find increases in the firms' disability risk to be positively associated with the risk of bankruptcies. As to the risk of mergers or re-starts, however, coefficient values are not significant. Taken together, this suggests that the positive relationship of disability risks and firms exits is mainly driven by exits caused by bankruptcies.

[13]The standard deviation of the experience-rated premium is 0.86 percentage point.

[14]Recall from Section 4.2 that the definition of the disability risk changed after 2006 as full and permanent DI benefits were no longer experience rated. The estimation results do not change substantially if we include an alternative measure for DI risk which includes full and permanent DI benefits after 2005.

When moving to the experience-rated premium adjustments as our variable of interest, it seems that higher DI premium costs increase both the risk of bankruptcies and mergers. The estimates imply that an adjustment of the premium of 1 percentage point increases the probability of a bankruptcy with 0.44 percentage points and the probability of a merge with 0.20 percentage points, respectively. This again suggests that higher costs of experience-rated premiums may be a financial burden for firms. At the same time, however, increased mergers may also point at strategic behavior of firms. That is, mergers provide firms with the opportunity to continue their activities without current DI benefit costs that are assigned to them (and start fresh with a risk percentage of 0%). Likewise, re-starting as a new firm may be an alternative way to leave behind current DI benefit costs, but there is no evidence in this direction.

4.4.3 Robustness analyses

In this subsection we assess our estimation results for firm exits and firm exits by type in greater detail by performing several robustness analyses – see Table 4.6 for the concerning results. These robustness analyses will address our research design, measurement errors in the disability risks and DI premiums, the specification of our model and the use of stock samples of firms.

Our first robustness check focuses on one of the two major sources of identification. Recall that in the baseline specification, we disentangle the effect of a premium adjustment from the effect of the DI risk by exploiting changes in the experience-rating system. The removal of experience rating for small firms between 2003 and 2007 provides us with exogenous variation between small and large firms, while changes in the slope of the experience-rated premium provide us with year-to-year variation within firms. We believe the removal of experience rating to be the most powerful source of identification.[15] However, including firms with a wage sum far

[15] An alternative robustness check on our identification strategy would be to use a regression kink design around the maximum premium, given that below the threshold of the maximum premium the DI premium is increasing with DI risk, but the premium is constant for firms with DI risks above the threshold. When using a regression kink design around the maximum premium, the signs of the estimated kinks - although insignificant - in are line with our baseline specification. We have decided not to present the estimates from the regression kink design because of two distinct disadvantages of this identification method. First, firms with a DI risk close to the maximum premium are likely to switch from having a DI risk just below the threshold in one year, to having a DI risk just above the threshold in the next year. This will lead to an underestimation of the effects of the experience-rated premium. Second, a regression kink strategy reduces our effective sample size considerably.

Table 4.5: Estimation results of duration models for firm exits and by type of exit.

	All firm exits	Bankruptcy	Merge	Starts as new firm
Adjustment of the premium	6.921**	13.462**	2.738**	0.380
	(1.363)	(2.467)	(0.493)	(0.398)
Risk percentage	0.131**	0.369**	0.227	-0.492
	(0.057)	(0.087)	(0.279)	(0.405)
Vacancies in sector	-0.029**	-0.009**	-0.002	-0.019**
	(0.002)	(0.004)	(0.010)	(0.007)
Firm characteristics				
Small firm	-0.122**	-0.462**	-0.596**	-0.057
	(0.025)	(0.070)	(0.165)	(0.080)
Less than 15 workers	0.0201*	1.380**	-0.863*	-0.510
	(0.121)	(0.346)	(0.462)	(0.334)
15-25 workers	0.264**	1.285**	-0.371	0.472
	(0.121)	(0.346)	(0.453)	(0.331)
25-50 workers	0.295**	1.163**	-0.343	0.660**
	(0.120)	(0.343)	(0.437)	(0.325)
50-100 workers	0.321**	0.877**	-0.090	0.533
	(0.120)	(0.345)	(0.432)	(0.328)
100-500 workers	0.208*	0.575*	0.024	0.335
	(0.122)	(0.349)	(0.430)	(0.333)
500-1000 workers	0.203	0.934**	-0.225	0.024
	(0.158)	(0.407)	(0.604)	(0.447)
Average age	0.018**	0.034**	0.031**	0.003
	(0.001)	(0.001)	(0.004)	(0.003)
Percentage men	0.281**	0.274**	0.259**	-0.103
	(0.018)	(0.041)	(0.128)	(0.074)
Percentage of immigrants	0.007	0.355**	-0.336**	-0.010
	(0.021)	(0.046)	(0.170)	(0.089)
Percentage of single households	-0.001	-0.209	0.493	0.374
	(0.101)	(0.200)	(0.845)	(0.598)
Percentage of single parents	-0.137	-0.431**	0.779	-0.027
	(0.106)	(0.216)	(0.858)	(0.617)
Percentage of couples	-0.074	-0.453**	0.376	0.528
	(0.099)	(0.196)	(0.827)	(0.592)
Average wage below €7,500	0.626**	1.987**	0.277**	-1.510**
	(0.021)	(0.056)	(0.140)	(0.142)
Average wage €7,500-15,000	0.328**	1.365**	0.170	-0.441**
	(0.021)	(0.059)	(0.133)	(0.096)
Average wage €15,000-2,5000	0.181**	0.672**	-0.098	-0.107
	(0.020)	(0.061)	(0.123)	(0.079)
Average wage €25,000-40,000	0.179**	0.074	-0.188	0.226**
	(0.019)	(0.066)	(0.117)	(0.071)
Effect of 1pp increase in premium	0.83	0.44	0.20	0.00
Observations	124,775	124,775	124,775	124,775
Log likelihood	-154,289	-62,928	-8,846	-21,385

Note: Estimations results of Cox proportional hazard model (column 2) and competing risks models (column three to five). Standard errors in parentheses. * significant at a level of 10%, ** significant at a level of 5%. Regressions include sector fixed effects and year fixed effects for small and large firms.

from the threshold of small firms – as we do in the baseline specification – might reduce the strength of this source of identification as it makes small and large firms less comparable to each other. For this reason, we restrict the sample to firms with a wage sum close to the threshold of small firms, i.e. firms with a wage sum that is between ten and 40 times the average wage in the Netherlands (the threshold of a small firms is 25 times the average wage). As such, we obtain a difference-in-difference set-up where we compare firms just above the threshold for which a high (low) DI risk translates into a positive (negative) adjustment of the premium, to firms just below the threshold which pay an average premium between 2003 and 2007 and are experience-rated in all other years. The disadvantage of this approach, however, is that we lose over 80% of the firms in our sample and that we only observe roughly 1,000 exits per observed year. With this restricted sample, we find a smaller and insignificant effect of DI premiums on exits and bankruptcies.

The second panel in Table 4.6 concerns the issue of measurement errors in the reconstructed disability risks. In Section 4.3 we stated that the reconstructed disability risks and DI premiums may be susceptible to measurement errors, as disabled workers with a temporary contract are unjustly assigned to their firms. Particularly for the years 2001-2005 where we do not observe the type of contract, we may overestimate disability risks and DI premiums. To offset the potential effect of any biases due to measurement errors, we therefore repeated our estimations on a sample of firms with at least 80% of their employers having permanent contracts between 2006 and 2013. This yields similar results as our baseline model.

Third, we have used a more flexible specification of the effect of disability risks on firm exits. That is, we included second-order and/or third-order polynomials of the firms' disability risk. Again, the point estimates of the effect of the premium adjustment – except for bankruptcies – appear to be insensitive to this. When using the Akaike information criterion, we do not find the inclusion of second or third-order polynomials to significantly improve the fit of our model.

We also changed the specification of the effect of the adjusted premium on firms effects by allowing for non-linear effects. If we include a quadratic term of the premium adjustment, we find a significantly negative coefficient estimate for this quadratic term for exits and bankruptcies. This means that the the risk of firm exits increases over the relevant support of DI premiums, but the total effect is concave. To further explore the potential non-linear effects of a premium adjustment, we included dummies by size of the adjustment. The positive effect of the premium adjustment on firm exits, bankruptcies and mergers seems to be driven by firms which experience an upward adjustment of more than 0.5%.

Our final robustness test addresses the effect of flow sampling on our results. In our baseline model, we avoid left-hand censoring by considering only the inflow of new firms. This means, however, that we exclude firms that exist for a longer period of time. We therefore re-estimated our model on a stock sample using a logit specification. The estimation results are given in the last panel of Table 4.6. As the table shows, we observe similar coefficient estimates compared to the baseline model estimate for bankruptcies and mergers, but no significant effect for all firm exits.

4.4.4 Heterogeneous effects

The effect of a premium adjustment could be different for firms with different characteristics. More specifically, we would expect effects to be larger for specific groups of firms. For example, as the DI premium is calculated as a percentage of the labor costs, an increase of this premium would have a larger impact on firms for which labor costs comprise a higher share of the total costs. The effects could also be larger for firms which are more prone to exit the market, irrespective of premium adjustments, for example because they are more liquidity constrained. A third group of firms for which we would expect larger effects are firms with a lower share of temporary workers, given that it will be more difficult for these firms to lower their labor costs in response of a premium adjustment. Although we do not observe all of these firm characteristics in our data, we can test for differences in the size of the DI premium effect by other, related characteristics.

Table 4.7 presents the results of these estimations on several subsamples of firms. As the number of exits because of a merger or restart is too low to estimate the effect for all of the subsamples, we only consider all firm exits and exits due to bankruptcies. According to data from Statistics Netherlands, the share of labor costs are highest in the business and health sector. However, we do not observe larger effects of a premium adjustment on firm exits for these sectors.

In Table 4.3 we observed a higher exit rate for firms in the trade and business sector and firms with a low average wage, so we would expect that a premium adjustment has a larger effect on these firms. Our findings are in line with this, firms with a higher risk of bankruptcy in general are more responsive to premium adjustments. Finally, the effects are larger for firms with a low share of temporary workers, which is in line with the hypothesis that less flexible firms react to a premium adjustment more strongly.[16]

[16] As 85% of the firms do not have any temporary workers, the number of firms with a share of temporary workers below the mean is much larger than the number of firms with a share of temporary workers above the mean.

Table 4.6: Estimates of the premium adjustment on firm exits and exits by type: robustness checks.

	Firm exits	Bankruptcy	Merge	Starts as new firm
Baseline, full population	6.921**	13.462**	2.738**	0.380
	(1.363)	(2.467)	(0.493)	(0.398)
Test of research design				
Selection of firms around threshold	2.701	3.530	2.297**	-0.148
small firm (10-40 workers)	(2.386)	(4.992)	(0.650)	(0.578)
Test of sensitivity measurement errors				
Selection of firms with less measurement	7.538**	16.316**	2.753**	0.173
error (share permanent contracts >80%)	(1.573)	(2.574)	(0.529)	(0.478)
Test of specification DI risk				
Second order polynomial DI risk	6.162**	5.292*	1.978**	0.144
	(1.572)	(2.768)	(0.536)	(0.443)
Third order polynomial DI risk	5.988**	1.882	1.421**	0.077
	(1.572)	(2.959)	(0.602)	(0.047)
Test of non-linear effects premium adjustment				
Including quadratic term (one regression)				
Adjustment premium (x100)	12.109**	26.473**	3.910**	0.826
	(2.070)	(4.739)	(0.924)	(0.560)
Adjustment premium squared	-0.306**	-0.629**	-0.481	-0.032
	(0.129)	(0.229)	(0.334)	(0.026)
Dummies by size of the adjustment (one regression)				
Negative, more than 1%	0.045	0.267	0.055	0.171
	(0.107)	(0.252)	(0.418)	(0.279)
Negative, 0.5-1%	0.011	-0.166	0.498*	-0.203
	(0.073)	(0.142)	(0.273)	(0.167)
Negative, less than 0.5%	-0.128**	-0.205**	0.261	-0.103
	(0.043)	(0.096)	(0.237)	(0.111)
Positive, less than 0.5% (reference)	-	-	-	-
Positive, 0.5-1%	0.103	0.257*	0.657**	0.013
	(0.066)	(0.149)	(0.318)	(0.164)
Positive, more than 1%	0.090**	0.226**	1.049**	0.047
	(0.045)	(0.098)	(0.240)	(0.131)
Test of sensitivity flow sample				
Logit specification on stock sample	0.336	9.562**	12.539**	-0.378
	(0.412)	(0.700)	(1.061)	(1.301)

Note: Based on data between 2001 and 2012. Standard errors in parentheses. * significant at a level of 10%, ** significant at a level of 5%. All cells represent separate regressions, with the exception of row 6-13. Regressions include firm characteristics, time fixed effects and controls for economic conditions. Except for the last panel, all estimates are obtained by using Cox proportional hazard (all exits) and competing risks (exits by type).

Table 4.7: Estimates of the premium adjustment on firm exits and exits by type: heterogeneity analysis.

	Firm exits		Bankruptcy		Observations
Baseline, full population	6.921**	(1.363)	13.462**	(2.467)	124,775
By sector					
Trade	10.728**	(2.923)	20.875**	(5.222)	26,137
Business	6.053*	(3.437)	15.397**	(6.957)	20,504
Industrial	3.828	(3.226)	15.550**	(5.966)	13,277
Health	3.383	(5.930)	11.706	(10.491)	10,676
By average wage					
Below the mean	6.182**	(2.239)	13.560**	(3.747)	61,294
Above the mean	3.955**	(1.776)	10.606**	(3.508)	63,481
By share of temporary workers					
Less than average	7.177**	(1.434)	14.692**	(4.342)	111,605
More than average	5.696	(4.467)	-4.376	(13.868)	23,325

Note: All regressions are performed on separate subsamples of firms, for the years 2001 to 2012. Standard errors in parentheses. * significant at a level of 10%, ** significant at a level of 5%. All cells represent separate regressions. Regressions include firm characteristics, time fixed effects and controls for economic conditions. All estimates are obtained by using Cox proportional hazard (all exits) and competing risks (exits because of bankruptcy).

4.5 Empirical analysis of layoff effects

4.5.1 Model specification

So far, our estimation results indicate that increases in experience-rated DI premiums increase the likelihood of a firm exit. This suggests that increased premiums may cause acute financial problems for firms. If so, firms may also have responded by cutting labor costs. One way to cut labor costs is by firing workers. If firms respond to a premium increase by firing workers, one would expect increases in the inflow into Unemployment Insurance (UI). One way to cut labor costs is by increasing layoffs, thus increasing the firms' UI risk; yet another way to reduce (future) labor costs is by substituting workers from the DI scheme to non-experience rated scheme for permanently and fully disabled workers (IVA).[17] One would thus expect increases in the inflow into UI and IVA.

To address these effects, in this section we test whether the experience-rated DI premium has an effect on the inflow into the schemes of UI and IVA. Our estimation

[17] Another potential response of firms is that they become more selective in hiring new workers. Unfortunately, we do not observe the health of the workers and are not able to estimate the effects on hiring workers with less favorable health characteristics.

strategy is similar to the one that is followed for firm exits: we identify the effect of adjusted DI premiums by exploiting annual changes in the mapping of disability risks to DI premiums, while conditioning on firms' disability risks. We measure the inflow into UI and IVA at the firm level as the fraction of workers that entered UI or IVA in a given year. As we observe a panel of firms and the dependent variables are fractions, we choose a fractional probit specification. We use the pooled Bernoulli quasi maximum likelihood estimator, as described in Papke and Wooldridge (2008). In order to be able use an unbalanced set of firms, we extend the model as proposed by Wooldridge (2010):

$$\begin{aligned} E(Y_{it}^k|X_{it}, r_{it}, a_{it}, \mu_{t,s}^k, T_i, A_i) = \Phi(\omega^k + \beta^k X_{it} + \gamma^k r_{it} + \delta^k a_{it} + \mu_{t,s}^k \\ + \xi^k T_i + \zeta^k A_i + c_i) \end{aligned} \tag{4.4}$$

for firm i with size s in year t, where Φ is the standard normal cumulative distribution function and c_i is a firm effect that is assumed to follow a normal distribution, conditional on the regressors. Indicator k denotes either inflow into UI (k=1) or IVA (k=2). The premium adjustment due to experience rating is again denoted as a_{it}. Similar to the model for firm exits (Section 4.4), we include firm and sector characteristics X_{it}, year fixed effects for small and large firms $\mu_{t,s}$ and the employer DI risk r_{it}. We include time averages of all covariates in A_i to allow for correlated random effects. Dummies for the number of observed years T_i are included to control for the number of years that a firm is included in the panel. T_i is also included in the variance equation to allow the variance of c_i to vary with T_i.

4.5.2 Main estimation results

Table 4.8 shows the Maximum Likelihood results for inflow into the UI scheme for unemployed workers and the IVA scheme for permanently and fully disabled workers. To start with, we observe that firms with high disability risks have less inflow into UI. This finding may point at substitution effects between the DI and the UI scheme. That is, workers with a high risk of disability also are more likely to be laid off. At the same time, we do find a significant and positive coefficient estimate of a premium adjustment due to experience rating. In particular, an increase of the premium of one percentage point leads to an increase in the UI inflow of 0.12 percentage point. Taking into account that the difference between maximum and minimum DI premiums has amounted to about 9% at maximum in the first years of experience rating, this is a potentially sizable effect.

For the inflow into the IVA scheme that is not experience rated, we do not find a significant coefficient estimate of the DI premium. This result may come as a surprise, as firms may have an interest in using the non-experience-rated IVA scheme to reduce wage costs. Still, one should bear in mind that eligibility conditions for the IVA scheme are very strict. Moreover, firms with higher DI premiums may aim at reducing further premium increases for the partial and/or temporary scheme (WGA) by increasing their health and safety practices. This in turn may reduce the non-experience rated risk of permanent and full disability.[18]

4.5.3 Robustness analyses and heterogeneous effects

To analyze the sensitivity of the layoff effects stemming from experience-rated DI premiums, we performed several robustness tests that are more or less similar to those for firm exits. The results of these additional estimations are given in Table 4.9. In the first panel, we repeat the estimation for a sample of firms close to the threshold of being classified as a small firm. Recall from the previous section that using this sample amplifies the identification through the removal of experience rating for small firms. We find a smaller and insignificant coefficient if we only select firms with more than 10 and less than 40 workers. One potential explanation for this finding is that the effect of the experience-rated premium on inflow into UI is larger for very small firms, as they are likely to be more liquidity constrained than larger firms.

In the second panel, we test the sensitivity of our results to measurement errors by re-estimating out model for firms with a share of permanent workers of 80% or higher. Again, this yields similar outcomes for the DI premium as for the baseline model, suggesting that measurement errors do not bias our results. Our results are also not sensitive to including second- or third-order polynomials of the DI risk as controls – see the results in the third panel of the table. We also adopted a more flexible specification of the effect of the DI premium by including a quadratic term of the premium adjustment. The coefficient of this term is negative and significant, indicating that a premium adjustment increases the inflow into UI, but the total effect is concave. The inclusion of dummies by size of the adjustment tells us that the effects on UI inflow are largest for big premium adjustments.

[18] We have estimated a similar model for the inflow into the experience-rated WGA scheme (partial and temporary disability). We find significant negative coefficients of both the DI risk and the premium adjustment.

Table 4.8: Effects of DI premium increase because of ER on inflow into UI and IVA

	Inflow UI		Inflow IVA	
Adjustment of the premium because of ER	0.2039**	(0.0207)	-0.232	(0.351)
Risk percentage	-0.5454**	(0.0267)	-0.132	(0.240)
Vacancies	-0.0024**	(0.0003)	0.0006	(0.0012)
Firm characteristics				
Small firm	-0.141**	(0.012)	0.196**	(0.091)
Less than 15 workers	0.122**	(0.019)	0.035	(0.053)
15-25 workers	0.153**	(0.018)	0.048	(0.048)
25-50 workers	0.132**	(0.017)	-0.040	(0.046)
50-100 workers	0.120**	(0.017)	-0.009	(0.045)
100-500 workers	0.104**	(0.016)	0.011	(0.042)
500-1000 workers	0.062**	(0.019)	-0.018	(0.047)
More than 1,000 workers (ref)	-		-	
Average age	0.018**	(0.0004)	0.011**	(0.004)
Percentage men	-0.028*	(0.016)	-0.079	(0.133)
Percentage of immigrants	0.139**	(0.021)	-0.013	(0.133)
Percentage of single households	0.101**	(0.014)	-0.258**	(0.080)
Percentage of single parents	0.085**	(0.019)	-0.059	(0.098)
Percentage of households with children	-0.078**	(0.013)	0.009	(0.065)
Average wage below €7,500	0.690**	(0.019)	1.056**	(0.120)
Average wage €7,500-15,000	0.574**	(0.020)	0.856**	(0.109)
Average wage €15,000-2,5000	0.392**	(0.017)	0.646**	(0.087)
Average wage €25,000-40,000	0.143**	(0.013)	0.343**	(0.097)
Average wage above €40,000 (ref)	-		-	
Effect of 1pp increase in premium	0.12		0.00	
Observations	286,657		168,111	
Log pseudolikelihood	-194,050		-2,973	

Note: Heteroskedastic fractional probit estimation results on inflow into UI and inflow into IVA scheme, 2001-2013 (UI) and 2006-2013 (IVA) Standard errors in parentheses, obtained using bootstrap with 50 replications. * significant at a level of 10%, ** significant at a level of 5%. All regressions include sector fixed effects, time fixed effects for small and large firms separately, time averages of all characteristics and controls for observed number of years in data.

As a final test on our results, we re-estimated our model on a sample of firms that are observed in all months in a particular year. As a result, we exclude firms that exited somewhere during the year. The idea behind this approach is that we exclude the possibility of layoffs stemming from increased firm exits, thus focusing on changes in layoffs of firms that continued to exist. When following this approach, we find a significant positive coefficient of the premium adjustment, with a size of about one quarter of the estimated coefficient for the full sample. This indicates that a large part of increased UI inflow is driven by firm exits, but not all of it.

To test whether the effects of a premium adjustment differ by characteristics of the firm, we have estimated the models on the same subsamples of firms as in the previous section. Table 4.10 shows the estimated coefficient of the premium adjustment for each subsample. In the previous section we found that premium effects on firm exits are larger for firms with a higher exit probability in advance – e.g. firms in the trade and business sector and firms with a low average wage. With respect to the inflow into UI, we also find larger effects for those types of firms. Given that it is more difficult to adjust the labor costs for firms with a low share of temporary workers, we would expect that these firms will react more strongly to a premium adjustment by firing workings. Our findings are in line with this.

For most of the considered subgroups, we do not find a significant effect of a premium adjustment on the IVA inflow. For firms in the trade sector we observe a positive effect, which would imply that these firms divert workers to the non-experience rated IVA scheme. For firms with a relatively low wage we observe the opposite effect, a premium adjustment reduces the inflow into IVA. A potential explanation is that firms react to a premium adjustment by improving their preventive measures and re-integration activities targeted at sick workers, leading to spillover effects in the IVA scheme.

Table 4.9: Effects of DI premium increase because of ER on inflow into UI and IVA: robustness checks.

	Inflow UI		Inflow IVA	
Baseline, full population	0.204**	(0.021)	-0.232	(0.351)
Test of research design				
Selection of firms around threshold small firm (10-40 workers)	0.016	(0.022)	-0.430	(0.516)
Test of sensitivity measurement errors				
Selection of firms with less measurement error (>80% permanent contracts)	0.189**	(0.020)	-0.298	(0.391)
Test of specification DI risk				
Second order polynomial DI risk	0.173**	(0.021)	-0.293	(0.388)
Third order polynomial DI risk	0.155**	(0.021)	-0.161	(0.305)
Test of non-linear effects premium adjustment				
Including quadratic term (one regression)				
Premium adjustment	0.820**	(0.041)	-0.351	(0.257)
Premium adjustment squared (x1000)	-0.017**	(0.001)	0.008	(0.0008)
Dummies by size of the adjustment (one regression)				
Decrease, more than 1%	-0.033**	(0.011)	-	
Decrease, 0.5-1%	0.009	(0.009)	0.013	(0.033)
Decrease, less than 0.5%	0.006	(0.007)	0.019	(0.023)
Increase, less than 0.5% (ref)	-		-	
Increase, 0.5-1%	0.002	(0.006)	-0.015	(0.036)
Increase, more than 1%	0.032**	(0.007)	-0.034	(0.066)
Excluding inflow caused by firm exits				
Only firms with year fully observed (logit)	0.047**	(0.022)	-0.185	(0.326)

Note: Heteroskedastic fractional probit estimation results. Estimations are based on firms with one plant between 2001 to 2013 (UI) and 2006 to 2013 (IVA). The estimates of the bottom row were obtained using a logit specification. Standard errors in parentheses, obtained using bootstrap with 50 replications (except for bottom row).
* significant at a level of 10%, ** significant at a level of 5%. All cells represent separate regressions, with the exception of row 6 to 12. All regressions include firm characteristics, time fixed effects for small and large firms, controls for economic conditions, time averages of all characteristics and controls for observed number of years in data.

Table 4.10: Effects of DI premium increase because of ER on inflow into UI and IVA: heterogeneity.

	Inflow UI		Inflow IVA	
Baseline, full population	0.204**	(0.021)	-0.232	(0.351)
By sector				
Trade	0.284**	(0.040)	1.168**	(0.447)
Business	0.294**	(0.062)	-0.341	(0.482)
Industrial	0.139**	(0.050)	-0.533	(0.415)
Health	-0.046	(0.070)	0.506	(0.464)
By average wage				
Below the median	0.273**	(0.024)	-1.400**	(0.676)
Above the median	0.169**	(0.027)	0.170	(0.151)
By share of temporary workers				
Below the mean	0.234**	(0.020)	-0.251	(0.488)
Above the mean	0.057*	(0.030)	-0.345	(0.301)

Note: Heteroskedastic fractional probit estimation results. Estimations are based on firms with one plant between 2001 to 2013 (UI) and 2006 to 2013 (IVA). Standard errors in parentheses, obtained using bootstrap with 50 replications. * significant at a level of 10%, ** significant at a level of 5%. All cells represent separate regressions. All regressions include firm characteristics, time fixed effects for small and large firms, controls for economic conditions, time averages of all characteristics and controls for observed number of years in data.

4.6 Conclusion

This chapter studies the effect of experience-rated firms' DI premiums on firm exits, layoffs and substitution to other social security schemes. We use matched firm- and worker data from the Netherlands from which we derive disability risks and experience-rated DI premiums in the years 2001 to 2013. Our estimation strategy exploits exogenous changes in the mapping of disability risks to DI premiums that occurred over the years. When conditioning on the firms' disability risks, we thus use variation in the translation of disability risks to constrained DI premiums. In this setting, the complete removal of experience rating for small firms between 2003 and 2007 – which implied the use of uniform premiums for those years – can be considered as the most drastic policy change that can be used to identify the isolated impact of DI premium adjustments.

Our main finding is that a positive premium adjustment due to experience rating increases the probability of a firm exit. In particular, an upward adjustment of the DI premium of one percentage point of the wage sum increases the probability of a bankruptcy with 0.44 percentage point and the probability of a merger with 0.20

percentage point. Bearing in mind that DI premiums could range from 0% to 9% of the wage sum until 2004, these are substantial effects.

The effect of DI premiums on the probability of bankruptcies suggests that some firms cannot bear the additional labor costs. At the same time, however, the effect on mergers that we find may point at strategic behavior of firms: by moving to another firm, they start paying the minimum premium again. Firms could also avoid paying a high premium by exiting the market and restarting as a new firm, but we do not find evidence for this type of behavior. One explanation for this could be that the Employee Insurance Agency is successful in tracking these firms and re-assigning DI benefit costs to them.

To gain further insight in the adverse effects of the experience-rated DI premium, we have broadened our perspective by assessing the effects of a premium adjustment on the inflow into the UI scheme and the (not-experience-rated) DI scheme for full and permanent disability. We find that a premium adjustment of one percentage point increases the inflow into UI with 0.12 percentage point. Most of this effect is driven by layoffs of firms that exit the market. We do not find evidence of substitution of workers with health problems to the not-experience-rated DI scheme.

The overall picture that emerges is that the effects of DI experience rating were sufficiently large to put some firms under financial distress. If firms are risk averse or liquidity-constrained, this may be an argument against the use of experience rating. One may also argue that firms would benefit from increasing screening practices in order to reduce the risk of increases in experience-rated premiums. At the same time, it should be stressed that the vast majority of firms has gained from experience-rated premiums, as they paid minimum premiums only. Back-of-the-envelope calculations reveal that the net effect of experience rating is negative: experience rating caused more firms to exit the market than to stay. At the same time, however, one may argue that an increase in firm exits is not necessarily bad, as more profitable and productive firms remain. Therefore, more research is needed to come to an overall assessment of the welfare implications of DI experience rating.

CHAPTER 5

A randomized experiment on improving job search skills of older unemployed workers.*

5.1 Introduction

When losing their job older job seekers have a relatively large risk of becoming long-term unemployed. In the Netherlands, for example, almost half of the job seekers aged 55 years and older are unemployed for more than two years, compared to roughly 20% of job seekers below the age of 45. Besides having longer unemployment durations and lower re-employment probabilities, older job seekers are also more likely to suffer wage losses in their new job (OECD (2006)). At the same time unemployment rates of younger and older workers do not substantially differ from each other, which implies the unemployment dynamics of younger and older workers are different.

There are several reasons why the job search behavior of younger and older workers is different. Older unemployed workers have a shorter residual time on the labor market since they are closer to retirement. Therefore, the first job after unemployment acts less as a "stepping stone" to better jobs in the future which makes older workers more selective on which jobs to accept. Another reason is that the benefits system is often more generous for older workers. In many countries, the length of the entitlement period depends on employment history, which means

*This chapter is based on De Groot and Van der Klaauw (2017b).

that older workers are entitled to longer benefits and thus have fewer incentives to accept a job quickly.[1] Not only the job search behavior of older unemployed workers may cause that they have lower re-employment rates. There can also be demand side effects. Employers can be reluctant to hire older workers because of lower returns to training and acquiring firm specific capital, increasing labor costs with age and because of negative perceptions about the flexibility and productivity for older workers (OECD (2006), Daniel and Heywood (2007)).

In this chapter we use a large-scaled randomized controlled trial (RCT) to estimate the effect of a Dutch job search assistance program called “Successfully To Employment Program” (STEP) targeted at short-term older unemployed. The aim of STEP is to increase job finding rates of older unemployed workers who have not been capable of finding work quickly. STEP is a ten-week program where groups of about 12 older unemployed workers learn several basic job search skills with a special focus on exploiting the social network of the unemployed worker. Participants learn to discuss potential employment opportunities with friends, family and former co-workers.

The RCT includes all Dutch job seekers aged between 50 and 63 who entered UI between November 2014 and July 2015 and remained unemployed for at least three months. Out of the total sample of about 50,000 job seekers, 10,000 job seekers were randomized in the control group. We use an encouragement design (Duflo et al. (2007)), in which the treatment group is encouraged to participate in STEP instead of being imposed to participate in the program. Job seekers in the treatment group were invited to STEP by their caseworker after three to four months of UI receipt, while job seekers in the control group were - in principle - not invited to participate in STEP. This resulted in a participation rate of STEP in the treatment group of 54%. Because job seekers in the control group could ask to participate in STEP themselves and because some job seekers were invited by mistake, 8% of the job seekers in the control group participated. This is still a seizable difference compared to the treatment group. By combining administrative data from several sources, we are able to estimate the effects of the program on outflow from UI and employment probabilities, but also on subsequent labor market outcomes such as earnings, contract hours and type of contract.

We find positive effects of the training on both the probability of exit from UI and the job finding probability within one year after inflow into UI. The intention

[1] Several studies show that longer entitlement periods lead to lower job finding probabilities as unemployed workers become more selective in which jobs to accept (f.e. Lalive (2008), Schmieder et al. (2012), De Groot and Van der Klaauw (2017a)).

to treat effect on the UI exit rate in the first 12 months after inflow is about two percentage points, which implies that the availability of STEP increases the exit rate within twelve months from 38 to 40 percent. As we have non-compliance in both the control and treatment group - the participation rates of STEP in the treatment and control group are 54% and 8% respectively - the local average treatment effect is substantially larger: participation in the training increases the exit from UI with 4.4 percentage points. The vast majority of the additional outflow from UI is due to increased job finding. We find that participation in the training increases the probability of having a job after one year since UI inflow with 2.2 percentage points. When we consider a longer time window of 18 months, we find that the positive effects of STEP on UI outflow persist, while the effects on employment disappear. This implies that STEP leads to faster job finding instead of additional job finding.

Our estimates indicate that participation in the training decreases the government expenditures on UI. The reduction in UI benefits increases over time and it exceeds the costs of the program after fourteen months since UI inflow. As the drop in income from UI benefits is substituted by an increase in earned wage, the total income of older unemployed workers does not seem to be affected by participation. Moreover, we find no evidence that STEP affects the quality of the first job after UI in terms of wages, the number of contract hours or type of contract.

This chapter contributes to several strands of the literature. Although there exists a wide literature on active labor market polices (ALMP) in general, so far only a handful of studies assess ALMP targeted at older unemployed workers.[2] The literature on ALMP for older unemployed mainly studies hiring subsidies or changes in job search requirements. Both Bloemen et al. (2013) and Koning and Raterink (2013) find that stricter search requirements increase the employment rates for older unemployed in the Netherlands. Boockmann et al. (2012) study the effect of hiring subsidies for older unemployed workers in Germany and do not find any effects of the subsidies on exit from unemployment to employment, except for women from East Germany. To our knowledge, only Arni (2015) studies the effect of a job search assistance program for older unemployed workers.[3] Using a randomized experiment he finds that an early intensive counseling and coaching program increases job finding

[2]See Card et al. (2010) or Card et al. (2015) for an overview.

[3]Another study that is related but focuses on older welfare recipients is that of Boockmann and Brändle (2015). They exploit regional variation in program participation to estimate the effect of a large-scale ALMP targeted at older welfare recipients, which consists mostly of coaching, job search assistance and skills assessment. They find that the program increased the probability of entering non-subsidized employment, but that participants also had a higher probability of remaining on welfare assistance because of substantial lock-in effects.

rates and decreases reservation wages for job seekers between 45 and 55 years in Switzerland. He does not find a positive effect for individuals older than 55 years.

We also contribute to the literature that uses randomized experiments in labor economics (see for example Rothstein and Von Wachter (2017) for an overview). To our knowledge, the STEP experiment with 50,000 individuals is the second-largest RCT so far.[4] In contrast to other evaluations using randomized experiments, the scale of our experiment allows us to study the heterogeneity of the treatment effects of STEP. Many studies using smaller experiments have limited external validity because they only apply to a specific region or to specific caseworkers. This is less of a concern for this study since the experiment involved the entire inflow of older job seekers who were eligible for the program in the Netherlands.

This chapter also contributes to the literature on social networks. It is widely acknowledged that social networks play a key role in finding employment. In his seminal work, Granovetter (1995) reports that more than half of the workers find their new job through family, friends, neighbors or former colleagues. In general, the employment probability of an unemployed worker seems to increase with the employment rate in the network of former co-workers (Cingano and Rosolia (2012), Glitz (2017)), friends (Cappellari and Tatsiramos (2011)) and neighborhood (Jahn and Neugart (2017)). Other studies find evidence that workers from the same neighborhood are more likely to cluster in the same firm (Bayer et al. (2008) and Hellerstein et al. (2011)) and that young workers are more likely to get a job at the plant where their parents work (Kramarz and Skans (2014)). From an alternative perspective, Brown et al. (2016) find that informally referred workers are more likely to be hired and experience a higher initial wage than non-referred workers.

Despite the literature on the importance of social networks in job finding, there is – to our knowledge – no available literature on the effects of *stimulating* the use of social networks amongst unemployed workers. This is the first study that tries to estimate the effect of such a program. Although we are not able to disentangle the effect of learning basis job search skills from the effects of stimulating using the social network of the job seeker, the estimated effects of STEP are relatively large compared to the previous literature on job search programs, which suggests that stimulating the use of social networks increases the exit rate from UI.

This chapter proceeds as follows. In the next section, we explain the job search assistance program for older unemployed workers. In section 5.3 we discuss the

[4]The largest social experiment is the Indian Welfare Reform evaluation with roughly 66,000 individuals, of which 3,000 in the control group (Beecroft et al. (2003)).

details of the randomized experiment and in section 5.4 we give an overview of the administrative data that is used to estimate the effects of the program. Section 5.5 describes the estimation strategy and presents the results. We conclude in section 5.6.

5.2 The job search assistance program STEP

Between 2008 and 2012, the unemployment rate of workers above 50 in the Netherlands more than doubled. To bring the rising unemployment rate of older workers to a halt, the Dutch government started in 2013 the four-year project "Actieplan 50pluswerkt", which translates as "Plan of Action 50-plus works". This plan is carried out by the benefits administration called UWV and consists of several elements, all directed at unemployed workers of 50 years and older collecting UI benefits. The main element of the Plan of Action is a job search program called "Succesvol naar werk", which translates as "Successfully To Employment" (STEP).[5] The goal of the program is to increase the job finding probabilities of participants by improving their job finding skills and learning how to use their social network to find a job. The program involves ten group meetings of about four hours each and two short individual meetings with a trainer. The groups consist of around twelve older job seekers and the composition of the group does not change during the program.

An overview of the subjects per meeting is given in Table 5.1. The program consists of learning basic job search skills such as rehearsing a job interview, writing a resume and finding job openings, but also improving networking skills and the use of social media. In two or three of the group meetings - most often meetings 4, 8 and 9 - a consultant specialized in contact with employers attends the meeting. This consultant gives advice on how participants can convince employers to hire them and provides recent information on job openings. In addition to the weekly meetings, participants were encouraged to have at least one talk with someone in their own social network about potential employment opportunities. The total costs of STEP are about €470 per participant.

The standard procedure is that the job seeker receives an invitation for STEP in his or her first meeting with the caseworker, which takes place around four months

[5] The other elements of the plan of action are a schooling subsidy, a fee for private temporary employment agencies if they find employment for a job seeker over 50, matching events between older job seekers and employers and a nationwide publicity campaign targeted at employers.

Table 5.1: Outline of STEP

Meeting	Subject
Group meeting 1	Introduction and test of abilities and job interests
Group meeting 2	Abilities, results of test meeting 1
Group meeting 3	Networking
Group meeting 4	STARR technique and analysis of job openings
Group meeting 5	Social media
Individual meeting 1	Discuss progress
Group meeting 6	Repetition, extension and questions
Group meeting 7	Ways of communication
Group meeting 8	Job interview
Group meeting 9	Elevator pitch
Group meeting 10	Repetition, extension and questions
Individual meeting 2	Concluding the program

Source: UWV

after entering unemployment.[6] Participation in STEP is voluntary and a caseworker is not able to sanction a job seeker for not participating, although caseworkers should strongly encourage individuals to participate.

The trainer of STEP is usually a regular caseworker of UWV who received a short course in order to provide the training. The outflow from the program is not monitored by UWV and there is no financial incentive for the trainer to stimulate outflow from the program.

The Ministry of Social Affairs and Employment provided the budget for the project for older job seekers under the condition that UWV would provide the program to at least 40,000 individuals every year. This target became one of the ten annual targets that UWV has to fulfill.[7] The nationwide targets are transformed into targets for the local offices. The outcome on the targets is part of the evaluation of the managers. In the year of our study, the target meant that each office had to provide STEP to at least 50% of the expected inflow of unemployed workers between 50 and 63 who do not exit within three months.

[6]At the time of our study, two out of the 30 local labor market offices did not organize the fourth month meetings for the majority of the unemployed workers in their region. These offices were part of a special group of offices which are, to a certain extent, free to organize the communication with job seekers as they like. They mostly invite the job seekers to group meetings in the fourth month of UI, where they inform job seekers about STEP.

[7]If UWV does not meet one or more targets it has to develop a plan in order to meet the target in the future.

5.3 The randomized experiment

In this section we describe the randomized experiment. We first discuss the set-up of the experiment and then discuss the treatment itself. Next, we describe how we implemented the experiment.

5.3.1 Set-up of the experiment

The experiment concerns all job seekers who started collecting UI benefits between November 2014 and July 2015, were between 50 and 63 years at the moment of entry into UI and collected UI benefits for at least three months.[8] After the sample selection the experiment includes about 50,000 older unemployed workers.

We randomized job seekers into a treatment and a control group based on the last digit of the social security number. We used an encouragement design, in which only the treatment group is encouraged to participate in STEP. We assigned 20% of the individuals into the control group, such that UWV would still be able to meet the participation targets set by the Ministry (see Section 5.2). As the training is restricted to individuals of 50 years and older, we picked individuals with a last digit "5" or "0" to serve as control group.

5.3.2 The treatment

The treatment that is randomized in the experiment is the invitation to STEP by the caseworker in the first meeting with the caseworker, which generally takes place in the fourth month of UI. The experiment population had this first meeting between February and October 2015. All the caseworkers were informed that they should not invite individuals from the control group to participate in STEP. However, for ethical reasons job seekers from the control group can still participate in STEP if they ask to participate themselves. Job seekers in the control group would be able to find information about the availability of STEP on the UWV website.

We stressed to the caseworkers that individuals in the control group should not receive an alternative to the program, unless that alternative would also have been

[8] In the Netherlands, a worker is entitled to UI benefits if she loses at least five working hours, or 50 percent of her working hours if she works less than ten hours. We restrict the experiment to individuals to job seekers who were not employed at the start of UI for more than eight hours a week. In the Netherlands it is also possible to collect DI benefits for partial disability and hold a part-time job at the same time. If the worker loses this job, she will be entitled to both Disability Insurance (DI) benefits and UI benefits. These job seekers are also excluded from the experiment.

provided if the individual would have been in the treatment group.[9] Even though the experiment officially ended in October 2015, individuals in the control group are still not supposed to be invited for the training up to three years after inflow into UI.

5.3.3 Implementation of the experiment

Over 1,250 caseworkers are involved in the experiment. Because of the size of the experiment we took several measures to ensure that caseworkers complied. Before the experiment started, we made a visit to each local office to inform them about the experiment. We presented the set-up of the experiment and answered questions of caseworkers or managers. To make sure caseworkers do not forget about the experiment, the operating system has been altered. If a caseworker plans a meeting with someone from the control group, he or she receives a warning not to invite the job seeker to STEP.

Every week we monitor the progress of the experiment. If we observe that an individual in the control group participates in STEP, a manager from the head office of UWV notifies the manager of the responsible caseworker. A summary of the average participation rates in the treatment- and control group in each office is sent around every month so local offices can compare their progress with other offices. The monitoring will continue until July 2018, because the control group is excluded from participation in the training for three years.

5.4 Data

5.4.1 Data sources

In our analysis, we use administrative data from several sources with information on employment and UI spells. Using social security numbers, we are able to match monthly data from the Unemployment Register to monthly data in the Employment Register. These registers contain detailed information such as wages, type of contract and the level of UI benefits. We observe the unemployment and employment spells for the five years before the start of the experiment up to at least 18 months since UI inflow. The administrative data also include information on the personal characteristics of the unemployed workers.

[9] At the time of the experiment, UWV had little means to provide alternative job search assistance to unemployed. They could offer short online courses and occasionally organized a "speeddate event", where unemployed workers could meet temporary employment agencies.

Detailed information on re-integration activities and caseworker meetings is registered in a separate data set. We match the UI spells to this data in order to observe the date of the first meeting with the caseworker - which is the moment when the treatment group is invited to STEP - and the start date of STEP.[10] We also observe whether job seekers received additional job search assistance.

5.4.2 Descriptive statistics

Table 5.2 shows the main characteristics of individuals in the treatment and control group. The first panel describes the meetings with the caseworker and participation in job search programs. Recall from Section 5.2 that unemployed workers are generally invited to STEP in the first meeting with the caseworker, which takes place in the fourth month of UI. We observe in Table 5.2 that around 83 percent of the unemployed attended this first meeting with the caseworker. There are several reasons for not attending the first meeting, some unemployed workers accepted a job before their first meeting took place, others were unable to attend due to sickness, a part-time job or a holiday and two out of the 30 local UI offices only organized the first meeting for about 20% of the unemployed workers. Because one of these two UI offices organized (unregistered) group meetings for individuals in the treatment group while 50% of the individuals in the control group were invited for the first meeting with the caseworker, we observe a somewhat higher attendance of the first meeting in the control group.[11]

In the second row of Table 5.2, we observe that over half of the job seekers in the treatment group participates in STEP. According to caseworkers, job seekers can have various reasons for not participating in the training. The reasons that they mention most often are that the job seeker has health problems, already has a (part-time) job with irregular working hours, participated in a similar program in a previous UI spell or simply does not want to participate. As a result of the experiment, the participation in STEP is about seven times as high in the treatment group compared to the control group. Of the 8% non-compliance in the control group, 2% asked the caseworker if they could participate in STEP themselves. The remaining 6% was mostly invited by mistake. There is also some evidence that a few caseworkers

[10] Unfortunately, we do not know whether the individual attended more than one group meeting, as usually only the attendance to the first meeting of STEP is recorded. Survey information shows that roughly 30 percent of the respondents who participated in STEP attended all ten group meetings, 42 percent attended eight or nine meetings, 20 percent attended between five and seven meetings and the remaining eight percent attended four meetings or less.

[11] Our results are robust to removing this UI office from the analysis.

ignored the rules of the experiment if they believed the unemployed worker would really benefit from the training. This means that we might underestimate the effect of the training.

We do not observe significant differences for other re-integration programs. Here, we only include re-integration activities without a direct link to employment, because other activities such as a trial placement could be a direct effect of participation in STEP. We will discuss participation in those activities in the next section.

The other panels of Table 5.2 show the average observed characteristics for both control group and treatment group, which are very well balanced. The average age of the individuals in the experiment is 55.9 years. About 45% of the unemployed workers are female and the majority is married or cohabiting. About 45% of the individuals attended secondary education, and more than 23% have at least a bachelor degree. The fourth panel shows the characteristics of the UI spell. On average the job seekers in the experiment are unemployed for 32 hours per week and entitled to monthly UI benefits of €1,511 for a maximum period of 153 weeks. About 16% have a (part-time) job at the start of their UI spell. Seven percent of the unemployed workers experienced a UI spell in the half year before the current UI spell, in which case the previous UI spell continues and the length of the entitlement period is reduced by the length of the previous UI spell.

The fifth panel considers the characteristics of the last job before UI. The average wage in the last job was about €30,500, they worked on average 129 hours per month and 43% of the workers had a fixed-term contract before UI. Two percent of the job seekers did not have a job just before entering UI but collected DI benefits. Roughly 20% of the unemployed workers had a job in health care before UI and an equal share a job in the business sector. In comparison, the share of those sectors for the full UI inflow of individuals aged between 15 and 67 in 2014 was about 15% for both sectors (UWV (2015)) Roughly 11% of our sample had a job at a private temporary employment agency, 12% had a job in the industrial sector and about 9% in the trade sector.

5.4.3 The participants of STEP

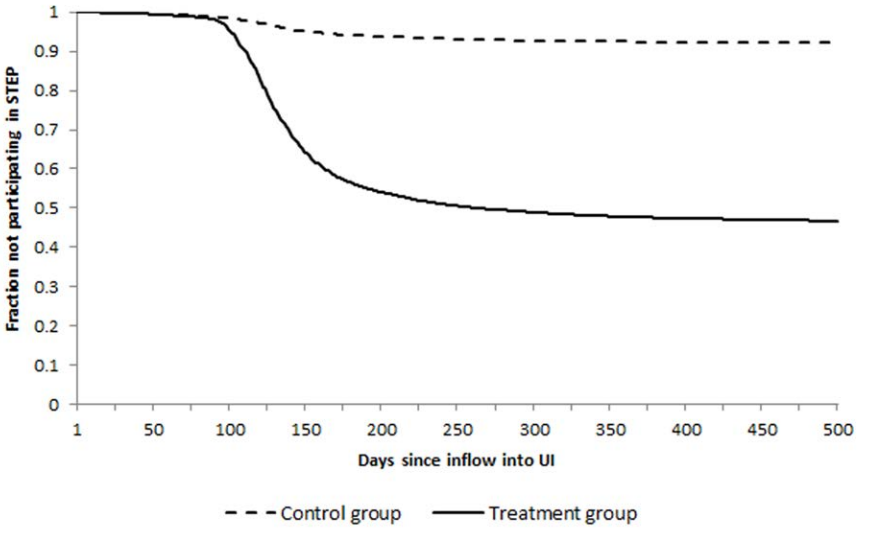
To explore the timing of participation in the training, Figure 5.1 shows the survival curves of not participating in the training for the control and treatment group. Although the majority of the job seekers is invited to the training after three months of collecting UI benefits, some job seekers start the training within the first three

Table 5.2: Descriptive statistics for treatment and control group and p-values of t-test of different means

	Treatment group	Control group	Difference	P-value
Meetings, other programs and participation in STEP				
Attended first meeting caseworker (%)	82.9	83.7	-0.8	0.05
Participated in STEP (%)	53.7	8.1	45.6	0.00
Other re-integration activities (%)	5.3	5.0	0.3	0.19
Personal characteristics				
Average age	55.9	55.9	-0.1	0.14
Women (%)	45.0	44.5	0.4	0.45
Single (%)	24.5	24.9	-0.3	0.49
Immigrants (%)	1.1	1.2	-0.1	0.57
Education (%)				
At most primary education	13.8	13.7	0.1	0.84
Vocational education	17.0	17.3	-0.3	0.43
Secondary education	45.2	44.8	0.4	0.52
Bachelor or master	23.7	23.7	0.0	0.98
UI characteristics				
Average UI benefits (€ per month)	1,511	1,513	-2	0.83
Maximum entitlement period (in weeks)	153	153	0	0.22
Number of hours unemployed	31.6	31.6	-0.1	0.41
Employed at start of UI spell (%)	16.3	16.5	-0.2	0.71
Previous UI spell in six months before	6.5	6.9	-0.4	0.14
Characteristics job before UI				
Average wage (€ per year)	30,498	30,527	-29	0.93
Temporary contract (%)	42.5	42.5	0.0	0.98
Average monthly contract hours	129	130	-1	0.51
Received disability benefits	2.0	2.1	-0.1	0.59
Sector last job (%)				
Business	20.4	20.4	0.0	0.99
Health care	20.5	20.1	0.4	0.33
Industrial	11.7	12.2	-0.5	0.17
Temporary employment agency	11.1	11.6	-0.4	0.24
Trade	9.0	8.6	0.4	0.17
Transport	6.6	6.6	0.0	0.93
Other	20.6	20.6	0.1	0.91
Observations	39,984	9,938		

months after UI. In both the control and the treatment group, most job seekers start the training between three and five months after entering UI. The gap in participation rates between treatment and control group slowly increases over time.

Figure 5.1: Survival curve of not participating in the training STEP



So far, we focused on differences in characteristics between job seekers in the treatment and control group. To see whether participants significantly differ from non-participants, Table 5.3 shows the estimation results of two regressions where we estimate participation in STEP on the characteristics of job seekers within the treatment or within control group. If we impose monotonicity - there are no job seekers who would participate in STEP if they are in the control group and who would not participate if they are in the treatment group - we can define the non-participants in the treatment group as never-takers and the participants in the control group as always-takers. This means we can interpret the estimation results in column two and three as the correlation between the job seekers' characteristics and the probability of being a complier or always-taker compared to a never-taker. The main take-away from Table 5.3 is that job seekers are more likely to be a never-taker if they are men, immigrants or above the age of 60. Job seekers are also more likely to be a never taker if they are employed at the start of UI or if they received UI of DI benefits in the period before UI.

Table 5.3: Estimation results for participating in STEP (dependent variable) within the treatment and control group

	Treatment group		Control group	
	Coefficient	Standard error	Coefficient	Standard error
Personal characteristics				
Man	-0.100**	(0.006)	-0.031**	(0.007)
Couple	-0.002	(0.005)	-0.018**	(0.006)
Immigrant	-0.254**	(0.023)	-0.031	(0.026)
At most vocational education	-0.083**	(0.007)	-0.011	(0.008)
Secondary education	-0.002	(0.006)	0.004	(0.007)
Bachelor or master (reference)	-		-	
Younger than 55 (reference)	-		-	
55-59 years at inflow	0.009	(0.006)	-0.002	(0.007)
60-63 years at inflow	-0.085**	(0.007)	-0.027**	(0.008)
UI characteristics				
Maximum entitlement period	0.002**	(0.000)	0.000	(0.000)
Number of hours unemployed	-0.001*	(0.000)	0.001**	(0.000)
Employed at entry UI	-0.129**	(0.007)	-0.028**	(0.008)
Characteristics job before UI				
Re-entry unemployment	-0.197**	(0.010)	-0.030**	(0.011)
Received DI benefits	-0.162**	(0.017)	0.058**	(0.019)
Had temporary contract	-0.092**	(0.006)	-0.014**	(0.006)
Wage below €25,000 (ref.)	-		-	
Wage above €25,000	0.040**	(0.006)	-0.005	(0.007)
Sector: Health care	0.031**	(0.008)	0.002	(0.009)
Sector: Business	0.072**	(0.007)	0.010	(0.008)
Sector: Temp. empl. agency	-0.026**	(0.009)	-0.009	(0.010)
Sector: Industrial	0.059**	(0.008)	0.013	(0.009)
Sector: Trade	0.047**	(0.009)	0.001	(0.011)
Sector: Other (ref.)	-		-	

Note: Regressions include month of UI inflow fixed effects and local UI office fixed effects

Column four and five of Table 5.3 show the estimation results for participation in the control group. These results can be interpreted as the correlation between individual characteristics and the probability of being an always-taker. Job seekers in the control group are more likely to be an always-taker if they are younger than 60 years, are not employed at the moment of UI inflow or if they collected DI benefits before entering UI.

There are three reasons why someone in the control group participates. The job seeker asks the caseworker to participate in STEP, the caseworker invites the job seeker by mistake or the caseworker ignores the experiment and intentionally invites someone in the control group. We are not able to distinguish between the latter two reasons, but we do observe whether someone asked to participate themselves. In Table 5.A1 in the Appendix to this chapter we estimate the probability of asking for STEP in the control group. We observe that women, individuals with high UI benefits and individuals with secondary education or higher are more likely to ask to participate in STEP.

5.5 Effects of STEP

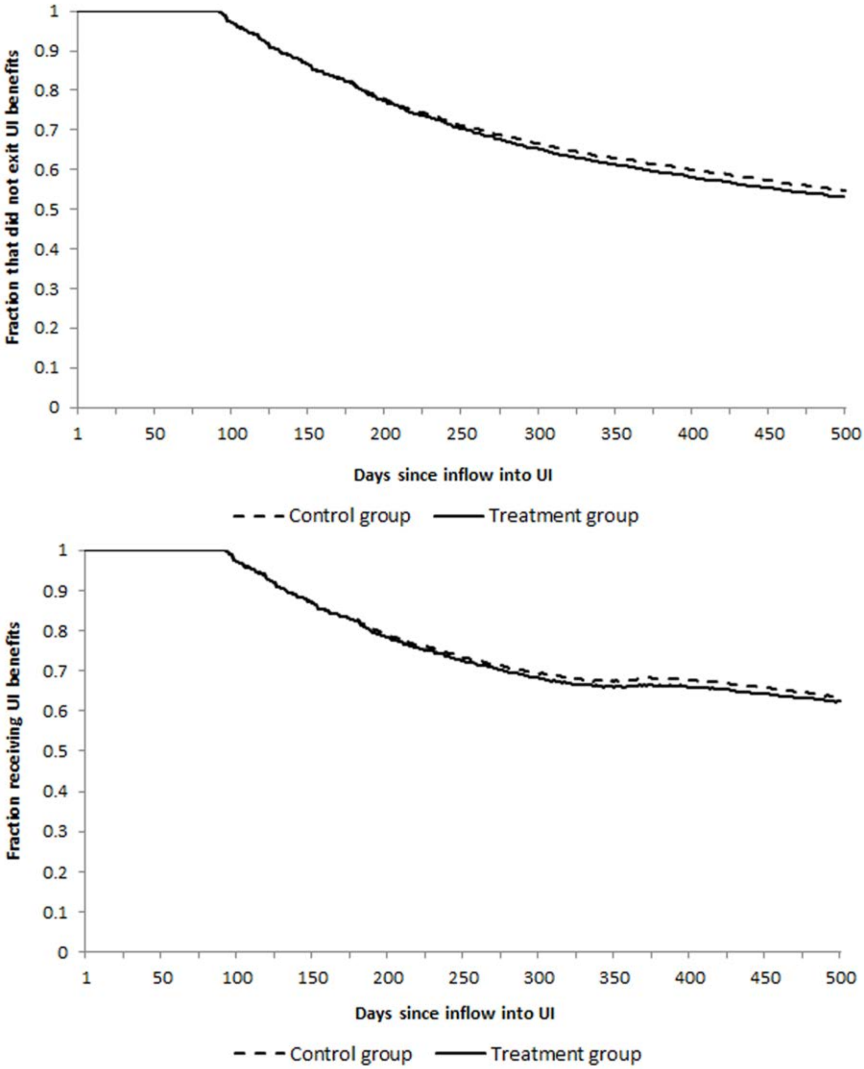
In this section, we present the estimated effects of STEP. We first show some graphical evidence of the potential effects of STEP. Next, we describe our estimation strategy and estimate Intention To Treat (ITT) and Local Average Treatment Effects (LATE) of participating in STEP. Finally, to test whether the effects differ by characteristics of the participants, we present the estimates for several subgroups.

5.5.1 Graphical evidence

As a first examination of the potential effects of STEP, the upper panel of Figure 5.2 shows the Kaplan-Meier estimate of the survival in unemployment benefits for the treatment and control group. Because we only select individuals that received UI benefits for at least three months, we do not observe outflow from UI in that period. Recall that according to UWV guidelines, an individual should be invited to the training after three months and that the training will last about ten weeks. This implies that someone would participate in the training between month four and six. For this period, we do not observe a difference in the survival curve between the control and treatment group, which could imply that there is no substantial locking-in effect of participation in the training. This is not surprising as participating in STEP is not too time consuming and participants are encouraged to keep searching and applying for jobs. After about seven months - when most participants have finished STEP - the survival curve of the treatment group drops below the survival curve of the control group. The difference between the control and treatment group slowly increases over time.

In the lower panel of Figure 5.2 we show the fraction of individuals in the treatment and control group who receive UI benefits. In contrast to the upper panel, here we also account for re-entry into UI. Again we observe a lower fraction of benefit receipt in the treatment group after about seven months since UI inflow. After about one year since the first inflow into UI, we observe a small increase in the fraction of

Figure 5.2: Kaplan-Meier estimate for the survival in unemployment benefits (upper panel) and the fraction of individuals receiving UI benefits including re-entry into UI (lower panel) in the treatment and control group.



benefit receipt, which implies that some workers return to UI. However, the difference in the fraction of benefit receipt between the treatment and control group persists.

5.5.2 Estimation strategy

To estimate the effect of STEP on several outcomes Y, each measured at different moments since inflow into UI, we specify the model

$$Y = \alpha + STEP\gamma + X\beta + \epsilon \tag{5.1}$$

The variable $STEP$ indicates whether the job seeker has received the job search program in the time period under consideration. The vector X contains individual characteristics, such as gender, household composition, nationality, earnings before entering UI, sector, age, the maximum entitlement period and local UI office fixed effects. We also include dummy variables for each month of inflow into UI in X to control for calendar time variation.

It is likely that participation in the training is not independent of the potential outcome, for example because more motivated individuals have a higher probability to participate or because caseworkers select those individuals of whom they expected the highest return of the training. For this reason we exploit the experimental design of our study and replace $STEP$ by T, a dummy variable which is equal to one if the job seeker was assigned to the treatment group:

$$Y = \alpha + T\gamma + X\beta + \epsilon \tag{5.2}$$

Since only 54% of the individuals in the treatment group actually participates in the training, γ describes the Intention To Treat effect (ITT). Given that at the time of our experiment UWV had a target to maximize the number of participants in STEP and that the budget to facilitate STEP was not exhausted, we can assume that none of the eligible job seekers in the treatment group were denied access to STEP. Therefore, it seems that not every eligible job seeker will eventually participate in STEP. The intention to treat effect thus seems to be a policy relevant effect, as it tells us what the effect of STEP is for the entire group of job seekers over 50. Note, however, that we underestimate the intention to treat effect because there is non-compliance in the control group.

To estimate the effect of participating in STEP, we estimate the Local Average Treatment Effect (LATE) by estimating equation (5.1) using instrumental variables. We now use the treatment dummy T as an instrument for participation in $STEP$ in the considered time period. This gives us the first stage estimation:

$$STEP = \kappa + T\lambda + X\zeta + \upsilon \tag{5.3}$$

In order for assignment to the treatment group T to be a valid instrument for participation in STEP, we need to assume that never-takers in the treatment group are not affected by assignment to the treatment. In other words, if the invitation itself - regardless of whether someone participates - affects the outcomes Y, for example if job seekers in the treatment group anticipate the invitation to STEP and try to avoid participation, for example by increasing their job search activity to find employment in order to leave UI. We argue that this is unlikely to occur, because participation in STEP is voluntary and caseworkers are not able to sanction job seekers who do not want to participate in STEP.

5.5.3 Estimated effects of STEP

5.5.3.1 UI benefit receipt

In Table 5.4 we present the estimated ITT and LATE effects for the outflow from UI, UI benefit receipt and cumulative UI benefits at different moments after inflow into UI. We find a significant effect of the invitation to STEP on the outflow from UI after nine, 12 and 18 months since the moment of inflow. One year after inflow into UI, the exit probability of the treatment group is 2.0 percentage points higher than in the control group, which implies an increase from 38% to 40%. We find a local average treatment effect of 0.044 for the outflow within 12 months, implying that participation in STEP increases the exit rate from UI with 4.4 percentage points. The point estimate of the effect of STEP on the outflow within 18 months is similar to the effect on outflow within 12 months, which suggests that the effect stabilizes one year after UI inflow.

A job seeker can exit UI for different reasons. To see if STEP affects all types of exits in the same way, we split the outflow from UI within twelve months by the four main reasons of exit. The second panel of Table 5.4 shows the estimations results of the effect of STEP by type of outflow. The most common reason to exit UI is because of work, this accounts for about two-thirds of the outflow in the control group. Employment seems to be the only channel of the increased outflow due to STEP, as we only observe a significant positive effect on outflow to work. Participation in STEP increases the outflow to work with 2.6 percentage points. We do not find a significant impact on exits because of sick leave, exhaustion of UI entitlement or other types of outflow.

Potentially, individuals who exit UI could return to unemployment almost immediately after their exit, for example because they found a very short lasting job. In the third panel of Table 5.4 we account for re-entry into UI by measuring the effects of STEP on the probability of receiving UI benefits at different moments after (first) inflow into UI. We find significant effects of the training which are somewhat smaller in size than the effects on outflow UI. This means that a fraction of the additional outflow from UI returns to unemployment at a later stage. Participation in the training decreases the probability of UI receipt after 12 months with 3.5 percentage points. If we compare the effects on outflow to the effects on UI benefit receipt, the estimates on benefit receipt are roughly 75% the size of the effects on outflow. This means about one quarter of the additional outflow because of STEP, returns to unemployment benefits in the following months.

Finally, the last panel shows the cumulative UI benefits for different periods since UI inflow. This outcome variable also contains the UI benefits of re-entering UI spells. The estimated ITT- and LATE-effects are significant for the UI benefits of nine months and longer. Participation in the training reduces the received UI benefits within one year with €393. Eighteen months after inflow into UI, the reduction in UI benefits exceeds the costs of STEP of €470 per participant. This means that for the group of participants in the experiment, STEP is a cost-effective program.

To study the effect of STEP over time in more detail, Figure 5.3 shows the estimated local average treatment effect on the outflow from UI, UI benefit receipt and cumulative UI benefits for every month after inflow into UI with the corresponding confidence interval. In the upper panel we see a steadily increasing effect of participating in STEP on the outflow from UI up to 14 months after inflow. In the subsequent months the effect appears to stabilize around four percentage points. In the middle panel we observe the opposite pattern: the negative effect of STEP on UI benefit receipt increases in size up until roughly one year after inflow and stabilizes at three percentage points afterwards. Finally, in the lower panel we see that the effect on cumulative UI benefits is significant from eight months after UI inflow and appears to be linearly increasing for every additional month that is added to the time window. Fourteen months after inflow, the reduction of cumulative UI benefits exceeds the costs of STEP per participant.

Figure 5.3: Estimated Local Average Treatment Effects on UI outflow (upper panel), UI benefit receipt (middle panel) and cumulative UI benefits (lower panel) by month after inflow UI

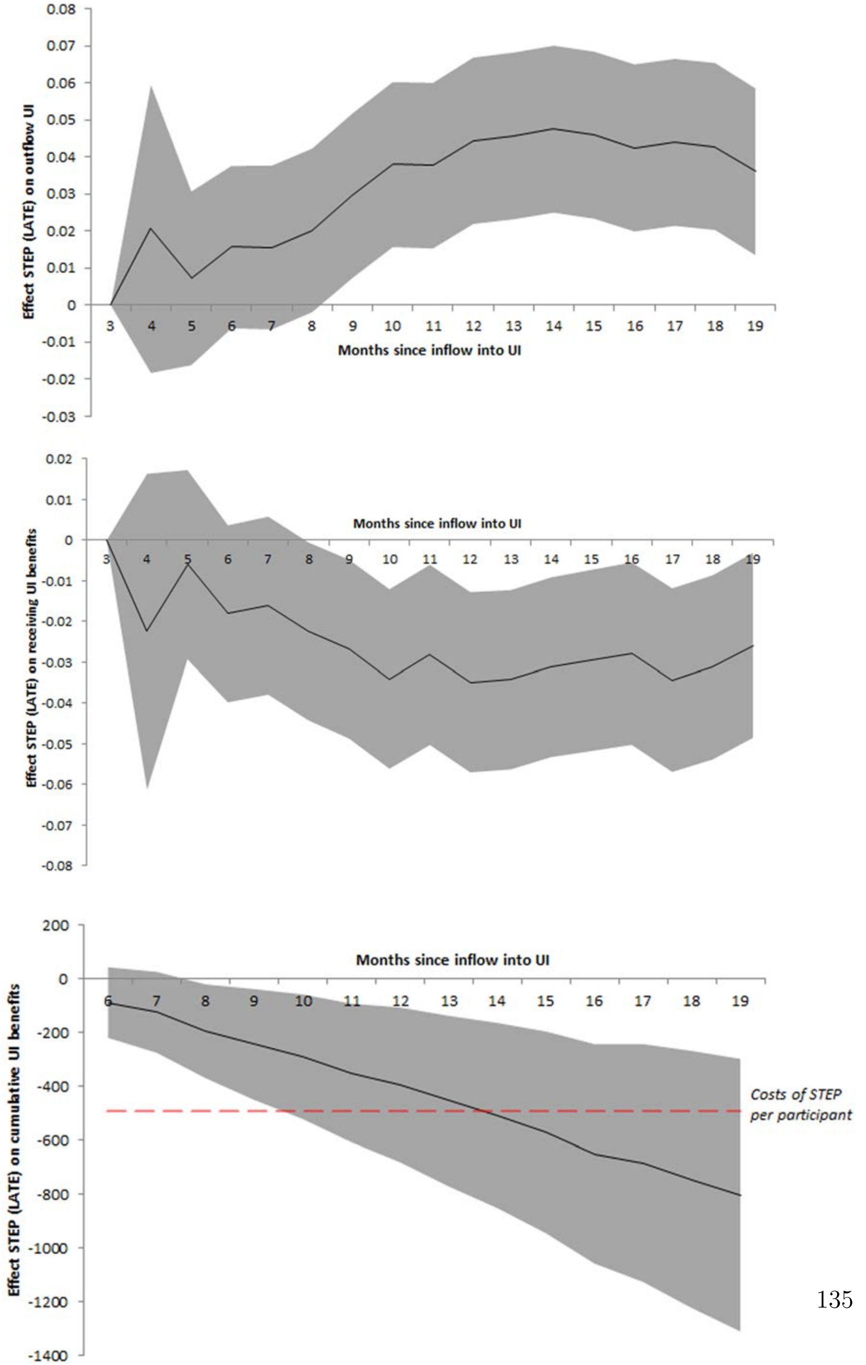


Table 5.4: Estimation results for the effect of the training on outflow from UI, UI receipt and cumulative UI benefits, intention to treat and local average treatment effect

	ITT		LATE		Mean control
Outflow from UI					
Outflow UI within 6 months	0.006	(0.004)	0.016	(0.011)	0.19
Outflow UI within 9 months	0.012**	(0.005)	0.030**	(0.011)	0.31
Outflow UI within 12 months	0.020**	(0.005)	0.044**	(0.012)	0.38
Outflow UI within 18 months	0.019**	(0.005)	0.043**	(0.011)	0.47
Outflow UI within 12 months by reason of exit					
Outflow to work	0.012**	(0.005)	0.026**	(0.010)	0.28
Outflow to sick leave or disability	0.004	(0.003)	0.009	(0.006)	0.06
Outflow because end of entitlement	0.001	(0.001)	0.002	(0.002)	0.01
Outflow other	0.003	(0.002)	0.007	(0.004)	0.03
Receives UI benefits					
Receives UI after 6 months	-0.007	(0.004)	-0.018	(0.011)	0.82
Receives UI after 9 months	-0.011**	(0.005)	-0.027**	(0.011)	0.72
Receives UI after 12 months	-0.016**	(0.005)	-0.035**	(0.011)	0.70
Receives UI after 18 months	-0.015**	(0.005)	-0.031**	(0.012)	0.64
Cumulative received UI benefits after inflow (€)					
UI benefits within 6 months	-28	(25)	-88	(67)	9,082
UI benefits within 9 months	-95**	(45)	-244**	(105)	12,789
UI benefits within 12 months	-163**	(64)	-393**	(146)	15,827
UI benefits within 18 months	-326**	(109)	-745**	(244)	22,381

Note: Every cell represents a separate regression. The estimations include controls for personal characteristics, time fixed effects, regional effects and characteristics of the job before UI. Standard errors in parenthesis. * significant at the 10% level, ** at the 5% level.

5.5.3.2 Labor market outcomes

In the previous subsection we observed that participation in STEP increases the outflow to work. To study the effects of STEP on labor market outcomes in more detail, Table 5.5 shows the estimated ITT and LATE effects on several labor market outcomes after entry into UI. In the first panel we take into account that not all jobs are consistent by considering the probability of having a job at different moments since inflow into UI. This variable is equal to one if the job seeker has a job with a positive wage at the considered moment, irrespective of the number of contract hours of the job. We find that STEP has a positive effect on having a job at six, nine and

12 months after UI inflow. One year after inflow, someone in the treatment group is 1.0 percentage point more likely to have a job compared to job seekers in the control group. This is an increase of about 3%. When we estimate the effect of STEP using IV, we find that participation in STEP increases the probability of having a job after one year by 2.2 percentage points. Eighteen months after inflow into UI, however, STEP no longer has a significant effect on the probability of having a job, implying that either the additional jobs found because of STEP were not persistent or that the control group caught up. It also means that the effect on outflow to work was mostly due to *earlier* job finding instead of *additional* job finding.

The effects on the cumulative earnings from employment and total income are given in panel two and three. The cumulative earnings equal the sum of the earned wage in all jobs since inflow into UI. Unfortunately, we are not able to observe earnings from self-employment so these earnings are not included.[12] We find a positive effect of the training on the earned wage for all considered time windows. Cumulative over the first year after inflow UI, participants of STEP earn about €500 more because of the training. The negative effect on UI benefits (Table 5.4) is thus offset by a positive effect on wages, which is also reflected by the insignificant effect on the cumulative income (the sum of wages and UI benefits). This implies that participants substitute UI benefits with earnings from employment.

To provide some further insight in the quality of the job after unemployment, the fourth panel shows the effects of STEP on the characteristics of the first job after entry into UI. Obviously, these characteristics are only observed if someone accepted a job in the time period under consideration. Given that we observe higher probabilities of having a job one year after UI inflow in the treatment group, the composition of employed in the treatment group may differ from those employed in the control group. This would imply that differences in the characteristics of the job between the treatment group and control group are not necessarily the direct effect of STEP, but can also be caused by selection.

To shed light on this issue, we compare the characteristics of employed individuals between the treatment and control group in Table 5.A2 in the Appendix. Not surprisingly, employed workers in the treatment group more often participated in STEP than their counterparts in the control group. We also observe that the percentage of workers who attended the first meeting with a caseworker is about 2.5

[12]Benefits from welfare or sick leave are also not included. We believe this will not bias our results, because we do not find evidence for effects of STEP on the outflow from UI to sick leave. Given that only 1% in our sample has exhausted UI benefits, the take-up of welfare will be negligible.

percentage points lower in the treatment group. This difference is caused by one local UI office which invites some individuals from the treatment group in (unregistered) group meetings while the control group is invited to individual meetings. We do not observe any significant differences between employed individuals in the treatment and control group for other characteristics, such as pre-unemployment earnings, education, age or UI entitlement. Therefore, we argue that potential bias stemming from dynamic selection is limited.

Returning to panel four of Table 5.5, we do not find significant effects of STEP on post-unemployment wages or contract hours. This means that either STEP does not lead to better job offers, or that a potential positive effect of STEP on job offers is reversed by a negative effect of STEP on reservation wages. There is no strong evidence that STEP increases the number of job seekers who accept a fixed-term contract or a job at a temporary employment agency. One of the aims of STEP was to induce participants to search more broadly. However, we do not find that participants of STEP are more likely to accept a job in a different sector compared to the job before UI.

One of the channels through which a job seeker can find a job is via a referral of a caseworker or trainer. If participants of STEP are more likely to find a job via these referrals, this could come at the costs of non-participants because these vacancies are no longer available to them. UWV records the placement on vacancies in the database of UWV to track whether job seekers find work with the help of the caseworker. The last row of panel four shows the effect of STEP on the probability of having found a job through one of these vacancies in the UWV database. About 12% of the job seekers in the control group finds a job through one of these vacancies. We do not find evidence that participants of STEP are more likely to find a job via a referral of a caseworker or trainer of UWV.

In the last panel we estimate the effect of STEP on the take-up of several re-integration instruments of UWV which require that the unemployed has already found or is close to finding employment. We find that STEP increases the take-up of educational vouchers and increases the number of trial placements at firms. This could be a direct effect of the higher outflow to work in the treatment group, for example because certain jobs require certificates which can be paid for with an education voucher. However, during the training participants of STEP are informed about both instruments, so this can explain the higher take-up. For this reason, we should account for the costs of these two instruments if we compare the costs of STEP to the reduced UI benefits in the costs-benefits analysis. Instead of €470 per participant, the costs of STEP then amount to €490 per participant.[13] This does not change the result that the reduction in UI benefits exceeds the costs of STEP after 14 months.

To provide some further insight in the effects of STEP over time, Figure 5.4 shows the estimated local average treatment effect on the probability of having a job (upper panel), cumulative earnings from employment (middle panel) and cumulative income for every month after inflow into UI with the corresponding confidence interval. The effect of STEP on the probability of having a job occurs early in the UI spell - already during the period in which most participants are still participating in STEP - and seems to decrease somewhat after one year. The effect on cumulative earnings is steadily increasing over time while the effect on cumulative income is decreasing after the first year since inflow.

5.5.4 Heterogeneous treatment effects

In this subsection we explore whether the estimated treatment effects of the program differ by characteristics of the unemployed worker. We estimate the LATE of STEP on the monthly outflow from UI for several subsamples based on age, education and gender, and the earned wage, number of contract hours and type of contract in the last job before UI inflow. Figures 5.5 and 5.6 show the point estimates for these estimations by subgroup. In the first panel of Figure 5.5 we split the sample by age. We consider individuals aged between 50 and 54, between 55 and 59 and of 60 years and older. In the first year after UI inflow, the estimated effects of STEP show similar patterns for the three different age groups. After one year, the effect of STEP for the youngest group of job seekers is declining, while the effect increases for the older job seekers. 18 months after UI inflow, STEP increased the outflow of job seekers of 60 and older by roughly seven percentage points, while it increased the outflow of job seekers between 50 and 54 and job seekers between 55 and 59 by 2.5 and 4.4 percentage points respectively.

In the second panel of Figure 5.5 we observe somewhat larger effects of STEP for men. The differences by education (panel three) are more substantial: STEP does not seem to affect the outflow probability of individuals with at most vocational education while the effects of STEP are largest for individuals with secondary education. In the first panel of Figure 5.6we split the sample by wage. For job seekers who earned

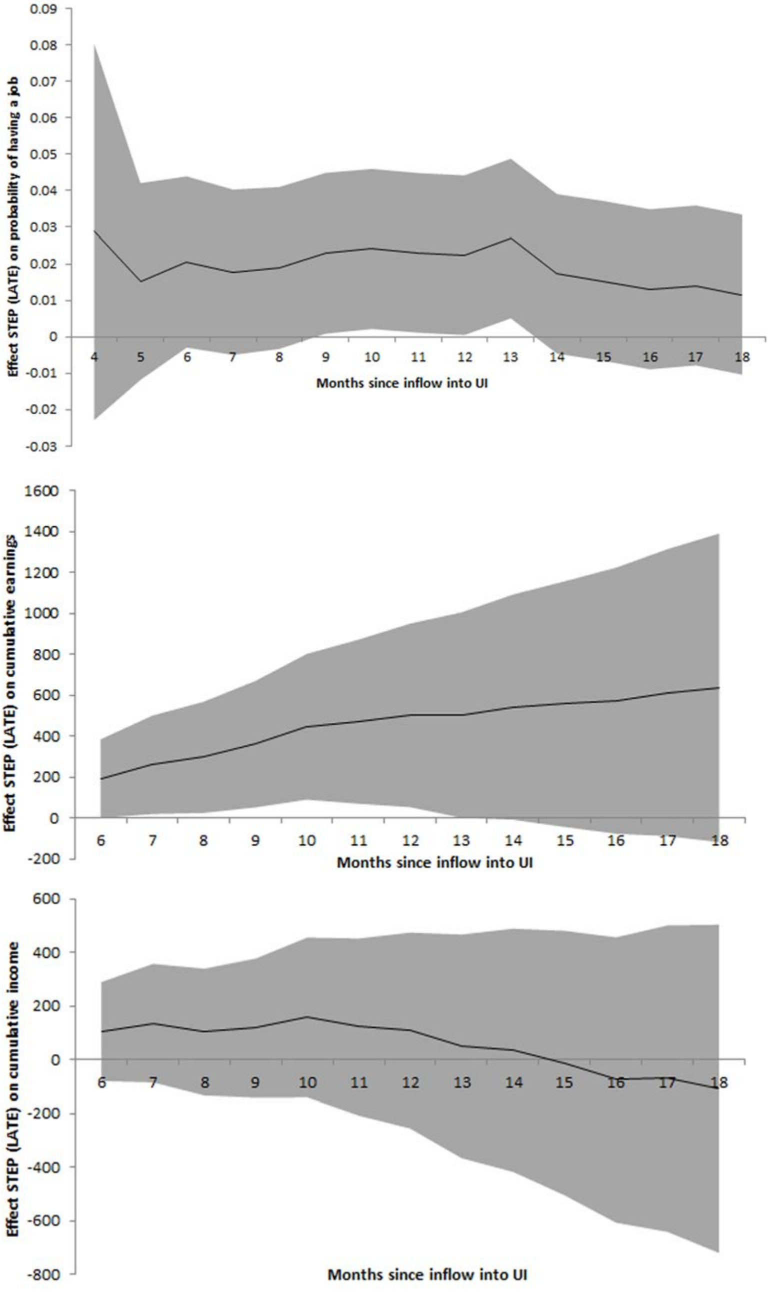
[13] The average cost of an educational voucher is €775. The costs of trial placements are already included in the cumulative UI benefits because the wage costs of a temporary placement are covered through UI benefits.

Table 5.5: Estimation results for different labor market outcomes, intention to treat and local average treatment effect

	ITT		LATE		Mean control
Has job after inflow UI					
Has job at 6 months	0.008*	(0.004)	0.021*	(0.012)	0.24
Has job at 9 months	0.010**	(0.005)	0.023**	(0.011)	0.31
Has job at 12 months	0.010**	(0.005)	0.022**	(0.011)	0.34
Has job at 18 months	0.005	(0.005)	0.012	(0.011)	0.39
Cumulative earnings after inflow (€)					
Earnings within 6 months	70*	(37)	193**	(98)	1,438
Earnings within 9 months	152**	(67)	362**	(158)	3,102
Earnings within 12 months	220**	(101)	502**	(229)	5,055
Earnings within 18 months	288*	(174)	637*	(385)	9,565
Cumulative income after inflow (€)					
Income within 6 months	42	(35)	105	(94)	10,520
Income within 9 months	57	(57)	118	(133)	15,891
Income within 12 months	57	(83)	109	(186)	20,892
Income within 18 months	-38	(141)	-108	(312)	31,947
Characteristics first job UI					
Wage (month, €)	7	(22)	19	(57)	1886
Monthly contract hours	-0.08	(0.75)	-0.28	(1.96)	111.8
Has a fixed term contract	0.008	(0.006)	0.019	(0.017)	83.6
Works at temp. employment agency	0.009	(0.006)	0.024	(0.017)	25.3
Works in different sector	-0.009	(0.008)	-0.025	(0.020)	58.1
Finds work through vacancy UWV	0.006	(0.005)	0.017	(0.013)	12.4
Re-integration instruments linked to employment					
Educational voucher	0.014**	(0.003)	0.032**	(0.008)	9.7
Placement fee temp. agencies	-0.001	(0.003)	-0.003	(0.007)	10.2
Trial placement	0.003*	(0.002)	0.007*	(0.003)	2.4

Note: Every cell represents a separate regression. The estimations include controls for personal characteristics, time fixed effects, regional effects and characteristics of the job before inflow UI. Standard errors in parenthesis. * significant at the 10% level, ** at the 5% level.

Figure 5.4: Estimated Local Average Treatment Effects on having a job (upper panel), cumulative earnings from employment(middle panel) and cumulative income (lower panel) by month after inflow into UI



a wage below the median, the effect of STEP occurs much later in the UI spell than for those who earned a wage above the median. Six months after UI inflow, the effect on outflow for high wage job seekers is 3.2 percentage points compared to an insignificant effect of 0.3 percentage points for low wage job seekers. Eighteen months after UI inflow the effect sizes of the two groups are comparable. In the fifth panel we split the sample by the number of contract hours of the job before UI. The effects are largest for individuals who worked less than 20 hours a week. Finally, in the last panel we compare job seekers who had a fixed-term contract before UI to job seekers who had a permanent contract. The effect of STEP occurs much later in the UI spell for job seekers who had a fixed-term contract compared to those who had a permanent contract. One year after UI inflow the job seekers with fixed-term contracts caught up and the effect sizes of the two groups are similar.

STEP does not seem to have an effect on the outflow for job seekers with at most vocational education and the effects of STEP occur later in the UI spell for job seekers who had a fixed-term contract or a low wage before UI. These characteristics are, however, highly correlated. To see which of the characteristics can explain the observed differences between the samples, we estimate the LATE effect of STEP and gradually include interactions between participation in STEP and several individual characteristics in this regression.[14] The results of these estimations are given in Table 5.6.

We first test whether the effects of STEP differ by demographic characteristics by including interactions of participating in STEP and age and gender. All the interaction terms are insignificant, so we conclude that the effects of STEP do not depend on age or gender. If we include interaction terms by education, we find a significant negative coefficient of having at most vocational education interacted with STEP. The point estimate of this interaction term is also robust against including interaction terms by characteristics of the job before UI, such as wage, having a fixed-term contract, the number of hours or employment at a private employment agency previous to UI. None of these interaction terms are significant, although the size of the coefficient of previous employment at a temporary employment agency suggests that this characteristic might matter for the effectiveness of STEP.

In the last step we only include an interaction term for having at most vocational education and previous employment at a temporary agency. We are not able to reject that both interaction terms are different from zero, so we conclude that the

[14]The interactions of participation in STEP and the individual characteristics are instrumented by interactions of being part of the treatment group and the individual characteristics.

Figure 5.5: Estimated Local Average Treatment Effects of STEP on exit from UI by month since UI inflow, by age, gender and education.

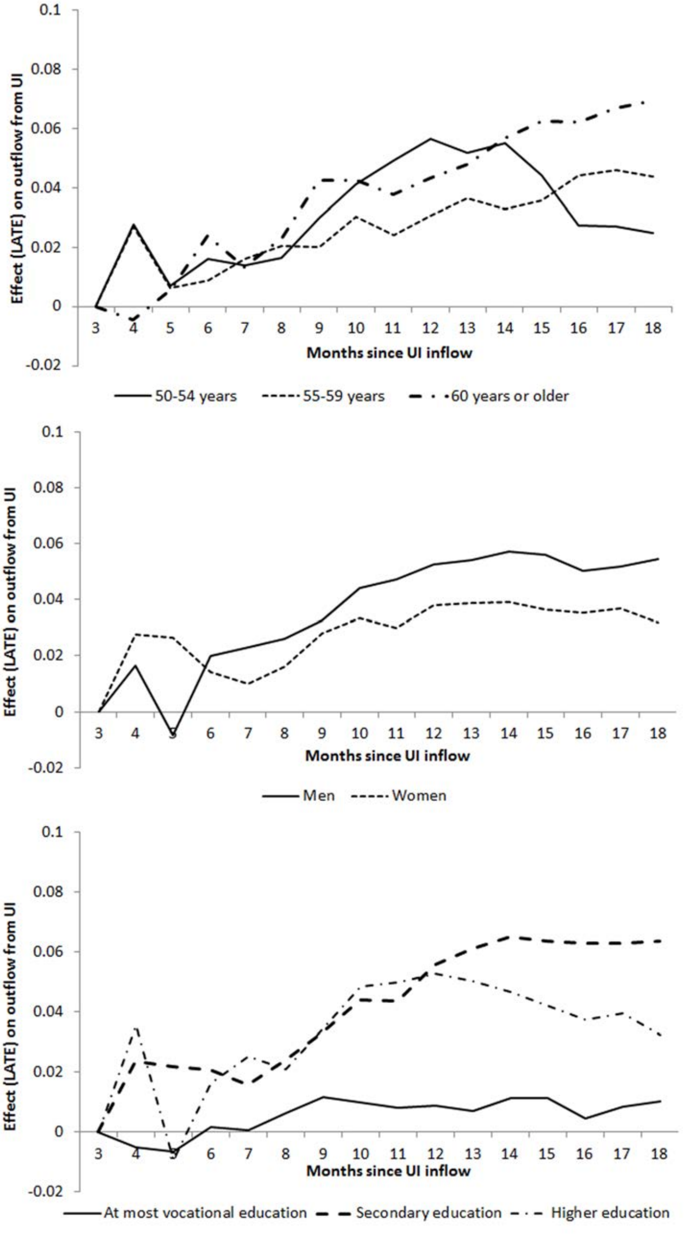


Figure 5.6: Estimated Local Average Treatment Effects of STEP on exit from UI by month since UI inflow, by type of job before UI.

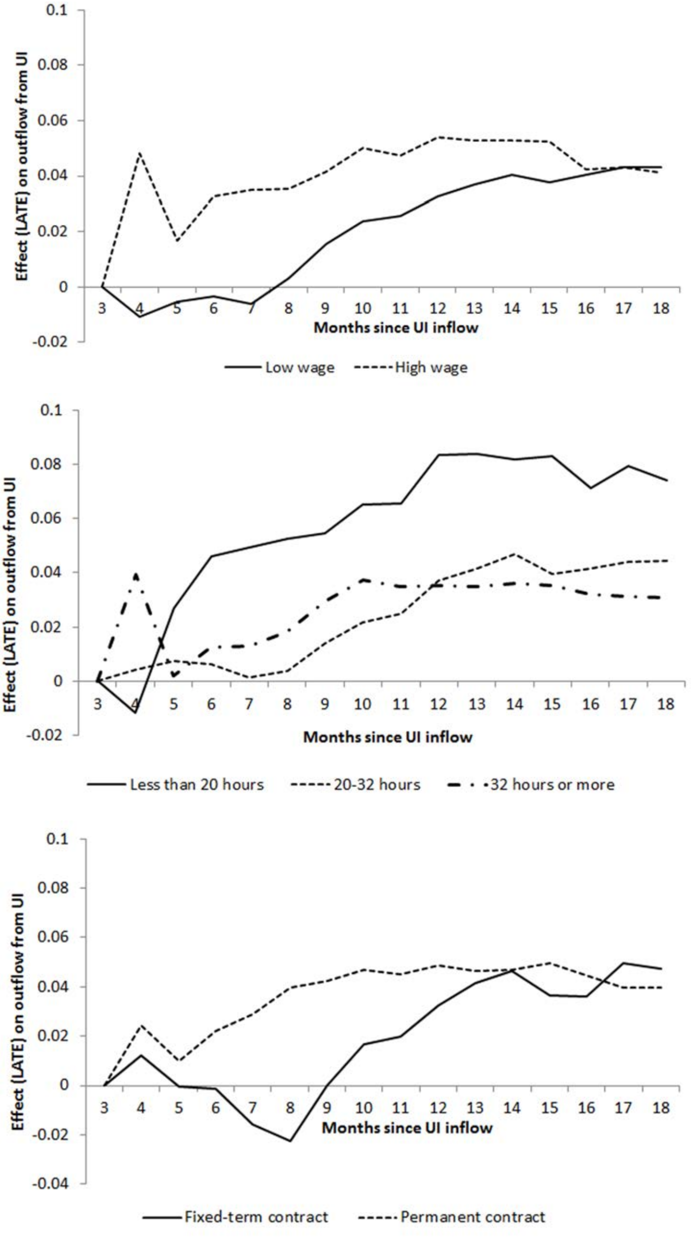


Table 5.6: Local Average Treatment Effects of STEP on exit from UI within twelve months with interactions for different subgroups. The last column shows the take-up of STEP in the treatment group.

	(1)	(2)	(3)	(4)	(5)	(6)	Take-up
Participation in STEP	0.037	0.055*	0.069**	0.065**	0.063**	0.063**	0.52
	(0.027)	(0.033)	(0.021)	(0.015)	(0.034)	(0.013)	
Interaction terms with STEP, personal characteristics							
Younger than 55	0.013	-	-	-	-	-	0.53
	(0.022)						
Between 55 and 60	0.015	-	-	-	-	-	0.57
	(0.030)						
Men	0.013	-	-	-	-	-	0.49
	(0.023)						
At most voc. education	-	-0.054*	-0.056*	-0.052*	-0.052*	-0.052*	0.46
		(0.032)	(0.028)	(0.027)	(0.027)	(0.027)	
Secondary education		-0.001	-	-	-	-	0.56
		(0.026)					
Interaction terms with STEP, job before UI							
Wage below 12500	-	-	0.024	-	-	-	0.47
			(0.038)				
Wage 12500-20000	-	-	-0.014	-	-	-	0.49
			(0.036)				
Wage 20000-40000	-	-	-0.007	-	-	-	0.52
			(0.027)				
Had fixed-term contract	-	-	-	-0.006	-	-	0.42
				(0.026)			
Temp. empl. agency	-	-	-	-0.069	-0.074	-0.073	0.32
				(0.061)	(0.059)	(0.058)	
1-20 hours per week		-	-		0.011	-	0.47
					(0.019)		
20-32 hours per week	-	-	-		-0.007	-	0.56
					(0.013)		
P-value of F-test joint significance	0.71	0.17	0.34	0.17	0.18	0.08	

Note: The estimations include controls for personal characteristics, time fixed effects, regional effects and characteristics of the job before inflow UI. Standard errors in parenthesis. * significant at the 10% level, ** at the 5% level

observed differences in Figures 5.5 and 5.6 are all driven by education and previous employment at a temporary agency. The negative coefficients of the interaction terms of being lower educated and temporary agency employment are roughly the same size as the positive baseline effect of STEP, which implies that STEP does not have an effect on the outflow for job seekers with at most vocational education or job seekers who were employed at a temporary agency before UI.

Both lower educated and job seekers who were previously employed at a temporary employment agency are also less likely to participate in STEP (see Table 5.3 in Section 5.4). This means that job seekers who are less likely to benefit from STEP are also less likely to participate in STEP.

5.6 Conclusion

This chapter studies the effects of STEP, a job search program in the Netherlands targeted at older unemployed workers. We use a large-scaled randomized experiment that involved around 50,000 individuals. Twenty percent of the job seekers were randomized into a control group and did not receive an invitation to STEP, while the treatment group consisting of 80% of the sample was encouraged to participate in STEP. Because individuals in the control group can participate in the training if they asked for it themselves and due to noncompliance, 8% of the control group participated. This is still a seizable difference compared to the treatment group, where 54% of the individuals participated.

We find that STEP increases the outflow from UI in the first year after UI inflow. Twelve months after inflow into UI, job seekers from the treatment group have a two percentage points higher probability to exit from UI compared to job seekers in the control group. The increase in outflow from UI is mostly driven by increased job finding in the treatment group, the availability of STEP increases the outflow to work within a year from 27.7% to 28.9%. Because only about half of the eligible job seekers in the treatment group participated in STEP, the local average treatment effects are more substantial: participation in STEP increases the probability to exit UI within one year with 4.4 percentage points and increases the probability of having a job with 2.2 percentage points. The effects on UI outflow do not change if we consider a longer time window of 18 months, but the effects on having a job become insignificant after 14 months since inflow into UI. This implies that participating in STEP leads to *faster* job finding, but in the longer run does not cause *additional* jobs.

Participating in STEP reduces the cumulative UI benefits within a year after inflow with about €400. Fourteen months since inflow into UI, the reduction in cumulative UI benefits due to participation in the training exceeds the costs of the program. At the same time, the total income of participants is not affected by participating in STEP, as they substitute UI benefits with earnings from employment. We find no evidence suggesting that STEP affects the quality of the jobs after UI, there are no significant effects on wages, number of contract hours, type of contract or employment at temporary agencies. Even though STEP aims to stimulate participants to search for jobs more broadly, participants are not more likely to change sectors.

The size of the randomized experiment allows us to study the effect of STEP for different subgroups. We find that the positive effects of STEP on outflow from UI seem to be confined to individuals with at least secondary education. We also find evidence that STEP is not effective for job seekers who worked for private temporary employment agencies previous to UI. These job seekers are also less likely to participate in STEP, which means that caseworkers were successful in providing STEP to job seekers who would benefit more from the training.

5.A Appendix: Additional tables

Table 5.A1: Estimation results (Linear Probability Model) of asking to participate in STEP in the control group.

	Coefficient	Standard error
Personal characteristics		
Man	-0.016**	(0.003)
Couple	-0.002	(0.003)
Immigrant	-0.010	(0.012)
At most vocational education	-0.006	(0.004)
Secondary education	0.003	(0.003)
Bachelor or master (reference)	-	
Younger than 55 (reference)	-	
55-59 years at UI inflow	0.002	(0.003)
60-63 years at UI inflow	-0.007*	(0.004)
UI characteristics		
Maximum entitlement period	0.000	(0.000)
Number of hours unemployed	0.0001	(0.0002)
Employed at entry UI	-0.008**	(0.004)
Characteristics job before UI		
Re-entry unemployment	-0.008	(0.005)
Received DI benefits	-0.007	(0.009)
Had temporary contract	0.004	(0.003)
Wage below €25,000 (ref.)	-	
Wage above €25,000	0.003	(0.003)
Sector: Health care	-0.003	(0.004)
Sector: Business	0.004	(0.004)
Sector: Temp. empl. agency	-0.002	(0.005)
Sector: Industrial	0.003	(0.005)
Sector: Trade	0.000	(0.005)
Sector: Other (ref.)	-	
Observations	9,938	

Note: Regression includes month of UI inflow fixed effects and local UI office fixed effects. Standard errors between parenthesis. * significant at the 10% level, ** at the 5% level.

Table 5.A2: Descriptive statistics for individuals who found a job in the first 18 months after inflow into UI for treatment and control group and p-value of t-test of different means.

	Treatment	Control	Difference	P-value
Attended meeting (%)	79.1	81.2	-2.1	0.00
Participated in STEP (%)	45.4	6.6	38.8	0.00
Personal characteristics				
Average age	54.7	54.8	-0.1	0.31
Men (%)	61.0	61.6	-0.6	0.42
Single (%)	23.5	24.5	23.7	0.18
Immigrants (%)	1.2	1.4	-0.1	0.53
Education (%)				
At most primary education	12.1	11.8	0.3	0.57
Vocational education	16.5	17.4	-0.9	0.16
Secondary education	48.2	48.1	0.1	0.90
Bachelor or master	23.0	22.4	0.5	0.43
UI characteristics				
UI benefits (€, month)	1,464	1,456	8	0.56
Maximum entitlement (weeks)	150	151	0	0.21
Hours unemployed	33.1	33.2	-0.1	0.44
Employed at start of UI (%)	22.1	22.9	-0.7	0.28
Characteristics job before UI				
Re-entry unemployment	9.9	10.3	-0.5	0.34
Collected DI benefits	1.0	1.1	-0.1	0.52
Wage before UI (€, year)	29,185	28,839	347	0.40
Temporary contract (%)	58.3	58.4	-0.1	0.88
Monthly contract hours	129	129	0	0.87
Sector last job (%)				
Health care	16.1	15.6	0.5	0.39
Business	18.8	18.6	0.2	0.75
Temp. empl. agency	17.8	18.5	-0.7	0.26
Industrial	10.8	11.4	-0.6	0.25
Trade	7.7	7.4	0.3	0.44
Transport	7.6	7.5	0.0	0.97
Other	21.2	21.0	0.2	0.75
Observations	19,191	4,706	23,897	

Table 5.A3: Estimation results of the first stage for exit from UI within twelve months (dependent variable is participation in the training within twelve months)

	Coefficient	Standard error
Treatment group	0.445**	(0.005)
Personal characteristics		
Man	-0.088**	(0.005)
Single	0.016	(0.016)
Immigrant	-0.184**	(0.019)
Vocational education	0.075**	(0.007)
Secondary education	0.103**	(0.006)
Bachelor or master	0.087**	(0.007)
UI characteristics		
Maximum entitlement period	0.001**	(0.000)
Number of hours unemployed	-0.002**	(0.000)
Had a job at entry UI	-0.087**	(0.006)
Level of UI benefits (x1000)	0.061**	(0.004)
Characteristics job before UI		
Re-entry unemployment	-0.134**	(0.008)
Disabled before UI	-0.136**	(0.014)
Average wage before UI (€ per year, x 1000)	-0.001**	(0.000)
Had temporary contract before UI	-0.073**	(0.005)
Average monthly contract hours before UI	0.000	(0.000)
F-statistic	321.05	

Note: The estimations include age dummies, sector fixed effects, regional effects and month of inflow fixed effects. Standard errors between parenthesis. * significant at the 10% level, ** at the 5% level

CHAPTER 6

Summary and conclusions

This thesis consists of four studies which investigate the effects of labor market policies in the Netherlands. This chapter summarizes the main findings and discusses the policy implications.

The first chapter estimates the effects of the entitlement period to UI benefits on job finding and post-unemployment job quality using two identification strategies. It exploits a substantial UI reform in the Dutch UI system which reduced the entitlement period for most - but not all - unemployed workers in a difference-in-difference model and it uses discontinuities in the calculation of the entitlement period in a regression discontinuity approach. The results from both approaches concur and are in agreement with earlier literature. Reducing the UI entitlement period increases the job finding rate, which suggests that providing UI benefits causes moral hazard. Moral hazard is not necessarily bad if unemployed workers can improve the post-unemployment job quality by being more selective on job offers. The results show, however, at most modest effects of the entitlement period on the quality of the first job after unemployment. With shorter entitlement periods job seekers are somewhat more inclined to accept a temporary job, but these effects disappear in the long run.

The Dutch UI reform of October 2006 reduced the average entitlement period with four months. Simulations based on the empirical results indicate that if this reform had been implemented one year earlier, the governmental expenditures on UI benefits would have been reduced by 86 million euros, which is a reduction of 3.1 percent. At the same time, the cumulative earnings from employment would have increased with 182 million due to increased job finding. The total income of individuals who entered UI in the year before the reform would have increased with 96 million euros, which is an increase of 0.6 percent.

Although the empirical results suggest that a shorter UI entitlement period increases both job finding and the average cumulative income without decreasing job quality, this does not necessarily mean that all unemployed workers are better off with a short entitlement period. Shorter entitlement periods can also imply that more unemployed workers exhaust their UI benefits and suffer a drop in income after UI exhaustion. Moreover, the effects of the UI entitlement period are heterogeneous and appear to be most substantial for those with already relatively short entitlement periods. If the UI entitlement period becomes too short, the UI benefits fail to act as a search subsidy and unemployed workers can be forced to accept a less suitable job or suffer an income loss.

Chapter three studies the effect of firm experience rating on DI inflow and DI outflow. It exploits the removal of experience rating for small firms between 2003 and 2007 in a difference-in-difference design. The estimation results indicate that before 2005 - when the sick leave period preceding DI benefits was equal to one year - the removal of experience rating increased the DI inflow and reduced the DI outflow for workers of small firms. The total DI stock in 2004 was about 0.4% larger because of the reform.

However, the effects of DI experience rating seem to depend on the institutional setting. For example, the cap that was used for the experience-rated premiums had substantial disincentive effects. Firms paying the maximum premium had higher DI inflow rates and lower DI exit rates, suggesting that they responded to the absence of prevention and reintegration incentives (at the margin). In addition, after the sick leave period was extended from one to to two years, there is no evidence suggesting that DI experience rating affects DI inflow or outflow. Because firms are already financially responsible for the sick leave period preceding DI benefits and that these costs are generally substantially larger than the experience-rated premium, the additional incentive of DI experience rating no longer seems to contribute to lower DI rates.

Policymakers often argue that with DI experience rating firms may have to bear financial risks that are beyond their scope of control, particularly benefit costs that stem from non-occupational diseases or worker moral hazard. This could cause financial distress amongst firms, but it could also lead to strategic behavior of firms to avoid future costs. Chapter four investigates whether increases in the experience-rated DI premium have an effect on firm exits, layoffs and substitution to other social security schemes. To disentangle the effect of the disability risk from the effect of the experience-rated premium, it exploits exogenous variation in the mapping of disability risks to DI premiums over the years.

In line with the expectations of policymakers, the estimation results indicate that a positive premium adjustment due to experience rating increases the probability of a firm exit. In particular, an upward adjustment of the DI premium of one percentage point increases the probability of a bankruptcy with 0.44 percentage point and the probability of a merger with 0.20 percentage point. While bankruptcies are signals of financial distress, mergers point at strategic behavior of firms, as firms start paying the minimum premium again after they move to another firm. The findings also suggest that a premium adjustment increases the inflow into UI. Most of this effect is driven by layoffs of firms that exit the market. There is no evidence of substitution of workers with health problems to the not-experience-rated DI scheme.

Combined with the results from chapter three - especially the absence of effects of DI experience rating on DI inflow and outflow after 2005 - one may argue that the positive effects of the experience-rated premium on firm exits and inflow into UI are reasons to remove the incentive of DI experience rating. At the same time, however, it should be stressed that the vast majority of firms has gained from experience-rated premiums, as they paid minimum premiums only. Therefore, more research is needed to come to an overall assessment of the welfare implications of DI experience rating.

The last chapter evaluates the effects of STEP, a job search program targeted at older unemployed workers. In a large-scaled randomized experiment which involved around 50,000 individuals, job seekers who were randomly assigned to the treatment group are invited to participate in STEP while job seekers in the control group do not receive an invitation. The results show that participating in STEP increases the outflow from UI of older job seekers. In particular, their probability to exit UI within one year is increased with 4.4 percentage points and most of this outflow is to work. Considering a longer time window of 18 months the estimated effect on UI outflow persists while the effects on employment become insignificant. This implies that STEP leads to faster job finding instead of additional job finding.

The returns of the program - measured as the reduction in cumulative UI benefits - exceed the costs of the program. For every participant, STEP earns roughly €250 in the first eighteen months of UI. At the same time, the total income of participants is not affected by participating in STEP, as they substitute UI benefits with earnings from employment. Despite these positive effects of STEP, policymakers should consider for which job seekers the program becomes available. For example, the program is not effective for individuals with at most vocational education. These job seekers are also less likely to participate in STEP, which means that caseworkers were successful in providing STEP to job seekers who would benefit more from the training.

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Samenvatting (Summary in Dutch)

Beleidsmakers beschikken over verschillende instrumenten om de overheidsuitgaven aan sociale zekerheid te beïnvloeden. Zo kunnen ze bijvoorbeeld uitkeringen meer of minder genereus maken, werkgevers met financiële prikkels stimuleren om uitkeringsontvangers aan te nemen of werkzoekenden verplichten aan sollicitatietrainingen deel te nemen. Deze dissertatie bestaat uit vier hoofdstukken waarin wordt onderzocht hoe effectief verschillende beleidsinstrumenten in Nederland zijn.

In hoofdstuk twee worden de effecten van de maximale WW-duur op de baanvindkans en de kwaliteit van de gevonden banen in kaart gebracht. Hierbij worden twee verschillende identificatiestrategiën gebruikt. Ten eerste wordt een difference-in-difference model geschat door gebruik te maken van een omvangrijke hervorming van de WW waarbij de maximale WW-periode voor de meeste - maar niet alle - werkzoekenden werd verlaagd. De tweede identificatiestrategie maakt gebruik van discontinuiteiten in de berekening van de maximale WW-periode door middel van een regression discontinuity design. De resultaten van beide methodes komen overeen en zijn in overeenstemming met de literatuur. Het verkorten van de maximale WW periode verhoogt de baanvindkans, wat erop wijst dat het verschaffen van WW-uitkeringen *moral hazard* veroorzaakt. Als werkzoekenden langer recht hebben op een WW-uitkering, doen ze er langer over om een baan te vinden. Moral hazard hoeft niet altijd slecht te zijn, bijvoorbeeld als werkzoekenden banen met een betere kwaliteit kunnen vinden door selectiever te zijn bij het solliciteren naar banen. Uit het onderzoek beschreven in hoofdstuk twee blijkt echter dat een verkorting van de WW geen of zeer bescheiden effecten heeft op de kwaliteit van de eerste baan na WW. Met een kortere maximale WW-periode zijn werkzoekenden wel iets eerder geneigd om een baan met een tijdelijk contract te accepteren, maar dit effect verdwijnt op de lange termijn.

De hervorming van de WW in oktober 2006 verkortte de gemiddelde maximale WW-duur met vier maanden. Uit simulaties gebaseerd op de empirische resultaten blijkt dat als deze hervorming een jaar eerder had plaatsgevonden, de overheidsuitgaven aan WW-uitkeringen dan 86 miljoen euro - oftewel 3,1 procent - lager waren geweest. Tegelijkertijd waren de cumulatieve inkomsten uit arbeid dan met 182 miljoen euro gestegen doordat werkzoekenden eerder en vaker een baan hadden gevonden. Het totale inkomen van werkzoekenden die in het jaar vóór de hervorming in de WW terecht zijn gekomen was dan 96 miljoen euro hoger geweest, wat 0,6 procent hoger is dan hun daadwerkelijke inkomen.

Hoewel uit de empirische resultaten blijkt dat een kortere maximale WW-periode zowel de baanvindkansen als het gemiddelde cumulatieve inkomen verhoogt zonder de baankwaliteit te verlagen, betekent dit niet dat alle werklozen baat hebben bij een kortere WW-periode. Een verkorting van de maximale WW kan er ook voor zorgen dat meer werkzoekenden het einde van hun WW-uitkering bereiken en hierna geen of een lager inkomen hebben. De effecten van het verkorten van de WW zijn heterogeen en zijn het grootst voor personen die al een relatief korte WW-periode hadden. Als de maximale WW-periode te kort wordt, betekent dit dat de WW-uitkering onvoldoende kan fungeren als een zoeksubsidie waardoor werklozen een inkomensverlies ervaren of gedwongen worden om minder geschikte banen te accepteren.

In het derde hoofdstuk wordt onderzocht of differentiatie van de werkgeverpremies voor arbeidsongeschiktheid een effect heeft op de in- en uitstroom in de arbeidsongeschiktheid. Daarbij wordt gebruik gemaakt van de afschaffing van premiedifferentiatie voor kleine bedrijven tussen 2003 en 2007. In een difference-in-difference analyse wordt een controlegroep van grote bedrijven - voor wie premiedifferentiatie de gehele periode in stand bleef - vergeleken met een treatmentgroep van kleine bedrijven voor wie premiedifferentiatie voor enkele jaren werd afgeschaft. Uit de schattingen blijkt dat het afschaffen van premiedifferentiatie voor 2005 - toen de periode van loondoorbetaling maar een jaar was - de instroom in de WAO verhoogde en de uitstroom uit de WAO verlaagde. Door het afschaffen van premiedifferentiatie was het totaal aantal WAO-uitkeringen in 2004 ongeveer 0,4% hoger dan zonder de afschaffing.

Tegelijkertijd laten aanvullende analyses zien dat de effecten van premiedifferentiatie in de arbeidsongeschiktheid sterk afhangen van de manier waarop de prikkel is ingericht. Het afkappen van premies boven een bepaald maximum verkleint bijvoorbeeld het effect van premiedifferentiatie. Bedrijven die de maximumpremie betalen hebben geen directe prikkel meer om WAO-instroom te voorkomen. Dit is terug te zien in een hogere WAO-instroom en een lagere WAO-uitstroom voor deze bedrijven. Daarnaast lijkt ook de aanwezigheid van andere financiële prikkels voor werkgevers van belang. Voor de periode na 2005, oftewel nadat loondoorbetaling bij ziekte werd verlengd van een naar twee jaar, zijn er geen aanwijzingen meer dat premiedifferentiatie een effect heeft op de in- of uitstroom. Dit wijst erop dat de extra prikkel van premiedifferentiatie geen effect meer heeft als werkgevers al financieel verantwoordelijk zijn voor een lange ziekteperiode.

Nederland is een van de weinige landen met premiedifferentiatie in de arbeidsongeschiktheidsverzekeringen. Beleidsmakers menen vaak dat bedrijven door premiedifferentiatie in de arbeidsongeschiktheid financiële risico's lopen waar ze onvoldoende invloed op hebben. Ze denken dan bijvoorbeeld aan kosten van arbeidsongeschiktheid door niet-werkgerelateerde aandoeningen of moral hazard van werknemers. Deze financiële risico's kunnen bedrijven in financiële moeilijkheden brengen, maar kunnen er ook voor zorgen dat bedrijven zich strategisch gaan gedragen en toekomstige kosten gaan ontwijken. Hoofdstuk vier onderzoekt of toenames van de gedifferentieerde AO-premie een effect heeft op bedrijfsbeëindigingen, ontslagen en substitutie naar andere werknemersverzekeringen. De hoogte van de gedifferentieerde AO-premie hangt sterk samen het arbeidsongeschiktheidsrisico van het bedrijf. Om het effect van een hoger arbeidsongeschiktheidsrisico te scheiden van het effect van de premie, wordt exogene variatie binnen en tussen jaren in het vertalen van arbeidsongeschiktheidsrisico's naar gedifferentieerde premies gebruikt.

De schattingsresultaten bevestigen de verwachtingen van beleidsmakers: een stijging van de werkgeverspremie door premiedifferentiatie verhoogt de kans op een bedrijfsbeëindiging. Een stijging van de gedifferentieerde premie met een procentpunt van de loonsom, verhoogt de kans op een faillissement met 0,44 procentpunt en de kans op een fusie of overname met 0,20 procentpunt. Terwijl de stijging van het aantal faillissementen wijst op meer financiële moeilijkheden, wijst de stijging van het aantal fusies en overnames juist op strategisch gedrag van bedrijven, aangezien bedrijven weer de minimumpremie gaan betalen nadat ze zijn gefuseerd met een ander bedrijf. Uit de resultaten blijkt ook dat een stijging van de gedifferentieerde premie de instroom in de WW verhoogt. Het grootste deel van dit effect wordt veroorzaakt door werkloosheid na faillissementen. Er zijn geen aanwijzingen dat de kosten van werknemers met gezondheidsproblemen worden afgewenteld op de niet gedifferentieerde arbeidsongeschiktheidsverzekeringen voor volledige en permanente arbeidsongeschiktheid.

Het laatste hoofdstuk bestudeert de effecten van Succesvol naar Werk (STEP), een sollicitatietraining voor oudere werkzoekenden. Deze training is geëvalueerd op basis van een grootschalig gerandomiseerd experiment, waar rond de 50.000

werkzoekenden bij betrokken waren. Werkzoekenden die waren toegewezen aan de treatmentgroep werden uitgenodigd om deel te nemen aan STEP, terwijl de controlegroep geen uitnodiging ontving. Uit de analyses blijkt dat deelname aan STEP de uitstroom uit de WW verhoogt: de kans op uitstroom binnen een jaar is door de deelname aan STEP met 4,4 procent punt verhoogd en het grootste deel van deze extra uitstroom is naar werk. Anderhalf jaar na instroom WW heeft STEP nog steeds een significant effect op de uitstroom, terwijl het effect op de baanvindkansen dan niet meer significant is. Dit suggereert dat STEP ervoor zorgt dat oudere werkzoekenden sneller een baan vinden, maar niet dat ze ook vaker werk vinden.

Uit de analyses blijkt ook dat de training kosteneffectief is. De opbrengsten van de training - gemeten als de reductie in de cumulatieve WW-uitkeringen - zijn hoger dan de kosten van de training. Per deelnemer aan STEP zijn de opbrengsten in de eerste anderhalf jaar na instroom ongeveer 250 euro hoger dan de kosten. Tegelijkertijd heeft deelname aan STEP geen invloed op het totale inkomen van deelnemers, aangezien ze inkomsten uit WW substitueren door loon uit werk. Ondanks deze positieve effecten van STEP, moeten beleidsmakers goed bedenken aan wie ze de training aanbieden. De training is namelijk niet effectief voor alle werkzoekenden, lager opgeleiden hebben bijvoorbeeld geen baat bij deelname aan STEP.

The Tinbergen Institute is the Institute for Economic Research, which was founded in 1987 by the Faculties of Economics and Econometrics of the Erasmus University Rotterdam, University of Amsterdam and VU University Amsterdam. The Institute is named after the late Professor Jan Tinbergen, Dutch Nobel Prize laureate in economics in 1969. The Tinbergen Institute is located in Amsterdam and Rotterdam. The following books recently appeared in the Tinbergen Institute Research Series:

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