IFAU
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Market and Education Policy

Insurances against job loss and disability

Private and public interventions and their effects on job search and labor supply

Josefine Andersson

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

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Abstract
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Essay I: Employment Security Agreements, which are elements of Swedish collective agreements, offer a unique opportunity to study very early job search counselling of displaced workers. These agreements provide individual job search assistance to workers who are dismissed due to redundancy, often as early as during the period of notice. Compared to traditional labor market policies, the assistance provided is earlier and more responsive to the needs of the individual worker. In this study, I investigate the effects of the individual counseling and job search assistance provided through the Employment Security Agreement for Swedish blue-collar workers on job finding and subsequent job quality. The empirical strategy is based on the rules of eligibility in a regression discontinuity framework. I estimate the effect for workers with short tenure, who are dismissed through mass-layoffs. My results do not suggest that the program has an effect on the probability of becoming unemployed, the duration of unemployment, or income. However, the results indicate that the program has a positive effect on the duration of the next job.

Essay II: The well-known positive relationship between the unemployment benefit level and unemployment duration can be separated into two potential sources; a moral hazard effect, and a liquidity effect pertaining to the increased ability to smooth consumption. The latter is a socially optimal response due to credit and insurance market failures. These two effects are difficult to separate empirically, but the social optimality of an unemployment insurance policy can be evaluated by studying the effect of a non-distortionary lump-sum severance grant on unemployment durations. In this study, I evaluate the effects on unemployment duration and subsequent job quality of a lump-sum severance grant provided to displaced workers, by means of a Swedish collective agreement. I use a regression discontinuity design, based on the strict age requirement to be eligible for the grant. I find that the lump-sum grant has a positive effect on the probability of becoming unemployed and the length of the completed unemployment duration, but no effect on subsequent job quality. My analysis also indicates that spousal income is important for the consumption smoothing abilities of displaced workers, and that the grant may have a greater effect in times of more favorable labor market conditions.

Essay III: Evidence from around the world suggest that individuals who are awarded disability benefits in some cases still have residual working capacity, while disability insurance systems typically involve strong disincentives for benefit recipients to work. Some countries have introduced policies to incentivize disability insurance recipients to use their residual working capacities on the labor market. One such policy is the continuous deduction program in Sweden, introduced in 2009. In this study, I investigate whether the financial incentives provided by this program induce disability insurance recipients to increase their labor supply or education level. Retroactively determined eligibility to the program with respect to time of benefit award provides a setting resembling a natural experiment, which could be used to estimate the effects of the program using a regression discontinuity design. However, a simultaneous regime change of disability insurance eligibility causes covariate differences between treated and controls, which I adjust for using a matching strategy. My results suggest that the financial incentives provided by the program have not had any effect on labor supply or educational attainment.

Keywords: Employment Security Agreements, job search assistance, job loss, notification, lump-sum severance grant, liquidity effect, disability insurance, financial incentives, continuous deduction, regression discontinuity design, propensity score matching, nearest neighbor matching

Josefine Andersson, Department of Economics, Box 513, Uppsala University, SE-75120 Uppsala, Sweden.

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Dedicated to my grandmother Dagmar

List of Papers

The following papers are included in this thesis:

I. Andersson, J. (2017), Early counselling of displaced workers. Effects of collectively funded job search assistance. (In manuscript)

II. Andersson, J. (2017), Lump-sum severance grants and the duration of unemployment. (In manuscript)

III. Andersson, J. (2017), Financial incentives to work for disability insurance recipients. Sweden's special rules for continuous deduction. (In manuscript)

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Uppsala, August 2017
Josefine Andersson

Contents

Introduction

This thesis is comprised of three self-contained essays, or thesis chapters, that in one way or another relate to labor supply and job search. The first two essays evaluate two different interventions in the job search process for workers in Sweden who are notified of job termination. One involves intervention early in the job search process through individual job search counselling, aimed at helping each worker find a new job. The other involves a contribution of liquidity, in the form of a lump-sum severance grant, aimed at easing the effects of the transitory income shock that is caused by job loss. In both cases, the concern is with how these interventions affect the job search process. The third and final essay in this thesis evaluates financial incentives within the disability insurance system, aimed at increasing the return to work among disability insurance recipients who may have some residual working capacity that is not being used. The research question here is to what extent these incentives actually increase labor supply amongst the targeted individuals. The choice of letting these research topics form my thesis is inspired by my interest in labor economics and the drivers of labor supply in particular.

The three essays are also related with respect to the econometric methodology used; the starting point for identification of causal effects is in all three essays some policy rule that provides a situation similar to a natural experiment. All three interventions require some circumstance to be met that separates eligibility among individuals in a seemingly random manner, which enables causal evaluations of their effects. The rest of this introduction summarizes the work and findings of this thesis.

Early job search counselling

The labor market is not a fixed institution. Its workings vary across countries and over time. Economic globalization forces structural change of the economy. The globalization process has come in waves but has long been ongoing. Historian Robert B. Marks (2002) argues that globalization began with the European colonialization of the Americas in the fifteenth century. A more accustomed view is that the first wave of globalization came in the nineteenth century with new technologies that helped bridge geographical distances and more liberal trade policies (Johnson 2007). Sweden, together with many western economies, developed from an agrarian to an industrial society during the late nineteenth and the twentieth century, while the present structural changes of the economy involve a decline of the manufacturing industry, and an increase in employment within the service sector (Lundh, 2002). This revolution has also entailed that lifelong employment arrangements are no longer the norm, and that workers may not only have to accept more frequent job changes but also update and change the orientation of their competence profiles more frequently, to keep up with the changing demands in the labor market. Unless the supply side can keep up with the changes in labor demand that accompanies continued globalization, unemployment will result. Institutional arrangements that are equipped to handle swift transitions and facilitate efficient job search processes are essential in the modern labor market, to keep job transitions from resulting in long term unemployment.

There have always been people without a stable source of income, but before the industrial revolution, the concept of unemployment was not invented. The poor, who struggled to support themselves, were generally regarded with skepticism. It was only following the industrialization that unemployment was acknowledged as a social issue. Unionizing workers in Europe invented the first unemployment insurance schemes and local labor exchanges. These evolved during the economic downturn after World War I, and in Sweden, the emergence of a nationally organized Public Employment Service happened during World War II. The concept of active labor market policies was introduced on a larger scale during the 1960s, after Swedish trade union economists Gösta Rehn and Rudolf Meidner introduced the idea that, as uncompetitive firms needed to rationalize, the state should invest in the retraining of laid off workers though labor market programs, designed to redirect the competences in the work force toward competitive firms where their labor was needed. The concept quickly spread throughout the industrialized world. (Weishaupt, 2011) The concept of active measures to fight unemployment has become increasingly important and is now a central element in the unemployment fighting strategies of the OECD and the European Union (Martin, 2014). Today, OECD countries spend 0.02 to 0.4 percent of GDP on public employment services and unemployment administration, and another 0.1 to 1.7 percent of GDP on other active labor market policies (OECD 2015).

In Sweden, institutional arrangements to facilitate forced transitions between jobs have also developed aside the public labor market policies. Bargaining between employer unions and labor unions has resulted in collective agreements concerning job transition benefits for most parts of the labor force in Sweden today. These Employment Security Agreements have emerged as a complement to public labor market policy, starting in the 1970s. White-collar workers considered public labor market policies inadequate for achieving smooth transitions between jobs for their sector in the event of job loss, and assistance that was better adapted to meet these needs were ultimately incorporated into the collective agreement. Similar agreements have since been formed for a large portion of the Swedish labor force, providing assistance and benefits to permanently employed workers who are dismissed due to redundancy. State employees enjoy the most generous Employment Security Agreement, first formed in the 1990s, which now even includes some benefits for temporarily employed workers who are not offered further employment. Privately employed blue-collar workers, whose union was long sceptic about incorporating these types of benefits into the collective agreement, feeling that the appropriate job search support should be publicly funded, negotiated an Employment Security Agreement in 2004. The municipal sector enforced an all-encompassing agreement in 2012, entailing that all four main sectors of the Swedish labor market are now covered.

The content varies between the agreements, but often includes both active and passive measures. Passive measures are e.g. additional unemployment benefits, financial support to retrain or start a company, payed internships, and in some cases moving allowances and wage supplements. The common feature among agreements is the active part, taking the form of individual job search counselling which is to be tailored to each displaced workers' individual needs. These job search programs can typically start even as early as during the workers notice period.

In the first chapter of this thesis, I use data on which workers have received assistance by means of the largest Employment Security Agreement in terms of enrollment; that which covers privately employed blue-collar workers. My study analyzes the effects of this very early and intensive job search counselling on the rate at which jobs are found and the quality of jobs found. The assistance provided by this agreement includes individual counseling and job search assistance from a personal coach, who can e.g. help map the workers competences, compose a CV and write job applications and train for job interviews. Workers may also receive some training. A theoretical concern with intervention early in the job search process is that it may be a wasted investment, if costs are associated with assisting workers who would have found a job as quickly without the assistance (Weber & Hofer, 2004). There are no previous studies attempting to scientifically evaluate the causal effects of the job search assistance provided through Employment Security Agreements in Sweden, although those providing the assistance are optimistic and their self-evaluations show that the assistance is successful. These typically compare the results against those of the Public Employment Service However, such a comparison is not fair, as the assistance start much earlier in the job search process, entailing that the clientele is undoubtedly different from those who later become unemployed and seek assistance from the Public Unemployment Service. My thesis provides the first pieces of evidence on how the assistance through Employment Security Agreements affect the Swedish labor market.

I use information on which workers have been given notice from firms who are affiliated with the Employment Security Agreement that I am evaluating. This information is available for notifications of the size of five workers or more. I compare workers among these, who have and have not received job search assistance through the agreement. To estimate the causal effects of the assistance, I use a regression discontinuity design. This design utilizes the fact that there is a discontinuity in the probability of receiving the treatment being studied, at some value of a variable, caused by a policy rule, enabling the researcher to compare those with values just above and below this value. These should be similar enough that treatment is as if randomly assigned between them. In this case, I compare the labor market outcomes of those who just meet the tenure requirement to receive assistance, to the outcomes of those who are just below the required threshold of twelve months of consecutive employment, and do not receive assistance. My results do not suggest that the counselling program has had any effect on unemployment or subsequent income. However, they do suggest that it has had a positive effect on the duration of the next job. These results indicate that, at least for those workers who have relatively short tenure of around twelve months, the assistance may be ineffective in increasing job finding rates and that these workers find jobs as quickly without the assistance, but the results on job duration may also imply that the counselling focuses more on increasing the quality of employer-employee matches for this group of workers.

Provision of liquidity

The consequences of unemployment insurance schemes have received considerable attention within the field of labor economics. The positive association between the size of the unemployment benefits and the length of the unemployment period has been established in numerous studies (a few examples are Meyer 1990, Lalive 2008 and Card et al. 2015). The economic literature has in large treated this result as proof of moral hazard within unemployment insurance systems. If unemployment benefits are increased, the relative price of leisure decreases, which means that finding work becomes less profitable. A positive response to this creation of a wedge between private and social marginal costs of job search is a suboptimal response known as moral hazard. If moral hazard is the explanation for a positive association between benefit size and unemployment duration, increasing the liquidity of the unemployed through, e.g., lump-sum severance grants, which do not affect the relative price of leisure, should have no effect on unemployment durations.

Economic theory can, however, also outline another explanation to the well-known association between the benefit size and unemployment durations, which has received less attention in the literature. Unemployment benefits provide liquidity, increasing the ability of unemployed workers to smooth consumption during the transitory income shock. If credit and insurance markets are imperfect, this liquidity contribution, which also lowers the value of finding employment, may instead be the explanation for any positive association between unemployment benefit levels and durations. Since this is a response to the mending of credit and insurance market failures, rather than a response to the wedge between private and social marginal costs of job search, it may, contrary to moral hazard, be a socially optimal response. If this "liquidity effect" can underlie a positive response of increasing unemployment benefits from current levels, a lump-sum severance grant may well also have a positive effect on unemployment durations. Such effects from lump-sum severance grants have been found in the U.S., Austria and Norway (Kodrzycki 1998, Card, Chetty & Weber 2007 and Basten, Fagereng & Telle 2014).

In the second chapter of this thesis, I investigate the effects of another component of the Employment Security Agreement for privately employed blue-collar workers in Sweden, namely a lump-sum severance grant which is provided to displaced workers above the age of 40. Again, I use the regression discontinuity framework to identify the effects, this time comparing workers being notified of termination who are just above and below the age requirement to receive the grant. I find that the lump-sum grant has a positive effect on the probability of actually becoming unemployed and the completed unemployment duration, but no effect on subsequent job quality. Results for the effects on non-employment show a similar pattern, but are not significant. Dynamics through an effect on staying in the labor force, while unlikely at ages for which the effect is estimated, thus cannot be ruled out. The effects are driven by workers who do not have a higher family disposable income than their individual disposable income, suggesting that spousal income is important for the consumption smoothing abilities of displaced workers.

Financial incentives to work for disabled workers

The final chapter of this thesis does not have any connection to Employment Security Agreements, although it is also connected to the determinants of labor supply. It concerns financial incentives for work within the public disability insurance system. Like unemployment insurance, the sickness and disability insurances make up a large share of public expenditures. Both unemployment and disability insurance enrollment is contra-cyclical and thus rise in economic downturns (e.g. Mueller, Rothstein & von Wachter

2016). However, contrary to unemployment benefit payments, that in many cases end reasonably quickly as a new job is found, it is rare that disability insurance recipients return to work (see e.g. Jans, 2007 for Swedish evidence). This is despite the fact that disability insurance recipients have been shown to possess residual working capacities in a number of instances (e.g. Bound 1989, Gruber & Kubik 1997, Staubli 2011, Moore 2015). Rising costs for sickness absence is generally acknowledged as a fiscal problem for many countries. Disability insurance systems provide recipients with considerable disincentives for work, and there are few policy attempts to increase the return to work among disability insurance recipients, although a few such initiatives have been made in recent time. The literature on the effect of such incentives is, however, still fairly small.

In the final chapter, I evaluate a policy initiative implemented in Sweden in 2009, which gives certain recipients of permanent disability insurance benefits the possibility to work while receiving benefits. Eligible recipients can keep some or all of their benefits while at the same time earning a working income, according to a scheme which is aimed at increasing labor supply of disability insurance recipients. The program also allows recipients to study without affecting benefits. I evaluate the effects of the financial incentives provided by the scheme on labor supply and educational attainment.

Eligibility to the program is based on the time of benefit award, and the starting point for identification is therefore the regression discontinuity design where I compare recipients of permanent disability benefits awarded benefits just prior to and after the eligibility threshold. However, as this threshold is simultaneous to the enforcement of stricter requirements for being awarded disability benefits, recipients awarded benefits just before and after the threshold is systematically different with respect to working capacities. The empirical strategy is therefore complemented with a matching strategy, to compare only recipients who were not affected by the tightening of benefit eligibility. My results suggest that the financial incentives did not induce these recipients, who have relatively more severe reductions in working capacity than eligible recipients in general, to increase their labor supply or educational attainment. This may imply that financial incentives are ineffective in increasing labor supply amongst these disabled, but may also reflect a lack of residual working capacities within this group or a lack of demand for their labor. In either case, the findings of the last essay indicate that other measures need to be considered to increase the return to work among disability insurance recipients if public expenditures associated with deteriorations in working capacities are to be significantly reduced.

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I: Early counselling of displaced workers. Effects of collectively funded job search assistance

1 Introduction

Issues of job transition have become more prominent following globalization and technological change, and even more so since the recent economic crisis. The move from a labor market dominated by lifelong employment to one were workers (may be forced to) change jobs more frequently is becoming more and more noted. In response to this, an adaption of social security systems toward a focus on "employment security" rather than "job security" – meaning security of being employed rather than staying with the same employer – has been proposed in both the academic and policy debates (Borghouts-van de Pas, 2012). The European Commission has formulated a set of policy components essential in implementing so called "flexicurity" policies aimed toward providing such employment security, among which effective active labor market policies are a cornerstone (European Commission, 2007). Active labor market policies involve a wide range of different strategies for improving the functioning of the labor market and increasing the arrival rate and quality of matches, such as counseling and job search assistance. There is a large literature analyzing the effects of active labor market policies on unemployment and job finding rates. This literature generally shows that job search assistance programs have favorable impacts, although the design of programs, as well as their effectiveness, varies greatly.

One factor that may affect the effectiveness of job search programs is the timing of program start. The OECD advocates the use of early intervention, particularly for displaced workers for whom intervention can occur even during the notice period. Some countries, such as Switzerland and Germany, have imposed job search obligations for displaced workers even before the current job has ended (OECD, 2016). Several OECD countries now also require firms that conduct mass-layoffs to provide a social plan to compensate workers being displaced via monetary compensation or reemployment and retraining measures (OECD, 2013). Such social plans often involve outplacement services, which resemble what is traditionally referred to as job search assistance, but also focus on the psychological challenges of coming to terms with being displaced. These outplacement services, which are aimed at easing the job-to-job transition of displaced workers, are carried out by private agencies while financed by the dismissal firm, and they can even start before the end of the current job (van den Berge, 2016). There is, however, little evidence on the effects of outplacement services or other intervention early in the unemployment spell, and even less evidence on the effects of intervention starting as early as before unemployment actually starts.

In Sweden, collective agreements feature an element that allows for the study of job search assistance provided to displaced workers as early as during the notice period. Job transition services in the form of job search assistance and other benefits, bargained over by employer- and worker unions, are provided through Employment Security Agreements (*Omställningsavtal*) and are collectively funded by employers. The purpose of these agreements is to provide assistance to workers that are dismissed due to redundancy, in addition to regular public labor market policies. Eligible workers can enjoy both active and passive measures through these agreements, such as individual counseling and job search assistance together with various kinds of financial benefits. The job search program can start as early as during his or her notice period.

Job search assistance arranged in this form, as an insurance provided through collective agreements, is to my knowledge unique to the Swedish labor market. The content, however, resembles the assistance provided to the unemployed by Public Employment Services (henceforth PES) in many countries around the world. In the U.S. there are federally funded training programs for dislocated workers through the Workforce Innovation and Opportunity Act[1]. The most important difference is that the assistance provided through the Employment Security Agreements typically starts much sooner after the dismissal than in any of these cases. Outplacement services offer similar assistance, however, there is not much evidence on their effects.

Approximately 60 percent of the Swedish labor force is covered by Employment Security Agreements. Surprisingly, evidence is lacking on how these agreements affect the Swedish labor market and those enjoying the benefits. They could potentially have large effects on the functioning of the labor market, through the assistance provided in itself and through its interaction with public labor market policies. They may affect the body of unemployed, as redundant workers often receive assistance even before they leave their current employer, and the effectiveness of public unemployment assistance may also be affected by the complementing measures provided through the agreements.

This is the first study of the causal effects of Employment Security Agreements. Up until now data on who gets assistance through the agreements have been restricted to the private agencies carrying them out (Employment Security Funds/Councils) and unavailable to researchers. In this study, I use data on which individuals have received assistance by means of the largest Employment Security Agreement in terms of enrollment; that which covers privately employed blue-collar workers in Sweden.

This study is not only interesting by providing the first pieces of knowledge on how the assistance provided through Employment Security Agreements affect the Swedish labor market. The feature of Employment Security Agreements in the Swedish labor market, while an interesting phenomenon in itself, can also provide further answers to how the optimal public labor market policy should be designed. This study analyzes effects of very early and intensive assistance to job seekers as the assistance offered within the agreements typically start as soon as the worker has been given

[1] And previously the Workforce Investment Act.

notice and continues throughout and after his or her period of notice. More than 85 percent of the sample starts the counselling program before their last day of employment with their current employer. The main objection to early intervention is the risk of deadweight losses; that providing assistance to all unemployed early in the unemployment spell might not be cost effective because of the unnecessary costs of assisting workers who would have found a job on their own regardless. There is little empirical evidence to support this objection (Weber & Hofer, 2004a). Thanks to the unique setting of the counselling program studied, this study contributes to the knowledge about the effects of very early intervention. To my knowledge, this is the first study that investigates the sole effects of job search counselling provided this early in the process of job loss.

Another contribution of this paper is the analysis of counselling of job seekers without the element of monitoring. The previous literature on the effects of job search assistance and counseling mostly study the combined effects of counseling and monitoring as case workers at the PES, typically providing the counseling, are at the same time also responsible for monitoring unemployment insurance recipients. The assisting function has rarely been analyzed in itself (Crépon, Dejemeppe & Gurgand, 2005). While there are studies that analyze the impact of changing only the level of monitoring, few assess the impact of increasing the counselling element without changing the level of monitoring. My study therefore contributes to the knowledge on the sole effects of job search counseling, as the counselors who help workers through the Employment Security Agreement have no monitoring function.

I analyze the effects of the Employment Security Agreement for Swedish blue-collar workers displaced through mass-layoffs. I use data from the PES on individuals dismissed through layoffs of five workers or more, and data from the TSL Employment Security Fund (henceforth TSL) on which of these workers have received assistance through the agreement, from 2006 to 2012. I combine this data with Swedish register data, providing a rich set of background variables as well as data on labor market outcomes. To find the causal effects of receiving assistance through the agreement, I use a regression discontinuity approach based on the eligibility criteria for the assistance. I use the fact that workers must have been employed with one or several employers affiliated with the agreement for at least twelve consecutive months to be eligible for the assistance, to estimate causal effects using a fuzzy regression discontinuity design.

The assistance provided by this agreement includes individual counseling and job search assistance from a personal coach, who can help map the workers competences, compose a CV and write job applications, train for job interviews etc. Workers may also receive some training as part of the program. I study the effects of this assistance on job finding rates and the quality of jobs found for individuals treated[2]. Since assistance is provided during the period of notice, I study how the agreement affects the probability of becoming unemployed, as well as the job finding rate and the effects on subsequent income. The indicators of job quality studied are job duration and average monthly income in the first job after the displacement.

My results do not suggest that the counselling program has any effect on the probability of becoming unemployed or the unemployment duration. It also does not seem to have any significant effect on subsequent income within two years following termination, or the average monthly income within the first job found. My results do, however, indicate that the program has a strong, positive effect on the duration of the next job. The results do not suggest that the effect depends on how soon the counselling program starts.

The rest of the paper is organized as follows. Section 2 provides some background on Employment Security Agreements and previous studies. Section 3 outlines the empirical strategy and data. In section 4 my results are presented, and section 5 concludes.

2 Background

2.1 Employment Security Agreements

Employment Security Agreements emerged as a complement to public labor market policy and has a long history in the Swedish labor market. The first agreement was signed during the 1970s, initiated by white-collar workers who considered regular labor market policies inadequate to meet their needs for assistance when transitioning between jobs. The union and employers agreed to incorporate assistance that was better adapted to meet these needs, into the collective agreement. Nowadays, such an agreement is no longer exclusive to white-collar workers. Similar agreements have been incorporated into collective agreements for a large proportion of the labor force. Today, around 60 percent of the Swedish labor force is covered by Employment Security Agreements[3].

Assistance through the agreements is provided to workers who are dismissed due to redundancy, and who meet a set of eligibility criteria that differs between agreements. As a rule, only permanently employed workers are covered, but in recent years, temporary workers have been made eligible for

[2] The empirical strategy does not allow the study of potential crowding-out effects for other job seekers, which are therefore ignored in this study.
[3] There are four large Employment Security Agreements in Sweden, basically divided by sector. The municipal sector agreement is the largest one in terms of workers covered, covering 1.1 million workers. The other two large agreements, aside the on being studied in this paper, cover 850,000 privately employed white-collar workers and 250,000 state employees respectively. There is also a number of smaller Employment Security Agreements that cover a few thousand workers each.

at least some of the benefits within some agreements. The scope of Employment Security Agreements is expanding and remains an important matter in collective bargaining in Sweden.

The agreement that is studied in this paper, reached between the Confederation of Swedish Enterprise (*Svenskt Näringsliv, SN*) and the Swedish Trade Union Confederation (*Landsorganisationen, LO*), covers around 900,000 privately employed blue-collar workers in Sweden, or over 30 percent of all employed workers[4]. Almost 100,000 companies are affiliated with the agreement. The agreement covers all blue-collar workers employed with employers who have signed the collective agreement between these two parties[5], regardless of whether the worker is a union member or not. This is the largest Employment Security Agreement in terms of the number of workers enrolled (Walter, 2015). Out of all blue-collar workers being notified of displacement during the period of study, according to the PES register on notices, 78 percent are notified from firms affiliated with this agreement. Out of these 64 percent are treated through the counselling program, or 50 percent of all notified blue-collar workers. Out of all workers in Sweden who are notified of displacement according to the PES register, 35 percent enter the TSL counselling program.

Employment Security Agreements are administered by specific organizations called Employment Security Funds or –Councils. The benefits stipulated in the agreements are financed through a fee paid by employers, amounting to a small percentage of their total wage costs[6]. The SN-LO Employment Security Agreement has been in place since 2004. The agreement is administered by the TSL Employment Security Fund. The fee paid by SN member companies is 0.3 percent of total wage costs throughout the affiliation period. Workers do not apply for the program themselves. The union and the firm together file the application for workers involved in a layoff. In the case of bankruptcy, the union alone files the application on the workers behalf. The counselling itself is not provided in-house by TSL, but is instead purchased from local suppliers. The employer and union choose which supplier will provide the counselling for all workers involved in the specific layoff, from a list of suppliers preapproved by TSL. It is voluntary for the worker to take part in the program.

The assistance provided is different from traditional labor market policies in the sense that it is earlier, more intensive, more focused on individual counselling and more responsive to the needs of the individual worker. In Sweden, the PES provides more intense measures only to those who have spent a long time in unemployment or to targeted groups of unemployed, e.g.

[4] The total number of employed workers is specified in Kjellberg, 2017.
[5] Local parties can negotiate beforehand to exclude their workers from the agreement.
[6] This percentage differs between agreements, but is typically around 0.3 percent of total wage costs.

young unemployed or individuals who are deemed at risk of becoming long-term unemployed. Through the Employment Security Agreements, all workers who lose their job due to redundancy enjoy early and individually oriented counseling and assistance, as long as they meet the basic eligibility criteria. Each eligible worker is provided with a personal coach who councils the worker in the search for a new job. The aim is to minimize the unemployment duration (or even avoid unemployment all together). Examples of services that the coach provides is to help the worker to map his or her competences, define the range of possible job opportunities, compose a CV, write job applications, and train for job interviews. Once the worker is provided with a coach they compose the job search program together according to the workers' individual needs. There are no guidelines stating how often the coach and worker should meet or how, or in which activities to engage in. This is entirely up the worker and coach to decide. Workers can also receive a shorter training effort, such as training to get a forklift operating license or the like, but this is only possible if it can be shown to yield a concrete employment opportunity, and should not intervene too much with the counselling. In special cases, more intensive training efforts can substitute the counselling.

The agreement allows the assistance program to start directly when the worker has been given notice. The program typically starts before the worker has left the old job. *Figure 1* shows a histogram of the timing of program start, as the number of days before the notified last day of employment. The starting date is defined by the second meeting between the worker and the coach (the first is an information meeting), and this date is reported to TSL. More than 85 percent start the program before the last day of employment. Other than that, meeting frequencies or activities are not reported, which means that we know little about what the job search program contains for different workers. Survey evidence produced by TSL 2013 shows that the median number of meetings between the coach and the worker is three meetings, and 25 percent of respondents meet their coach more than five times.

Without knowing much about the intensity of the program for each individual worker, we know that the intensity of the program during the period of notice varies depending on the character of the current job. If possible, the worker can leave work to take part in meetings with the coach. However, it is up to the employer to allow this. Many jobs typical for the group covered are of such a character that it is difficult for employers to allow workers to step away. It may be more costly for the employer to allow a worker to step away from the assembly line or a truck driver to reschedule his or her route, than to allow an electrician to leave an hour early.

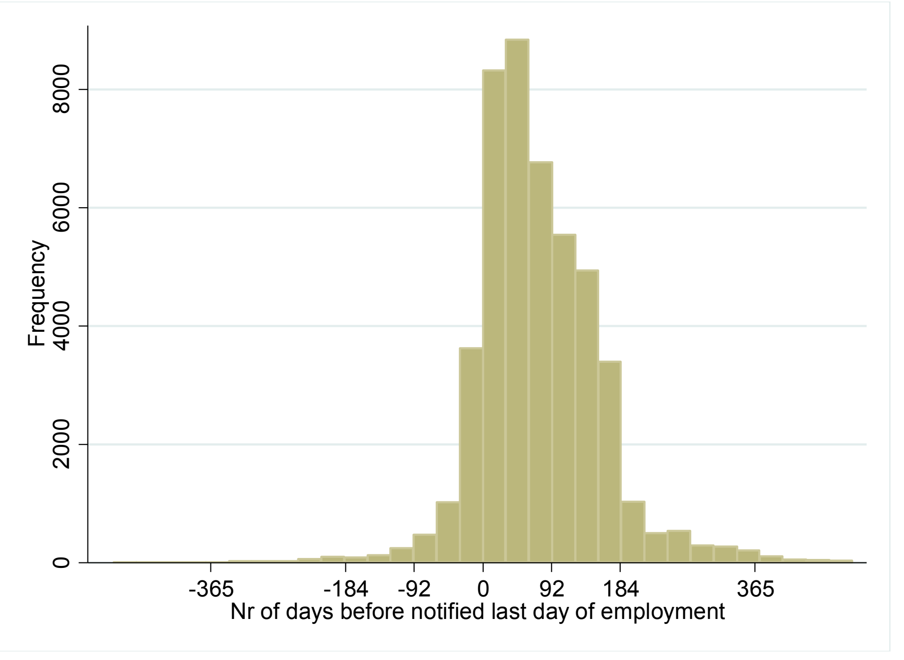


Figure 1. Timing of program start

Note: The histogram shows the frequency of workers starting their job search program within the number of days before their notified last day of employment specified in the x-axis, in one month bins. Absolute values above 497 days have been excluded for symmetry.

The workers can be in the program for at most one year after their notified termination date. However, the supplier gets a fixed amount, currently SEK 22,000 (around USD 2,500) for each worker they counsel, which buys counselling for however long it lasts. The amount can be distributed among workers within the same notification. At the first individual meeting between the worker and the coach, the worker is informed about the program. At the second individual meeting, the worker signs an enrollment note stating that he or she wants to take part in the program. The supplier can bill half the total amount as soon as this note is sent to TSL. The second half can be billed at the earliest three months after the second meeting, or when the worker has found a job or otherwise ended the program. The bill is sent to the employer, who is reimbursed by TSL within a week. Employers facing liquidity difficulties have the right to ask for divided payment in sequential invoices.

2.2 Previous literature

Counselling of job seekers has the purpose of increasing the arrival rate of job offers and improving match quality. Better matches are characterized by more productive and therefore longer lasting jobs. The economic literature on job search assistance programs generally shows no significant or positively significant effects on labor market outcomes. These programs have a greater effect in the short run, while training programs give greater gains in the long run. Subsidized public sector employment is less likely to have favorable effects (Card, Kluve & Weber 2010). Job search assistance programs have stable or declining effects over time and the effects are less countercyclical than those of e.g. training programs. These programs are also on average more productive for young or older participants and for specific "disadvantaged" participants than for regular UI-recipients (Card, Kluve & Weber 2015). Many studies on counselling of job seekers use randomized social experiments to estimate causal effects. The ex-ante, or "threat", effect of job search programs seems to be important, and individual caseworker meetings seem to have more favorable effects than group meetings.

Meyer (1995) finds that five job search experiments in the U.S., aimed at better counselling but often including additional monitoring, had significantly favorable effects on UI receipt and earnings. Gorter & Kalb (1996) study an intensive job search assistance program in the Netherlands using an experiment, and find positive but insignificant effects on the exit rate from unemployment. Van den Berg & van der Klaauw (2001) analyze an experiment involving low intensity job search assistance in the Netherlands and find no effect from treatment on the exit rate from unemployment. The increased monitoring seemed to induce qualified job seekers to switch from informal to formal search channels.

Maibom Pedersen, Rosholm & Svarer (2017) compare the effects of three experiments involving early and intensive active labor market policies in Denmark. The experiments involved intensified counselling. They find that bi-weekly individual meetings during the first 13 weeks of unemployment have a positive effect on the accumulated number of weeks employed from program start. It appears that men benefit more, and the results indicate that the positive effect on the accumulated weeks of employment is not due to a positive effect on job finding but rather men staying employed longer. They also find that weekly group meetings have a positive but insignificant effect on employment, driven by longer subsequent employment duration rather than shorter unemployment duration. They conclude that early and frequent individual meetings with caseworkers is the most cost-effective way of assisting the unemployed. Graversen & van Ours (2008) study another experiment in the Danish labor market. The mandatory treatment combined a short job search course, intensified counselling by caseworkers and a training program if the treated worker reached four months of unemployment. Their findings suggest that the treatment was very effective, decreasing median unemployment duration by 18 percent and increasing the job finding rate by 30 percent. The treatment effect does not vary over gender or age groups.

The intensified counselling and threat of the training program seems to drive the results.

Hägglund (2009) studies the effects of five randomized experiments in Sweden involving more frequent contact between case workers and unemployed through group meetings. All experiments resulted in shorter unemployment duration for the treated, but this effect was only significant in one of the experiments. Hägglund also concludes that a large part of the effect was an ex ante effect. The treatments also had an average positive effect on earnings. The effects of the combined job search assistance and monitoring were positive, while not for monitoring alone.

Through a series of experiments, Klepinger & Johnson (1994) show that job search assistance in the form of a two-day workshop in addition to monitoring reduced the length of the first unemployment spell by 0.7 weeks, and in a later study with a similar setup, Klepinger, Johnson & Joesch (2002) find that increasing the counseling element by adding a mandatory job search workshop for UI recipients reduces UI receipt by half a week. Crépon, Dejemeppe & Gurgand (2005) evaluate a French reform that strengthened the individual counselling services to unemployed workers while not altering the level of monitoring. They argue that the reform improved match quality for the treated as they find, aside significant positive effects on the exit rate from unemployment, even stronger positive effects on subsequent employment duration. Services were provided directly by the French public unemployment agency or they were subcontracted. The authors conclude that the treatment studied, with the increasing use of private suppliers, is the right direction of labor market policies. Weber & Hofer (2004b) analyze a similar reform in Austria which they found significantly reduced unemployment durations. The lock-in effect was minor with small positive effects already at program start and the full effect reached halfway into treatment. Women seemed to benefit more. Weber & Hofer (2004a) study how this program effect varies with the timing of program entry, and find that the effect is similar for entry at any time during the first year of unemployment but disappear thereafter.

Direct evidence on the effects of outplacement services is scarce. Arellano (2007, 2009) study the effects of outplacement by one large outplacement agency in Spain and finds that the outplacement services actually increases time in employment, which could be explained by a "reservation wage effect". Subsequent wages are found to be higher for those receiving outplacement. The results, however, rely on a small sample of treated and a matching on observables approach. Van den Berge (2016) studies the combined effect of a lump-sum severance grant and job search assistance provided through social compensation plans drafted for workers displaced in mass-layoffs in the Netherlands. The job search assistance is provided before the job ends. The combined effect of these measures is a reduced probability of non-employment but an increased overall unemployment duration and a negative effect of subsequent wages. It is not possible to discern whether the severance grant or job search assistance is driving these results, but van den Berge argues that a reasonable interpretation is that job search assistance is driving the first result whereas the opposing effect of the severance grant dominates the job search support effect when unemployment starts. The results rely on the assumption that workers displaced through collective dismissals (treated) are similar to workers displaced through bankruptcies (controls). In my study, a quasi-experimental design is used to study similar job search services, providing a better opportunity to capture the causal effects of this type of treatment.

This evidence suggests that early and intensive job search assistance has favorable effects on unemployed workers' job finding and match quality in terms of employment duration. Weber & Hofer (2004a), however, find that the timing of job search program start does not matter for the effect within the first year. No study (solely) evaluates job search measures taken as early as in the case in this study, however. The evidence from the few studies available on the effects of outplacement services is far from conclusive. With earlier measures the risk of deadweight losses, in terms of means wasted on workers who would have found a job without assistance, are greater. This study focuses on counselling often provided as early as before the displaced worker even leaves his or her current employer. Previous findings also suggest that increased job search assistance without any additional monitoring has favorable effects on both job finding and match quality.

3 Empirical strategy and data

3.1 The regression discontinuity design

I base my empirical strategy on the rules of eligibility for the assistance offered by the Employment Security Agreement for Swedish blue-collar workers. Eligibility to the program requires that a worker has been employed with one or more employers affiliated with TSL for at least twelve consecutive months before his or her last day of employment. I use this eligibility requirement to estimate the causal effect of the program using a fuzzy regression discontinuity design. With this strategy, I compare individuals who are comparable in all other aspects but who just happened to end up on opposite sides of the qualification requirements, so that treatment is as good as randomly assigned among the individuals in my sample. I compare those who on their last day of employment had worked just long enough to be eligible to those who were just below the limit. The regression discontinuity model can, in its simplest general form, be summarized by the following equation:

$$y_i = \alpha + \tau D_i + \beta_1(1\text{-}D_i)(X_i\text{-}x_0) + \beta_2 D_i(X_i\text{-}x_0) + \varepsilon_i \qquad (1)$$

where y_i is the labor market outcome of interest and D_i is a dummy variable for treatment status. X_i is the forcing variable; the variable that determines treatment status, and x_0 is the cutoff value of the forcing variable, where those with values above it receive treatment and those with values below it are untreated. I use consecutive time in employment with employers affiliated with TSL as the forcing variable, and the cutoff is twelve months of employment. The estimator of interest is τ, the effect of the treatment on the labor market outcome of interest. β_1 and β_2 determines the effect of the forcing variable on the outcome for the untreated and the treated respectively, and ε_i is an error term.

Even though the cutoff is quite sharp, it does not alone determine treatment status. There are a number of other basic requirements that must be met to be eligible for treatment. The agreement covers workers with a permanent employment who are dismissed due to redundancy according to the Act of Employment Protection (LAS). Eligibility also requires employment with an average of at least 16 hours of work per week. Only dismissed workers below the age of 65 are covered. The worker also cannot be in dispute with the employer about the termination of his or her employment. The data does not include information about all of these criteria. Therefore, the RD-design used in this study is a fuzzy RD. Eligibility according to the forcing variable will be used as an instrument for treatment status D_i in equation (1).

Using the RD-design, I compare individuals who are as equal as possible except for treatment status. However, the design in itself is based on the fact that individuals have different values of the forcing variable, which drives treatment eligibility. If the forcing variable affects the outcome, the results will be biased. It is to circumvent this issue that the sample is restricted to those with values of the forcing variable that lie within a small range just around the cutoff. Since these workers are similar also with respect to the forcing variable, the hope is that the bias is negligible. How wide this range should be is a trade-off between precision, which increases with the range, and comparability of the individuals, which increases the narrower the range is. There are some data-driven methods to find optimal bandwidth sizes. The optimal bandwidth size according to, for example, Imbens & Kalyanaraman (2012), varies greatly across the outcome variables used in this study and also does not take into account the monthly character of the employment records that the forcing variable is based partly on. I have chosen a three month bandwidth for the baseline model (however, as I will show, the conclusions are not changed using a somewhat smaller or larger bandwidth). This means that observations are reasonably close to the cutoff, while the sample size is not too small. The fuzzy nature of the cutoff in this study also means there is room for some overlap with values of the forcing variables above the cutoff in both the treatment and control groups. The same bandwidth is used for the estimation of the first and second stage results, and instead of the simple RD model in (1), I use a triangular kernel local linear regression model.[7]

By restricting the sample to observations close to the cutoff, the bias is minimized, but, unless we are willing to assume a constant treatment effect over all values of the forcing variable, the results found must be thought of as a local average treatment effect around the cutoff. In this study, assuming a constant treatment effect over the forcing variable is not realistic. A short qualifying time of employment also means that the worker has recently changed jobs, which means that individuals close to the cutoff have more recent job search experience than individuals with a long qualifying time of employment on average have. Recent job changes can also be a signal of a higher employability than the average among all notified workers. It is also possible that individuals close to the cutoff are given a different treatment, or a smaller dosage of the same treatment, than individuals with higher values of the forcing variable. Other, more intense counselling and training measures may be required, and used, to place an individual with more job specific competences gained from working at the same firm for a number of years, than required for those recently employed. It is therefore likely that the local average treatment effect estimated in this study is smaller than the overall treatment effect of the treated.

3.1.1 Measurement error issues

I calculate the forcing variable, qualifying employment time, using employment records collected by the Swedish Tax Agency and provided by Statistics Sweden. Employment records contain monthly data on employment periods.[8] I know the exact date of each worker's notified termination date, the date which is relevant for the determination of eligibility, but since I use the employment records to find the start of the employment, I do not know the exact start date. Assuming that employment always starts the first day of the first month, this induces a one-sided measurement error in the forcing variable. My measure of the forcing variable, x, is an overestimate of the true value, x^*, by at most one month:

$$x_i - 31 < x^*_i \leq x_i \tag{2}$$

[7] The baseline is a triangular kernel local linear model. With covariates included in the fuzzy RD model, a predicted value of treatment lies outside the feasible range, and local mean smoothing is used to estimate the treatment discontinuity. Without covariates in the model, however, the conclusions are unchanged.

[8] Employers must report the period when the employee is employed at the employer and the earnings that have been paid out. The employment period can only be reported with the start and end month, so the time actually worked will always be over-reported unless the worker starts his or her employment the first day of the first reported month and leaves the last day of the last reported month.

Within a range of the forcing variable just at the cutoff, I do not know which observations truly lay above or below the cutoff. For measured values of the forcing variable below 365, I know for sure that they are not above the cutoff, since the maximum value of x^*_i is 364 if x_i=364. For measured values above 395, I know for sure that they are not below the cutoff, since the minimum value of x^*_i is 365 if x_i=396. But for measured values of the forcing variable between these values, I cannot be sure whether the true value x^*_i is above or below the cutoff.

This is a problem when using a regression discontinuity since, while treatment jumps at the true value, x^*, at the cutoff, treatment will not jump at the measured value, x, at the cutoff, unless the assumption that the starting date is always the first day of the month is true. If the within month starting date is uniformly distributed, there will instead be a gradual increase in the share of treated over the one month window of 365 and 395 days of qualifying days of employment according to x. Just at the cutoff of 365 days, there will be a kink rather than a jump in treatment status. Dong (2015) discusses measurement errors in regression discontinuity designs, and proposes a so called ”donut-RD” to deal with similar measurement errors. I discard observations between 365 and 395 days of qualifying employment in my estimations. This strategy assumes that the true value x^*_i of the forcing variable, as well as outcomes, develops smoothly within the discarded range so that adjacent points can be used to extrapolate values within the discarded range (Eggers et al. 2015).

There is an additional measurement problem in the employment records, which will affect the measurement of the forcing variable and outcomes in my study. It seems that there is overrepresentation of employment periods starting in January and ending in December. Employers have the opportunity to check a “full year”-box as they report employment periods to the Tax Agency, which is likely to be (at least to a large part) the reason for misreported employment periods. As the cutoff of the forcing variable is twelve months, this measurement error may be systematically different across the cutoff. I use month of termination fixed effects in all estimations to pick up the effects of this possible measurement error. It turns out that the first stage is only marginally affected by the inclusion of these fixed effects. Only results for outcome variables that are based on employment records are affected, which is in line with the expectations given the source of the measurement error.

For the estimation of all reported results, I use, besides the month of termination fixed effects, fixed effects for year of termination and municipality of residence at notice. Using fixed effects changes the units of comparison in the estimation. These fixed effects are included to come as close as possible to a natural experiment, where I compare individuals who are displaced in similar labor market conditions, i.e. in the same region at the same point in time. Standard errors are clustered on distinct values of the forcing variable, as suggested by Card & Lee (2008). I also include covariates for age, gender, years of education, marital status, number of children, fixed effects for region of birth and parents region of birth, years with income, mean wage earnings the last five years prior to notice, time in unemployment, local unemployment rate (at county level), firm size, size of notice, the share of employees given notice from the firm, receiving a lump-sum severance grant (which is another benefit stipulated to some displaced workers, based on age, within the same Employment Security Agreement[9]), and the order of termination. The purpose of including covariates within the regression discontinuity approach is to increase precision. If the approach is valid, results should not change by the inclusion of these covariates. However, if homoscedasticity does not hold or if the true functional form of the covariates is not used, the result could change without necessarily invalidating the design itself (Lee & Lemieux, 2010). The inclusion of covariates does not change any of the conclusions in this paper. As the fixed effects changes the units of comparison, it induces some changes of the point estimates, but the conclusions remain the same.

3.2 Data

I use individual-level data from the TSL Employment Security Fund over assistance provided through the SN-LO Employment Security Agreement over a period of seven years. The data covers workers who were notified during the period 2006 to 2012. The sample is based on data provided by the Swedish Public Employment Service on mass-layoffs. By law, Swedish employers must report notices to the PES if it involves at least five employees within a county at the same time or at least 20 employees over a 90-day period (1§ lagen (1974:13) om vissa anställningsfrämjande åtgärder). The data collected include data on which workers are given notice and from which firm, and the individual level data consists of workers given notice after union negotiations have taken place and a list of displaced workers have been composed in this process. The list is based on the principle of last-in, first-out, however exceptions can be agreed upon during the negotiations. These data are combined with information provided by TSL about all firms that have been affiliated with the Employment Security Agreement and when. The data from the PES include information about whether each notified worker is a blue- or white-collar worker, and together with the data from TSL, blue-collar workers given notice from affiliated firms are identified, as well as their treatment status.

These data are matched to Swedish register data that provide a rich set of background variables as well as information on labor market outcomes. The register data stretches back to 1985 in many cases, and data on outcomes are

[9] This benefit is evaluated separately in the second chapter of this thesis.

available up until 2014. The register data are also used, together with the list of affiliated employers, to identify the total qualifying time of employment, for the implementation of the RD-design. The notification date is not included in the data from the PES, and is therefore estimated for the control group. I use the most common notification date according to the TSL register among those within the same notification. As a robustness check, I have also used the date when the PES received the list of notified individuals, which must be done at least one month before the first person leaves the employer and must include copy of the written notification letter handed to the employee. The conclusions remain unchanged.

I investigate the effect of the TSL counselling services on the probability of becoming unemployed and unemployment duration. For the main results, I define unemployment as receiving UI benefits between the notification date and three months after the notified termination date.[10] The unemployment duration is defined as the number of days between the first week with UI benefits payment and the last, allowing for gaps of a maximum of four weeks between payment periods. If no UI benefit is received, unemployment duration is zero. As treatment in this case can affect the probability of becoming unemployed, this outcome may be considered endogenous. I also investigate the effects on the quality of jobs found, measured as job duration and average monthly income. These outcomes are measured using the employment records described above, which include earnings for each employment period reported. I also look at total earnings during the first and second year after notice. Duration of the first job found is measured as the number of months consecutively employed with the first employer after the notification date[11]. If the consecutive employment period is right censored, this outcome value is missing. Since the employer can only report one starting and ending month per year in the register data on employment periods, a gap between periods will not be reported if they occur during the same calendar year. This poses a problem when trying to identify time until reemployment with the same firm. Rehires without gaps in employment periods according to employment records are counted as occurring within the period of notice in the main result estimations[12].

[10] I allow for a maximum of three months gap following Jans (2002), who use similar data to investigate flows to unemployment following notifications. The argument is that workers may get some compensation from the employer that may postpone the first day of UI eligibility, or the employment may be extended for a limited period. Unlike Jans, I have access to notification dates and therefore allow unemployment to start from that date on. Using a three month gap before the notified termination date instead, as in Jans (2002), produces similar results.

[11] The first job is defined as an employment where the recorded income is at least SEK 10,000 (around USD 1,100).

[12] As previously mentioned, register data on employment records contain monthly data on employment periods. This means that there is measurement error in employment periods if a worker has multiple employment periods with the same employer during the same calendar year. When no gap is observed in employment periods, and the worker continues working at the dismissal firm the following calendar year after the notified last day of employment, I

The TSL data include information about all workers receiving treatment through the agreement. This means that the data includes workers given notice within smaller notices than those reported to the PES. As these treated differ systematically in terms of the size of the notice from the notified individuals who are found in the data from the PES, I have restricted my sample to the sample of notified workers reported to the PES[13] so that the treatment and control groups are comparable in this respect. This means that I ignore 61 percent of the available sample of treated[14]. This also affects the interpretation of the results. I estimate the effect from treatment on individuals displaced through layoffs of five people or more, rather than the average treatment effect of all treated. It is more likely that larger companies, who are more likely to be the source of these mass-layoffs, are better equipped to provide those given notice with additional assistance from the company side which may affect the effectiveness of treatment negatively, assuming decreasing marginal utility of assisting measures. In very large layoffs it is also possible that other stakeholders, such as the government, steps in. Therefore, it is possible that the estimated results for treated from mass-layoffs underestimate the true treatment effect of *all* treated.

3.3 Descriptive statistics

Descriptive statistics for the full sample, the observations within the three month bandwidth around the cutoff, and an extended sample including all treated within the TSL registers, are presented in Table 1. Differences are larger comparing both the full and the extended sample to the sample close to the cutoff. The qualifying time of employment is of course shorter, and this is accompanied by differences in some other characteristics as well. The sample close to the cutoff are on average seven years younger than the full sample and thus have shorter prior labor market experience (5.5 years on

interpret this as a rehire. The timing of the rehire decision is however unknown, which is a problem for the estimation of job finding rates and job duration. It might be during the period of notice, or thereafter but within the same calendar year. Using data from the PES on unemployment periods from enrollment periods and unemployment insurance payment periods, I have calculated alternative rehire dates based on ending dates from these records. An enrollment period ends when the worker is not registered as unemployed without employment according to unemployment categories, and when UI payment periods end for a period longer than four week. If the worker is not enrolled or receives UI payments between the notice and the next job according to employment records, or between the notice and the next calendar year after the last day of employment for rehires, they are assumed to not have become unemployed and reemployment happened during the period of notice. It turns out that the vast majority of rehires happens within the period of notice according to these calculations.

[13] I have only included individuals who appear once in the matched sample of notified workers from the PES and TSL, or more than once but from the same data source, to ensure individuals are not double counted once as treated and once as controls, due to misreporting of dismissal firm or –date, so that they are not matched but is in fact the same dismissal.

[14] The number of TSL application project numbers is reduced from 26,838 to 4,514.

average), have earned almost half as much income the past five years and are less often married. They are also involved in smaller layoffs on average (among the layoffs of at least five people) than the full sample, and are displaced from smaller firms accordingly. This is not surprising given the priority principle provided by Swedish law for dismissals due to redundancy, where the default is that the last hired is first displaced. Qualifying time of employment is highly correlated with tenure with the company. Individuals with a longer qualifying time of employment are thus less often notified when the layoff is small. Individuals close to the cutoff are also somewhat less often women, have somewhat longer education, have spent more time in unemployment and are somewhat less often born in Sweden than the full sample.

Table 1. *Descriptive statistics*

	Close to cutoff	Full sample	Extended sample
Days of qualifying employment	372.55	2,394.66	2,443.11
	(67.21)	(1,926.33)	(1,944.87)
Age	32.07	39.10	39.97
	(11.59)	(12.88)	(12.76)
No. of years with income	9.00	14.45	15.14
	(7.68)	(7.96)	(7.95)
Gender (1=Woman)	0.24	0.26	0.27
	(0.43)	(0.44)	(0.44)
Years of education	11.24	11.08	11.03
	(1.60)	(1.59)	(1.57)
Married	0.23	0.32	0.33
	(0.42)	(0.47)	(0.47)
Mean annual earnings five years before notice	1,130.24	2,216.16	2,222.01
(SEK 100)	(951.80)	(973.28)	(943.07)
No. of children in household below 18	0.62	0.63	0.63
	(0.95)	(0.98)	(0.97)
Days of unemployment	903.36	819.26	874.84
	(1,162.48)	(1,035.52)	(1,083.46)
Local unemployment rate (county level)	7.55	7.70	7.76
	(1.50)	(1.47)	(1.46)
Born in Sweden	0.77	0.80	0.81
	(0.42)	(0.40)	(0.39)
Size of notice	39.91	115.45	–
	(105.90)	(308.63)	
Firm size	1,116.46	1,483.43	1,250.47
	(2,172.23)	(2,972.27)	(2,714.03)
No. of observations	2,750	68,661	143,980

The characteristics of the full sample are similar to the characteristics of the extended sample. The only pronounced difference between these samples is the difference in firm size. Since we know that the size of the notice, which is correlated with firm size, is smaller in the extended sample (since the extended sample includes all treated while the baseline sample only includes workers displaced in mass-layoffs), this is not surprising. This is also the reason for excluding those treated that are not found in the PES notification data, to ensure that the treatment and control groups used are not systematically different. The information on the size of the notice comes from the PES notification data and is therefore not available for the extended sample, but we know that this is the main variable where these samples differ.

3.4 Validity of the empirical strategy

To be able to use the fuzzy RD design there must be a strong first stage relationship. The discontinuity plot in *Figure 2* shows the share of treated by days of qualifying employment. The plot shows that there is a jump in treatment at the cutoff when I exclude the observations with values of the forcing variable just above the cutoff (my donut). The underlying scatterplot reveals that the probability of treatment does not have an equally clear jump at the cutoff without the donut, but instead, as expected, increases gradually within the "donut-range". This suggests that the number of workers who truly cross the threshold of twelve months of qualifying employment increases as my overestimated measure of the forcing variable increases within the discarded range, in the expected manner.

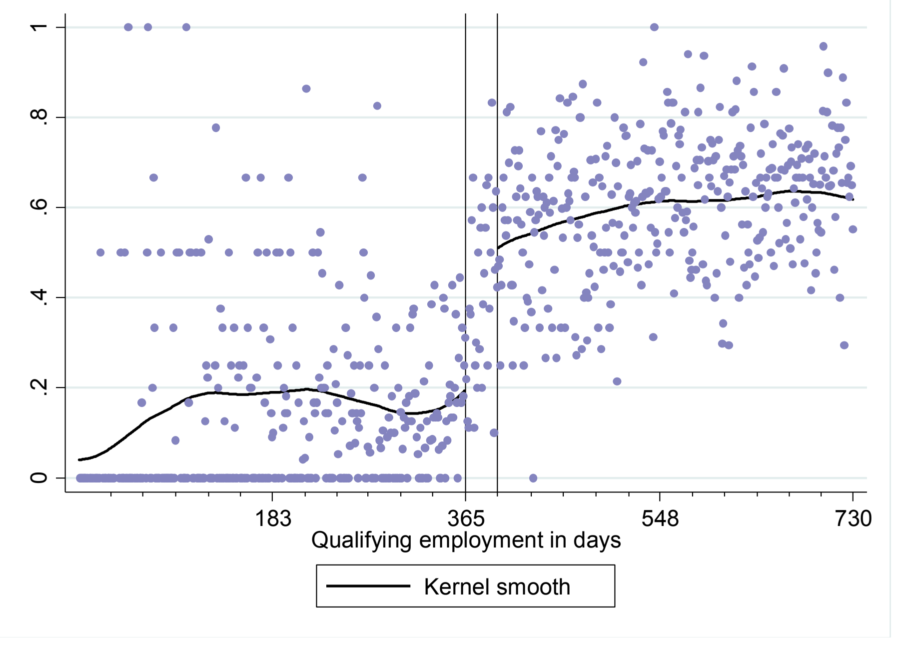


Figure 2. Share of treated by days of qualifying employment

The first stage results in Table 2 confirm that having qualifying employment time above the cutoff of twelve months increases the probability of being treated, by 35 percent.

Table 2. *First stage relationship*

	(1)
Probability of treatment	0.352***
	(0.050)

Note: Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively. The number of observations is 2,449 within the bandwidth.

Figure 2 also reveals that there is a significant share of treated also below the cutoff. One possible explanation for this is that there is a lack of stringency in the implementation of the eligibility rules. However, according to TSL, they are quite strict in enforcing the rules for eligibility. Another possible explanation is that the list of firms affiliated with the agreement contains errors which yields further measurement error in the forcing variable.[15] Since I use the fuzzy RD approach, as long as this source of error is smooth at the cutoff, it does not bias the results.

For the RD estimation strategy to be valid, an assumption that must be fulfilled is that treatment assignment is independent of potential outcomes, i.e.:

$$(Y_{1i,} Y_{0i}) \perp T_i | \mathbf{X}_i \qquad (3)$$

where Y_1 denotes the potential outcome when treated and Y_0 the potential outcome when not, T_i denotes the treatment status and $\mathbf{X}_i$ a set of predetermined characteristics (in the regression discontinuity case the forcing variable should be sufficient). In other words, we need that individuals are not able to exactly control the value of the forcing variable around the cutoff, so that they in effect choose their own treatment status according to potential outcomes. Workers are dismissed by the firm due to redundancy, and it is not likely that they can plan their notified last day of employment to receive or not receive treatment. However, we might worry that firms manipulate the notification date or the length of the notice period to ensure that workers are treated. Since the assistance is paid collectively through the continuous fee and is not a direct cost to the dismissing firm, incentives to withhold assistance from workers is small. The payment procedure does however require

[15] An argument to support this explanation is that around 10 percent of the baseline sample is not employed at the dismissal firm at the time of notification according to employment records from Statistics Sweden. Some individuals also appear in the data from TSL and the PES with the same notified termination date at the firm but at different firms, suggesting that the unique firm identifier is in some cases entered with error in either TSL or PES registers.

firms to pay for the assistance before they are reimbursed by TSL. This may provide incentives to withhold assistance, but on the other hand the union is also involved in the application process and is likely to counteract such incentives from affecting treatment status. There is no way to know for sure if this is the case or not. However, it can be tested by investigating how the density of notified workers in the sample evolves at the cutoff. *Figure 3* is a histogram of the distribution of workers above and below the cutoff in the forcing variable, normalized so that the cutoff value is at zero. To account for the structure of the data, a bin size of one month is used. The number of notified workers does not exhibit a significant jump at the cutoff. This is confirmed by the result of the McCrary density test, which delivers an insignificant estimate of the discontinuity at the cutoff[16]. There is thus no evidence of manipulation of the forcing variable.

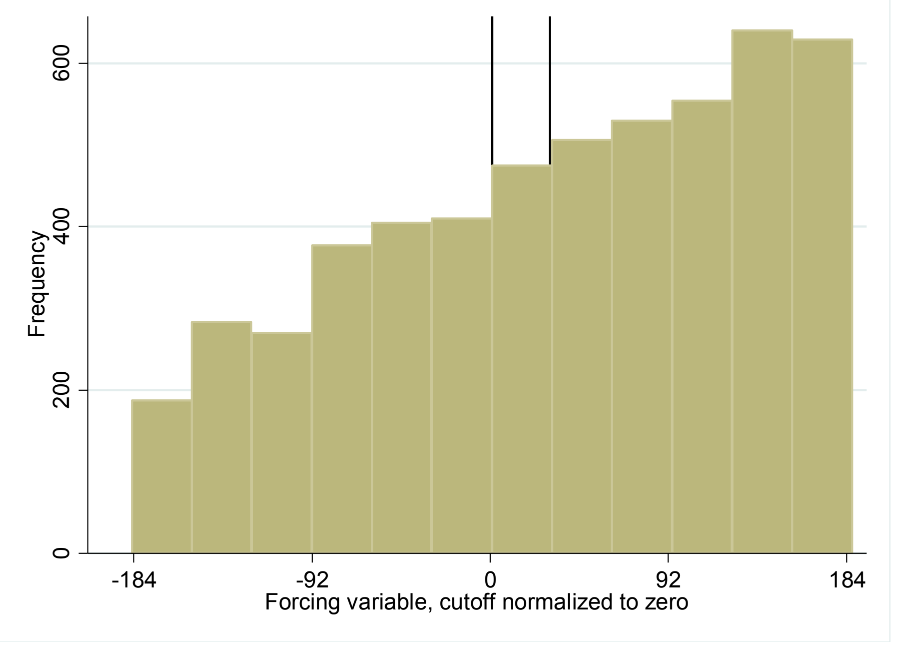


Figure 3. Distribution of displaced workers along the forcing variable

The regression discontinuity approach also relies on the assumption that observations on either side of the cutoff value of the forcing variable are similar so that the treatment assignment can be considered as if random just

[16] The McCrary density test is a test commonly used in with the RD approach to test whether there is a discontinuity in the density of the forcing variable at the cutoff. The bin size used to perform the test is one month, again to account for the structure of the data, which places the start of each employment period in the beginning of the reported starting month, and the bandwidth size used is three months. A detailed description of the test is provided by McCrary (2008).

around the cutoff. This means that we assume that the expected value of potential outcomes given the value of the forcing variable, are developing smooth at the cutoff, i.e.:

$$E(Y_1|X_i) \text{ and } E(Y_0|X_i) \text{ are continuous at } X_i = x_0 \qquad (4)$$

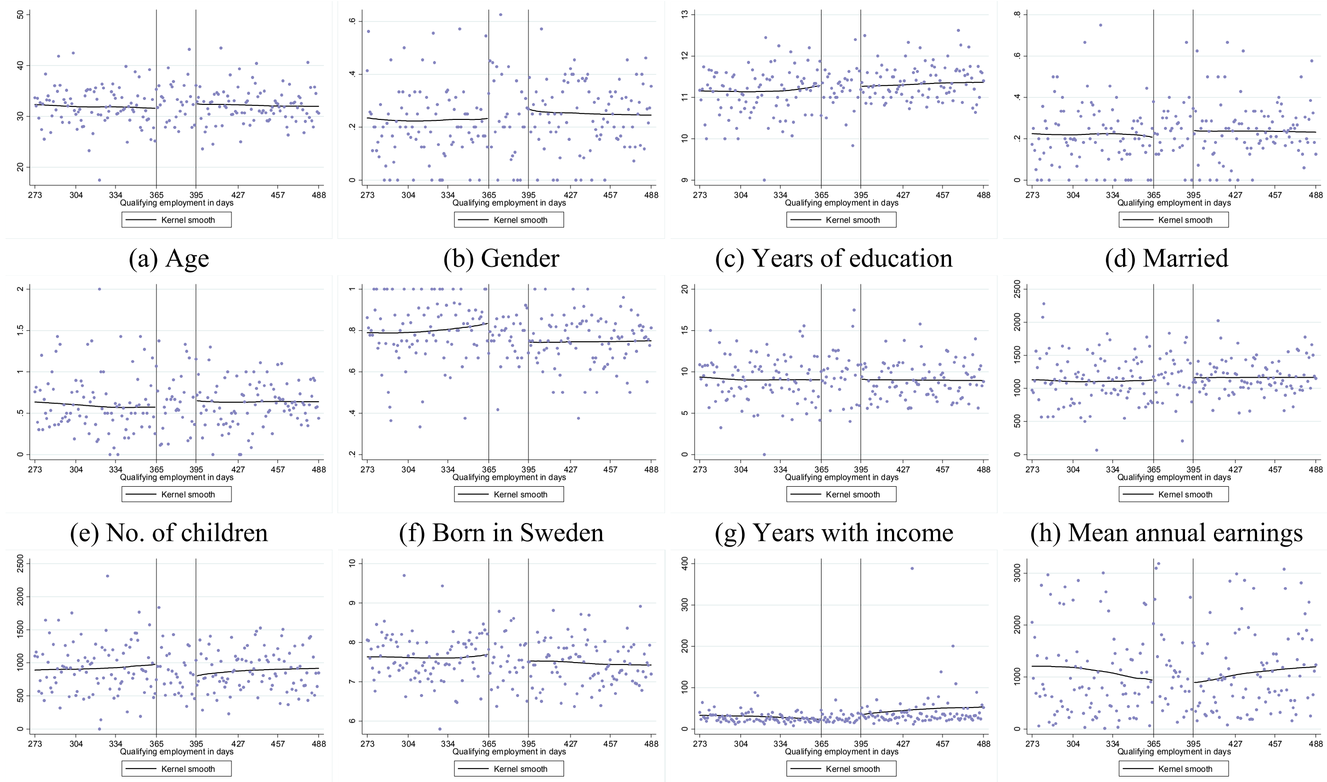
Figure 4 shows plots the potential discontinuities of some basic characteristics at the cutoff. Table A.1 shows regression discontinuity estimates of the same characteristics. Mean values of most characteristics develops smoothly over the cutoff, which supports the validity of the regression discontinuity approach used. If there are no jumps in observable characteristics at the cutoff, it is more probable that there are also no jumps at the cutoff for possible unobserved confounders.

There is, however, one characteristic that appear less continuous across the cutoff; being born in Sweden. It is significantly discontinuous at the cutoff according to the estimates of a reduced form estimation in Table A.1, which is a test of the continuity of basic characteristics at the cutoff. This is difficult to explain, however, when testing multiple variables, it is possible that some estimates are significant even by chance. According to the plot, the jump is not that pronounced. In the estimations of the results, fixed effects for region of birth are used, and the inclusion of these does not affect the results.

Other measures, for example training, that the dismissal firm might provide displaced workers with are not observable in the data available. If firms provide such measures to workers not eligible for the assistance provided by the Employment Security Agreement to compensate them, for example because the firm feels that the eligibility criteria are unfair, it would bias the results in this study. Since this is not observable, I cannot test for whether the probability of receiving such treatment is discontinuous at the cutoff. It is however unlikely that firms would discriminate measures provided to notified workers. According to TSL, measures of this type are sometimes provided by firms, but if so on the principle of equal treatment. If so, the probability of receiving such measures is continuous at the cutoff. Such measures are more likely to occur when a layoff is large. I test whether this affects my results by estimating effects separately for layoffs of different sizes in section 4.2.

4 Results

The main results of the effect of the TSL counselling program are found in Table 3. The reduced form (RF) estimates in column 1 show regression discontinuity estimates from a sharp RD model around the cutoff. This would



(a) Age (b) Gender (c) Years of education (d) Married

(e) No. of children (f) Born in Sweden (g) Years with income (h) Mean annual earnings

(i) Days of unemployment (j) Local unemployment rate (k) Size of notification (l) Firm size

Figure 4. Basic characteristics by days of qualifying employment

be considered an intention to treat effect if the forcing variable was the only eligibility criteria. Since the forcing variable does not alone determine treatment, the fuzzy RD results (FRD) in column 2 uses the forcing variable as an instrument for treatment to estimate the causal effects from treatment. The point estimates for the probability of unemployment is positive, but insignificant, in both columns. The same is true for the unemployment duration.

Table 3. *Main results*

Outcome	(1) RF	(2) FRD
Probability of unemployment	0.030	0.072
	(0.067)	(0.151)
Unemployment duration, days	9.130	22.080
	(16.633)	(37.518)
Duration of first job, months	3.057	7.043
	(2.173)	(4.684)
at least 6 months	-0.024	-0.057
	(0.055)	(0.122)
at least 12 months	0.147**	0.352**
	(0.075)	(0.174)
at least 18 months	0.177***	0.425***
	(0.064)	(0.151)
at least 24 months	0.117**	0.341**
	(0.057)	(0.172)
Average monthly income at first new job	668.474	1596.613
	(2093.782)	(4681.120)
Total income first year after notification	3700.312	8948.877
	(13646.811)	(30982.060)
Total income second year after notification	19506.061	47173.685
	(16407.603)	(37578.676)

Note: Each cell represents the result from a separate regression, with each row showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively. The number of observations is 2,449 within the bandwidth.

The effects on the quality of jobs found can only be estimated for those who find a new job during the period I study and is therefore an endogenous outcome. 93 percent of the workers in the sample, however, do find new employment within the follow up period. The effect on the completed duration of jobs found is positive, but not significant. The average completed job duration increases with around seven months according to the point estimate. For the estimation of the effect on the duration of the new job, we also need that the employment has ended during the period of study. The sample size is therefore naturally lower for the average duration outcome. Within the bandwidth, 73 percent in total or 77 percent of those who find a new job have ended it during the follow up period. Since the completed job duration is right censored, I have estimated the effect by month, which means that the censoring problem is smaller since the data can be informative even if the job duration is not completed at the end of the follow up period. In the main results the effect on the next job lasting at least 6, 12, 18 and 24 months are presented (in *Figure 6* this effect is estimated for all months from 2 to 24). The results show that there is a significantly positive effect on the duration of the next job, in terms of the job lasting at least 12, 18 and 24 months, but not for lasting at least 6 months. The highest point estimate, for the effect of the job lasting 18 months, suggests that there is a 42.5 percentage points higher probability that the job lasts at least 18 months if you are treated. There is no statistically significant effect on average monthly income at the first job found after notice, or for the total income the first or second year after notice, although the point estimates are positive.[17] Estimations of all results in Table 3 include covariates, as stated in section 3.1.1. If these covariates are not included, the conclusions remain unchanged.[18]

Since the follow up period ends 2014, both the unemployment duration and the subsequent job duration are right censored. I have therefore estimated the effect on the job finding rate, as the opposite of (still) being unemployed, each week within the first two years after the notified termination date, and the effect on the next job lasting at least 2-24 months, and plotted the results in *Figure 5* and *Figure 6*, respectively. *Figure 5* shows the fuzzy RD results for the job finding rate the first 24 months after the notified termination date. The value at zero months after termination in the figure shows the inverse effect on the probability of unemployment, i.e. the effect on *not* starting an unemployment spell at all, and weeks above show the effect on such an unemployment spell ending within 1-104 weeks. This is an attempt to show the job finding rate, where some jobs are found within the notice period. The results show that there is no significant effect on this outcome during the first two years. All estimates are insignificant. Since the time limit for the job search program is at most one year, any positive effect should be detectable during this follow up period.

Figure 6 shows the treatment effect on jobs lasting at least 2-24 months, respectively. The effect is negative but insignificant months 2-6, but is thereafter positive, and significant estimates are found for jobs lasting at least 12 months and most estimates thereafter.

[17] Without the donut, the first stage relationship is just over half as large, yet still significant. The conclusions are similar with respect to the results. The point estimates for the effect on jobs lasting at least 12, 18 and 24 months go in the same direction, but are also smaller and not significantly different from zero.

[18] The point estimates for the probability of unemployment and unemployment duration change sign but remain insignificant, and the point estimates for the job duration effect are larger without including covariates. The effect on the average monthly income in the first job and total income the first year after notice is negative but insignificant without covariates, while the effect on total in income the second year after notice then shows a smaller but positive and insignificant point estimate. The year of termination and municipality fixed effects affect the estimates most out of the covariates included.

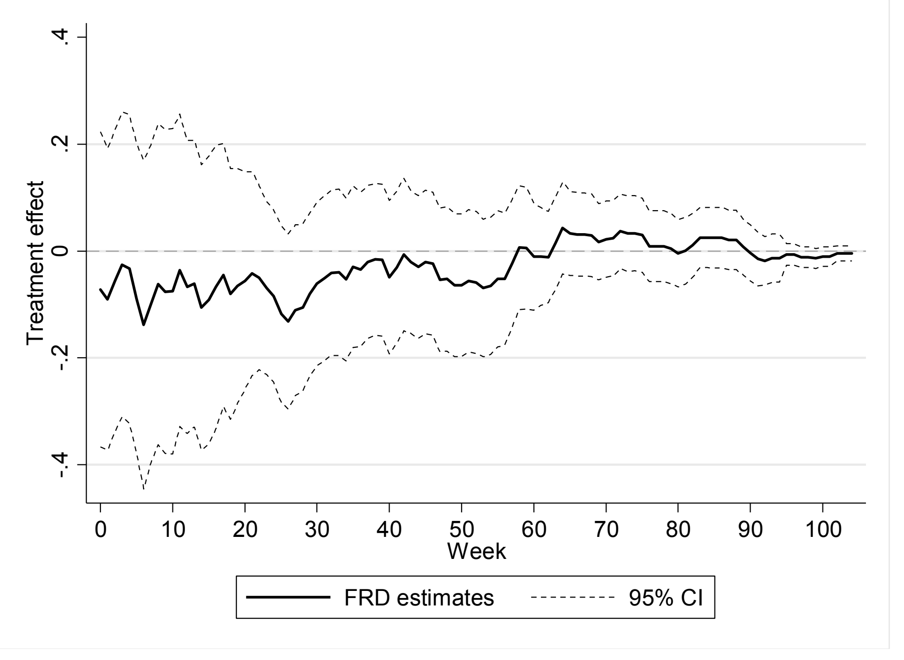


Figure 5. Treatment effect on job finding rates

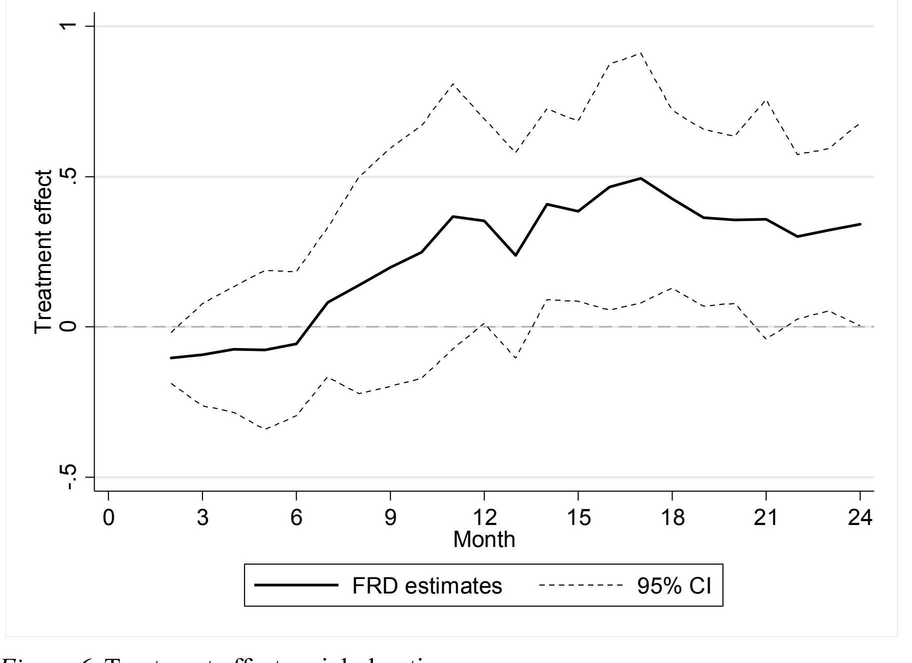


Figure 6. Treatment effect on job duration

The reduced form results can be plotted to get a sense of the discontinuity. Plots of the raw potential discontinuities at the cutoff for the outcomes from Table 3 are shown in *Figure A.1*. Although the estimates are smaller with the reduced form, the plots show the same pattern as the results above.

4.1 Robustness analysis

I have performed a number of robustness checks to examine the robustness of the main results presented above. The first involves estimating effects using alternative measures of the outcomes above. These results are shown in Table 4. Using enrollment at the PES[19] instead of UI receipt to measure the probability and duration of unemployment yields the same conclusions as above. The point estimates are somewhat smaller for the unemployment probability while the unemployment duration effect is larger, perhaps reflecting an effect on registering at the PES before unemployment and eligibility for unemployment benefits start, but the point estimate is insignificant using this measure as well. Using employment records to instead measure the probability of non-employment and the non-employment duration[20], a somewhat different picture emerges. The point estimate for the probability of non-employment is large and negative, although not significant, but the estimate for the effect on non-employment duration shows a very large and significant negative effect. Note that the non-employment duration is negative if a job is found during the notice period. These estimates, since based on employment records, are highly sensitive to the inclusion of the fixed effects for the month of notified termination. Concerns of measurement error in employment records is the reason for including these fixed effects in the first place. Without including covariates, the effect is instead insignificant and very close to zero.

The difference in the result for unemployment and non-employment could be explained by a negative effect from treatment on leaving the labor market (this interpretation is to some extent supported by a negative effect on going into higher education after termination, shown in section 4.3). However, this result is sensitive to the strategy used to estimate the notification date, since I allow the value to be negative if the next job is found during the notice period. Using an alternative estimation strategy for the notification date for the

[19] Unemployment is here as being registered as unemployed at the PES starting between the notification date and three months after the notified termination date. Unemployment duration is measured as the length of the first such spell, and zero if no unemployment is registered. If the spell does not end within the follow up period the value of unemployment duration is missing.

[20] Non employment is measured as having a gap in employment periods according to employment records. The length of this gap is measured in days since I have the precise notified termination date from the TSL and PES data, although the employment records contain monthly data. If the new employment is found during the notice period, the value of the non-employment duration is negative.

control group, discussed below, the point estimate for the non-employment duration is smaller and insignificant.

For the main results, I calculate total income the first and second year after notice using employment records, averaging the income in each employment period by the number of months this employment is reported to span. As there is probable misreporting of the length of these periods, an alternative measure to use is the total income the second calendar year after termination, a measure which is not affected by the length of employment spells. Using this measure yields an estimate quite similar to the main estimate for income the second year after notice, and it is weakly significant. This may suggest that the earning prospects are improved as a result from treatment. For the job duration outcome, I use an alternative definition of the next job which attempts to more closely capture the first steady job found. I have defined the first steady job as the first job where the combined income is at least 0.5 times the median income of a 45-year old, a measure which has previously been used for yearly income to define the time of labor market entry (i.e. Engdahl & Forslund 2016 and Erikson et al 2007). The income level used is SEK 145,000, around USD 16,500. Using this measure, the average job duration effect is stronger than for the main job duration measure, and significant.

Table 4. *Results, alternative outcomes*

Outcome	(1) RF	(2) FRD
Probability of unemployment, PES enrollment	0.018	0.042
	(0.066)	(0.149)
Unemployment duration, PES enrollment	25.829	61.609
	(35.535)	(79.019)
Probability of non-employment	-0.086	-0.207
	(0.060)	(0.137)
Non-employment duration	-88.899**	-212.473**
	(44.800)	(101.878)
Total income two years after termination	226.107	550.638*
	(141.002)	(322.613)
Duration of first steady job	5.165**	12.567**
	(2.570)	(5.687)

Note: Each cell represents the result from a separate regression, with each row showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively. The number of observations is 2,449 within the bandwidth.

Since I have estimated the notification date for the control group, I have, as a robustness check, used the date when the individual level data on who is given notice is reported to the PES as notification date for this group instead. The conclusions are unchanged. The results are found in Table A.2. The point estimate for the effect on the probability of unemployment changes sign but is close to zero, while the estimate for the unemployment duration effect is similar to the main point estimate. The job duration results are similar as well, as is the estimate for the total income the second year after notice. The estimates for the effect on average monthly income and total income the first year after notice has the opposite sign from the main specification, but is not significant in this specification either.

The choice of bandwidth in a regression discontinuity design is ad hoc, and a risk is that results are sensitive to this choice. As a robustness check, I have calculated the results using both larger and smaller bandwidths of 2, 4, 5 and 6 months, to check that the conclusions are not affected by the bandwidth choice. The results are shown in Table A.3. My conclusions are robust to changes in the bandwidth size. The first stage relationship is somewhat stronger using larger bandwidths. The point estimate for the effect of treatment on the unemployment probability and duration are not significant irrespective of the bandwidth used, and the duration estimates decrease when the bandwidth is increased. The point estimate for the unemployment probability is negative using the smaller two month bandwidth, and more positive with the largest bandwidth of six months. The estimates for the job duration effect decreases as the bandwidth is increased, and significance levels also decrease for the effect on jobs lasting at least 12-24 months, even though standard errors decrease, but where the effect is strongest, for jobs lasting at least 18 months, the effect is significant with all bandwidths used. For the income-related outcomes, there is no significant effect no matter which bandwidth is used, and the estimate sizes decrease with the bandwidth size. The effect on average monthly income and total income the first year even changes sign when using the six month bandwidth. The estimated effect on total income the second year, on the other hand, is always positive but never statistically significant.

My sample is defined by one data source, the PES register on notices, while treatment status is identified using another, the TSL register. To estimate causal effects, I need to compare treated with comparable control units *at the start of treatment.* Since controls are collected using a different data source than treated units, there is a risk that the timing of inflow into the different data sources differs in such a manner that the results are affected. If sample inclusion is determined at a point in time prior to the determination of the treatment status, there is a risk that the probability of entering the treatment program is affected by outcome variables. If a notice is first reported to the PES, and it then takes a while before TSL is involved through an application, some affected individuals may have been rehired and are therefore not included in the treatment group even though they were eligible. I have examined this possibility by comparing the date that notified individuals are reported to the PES and TSL receives the application for transition support for all eligible workers. It does not seem to be a systematic timing difference – the dates are often close in time[21] and the PES date is before the TSL date about half the time and vice versa. This however does not ensure that individuals may not find a job before they have had their second meeting with the coach and thereby started the counselling program.

Another possibility is that entire notices are retracted before TSL is involved, which would bias the results due to a higher rehire rate among control units. This would not be detected by examining application dates since no application is made. To test whether this drives any of my results, I have excluded all notices where none of the individuals involved are treated. This reduces my baseline sample within the bandwidth by 18.5 percent, or the number of control units by 28.5 percent. The results are found in Table A.4. By reducing the sample like this, the conclusions are not affected. The point estimates for the effect on the unemployment probability and duration change sign but are still far from significant, but the estimates for the job duration effect in terms of jobs lasting at least 12, 18 and 24 months respectively are close to the main estimates and significant. The estimates for the income related outcomes are positive but not significant as when using the main sample.

One concern about the causal interpretation of the results, even though they seem robust, is that there could be some other discontinuity at the same cutoff that affects the outcomes as well. Using the twelve month cutoff of qualifying employment, one such factor could be discontinuities in the length of the notice period. In many cases, the length of the notice period is dependent on the length of the total employment period within the firm, which is likely to correlate strongly with the forcing variable used in my RD-approach. I test the exclusion restriction with respect to this factor by estimating the reduced form model on the estimated length of the notice period. I use both the estimated notification date, and the date when the individual level data on who is given notice is reported to the PES.

Table 5. *Reduced form results, notice periods*

Outcome	(1) True cutoff	(2) Placebo cutoff
Length of notice period	52.735***	18.294
	(19.238)	(18.860)
Length of notice period, PES estimate	11.542*	13.538**
	(6.695)	(6.044)
Observations	2,449	4,392

Note: Each cell represents the result from a separate regression, with each row showing the reduced form results for a separate outcome, using the 12 and 24 month cutoff, respectively. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

[21] The difference is at most one month in half of the sample for which both dates are available.

The results, shown in column 1 in Table 5, show a strong discontinuity in the former and a smaller but significant discontinuity in the latter. It is not clear which of these estimates of the true notification date for the control group is closer to the true value. Nevertheless, as a placebo test, I have estimated the same results using a placebo cutoff of 24 months, instead of 12. Column 2 in Table 5 show the results of same test of the exclusion restriction using a placebo cutoff of 24 month of qualifying employment. The results are similar with respect to the PES notification date, but much smaller, yet positive, with respect to my estimated notification date.

Table 6. *Results using the placebo cutoff*

Outcome	(1) RF
Probability of unemployment	0.015
	(0.054)
Unemployment duration, days	5.742
	(17.453)
Duration of first job, months	-0.637
	(1.709)
at least 6 months	-0.008
	(0.045)
at least 12 months	-0.029
	(0.054)
at least 18 months	-0.023
	(0.046)
at least 24 months	-0.051
	(0.045)
Average monthly income at first new job	317.934
	(1261.438)
Total income first year after notification	6156.213
	(8430.660)
Total income second year after notification	1587.126
	(11091.544)
First stage relationship	0.025
	(0.049)

Note: Each cell represents the result from a separate regression, with each row showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively. The number of observations is 4,392 within the bandwidth.

The results from the placebo cutoff are shown in Table 6. At this cutoff, there is no discontinuity in treatment assignment and no effect should therefore be found using this specification. Any measurement error- or spurious effects due to having a full year value of the forcing variable should be picked up using this cutoff value. The first stage relationship is close to zero and not statistically significant. The reduced form estimates are close to zero and insignificant (except for the income related outcomes, which are in any case also insignificant). This indicates that the results are not driven by the discontinuity in the length of the notice period.

4.2 Heterogeneous effects

There are a number of different factors that determine the nature of the assistance provided to notified workers that may affect its effects, and are therefore interesting to investigate further. The timing of program start is such a factor. The size of the notice and the experience of the supplier are factors that indicate the quality of assistance given. The sample size close to the cutoff can become very small when the sample is divided into different subgroups, and the interpretation of the results should take this into account. I compare estimates between groups but without putting too much trust in the point estimates themselves. Effects are not significantly different between groups.

How soon after being given notice the program starts is interesting given the aim of this study, to increase the knowledge about the effectiveness of early assistance to job seekers. As previously discussed this is mainly determined by the character of the job and the employers' possibilities of giving the worker time off during work hours. Table A.5 shows the effects by the timing of program start. Note that each subgroup is compared to those that never receive treatment. The first stage relationship is stronger the sooner the program starts. There is no pattern with respect to the point estimates that suggests that the program is more effective the sooner it starts. No subgroup has a significant effect on unemployment outcomes. If anything, point estimates suggest that the positive effect on job duration is stronger the later the program start. The fact that the effects are not stronger when assistance starts early also indicates that my results are not downward biased by the timing of the inflow to PES and TSL data sources. If results were biased due to workers finding jobs after being given notice but before entering treatment, so that the treatment and control groups are systematically different at (potential) program start, this bias would be smaller the sooner the program starts.

I have also investigated how the effect differs depending on starting the program during or after the period of notice, seen in Table A.6, although the sample size for the latter subgroup is small since 85 percent of the total sample starts the program during the period of notice. This analysis yields the same conclusions. The results do not suggest that early intervention is more effective in reducing unemployment or improving job quality.

I have examined how effects differ depending on how many workers were involved in the notice. As previously discussed, in very large layoffs it happens that the firm provides the workers with complementing measures, which could decrease the effectiveness of the counselling program. Since the firm and union choose a provider within the application process, it is also the case that all notified workers in a layoff enters the program with the same provider. With large layoffs, there is a risk that the quality of the treatment for each worker is lower due to congestion. Both of these things suggest that the effectiveness of the treatment would be greater for smaller layoffs. This is also what is suggested by the estimates in Table A.7. The first stage relationship is similar in the subgroups with notices up to 15 people, 16-25 people and 26-80 people. Within notices of more than 80 people, the first stage is only somewhat smaller, but not significant. Although there is no significant effect on unemployment probability or duration for any of the subgroups, both point estimates are negative for the smallest notices, of up to 15 people, while not for larger notices. The effect on job duration points in the same direction. There is, however, not a linear pattern suggested by the point estimates that smaller is monotonically better.

I have also studied the effect depending on the number of clients the supplier has had in total during the period I study. Among 280 suppliers there are five that have had more than 10,000 clients in total. These have supplied 76 percent of the main sample. All workers given assistance from suppliers with less than 10,000 clients are therefore in one single group in Table A.8. Both treated subgroups are compared to all not treated. The results suggest that the effects are more favorable with the smaller suppliers. However, it is difficult to draw conclusions from these results, since the group of small suppliers consists of 275 of 280 suppliers in total and their size with respect to the number of client, within the 1-10,000 range, varies greatly.

It is also interesting to consider how the effect of the counselling provided by the agreement differs between workers according to their own characteristics. I investigate how the effect differs with respect to workers age. The results are shown in Table A.9. Since the sample close to the cutoff is on average younger than the overall sample of notified workers, the division of workers across ages must keep age groups relatively young compared to the age distribution in general among notified workers. I estimate treatment effects separately for workers below 25 years, between 25 and 39 years and 40 years or older. The first stage relationship is not significant for the oldest group. The results indicate that the effects are better for those youngest. The unemployment duration estimate is negative, although not significant, and the job duration effect is completely driven by this group.[22]

[22] The first stage relationship is small and insignificant for the women in the sample, therefore it is not possible to evaluate differences between genders. Since 70 percent of the sample has a high school education, differentiating the effect across educational levels is not very informative either. Comparing the effects over the business cycle is also uninformative since the first stage relationship is only significant for those displaced during the years 2008-2009, during the global financial crisis, when most of the notifications were also made.

4.3 Extension

Aside the labor market outcomes studied so far, there are a few other outcomes that may be affected by the counselling program provided by the SN-LO Employment Security Agreement. The purpose of the counselling is to minimize the time spent in unemployment, and to do so the individual needs of each worker is in focus. For some workers, this may not mean finding a new job. I therefore study the effect of the treatment on a number of other outcomes, as an extension. In particular, I investigate whether the counselling program had any effect on the probability to start an own firm, going into higher education, and receiving disability insurance or getting social assistance. Starting an own firm is a binary variable that does not take into account the success of the company. Going into higher education is proxied by the receipt of student aid. In Sweden, higher education is free and student aid is granted to all students accepted to a course or program[23]. The receipt of student aid is therefore almost universal among students within higher education. The results show that the program has no significant effect on starting a company or receiving disability insurance or social assistance[24]. However, there is a negative effect on going into higher education which is statistically significant the second year and onwards. Estimates of the effect for this outcome are presented in Table 7.

Table 7. *Results, higher education*

Outcome	(1) RF	(2) FRD
Studying the first year after termination	-0.051	-0.123
	(0.041)	(0.093)
Studying the second year after termination	-0.086**	-0.210***
	(0.037)	(0.087)
Studying the third year after termination	-0.107***	-0.309**
	(0.044)	(0.136)
Studying the fourth year after termination	-0.107***	-0.280***
	(0.038)	(0.106)

Note: Each cell represents the result from a separate regression, with each row showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

The results suggest that the counselling program may convince some workers to stay in the labor market instead of going into education after being notified. This is also in line with the difference between the results for non-

[23] Student aid in Sweden consists of a grant and a loan. Students can apply for the grant only or both the grant and the loan. A prerequisite for receiving student aid is to pass a set number of course point per semester. If the student fails to do so, the student aid must be returned. Student aid can be granted for a period of at most twelve semesters, or six years, of higher education full time.

[24] Estimated results are available on request.

employment and unemployment probabilities and durations previously presented (although the main results for the non-employment outcomes are not robust to the notification date estimation strategy).

5 Conclusions

Matching of the right workers to the right jobs is important for the efficiency of the labor market. Active labor market policies aim to facilitate the search process for the unemployed, so that they find better jobs faster. In this study, I have evaluated the effects of early job search assistance, provided to displaced workers even during the period of notice, by means of a Swedish collective agreement. More than 85 percent of the sample starts the counselling program before their notified termination date for their current employment. My results are estimated using a fuzzy regression discontinuity design based on the tenure requirement for eligibility. I estimate the effect for workers with short tenure, displaced through mass-layoffs, on job finding rates and subsequent quality of jobs found.

My results do not suggest that the counselling program has had any effect on the probability of becoming unemployed or the unemployment duration. It also has not had any significant effect on subsequent income within two years following termination, or the average monthly income within the first job found. My results do, however, indicate that the program has had a strong, positive effect on the duration of the next job. The main results suggest that the average job duration increases by on average seven months, and although this estimate is not significant, there is a significant effect on the probability that the next job last at least 12, 18 and 24 months. The largest point estimate suggests that there is a 42.5 percentage point higher probability that the next job lasts at least 18 months for the treated. The results also suggest that there is a negative effect from treatment on going into education after termination, and in line with this there is some indication that the program may decrease the probability of leaving the labor market. My results do not indicate that the effect of the program depends on how soon it starts after the worker has been given notice, in line with previous results. The positive effect on job duration is driven by young workers, below 25 years.

My study evaluates the effect of early and individually focused job search counselling without any element of monitoring with respect to the unemployment insurance. The overall results do not imply that early intervention is effective with respect to unemployment. However, the design of the program does seem to have favorable impacts on the quality of the next job in terms of duration. There are different ways that these results may be understood. The lack of a significant effect on the unemployment probability and duration can either be a sign that the counselling program offered through the Employment Security Agreement for blue-collar workers in Sweden is ineffective with respect to increasing job finding rates. It could also be the case that the counselors have a different focus than simply minimizing unemployment spells, which could be indicated by the result found for the non-employment probability and the probability of going into higher education. The positive effect on job duration could also be a sign that the counselors focus on helping workers find a better match, which could have an adverse effect on job finding (and offer acceptance) rates. The aim of the counselling as stated in the standard agreement between TSL and their counselling suppliers is to help each participant to find a new employment or to start their own firm as soon as possible. However, this is to be achieved according to the needs and prerequisites of the participant, and suppliers are evaluated on their results according to two targets; a 70 percent job finding rate among participants within a year, including start of an own firm, and an 80 percent satisfaction rate among participants, the union and the dismissal firms. These stipulations may steer the focus of the counselling in another direction than simply minimizing unemployment durations.

Another explanation for the lack of a significantly positive effect on job finding rates could be a low take-up of the counselling services, in this case for example through little contact with the counsellor after starting the program. The fuzzy RD design implies that the results are driven by compliers, i.e. those taking up treatment. TSL evaluations suggest that the overall take-up rate is quite high. In principle, there is no reason to suspect that the individuals in my sample should have a lower take-up than on average. There is, however, a possibility for suppliers to redistribute funds between individuals within a project (i.e. between individuals within the same TSL-application), which may result in lower effort pointed towards those close to the cutoff, if these are perceived as more easily placed in a new job or having better chances of finding a job on their own.

As my confidence intervals are quite large, it may also be the case that the lack of significant effects for certain outcomes is simply due to a lack of power. Even though estimates, if taken at face value, indicate a positive effect on the unemployment duration, the range of the confidence interval does not exclude quite large negative effects, a direction more in line with previous studies.

My estimates of the local average treatment effect around the cutoff may be different than the overall effect of the program. I estimate the effect for blue-collar workers with consecutive tenure within the agreement of around one year. These are younger, less often married, and have shorter labor market experience than workers eligible for the counselling program in general. They also have more recent job search experience, and short tenure can also be a signal of higher employability. This in turn would suggest that the content of the program within this sample is less intensive than on average. The baseline sample also excludes 61 percent of all treated individuals who are notified within small notices and therefore are systematically different from the available control group, while the results provide some indication that the program is more effective when the notice consists of few workers. These arguments imply that the estimated effect, at least with respect to the job finding rate, may be a lower bound of the average treatment effect of all treated. One hypothesis could be that the positive effect on subsequent job quality is the result of a shifted focus of the counselling of this group. If individuals close to the cutoff are more likely to find a job on their own, counsellors may help them improve, rather than find, matches, to reach the target satisfaction rate among participants.

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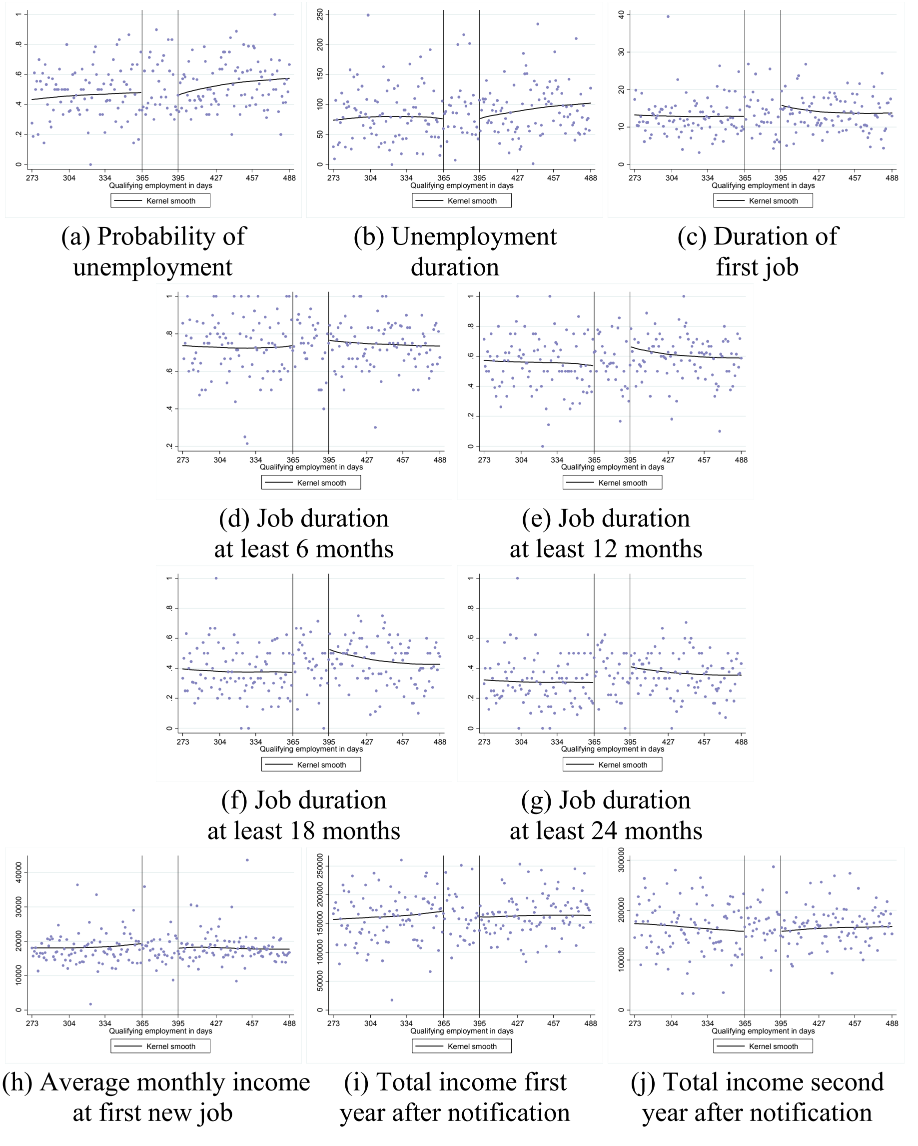
Appendix



(a) Probability of unemployment

(b) Unemployment duration

(c) Duration of first job

(d) Job duration at least 6 months

(e) Job duration at least 12 months

(f) Job duration at least 18 months

(g) Job duration at least 24 months

(h) Average monthly income at first new job

(i) Total income first year after notification

(j) Total income second year after notification

Figure A.1. Outcomes by days of qualifying employment

Table A.1. *Reduced form estimates of basic characteristics*

Outcome	(1) RF
Age at notice	1.086
	(1.384)
No. of years with income before notice	0.042
	(1.024)
Gender (Woman=1)	0.035
	(0.064)
Years of education before notice	-0.110
	(0.165)
Married at notice	0.042
	(0.047)
Mean annual earnings five years before notice (SEK 100)	27.130
	(119.652)
No. of children in household below 18 at notice	0.081
	(0.110)
Days of unemployment before notice	-233.940
	(150.105)
Local unemployment rate (county level)	-0.197
	(0.217)
Born in Sweden	-0.107***
	(0.037)
Size of notice	9.220
	(8.696)
Firm size	-45.385
	(377.364)
Observations	2,750

Note: Each cell represents the result from a separate regression, with each row showing the reduced form (RF) results for a separate variable. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Table A.2. *Results using PES date as notification date*

Outcome	(1) RF	(2) FRD
Probability of unemployment	-0.004	-0.009
	(0.066)	(0.141)
Unemployment duration, days	7.599	17.487
	(17.964)	(38.345)
Duration of first job, months	2.360	5.195
	(2.503)	(5.031)
at least 6 months	-0.044	-0.098
	(0.057)	(0.119)
at least 12 months	0.142*	0.319**
	(0.075)	(0.161)
at least 18 months	0.183***	0.414***
	(0.067)	(0.144)
at least 24 months	0.154***	0.350***
	(0.055)	(0.119)
Average monthly income at first new job	-764.162	-1720.212
	(1746.263)	(3642.357)
Total income first year after notification	-6203.525	-14275.120
	(15448.327)	(33076.919)
Total income second year after notification	21874.572	50336.244
	(18973.580)	(40981.551)

Note: Each cell represents the result from a separate regression, with each row showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively. The number of observations is 2,204 within the bandwidth.

Table A.3. *Main results using different bandwidths*

Bandwidth	2 months		3 months		4 months		5 months		6 months	
Outcome	(1) RF	(2) FRD	(3) RF	(4) FRD	(5) RF	(6) FRD	(7) RF	(8) FRD	(9) RF	(10) FRD
Probability of unemployment	-0.006	-0.015	0.030	0.072	0.022	0.053	0.038	0.092	0.045	0.109
	(0.095)	(0.211)	(0.067)	(0.151)	(0.055)	(0.126)	(0.047)	(0.109)	(0.041)	(0.095)
Unemployment duration, days	10.562	25.857	9.130	22.080	4.604	11.064	2.230	5.374	1.506	3.633
	(23.119)	(51.252)	(16.633)	(37.518)	(13.647)	(31.161)	(11.691)	(27.073)	(10.276)	(23.990)
Duration of first job, months	5.478*	13.051*	3.057	7.043	2.151	4.909	1.251	2.860	0.879	2.007
	(3.064)	(6.862)	(2.173)	(4.684)	(1.820)	(3.924)	(1.619)	(3.524)	(1.407)	(3.083)
at least 6 months	-0.051	-0.124	-0.024	-0.057	0.003	0.006	0.011	0.027	0.016	0.038
	(0.078)	(0.171)	(0.055)	(0.122)	(0.046)	(0.104)	(0.040)	(0.091)	(0.035)	(0.080)
at least 12 months	0.165	0.406	0.147**	0.352**	0.111*	0.262*	0.091*	0.214*	0.090*	0.213*
	(0.111)	(0.258)	(0.075)	(0.174)	(0.062)	(0.143)	(0.054)	(0.126)	(0.047)	(0.110)
at least 18 months	0.186*	0.460**	0.177***	0.425***	0.142***	0.337***	0.106**	0.251**	0.087*	0.206**
	(0.095)	(0.227)	(0.064)	(0.151)	(0.054)	(0.127)	(0.050)	(0.117)	(0.044)	(0.104)
at least 24 months	0.135	0.336*	0.117**	0.281**	0.099**	0.235**	0.071*	0.169*	0.058	0.138
	(0.083)	(0.194)	(0.057)	(0.131)	(0.047)	(0.109)	(0.043)	(0.100)	(0.038)	(0.089)
Average monthly income at first new job	271.934	661.281	668.474	1596.613	1035.306	2440.178	658.754	1552.904	-13.319	-31.354
	(2865.499)	(6319.703)	(2093.782)	(4681.120)	(1659.391)	(3725.718)	(1356.444)	(3074.530)	(1167.033)	(2656.993)
Total income first year after notification	1934.497	4735.765	3700.312	8948.877	5571.856	13389.734	3747.593	9033.483	-350.710	-845.846
	(19993.899)	(44600.625)	(13646.811)	(30982.060)	(11108.205)	(25456.872)	(9436.620)	(21902.451)	(8250.917)	(19282.029)
Total income second year after notification	6193.719	15162.594	19506.061	47173.685	19262.352	46289.377	14298.761	34466.817	9346.494	22541.975
	(22753.807)	(50856.604)	(16407.603)	(37578.676)	(12964.719)	(29879.303)	(10716.245)	(24910.638)	(9206.808)	(21524.958)
First stage relationship	0.353***		0.352***		0.380***		0.400***		0.406***	
	(0.067)		(0.050)		(0.043)		(0.037)		(0.034)	
Observations	1,568		2,449		3,194		4,109		4,915	

Note: Each cell represents the result from a separate regression, with each row showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome with the specified bandwidth. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Table A.4. *Results excluding notices where no workers are treated*

Outcome	(1) RF	(2) FRD
Probability of unemployment	-0.019	-0.040
	(0.067)	(0.131)
Unemployment duration, days	-5.389	-11.433
	(19.329)	(38.036)
Duration of first job, months	2.755	5.670
	(2.274)	(4.283)
at least 6 months	-0.050	-0.104
	(0.060)	(0.116)
at least 12 months	0.167**	0.349**
	(0.078)	(0.155)
at least 18 months	0.221***	0.464***
	(0.070)	(0.140)
at least 24 months	0.146***	0.307***
	(0.060)	(0.118)
Average monthly income at first new job	1119.882	2330.272
	(1492.835)	(2868.842)
Total income first year after notification	7015.314	14884.233
	(14204.796)	(27908.063)
Total income second year after notification	18828.053	39947.054
	(16602.660)	(32799.178)

Note: Each cell represents the result from a separate regression, with each row showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively. The number of observations is 2,003 within the bandwidth.

Table A.5. *Results by timing of program start*

	First meeting within 1 month		First meeting between 1 and 2 months		First meeting between 2 and 4 months		First meeting after 4 months	
Outcome	(1) RF	(2) FRD	(3) RF	(4) FRD	(5) RF	(6) FRD	(7) RF	(8) FRD
Probability of unemployment	0.018	0.066	-0.021	-0.099	-0.013	-0.094	-0.035	-0.511
	(0.077)	(0.254)	(0.078)	(0.354)	(0.081)	(0.543)	(0.083)	(1.272)
Unemployment duration, days	-2.874	-10.357	6.484	31.323	-5.642	-41.351	-1.245	-18.328
	(16.868)	(56.119)	(22.791)	(99.614)	(18.615)	(125.107)	(18.322)	(249.055)
Duration of first job, months	3.510	11.848	4.248*	16.313*	4.249*	28.301*	4.665	73.333
	(2.914)	(9.129)	(2.557)	(9.037)	(2.476)	(15.253)	(2.924)	(45.556)
at least 6 months	0.011	0.040	0.011	0.052	0.005	0.038	0.051	0.731
	(0.072)	(0.236)	(0.072)	(0.323)	(0.067)	(0.454)	(0.073)	(1.092)
at least 12 months	0.158*	0.557*	0.188**	0.969*	0.198***	1.708*	0.203**	3.034**
	(0.089)	(0.309)	(0.088)	(0.519)	(0.084)	(0.960)	(0.093)	(1.521)
at least 18 months	0.187**	0.666**	0.213***	1.116**	0.201***	1.501***	0.212***	3.169**
	(0.083)	(0.290)	(0.078)	(0.505)	(0.069)	(0.581)	(0.080)	(1.423)
at least 24 months	0.139*	0.496**	0.195***	1.018***	0.117*	0.873*	0.173***	2.605**
	(0.071)	(0.243)	(0.067)	(0.429)	(0.068)	(0.525)	(0.074)	(1.294)
Average monthly income at first new job	1214.564	4316.246	2044.799	10080.853	1631.796	12006.415	2543.536	39029.391
	(2639.298)	(8594.836)	(2657.831)	(12090.071)	(3023.719)	(20567.424)	(3173.504)	(46642.768)
Total income first year after notification	16237.479	58523.860	14169.411	68450.799	4205.949	30826.351	12665.996	186410.390
	(14009.072)	(47005.875)	(15206.079)	(69356.333)	(15394.812)	(103522.130)	(17281.327)	(284852.594)
Total income second year after notification	27680.856	99768.596*	37779.296**	182507.440**	13409.769	98283.224	29347.436	431917.647
	(17232.424)	(58251.119)	(19106.096)	(88379.470)	(19240.155)	(132586.364)	(21874.322)	(423966.305)
First stage relationship	0.223***		0.207***		0.126***		0.068*	
	(0.062)		(0.043)		(0.047)		(0.040)	
Observations	1,919		1,892		1,753		1,645	

Note: Each cell represents the result from a separate regression, with rows showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome within a separate subgroup. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Table A.6. *Results for program start during vs after notice period*

Outcome	First meeting during notice period		First meeting after notice period	
	(1)	(2)	(3)	(4)
	RF	FRD	RF	FRD
Probability of unemployment	0.034	0.092	-0.011	-0.051
	(0.067)	(0.167)	(0.086)	(0.357)
Unemployment duration, days	7.380	19.859	2.207	9.988
	(16.499)	(41.236)	(19.232)	(79.490)
Duration of first job, months	2.810	7.069	4.087*	17.849*
	(2.547)	(5.897)	(2.352)	(9.418)
at least 6 months	-0.024	-0.063	0.029	0.129
	(0.064)	(0.157)	(0.065)	(0.268)
at least 12 months	0.109	0.289	0.237***	1.487*
	(0.084)	(0.212)	(0.084)	(0.809)
at least 18 months	0.149**	0.397**	0.236***	1.063***
	(0.075)	(0.190)	(0.072)	(0.353)
at least 24 months	0.103*	0.276*	0.167***	0.757***
	(0.062)	(0.156)	(0.067)	(0.316)
Average monthly income at first new job	1154.952	3072.302	1649.399	9929.257
	(2395.345)	(5924.221)	(2681.556)	(15431.396)
Total income first year after notification	10761.710	28959.811	6250.258	28289.638
	(13251.726)	(33225.983)	(16229.983)	(67975.873)
Total income second year after notification	30763.229*	82783.992*	16605.462	75158.891
	(16757.199)	(42404.261)	(18883.884)	(80331.626)
First stage relationship	0.334***		0.166***	
	(0.051)		(0.061)	
Observations	2,211		1,828	

Note: Each cell represents the result from a separate regression, with rows showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome within a separate subgroup. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Table A.7. *Results by notice size*

Outcome	Up to 15 people		16-25 people		26-80 people		More than 80 people	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	RF	FRD	RF	FRD	RF	FRD	RF	FRD
Probability of unemployment	-0.025	-0.057	0.081	0.178	-0.184	-0.496*	1.093***	3.151***
	(0.107)	(0.208)	(0.093)	(0.174)	(0.133)	(0.284)	(0.431)	(0.951)
Unemployment duration, days	-17.679	-40.966	15.741	34.442	43.401	117.017	241.674	696.686*
	(32.138)	(62.464)	(24.173)	(45.557)	(47.520)	(106.356)	(159.754)	(357.667)
Duration of first job, months	9.748**	21.522***	-2.497	-5.407	14.635**	34.192**	-38.496***	-60.133***
	(4.700)	(8.213)	(3.612)	(6.456)	(6.954)	(15.962)	(13.254)	(13.860)
at least 6 months	0.108	0.236	-0.182*	-0.400**	0.290*	0.691**	-0.539	-1.182
	(0.106)	(0.194)	(0.104)	(0.200)	(0.170)	(0.316)	(0.910)	(1.143)
at least 12 months	0.330***	0.735***	-0.001	-0.002	0.362**	0.881***	-0.722	-1.584
	(0.112)	(0.219)	(0.127)	(0.237)	(0.171)	(0.367)	(0.940)	(1.170)
at least 18 months	0.409***	0.918***	0.079	0.171	0.265	0.641*	-0.940	-2.062**
	(0.109)	(0.214)	(0.110)	(0.208)	(0.195)	(0.368)	(0.835)	(1.035)
at least 24 months	0.349***	0.802***	-0.053	-0.114	0.186	0.470	-1.134	-2.487***
	(0.112)	(0.223)	(0.101)	(0.188)	(0.173)	(0.342)	(0.697)	(0.919)
Average monthly income at first new job	2204.714	4847.484	3348.434	7355.703	-4790.348	-11561.936*	-4298.475	-9394.588
	(4102.134)	(7484.152)	(3292.932)	(6234.405)	(3086.018)	(6867.806)	(6877.385)	(10011.241)
Total income first year after notification	37935.835	87903.945*	-6493.620	-14208.298	-50647.092	-1.366e+05	-62735.334	-1.809e+05
	(25790.477)	(50090.154)	(18421.582)	(34847.727)	(38144.148)	(92189.713)	(184613.415)	(305771.372)
Total income second year after notification	81817.026***	189584.315***	-9400.656	-20569.007	-25126.575	-67746.419	-17095.067	-49280.761
	(25348.813)	(50743.524)	(26678.672)	(50139.398)	(38377.273)	(81403.214)	(212399.030)	(377791.132)
First stage relationship	0.352***		0.385***		0.371***		0.288	
	(0.093)		(0.095)		(0.123)		(0.493)	
Observations	810		993		468		178	

Note: Each cell represents the result from a separate regression, with rows showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome within a separate subgroup. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Table A.8. *Results by supplier size*

Outcome	Small suppliers (1) RF	(2) FRD	Large suppliers (3) RF	(4) FRD
Probability of unemployment	-0.042	-0.200	0.040	0.111
	(0.079)	(0.347)	(0.069)	(0.176)
Unemployment duration, days	-14.989	-70.520	23.035	63.705
	(20.346)	(89.935)	(15.778)	(40.696)
Duration of first job, months	5.256**	24.725**	2.693	6.873
	(2.496)	(11.091)	(2.494)	(5.860)
at least 6 months	0.049	0.225	-0.035	-0.095
	(0.066)	(0.284)	(0.061)	(0.156)
at least 12 months	0.236***	1.105***	0.117	0.318
	(0.086)	(0.412)	(0.081)	(0.210)
at least 18 months	0.254***	1.206***	0.143**	0.391**
	(0.075)	(0.375)	(0.068)	(0.180)
at least 24 months	0.239***	1.137***	0.062	0.170
	(0.065)	(0.318)	(0.062)	(0.161)
Average monthly income at first new job	2296.964	10595.189	489.146	1340.359
	(2686.928)	(11371.519)	(2351.966)	(5987.889)
Total income first year after notification	14998.036	70562.175	2925.029	8089.174
	(15525.148)	(69049.051)	(14089.468)	(36278.201)
Total income second year after notification	33606.237*	158109.309*	16493.725	45613.439
	(18761.115)	(84324.088)	(17112.903)	(44496.732)
First stage relationship	0.172***		0.324***	
	(0.050)		(0.052)	
Observations	1,796		2,243	

Note: Each cell represents the result from a separate regression, with rows showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome within a separate subgroup. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Table A.9. *Results by age*

	Younger than 25		25-39 years		40 or older	
Outcome	(1) RF	(2) FRD	(3) RF	(4) FRD	(5) RF	(6) FRD
Probability of unemployment	0.108	0.261	0.054	0.114	0.059	0.426
	(0.093)	(0.193)	(0.123)	(0.219)	(0.102)	(0.543)
Unemployment duration, days	-6.809	-16.460	30.764	65.070	68.927	493.950
	(14.650)	(30.571)	(37.143)	(66.400)	(44.755)	(458.793)
Duration of first job, months	6.001*	14.549**	-0.170	-0.338	5.541	234.111
	(3.206)	(6.666)	(4.239)	(6.756)	(6.178)	(1271.224)
at least 6 months	-0.107	-0.265	-0.037	-0.075	-0.003	-0.020
	(0.084)	(0.179)	(0.112)	(0.190)	(0.144)	(0.840)
at least 12 months	0.170*	0.421**	-0.060	-0.122	0.264	2.188
	(0.093)	(0.208)	(0.134)	(0.226)	(0.167)	(2.675)
at least 18 months	0.276***	0.684***	0.060	0.120	0.360***	3.303
	(0.082)	(0.187)	(0.114)	(0.195)	(0.146)	(3.970)
at least 24 months	0.217***	0.543***	0.036	0.072	0.190	2.032
	(0.080)	(0.176)	(0.103)	(0.172)	(0.138)	(3.019)
Average monthly income at first new job	4339.957	10707.768	-3610.533	-7310.036*	1538.706	12420.590
	(4005.138)	(8559.246)	(2369.415)	(4057.193)	(4560.123)	(29894.956)
Total income first year after notification	10222.934	24714.619	3224.386	6819.993	-20284.794	-1.454e+05
	(18575.298)	(39187.985)	(24646.885)	(44042.331)	(32009.426)	(197090.904)
Total income second year after notification	47903.425*	115809.703**	-3051.389	-6454.080	-3250.594	-23294.729
	(24596.773)	(52365.720)	(24985.242)	(44655.538)	(33363.561)	(181111.560)
First stage relationship	0.339***		0.421***		0.140	
	(0.083)		(0.090)		(0.141)	
Observations	1,039		813		592	

Note: Each cell represents the result from a separate regression, with rows showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome within a separate subgroup. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

II: Lump-sum severance grants and the duration of unemployment

1 Introduction

Unemployment insurance is an insurance against large consumption drops in the event of unemployment. Unemployment benefits provide liquidity when workers become unemployed to help smooth consumption during the unemployment spell (Holmlund 1999, Bloemen & Stancanelli 2005, Shimer & Werning 2008). There is a vast literature suggesting that higher unemployment insurance benefit levels are associated with longer unemployment duration (e.g. Meyer 1990, Lalive 2008, Card et al. 2015 etc.).

The literature mostly focuses on moral hazard aspects to explain this relationship. If search effort is reduced due to the reduction of the relative price of leisure when the benefit level is increased, the response is indeed a suboptimal moral hazard effect. Chetty (2008), however, argues that there is a second component to this relationship that could give rise to the same response. The response could be explained by the increased ability of the unemployed to smooth consumption, which lowers the value of finding employment. In contrast to the prolongation of unemployment caused by the creation of a wedge between private and social marginal costs, this "liquidity effect" is a socially beneficial response to the mending of credit and insurance market failures. Better ability to smooth consumption also enables the worker to hold out longer for a good worker-employer match. Both these effects are welfare-enhancing.

The social optimality of an unemployment insurance policy can be revealed by estimating the effect of a lump-sum severance grant on the unemployment duration. This type of grant does not distort marginal incentives. If this non-distortionary lump-sum liquidity contribution creates a positive response on unemployment duration, it implies that also an increase in unemployment benefits would permit the worker to make a more socially optimal consumption choice. This is argued by Chetty (2008). If, on the other hand, there is no duration response of the grant, any positive response of an increased benefit level is due to moral hazard and the policy is thus suboptimal.

This study evaluates the effects of such a lump-sum severance grant in Sweden. A collective agreement, which covers most Swedish blue-collar workers, stipulates that certain workers can receive a lump-sum severance grant, equivalent to between around one and two months of the previous monthly income if they are displaced due to redundancy. Eligibility for the grant has a strict age requirement, which enables me to study the effect of this grant using a regression discontinuity design.

Little is known about the effects of severance pay, despite the fact that many employers offer severance packages to displaced workers[1]. Lack of

[1] Severance payments are e.g. common components of employment protection against no-fault dismissals among OECD countries (OECD 2013).

data and non-random treatment assignment constitute problems for the estimation of causal effects. Three studies that do directly study the effect of lump-sum severance grants are Card, Chetty & Weber (2007), Basten, Fagereng & Telle (2014), and an earlier study by Kodrzycki (1998). Card, Chetty & Weber (2007) study the effects of severance grants in Austria. They find that a lump-sum severance payment of two months of earnings, around the same level as the grant studied in this paper, reduces the job finding rate by, on average, 8-12 percent. Basten, Fagereng & Telle (2014) find that a lump-sum severance grant of on average 1.2 months of previous earnings reduces the fraction re-employed after about a year by 14 percent in Norway. Kodrzycki (1998) estimates the effects of severance pay in the U.S., and also finds that it causes substantially longer unemployment durations.

Easing of liquidity constraints of the unemployed might also be expected to increase the quality of matches, as workers are less desperate for a job and can hold out longer for a better worker-employer match. I study the effect of the severance grant on both unemployment durations and the quality of subsequent matches. The availability of a setting resembling a natural experiment, provided by the sharp age discontinuity in eligibility for the grant, and rich register data that matches all employers in Sweden to their employees, provides a unique opportunity to credibly estimate the labor market effects of severance grants. Given the small number of studies on the effect of severance pay on these outcomes, this study is an important contribution to the literature. It also contributes to the knowledge of the relative importance of liquidity and moral hazard effects of unemployment benefits and the socially optimal unemployment benefit level. Chetty's (2008) results imply that the optimal unemployment benefit level exceeds 50 percent of the wage. The results of Card, Chetty & Weber (2007) and Basten, Fagereng & Telle (2014) concur with this as they find significant negative effects on re-employment rates from lump-sum severance grants in settings with unemployment benefits of a baseline replacement rate of 55 and 62 percent, respectively. The average actual replacement rate in my sample is, although lower than the baseline of 80 percent in Sweden, higher than in both Norway and Austria. In this study, I investigate whether a similar lump-sum grant has similar effects in Sweden, and if so, for what workers.

To be eligible for the grant, workers must be at least 40 years old at the termination date. I use the resulting discontinuity in eligibility to estimate its effects using a fuzzy regression discontinuity design. I identify displaced workers using data from the Swedish Public Employment Service and the TSL Employment Security Fund between 2006 and 2012. I match this data to data on what workers have received the severance grant from AFA insurance, the insurance company that administers the grant, and to Swedish register data providing a rich set of background characteristics and information about outcomes.

I find that the lump-sum grant has a positive effect on the probability of becoming unemployed and the completed unemployment duration, while this effect is less evident for non-employment. There is an initial positive and significant effect on unemployment that diminishes over time. Point estimates for the effect on the job finding rate using the non-employment definition, although insignificant for the most part, shows a similar pattern. The difference could reflect the fact that there is measurement error in the latter measure of job search duration, causing noisy estimates, although dynamics through an effect on staying in the labor force, while unlikely at ages for which the effect is estimated, cannot be ruled out. I find no effect on subsequent job quality in terms of job duration or average monthly income the first year in the new job. My analysis also indicates that spousal income is important for the consumption smoothing abilities of displaced workers, as the effects found are driven by workers whose family disposable income is not higher that their individual disposable income. The results also suggest that the effect of this type of grant may be larger in times of more favorable labor market conditions.

The rest of this paper is organized as follows. Section 2 discusses the moral hazard and liquidity effects of a change in the unemployment benefit level and the expected effects of a lump-sum severance grant in a theoretical context, and reviews the related literature. Section 3 describes the institutional setting, and section 4 outlines the empirical strategy and data. The results are presented in section 5, and section 6 concludes.

2 Theory and empirical evidence

2.1 Theoretical background

In a simple permanent income model, if households cannot smooth consumption over transitory income shocks because of imperfect credit markets, both traditional unemployment benefits and lump-sum grants will increase unemployment durations. In addition to the moral hazard effect, a liquidity effect affects workers search intensity by enabling them to smooth consumption in a state of a negative income shock relative to their permanent income level. The empirically established positive relationship between the unemployment benefit level and unemployment duration is a pure moral hazard effect only if workers have access to perfect credit and insurance markets, or if the benefit level is so high that consumption is perfectly smooth between the employed and unemployed states. Since the former is rarely the case in practice, the liquidity effect could explain part of the relationship. This is shown using the job search model outlined below.[2] The model closely fol-

[2] For further details and proofs, see Chetty (2008).

lows Chetty (2008). In this model, credit and insurance markets are imperfect. The analysis of this model also shows the theoretical predictions of the effect of a lump-sum severance grant on unemployment durations.

Consider a discrete time setting, where the agent lives for a finite time of T periods. To simplify, assume that the interest rate and the agent's time discount rate is zero. Also assume that jobs pay a fixed wage, w_t, and that they last infinitely once found. Assets, A_t, are exogenous before job loss.[3] Let s_t denote search effort in each unemployed period, normalized to equal the probability of finding a job in that period. The cost of search effort is denoted $\mu(s_t)$. Each agent pays a tax, τ, when working, and τ is independent of time. Assume that the unemployment insurance benefit in each period, b_t, is strictly lower than $w_t - \tau$.

The agent becomes unemployed at time $t=0$. In each period, the agent puts in search effort s_t, and either finds a job or does not. If a job is found, work begins immediately and the agent gets $w_t - \tau$, and consumes $c_t^e = A_t - A_{t+1} + w_t - \tau$. If a job is not found, the agent gets unemployment benefits b_t, and consumes $c_t^u = A_t - A_{t+1} + b_t$. The flow consumption utility in these two states is denoted $v(c_t^e)$ and $u(c_t^u)$ respectively. The value function of finding a job is:

$$V_t(A_t) = \max_{A_{t+1}} v(A_t - A_{t+1} + w_t - \tau) + V_{t+1}(A_{t+1}) \tag{1}$$

The value function of not finding a job is:

$$U_t(A_t) = \max_{A_{t+1}} u(A_t - A_{t+1} + b_t) + J_{t+1}(A_{t+1}) \tag{2}$$

where

$$J_t(A_t) = \max_{s_t} s_t V_t(A_t) + (1 - s_t)U_t(A_t) - \mu(s_t) \tag{3}$$

$V_t(A_t)$ is unambiguously concave[4], but we have to assume that $U_t(A_t)$ is also concave[5] and that $\mu(s_t)$ is strictly increasing and convex. In each unemployed period, the agent must maximize utility with respect to s_t to choose the opti-

[3] These assumptions exclude reservation wage choices and any effect of the unemployment insurance policy on savings before job loss, which would complicate the model.

[4] This follows from the fact that we assumed that jobs last infinitely once found so there is no uncertainty once the job is found.

[5] To solve the problem of possible convexities, Lentz & Tranaes (2005) introduce a wealth lottery to the job search model with savings, which has a zero risk premium and will therefore only be entered if the value function is convex. The introduction of this lottery smooths out any local convexities. They also show that non-concavity never arises even without the lottery in the model, through simulations using a wide range of model parameters.

mal level of search effort, which depends on the value functions of finding a job or not and the cost of search effort. The first order condition is:

$$\mu'(s_t) = V_t(A_t) - U_t(A_t) \tag{4}$$

This is an intuitive result; the marginal cost of search in period t equals the gain from finding a job in period t compared to not finding a job at the optimal level of search effort. From this first order condition, the effect of an increase in unemployment benefits on the chosen search effort, and thus the probability of finding a job and thereby the unemployment duration, can be disentangled into two components; the moral hazard effect and the liquidity effect. From equation 4, the relation between the asset level and search effort can be derived as:

$$\frac{\partial s_t^*}{\partial A_t} = \frac{v'(c_t^e) - u'(c_t^u)}{\mu''(s_t^*)} \leq 0 \tag{5}$$

The relation in (5) can be interpreted as an expression for the effect of a lump-sum severance grant on search effort. The value of this expression is non-positive since the value depends on the difference between marginal utilities in the employed and unemployed states. Since $b_t < (w_t - \tau)$, if assets do not allow perfect consumption smoothing between the unemployed and employed states, the value of expression 5 is negative because the marginal utility of consumption is higher in the unemployed state. If consumption smoothing between states is perfect, the marginal utilities are equal and the value of the expression 5 is zero. The consumption smoothing abilities of the unemployed can thus be tested by investigating the effect of a liquidity contribution such as the lump-sum severance grant in this paper.

The ability to smooth consumption between states of course depends on the initial asset level, A_0, but also on the wage and tax levels and the unemployment benefit level. If the gap between the inflow of liquid assets between the employed and unemployed state, $(w_t - \tau) - b_t$, decreases, the assets needed to smooth consumption decreases. If this is the case, the liquidity effect goes towards zero, and the unemployment insurance policy comes closer to the optimal level.[6] This is the case when the benefit or tax level increases, or if the wage level goes down.

The following two relations can also be derived directly from equation 4:

$$\frac{\partial s_t^*}{\partial w_t} = \frac{v'(c_t^e)}{\mu''(s_t^*)} > 0 \tag{6}$$

[6] This is true provided that the liquidity effect is negative at the starting point.

$$\frac{\partial s_t^*}{\partial b_t} = \frac{-u'(c_t^u)}{\mu''(s_t^*)} \tag{7}$$

The value of the relation in (6) is positive since we have assumed that the cost of search is strictly increasing and convex, and the marginal utility of consumption is positive. By using estimates of the liquidity effect from expression 5, and the total effect of the benefit level on search effort from expression 7, the welfare effects of the unemployment benefit level can be evaluated. If c_t^u is already close to c_t^e, the effect of the liquidity contribution on immediate consumption will be small (Card, Chetty & Weber, 2007). If this is the case, there is no liquidity effect, and the generosity of the unemployment policy is at or above the socially optimal level.

Inserting (6) and (7) into expression (5) and rearranging, we get:

$$\frac{\partial s_t^*}{\partial b_t} = \frac{\partial s_t^*}{\partial A_t} - \frac{\partial s_t^*}{\partial w_t} < 0 \tag{8}$$

Both components of the effect of an increase in unemployment benefits on search effort contribute negatively to the total effect. The first term on the right hand side of expression 8 is the liquidity effect and the second term is the moral hazard effect. The less opportunity to smooth consumption the agent has, the larger is both the liquidity effect and the total effect of a benefit increase on search effort.

The model assumes a fixed wage level. It therefore does not provide any predictions on the effect of a benefit or asset increase on the quality of the next job. In a more general model, an increase in unemployment benefits or a liquidity contribution in the form of a severance grant could potentially increase the reservation wage and match quality (Card, Chetty & Weber, 2007). When there is heterogeneity in the quality of job offers, if the agent is not as desperate to find a job because of liquidity constraints, he or she can hold out longer for a good match by waiting for a better offer. Such a model is not presented here, but the effect of the grant on match quality is empirically evaluated.

2.2 Previous studies

Chetty (2008) shows that the liquidity effect explains 60 percent of the increase in unemployment duration from increased unemployment benefits in the U.S. Among liquidity constrained households, he finds that lump-sum severance payments of on average USD 4,000 prolong unemployment durations substantially, and that the effect is stronger with larger payments. As previously mentioned, Card, Chetty & Weber (2007) estimate the effect of a lump-sum severance grant in Austria using an RD-design with tenure as the determinant of eligibility. Job finding hazards are lower throughout the unemployment spell in the treatment group. The unemployment duration increases from 150 to 160 days at the discontinuity, and the results are highly significant. The effect is strongest after about five weeks of unemployment and drops after about 25 weeks. This timing is consistent with what we would expect from a liquidity effect; we expect agents to become increasingly sensitive to liquidity as the spell elapses, while the ease of the constraints from the severance payment fades as the grant is exhausted. Card, Chetty & Weber do not find any effects on job match quality, for any subgroup. They study various aspects such as subsequent wages and employment duration as well as probabilities of moving and changing occupation and industry. Kodrzycki (1998) also finds no effect of lump-sum grants on subsequent pay, even though unemployment durations are prolonged. She does however show that severance grants have positive effects on the probability of going into general education.

Severance grants have also been studied in a Scandinavian context. Basten, Fagereng & Telle (2014) study the effects of a lump-sum grant provided through collective agreements by similar means and for the corresponding labor market sector in Norway as the Swedish grant in this study. Their empirical strategy is also similar, but the age requirement is 50 years for eligibility for the Norwegian grant. They estimate the reduced form effects of the grant, since they have no individual recipient information, only which individuals are laid off from firms that are associated with the collective agreement where the grant is stipulated. They find that re-employment rates are reduced by 8 percentage points, or 14 percent, and that an effect is only present for the non-wealthy. They find no significant effects on job duration or wage growth. The estimated effect on the re-employment rate is, however, insignificantly positive the first five months after layoff, and then becomes increasingly negative until the negative effect reaches its maximum after about a year. The effect does not seem to fade during the follow up period of two years. This timing differs from that found by Card, Chetty & Weber (2007). Unemployment insurance benefits are more generous in Norway than in Austria, with higher benefit levels and a significantly longer maximum benefit period. This may imply that liquidity constraints manifest later in the unemployment spell in Norway, explaining the delayed effect, but does not explain why the effect does not fade over time. Uusitalo & Verho (2010) studies the effect of replacing a lump-sum severance grant in Finland with a higher unemployment benefit level at the start of unemployment. Some individuals are however only affected by the loss of the severance grant and not compensated through higher UI benefits. The sample size for the evaluation of this treatment is small and there is no significant effect. However, the point estimate suggest that he loss of the grant has a negative effect on re-employment rates, contrary to previous findings.

Empirical evidence suggests that credit and insurance markets are not perfect and that many people are liquidity constrained during unemployment. Sullivan (2008) shows that, in the US, unsecured credit markets do help low-asset households to smooth consumption in times of temporary income loss due to unemployment. Unsecured debt increase by more than 11 percent of earnings lost. Households in the bottom decile of total assets, however, do not increase their borrowing, suggesting that these households do not have access to unsecured credit during unemployment. High-asset households do not use unsecured debt to smooth consumption over the unemployment spell. Bloemen & Stancanelli (2005) find that unemployment insurance helps recently unemployed workers to smooth consumption in the UK. They study the impact of unemployment benefits on changes in food expenditure, and find that liquidity constrained households reduce consumption more when the replacement rate is lower, while the same relationship is not observed for non-liquidity constrained households. Their findings suggest that unemployment benefits help liquidity constrained workers to smooth consumption.

Kolsrud et al. (2015) study the effect of the replacement rate on unemployment duration, as well as the consumption patterns of the unemployed. They show that a higher replacement rate is associated with longer unemployment durations in Sweden. A benefit decrease late in the unemployment spell affects search effort and unemployment duration early in the spell, which suggests that agents are forward looking. Kolsrud et al. conclude that the Swedish unemployment insurance policy is too generous throughout the unemployment spell. As consumption is measured by expenditure, ignoring e.g. leisure as a consumption good, this result need not be contradictory to the finding that many unemployed workers cannot perfectly smooth consumption between the employed and unemployed states. They find that consumption drops immediately when workers become unemployed, by on average 19 percent, and consumption drops further throughout the spell. There is heterogeneity in the consumption response further suggesting that unemployed workers are liquidity constrained. They also show that most unemployed have few assets, but that those who do have liquid assets use them to smooth consumption.

3 Institutional background

In Sweden, trade unions are traditionally strong and around 90 percent of workers are covered by collective agreements (Kjellberg, 2017). These collective agreements often include so called Employment Security Agreements that stipulate various benefits to workers if they are dismissed due to redundancy. Employment Security Agreements complement public labor market policies in Sweden. These types of complementary benefits for dismissed workers have a long history in Sweden and today approximately 60 percent of the labor force is covered by an Employment Security Agreement. Most agreements include a severance compensation that adds on to the public unemployment benefits above the cap for those with wages high enough to hit it. The main Employment Security Agreement for privately employed blue-collar workers, however, instead includes a severance grant that workers above a certain age are entitled to if they are displaced from a firm that has the agreement. The agreement is one of the largest Employment Security Agreements and covers around 900,000 blue-collar workers, or over 30 percent of all Swedish workers[7]. Out of all blue-collar workers being notified of displacement during the period of study, according to register data on notifications from the PES, 78 percent are notified from firms affiliated with this agreement.

The severance grant is a lump-sum grant that can be given to displaced workers above the age of 40, the size of which depends on the workers' age. In addition to this age limit, the worker must also have been employed by one or several firms, who were affiliated with the agreement in question during the employment period, for at least 50 months during the five years preceding the last day of employment. The dismissal must be due to redundancy from a permanent contract[8], and the worker must also be under 65 years of age to be eligible for the grant. The worker can also not be offered reemployment at the dismissal firm within three months after termination. The worker needs, however, not be a member of the union to be eligible.

The exact amount of the severance grant depends on the workers age. Workers aged 40 to 49 years receive a severance grant amounting to SEK 33,850 (corresponding to around USD 3,850). Above age 49 the amount increases by SEK 1,400 per year of age. The maximum amount of SEK 49,250 is reached at the age of 59 with this scheme, and this is thus the amount given to workers between 59 and 64 years old.[9] The grant is equivalent to between one and two months of the previous monthly income.[10] Workers themselves apply for the severance grant directly to AFA Insurance. The application must be submitted within two years of termination and must be signed by both the worker and the employer. Applications are in most cases submitted close to the termination and the payment is made

[7] The total number of employed workers is specified in Kjellberg, 2017.

[8] The dismissal can be both complete and for part of the employment, meaning that the worker can stay on but work fewer hours than previously. The grant is then given in proportion to the decrease in working hours. For the purpose of this study, I only include full dismissals.

[9] The exact monetary amount changes over time and these are the amounts valid during 2017. Amounts are before tax. Normally a 30 percent tax is withdrawn from the payment. The final municipal tax varied between 28.89 and 34.32 percent during 2006-2012.

[10] The grant replacement rate depends on the previous wage and the age of the worker. I do not have information about wages for the whole sample. As a proxy for the previous wage, I use average monthly income during the five years before the termination year. The 10th and 90th percentile of the grant replacement rate is 1 and 2, respectively.

shortly after the termination date. 50 percent of the full sample of treated workers receives the payment within two weeks after termination, and another 20 percent within one month.

The severance grant does not have any distortionary effects on marginal incentives; eligibility does not depend on unemployment status and the grant does not affect public unemployment benefits (The Swedish Unemployment Insurance Board, 2013). It is set up as an employment security insurance that is financed by the employer throughout the time that the employer is affiliated with the agreement, through an employer fee amounting to a small percentage of total wage costs.[11] The fee thus does not depend on, e.g., past layoffs, and there is no additional cost for the employer when the insurance is used, i.e. when the severance grant is paid to a worker.

The Swedish public unemployment insurance is an insurance against income loss associated with unemployment. Unemployment benefits are generous, especially in an international comparison. The baseline replacement rate is 80 percent of the previous wage the first 40 weeks of unemployment and 70 percent for the rest of the benefit period.[12] Maximum benefit duration is 60 weeks, but for parents with children under 18 it is prolonged to 90 weeks. Before March 2007[13], the baseline replacement rate was 80 percent throughout the benefit period. The baseline replacement rate is subject to a cap, which lowers the replacement rate for those with earnings high enough to hit it. About 50 percent of unemployment benefit recipients in Sweden are affected by the cap (Kolsrud et el. 2015). The average replacement rate among the workers in my sample is therefore lower, on average 67 percent. As mentioned above, many workers are eligible for additional unemployment compensation through Employment Security Agreements, usually providing compensation to counteract the downward effect of the cap on the replacement rate for those who are affected by it. This means that the average actual replacement rate in Sweden is even higher. The sample in this study, however, is not affected by any other compensation through the Employment Security Agreement than the severance grant that is being studied.

[11] The percentage is around 0.3 percent of total wage costs. This fee does not only finance the severance grant. It also finances other benefits stipulated in the same agreement, such as a job search counselling program, which is studied separately in the first chapter of this thesis.

[12] Not everyone who becomes unemployed receives unemployment benefits. Eligibility criteria involve a previous employment requirement and membership requirement, as well as search requirements during the benefit period which is monitored by the PES.

[13] A previously higher cap for the first 20 weeks of unemployment was also abolished at the same time.

4 Empirical strategy and data

The Employment Security Agreement for privately employed blue-collar workers was formed in 2004, although the severance grant existed as part of the collective agreement even before that. This study uses data from 2006 on recipients of the lump-sum severance grant provided by the agreement. The eligibility criteria for the grant creates a unique natural experiment type setting, which I use to find the causal effects of the grant on unemployment durations and the subsequent job quality of those who do find a job.[14] To be eligible for the severance grant, the displaced worker must be at least 40 years old on his or her proposed termination date, which creates a sharp discontinuity in eligibility over age that I use to estimate effects using a regression discontinuity design.[15] This close to exogenous variation in eligibility created by the sharp age requirement ensures that the treatment and control groups only differ with respect to treatment and the exact age. The regression discontinuity design compares individuals just at the cutoff at age 40, making sure that individuals are similar enough also in terms of age that the estimated effect can be interpreted causally. The regression discontinuity model can, in its simplest general form, be summarized by the following equation:

$$y_i = \alpha + \tau D_i + \beta_1(1\text{-}D_i)(X_i\text{-}X_0) + \beta_2 D_i(X_i\text{-}X_0) + \varepsilon_i \tag{9}$$

where y_i is the labor market outcome of interest and D_i is a dummy variable for treatment status. X_i is the forcing variable, the variable that determines treatment, in this case age, and X_0 is the cutoff value of the forcing variable, in this case 40. The estimator of interest is τ, the effect of the treatment on the labor market outcome of interest. β_1 and β_2 determines the effect of the forcing variable on the outcome for the untreated and the treated respectively, and ε_i is an error term.

The design in itself is based on the fact that individuals have different values of the forcing variable. If age affects the outcome, the results will be biased. For this reason, the sample is restricted to observations within a

[14] 90 percent found a new job during the follow up period.

[15] The eligibility criteria also allow a similar FRD design, using the number of qualifying months as the forcing variable, with a cutoff at 50 months the five years preceding the termination date. There is measurement error in the forcing variable, which is based on monthly employment period data. This causes problems for estimating the effects using this criterion as the basis for the RD-analysis. Since the start and end dates of employment spells are unknown, the data yields a maximum of two months over-estimate of each employment period. This one-sided measurement error can be handled using a donut RD approach (see Dong, 2015 and the first chapter of this thesis where I deal with the same issue). However, it turns out that even with the donut, the first stage relationship is small, although significant. There are also jumps in several other characteristics at the cutoff, suggesting that the assumption of continuous potential outcomes at the cutoff is violated. This alternative estimation strategy is therefore not used in this study.

small region around the cutoff so that they are similar also in terms of age, minimizing the potential bias. The size of this region is a trade-off between precision and bias. If the treatment effect cannot be assumed to be homogeneous over age, the results found must be thought of as a local average treatment effect. The baseline bandwidth used in this study is one year, so that I compare individuals who are 39 versus 40 years of age[16]. The same bandwidth is used for the estimation of the first and second stage results, using a triangular kernel local linear regression model[17]. I use standard errors clustered on the distinct values of the forcing variable, as suggested by Card & Lee (2008).

Even though the age discontinuity is sharp, age alone does not determine treatment status. A number of other basic requirements must be met to be eligible for treatment. It is also a fact that not all eligible apply for the grant, which is most likely due to lack of information about its existence. I therefore use a fuzzy regression discontinuity design, which means that age over 40 is used as an instrument for treatment status. This also means that there is some overlap in age above the cutoff, which decreases the risk of bias.

For the estimation of all reported results, I include covariates for gender, years of education, marital status, number of children within the household, fixed effects for region of birth and parents region of birth, the number of years with income, mean wage earnings the last 5 years prior to notice, time in unemployment, local unemployment rate at the county level, the number of qualifying months of employment, and the order of termination. I also include fixed effects for year of termination and municipality of residence at notice. These fixed effects are included to come as close as possible to a natural experiment, where I compare individuals that are displaced in similar labor market conditions, i.e. in the same region at the same point in time. However, the inclusion of these covariates only marginally changes the estimates.

4.1 Data

I use data from AFA Insurance on workers that have received the lump-sum severance grant through the Employment Security Agreements 2006-2012. Data on displaced workers who have not received the grant come from the Swedish Public Employment Service (henceforth PES) and the TSL Em-

[16] There are some data-driven methods to find optimal bandwidth sizes. The optimal bandwidth size according to, for example, Imbens & Kalyanaraman (2012), varies greatly across the outcome variables in this study. The smallest bandwidth suggested is just above the one year bandwidth used. I use the conservative bandwidth of one year, but test the robustness of my results against smaller and larger bandwidths.

[17] The baseline is a triangular kernel local linear model. With covariates included in the fuzzy RD model, a predicted value of treatment lies outside the feasible range, and local mean smoothing is used to estimate the treatment discontinuity. Without covariates in the model, however, the results are unchanged.

ployment Security Fund. By law, Swedish employers must report notices to the PES if they involve at least five employees within a county at the same time or at least 20 employees over a 90-day period (1§ lagen (1974:13) om vissa anställningsfrämjande åtgärder). The data collected by the PES on these notifications include individual level data on what workers have been notified and from which firm, as well as information about whether the worker is blue- or white-collar. These data are combined with information provided by the TSL Employment Security Fund to construct a control group for the estimation. The data from TSL include information about all firms that have been affiliated with the Employment Security Agreement as well as which time period(s) they were affiliated. Together with the data from the PES, blue-collar workers given notice from these firms during the period of study are identified. The TSL data also include individual information about notified workers, including workers notified within smaller notifications than those reported to the PES.[18] The data only include workers receiving job search assistance through the Employment Security Agreement, which means that only workers with more than twelve months of tenure within the agreement are included. While this is not completely in line with the tenure eligibility criteria for the severance grant, it is likely that most workers that would be eligible for the severance grant according to criteria other than the age limit are included in this register.

However, as it turns out, there is a large number of workers within the data from AFA Insurance who are not found in the registers of notified workers from the PES and TSL. There is therefore a jump in the density of notified workers at the cutoff, only due to the additional data source used to identify treated workers. It also follows that there is a jump at the cutoff with respect to variables related to the other eligibility criteria, i.e. being rehired within three months and receiving outplacement services through the agreement, with the full sample (the density around the cutoff and reduced form analysis of characteristics for the full sample are found in *Figure A.1* and column 2 of Table A.1). Since this is related to the data collection process, it does not invalidate the empirical strategy per se. However, there is a risk that these treated individuals are systematically different in more ways, due to the different data collection processes of the different data sources, and I have therefore excluded all workers not in the PES or TSL registers from the baseline sample. This reduces the sample of treated by close to 32 percent.[19]

[18] This is because they are eligible for other benefits provided through the same Employment Security Agreement, which are administered by the TSL Employment Security Fund. (Specifically job search counselling, which is evaluated in the first chapter of this thesis).

[19] I have also only included individuals who appear once in the matched sample of notified workers from the three registers, or more than once but from the same data source, to ensure that individuals are not double counted once as treated and once as controls, due to misreporting of dismissal firm or –date, so that they are not correctly matched between the different data sources but is in fact the same event.

The results are, however, the same using the full sample when controlling for the two other eligibility criteria mentioned.

The resulting dataset is matched to register data, using unique individual and firm identifiers, which provides the full dataset with a rich set of background variables as well as information on the labor market outcomes studied. I study the effects of the grant on the probability of unemployment and unemployment duration. I define unemployment as receiving UI benefits at some point between the notification date[20] and three months after the notified termination date.[21] The unemployment duration is defined as the number of days between the first week with UI benefits payment and the last, allowing for gaps of a maximum of four weeks between payment periods. If no UI benefit is received during the window used, unemployment duration is zero. As treatment in this case can affect the probability of becoming unemployed, this outcome may be considered endogenous.

I also present result for a more direct measure of the job search duration, the non-employment duration, and the probability of non-employment, i.e. not finding a job before the old job ends. This measure is used previously in the literature (i.e. Card, Chetty & Weber, 2007 and Basten, Fagereng & Telle, 2014). Non-employment is measured as having a gap in employment periods, according to Swedish employment records. Employment periods and earnings must be reported by all employers for tax purposes, and I use this data to study the job finding rates. I have information about the precise proposed termination date, and the length of this gap is therefore measured in days, although the employment records contain monthly data. The employment is assumed to start the first day of the first employed month according to employment records.[22] If the new employment is found during the

[20] The notification date is not included in the data from the PES, and is therefore estimated for this group. I use the most common notification date according to the TSL register among those treated within the same notification.

[21] I allow for a maximum of three months gap following Jans (2002), who use notification data to investigate flows to unemployment following notifications. The argument is that workers may get some compensation from the employer that may postpone the first day of UI eligibility, or the employment may be extended for a limited period. Unlike Jans, I have access to notification dates and therefore allow unemployment to start from that date on.

[22] The first job is defined as an employment where the recorded income is at least SEK 10,000 (around USD 1,100). The monthly structure of the data on employment periods means that there is measurement error in employment periods if a worker has multiple employment periods with the same employer during the same calendar year. When no gap is observed in employment periods, and the worker continues working at the dismissal firm the following calendar year after the notified last day of employment, I interpret this as a rehire. The timing of the rehire decision is however unknown, which is a problem for the estimation of job finding rates and job duration. It might be during the period of notice, or thereafter but within the same calendar year. Using data from the PES on unemployment periods from enrollment periods and unemployment insurance payment periods, I have estimated alternative rehire dates based on ending dates from these records. An enrollment period ends when the worker is not registered as unemployed without employment according to unemployment categories, and when UI payment periods end for a period longer than four week. If the worker is not enrolled or receives UI payments between the notice and the next job according to employ-

notice period, the value of the non-employment duration is negative, to avoid endogeneity. However, setting this to zero does not affect the estimates.

Since there is a lot of misreporting in the Swedish employment records, and thus measurement error in the non-employment variables[23], I use this as a complement to the direct unemployment measure rather than exclusively investigating the effects on the non-employment probability and duration. Theoretically, these two outcomes could differ through dynamic effects on leaving the labor force. However, such dynamics are unlikely at the ages around the cutoff, since these workers are too young to flow into early retirement, and too old to i.e. go into education, to any significant extent. These two measures of the job finding rate are therefore expected to yield similar results[24]. The only expected source of discrepancies is therefore the presence of measurement error in the employment data.

I also investigate the effects of the severance grant on the quality of jobs found, measured as job duration and average monthly income. These outcomes are measured using the employment records described above, including earnings for each employment period reported. Duration of the first job found is measured as the number of months consecutively employed with the first employer after the notification date. The follow-up period extends to 2014. If the consecutive employment period is right censored, this outcome value is missing.

4.2 Descriptive statistics

Descriptive statistics of the baseline sample, as well as of a few subsamples, are presented in Table *1*. Subsamples include those within the baseline sample above the age of 39, i.e. those eligible for the severance grant with respect to the age criteria used for the estimation strategy, all individuals in the sample who have received the severance grant, and the sample close to the cutoff, workers aged 39 and 40 at the termination of the employment.

Comparing all individuals in the sample above the age of 39 to the sample of all treated workers, differences in terms of observed characteristics seem to be associated with the other eligibility criteria for treatment. The treated sample are less often rehired within three months, which is natural since a prerequisite to keep the grant is that the worker is not offered reemployment within 3 months from termination. The number of months employed at firms

ment records, or between the notice and the next calendar year after the last day of employment for rehires, they are assumed to not have become unemployed and reemployment happened during the period of notice. It turns out that the vast majority of rehires happens within the period of notice according to these calculations.

[23] Data is monthly but employers sometimes over-report the length of employment periods by checking the full-year box when the real employment period is actually not the full year.

[24] Card, Chetty & Weber (2007) find similar results using the unemployment duration and non-employment duration measure of the length of the job search period.

Table 1. *Descriptive statistics*

	Baseline sample	Baseline sample above 39	All treated	Close to cutoff
Age	39.81	51.42	52.05	40.01
	(12.81)	(7.31)	(7.35)	(0.61)
Months of qualifying employment	48.45	52.61	57.46	49.90
	(16.86)	(14.71)	(8.73)	(16.53)
No. of years with income	15.05	21.20	21.79	18.03
	(8.03)	(5.25)	(4.37)	(5.58)
Gender (1=Woman)	0.27	0.29	0.29	0.31
	(0.44)	(0.45)	(0.45)	(0.46)
Years of education	11.04	10.56	10.45	11.05
	(1.58)	(1.62)	(1.57)	(1.46)
Married	0.33	0.48	0.49	0.40
	(0.47)	(0.50)	(0.50)	(0.49)
Mean annual earnings five years before notification (SEK 100)	2,184.90	2,528.95	2,708.02	2,371.80
	(965.14)	(842.14)	(691.55)	(877.25)
Annual earnings one year before notification (SEK 100)	2,589.72	2,717.25	2,795.76	2,668.80
	(939.23)	(888.60)	(796.66)	(914.15)
No. of children in household below 18	0.63	0.59	0.56	1.34
	(0.97)	(0.96)	(0.94)	(1.19)
Days of unemployment	877.09	1,001.13	796.97	1,369.71
	(1,090.84)	(1,230.99)	(1,079.76)	(1,264.76)
Local unemployment rate (county level)	7.78	7.82	7.79	7.76
	(1.46)	(1.44)	(1.43)	(1.43)
Born in Sweden	0.81	0.79	0.81	0.76
	(0.39)	(0.41)	(0.40)	(0.42)
Rehired within three months	0.20	0.19	0.11	0.20
	(0.40)	(0.39)	(0.31)	(0.40)
Job search assistance through the ESA	0.83	0.86	0.92	0.86
	(0.37)	(0.35)	(0.26)	(0.35)
Share treated	0.29	0.62	1.00	0.29
	(0.46)	(0.49)	(0.00)	(0.46)
Grant amount (SEK)	31,577.13	31,584.50	31,577.13	27,301.80
	(6,189.23)	(6,192.41)	(6,189.23)	(2,409.34)
Grant replacement rate	–	–	1.64	1.51
			(9.23)	(2.85)
UI replacement rate	0.67	0.67	0.67	0.68
	(0.11)	(0.10)	(0.10)	(0.11)
Firm size	1,224.86	1,018.43	1,063.05	1,270.11
	(2,660.44)	(2,407.31)	(2,555.72)	(2,832.70)
No. of observations	158,965	75,350	46,822	7,758

affiliated with the agreement the five years preceding termination is larger among treated than among all displaced workers who meet the age requirement. Consequently, average income during these years is higher among the treated, while the income the year directly preceding the termination year is similar across these samples. Shorter time spent in unemployment among the treated could mirror the fact that they have longer qualifying time of employment. The fact that the treated are on average 0.6 years older than the full sample above the age of 39 cannot directly be explained by any eligibility criteria for treatment, but the difference is also not significant. In terms of observed characteristics, there is little evidence that workers receiving the severance grant differ systematically from eligible workers who did not apply for the grant.

Compared to all workers receiving the grant, those within a year of the age cutoff are much younger (12 years on average), less often married, and have almost one more child living in the household one average, paired with a lower average income. These things suggest that individuals close to the cutoff are more liquidity constrained than the average worker who gets the grant. They size of the grant, and the grant replacement rate, is however, naturally, lower at the cutoff than on average, since the grant size, as well as the replacement rate with respect to previous income, increases with age.

4.3 Validity of the regression discontinuity design

A few assumptions must be fulfilled if the regression discontinuity is to be a valid estimation strategy. First, individuals must not be able to exactly control the value of the forcing variable around the cutoff, thereby determining their treatment status. Treatment assignment must be independent of potential outcomes, i.e.:

$$(Y_{1i,} Y_{0i}) \perp T_i|\mathbf{X}_i \tag{10}$$

Y_1 denotes the potential outcome when treated and Y_0 the potential outcome when not, T_i denotes the treatment status, and $\mathbf{X}_i$ a set of predetermined characteristics (in the regression discontinuity case the forcing variable should be sufficient).

It is unlikely that workers can directly plan their notified last day of employment since the firm decides when to displace workers according to when redundancies occur and contractual notification periods etc. The firm, however, might manipulate the date of termination. The severance grant is not paid directly by the firm at termination, since it is financed collectively, which means that firms have no incentives to adjust termination dates or time of notice to avoid eligibility for the grant. Workers themselves apply for the grant, directly to AFA Insurance, who transfers the grant to workers who are eligible. Firms might however, on the margin, manipulate in the other direction so that workers are eligible, by postponing the termination date. There is no way of knowing whether this is the case. However, it can be tested by inspecting the density of displaced workers around the cutoff. *Figure 1* shows the density of workers in the baseline sample between the ages of 35 and 45. The bar width in the histogram is a quarter of a year. As shown in the figure, the density develops smoothly at the cutoff. The lack of discontinuity in the density at the cutoff is confirmed by the result of the McCrary density test, which delivers an insignificant estimate[25]. There is thus no evidence of manipulation of the forcing variable.

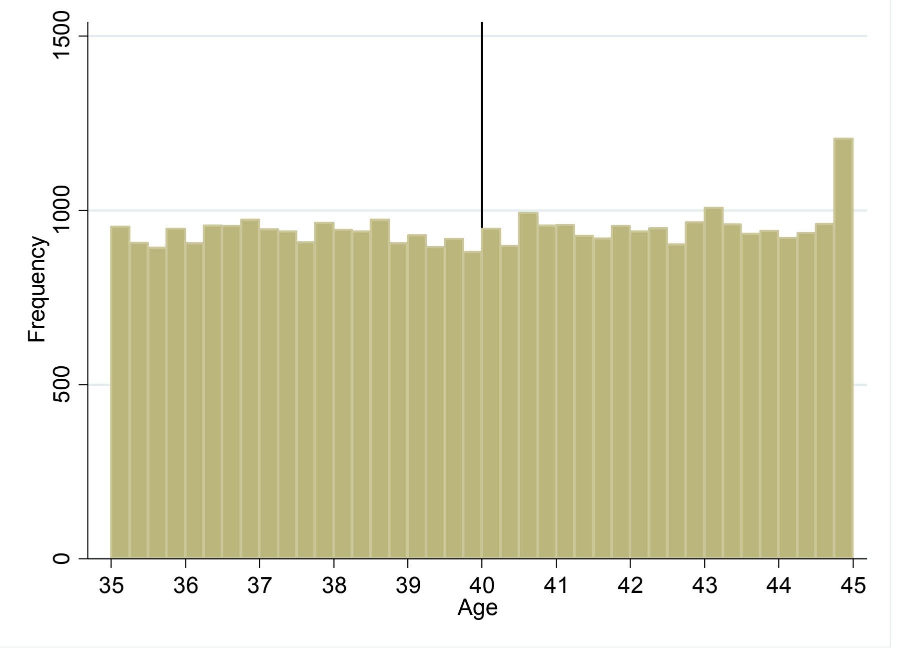


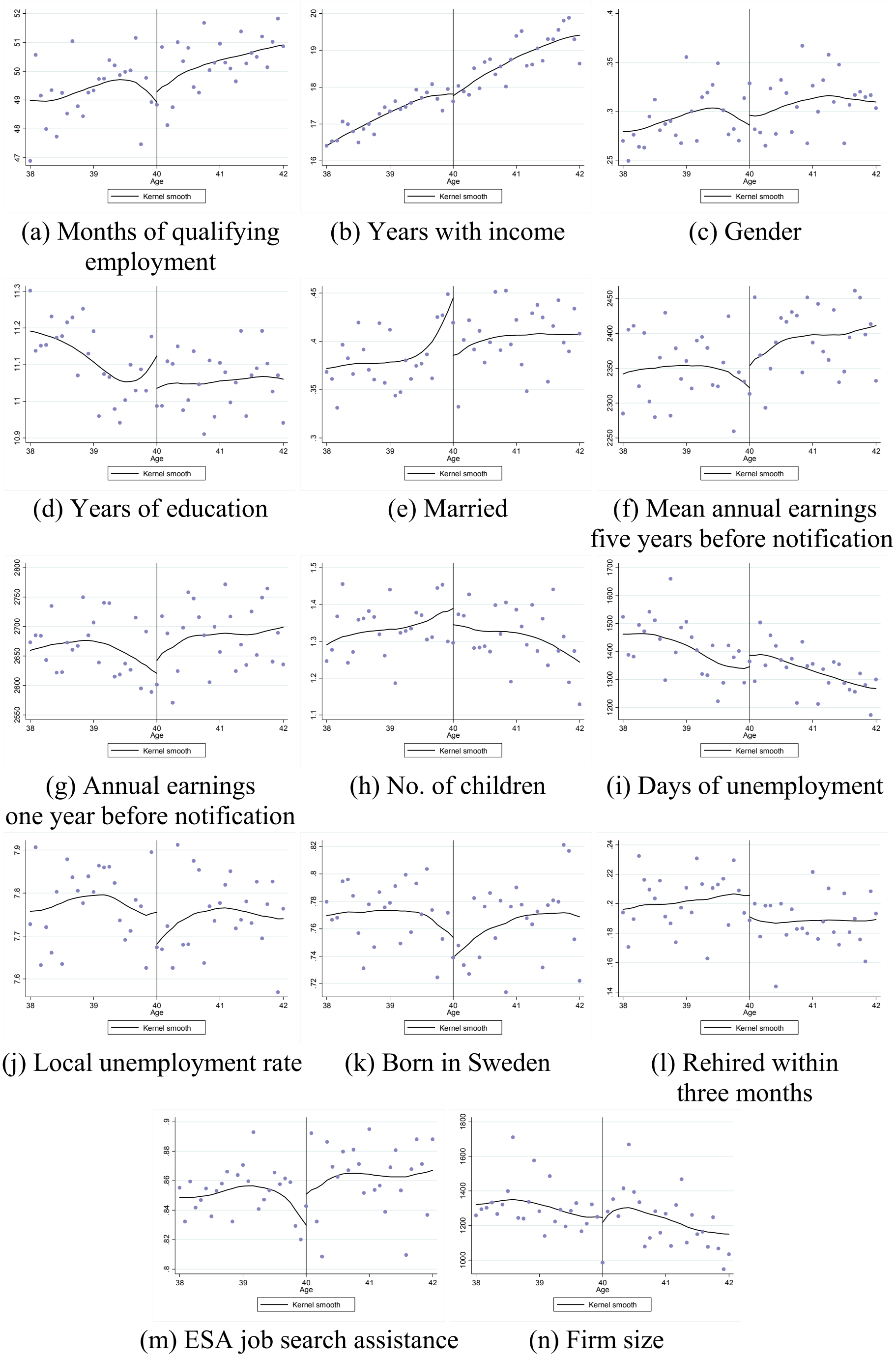
Figure 1. Distribution of displaced workers along the forcing variable

Another assumption needed for the validity of the estimation approach is that workers around the cutoff, on either side, are not systematically different in any other respects than treatment, so that the treatment assignment can be considered as if random at the cutoff. The assumption is formally that the expected values of the potential outcomes, given the forcing variable, are continuous at the cutoff, i.e:

$$E(Y_1|X_i) \text{ and } E(Y_0|X_i) \text{ are continuous at } X_i=x_0 \tag{11}$$

This assumption can be tested by investigation of how observables develop at the cutoff. If these are continuous at the cutoff, it is more likely that unobservables, and potential outcomes, are also continuous at the cutoff. I examine the continuity of observed characteristics at the cutoff by estimating the reduced form results for these variables using the same forcing variable and cutoff. These are presented in Table A.1. Graphical illustrations of the poten-

[25] A detailed description of this test is provided by McCrary (2008).The bin and bandwidth sizes used to perform the test is a quarter of a year and one year, respectively.



(a) Months of qualifying employment
(b) Years with income
(c) Gender
(d) Years of education
(e) Married
(f) Mean annual earnings five years before notification
(g) Annual earnings one year before notification
(h) No. of children
(i) Days of unemployment
(j) Local unemployment rate
(k) Born in Sweden
(l) Rehired within three months
(m) ESA job search assistance
(n) Firm size

Figure 2. Basic characteristics by age

tial discontinuities are shown in *Figure 2*. All characteristics are smooth at the cutoff, except one. There is a significant decrease in the share married at the cutoff. There is no obvious explanation for this. However, when testing multiple variables it is possible that some estimates are significant even by chance. In the estimation of results, I control for being married.

In addition to the assumptions discussed above, to estimate effects using a fuzzy RD, a prerequisite is that the first stage relationship is strong, i.e. that the probability of treatment is discontinuous at the cutoff.[26] The first stage relationship is determined by the reduced form estimate of the jump in treatment status at the cutoff. The result is presented in Table 2. The first stage relationship is strong and significant. The probability of treatment increases by 42 percent as the age threshold of 40 years is crossed.

Table 2. *First stage relationship*

	(1)
Probability of treatment	0.417***
	(0.042)

Note: Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively. The number of observations is 7,476 within the bandwidth.

Figure 3 graphically illustrates the first stage relationship as the jump in the share of treated at the cutoff. I use monthly birth data, and the figure reveals that the age requirement is quite strict; few workers who have not actually turned 40 at termination according to the data receive the severance grant.

Since I do not have data on all determinants of eligibility for the grant, I cannot determine the precise take up rate among eligible workers. According to *Figure 3*, the take up rate is around 60 percent, but some workers above the age of 40 do not take up the grant because they do not meet some of the other eligibility criteria. There is however an ongoing discussion about the fact that the take up rate for the grant is low and that many eligible workers do not apply for the grant. The explanation for this is likely a lack of information among workers, about this and other benefits stipulated within the collective agreement. The effort to apply for the grant is small and the amount that would be received is non-negligible, and there should be no stigma associated with receiving the grant. It is more likely that those that have information about the grant are workers displaced from larger firms

[26] A related assumption needed for validity of the fuzzy RD design is monotonicity, which means that workers do not receive the severance grant when they are below 40 years of age, but would not receive it if they were above the cutoff. If so, these so called defiers would counteract the effect of the compliers, those who receive treatment because they meet the age requirement, so that the estimated results are not the true treatment effect. From the nature of the treatment and the fact that the age requirement is difficult to manipulate, e.g. because of the widespread use of the Swedish personal identifying numbers, the presence of defiers is unlikely.

with an HR-department that is familiar with all parts of the collective agreement, or through large layoffs were it is more likely that an information drive by the providers of this grant and other collectively agreed benefits takes place.

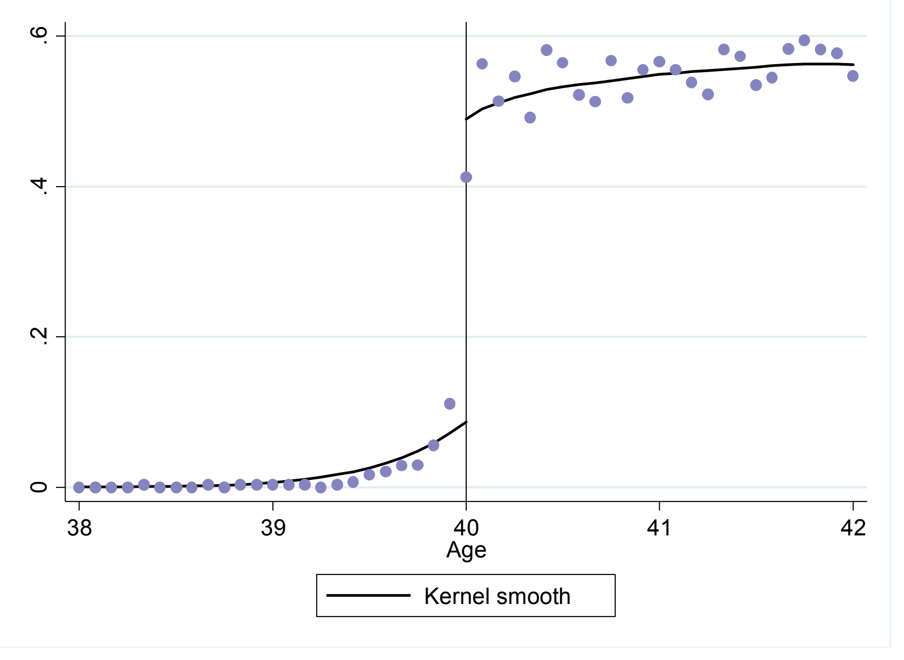


Figure 3. Probability of treatment by age

5 Results

I study the effect of the lump-sum severance grant, provided by a Swedish Employment Security Agreement for blue-collar workers, on unemployment and non-employment duration and the quality of subsequent matches in terms of average monthly income the first year in the new job and job duration. As the severance grant is not dependent on unemployment status, I study the probability of becoming unemployed (and non-employed) as a part of the effect on the duration. Graphical illustrations of the effect on the outcomes are found in *Figure A.2*. Table 3 shows the main results.

The results from the reduced form model in column 1 and the fuzzy RD model in column 2 both show that there is no significant effect on the non-employment probability or duration. There is a significantly positive effect on the unemployment probability and a weakly significant positive effect on the unemployment duration. This is in line with previous findings that lump-sum severance grants decrease search effort and prolong unemployment. The point estimate suggests that the lump-sum grant increases the unemployment duration by around one month on average.[27] This result is similar to the findings from Norway, where the lump-sum grant was found to prolong non-employment duration by between 37 and 41 days. The estimated effect on the non-employment probability is also positive, although not significant, while the point estimate for the effect on the completed non-employment duration is even negative, contrary to previous findings. The conclusions are unchanged if covariates are not included in the analysis.[28] As no differences are expected between these outcomes, the only potential source for these observed differences coming to mind is measurement error with respect to the non-employment measure. The differences could, however, also be explained by an effect of the grant on staying in the labor force. The analysis below, showing the timing of the effects following job termination, shows that these two measures yield more similar results at the start of the job search period than suggested by the estimates of the completed duration in Table 3.

Table 3. *Main results on job finding*

Outcome	(1) RF	(2) FRD
Probability of unemployment	0.062***	0.125***
	(0.011)	(0.023)
Unemployment duration	15.301*	30.691*
	(8.205)	(16.466)
Probability of non-employment	0.022	0.044
	(0.019)	(0.037)
Non-employment duration	-2.985	-5.988
	(21.144)	(41.471)

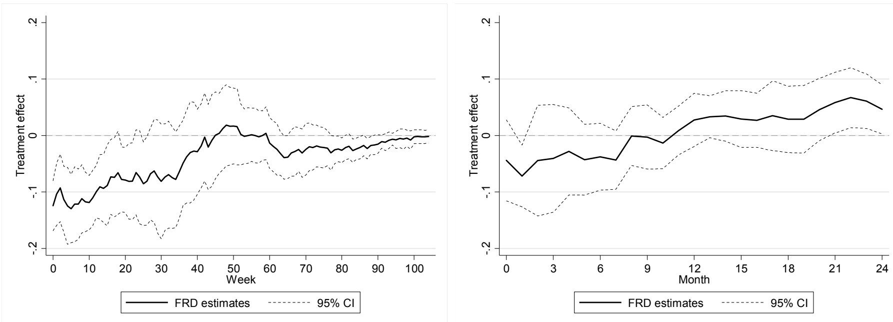
Note: Each cell represents the result from a separate regression, with each row showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively. The number of observations is 7,476 within the bandwidth.

90 percent of the baseline sample finds new employment within the follow up period (while only 68 percent of these, or 65 percent of the sample in total, also end this employment, making it possible to observe the completed job duration). There is thus a right censoring problem with respect to both

[27] Using enrollment at the PES instead of UI receipt to measure the probability and duration of unemployment yields the same conclusions as above. Unemployment is then as being registered as unemployed at the PES starting between the notification date and three months after the notified termination date. Unemployment duration is measured as the length of the first such spell, and zero if no unemployment is registered. If the spell does not end within the follow up period the value of unemployment duration is missing.

[28] The point estimates for the effect on the probability of non-employment and unemployment duration are marginally affected, the estimate for the effect on the probability of unemployment is somewhat larger and strongly significant, while the estimate for the effect on non-employment duration is more negative but insignificant.

the completed non- and unemployment durations (and even more so for the completed job duration). To use all the available data, I have also estimated the effect on non-employment and unemployment duration, or rather the job finding rate, by considering the effect on finding employment within 0-24 months and 0-104 weeks (for which there is less censoring), in separate regressions, using the UI benefit periods and the employment records in (a) and (b), respectively. The results are shown graphically in *Figure 4*.



(a) UI data (b) Employment records

Figure 4. Results on the job finding rate

These figures show that the effect on job finding is more similar using these two measures, at least during the UI eligibility period, than suggested by the estimates of the effect on the average completed durations. There is a negative effect on job finding according to unemployment spells, which is strongest in the beginning of the period and slowly fades over the initial 40 weeks or so of the unemployment spell. This evolution of the effect over the spell is in line with the expectation that the duration of unemployment is prolonged by the receipt of the grant until depletion of the liquid assets. The estimates for the effect on the job finding rate according to the non-employment measure is smaller and only significant the first month after termination. The estimated effect on job finding rates fades as the unemployment spell elapses, and the effect even becomes positive after about a year according to employment records. This clarifies how there is a negative estimate for the completed non-employment duration even though there is an initial negative effect on the job finding rates. The theory about the liquidity effect does not explain why the effect changes sign and becomes positive as the spell elapses.

Both measures suggest that the negative effect on the job finding rate is strongest in the beginning and emerges even during the notice period, as suggested by the positive effect on the probability of becoming unemployed, of 12.5 percentage points according to the point estimate in Table 3. This suggests that there is an anticipation effect even before unemployment starts; workers who know that they will receive the grant are less desperate to find a job quickly to avoid unemployment, because of the anticipated receipt of the grant once unemployment starts.

Table 4. *Main results on job quality*

Outcome	(1) RF	(2) FRD
Duration of first job, months	0.763	1.864
	(1.128)	(2.759)
Average monthly income at first new job	-234.489	-470.146
	(1678.606)	(3279.766)

Note: Each cell represents the result from a separate regression, with each row showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively. The number of observations is 7,476 within the bandwidth.

I also study the effect on the quality of jobs found. The results are shown in Table 4. The point estimate for the effect on job duration is positive, but not significant. To account for the censoring problem, I have estimated the effect on the first job lasting at least 2-24 months, in separate regressions. These results are shown in *Figure 5*. The figure shows that there is no significant effect on jobs lasting any of these durations. There is also no significant effect on the average monthly income during the first year in the first job found. The point estimate is small and negative. Without the inclusion of covariates, the point estimate for the effect on job duration is virtually unchanged while the estimate for the effect on average income is small and insignificant but positive.

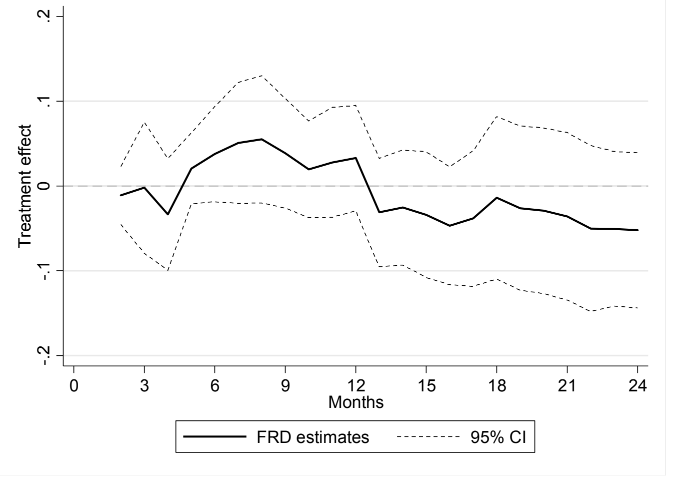


Figure 5. Results on leastwise job duration

5.1 Robustness analysis

As previously mentioned, I have excluded a number of treated workers from the sample since they do not appear in the PES or TSL registers of notified workers, and might therefore differ systematically from those individuals used as control units. If these individuals are included in the estimation of the results, while adding controls for the other eligibility criteria that I have data on, the results are unchanged. The results are shown in Table A.2. Estimates are very close to the baseline estimates, and the estimate for the effect on the unemployment duration is highly significant using the full sample. The estimate for the effect on job duration is closer to zero, and the estimate for the effect on income is positive but close to zero. Neither of these estimates are significant. The corresponding result for the full sample to those shown in *Figure 4* and *Figure 5* for the baseline sample, presented in *Figure A.3*, are very similar to the baseline results.

The expected effects of the severance grant depends on when the payment is made. Workers can apply for (and receive) the grant up to two years after termination, but most workers apply for and receive the grant closely after the termination date. As another robustness check, I estimate the results including only workers who receive the grant within one month from the termination date in the treated sample. This is true for around 70 percent of the baseline sample. The results are shown in Table A.3. The estimates are similar to those using the baseline sample, especially the effect for the unemployment probability and duration.

To ensure that my results are not driven by the choice of bandwidth, I have estimated the same models using a bandwidth of half and twice the size of my baseline bandwidth. The reduced form and fuzzy RD results are presented in Table A.4, together with my baseline results using the bandwidth of one year. There is no significant effect on the non-employment probability or duration with a larger, or smaller, bandwidth, but the estimated negative duration effect is stronger with a smaller bandwidth. The estimate for the effect on the unemployment probability is significant irrespective of the bandwidth used and stronger with a smaller bandwidth of half a year while marginally smaller using the two year bandwidth. The weakly significant effect on completed unemployment duration is positive with all three bandwidths but not significant with the smaller or larger bandwidths. There are no significant effects on job quality, irrespective of the bandwidth used. *Figure A.4,* showing the evolution in the effect over time, shows the same pattern.

Another concern about the validity of my conclusions is that the effects found are simply an age effect, which could be the case if the control for age, close to the cutoff, is not sufficient. As a sensitivity analysis, I have estimated the same model using a number of other age discontinuities where there is no discontinuity in treatment. The reduced form result for cutoffs at ages 35-

45 are shown in Table A.5. For age cutoffs below 40 there is no treatment. For age cutoffs above 40, there is no discontinuity in treatment, as the grant amount is the same between ages 40 and 49. This analysis shows that there is no jump in treatment at any other threshold. It also shows that the results are not driven by a systematic age effect.[29]

Within some collective agreements, that may apply to some of the workers in my sample, it is stipulated that the notice period is extended for workers above the age of 40. If this is the case, the exclusion restriction would be violated, if notice periods were significantly longer for those above the cutoff. To test whether this is the case, I use information on notice periods given by the difference between the notification date and the proposed termination date within the TSL register.[30] The reduced form estimates in Table A.6 show that there is a small but significant jump at the cutoff, but not in the expected direction. Notice periods are on average seven days shorter above the cutoff according to the data. It is unlikely that this difference would have produced the effects found.

5.2 The role of liquidity and other factors

As I have shown above, the lump-sum severance grant has a causal effect on the probability to become unemployed and on the unemployment duration. If this effect is truly due to liquidity constraints, the effect should be stronger among workers who have less liquidity. To investigate this further, I have divided the sample into different subgroups to study whether the effect differs between workers who are more or less liquidity constrained. The results from the analyses are found in Tables A.7-13. I take the relation between the estimates across subgroups as indicative evidence of differences, but the differences should be interpreted with caution since the sample size close to the cutoff can become small when the sample is divided into different subgroups, and effects are not necessarily significantly different between subgroups.

There is no direct measure of liquidity constraints. An indicator used by Basten, Fagereng & Telle (2014) is household holdings. This information is not available in my dataset. Instead, I use a number of other indicators to proxy liquidity constrained households. One such indicator is capital income. Information about this variable is available on a calendar year basis, and I use capital income the year before notification to separate workers into three

[29] A significantly negative effect is found for unemployment duration at age 38, while there are significant effects with respect to non-employment durations at age thresholds 41 and 43, however. Why these estimates are strongly significant is puzzling, but they are most likely random effects. There might of course be some other policy discontinuities at these thresholds that affect the outcomes, but I am not aware of any such policies.

[30] Since notification dates are estimated for the workers added to the sample through the PES register, I don’t use these for this test.

groups. I estimate the effects separately for workers with non-negative capital income and workers with capital income above and below the median negative capital income. The results in Table A.7 does not show the expected pattern; that individuals with higher capital income are less liquidity constrained and therefore respond less to the grant. Instead, the relatively small group with non-negative capital income has a positive and significant response with respect to unemployment probability, while the effect for workers with negative capital income is not significant, and estimates are smaller or even negative.

Another indicator of liquidity constraints is family disposable income, calculated by Statistics Sweden. I use the difference between family disposable income and the workers individual disposable income the year before notification. The results are estimated separately for individuals with low relative family disposable income; workers for whom family disposable income is equal to or lower than the individuals' disposable income, and workers who have family disposable income higher than the individual disposable income, separated by the median level among these. The results are presented in Table A.8. This analysis reveals that relative family disposable income matters for the effect of the grant. While there is small positive, but insignificant, estimates for the effect on the probability of unemployment, and small negative and insignificant estimates for the effect on unemployment duration for workers with high and medium family disposable income, the effect on both these estimates are strongly positive and significant for workers with low family disposable income. The estimate for the effect on the probability of non-employment is also largest for these workers and weakly significant. This implies that the family situation matters for the response to the severance grant. Having a spouse with income provides extra consumption smoothing opportunities for unemployed workers, and the results suggest that having high relative family disposable income decreases the effects of the grant.

There is, however, no suggestion from the results for groups with different income levels that the effect is greater with a smaller UI replacement rate. Due to the cap in the UI system, workers with higher previous income will have a lower replacement rate of unemployment benefits. In Table A.9 the results are shown separately for workers with different replacement rates.[31] For instance, while the group with the lowest replacement rates of up to 60 percent exhibits a significantly positive effect on the unemployment probability and duration, the effect for the workers who get the maximum replacement rate is stronger with respect to the probability of becoming unemployed and the estimate is similar for the effect on unemployment duration (although not significant). The effect from the severance grant does not

[31] The results are the same when focusing on those who actually become unemployed, using the actual replacement rate instead of income the year before to separate groups.

seem to depend on the size of the grant in relation to previous income either. The analysis presented in Table A.10, which separates the effects according to the grant replacement rate, does not show the expected pattern, that a higher grant replacement rate yields a more positive effect on unemployment duration. Instead, the effects for those with a high and low replacement rate are more similar, while the response is smaller (or even negative with respect to the non-employment duration) in the group with a medium replacement rate, according to the estimates.

I have also investigated whether the response differs with respect to education level or gender. The results, presented in Table A.11 and A.12, suggest that the effect is stronger for women. The effect on unemployment is similar among workers with compulsory and high school education, while not present within the small group with tertiary education.

The effect of a severance grant might also depend on the state of the labor market. I use data for a number of years that span varying stages of the business cycle. The labor market was bleak, especially for blue-collar workers, in the financial crisis that emerged in 2008-2009. If the supply of available jobs is low the effect of the severance grant on unemployment duration might be lower than if labor market conditions are good. I have therefore investigated how the results depend on the time of the termination. The effects for terminations up until 2009 are compared to the effects when the termination was made after 2009. The results in Table A.13 suggest that the effect is stronger after 2009 than before. The effect for workers displaced in 2010-2012 is positively significant for both unemployment and non-employment probabilities and durations. The size of the estimates are also similar between the two outcome measures within this sample, while workers displaced in 2006-2009 only exhibit a much smaller significant effect for the probability of unemployment and an insignificant estimate for the duration, and no significant effect on non-employment outcomes. This could indicate that those workers displaced before 2009, most during the financial crisis, faced worse job finding opportunities overall and that the smaller positive effect on unemployment for this group reflects a negative effect from receiving the grant on leaving the labor market during this period, while the stronger effect on unemployment after 2009 is primarily due to a the negative re-employment effect. It is reasonable that the potential effect is greater when labor market conditions are more favorable, and this may be reflected by these results.

6 Conclusions

Unemployment benefits help workers smooth consumption in the event of a negative income chock due to unemployment. The positive relationship between the unemployment benefit level and unemployment duration, established in the economic literature, can be separated into two potential sources; a moral hazard effect, caused by the change in the relative price of leisure, and a liquidity effect, pertaining to the increased ability to smooth consumption. While the former causes deadweight losses, the latter is a socially optimal response to the mending of credit and insurance market failures.

These sources are difficult to separate empirically. The social optimality of an unemployment insurance policy can, however, be evaluated by studying the effect of a non-distortionary lump-sum severance grant on unemployment duration. In this study, I evaluate the effects of a lump-sum severance grant provided to workers dismissed due to redundancy through a collective agreement which covers the majority of Swedish blue-collar workers. I use a fuzzy regression discontinuity design which utilizes the fact that there is an age requirement to be eligible for the severance grant. I find that the lump-sum grant has a positive effect on the probability of becoming unemployed and the length of the completed unemployment duration, while this effect is less evident for the non-employment outcome. There is an initial positive effect on both unemployment and non-employment probabilities, although only significant for the former, that diminishes over time and is zero around 8-9 months from the termination date. The difference could reflect the fact that there is measurement error in the latter measure of job search duration, causing noisy estimates, although dynamics through an effect on staying in the labor force, while unlikely at ages for which the effect is estimated, cannot be ruled out. I find no effect on subsequent job quality in terms of job duration or average monthly income the first year in the new job. Within the sample used to estimate these effects, the average replacement rate is 68 percent. This is higher than in the countries where lump-sum severance grants of a similar magnitude have previously been studied. The effect on job finding is strongest early on, during the notice period, and decreases thereafter. This suggests that there is an anticipation effect and that workers who will receive the grant after the termination date are less desperate to avoid unemployment, and that the effect fades as the grant is depleted.

My analysis also shows that spousal income seems to affect the consumption smoothing opportunities of the unemployed and matters for the effect of the grant. I do not find any significant effects for workers whose family disposable income is higher than the individual disposable income. The UI replacement rate does not seem to be directly decisive to the effects of the grant. This suggests that the level of unemployment benefits may not be above the optimal level for groups with limited opportunities to smooth consumption, while it may be too high for other workers who are less liquidity constrained, due to, e.g., spousal income that helps smooth consumption. The effect is stronger for workers displaced after 2009 than before, suggesting that the effect of this type of grant is larger in times of more favorable labor market conditions.

To draw any final conclusions about the relationship between moral hazard and liquidity effects within the unemployment insurance and the optimal level of unemployment benefits, a combined analysis of the dynamics of the duration effect of the benefit level and this type of severance grant is needed. This is left to future research.

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Appendix

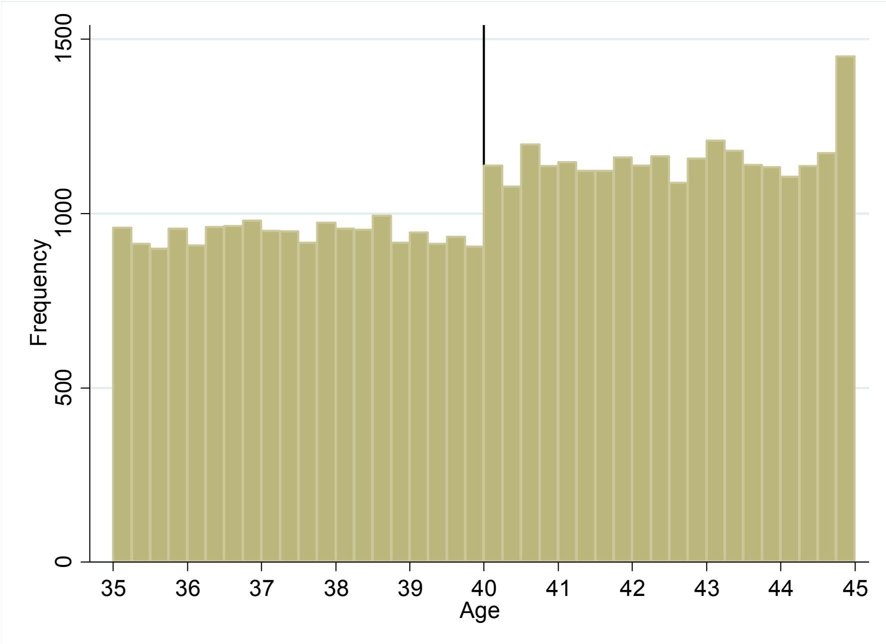
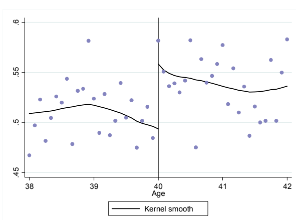
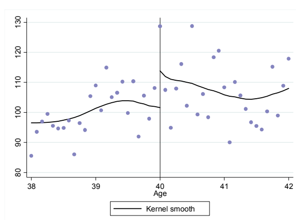


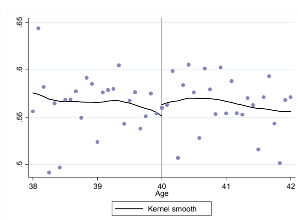
Figure A.1. Distribution of displaced workers along the forcing variable, full sample



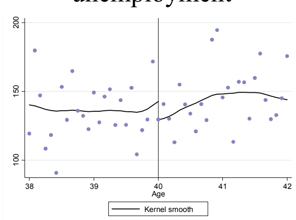
(a) Probability of unemployment



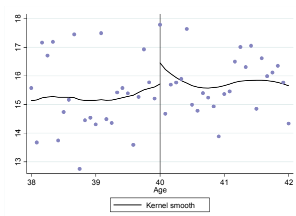
(b) Unemployment duration



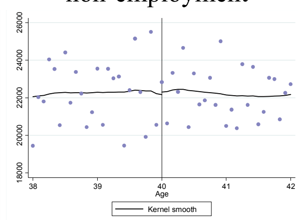
(c) Probability of non-employment



(d) Non-employment duration

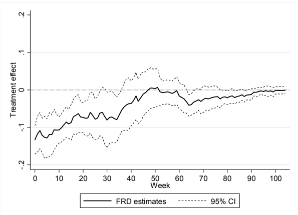


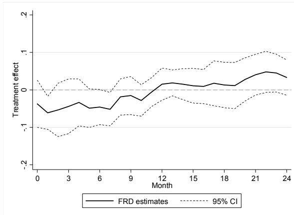
(e) Duration of first job

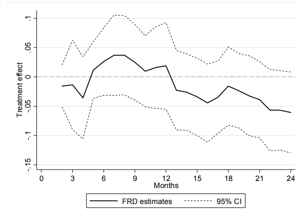


(f) Average monthly income at first new job

Figure A.2. Outcomes by age





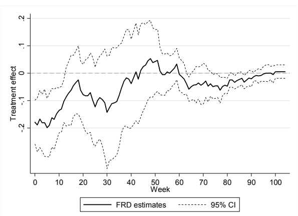


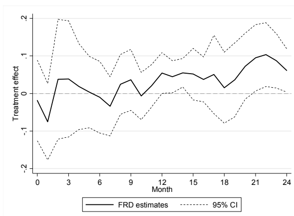
(a) Job finding rate, UI data

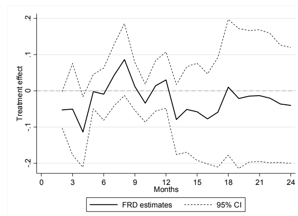
(b) Job finding rate, employment records

(c) Leastwise job duration

Figure A.3. Results on the job finding rate and job duration, full sample





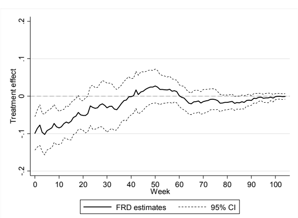


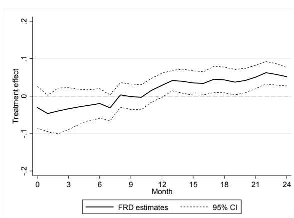
(a) Job finding rate, UI data

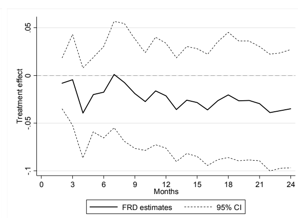
(b) Job finding rate, employment records

(c) Leastwise job duration

Bandwidth a-c: 6 months







(d) Job finding rate, UI data

(e) Job finding rate, employment records

(f) Leastwise job duration

Bandwidth e-f: 2 years

Figure A.4. Job finding rates and job duration using different bandwidths

Table A.1. *Reduced form estimates of basic characteristics*

Outcome	(1) Baseline sample	(2) Full sample
Months of qualifying employment	0.367	0.800
	(0.858)	(0.868)
No. of years with income	-0.049	0.132
	(0.209)	(0.157)
Gender (1=Woman)	0.010	0.006
	(0.023)	(0.028)
Years of education	-0.088	-0.129**
	(0.055)	(0.063)
Married	-0.060**	-0.061***
	(0.026)	(0.022)
Mean annual earnings five years before notice (SEK 100)	31.605	38.107
	(43.656)	(42.440)
Annual earnings one year before notice (SEK 100)	21.779	4.100
	(46.670)	(48.884)
No. of children in household below 18	-0.045	-0.037
	(0.062)	(0.056)
Days of unemployment	40.379	45.658
	(57.508)	(56.238)
Local unemployment rate (county level)	-0.074	-0.086
	(0.089)	(0.079)
Born in Sweden	-0.015	0.006
	(0.015)	(0.012)
Rehired within three months	-0.015	-0.031***
	(0.011)	(0.011)
Job search assistance through the ESA	0.021	-0.094***
	(0.018)	(0.016)
Firm size	-29.137	-75.005
	(109.351)	(99.420)
No. of observations	7,758	8,664

Table A.2. *Results, full sample*

Outcome	(1) RF	(2) FRD
Probability of unemployment	0.071***	0.134***
	(0.010)	(0.019)
Unemployment duration	16.293***	30.515***
	(5.984)	(11.191)
Probability of non-employment	0.020	0.037
	(0.018)	(0.032)
Non-employment duration	-5.871	-11.040
	(21.485)	(39.686)
Duration of first job, months	0.161	0.297
	(1.047)	(1.877)
Average monthly income at first new job	73.634	138.381
	(1652.159)	(3037.963)
First stage relationship	0.460***	
	(0.040)	

Note: Each cell represents the result from a separate regression, with each row showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively. The number of observations is 8,377 within the bandwidth.

Table A.3. *Results, sample with payment within one month from termination*

Outcome	(1) RF	(2) FRD
Probability of unemployment	0.043***	0.120***
	(0.011)	(0.035)
Unemployment duration	10.380	29.171
	(7.791)	(22.999)
Probability of non-employment	0.010	0.027
	(0.019)	(0.052)
Non-employment duration	-8.216	-23.092
	(22.036)	(60.438)
Duration of first job, months	0.394	0.892
	(0.980)	(2.151)
Average monthly income at first new job	556.682	1307.647
	(1829.617)	(4195.078)

Note: Each cell represents the result from a separate regression, with each row showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively. The number of observations is 6,805 within the bandwidth.

Table A.4. *Results using different bandwidths*

Outcome	Bandwidth: 0.5 years		Bandwidth: 1 year		Bandwidth: 2 years	
	(1)	(2)	(3)	(4)	(5)	(6)
	RF	FRD	RF	FRD	RF	FRD
Probability of unemployment	0.061***	0.179***	0.062***	0.125***	0.052***	0.100***
	(0.015)	(0.041)	(0.011)	(0.023)	(0.012)	(0.023)
Unemployment duration	12.501	36.489	15.301*	30.691*	8.871	16.972
	(10.356)	(30.125)	(8.205)	(16.466)	(5.998)	(11.536)
Probability of non-employment	0.006	0.019	0.022	0.044	0.016	0.030
	(0.020)	(0.055)	(0.019)	(0.037)	(0.015)	(0.029)
Non-employment duration	-36.884	-106.043	-2.985	-5.988	-3.984	-7.618
	(23.905)	(69.400)	(21.144)	(41.471)	(12.783)	(24.184)
Duration of first job, months	0.053	0.148	0.763	1.864	0.653	1.212
	(1.457)	(3.810)	(1.128)	(2.759)	(0.800)	(1.466)
Average monthly income at first new job	-238.691	-684.069	-234.489	-470.146	213.268	407.702
	(2241.000)	(6057.580)	(1678.606)	(3279.766)	(1127.083)	(2130.600)
First stage relationship	0.343***		0.417***		0.472***	
	(0.044)		(0.042)		(0.030)	
Observations	3,834		7,476		14,680	

Note: Each cell represents the result from a separate regression, with each row showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Table A.5. *Results, placebo cutoffs*

Age threshold	First stage relationship	Probability of unemployment	Unemployment duration	Probability of non-employment	Non-employment duration
35	0.000	-0.016	-0.866	0.046	18.897
	(0.000)	(0.023)	(6.357)	(0.031)	(16.159)
36	0.000	-0.028	-4.572	0.018	-12.717
	(0.000)	(0.024)	(9.337)	(0.019)	(13.611)
37	0.000	-0.015	-0.720	0.020	16.220
	(0.000)	(0.014)	(6.679)	(0.026)	(21.922)
38	-0.000	-0.020	-14.038***	-0.041	-19.907
	(0.000)	(0.017)	(4.578)	(0.035)	(19.305)
39	-0.008*	-0.031	10.677	-0.011	6.535
	(0.005)	(0.020)	(6.934)	(0.018)	(15.561)
40	**0.417*** **	**0.062*** **	**15.301***	**0.022**	**-2.985**
	(0.042)	**(0.011)**	**(8.205)**	**(0.019)**	**(21.144)**
41	0.004	-0.000	-11.902	-0.027	-37.767***
	(0.013)	(0.020)	(7.474)	(0.020)	(15.255)
42	-0.026*	0.008	1.714	0.001	5.872
	(0.013)	(0.021)	(5.345)	(0.017)	(6.850)
43	0.004	-0.004	-0.319	0.016	46.035***
	(0.013)	(0.025)	(4.669)	(0.017)	(9.285)
44	0.009	0.006	-7.840	0.011	9.785
	(0.019)	(0.027)	(7.522)	(0.020)	(12.370)
45	-0.000	-0.007	-2.369	-0.022	-19.009
	(0.013)	(0.025)	(4.948)	(0.020)	(12.642)

Note: Each cell represents the result from a separate regression, with each column showing the reduced form results for a separate outcome using a separate age threshold. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Table A.6. *Reduced form results, notice periods*

Outcome	(1)
Length of notice period	-6.879**
	(3.059)

Note: Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively. The number of observations is 6,107 within the bandwidth.

Table A.7. *Result by subgroups, capital income*

Outcome	Positive		Above		Below	
	(1) RF	(2) FRD	(3) RF	(4) FRD	(5) RF	(6) FRD
Probability of unemployment	0.108***	0.272**	0.036	0.073	0.048	0.090
	(0.039)	(0.117)	(0.052)	(0.101)	(0.038)	(0.065)
Unemployment duration	2.583	6.481	16.073	32.826	13.933***	26.070***
	(19.638)	(44.113)	(15.045)	(29.538)	(5.217)	(9.222)
Probability of non-employment	-0.037	-0.093	0.016	0.032	0.050	0.093
	(0.049)	(0.105)	(0.020)	(0.038)	(0.037)	(0.066)
Non-employment duration	49.025	105.566	-25.237	-51.804	-2.177	-4.093
	(59.333)	(115.428)	(24.258)	(47.316)	(18.202)	(32.241)
First stage relationship	0.446***		0.447***		0.448***	
	(0.040)		(0.035)		(0.042)	
Observations	1,424		2,986		3,066	

Note: Each cell represents the result from a separate regression, with each row showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Table A.8. *Result by subgroups, relative family disposable income*

	High		Medium		Low	
Outcome	(1) RF	(2) FRD	(3) RF	(4) FRD	(5) RF	(6) FRD
Probability of unemployment	0.005	0.014	0.017	0.032	0.102***	0.214***
	(0.033)	(0.082)	(0.042)	(0.073)	(0.022)	(0.038)
Unemployment duration	-1.879	-5.220	-8.648	-16.304	26.733***	55.864***
	(14.999)	(37.930)	(17.576)	(29.648)	(8.588)	(16.214)
Probability of non-employment	-0.057	-0.158	0.033	0.063	0.052	0.108*
	(0.053)	(0.135)	(0.065)	(0.112)	(0.032)	(0.063)
Non-employment duration	-30.265	-85.092	11.628	21.877	-0.448	-0.931
	(22.757)	(66.404)	(50.789)	(86.367)	(20.371)	(40.504)
First stage relationship	0.378***		0.479***		0.446***	
	(0.061)		(0.035)		(0.040)	
Observations	1,770		1,696		4,010	

Note: Each cell represents the result from a separate regression, with each row showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Table A.9. *Result by subgroups, UI replacement rate*

Outcome	<60%		60-70%		70-80%		80%	
	(1) RF	(2) FRD	(3) RF	(4) FRD	(5) RF	(6) FRD	(7) RF	(8) FRD
Probability of unemployment	0.075***	0.136***	0.034	0.067	0.043	0.090	0.120**	0.346***
	(0.030)	(0.052)	(0.044)	(0.076)	(0.099)	(0.175)	(0.052)	(0.146)
Unemployment duration	29.096***	52.680***	21.886	42.540	-30.144	-62.225	17.629	51.043
	(12.136)	(21.821)	(23.573)	(41.464)	(36.862)	(61.424)	(20.915)	(58.096)
Probability of non-employment	0.038	0.069	0.088	0.170	0.105	0.216	-0.042	-0.122
	(0.035)	(0.060)	(0.064)	(0.112)	(0.086)	(0.160)	(0.056)	(0.150)
Non-employment duration	7.087	12.857	-17.852	-40.562	106.590*	221.971*	-17.188	-43.103
	(14.769)	(24.942)	(44.557)	(93.081)	(56.187)	(113.385)	(66.699)	(149.888)
First stage relationship	0.506***		0.471***		0.432***		0.365***	
	(0.034)		(0.034)		(0.068)		(0.033)	
Observations	2,908		1,873		1,027		1,668	

Note: Each cell represents the result from a separate regression, with each row showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Table A.10. *Result by subgroups, AGB replacement rate*

Outcome	High (1) RF	High (2) FRD	Medium (3) RF	Medium (4) FRD	Low (5) RF	Low (6) FRD
Probability of unemployment	0.084***	0.279***	0.001	0.002	0.131***	0.206***
	(0.023)	(0.088)	(0.034)	(0.052)	(0.033)	(0.045)
Unemployment duration	12.068	40.178	5.166	8.497	49.838***	78.525***
	(16.680)	(54.834)	(9.657)	(14.678)	(10.451)	(16.555)
Probability of non-employment	0.076*	0.254*	-0.080***	-0.132***	0.075	0.118*
	(0.041)	(0.134)	(0.033)	(0.049)	(0.049)	(0.071)
Non-employment duration	21.657	72.731	-46.564***	-77.560***	36.994	58.108*
	(38.145)	(122.300)	(16.703)	(26.529)	(22.630)	(31.142)
First stage relationship	0.314***		0.531***		0.583***	
	(0.028)		(0.041)		(0.060)	
Observations	3,376		2,309		1,791	

Note: Each cell represents the result from a separate regression, with each row showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Table A.11. *Result by subgroups, educational attainment*

Outcome	Compulsory		High school		Tertiary	
	(1)	(2)	(3)	(4)	(5)	(6)
	RF	FRD	RF	FRD	RF	FRD
Probability of unemployment	0.080	0.156	0.063***	0.121***	-0.038	-0.103
	(0.086)	(0.150)	(0.015)	(0.028)	(0.114)	(0.256)
Unemployment duration	20.381	39.706	17.377**	33.644**	-38.686	-105.165
	(28.022)	(48.288)	(8.329)	(16.453)	(44.246)	(100.225)
Probability of non-employment	0.004	0.008	0.040*	0.078**	-0.026	-0.071
	(0.045)	(0.077)	(0.021)	(0.037)	(0.065)	(0.145)
Non-employment duration	36.697	81.970	0.609	1.181	37.543	102.948
	(65.914)	(128.624)	(18.067)	(33.867)	(83.986)	(189.390)
First stage relationship	0.503***		0.431***		0.342***	
	(0.037)		(0.046)		(0.051)	
Observations	1,224		5,528		724	

Note: Each cell represents the result from a separate regression, with each row showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Table A.12. *Result by subgroups, gender*

Outcome	Men (1) RF	(2) FRD	Women (3) RF	(4) FRD
Probability of unemployment	0.054***	0.105***	0.118***	0.245***
	(0.018)	(0.034)	(0.042)	(0.088)
Unemployment duration	11.092	21.605*	34.135*	70.623*
	(6.892)	(12.971)	(18.652)	(37.844)
Probability of non-employment	0.008	0.015	0.083**	0.172**
	(0.022)	(0.041)	(0.042)	(0.082)
Non-employment duration	6.481	12.624	4.297	8.872
	(29.595)	(55.297)	(35.966)	(68.576)
First stage relationship	0.444***		0.427***	
	(0.051)		(0.033)	
Observations	5,187		2,289	

Note: Each cell represents the result from a separate regression, with each row showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Table A.13. *Result by subgroups, year of termination*

Outcome	2006–2009 (1) RF	(2) FRD	2010–2012 (3) RF	(4) FRD
Probability of unemployment	0.055***	0.109***	0.092***	0.241***
	(0.019)	(0.035)	(0.038)	(0.101)
Unemployment duration	13.411	26.383	24.313***	63.591**
	(8.761)	(16.677)	(10.101)	(27.709)
Probability of non-employment	0.003	0.006	0.075***	0.195***
	(0.028)	(0.053)	(0.025)	(0.058)
Non-employment duration	-17.880	-35.144	26.693*	55.538**
	(28.074)	(53.658)	(13.858)	(25.980)
First stage relationship	0.463***		0.392***	
	(0.035)		(0.036)	
Observations	4,855		2,621	

Note: Each cell represents the result from a separate regression, with each row showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

III: Financial incentives to work for disability insurance recipients. Sweden’s special rules for continuous deduction

1 Introduction

There is a large literature on the labor supply effects of disability insurance (DI). Evidence suggests that DI recipients have residual working capacities (e.g. Bound 1989, Gruber & Kubik 1997, Gruber 2000, Staubli 2011, Marie & Vall Castello 2012, Fevang, Hardoy & Røed 2013, Borghans, Gielen & Luttmer 2014, Moore 2015) and it is argued that disability insurance systems disincentivizes recipients to use these capacities. Costs for sickness absence are at the same time large in many countries, enhancing the importance of this issue. Many of these studies compare recipients to non-recipients who were denied DI benefits, or DI recipients with different benefit levels, and find that (higher) disability benefits decrease labor supply. The literature on work incentives for already awarded DI recipients, however, is not as large as the literature the on work disincentives of DI benefits. Recently, however, a number of examples of policy initiatives to increase the return to work among DI recipients have been introduced (e.g. the U.S.' "1$ for 2$ offset", the UK's "Pathways-to-Work"-program, Canada, Norway and Sweden). These programs provide incentives for DI recipients to use their residual working capacity and return to the labor market. The question is, can these financial incentives induce people with reduced working capacity to return to work and use any residual working capacity? Studies of some of these reforms suggest that there is a positive effect of such policies (Weathers & Hemmeter 2011, Campolieti & Riddell 2012, Kostøl & Mogstad 2014, Delin, Hartman & Sell 2015).

In January 2009, a reform was implemented in Sweden which gives certain disability insurance recipients the possibility to work while receiving benefit under so called *special rules of continuous deduction*. Those who are eligible can work or study without their recipient status being questioned and, additionally, they can keep some or all of their benefits while receiving a working income. Income below a specified level does not induce a reduction in benefits, while having income above this level reduces benefits by 50 percent of that income. Recipients can receive benefits according to this scheme as long as the benefits and the working income together are below a cap, and when the cap is hit benefits are reduced one-to-one with additional income. This reform is quite similar to the return-to-work scheme introduced in Norway in 2005. Kostøl & Mogstad (2014) evaluate the Norwegian reform and find positive effects on labor force participation and earnings.

In this study, I evaluate the Swedish continuous deduction program and its effects on labor market outcomes. I study effects on labor force participation and earnings, as well as having earnings above the earnings disregard. My study contributes to the relatively small literature on incentives for DI recipients to increase labor supply, by studying the effects of financial incentives to do so within a new context. Within this literature, even fewer studies evaluate the effects of work incentives for both full- and part-time recipients of DI benefits. Different responses to work incentives are expected for these groups as their working capacity and connection to the labor market differ. The Swedish continuous deduction program applies to both these two groups, and I study the effects of the program for full- and part-time recipients separately. The program also allows DI recipients to study without affecting benefits. Therefore, I also study its effects on increasing ones level of education, a use of residual working capacities possibly associated with fewer restrictions from the demand side than finding a job opportunity.

I use the criterion for eligibility to the program, based on time of DI award, for identification through a regression discontinuity (RD) setup. However, the retroactively set award date threshold for eligibility matches the timing of the enforcement of stricter requirements for being awarded disability benefits, causing compositional differences between DI recipients above and below the eligibility threshold. I therefore complement the RD design with a matching strategy, to compare only recipients who were not affected by the tightening of the DI eligibility criteria. This implies that I study the effects for a relatively weaker group in terms of health than the group of treated in general. My results suggest that the financial incentives provided by the continuous deduction program did not induce these DI recipients to increase labor supply or educational attainment.

The rest of the study is structured as follows. In section 2, I describe the Swedish disability insurance system and the continuous deduction program. Section 3 provides theoretical expectations and a review of the related literature. Section 4 explains the empirical strategy in detail and section 5 describes the data used. Section 6 provides the empirical results and section 7 concludes.

2 Institutional background

2.1 The Swedish disability insurance system

Individuals who partially or fully lose their ability to work due to health impairments can claim DI benefits through the Swedish Social Insurance Agency. Sick pay from the employer and longer periods[1] with sickness benefits usually precede DI benefits. Disability benefits are awarded when the working capacity is considered persistently reduced.

The Swedish DI system consists of two types of benefits designated for people of different ages. Disability benefits are awarded permanently[2] to

[1] Before the reforms in 2008, disability benefits were usually awarded after being on sick leave for one year (Government Bill 2007/08:124).

[2] Although called permanent benefits, the Swedish Social Insurance Agency can still revoke the right to these benefits if they find that the working capacity has increased. An assessment of the working capacity should be conducted every two years for disability insurance recipi-

people between the ages of 30 and 64. To qualify for permanent disability benefits, the individuals' health impairment must be severe enough for their working capacity to be considered permanently reduced. Benefits can be awarded full- or part-time depending on the severity of the impairment. To claim fulltime benefits, the working capacity must be considered fully or almost fully reduced, meaning a reduction of at least seven eights of fulltime work (i.e. 35 out of 40 hours per week). Part-time benefits can be claimed in quarters of fulltime (i.e. 25, 50, or 75 percent). To claim benefits of 50 or 75 percent of fulltime, the working capacity must be reduced by at least 50 or 75 percent, respectively. Claims of 25 percent benefits is more restrictively awarded, but can be awarded when the working capacity is considered reduced by at least 25 percent even after a longer period of sickness benefits or rehabilitation. Prior to July 2008, disability benefits could also be awarded temporarily for periods between 12 and 36 months depending on how long the reduction in the working capacity was predicted to last. Together with many other changes to the Swedish sickness and disability insurance system in 2008, temporary disability benefits where abolished.[3]

The counterpart to disability benefits for people between the ages of 19 and 29 is called activity benefits. Activity benefits can only be awarded for a fixed time period, between 12 and 36 months, at a time. When activity benefit recipients turn 30, they can instead be awarded disability benefits if their working capacity is considered permanently reduced. This study focuses on individuals between 30 and 64 years old, receiving permanent disability benefits, as this is the group that can be eligible for continuous deduction.

2.2 The situation before the reform

Large changes were made to the Swedish sickness and disability insurance system in 2008. The main motive behind the reforms was to increase the propensity to return to work among recipients of sickness and disability benefits. The changes were enforced in response to high costs for health-related insurances and the recent increase in the inflow to the DI system; the total number of DI-recipients had increased by around 25 percent in the five years prior to 2008. (Government Bill 2007/08:214)

The age-distribution among permanent DI-recipients was highly skewed to the right, with almost 40 percent in their 60s at the introduction of the

ents (if not eligible for the continuous deduction program). The possibility to do this type of assessment is removed for those eligible, as part of the program.

[3] Individuals already awarded a period of temporary disability benefits at the time of its abolition could be awarded an additional period of up to 18 month of temporary disability benefits after the period already awarded. The same was true for individuals with activity benefits who lost the right for this type of benefits due to age after July 1, 2008. Temporary disability benefits thus remained until the end of 2012.

continuous deduction program. *Figure 1* shows the age distribution of my sample at program start.

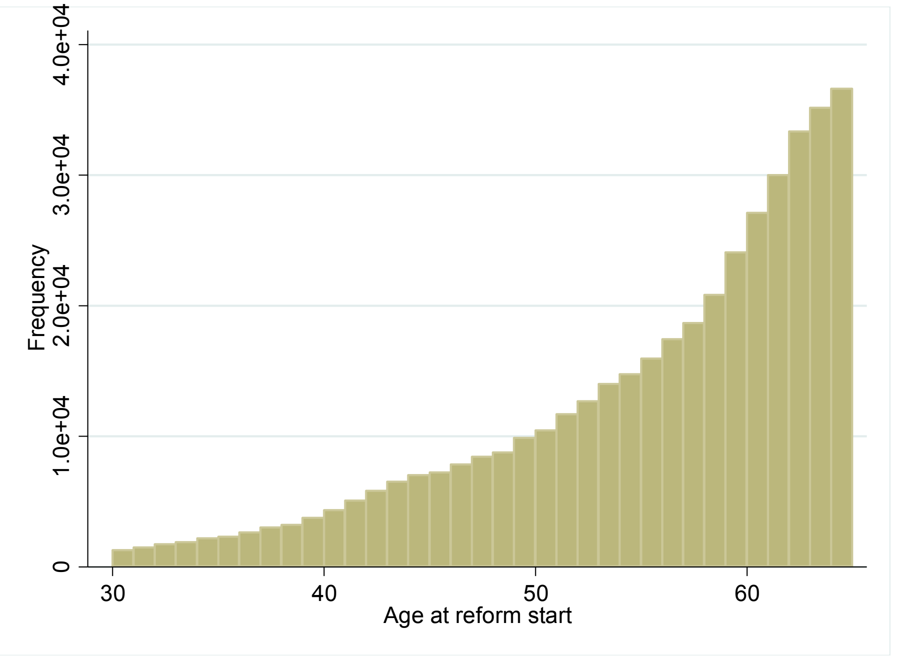


Figure 1. Age distribution at program start

The general retirement age in Sweden is 65 years of age. Almost all the outflow from DI-benefits is due to old-age retirement or death. Less than one percent of all DI-recipients (including temporary disability and activity benefit recipients) returned to the labor force each year before the reform. Among these, younger recipients were most likely to return; while the average age in the stock of DI-recipients was 55 years, the average age among the few who exited disability insurance for work or unemployment was around 40. Around 2.5 percent of activity benefit recipients returned to the labor force, while only around 0.2 percent of DI-recipients above the age of 50 did the same. In the ages 30 to 49, the share was around one percent. (Jans 2007)

The continuous deduction program was introduced to increase the propensity to return to work among permanent DI-recipients. Since 2000, DI-recipients have had the opportunity to work for a limited time without losing their benefit status, within a system called *resting benefits*. Resting benefits meant that DI-recipients could put on hold part of their benefits in quarter steps of fulltime work and work on this "resting" part.[4] Resting benefits was

[4] Fulltime benefit recipients could work on one eighth of fulltime without directly affecting benefits, while part-time benefit recipients could not work at all on the part with benefits. This corresponds to the eligibility criteria for the different benefit extents, as a reduction to the

not possible the first 12 months with DI-benefits. To *rest* part of benefits meant that this part of the benefits were held back beyond the first three months, but the DI-recipient could at any time notify the Social Insurance Agency that he or she wanted to return to benefits and end the work trial period. Resting benefits was possible for a maximum of 12 months within a 24 month period before the benefit status could be reevaluated. The Social Insurance Agency had to be notified before starting work, and benefits also needed to be rested in order to study or do volunteer work.

Fewer than expected had used this opportunity, only about one percent of all DI-recipients (Ds 2008:14). Policymakers were convinced that more recipients could return to work, for instance because regional differences in the number of DI awards were considered too large to be explained by regional differences in health and working capacity among the awarded. A survey conducted by the Social Insurance Agency also revealed that, with some work adaptions, as much as twelve percent of responding DI-recipients believed that they would be able to do work to some extent (Larheden 2008). With this in mind, the continuous deduction program was introduced, to increase the financial incentives to return to work among recipients of permanent disability benefits. (Government Bill 2007/08:214)

2.3 The continuous deduction program

Since January 2009, certain disability insurance recipients have been eligible to work while receiving benefits under the so called special rules of continuous deduction. These involve the possibility to work or conduct studies without ones recipient status being questioned. Eligible DI-recipients get to keep some or all of their benefits while earning a working income. The aim of the reform was to increase incentives for DI-recipients to return to work and to improve the opportunities to make use of any residual working capacity present. The previous rules of resting benefits implied high marginal effects of increasing labor supply, which was believed to be the reason for the low take-up among individuals receiving disability insurance benefits.

Working under the rules of continuous deduction implies no reduction of benefits if annual income[5] from work is below an earnings disregard. If annual income exceeds the earnings disregard, benefits are reduced by SEK 0.5 for every additional SEK earned. The level of the earnings disregard depends on the extent of benefits, to take into account that part-time benefit recipients are presumed to work on the part without benefits. For fulltime DI-recipients

working capacity of at least seven eights is required to receive fulltime benefits while to receive part-time benefits the working capacity must be reduced by *at least* the percent of full time work that is awarded.

[5] Annual income is defined as any income source that is counted as pensionable income, such as wage, business income, sickness and unemployment benefits, parental insurance benefits, some education grants and stipends etc.

the earnings disregard was SEK 42,800 in 2009, which corresponded to around USD 6,000.[6] If annual earnings and benefits taken together exceed a cap, benefits are reduced one to one with earnings above this level. The marginal effect of working when income is above the cap is thus 100 percent due to lost benefits. However, considering previous income levels of DI-recipients in general, this cap is set at a high level and was therefore not expected to affect labor supply decisions negatively. The cap was SEK 342,400 in 2009, while the average annual earnings of a person with fulltime benefits before their first long sick leave was around SEK 100,000.

There is no time limit for working with continuous deduction. The earnings disregard scheme is instead constructed to stimulate outflow from the DI system. It is beneficial for DI-recipients to initiate a reduction of the extent of benefits, e.g. from fulltime benefits to 75 percent benefits, if their residual working capacity is large enough. This is because, if earnings are high enough, recipients will benefit from reducing the benefit level in terms of total income since the earnings disregard is higher with a lower benefit level. Even as initial benefits are reduced to a lower benefit extent, the higher earnings disregard means that total income will be higher with sufficient labor supply. The idea is that recipients will self-select the optimal level of DI-benefits given their working capacity, so that those with fully regained working capacity will exit the disability insurance system on their own accord.

2.3.1 Implementation

Eligibility for working under the special rules of continuous deduction is based on the date of benefit award. Those awarded permanent DI-benefits for a period starting before July 1 2008 are eligible[7], while those awarded thereafter are not. I use this setup in a regression discontinuity design to study the effects of program eligibility. However, this cutoff coincides with another reform, as eligibility for disability benefits was changed from July 1 2008 onwards. Stricter rules were imposed for being awarded permanent DI-benefits and temporary DI-benefits were abolished. I account for the resulting differences above and below the cutoff by combining the regression discontinuity approach with a matching strategy.

The cutoff date for being eligible for the special rules of continuous deduction was set retroactively, which works to avoid increases in inflow to permanent disability benefits in order to be eligible to work with benefits within the continuous deduction program. The parliamentary decision to pass the reform was made on October 30 2008, with retroactive eligibility

[6] In 2009 the earnings disregard was SEK 111,280 for recipients of 75 percent benefits, SEK 179,760 for 50 percent benefits and SEK 248,240 for 25 percent benefits. These amounts are adjusted each year to account for e.g. inflation.

[7] Eligibility is lost if the extent of benefits is expanded after July 1 2008.

for recipients awarded DI prior to July 2008[8]. Applying for disability benefits can be done retroactively for up to three months before the month of application, and a doctor's note has to be attached validating the claim for the full period. This means that in order to be considered for DI award by the less strict regulation that was applicable for benefit periods starting before July 1 2008, the application needed to have been submitted in September 2008, before the continuous deduction program was passed in the parliament. Therefore, there is little concern for self-selection into treatment based on anticipated potential outcomes.

For the rules of continuous deduction to be applied, an application must be submitted to the Swedish Social Insurance Agency before work starts. The continuous deduction program also allows beneficiaries to conduct studies or do volunteer work which is not otherwise allowed without affecting benefits. For doing unpaid work or studying within the program, no application is needed.

Since the introduction of the program, the share of eligible recipients applying to work with continuous deduction has risen steadily each year, from just above two percent the first year to around nine percent in 2014. (Swedish Social Insurance Agency, 2015) At least some of this increase is likely to be explained by a gradual change in the age distribution of eligible recipients, as a large proportion has left the DI system for retirement over time. The number of applicants increased the first few years, from around 7,500 in 2009 to 9,900 in 2012, and has since decreased a little each year. Two surveys were conducted in 2009 and 2010 among eligible beneficiaries. The first showed that the continuous deduction program was well-known among eligible recipients[9] (Demoskop, 2009). According to the second survey, working within the continuous deduction program is more common among women, younger, well-educated, and non-single recipients. Around 2.4 percent of the total number of eligible had applied when the second survey was conducted, and among these 80 percent was currently working while 10 percent had been working. The survey suggests that the working hours were increased by about as much for fulltime as part-time recipients. 8 percent of those that had not yet applied stated that they would likely apply in the coming years. 1.1 percent were studying with benefits and 4.5 percent were doing unpaid volunteer work. (Demoskop, 2010) This indicates that there is some residual working capacity among the eligible recipients, but also suggests that labor demand for workers with disabilities does not match their willingness to work since twice as many eligible beneficiaries were doing unpaid work as the share doing paid work. Unfortunately, unpaid volunteer

[8] The cutoff date was originally proposed to be in August 2007. After complaints by referral organizations that July 1 2008 would be a more appropriate cutoff date also for eligibility to work within the continuous deduction program, due to the changes in eligibility for DI-benefits after this date, the cutoff date was adjusted accordingly.

[9] 82 percent of non-applicants responded that they knew about the new rules.

work is unobservable in administrative data, therefore this study is limited to studying the effects on paid work and education.

Permanent DI-recipients not eligible for continuous deduction receive essentially the same treatment as before the rules of continuous deduction were implemented. Those who were awarded DI-benefits after July 1 2008 can only try to work if they rest part (or all) of their benefits. The only change that was made to the system of resting benefits was that recipients could previously maintain their benefits the first three months of work with resting benefits, but now they instead continuously maintain 25 percent of the resting amount tax free during the work trial period. This system provides high marginal effects from working, since benefits need to be rested by a quarter of full time work even if working hours are only increased by a few hours a week. This implies a high marginal cost of using ones residual working capacity if it is not high enough, which might discourage workers from trying to return to work.

3 Theoretical framework and previous empirical evidence

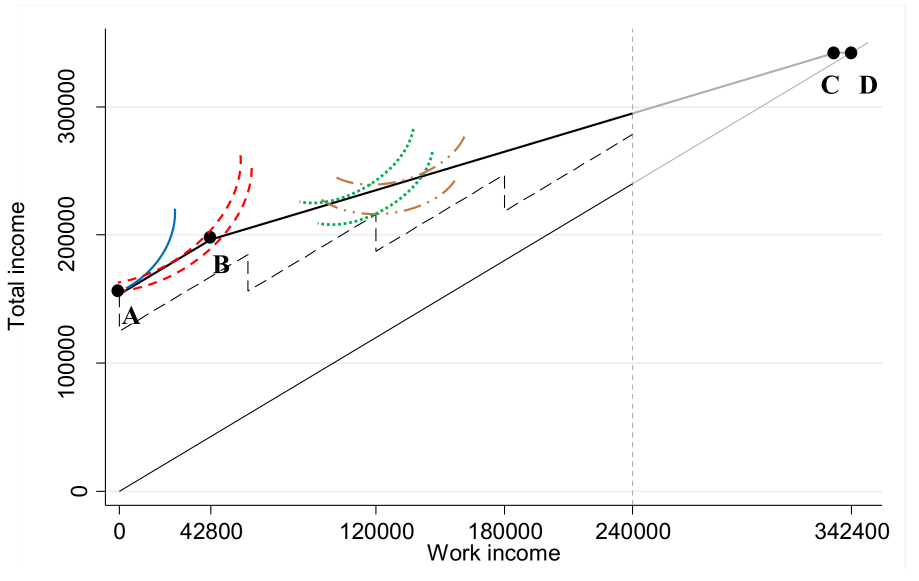
3.1 Theoretical predictions

Figure 2 shows the financial effects from the continuous deduction program compared to the rules for resting benefits, the option that is available for the control group. It is a simplified illustration of the basic economic forces at work for individuals maximizing utility, assumed to depend positively on consumption (corresponding to total income) and leisure (the negative of working income, corresponding to a certain number of working hours when wages are given).[10] Panel A shows a type case recipient of fulltime disability insurance benefits, and panel B shows a type case part-time disability benefit recipient with half-time benefits.

The solid and dashed lines show the budget constraints with continuous deduction and resting benefits, respectively. The diagonal line shows when total income corresponds to work income, and thus the distance between this line and the solid and dashed line illustrates the size of DI benefits under the continuous deduction program or resting benefits, respectively. The kinks in

[10] *Figure 2* does not take into account taxes or the effects on other benefits such as the means tested housing allowance. Work income is related to working hours in both cases by the hourly earnings that would be earned if the case individual was working. Both case individuals would earn an annual income of SEK 240,000 if working fulltime, which yields annual benefits of SEK 153,600 for the fulltime case recipient and combined annual benefits and earnings of 196,800 for the half-time case recipient, with 64 percent DI benefits.

Panel A Case example for a fulltime recipient



Panel B Case example for a part-time recipient

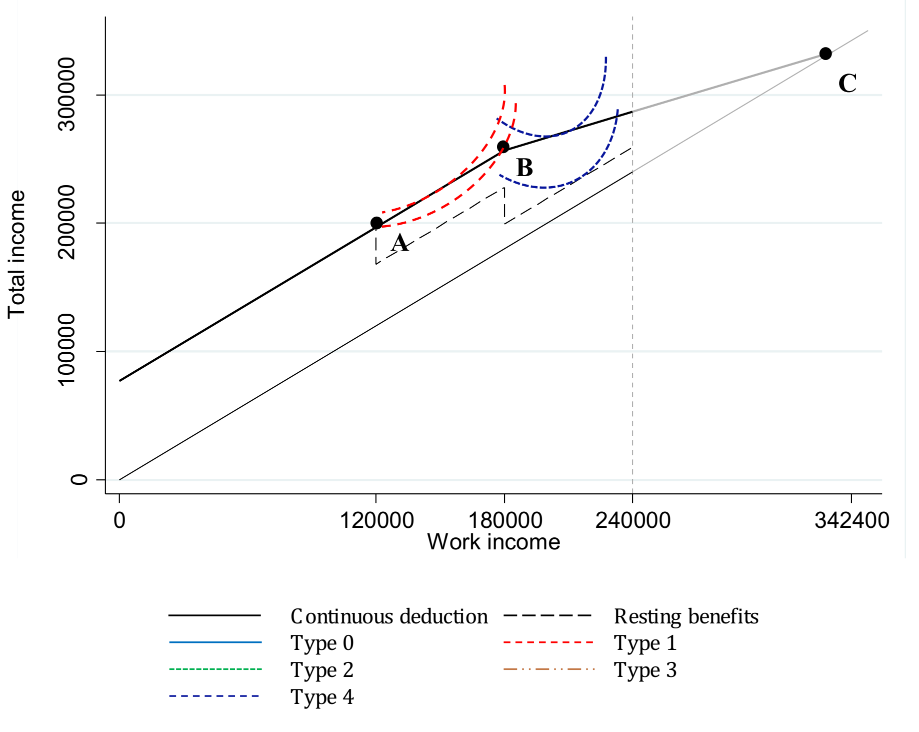


Figure 2. Case examples of the financial effects of the continuous deduction program

the dashed lines show when the benefit extent must be reduced within the system of resting benefits, to enable more working hours. The part AB of the continuous deduction budget constraint in panel A and B, respectively, is the part below the earnings disregard, where benefits are not reduced with earnings, while the part BC is the income range where earnings are reduced by half of the work income, and therefore has a flatter slope than the part AB. Fulltime work is reached between B and C in the examples below, as shown by the dashed horizontal line. The small part CD in panel shows were the fulltime case recipient would reach the cap and benefits would be phased out one-to one with additional income.

The figure also shows the responses of individuals of different types (0, 1, 2, 3, or 4) with respect to their utility functions. Leisure is assumed to be a normal good. The shape of the utility function is determined by preferences for consumption and leisure, which depends on the disutility from work, partly determined, of course, by the severity of the work related health impairment.

The effect on labor supply from the financial incentives induced by the continuous deduction program depends on where along the budget constraint each beneficiary's utility is maximized with and without continuous deduction. A fulltime recipient who would choose zero labor supply with the system of resting benefits, will either increase labor supply or not if faced with the option of continuous deduction, depending on the shape of his or her utility function. This is shown in panel A and B by individuals of two types with different utility functions; type 0 and type 1. For a type 0 individual, the labor supply does not change since utility is maximized at zero irrespective of which budget constraint he or she has. For a type 1 individual on the other hand, utility functions are such that zero working hours is chosen with resting benefits because the loss of benefits from increasing labor supply moves the individual to a lower utility level. With continuous deduction, however, the type 1 individuals' utility is increased by entering the labor force. Since both types have no previous labor income, there is no income effect and the predicted labor force participation response of the reform is thus positive (or zero if all fulltime DI-recipient are type 0 individuals). Since the budget constraint is not changed for the part without benefits, the same prediction is made for the effect on the intensive margin for part-time recipients who do not work on the part with benefits with the resting benefit system, as shown for type 1 in panel B.

Since the budget constraint with continuous deduction is always above the budget constraint for resting benefits when labor supply is above zero, there is a negative income effect from moving from the resting benefits budget constraint to the continuous deduction budget constraint at all other levels of (initial) labor supply. For both full- and part-time recipients maximizing utility at a kink other than zero labor supply, continuous deduction makes increasing working hours more profitable than with resting benefits, which induces a positive substitution effect from increasing labor supply. At these kinks, income and substitution effects have opposite signs and the labor supply response is thus ambiguous, as shown by the responses of types 2 and 3 in panel A. While both type 2 and 3 individuals maximize utility at the same labor supply level with resting benefits, depending on the type, individuals either increase (type 3) or decrease (type 2) labor supply when faced with the new budget constraint with continuous deduction. This case is not illustrated in panel B, but holds for part-time recipients at the one kink to the right in the figure as well.

For types choosing positive labor supply with resting benefits, but *not* positioned at any of the kinks along the budget constraint, the predicted labor supply response from getting continuous deduction is less ambiguous. Within segment AB of the continuous deduction budget constraint, the relative price of leisure is the same with both budget constraints, so there is only a negative income effect. Within segment BC, since benefits are reduced by 50 percent of the additional income earned, the relative price of leisure is lower than with resting benefits. This creates a negative substitution effect (Eissa & Liebman 1996). Since the income effect is also negative, the labor supply effect is unambiguously negative. This is illustrated for type 4 in panel B.

If benefits and earnings together exceed the cap level, benefits will be phased out one-to-one with additional income, further lowering the price of leisure. Within such a segment, an even more negative substitution effect would supplement the negative income effect, and decrease labor supply. This would be the case in segment CD in panel A in *Figure 2*. The case recipient in panel B does not have an income path high enough to ever hit the cap since benefits are fully phased out before the cap level.

The predicted total labor supply response of the continuous deduction program is thus ambiguous. The predicted response at the extensive margin is unambiguously nonnegative, but at the intensive margin, for full- and part-time recipients working on the part with benefits, the direction of the response depends on the shape of the utility functions of the DI-recipients. Nonetheless, because of the low share of DI-recipients returning to work prior to the reform, the expected labor supply response is, despite of this, positive. All but a few DI-recipients had zero labor supply, or zero labor supply beyond the part without benefits for part-time DI-recipients, before the continuous deduction program was introduced. For both of these cases, a positive labor supply response is predicted (assuming there are residual working capacities among DI recipients and not all being type 0 individuals).

3.2 Previous literature

This paper is related to the literature on the effects of financial incentives to work for disability insurance recipients. This literature is fairly limited, even though a few studies have been done recently. The earlier literature on the labor supply effects of disability insurance receipt suggests a presence of residual working capacities among DI recipients (Bound 1989, Gruber & Kubik 1997, Staubli 2011, Moore 2015). This literature has generally focused on the labor supply of rejected DI-applicants as the counterfactual for DI-receipt. Another related literature concerns the relation between the level of disability benefits and labor supply of DI-recipients. These studies show that higher benefit levels imply a lower labor supply (Gruber 2000, Marie & Vall Castello 2012, Fevang, Hardoy & Røed 2013, Borghans, Gielen & Luttmer 2014, Koning & van Sonsbeek 2016). The negative relationship between the benefit level and labor supply suggests that residual working capacities exist also among DI-recipients.

The more directly related literature on the effects of financial incentives that encourage people with disability benefits to return to work generally suggests positive effects from this type of treatment, although not all financial incentives seem to work. Weathers & Hemmeter (2011) show that the "$1 for $2 offset" pilot program in the U.S., which provides a gradual decrease instead of a full reduction of benefits if earnings are above an earnings disregard level, similar to the Swedish continuous deduction program, increased the share of beneficiaries with earnings above the earnings disregard (i.e. the substantial gainful activity (SGA) level, which amounts to earnings of USD 1,130 per month in 2016). However while their results show a positive effect on earnings for beneficiaries with earnings below the earnings disregard before the program, beneficiaries with earnings above the earnings disregard before the program decreased their earnings on average. The lack of any effect on labor force participation also found in the study might be explained by the composition of the sample since program eligibility was randomized among program volunteers. Delin, Hartman & Sell (2015) study the same program and find a delayed but positive effect on employment outcomes for the treated which increases with time since program start.

Campolieti & Riddell (2012) find an increased propensity to work after the introduction of an earnings disregard within the Canada Pension Plan disability program. They find no effects from the introduction of an automatic reinstatement without re-application for up to 24 months for DI-recipients who want to come back to disability insurance after working. Bütler et al. (2014) study the effects of a randomized experiment in Switzerland, which provided large financial incentives to work for DI-recipients. Recipients were offered a claim of up to the equivalence of USD 71,000, comparable to the average disposable yearly income of Swiss households, to expand work hours and reduce benefits. The call-back rates were low and unaffected by the size of the claim offered. The take-up rate was only half of a percent, and Bütler et al. conclude that the program most likely provided windfall gains to recipients who would have returned to work anyway, rather than incentivized work.

A reform similar to the one under study here, in terms of both content and setting, was implemented in Norway in 2005.[11] The cutoff for program eligibility was set retroactively with respect to time of DI award in Norway as well. Kostøl & Mogstad (2014) use an RD design to estimate causal effects of the Norwegian reform. The setting in which treatment is based on the date of DI award makes it possible to use the RD framework to compare individuals who are assumingly similar in all other aspects, except for the date of DI award. There was no other confounding differences between those awarded just prior to and just after the cutoff date in Norway. The retroactive setting of the cutoff increases the credibility of the RD design as no manipulation of the forcing variable in order to become eligible is possible. Kostøl & Mogstad find positive effects from the program on labor force participation and earnings for recipients aged 18 to 49 years. The positive effect is strongest and statistically significant three years after program introduction, at the end of their follow up period. They also show that the response to the financial incentives is highly heterogeneous. The response is stronger among males, well-educated, and recipients with more labor market experience. Areas with low unemployment also triggered a larger response, pointing to labor demand posing a problem for DI-recipients in returning to the labor market. Among DI-recipients above the age of 50, the study showed no positive effect from the return-to-work program.

In many of these studies, positive effects on beneficiaries' labor supply seem to be driven by the response of younger beneficiaries. Koning & van Sonsbeek (2016), who find that lower benefit levels after the income-related benefit period is exhausted for Dutch part-time disability beneficiaries increase labor supply, show that this effect is confined to younger recipients and strongest in the youngest age group below 35 years old. Kostøl & Mogstad (2014) find positive effects only among DI-recipients aged 18 to 49. Moore (2015) studies the employment response of recipients with alcohol- or drug-related disabilities that lost the eligibility for disability insurance in 1997 in the U.S. The positive effect was stronger for younger recipients, in this case 30-39 year olds, than for recipients aged 40-49, and even more so than for those aged 50-61. Moore also finds an interesting u-shape in the size of the effect over time spent with disability insurance. The effect was strongest for those who had received benefits for 2.7 years prior to termination.

Previous results are generally in line with the theoretical predictions described in the previous section. Both Campolieti & Riddell (2012) and Kostøl & Mogstad (2014) find positive effects on the extensive margin for

[11] The setting of the Norwegian return-to-work-program for DI recipients is similar to the rules of continuous deduction in Sweden. Benefits are reduced if earnings exceed an earnings disregard by approximately NOK 0.6 for every additional NOK 1 earned, up to an earnings ceiling level where all remaining benefits are lost. This ceiling is generally above fulltime work earnings. For more details see Kostøl & Mogstad (2014).

treatments where the budget constraint shifts similar to the continuous deduction in *Figure 2*. Weathers & Hemmeter (2011) find a positive effect for individuals previously positioned at the kink created by the full reduction of benefits at the SGA-level, suggesting that the positive substitution effect at the kink outweighs the negative income effect in their setting. The effect for individuals previously positioned above the kink is negative, corresponding to the predictions for individuals positioned away from the kinks where both the income and substitution effects are negative. Bütler et al. (2014), however, found no effect of a lump sum offer to expand work hours and reduce benefits. Perhaps the experimental setting provided more uncertainty than financial incentives within the DI-system would have, discouraging recipients from accepting the offer.

4 Empirical strategy

4.1 The regression discontinuity design

The objective of the empirical strategy is to estimate the causal effects of the reform by coming as close to a randomized experiment as possible. The basic idea of the regression discontinuity design is that there is a discontinuity in treatment assignment, caused by some policy rule, which can be considered to provide exogenous variation in treatment status. Treatment is assigned according to some assignment variable, denoted the *running* or *forcing* variable, and there is a threshold value of that variable which determines whether an individual is treated or not. In this case, treatment is determined by the time of award of DI-benefits. Outcomes are allowed to vary by the values of forcing variable itself, and the approach builds upon the notion that close to the cutoff threshold for treatment, individuals are so similar with respect to the forcing variable that treatment can be considered as good as randomly assigned. Critical for the validity of the approach is that individuals cannot precisely determine the value of the forcing variable and thereby their own treatment assignment, which would invalidate the local randomization concept.

As the cutoff date for eligibility was set retroactively, there is little concern for self-selection into treatment, at least based on anticipated potential outcomes, which would be difficult for the researcher to control for using observables. However, the local randomization concept is nonetheless invalidated by the fact that there was a regime change with respect to DI eligibility at the same time as the cutoff threshold for eligibility for the continuous deduction program. Above and below the cutoff, recipients therefore differ in terms of working capacity. Above the cutoff, the sample consists of individuals with more severe health impairments, directly related to their labor market prospects. From July 1 2008 and onwards, only beneficiaries with impairments severe enough for their working capacity to be considered permanently reduced were qualified for permanent DI benefits. This involved chronical illnesses or irreversible injuries where further rehabilitation measures could not improve the working capacity. Previously, these requirements were less strict, and other considerations than the health impairment such as age, education or residential considerations, could also be taken into account for the award of DI-benefits.[12] (Government Bill 2007/08:136)

Using the terminology of the potential outcomes framework (e.g. Rubin 2005), one of the assumptions for the RD approach to be valid is that the expectations of potential outcomes, Y_1 and Y_0, are continuous with respect to the forcing variable, C_i, at the cutoff value c_0, i.e.:

$$E(Y_1|C_i) \text{ and } E(Y_0|C_i) \text{ are continuous at } C_i=c_0 \tag{1}$$

Due to the differential selection into permanent DI-benefits before and after the cutoff, this assumption is not fulfilled. Formally, before July 1 2008, award of permanent DI-benefits required a reduction in work capacity, H_i, which satisfied $H_i \geq H_t$. Thereafter, award of permanent DI-benefits instead required a larger reduction in work capacity that satisfied $H_i \geq H_c > H_t$. It is expected that potential labor market outcomes will depend on the reduction in work capacity, such that $Y_{1i}=f(H_i)$ and $Y_{0i}=f(H_i)$ (Koning & van Sonsbeek 2016). In fact, I expect potential outcomes to depend negatively on H_i so that a more severe reduction in the working capacity (i.e. a higher value of H_i) worsens potential labor market outcomes.

Additionally, the unconfoundedness assumption states that conditional on covariates treatment and potential outcomes are independent, or formally:

$$(Y_{1i,} Y_{0i}) \perp T_i|\mathbf{X}_i \tag{2}$$

where $\mathbf{X}_i$ is a vector of observable characteristics. This assumption is often used in a broader context when causal effects are estimated, with the assumption of selection on observables. The RD approach generally fulfills this assumption by design, and also allows for selection on unobservables, since the design itself is expected to provide balance in all covariates due to local randomization. Conditioning on $\mathbf{X}_i$ is therefore not necessary, the only covariate conditioned on is the forcing variable, and in the common case that covariates are included in RD analyses, the purpose is to reduce variability in estimates (Lee & Lemieux 2010).

[12] According to Social Insurance Agency representatives, this possibility was rarely used in practice, and therefore this part of the eligibility changes would not imply any significant changes to the award of DI-benefits (Dutrieux et al. 2011a). As I will show (section 5.2) however, this does not seem to be the case.

In my case, however, the RD design alone does not ensure balance, even when individuals do not manipulate treatment assignment. I do not expect any exact manipulation of the forcing variable just around the cutoff in order to be eligible for the continuous deduction program. To do so, the individuals would have had to know what the cutoff date was going to be and have had the ability to affect their own value of the forcing variable. The cutoff date for the rules of continuous deduction was set retroactively, so manipulation around the cutoff to become eligible for these rules is improbable. There is, however, a clear surge in the inflow to permanent DI just before the cutoff (see *Figure 3*).

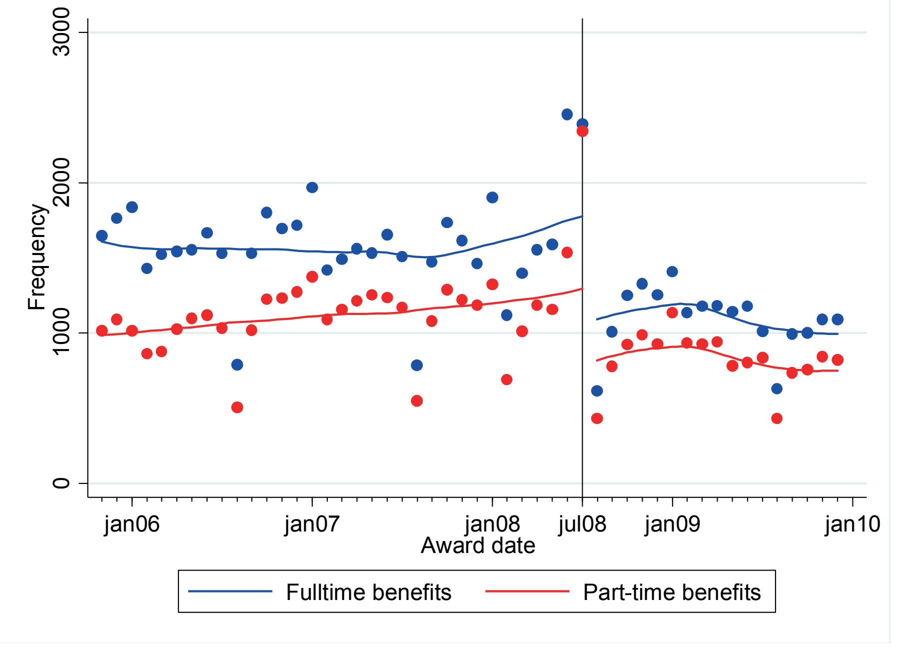


Figure 3. Inflow by award date

Besides applications for permanent DI, caseworkers could initiate transfers of cases from sickness benefits to disability benefits[13], and the reduction in these transfers is the main source for the drop in the number of DI-cases granted after July 2008 (Dutrieux et al. 2011b) According to the Swedish Social Insurance Agency, the increase in inflow just before the cutoff can be interpreted as a surge in case-worker output due to the announcement of the stricter requirements from July 2008 onwards (Swedish Social Insurance Agency 2013). This means that there is an increase in the density of the forc-

[13] This was usually done after about a consecutive year with sickness benefits (Government Bill 2007/08:124).

ing variable just before the cutoff which does not reflect manipulation of the forcing variable in order to get the treatment I study here, but instead reflects case-workers trying to give those with $H_i \in (H_c,H_t)$ (i.e. outside the region of common support in this estimation, see below) a greater chance to be awarded permanent DI by working down their (transfer) caseloads just before the cutoff. Case-worker initiated transfers are always executed the month after the transfer is decided, so this is a probable explanation for the peak in July 2008 for both full- and part-time recipients. Retroactive applications had to be sent in no later than September 2008 to be awarded according to the less strict rules starting June 2008. There is a peak in applications coming in in September (Sjögren Lindquist & Wadensjö 2011). The peak in June 2008 for fulltime recipients is therefore likely more problematic than the peaks in July.[14]

4.2 Inference with the local randomization violation and matching

4.2.1 The naive RD estimate

A valid RD design presumes local randomization, which ensures that covariates are balanced when restricting the analysis to a tight region around the cutoff of the forcing variable. In this case, as I have explained above, this is not fulfilled with regard to H_i. I therefore need to condition on H_i to be able to make credible inference even within the RD framework. There is little advice in the literature on how to do so.

Gerard, Rokkanen & Rothe (2016) show how sharp bounds on causal treatment effects can be derived within the regression discontinuity framework in case of manipulation of the forcing variable. They show that, if one is willing to assume that units who manipulate the forcing variable so that they are always on one particular side of the cutoff, have higher average potential outcomes under treatment than units that do not manipulate the forcing variable, and can thus be observed on either side, the naive RD estimate that ignores selection concerns is an upper bound of the treatment effect for the non-manipulators.[15] In this study, the direction of the intended selection is clear. The strictness of the DI-system was tightened, so that individuals awarded DI benefits above the cutoff are on average of worse health, i.e. have more severe working capacity reductions, than those awarded DI benefits below the cutoff. I have argued that potential outcomes depend negatively on the reduction in working capacity. If this expectation is valid, a

[14] There is a peak in applications September 2008, although the share of workers applying in September is not much higher than the monthly average before July 2008. With the exception of September, after July 2008 the share of workers applying for disability benefits declined substantially (Sjögren Lindquist & Wadensjö 2011).
[15] For details, see Gerard, Rokkanen & Rothe (2016).

traditional RD comparison between awarded just prior to and after the cutoff date would thus overestimate the effect, since those treated on average have better health than the control units.

It could of course be questioned whether case-workers have complied with the stricter regulations, and it is also possible that different components of the regime change had opposing effects on the expectation of potential outcomes of recipients before and after the regime. The regime change implied stricter screening with respect to health impairments directly, but also on other considerations that could potentially yield bias in the opposite direction.[16] As I will show, however, an analysis of pre-program outcomes indicates that working capacities are better among the treated.

4.2.2 RD and matching

Conditional on H_i, who ends up in the treatment or control group can be considered as good as random. I must thus include H_i together with the forcing variable in $\mathbf{X}_i$ in (2) even within the RD design for the unconfoundedness assumption to hold. Conditional on H_i, the continuity assumption holds. One way to obtain balance would be to restrict the sample based on some exclusion criteria, in this case including only observations where $H_i \geq H_c$. The question then becomes how to determine the exact cutoff value H_c.

Keele et al. (2015) and Linden & Adams (2012) try to manage the issue of covariate imbalance within the RD framework. Both suggest combining the RD framework with matching to obtain balance in covariate distributions between the treatment and control groups. Linden & Adams (2012) identify three potential ways to balance covariates; *i)* to apply some exclusion criteria in the data processing stage that ensures balance, *ii)* to apply regression adjustment to the RD model, and *iii)* to use the propensity score as a complement to the RD design to correct for imbalances in characteristics between the treated and control groups. In my case, there is no simple indicator to use as an exclusion criterion to ensure that balance is achieved. Regression adjustment is easy to apply, but may elicit biased results, especially in cases like this where overlap is limited. There is also no way to validate that imbalances have been properly adjusted, or that the correct functional form has been used (Linden & Adams, 2012). Linden & Adams propose to use the propensity score matching method combined with the RD design, either by matching pairs based on the propensity score and conducting statistical anal-

[16] Processing times were higher for applications coming in before the cutoff, and for applications coming in during the peak in September, which means that processing times are likely discontinuous at the cutoff. Autor et al. (2015) show that a longer processing time reduce long run labor supply and earnings in the U.S. However, they show that this effect is entirely driven by processing times that postpone the start of the trial work period. Since untreated can apply for resting benefits only after 12 months after award, average processing times are not likely to push the start of the trial work period above this time for treated. Instead, since the 12 month waiting period does not apply to the continuous deduction program, the results of Autor et al. further suggests that the bias should go in the expected direction.

ysis in the usual manner on the matched pairs alone, or by constructing weights based on the conditional probability of each individual being in the group he or she is in (treatment or control), i.e. the inverse probability treatment weighting (IPTW) technique. They argue that these weights easily can be added to the existing RD modelling methods, and show that their weighting strategy outperforms standard regression adjustment using example data.

Keele et al. (2015) suggest combining the regression discontinuity approach with conditioning on observables when balance in the covariates is lacking at the cutoff, under some conditions. They argue that, even if there may be an issue of selection on unobservables for the full sample, there may be cases where it is reasonable to assume that, within a tight region around the cutoff, such selection is ignorable, and selection on observables is plausible. They formulate a local unconfoundedness assumption under which this combination of methods is valid. Transforming this assumption to suit my notation, it reads:

$$\text{Within a small region } c_l<c_0<c_u\text{, we have that } (Y_{1i,} Y_{0i}) \perp Ti|\mathbf{X}_i \tag{3}$$

This means that, within a region around the cutoff, unobservables are balanced after conditioning on observables. Similar arguments are made in several studies (i.e. Battistin & Rettore 2008, Mealli & Rampichini 2012, Angrist & Rokkanen 2015). Keele et al. additionally argue that, if unconfoundedness holds within the region, this design allows for estimation of the treatment effect for the entire region around the cutoff as opposed to only at the cutoff value. They, too, propose using a matching strategy to implement this combined design.

The identifying assumption for the matching method is, aside the same unconfoundedness assumption as with the RD approach, the overlap condition:

$$0<P(T_i=1|\mathbf{X}_i)<1 \tag{4}$$

This means that the covariate distributions of the treatment and control groups are similar so that there is a comparable unit in the other group for each observation. In my case, this assumption is also not fulfilled with respect to the observable H_i. The requirements for the working capacity reduction in order to be awarded DI-benefits was changed at the same cutoff, which implies that there is not complete overlap in this variable, such that $0<P(T_i=1|\mathbf{X}_i)\leq 1$. To avoid bias, the treatment group must be trimmed so that I am able to find comparable control units for each treated in the estimation sample. Limiting the analysis to the region of common support with respect to the reduction in working capacity means that the estimation sample will only include observations that fulfill $H_i \geq H_c$. Therefore, I will not be able to

estimate the average treatment effect of all treated (ATT), but only for a subgroup of the treated who satisfy $H_i \geq H_c$, i.e. the average treatment effect of the untreated (ATU), since $H_c > H_t$. This estimate is however interesting in itself in the sense that it is the average treatment effect for the non-treated, which corresponds to the treatment effect for the group that is relevant if the program would be extended to more or all recipients of permanent DI.

Consider that DI recipients are of two types; an "always-type" with reductions in working capacity, $H_i \geq H_c$, such that they are eligible for DI benefits both before and after the regime change, and a "before-type" that only fulfill the screening requirements before the regime change, i.e. with reductions in working capacity according to $H_t \leq H_i < H_c$. Under the previous regime, both before types and always types were awarded DI, but after the regime change, DI awardees only consist of always types. If there were no before-types in the analysis, the naive RD estimates would be valid estimates of the causal effects of the reform. Before types are assumed to have higher average potential outcomes than always types, implying, as I have argued above, that a naive RD analysis of the effects of the difference in financial incentives at the cutoff should be interpreted as an upper bound of the effect for the always-types. To estimate the true causal effect for the always types, these must be separated from the before types among those that were awarded DI benefits before the regime change, i.e. limiting the analysis to the region of common support. To do this, I use a propensity score matching strategy. The aim of this strategy is to adjust the distribution of observables of treated and untreated recipients toward a target population[17], here the always types. The always types are well-defined in the sample of untreated, who were awarded benefits after the regime change, toward which I want to adjust the sample of treated, who were awarded benefits prior to the regime change. The propensity score is thus defined as the probability of not receiving treatment. To identify always-types among treated DI recipients, I match the sample using a nearest neighbor propensity score matching approach, where closest matches for the untreated sample are chosen without replacement from the treated sample. The nearest neighbor matching approach is more likely to avoid bad matches and thereby adjust the sample in terms of both observables and unobservables than, i.e. the IPTW technique proposed by Linden & Adams (2012), which gives some weight to all observations.

This matching approach instead assumes unconfoundedness within the target population of always types. With respect to one particular covariate, the forcing variable, this assumption always fails in RD-type situations. With the RD framework, individuals will be compared that are awarded DI quite close in time, reducing the bias that could arise from these effects. The trade-

[17] The target population concept was introduced by Lechner & Wunch (2009), who match participants and non-participants in labor market programs over time to analyze the effectiveness of these programs over a 10-year period.

off is between balance in the forcing variable and other covariates. My matching approach is, for this reason, also restricted to observations close to the cutoff. The identifying assumption is that, within a tight region around the cutoff, the forcing variable is ignorable given other observables, as stated in (3). This means that, unless we can assume constant treatment effects over the range of the forcing variable, what I estimate is a local average treatment effect (LATE). In my case, this means that the estimated treatment effect is valid for those who were awarded permanent DI-benefits in the middle of 2008 on outcomes in years thereafter, while the effect might be different for people who had been DI-recipients for a longer time when the reform was introduced. Date of award of permanent DI-benefits can directly affect outcomes in two ways. First, more time spent away from the labor market can make the return to work more difficult, for example due to depreciation of human capital. The time away is however not deterministically determined by the forcing variable since most beneficiaries have been on other forms of sickness leave before being awarded permanent DI-benefits. In fact, due to the stricter criteria for DI-benefits enforced at the same cutoff, total time spent away from the labor market on sick leave is on average longer for the control group. Second, the decision to award permanent DI-benefits takes into consideration the prospects of returning to the labor market at the time of award. What it of course cannot take into consideration is future innovations that improve these prospects, for example new work aids. The possibility of such innovations that increase the possibility of working with a permanent impairment might induce an opposite effect on outcomes from time since award of permanent DI-benefits.

4.2.3 Plausibility of the matching approach

In principle, the matching estimator requires the same assumptions as OLS. If the unconfoundedness assumption holds, regression adjustment would suffice to produce unbiased results. If we are worried about selection on unobservables, we need a more sophisticated method like the RD which has a greater credibility in providing an as good as randomized treatment assignment. Here, since unconfoundedness is not fulfilled, the matching estimator can more efficiently adjust for covariate imbalances by only comparing comparable individuals. The strategy however relies on the selection on observables assumption, at least within a region, as specified in the local unconfoundedness assumption (3).

In my case, unobservables I might worry about include trends in the selection into permanent DI. Unobservable changes in e.g. administrative norms, or the quality or importance of medical testimonials etc. may have occurred over time. A large time span brings variety in political majorities, with different views on the generosity of public insurance schemes which might affect caseworkers' decisions. Such effects could cause a selection on unobservables problem. Restricting the analysis to a tight region around the cutoff reduces these concerns. It also reduces variability of the forcing variable, for which we have no overlap between treated and controls.

For the local unconfoundedness assumption to hold, it must also be assumed that differences in the selection into DI under the different regimes are not determined by unobservables. This selection is determined by the change in eligibility criteria, selection by case-workers, and self-selection. The availability of rich data related to the changes in eligibility criteria makes this assumption more plausible. Case-workers of course have some discretion under both regimes and observe factors that are difficult to observe by the researcher, such as for example motivation. I use rich data on both health indicators and other basic characteristics, together with detailed information about labor market histories, which should provide a good proxy for such factors. Caliendo et al. (2017) show that, even though usually unobservable variables[18] matter for selection into, in their case, labor market programs in Germany, these variables do not make a significant difference in the estimation of the treatment effects of these programs when detailed administrative data are available. This is particularly true when observable information is used that is correlated with the unobservable variables of concern, as is labor market histories when evaluating program effects on labor market outcomes.

I have argued that self-selection in order to receive the treatment that is being studied is unlikely, due to the retroactive determination of the eligibility criteria for treatment. Self-selection due to the regime change in DI eligibility is, however, even probable, and may be related to the potential outcomes from the treatment. The heap in the number of awards just before the cutoff, at least for fulltime recipients, indicates this. According to the Swedish Social Insurance Agency, the peak reflects a surge in case-worker output, which means that selection at the peak should be made on the same grounds as before. This explanation is likely to be true for the peak in July. Case-worker initiated transfers are always executed the month after the transfer is decided. The peak in June, however, is more likely due to self-selection. Retroactive applications to be awarded DI benefits for June 2008 were possible three months ahead, and there was a peak in DI applications in September 2008, suggesting that individuals were trying to self-select into DI on grounds of the old regime. I will show that observable characteristics are clearly different for the sample awarded benefits in June 2008 compared to both before and after, suggesting that this is the case. Consider the presence of a third type of DI recipient, a "heap-type", with different average potential outcomes than both always- and before-types. This violates the continuity assumption further. Barreca, Lindo & Waddell (2011) argues that

[18] Unobservable variables examined include personality traits, expectations about the relevant treatment, labor market flexibility, intergenerational information, social networks and life satisfaction (Caliendo et al. 2017).

the most robust alternative is to use what is referred to in the literature as a "donut-RD" approach (e.g. Barreca et al. 2010, Almond & Doyle 2011, Bajari et al. 2011) in such cases, i.e. excluding observations at the peak. This assumes that the potential outcomes at the peak, without the presence of the heaped types can be extrapolated using adjacent points (Eggers et al. 2015, Angrist & Rokkanen 2015)[19]. If the self-selection is made on the grounds of observables, the matching strategy should of course already take care of this problem. I will also show that results are more similar with and without excluding observations at the peak with the matching approach than with the naive RD estimator. However, it is more likely that selection on observables is not sufficient to single out heaped types than before types, especially with respect to those awarded in June 2008, since unobservables such as motivation are more likely to differ among self-selectors than when selection is made by case-workers. In the main analysis, I therefore exclude the data at the peaks. In the main specification, I calculate propensity scores using observations three months from the cutoff on each side, after excluding recipients awarded DI in June and July 2008.[20] To produce the main RD estimates, I use a triangular kernel model with the same bandwidth around the cutoff.[21] Standard errors are clustered on municipality.[22] The matching model matches nearest neighbors for untreated without replacements, and I trim the sample to increase the common support by dropping control observations whose propensity score is higher than the maximum or lower than the minimum propensity score of the treated. I use heteroscedasticity-consistent analytical standard errors proposed by Abadie & Imbens (2006).

[19] Excluding recipients awarded DI-benefits in July 2008 is necessary also for another reason. A special rule was added by the parliament to the Government bill stipulating the special rules for continuous deduction, that benefit spells decided in June but starting in July 2008 (i.e. case-worker transfers made in June 2008), also qualifies the recipients for the rules of continuous deduction. This special rule means that it is unclear from the data what recipients awarded permanent DI from July 2008 onwards are treated and which are not, since the data do not contain information on what date the decision to award benefits was made. The fact that the treatment status of these observations is unknown is another reason I must exclude spells of permanent DI that start in July 2008, besides the peak in inflow.

[20] This is in agreement with excluding these in the estimation model. I am thus using awarded in March, April, and May as well as August, September and October for the main specifications.

[21] There are some data-driven methods to find optimal bandwidth sizes. The optimal bandwidth size according to, for example, Imbens & Kalyanaraman (2012), varies between 1.4 and 22 months across the outcome variables in this study. I have chosen the baseline bandwidth of three months, as a trade-off between precision and balance of the forcing variable, and I investigate the sensitivity of my results to the choice of bandwidth in section 6.1.

[22] Card & Lee (2008) suggest clustering standard errors on distinct values of the forcing variable when the forcing variable is discrete. However, in my application the number of clusters within the bandwidth is very small. There could be regional correlation of the error term due to regional differences in DI award and labor demand, which is why standard errors are clustered on municipality of residence.

5 Data

This study is based on administrative data. Administrative records from the Swedish Social Insurance Agency with information about social insurance spells and benefit types from the MiDAS database is used to identify permanent disability recipients and their sickness absence histories. Data from the Social Insurance Agency on diagnoses that beneficiaries are awarded permanent DI-benefits for are not available in the data. Instead analogous data is collected from the National Patient Register and the Prescription Drug Register from the National Board of Health and Welfare. The Prescription Drug Register contains information about all pharmacy collected drug prescriptions from July 2005, including drug type.[23] The National Patient Register contains information about all concluded inpatient care events, admissions to geriatric and psychiatric care and compulsory psychiatric care, acute outpatient care events, and doctors' treatments from outpatient care not categorized as primary care. I observe ICD-10 diagnose code category[24] for the main and secondary diagnoses for each observable care event. An advantage of using historical diagnose information from the National Patient Register is that these data are less likely to be affected by the changes to the DI-criteria in 2008. A diagnosis from at least one care event is available for 95 percent of the main sample; those awarded permanent DI within a three month bandwidth from the cutoff. The distribution of diagnoses in the data used is in line with statistics on diagnoses for disability insurance recipients published by the Swedish Social Insurance Agency. Table 1 compares the rank-

[23] The drug prescription data follows the Anatomical Therapeutic Chemical (ATC) Classification System and separates between the anatomical main group (first level ATC-codes): alimentary tract and metabolism (A), blood and blood forming organs (B), cardiovascular system (C), dermatologicals (D), genito-urinary system and sex hormones (G), systemic hormonal preparations, excluding sex hormones and insulins (H), antiinfectives for systemic use (J), antineoplastic and immunomodulating agents (L), musculoskeletal system (M), nervous system (N), antiparasitic products, insecticides and repellents (P), respiratory system (R), sensory organs (S), and Various (V).

[24] The ICD-10 categorization in the data separates between certain infectious and parasitic diseases (A00-B99), neoplasms (C00-D48), diseases of the blood and blood-forming organs and certain disorders involving the immune mechanism (D50-D89), endocrine, nutritional and metabolic diseases (E00-E90), mental and behavioral disorders (F00-F98), diseases of the nervous system (G00-G99), diseases of the eye and adnexa (H00-H59), diseases of the ear and mastoid process (H60-H95), diseases of the circulatory system (I00-I99), diseases of the respiratory system (J00-J99), diseases of the digestive system (K00-K93), diseases of the skin and subcutaneous tissue (L00-L99), diseases of the musculoskeletal system and connective tissue (M00-M99), diseases of the genitourinary system (N00-N99), pregnancy, childbirth and the puerperium (O00-O99), certain conditions originating in the perinatal period (P00-P96), congenital malformations, deformations and chromosomal abnormalities (Q00-Q99), symptoms, signs and abnormal clinical and laboratory findings, not elsewhere classified (R00-R99), injury, poisoning and certain other consequences of external causes (S00-T98), transport accidents (V00-V99), external causes of morbidity and mortality (V01-Y98), and factors influencing health status and contact with health services (Z00-Z99). The data also includes more specific information (undercategories) for the two most common main categories, musculoskeletal (M00-M99) and mental (F00-F98) diseases.

ing of the ten most common diagnoses (plus “Other”) according to the Swedish Social Insurance Agency (SSIA) official statistics for new awards of disability insurance 2006 with the ranking of these diagnoses according to the diagnose with most care time for each awarded permanent DI recipient 2006 from the National Patient Register (NPR). Apart from the “Other”-category, rankings are the same for the first five categories. The four remaining categories do not have the same exact ranking but their shares in the NPR data are quite similar. One diagnose category is not singled out by the NPR data however, neoplasms.

Table 1. *Most common diagnoses, SSIA vs NPR data*

	SSIA, percent	SSIA, rank	NPR, percent	NPR, rank
Diseases of the musculoskeletal system and connective tissue	41.1	1	17.7	1
Mental and behavioral disorders	26.7	2	12.6	2
Diseases of the circulatory system	7.7	3	9.8	3
Injury, poisoning and certain other consequences of external causes	5.3	4	7.8	4
Diseases of the nervous system	3.8	5	4.8	5
Neoplasms	2.7	6	–	–
Endocrine, nutritional and metabolic diseases	2.3	7	2.7	8
Diseases of the respiratory system	2.0	8	2.7	7
Diseases of the ear and mastoid process	1.6	9	2.5	9
Diseases of the eye and adnexa	0.5	10	3.3	6
Other	6.1		38.1	
Sum	99.8		102.0	

Note: Source for the SSIA ranking and shares is Ds 2008:14. ”Burnout and similar” shares have been added to mental and behavioral disorders in this table. The SSIA shares and ranking includes all new recipients of DI above 30 years of age. It thus includes temporary DI, while the NPR data only includes new recipients of permanent DI. NPR shares sum to more than 100 percent since recipients with two diagnoses with the same care time are counted in both shares. SSIA does not sum to 100 percent, most likely due to rounding.

The data also contain a rich set of background characteristics and outcome measures from Statistics Sweden. I study short- to middle-term outcomes as data is available up to 2013, five years from program start. I study labor supply outcomes on the extensive and intensive margin. The extensive margin is examined as whether an individual is working at all in either of the follow-up years. This is defined as having a positive income from work either of these years. Effects on the intensive margin is measured as having earnings above the individual earnings disregard[25] in the years following program start, and is also indicated by total earnings in these years. Since it is also possible to conduct studies and do volunteer work within the continuous

[25] The earnings disregard level for each individual according to their benefit extent at program start.

deduction program, I also study the effect of the continuous deduction program on increasing ones educational level since program start.[26]

To construct the sample, I use spells of permanent DI that were ongoing at program start, in January 2009. Some individuals have multiple separate spells of permanent DI in the data, and I only use the first spell of each individual. The forcing variable used for the regression discontinuity is the date of award for permanent DI. Disability benefits are awarded monthly and the cutoff is July 2008 since those awarded prior to that month are eligible while those awarded thereafter are not.

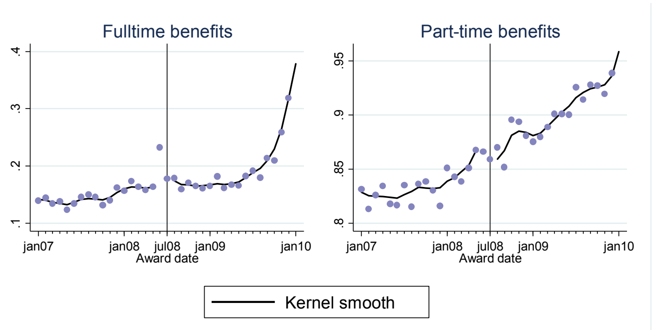
5.1 Graphical evidence

One of the advantages of the RD-design is that it provides a way to clearly illustrate the results graphically. In this case, as the RD-design needs to be combined with matching the observations with respect to health and other characteristics to estimate the causal effect of the program, the graphs in *Figure 4* should illustrate an overestimate of the ATT. *Figure 4* shows the total effect over the years after program start up to 2013 for the four separate outcomes[27]. The graphs in panel (a)-(c) measure labor supply effects. The graphs in panel (a) illustrate the effect of the program on the extensive margin, i.e. earning any income during these years. As shown in the graphs, around 16 percent of fulltime recipients work either year while above 80 percent of part-time recipients do.[28] The share with positive earnings either year is lower, around 8-10 percent respectively, for fulltime recipients. Panel (b) shows the labor supply outcome on the intensive margin, as the share of beneficiaries earning more than the individual earnings disregard at least one of the years 2010-2013. This share is lower than the share working at all for both part-time and fulltime recipients, around 22 and 5 percent respectively.

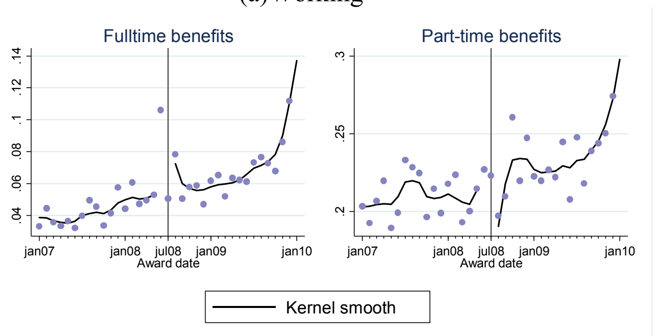
[26] The data provides information on highest education level on December 31 each year. Highest education level is reported as compulsory education up to nine years, compulsory education at least nine years, high school education, post-secondary education up to two years, post-secondary education at least two years, and graduate education.

[27] All labor supply outcomes are measured as sums from 2010 onwards. The reason for this is that, due to long processing time for each application, there is a chance that many of the awards from 2008 were not decided before the start of 2009. The average processing time for new DI-applications (temporary and permanent) during 2004-2009 was 120 days (Sjögren Lindquist & Wadensjö 2011). This could affect the labor supply 2009 and thereby the results if 2009 is included in the combined outcome variable. For example applicants could decrease their labor supply intentionally before their application is processed to "prove" their lack of working capacity to the case-worker to affect the outcome of the application. Including the outcomes in 2009 does not change the overall conclusions, however.

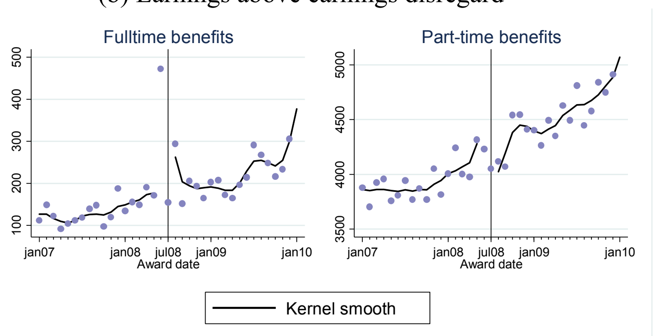
[28] The reason for the rising share working at the end of the period in the graph for fulltime recipients is most likely residual payments in the beginning of 2010 for work conducted before the award of DI-benefits. This is supported by graphical illustrations of the outcome each year separately. The graph starts moving upwards closer to the cutoff in plots of outcomes 2009 and is not visible in plots with the same range on the x-axis for outcomes year 2011, 2012 or 2013.



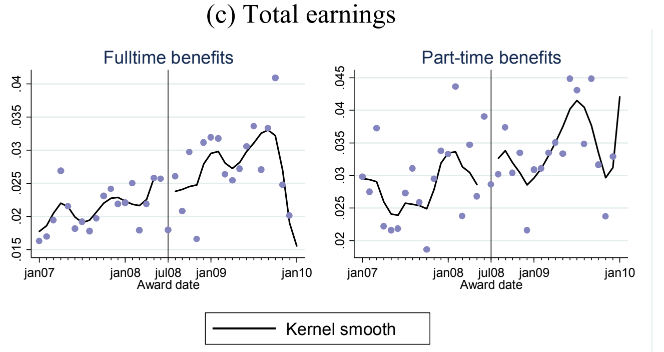
(a)Working



(b) Earnings above earnings disregard



(c) Total earnings



(d) Increase in education level

Figure 4. Outcomes by award date

Total earnings during 2010-2013 in hundreds of SEK is shown in panel (c). The amount is around SEK 20,000 over a four-year-period for fulltime recipients, which is around four percent of the average benefit amount, showing the extent of work for those with positive income is on average very low. The graphs in panel (d) illustrate the effect on the level of educational attainment.

The graphical illustrations show no clear jump around the cutoff in these outcomes for either fulltime or part-time recipients. Note, however, that the labor supply and average earnings of recipients awarded permanent DI-benefits from June 2008 onwards is substantially larger than that of recipients awarded benefits both before and after the cutoff. This implies that something affects the labor supply of recipients awarded permanent DI from June 2008 onwards that is not necessarily attributed to the continuous deduction program. This coincides with the increase in inflow to permanent DI which peaks in June for fulltime recipients.[29] A possible explanation is self-selection and/or that case-workers were less restrictive just before the regime change, so that individuals awarded benefits from June 2008 onwards are of better health than those awarded both before and after. Due to the peak in inflow, observations for awarded in June and July are excluded in the estimation of the main results.

5.2 Covariate (im)balance

As discussed above, changes to the eligibility criteria for permanent disability insurance induces imbalance in important characteristics between treated and untreated within the bandwidth. Table A.1 shows how observed characteristics differ around the cutoff for full- and part-time recipients, respectively.

It is clear from the table that significant differences between the groups are present in many respects. Treated fulltime recipients are on average one year older than untreated, and have 0.2 years less education. They are also more often married and have on average 0.06 fewer children. Treated also seem to have better labor market histories; their average previous income is around five percent higher than untreated and they have 0.6 years longer labor market experience (the latter could, however, be explained by the difference in age). Treated also face worse labor market conditions in terms of local unemployment rates at program start.

Previous sickness absences confirm that individuals awarded permanent DI after the cutoff are of worse health than those awarded prior to the changes in eligibility. The length of the sickness absence spell is often used as an indicator of health status. Treated individuals have shorter previous sickness absence in total as well as with respect to the current sickness spell. The

[29] And in July 2008 for both full- and part-time recipients.

average length of the current sickness spell for treated fulltime recipients is 1,457 days compared to 1,575 days for untreated; or around four months shorter than the average spell length for untreated of approximately four years and four months. Treated fulltime recipients have also spent a shorter time on fulltime sickness benefits directly prior to award of permanent DI, and more were receiving sickness benefits before award, while more untreated were receiving temporary DI-benefits before the award of permanent DI-benefits.

When it comes to the diagnose data, there are few differences between treated and controls. Diagnoses for which the individual has spent most time in care, which should capture the cause for sickness absence, displays two clear differences – treated are less often mainly diagnosed with mental or behavioral disorders (F00F98).

For part-time recipients similar patterns are observed, however fewer significant differences are observed than between treated and untreated fulltime recipients. Treated part-time DI-recipients are on average 0.7 years older and have 0.3 years shorter education than the untreated. Treated part-time recipients, like fulltime recipients, seem to have better labor market histories, but for part-time recipients the only significant difference is that they have on average around a third of a year longer labor market experience, and they also face worse labor market conditions at program start. Treated part-time recipients are in better health than untreated part-time recipients according to previous sickness absence – they have around five months shorter total sickness absence and a four months shorter current spell, also with the same extent of benefits, when permanent DI is awarded. Like fulltime recipients, a larger share of treated versus untreated were receiving sickness benefits versus temporary DI-benefits, respectively, before award, and according to diagnose data, treated part-time recipients are less often diagnosed with mental or behavioral disorders (F00F98).

Since the eligibility criteria for permanent DI were made stricter from July 2008, I expect the differences in characteristics between treated and controls to create an upward bias in the estimation of the effects using the regression discontinuity approach. Health characteristics often used to summarize health status, such as length of previous sickness absence, support this interpretation, and previous labor market outcomes also point to the treated being of better health or having better labor market prospects than the untreated in the sample. Evidence shows that DI-recipients with shorter DI-spells more often return to the labor market. Returnees are also mainly part-time recipients. (Jans 2007). There are however some other differences that could point towards bias in the opposite direction. Since the changes in eligibility criteria also removed the opportunity for case-workers to take some other characteristics into account than health when awarding DI, differences between treated and untreated are observed that could instead cause a downward bias to naive RD estimates. Some observed differences are in line with case-workers being more lenient in awarding DI before the cutoff with respect to characteristics that could affect labor market prospects negatively but are not directly related to health status. For instance, previous evidence on labor force returns of Swedish DI-recipients implies that being older, married, and less educated is associated with a lower probability of returning to the labor force. Also, mental and behavioral disorders are associated with a greater chance of returning to work (Jans 2007). If health differences are less important than imbalances in other characteristics, this might balance out or outweigh the upward bias caused by the health differences. Combining the RD with matching on observed characteristics serves to smooth these imbalances.

Discontinuity plots of some of these differences are shown in *Figure A.1*, including variables describing basic characteristics as well as labor market and sickness absence histories. The five most common diagnose categories (diseases of the musculoskeletal system and connective tissue, mental and behavioral disorders, diseases of the circulatory system, diseases of the nervous system and injury, poisoning and certain other consequences of external causes) are plotted. These show the same patterns as described in this section.

5.3 The propensity score

The matching method aims to identify comparable units on both sides of the cutoff, i.e. identifying the always types, to estimate causal effects of the continuous deduction program for these. Units should ideally be matched along all dimensions that matter for the outcome. In this study, I observe a rich set of background characteristics. Basic characteristics such as age, gender, education etc., as well as previous labor market outcomes and sickness absence are discussed in the previous section, as well as some previous diagnosis indicators. In addition to these, I have access to more detailed diagnosis data and data on drug prescriptions. I also observe region of residence and educational orientation.

To match directly on all these characteristics would most likely yield zero matches. Propensity score matching is a matching method that solves this problem. The propensity score is an index variable that measures the probability of being treated given the observed characteristics. Rosenbaum & Rubin (1983) have shown that if potential outcomes are independent of treatment conditional on a set of observed covariates, potential outcomes are also independent of treatment conditional on the propensity score based on these covariates. For the propensity score, aside from characteristics from the descriptive statistics table in the previous section, I use dummies for each diagnose category from the National Patient Register, specifying whether such a diagnose has ever been determined for the individual (main or secondary) as well as whether it has been determined at an admission the last twelve months or five years before award of permanent DI. The most common diagnoses for permanent DI-recipients are musculoskeletal and mental disorders. An interaction term between these is included, for both the full time span as well as the last five years. To include a measure of the severity of the illness, I use, as indicators, care time with each diagnose as the main diagnose, in the full time span as well as the last five years before award. I also include an indicator of which diagnose category individuals have spent the most time in care with as the main diagnose in these time spans. I use information on drug prescriptions, by dummies for having been prescribed drugs of each main drug type since July 2005 at award.

Since the month of DI award completely determines treatment, it cannot be included in the calculation of the propensity score. However another indicator of time spent away from the labor market is included; total days on sick leave prior to DI award. I calculate propensity scores separately for treatment for full- and part-time recipients, using the same observations as for the main RD specification bandwidth, i.e. awarded in March-May and August-October 2008. The density of the propensity score for treated and untreated is shown in *Figure A.2*. I use nearest neighbor propensity score matching without replacement to find matches for untreated and thereby receive a comparable sample. *Figure 5* illustrates the discontinuity in the propensity scores caused by the change in DI-criteria at the cutoff, and the smoother discontinuity of the matched sample.

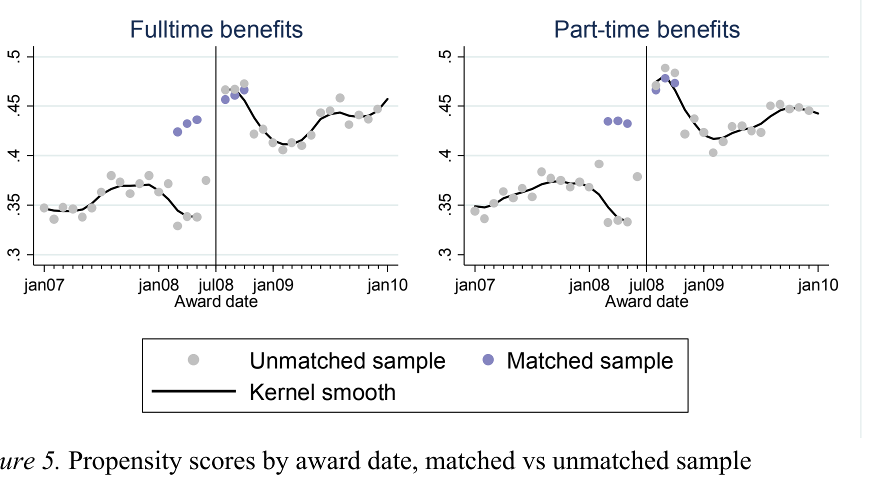


Figure 5. Propensity scores by award date, matched vs unmatched sample

A similar table as Table A.1 for the matched sample is found in Table A.2. It shows that in the matched sample there are no significant differences between treated and controls, except for the month of award, which is impossible to balance because of the institutional setup of the program. Only one other variable is, weakly, significant for part-time recipients; total days of sickness absence before award.[30]

However, within the three month bandwidth, observations are distributed so that not all characteristics are balanced just at the cutoff, even in the matched sample. Table A.3 shows RD estimates of pre-treatment characteristics for the unmatched and the matched sample. While the balance at the cutoff is better within the matched sample, some variables are significantly discontinuous also within the matched sample. For fulltime recipients, basic characteristics and most previous labor market outcomes, as well as sickness absence histories, are balanced at the cutoff in the matched sample. According to the RD estimates, the number of consecutive days with fulltime benefits is significantly discontinuous at the cutoff with the matched sample. A few diagnose- and drug prescription dummies are also not balanced at the cutoff. For part-time recipients, the same is true for diagnose and drug prescriptions for which there are some significant differences at the cutoff. This is due to the fact that the matching approach does not balance the forcing variable. The RD versus matching approaches means a trade-off between balance in the forcing variable and other unit characteristics.

6 Results

Table 2 shows the regression results for fulltime DI-recipients. Neither of the two models indicates any significant effect on any outcomes. The estimates from the propensity score nearest neighbor matching model in column 2 are close to zero and insignificant. The naive RD model in column 1 shows the upper bound of the effect of the continuous deduction program, under the assumption that potential outcomes depend negatively on the reduction in working capacity. In this model, the point estimates for the effects on the labor supply related outcomes are even negative, although the 95 percent confidence intervals are quite broad.

Panel A in *Figure A.3* plots estimates and 95 percent confidence intervals for each year separately for all four outcome variables studied. These plots show that there is no trend in the effect over time. The nearest neighbor matching estimates are not significant in any year for the effect on labor force participation or change in education level. The intensive margin outcomes, earnings above the earnings disregard and total earnings, are significantly negative in 2009, but not the following years. As previously argued, the outcomes in 2009 might be affected by processing times and should

[30] Due to space limitations, Table A.2 does not include all variables used for the calculation of the propensity score, only the most important indicators. The analysis of all variables used for the propensity score shows that there are no significant differences at the 95 percent level in the matched sample.

therefore be interpreted with more caution. The naive RD estimates, which should provide an upper bound of the effect, are not significantly different from zero for any outcome in any year from 2009 to 2013.

Table 2. *Main results for fulltime recipients*

Outcome	(1) RD	(2) NNM
Working	-0.013	0.004
	(0.024)	(0.011)
Earnings above the earnings disregard	-0.016	-0.002
	(0.014)	(0.007)
Total earnings	-81.004	6.174
	(58.541)	(30.421)
Increase in education level	0.005	-0.005
	(0.009)	(0.004)
Observations	7,250	4,940

Note: Each cell represents the result from a separate regression, with each row showing the regression discontinuity (RD) and propensity score nearest neighbor matching (NNM) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Since part-time recipients are expected to work on the part without benefits, and this part is not affected by the continuous deduction program, I do not expect the program to have an effect on the extensive margin for this group. It is however probably easier for part-time recipients to increase their labor supply, if they are already working on the part without benefits and thereby have an employer with whom it is probably easier to increase working hours than it is to find work as a non-working DI recipient.

Table 3. *Main results for part-time recipients*

Outcome	(1) RD	(2) NNM
Working	0.038	-0.013
	(0.024)	(0.010)
Earnings above the earnings disregard	0.038	0.001
	(0.032)	(0.013)
Total earnings	646.963**	21.537
	(302.742)	(118.593)
Increase in education level	-0.005	-0.002
	(0.014)	(0.005)
Observations	5,455	4,044

Note: Each cell represents the result from a separate regression, with each row showing the regression discontinuity (RD) and propensity score nearest neighbor matching (NNM) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

The results in Table 3 show that neither the nearest neighbor matching model, nor the naive RD model, suggests any significant effect on labor supply at the extensive margin for part-time recipients. The results from the matching model in column 2 do not show any significant effects on either outcome. The upper bound of the effect from the naive RD model, however, shows a positive effect on total earnings in 2010-2013.

Separate regressions for each year for these outcomes, plotted in panel B in *Figure A.3*, show that the naive RD effect of labor supply has a positive trend over time and is significantly positive the second half of the follow up period for all labor supply outcomes. The results from the matching model, however, show that there is no effect for the always types. Estimates for all three labor supply outcomes are close to zero and insignificant all years. Neither model suggests any effect on educational attainment.

These results suggest that the continuous deduction program has not had any effect on labor supply or educational attainment, for either full- or part-time DI recipients. The upper bound RD estimates on the effect on labor supply is negative but insignificant for fulltime recipients, and while the upper bound estimates for the effect on total earnings is positively significant for part-time recipients, and there is a positive trend in the labor supply effect over time, the matching results, estimated to come closer to the true causal effect for the always types, show that there is no effect from eligibility to the program for part-time recipients either.

6.1 Robustness analysis

Two other approaches to finding the effect for always takers proposed in the literature is to simply include covariates to the naive RD model, and to perform the usual RD analysis within the matched sample. I show results from these two approaches, for both full- and part-time recipients, in Table 4. The drawback of the covariate adjustment approach is that it is unclear how this method performs with respect to achieving balance, especially in this case when there is a lack of common support in important characteristics across the cutoff. Performing the usual RD analysis within the matched sample could be an attractive alternative since the propensity score matching model is unable to balance month of award within the bandwidth. If the timing of award is important even within this reasonably close region of the forcing variable, the lack of balance may bias the matching results, and RD within the matched sample provides better balance with respect to the forcing variable. However, since the nearest neighbor matching model does not provide balance of the forcing variable, it does not necessarily balance all characteristics included in the estimation of the propensity score right at the cutoff, only within the bandwidth used as a whole. It turns out, as I have shown, that there are imbalances at the cutoff with respect to characteristics other than the forcing variable. The choice between the propensity score matching model and RD within the matched sample is thus a trade-off between balance in the forcing variable, even within the bandwidth close to the cutoff, and balance in other covariates. Since limiting the sample to recipients awarded DI within a small region around the cutoff provides small differences in the forcing variable, the imbalance in other covariates at the cutoff should yield more bias to the RD estimates using the matched sample than the baseline nearest neighbor matching model. The results show that both the RD model with covariate adjustment and RD within the matched sample model yield estimates that are closer to the naive RD estimates, with negative but insignificant estimates for the effect on labor supply for fulltime recipients, and positively significant estimates for both intensive margin outcomes for part-times recipients.

Table 4. *Results using alternative models*

Outcome	(1) RD with covariates	(2) RD within NNM sample
Panel A. Fulltime recipients:		
Working	-0.009	-0.011
	(0.027)	(0.037)
Earnings above the earnings disregard	-0.021	-0.018
	(0.015)	(0.019)
Total earnings	-64.671	-90.775
	(66.736)	(80.620)
Increase in education level	-0.004	0.004
	(0.011)	(0.013)
Observations	6,557	4,940
Panel B. Part-time recipients:		
Working	0.015	0.021
	(0.024)	(0.029)
Earnings above the earnings disregard	0.058*	0.101***
	(0.031)	(0.038)
Total earnings	563.912**	636.345*
	(243.265)	(368.855)
Increase in education level	-0.009	-0.000
	(0.012)	(0.020)
Observations	5,358	4,044

Note: Each cell represents the result from a separate regression, with each row showing the results from a regression discontinuity model with covariate adjustment and the regression discontinuity results within the matched sample, for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

In the estimation of the main results I have excluded observations close to the cutoff through the so called “donut” RD approach. Individuals awarded DI benefits from June and July 2008 onwards may be systematically different since there is a peak in the inflow these months. There is a peak for fulltime recipients both months, while mostly in the later for part-time recipients. Another reason for excluding individuals awarded DI benefits in July is that it is unclear from the available data which of these are in fact eligible for continuous deduction. I have investigated the sensitivity of my results to the inclusion of these observations. The results when reducing the donut to only July are presented in Table A.4. The results show that, for fulltime recipients, the nearest neighbor matching model is less sensitive to the inclusion of individuals awarded DI in June 2008 than the naive RD model. The estimates for the effect on labor force participation and having earnings above the earnings disregard are positive and larger than the main estimated but not significant. There is, however, a significantly positive effect on total earnings also with the matching model, although the point estimate is much smaller than with the RD model. The naive RD, or upper bound, estimates, on the other hand, suggest that the continuous deduction program has a strong positive and significant effect on labor supply on both the extensive and intensive margin, while not on educational attainment. The results from this model are thus, as expected, very sensitive to the outlier values in June. This confirms that awardees from June 2008 are systematically different and have stronger working capacities than awardees both before and after. The selection on observables assumption is less likely to hold with respect to the previously excluded June-observations, and therefore the matching model is also expected to perform poorer when these observations are included, which may explain the difference compared to the main results also with the matching model.

For part-time recipients, the peak in inflow is prominent only in July. Including June in the estimation also does not affect the results as much for this group. This is in line with the hypotheses that the peak in June for fulltime recipients is due to self-selection with respect to the DI eligibility changes, while the peak in July consists of case-worker initiated transfers that may not be subject to as large differences in characteristics. The nearest neighbor matching model shows no significant effect on any outcome, as in the main results. The naive RD results again suggest an effect on labor supply on the intensive margin.

Since it is unclear from the available data which recipients awarded permanent DI from July onwards are eligible for the continuous deduction program, it is not clear on which side of the cutoff these should be included. To test the sensitivity of the results, I have included these observations on either side of the cutoff in separate regressions. The nearest neighbor matching model shows no significant effects and the point estimates are close to zero when July is treated as treated, while the effect on total earnings is significant (as above) when July is treated as untreated. Including the July-awards as treated yields significantly negative estimates of the effect with the naive RD model for fulltime recipients, while including them as untreated yields positively significant estimates as above, again because of the June outlier. For part-time recipients, there are no significant estimates with either model when July is also included, regardless of on which side of the cutoff.

A common robustness check when regression discontinuity designs are used is to change the bandwidth and see what happens to the results. The results from this exercise are shown in *Figure A.4*. The plots show results for bandwidths of 2 to 16 months, and their 95 percent confidence intervals are visible as dashed lines. The propensity score nearest neighbor matching model is close to zero and insignificant with bandwidths up to around six months for fulltime recipients, and becomes increasingly negative, even significantly so, as the bandwidth increases further. Since this model does not provide balance to the forcing variable, the differences in time since DI award, increases with the bandwidth. As time away from the labor market is negatively correlated with labor market prospects, difference in the time away between treated and untreated in the matching model is a likely explanation for the increasingly negative effects estimated as the bandwidth is increased. For part-time recipients, the same pattern is observed, but while the effect estimated by the matching model is closer to zero for the education and earnings above the disregard outcomes, it turns negative with quite small increases in bandwidth for the effects on labor force participation and total earnings. The naive RD estimates go toward zero when the bandwidth is increased, for both full- and part-time recipients, for all outcomes. Note that the significant effect on total earnings for part-time recipients using the baseline bandwidth is insignificant when the bandwidth is increased even by one month.

Another robustness check is to exclude the oldest individuals from the sample. These have often been excluded in previous studies, to make sure that the results are not driven by individuals close to retirement or those who have retired when the outcomes are measured. To check robustness in this dimension, I have excluded all individuals above the age of 61 within the follow-up period in the results shown in Table A.5. Excluding these observations does not change any conclusions.

I have also estimated results for a number of placebo cutoffs. If the cutoff in the RD model is set where there was no reform, the results should show no effect. *Figure A.5* plots such results from the regular RD model with each month from January 2006 to December 2012 used as cutoffs. No donut hole is used in these RD estimations. We see large and significant estimates using June, July and August 2008 as cutoffs for fulltime recipients. The evidence provided in this study shows that this is due to compositional differences accompanying the increase in the inflow in June and July 2008, and the corresponding decrease the month after. For part-time recipients, compositional differences at the cutoff where not as prominent, and consequently there are no peaks in effects estimated around the true cutoff for this group as for the fulltime recipients. The horizontal lines in each plot at the original cutoff show the main results on the outcome for the naive RD model (i.e. the "donut" RD model), and the dotted horizontal lines show the 95 percent confidence intervals. These are included in the plot to show how my baseline results relate to changes at other, placebo, cutoffs. This analysis shows that my upper bound RD estimates are not larger or more or less precise than estimates obtained when using these placebo cutoffs. This supports the conclusion that there was no effect from the reform.

If important characteristics are balanced, an analogous analysis to the main results for the years preceding program start should show no effect from the treatment. As a last robustness check, I examine whether the two models show any effect on two types of outcomes, referring to labor supply on the extensive and intensive margin, for five years preceding the introduction of the continuous deduction program. Estimated effects from the program on working and having an income above the earnings disregard, in 2004-2008, are shown in Table A.6. For fulltime recipients, all the results from the naive RD model are positive, and the estimate is significant with respect to working in 2007, and weakly significant for working in 2004 and having earnings above the earnings disregard in 2005. This suggests that imbalances in characteristics result in an overestimate of the effect from the program using the naive RD model, as I expected. The nearest neighbor matching model seems to smooth these differences. These estimates are close to zero and insignificant, except for having earnings above the earnings disregard in 2007 and 2008, where the estimates from the matching model are even weakly significantly and significantly negative, respectively. This may suggest some overcorrection by the matching model, but since both models show no effect of the reform, the conclusions are nonetheless straightforward. For part-time recipients, there are two positively significant estimates with the naive RD model, working in 2005, and weakly so for working in 2004. Most estimates are positive, although there are some negative but insignificant estimates. With the nearest neighbor matching model, however, there are no significant differences between the treatment and control group, and estimates are generally close to zero. This suggests that the naive RD estimates may be an overestimate of the effect within this group as well, while treated and untreated are more similar with the matching model.

6.2 Heterogeneity analysis

To investigate whether the average impacts presented above hide some effects for responsive subgroups, I have conducted a subgroup analysis. The results are presented in Tables A.7-A.11. The fact that I do not find any positive effects from the continuous deduction program could be associated with the age distribution of those awarded permanent DI in Sweden. 40 percent of those eligible were above the age of 60 at program start. Earlier studies of similar interventions find larger effects for younger DI-recipients. For example, Kostøl & Mogstad (2014), find no effect in the age group 50-61 year olds. Conducting the analysis separately for different age-groups, however, does not show that there is a stronger response to the continuous deduction program among younger recipients. There are no significant effects at the five percent level for any age group.

The previous literature has also detected stronger effects among males, better educated, and individuals residing in low unemployment areas. In this study I find no significant effects for men, nor women, at the five percent level. For women receiving part-time DI benefits, there is a weakly significant positive effect on income according to the naive RD model, but the point estimate for the same outcome for men is very similar to that of women, and it is in any case not robust to adjusting for covariate differences in the matching model. Naive RD results show stronger negative effects among higher educated for all labor supply outcomes for fulltime recipients, but nearest neighbor matching results show no effects for any educational level. For part-time recipients, the estimated effects on having income above the earnings disregard and on total earnings are positively significant for those with compulsory education with the naive RD model, but these effects are not robust to the matching model. Naive RD estimates also suggest a strong positive response among part-time recipients in low unemployment areas, a positive response with respect to labor supply on the intensive margin, and a negative response with respect to increasing the level of education, but neither of these effects are robust to adjusting for compositional differences through matching.

Labor market attachment might have a big impact on the labor market responses of the disabled as labor demand might be low for these individuals in comparison to other individuals on the labor market. The matching models suggests a positively significant effects on labor supply on the extensive margin for fulltime recipients who were working at or closely before DI-award, and a negatively significant effect on the same outcome for part-time recipients who were not working at or closely before DI-award. However, for both these groups the sample size is small.

7 Conclusions

Concerns about costs for sickness absence have brought a discussion about residual working capacities among disability insurance recipients and the disincentives to work provided by the DI system. Evidence from around the world suggest that individuals who are awarded disability benefits in some cases still have residual working capacity that could be utilized, and there are a few examples of policies introduced to incentivize that ability to be put to use in the labor market. One such initiative is the introduction of the continuous deduction program in Sweden in 2009. In this study, I investigate whether the financial incentives provided by the continuous deduction program can induce people with reduced working capacity to increase their labor supply.

The theoretical predictions imply that the response should be positive, since almost all of both full- and part-time recipients have zero labor supply before the reform on the part of fulltime work that they receive benefits on. The labor supply response predicted for these recipients is nonnegative. If there is residual working capacity among these individuals, the response should be positive, given that not all have preferences placing them at zero labor supply regardless of the benefit scheme.

My empirical findings, on the other hand, do not suggest that the program has had any effect on labor supply. The retroactively determined eligibility to the program with respect to time of DI award can be used as a natural experiment to estimate the effects of the program, combining a regression discontinuity design with matching to ensure balance in recipient characteristics between treated and untreated. Changes to the eligibility for DI at the same time as the retroactively set cutoff for eligibility to the continuous deduction program make the results from a naive regression discontinuity model biased due to compositional differences between treated and controls. I match similar individuals in the treatment and control groups close to the eligibility threshold using a nearest neighbor propensity score matching model to estimates unbiased results. The matched sample consists of individuals with more severe health impairments than the overall sample of treated. Assuming that potential outcomes are negatively related to the severity of the reduction in working capacity, it is reasonable to expect a lower response within this group, and the naive RD model then provides upper bound estimates of the true effect for this group. However, no positive effects are established for the unmatched sample using this approach for fulltime recipients, and the significant effect on total earnings for part-time recipients is sensitive to the bandwidth choice.

My results suggest that the financial incentives provided by the continuous deduction program do not induce eligible DI recipients to increase labor supply, neither for full- nor part-time recipients. The main upper bound results of the effects provided by the naive RD model imply that there may be an effect on the intensive margin for part-time recipients, but the results from the matching model does not suggest that this is the case, nor is the effect significant using a bandwidth larger than the baseline. The upper bound estimates are also not larger or more or less precise than RD estimates using placebo cutoffs to the forcing variable where there is no discontinuity in treatment. I have also studied the effects of the reform on educational attainment, an outcome that is less dependent on labor demand, but do not find any effects on that outcome either.

These results may imply that the financial incentives provided by the program are not enough to induce the eligible DI recipients to use their residual working capacities and increase their labor supply. One possibility is that there is a lack of credibility with respect to some program components. The continuous deduction program involves a promise not to reevaluate recipients' eligibility for permanent DI benefits. Even if financial incentives are strong, the effect may be absent because recipients do not trust that their recipient status will remain unquestioned after demonstrating a residual working capacity. Another possible explanation could be a lack of labor demand for workers with reduced working capacity. It should however, if this is the case, be easier for part-time recipients to increase labor supply, since they most often already have an employer and work on the part that they are not awarded DI for. My results do not show any effect for this group either, nor for educational attainment which is not directly affected by labor demand. There is also no conclusive evidence that responses are higher in low unemployment areas, or that people who were working closely before DI award respond positively to the reform.

Another possible explanation for my results is that there are no residual working capacities among recipients of permanent disability insurance benefits in Sweden. Some evidence does, however, suggest that there is in fact residual working capacity among the targeted group (Government Bill 2007/08:214, Larheden 2008). My analysis focuses on always types, recipients with so severe impairments that they are or would be awarded DI benefits also under the tighter regime. However, the analysis does not provide evidence of any effect even without adjusting for the compositional differences between treated and untreated due to the regime change, at least not for fulltime recipients. The findings are not in line with e.g. previous findings from the neighboring country of Norway; 95 percent confidence intervals are far below the point estimates found for comparable outcomes in Kostøl & Mogstad (2014). It is possible that working capacities among DI recipients in Norway are higher than in Sweden. In Norway, disability pension is a universal right not restricted to those previously on the labor market. The award of disability pension in Norway considers the applicants overall ability to engage in any substantial gainful activity, taking into account health status, age, education and work experience as well as the transferability of the applicant's skills (Kostøl & Mogstad, 2014). The OECD has voiced criticism for it being too easy to get disability benefits in Norway, which has the highest spending on sickness and disability benefits in the OECD (Kvam, 2013). It could also be that age is a very important factor for the response on these kinds of financial incentives. The age composition within the eligible DI recipients in Sweden is high, although my subsample analysis does not suggest any positive effect among young recipients of permanent DI either. However, compared to previous evidence, even the youngest recipients of permanent DI in Sweden are older than in other countries. Kostøl and Mogstad (2014) find positive effects in Norway only within the age group 18-49. Their analysis does not further indicate how the response is distributed over ages within this group. Perhaps financial incentives like these would be more effective if targeted to recipients even younger in Sweden, today awarded temporary disability benefits in the form of activity benefits and not eligible for the continuous deduction program.

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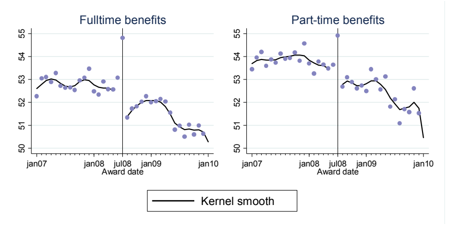
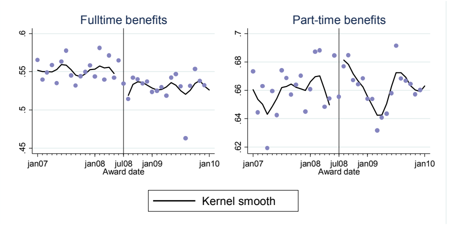
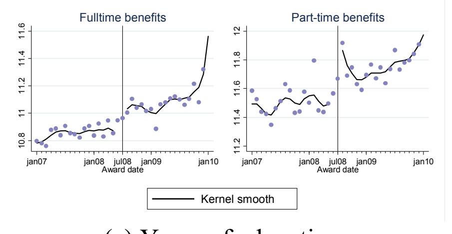
Appendix



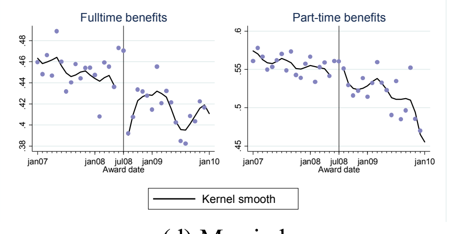
(a) Age



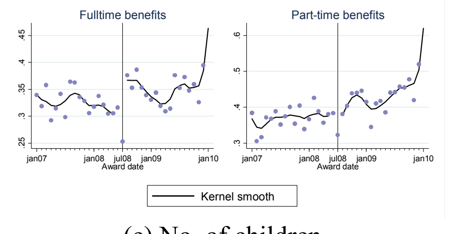
(b) Gender



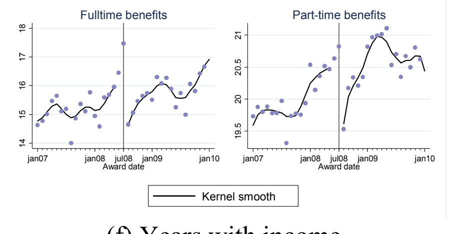
(c) Years of education



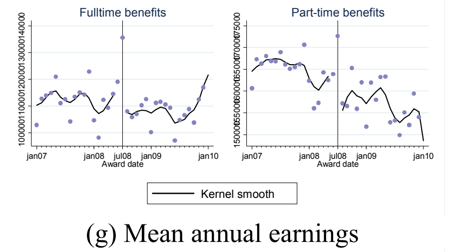
(d) Married



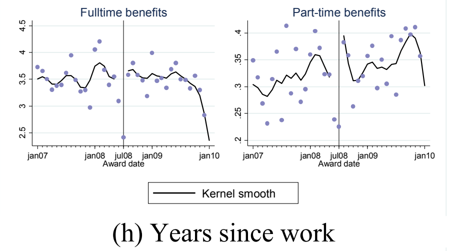
(e) No. of children



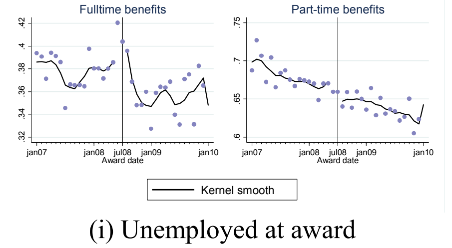
(f) Years with income



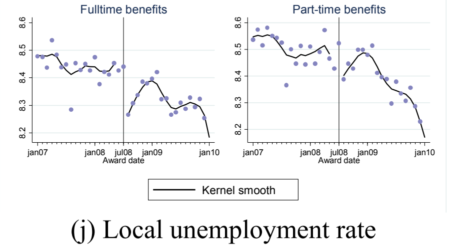
(g) Mean annual earnings



(h) Years since work

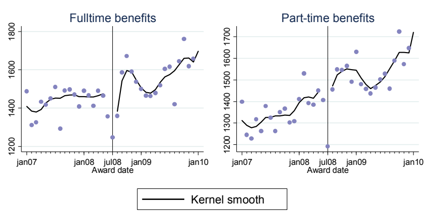


(i) Unemployed at award

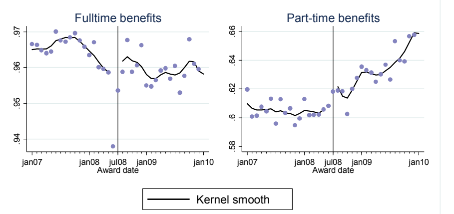


(j) Local unemployment rate

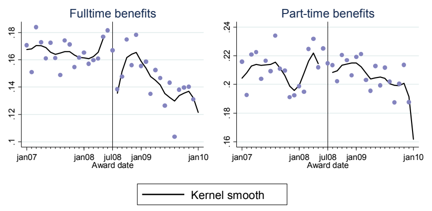
Figure A.1, part 1. Basic characteristics by award date



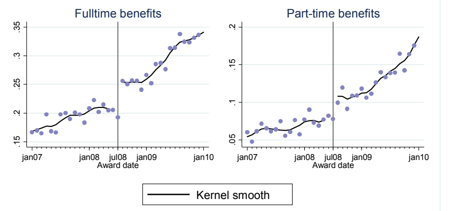
(k) Length of sickness spell before award



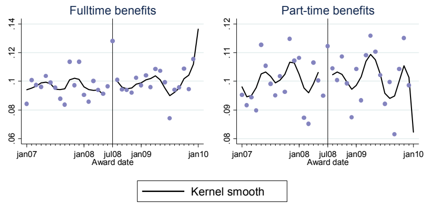
(l) Average extent of benefits five years before award



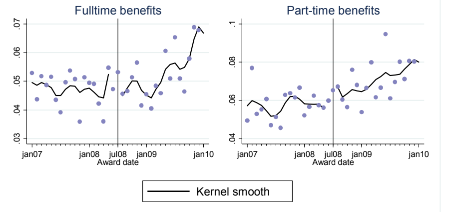
(m) Diagnosed with deceases of the musculoskeletal system



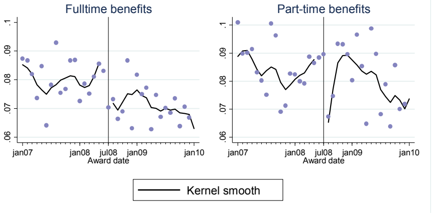
(n) Diagnosed with mental and behavioral disorders



(o) Diagnosed with deceases of the circulatory system



(p) Diagnosed with deceases of the nervous system



(q) Diagnosed with injury, poisoning etc. (external causes)

Figure A.1 part 2. Health characteristics by award date

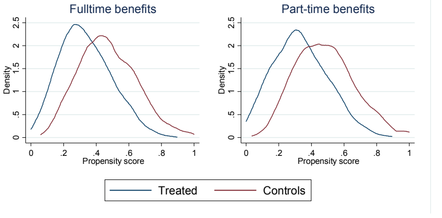
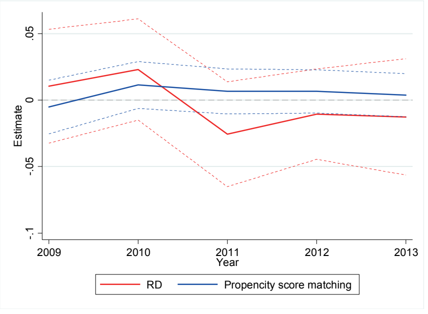
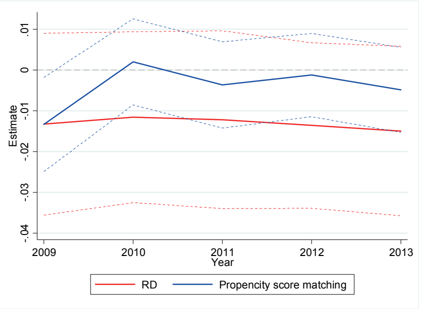


Figure A.2. Density of the propensity score

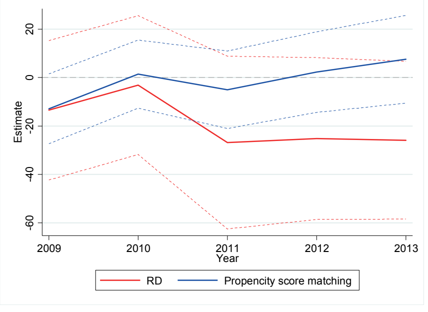
Panel A. Fulltime recipients



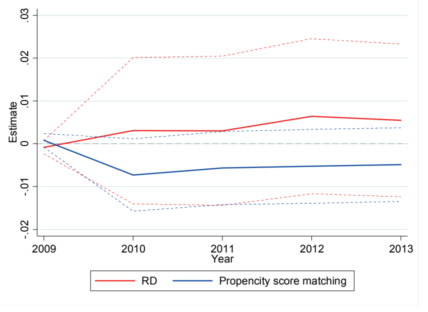
(a) Working



(b) Earnings above the earnings disregard

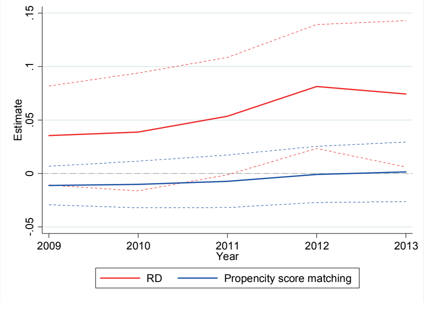


(c) Yearly wage earnings (SEK 100)

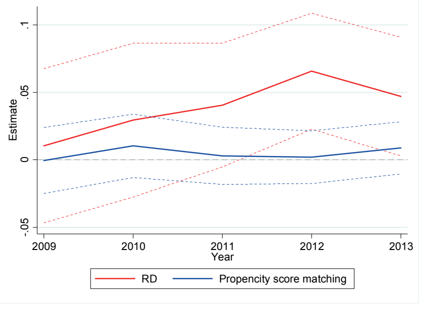


(d) Increase in education level

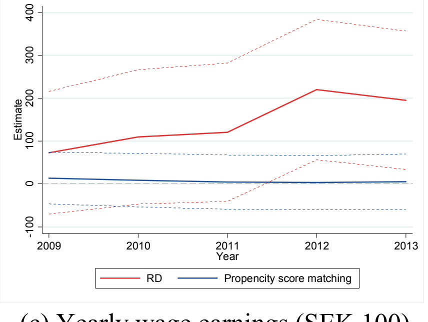
Panel B. Part-time recipients



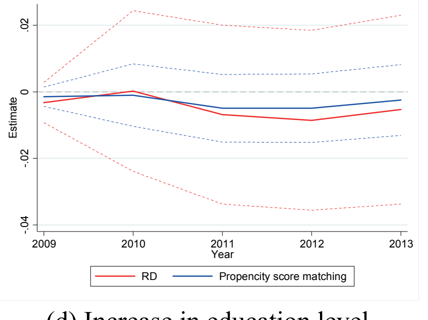
(a) Working



(b) Earnings above the earnings disregard



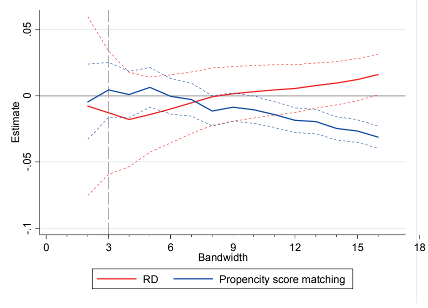
(c) Yearly wage earnings (SEK 100)



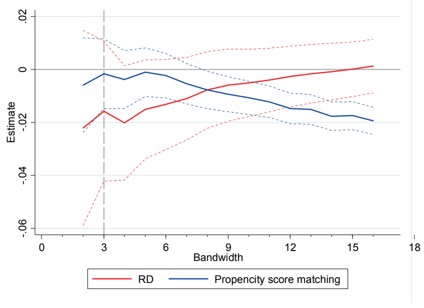
(d) Increase in education level

Figure A.3. Results year by year

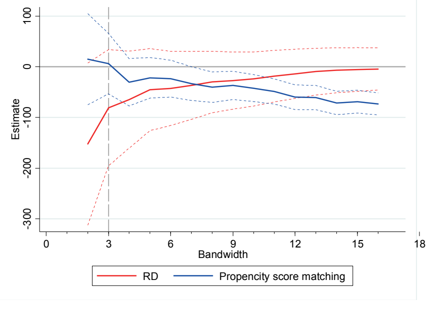
Panel A. Fulltime recipients



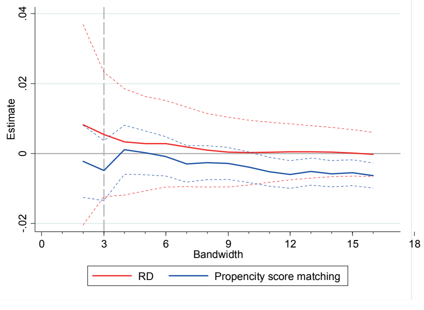
(a) Working



(b) Earnings above the earnings disregard

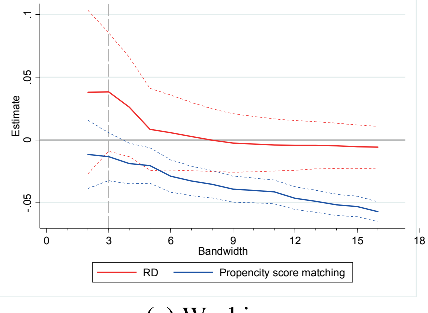


(c) Total earnings (SEK 100)

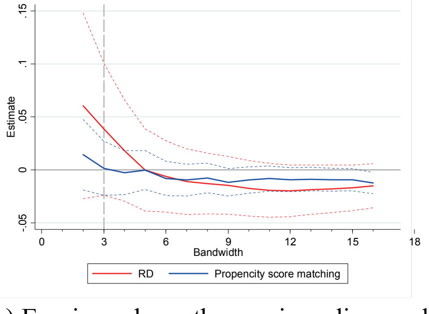


(d) Increase in education level

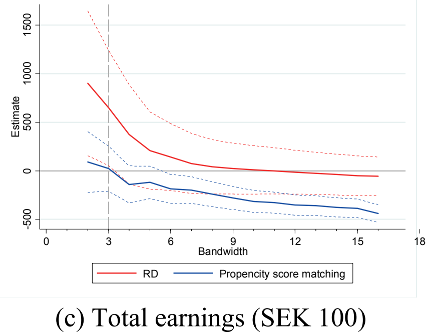
Panel B. Part-time recipients



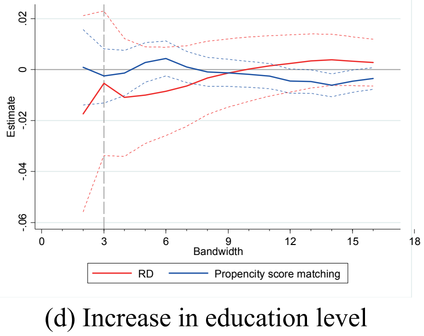
(a) Working



(b) Earnings above the earnings disregard



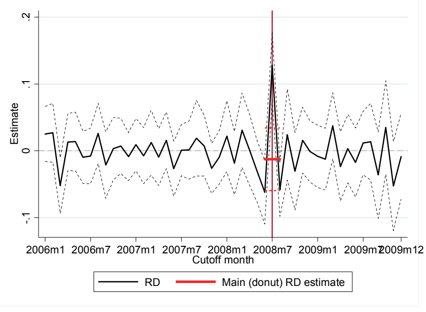
(c) Total earnings (SEK 100)



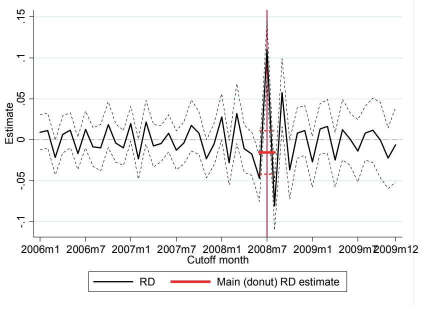
(d) Increase in education level

Figure A.4. Results using different bandwidths

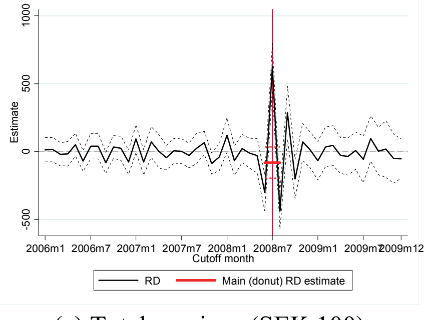
Panel A. Fulltime recipients



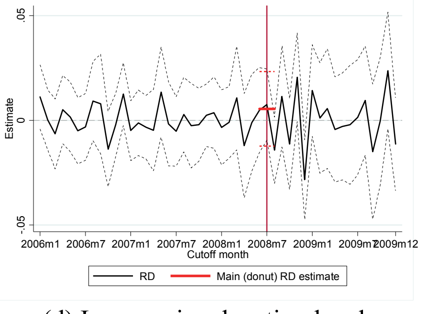
(a) Working



(b) Earnings above the earnings disregard

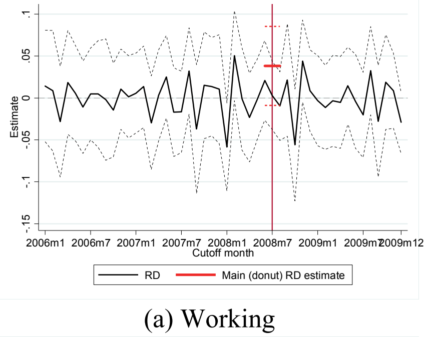


(c) Total earnings (SEK 100)

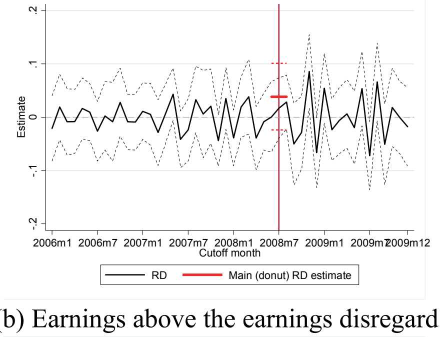


(d) Increase in education level

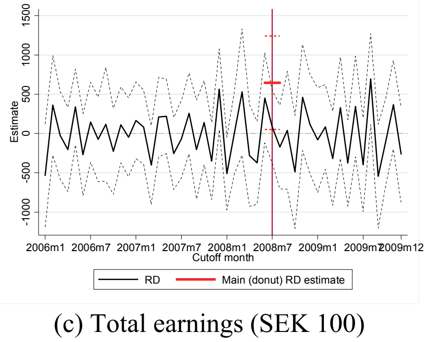
Panel B. Part-time recipients



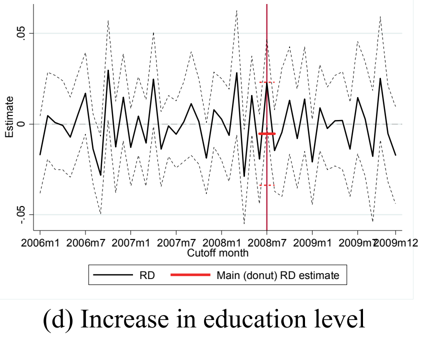
(a) Working



(b) Earnings above the earnings disregard



(c) Total earnings (SEK 100)



(d) Increase in education level

Figure A.5. RD results with placebo cutoffs Jan 2006 - Dec 2009

Table A.1. *Descriptive statistics, close to cutoff*

	Fulltime recipients			Part-time recipients		
	Treated	Controls	Difference, t-test	Treated	Controls	Difference, t-test
Month of award (months elapsed since Jan 1960)	579.0	584.2	-5.178*** (-272.66)	579.0	584.2	-5.187*** (-237.51)
Extent of benefits, 1=100 percent	0.999	1	-0.00143** (-2.72)	0.442	0.436	0.00588 (1.41)
Gender	0.551	0.535	0.0160 (1.34)	0.662	0.675	-0.0131 (-1.00)
Years of education	10.88	11.06	-0.177*** (-3.38)	11.46	11.76	-0.300*** (-4.67)
No. of children in household below 18	0.310	0.372	-0.0624** (-3.27)	0.375	0.414	-0.0395 (-1.79)
Married	0.450	0.415	0.0342** (2.87)	0.551	0.528	0.0233 (1.68)
Age	52.67	51.69	0.983*** (4.40)	53.63	52.91	0.712** (3.04)
Mean annual earnings before award (SEK 100)	112129.7	106829.4	5300.3* (2.35)	161544.2	160459.0	1085.1 (0.44)
Mean annual earnings five years before award (SEK 100)	64012.8	64650.6	-637.8 (-0.27)	151305.8	152300.5	-994.7 (-0.33)
No. of years with income	15.75	15.15	0.603*** (3.49)	20.45	20.11	0.339** (2.60)
No. of years with income five years before award	2.538	2.416	0.122* (2.50)	4.416	4.360	0.0565 (1.57)
No. of years with income above earnings disregard level five years before award	1.579	1.549	0.0297 (0.69)	1.681	1.721	-0.0391 (-0.77)
Years since working	3.534	3.659	-0.124 (-1.16)	0.338	0.322	0.0156 (0.36)
Working at award	0.108	0.125	-0.0173* (-2.28)	0.863	0.872	-0.00900 (-0.96)
Time in unemployment	1453.0	1481.2	-28.19 (-0.72)	1186.6	1244.2	-57.59 (-1.26)
Unemployed at award	0.379	0.366	0.0139 (1.21)	0.664	0.646	0.0175 (1.33)
Local unemployment rate at program start (county level)	8.429	8.312	0.117*** (4.42)	8.510	8.426	0.0841** (2.84)
Received sickness benefits at award	0.337	0.262	0.0755*** (6.88)	0.537	0.435	0.102*** (7.41)
Received activity benefits at award	0.0233	0.0317	-0.00836* (-2.18)	0.0113	0.0108	0.000543 (0.19)
Received temporary DI benefits at award	0.540	0.597	-0.0565*** (-4.78)	0.476	0.568	-0.0921*** (-6.67)
Extent of sickness benefits at award	0.322	0.246	0.0761*** (7.15)	0.251	0.206	0.0459*** (6.07)
Extent of activity or temporary DI benefits at award	0.549	0.614	-0.0652*** (-5.62)	0.235	0.281	-0.0461*** (-5.99)
Days of sickness absence before award	1836.5	1968.1	-131.7*** (-4.62)	1739.6	1902.5	-163.0*** (-5.89)
Average extent of benefits five years before award	0.959	0.962	-0.00243 (-0.99)	0.603	0.612	-0.00826 (-1.48)
Length of current sickness spell at award	1457.2	1574.8	-117.6*** (-4.24)	1409.6	1528.9	-119.2*** (-4.40)

continues on next page

Table A.1 *cont.*

	Fulltime recipients			Part-time recipients		
	Treated	Controls	Difference, t-test	Treated	Controls	Difference, t-test
Length of current sickness spell with same extent or higher at award	1236.8	1326.1	-89.31*** (-3.45)	1255.0	1372.2	-117.3*** (-4.47)
No. of sickness absence spells	4.086	3.921	0.165 (1.73)	4.323	4.524	-0.201 (-0.95)
<u>*Diagnose spent most time in care, ICD-!0:*</u>						
A00B99	0	0	0 (.)	0	0	0 (.)
C00D48	0	0	0 (.)	0	0	0 (.)
D50D89	0.0187	0.0118	0.00687* (2.29)	0.0287	0.0258	0.00283 (0.62)
E00E90	0.0291	0.0300	-0.000897 (-0.22)	0.0251	0.0272	-0.00216 (-0.49)
F00F98	0.207	0.254	-0.0469*** (-4.71)	0.0732	0.103	-0.0302*** (-3.91)
G00G99	0.0445	0.0484	-0.00395 (-0.79)	0.0585	0.0601	-0.00160 (-0.24)
H00H59	0.0275	0.0247	0.00279 (0.73)	0.0331	0.0324	0.000735 (0.15)
H60H95	0.0170	0.0125	0.00441 (1.51)	0.0340	0.0333	0.000691 (0.14)
I00I99	0.0949	0.0955	-0.000557 (-0.08)	0.0979	0.105	-0.00680 (-0.82)
J00J99	0.0277	0.0244	0.00336 (0.88)	0.0308	0.0254	0.00539 (1.17)
K00K93	0.0564	0.0554	0.000975 (0.18)	0.0618	0.0653	-0.00348 (-0.52)
L00L99	0.0286	0.0223	0.00633 (1.66)	0.0290	0.0296	-0.000627 (-0.13)
M00M99	0.166	0.157	0.00855 (0.97)	0.223	0.212	0.0104 (0.91)
N00N99	0.0630	0.0540	0.00897 (1.59)	0.0803	0.0714	0.00893 (1.21)
O00O99	0.0185	0.0188	-0.000317 (-0.10)	0.0281	0.0282	-0.000114 (-0.02)
P00P96	0	0	0 (.)	0	0	0 (.)
Q00Q99	0.00154	0.00139	0.000148 (0.16)	0.00328	0.00141	0.00188 (1.34)
R00R99	0.0769	0.0728	0.00403 (0.64)	0.0800	0.0752	0.00487 (0.65)
S00T98	0.0808	0.0690	0.0118 (1.87)	0.0851	0.0813	0.00384 (0.50)
V00V99	0.0198	0.0192	0.000656 (0.20)	0.0352	0.0310	0.00423 (0.85)
V01Y98	0	0	0 (.)	0	0	0 (.)
Z00Z99	0.0683	0.0836	-0.0154* (-2.46)	0.0723	0.0803	-0.00806 (-1.10)
No. of observations	4,541	2,870	7,411	2,129	3,349	5,478

Table A.2. *Descriptive statistics for matched sample, close to cutoff*

	Fulltime recipients			Part-time recipients		
	Treated	Controls	Difference, t-test	Treated	Controls	Difference, t-test
Month of award (months elapsed since Jan 1960)	579.1	584.3	-5.201*** (-232.71)	579.1	584.2	-5.176*** (-209.86)
Extent of benefits, 1=100 percent	1	1	0 (.)	0.434	0.435	-0.00161 (-0.35)
Gender	0.543	0.547	-0.00364 (-0.26)	0.681	0.679	0.00198 (0.13)
Years of education	11.05	11.10	-0.0514 (-0.84)	11.71	11.76	-0.0519 (-0.71)
No. of children in household below 18	0.347	0.360	-0.0126 (-0.54)	0.380	0.413	-0.0326 (-1.32)
Married	0.433	0.432	0.00162 (0.11)	0.535	0.530	0.00495 (0.32)
Age	52.28	52.14	0.147 (0.57)	53.40	53.02	0.378 (1.43)
Mean annual earnings before award (SEK 100)	116216.1	115412.5	803.7 (0.30)	165429.8	163631.4	1798.4 (0.62)
Mean annual earnings five years before award (SEK 100)	68749.5	70051.1	-1301.6 (-0.46)	157127.8	155752.3	1375.6 (0.39)
No. of years with income	16.40	16.35	0.0490 (0.27)	20.57	20.43	0.135 (1.00)
No. of years with income five years before award	2.632	2.648	-0.0166 (-0.29)	4.471	4.445	0.0262 (0.70)
No. of years with income above earnings disregard level five years before award	1.671	1.694	-0.0231 (-0.44)	1.779	1.766	0.0134 (0.23)
Years since working	3.347	3.294	0.0526 (0.46)	0.273	0.253	0.0198 (0.49)
Working at award	0.128	0.137	-0.00931 (-0.96)	0.882	0.885	-0.00346 (-0.34)
Time in unemployment	1506.9	1524.4	-17.52 (-0.37)	1197.3	1217.3	-20.00 (-0.38)
Unemployed at award	0.360	0.346	0.0138 (1.01)	0.651	0.647	0.00445 (0.30)
Local unemployment rate at program start (county level)	8.286	8.313	-0.0274 (-0.87)	8.404	8.427	-0.0237 (-0.69)
Received sickness benefits at award	0.304	0.299	0.00526 (0.40)	0.473	0.448	0.0257 (1.64)
Received activity benefits at award	0.0251	0.0275	-0.00243 (-0.53)	0.0114	0.0109	0.000495 (0.15)
Received temporary DI benefits at award	0.645	0.649	-0.00405 (-0.30)	0.561	0.579	-0.0173 (-1.11)
Extent of sickness benefits at award	0.287	0.281	0.00567 (0.45)	0.223	0.212	0.0110 (1.28)
Extent of activity or temporary DI benefits at award	0.652	0.660	-0.00709 (-0.53)	0.275	0.285	-0.00977 (-1.11)
Days of sickness absence before award	1988.3	2018.6	-30.23 (-0.92)	1833.8	1905.9	-72.13* (-2.29)
Average extent of benefits five years before award	0.960	0.961	-0.00104 (-0.37)	0.603	0.610	-0.00652 (-1.03)
Length of current sickness spell at award	1685.3	1714.8	-29.56 (-0.91)	1507.7	1554.7	-47.01 (-1.53)

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Table A.2 *cont.*

	Fulltime recipients			Part-time recipients		
	Treated	Controls	Difference, t-test	Treated	Controls	Difference, t-test
Length of current sickness spell with same extent or higher at award	1419.2	1437.5	-18.30	1353.4	1395.0	-41.59
			(-0.59)			(-1.39)
No. of sickness absence spells	4.231	4.186	0.0445	4.445	4.596	-0.151
			(0.40)			(-0.57)
<u>*Diagnose spent most time in care, ICD-!0:*</u>						
A00B99	0	0	0	0	0	0
			(.)			(.)
C00D48	0	0	0	0	0	0
			(.)			(.)
D50D89	0.00972	0.0109	-0.00121	0.0287	0.0262	0.00247
			(-0.42)			(0.48)
E00E90	0.0332	0.0312	0.00202	0.0272	0.0277	-0.000495
			(0.40)			(-0.10)
F00F98	0.246	0.255	-0.00850	0.0846	0.0999	-0.0153
			(-0.69)			(-1.68)
G00G99	0.0538	0.0522	0.00162	0.0559	0.0579	-0.00198
			(0.25)			(-0.27)
H00H59	0.0275	0.0243	0.00324	0.0341	0.0326	0.00148
			(0.72)			(0.26)
H60H95	0.0126	0.0130	-0.000405	0.0371	0.0341	0.00297
			(-0.13)			(0.51)
I00I99	0.0992	0.100	-0.000810	0.105	0.105	0.000495
			(-0.10)			(0.05)
J00J99	0.0279	0.0251	0.00283	0.0252	0.0262	-0.000989
			(0.62)			(-0.20)
K00K93	0.0603	0.0571	0.00324	0.0673	0.0673	0
			(0.48)			(0.00)
L00L99	0.0219	0.0219	0	0.0242	0.0282	-0.00396
			(0.00)			(-0.79)
M00M99	0.162	0.164	-0.00162	0.217	0.217	0.000495
			(-0.15)			(0.04)
N00N99	0.0538	0.0551	-0.00121	0.0732	0.0747	-0.00148
			(-0.19)			(-0.18)
O00O99	0.0170	0.0178	-0.000810	0.0277	0.0292	-0.00148
			(-0.22)			(-0.28)
P00P96	0	0	0	0	0	0
			(.)			(.)
Q00Q99	0.00121	0.00121	0	0.00148	0.00148	0
			(0.00)			(0.00)
R00R99	0.0794	0.0733	0.00607	0.0816	0.0762	0.00544
			(0.80)			(0.64)
S00T98	0.0721	0.0704	0.00162	0.0900	0.0811	0.00890
			(0.22)			(1.01)
V00V99	0.0190	0.0182	0.000810	0.0331	0.0307	0.00247
			(0.21)			(0.45)
V01Y98	0	0	0	0	0	0
			(.)			(.)
Z00Z99	0.0826	0.0879	-0.00526	0.0786	0.0811	-0.00247
			(-0.66)			(-0.29)
No. of observations	2,470	2,470	4,940	2,022	2,022	4,044

Table A.3. *RD estimates of pre-treatment characteristics*

	Fulltime recipients		Part-time recipients	
	RD, unmatched sample:	RD, matched sample:	RD, unmatched sample:	RD, matched sample:
Month of award (months elapsed since Jan 1960)	0.001 (0.001)	-0.000 (0.000)	0.003 (0.011)	-0.003 (0.019)
Gender	0.019 (0.029)	-0.037 (0.057)	-0.053 (0.038)	-0.044 (0.065)
Years of education	-0.179 (0.126)	-0.174 (0.239)	-0.356** (0.170)	-0.043 (0.271)
No. of children in household below 18	-0.084 (0.052)	0.182** (0.091)	0.007 (0.053)	0.044 (0.091)
Married	0.032 (0.030)	-0.045 (0.058)	-0.020 (0.036)	-0.112* (0.064)
Age	1.005 (0.659)	-1.176 (1.118)	0.360 (0.610)	0.285 (1.095)
Mean annual earnings before award (SEK 100)	9123.171 (6319.918)	911.353 (11380.312)	8956.579 (6642.869)	11792.812 (11271.564)
Mean annual earnings five years before award (SEK 100)	1890.878 (6600.573)	-9114.215 (11785.650)	5509.372 (8212.689)	10232.202 (15346.581)
No. of years with income	1.560*** (0.459)	-0.249 (0.737)	0.959** (0.456)	0.842 (0.568)
No. of years with income five years before award	0.327*** (0.131)	-0.148 (0.248)	0.198* (0.107)	0.106 (0.152)
No. of years with income above earnings disregard level five years before award	0.192 (0.128)	-0.184 (0.222)	0.114 (0.129)	0.400 (0.245)
Years since working	-0.047 (0.263)	0.616 (0.516)	-0.098 (0.124)	-0.016 (0.175)
Working at award	-0.003 (0.022)	-0.012 (0.042)	0.052* (0.027)	0.023 (0.042)
Time in unemployment	52.221 (95.000)	60.304 (185.148)	-122.551 (118.430)	-21.982 (198.777)
Unemployed at award	-0.000 (0.029)	0.003 (0.053)	0.040 (0.039)	-0.029 (0.066)
Local unemployment rate at program start (county level)	0.234** (0.102)	0.222 (0.200)	0.049 (0.093)	-0.170 (0.171)
Received sickness benefits at award	0.164*** (0.034)	0.014 (0.049)	0.186*** (0.045)	0.053 (0.066)
Received activity benefits at award	-0.021* (0.011)	-0.009 (0.022)	-0.005 (0.009)	-0.010 (0.015)
Received temporary DI benefits at award	-0.066* (0.036)	0.005 (0.053)	-0.113*** (0.044)	0.013 (0.068)
Extent of sickness benefits at award	0.166*** (0.034)	0.020 (0.048)	0.084*** (0.023)	0.034 (0.036)
Extent of activity or temporary DI benefits at award	-0.089*** (0.035)	0.000 (0.053)	-0.070*** (0.024)	-0.005 (0.039)
Days of sickness absence before award	2.909 (77.624)	193.991 (145.946)	-49.579 (80.747)	-19.965 (138.119)
Average extent of benefits five years before award	-0.004 (0.006)	0.005 (0.012)	-0.010 (0.015)	-0.026 (0.024)
Length of current sickness spell at award	86.632 (70.793)	265.425* (154.101)	37.689 (79.274)	36.935 (125.845)

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Table A.3 *cont.*

	Fulltime recipients		Part-time recipients	
	RD, unmatched sample:	RD, matched sample:	RD, unmatched sample:	RD, matched sample:
Length of current sickness spell with same extent or higher at award	86.710 (69.295)	330.599** (153.965)	41.235 (73.651)	73.935 (123.512)
No. of sickness absence spells	0.249 (0.264)	-0.096 (0.500)	-1.183 (0.982)	-1.824 (1.300)
<u>*Diagnose spent most time in care, ICD-!0:*</u>				
A00B99	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
C00D48	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
D50D89	0.007 (0.008)	0.015 (0.012)	-0.008 (0.011)	-0.015 (0.021)
E00E90	-0.001 (0.011)	0.006 (0.022)	-0.015 (0.011)	0.007 (0.021)
F00F98	-0.051* (0.029)	-0.028 (0.050)	-0.028 (0.019)	-0.047 (0.030)
G00G99	0.023* (0.014)	0.052* (0.028)	-0.010 (0.014)	-0.031 (0.026)
H00H59	-0.003 (0.011)	0.008 (0.018)	-0.002 (0.014)	0.010 (0.023)
H60H95	0.008 (0.007)	0.005 (0.012)	-0.009 (0.012)	-0.010 (0.022)
I00I99	-0.017 (0.019)	-0.014 (0.037)	0.006 (0.023)	-0.014 (0.041)
J00J99	0.011 (0.011)	0.016 (0.019)	0.032*** (0.013)	0.009 (0.022)
K00K93	-0.006 (0.013)	-0.017 (0.022)	0.011 (0.017)	0.072*** (0.030)
L00L99	0.016* (0.009)	0.018 (0.019)	0.002 (0.013)	0.021 (0.022)
M00M99	0.052** (0.024)	0.018 (0.040)	-0.006 (0.028)	-0.063 (0.054)
N00N99	0.014 (0.015)	-0.012 (0.028)	0.022 (0.024)	0.007 (0.036)
O00O99	-0.002 (0.009)	0.015 (0.018)	-0.018 (0.012)	-0.021 (0.020)
P00P96	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Q00Q99	0.002 (0.002)	0.004 (0.004)	0.006 (0.004)	0.006 (0.006)
R00R99	-0.020 (0.017)	0.010 (0.034)	-0.011 (0.020)	-0.075** (0.033)
S00T98	0.028* (0.017)	-0.023 (0.030)	0.029 (0.020)	0.066* (0.038)
V00V99	-0.006 (0.010)	-0.019 (0.014)	0.001 (0.014)	0.018 (0.023)
V01Y98	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	0.000 (0.000)
Z00Z99	-0.015 (0.017)	0.068** (0.032)	-0.019 (0.017)	0.009 (0.031)
No. of observations	7,250	4,940	5,455	4,044

Table A.4. *Results not excluding June 2008*

Outcome	(1) RD	(2) NNM
Panel A. Fulltime recipients:		
Working	0.093***	0.015
	(0.031)	(0.010)
Earnings above the earnings disregard	0.054***	0.011
	(0.019)	(0.007)
Total earnings	320.501***	68.700**
	(95.063)	(32.892)
Increase in education level	0.005	0.000
	(0.010)	(0.004)
Observations	8,297	4,952
Panel B. Part-time recipients:		
Working	0.021	-0.014
	(0.024)	(0.010)
Earnings above the earnings disregard	0.080**	0.015
	(0.035)	(0.013)
Total earnings	497.679	70.589
	(327.736)	(115.861)
Increase in education level	0.010	0.006
	(0.016)	(0.006)
Observations	5,982	4,068

Note: Each cell represents the result from a separate regression, with each row showing the regression discontinuity (RD) and propensity score nearest neighbor matching (NNM) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Table A.5. *Results excluding individuals aged above 61*

Outcome	(1) RD	(2) NNM
Panel A. Fulltime recipients:		
Working	-0.009	-0.013
	(0.027)	(0.012)
Earnings above the earnings disregard	-0.017	-0.004
	(0.015)	(0.007)
Total earnings	-23.666	-3.014
	(18.261)	(9.297)
Increase in education level	0.001	-0.003
	(0.014)	(0.006)
Observations	5,547	3,942
Panel B. Part-time recipients:		
Working	0.032	-0.001
	(0.027)	(0.010)
Earnings above the earnings disregard	0.046	-0.016
	(0.038)	(0.015)
Total earnings	179.826**	30.779
	(86.359)	(34.686)
Increase in education level	-0.011	-0.001
	(0.023)	(0.008)
Observations	4,101	3,188

Note: Each cell represents the result from a separate regression, with each row showing the regression discontinuity (RD) and propensity score nearest neighbor matching (NNM) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Table A.6. *Pre-program effects*

Outcome	(1) RD	(2) NNM
Panel A. Fulltime recipients:		
Working 2004	0.059*	0.008
	(0.032)	(0.013)
Working 2005	0.038	0.003
	(0.030)	(0.014)
Working 2006	0.051	-0.009
	(0.032)	(0.014)
Working 2007	0.099***	0.008
	(0.030)	(0.013)
Working 2008	0.047	0.006
	(0.030)	(0.013)
Earnings above the earnings disregard 2004	0.052	0.005
	(0.033)	(0.013)
Earnings above the earnings disregard 2005	0.055*	0.003
	(0.032)	(0.013)
Earnings above the earnings disregard 2006	0.026	-0.002
	(0.029)	(0.012)
Earnings above the earnings disregard 2007	0.011	-0.019*
	(0.025)	(0.010)
Earnings above the earnings disregard 2008	0.008	-0.019**
	(0.022)	(0.009)
Observations	7,250	4,940
Panel B. Part-time recipients:		
Working 2004	0.043*	0.006
	(0.024)	(0.010)
Working 2005	0.063***	-0.003
	(0.027)	(0.010)
Working 2006	0.040	0.004
	(0.029)	(0.010)
Working 2007	-0.005	0.011
	(0.023)	(0.009)
Working 2008	0.019	-0.003
	(0.021)	(0.008)
Earnings above the earnings disregard 2004	0.041	0.012
	(0.037)	(0.015)
Earnings above the earnings disregard 2005	0.026	-0.009
	(0.031)	(0.015)
Earnings above the earnings disregard 2006	-0.016	-0.010
	(0.034)	(0.014)
Earnings above the earnings disregard 2007	0.026	-0.008
	(0.030)	(0.013)
Earnings above the earnings disregard 2008	-0.005	-0.017
	(0.025)	(0.013)
Observations	5,455	4,044

Note: Each cell represents the result from a separate regression, with each row showing the regression discontinuity (RD) and propensity score nearest neighbor matching (NNM) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Table A.7. *Results by age*

	Fulltime recipients		Part-time recipients	
Outcome	(1) RD	(2) NNM	(3) RD	(4) NNM
Panel A. Age 30-44				
Working	-0.034	-0.025	0.019	-0.034*
	(0.047)	(0.023)	(0.042)	(0.018)
Earnings above the earnings disregard	-0.015	-0.016	0.001	-0.008
	(0.031)	(0.015)	(0.079)	(0.029)
Total earnings	-32.105	25.888	564.621	206.516
	(143.625)	(85.871)	(625.716)	(261.641)
Increase in education level	-0.011	-0.002	-0.033	-0.010
	(0.024)	(0.011)	(0.040)	(0.016)
Observations	1,634	1,118	1,020	774
Panel B. Age 45-54				
Working	-0.014	-0.002	0.016	0.020
	(0.051)	(0.020)	(0.045)	(0.018)
Earnings above the earnings disregard	0.004	0.003	0.052	0.006
	(0.030)	(0.014)	(0.063)	(0.025)
Total earnings	-135.967	-32.228	559.571	267.937
	(130.275)	(65.417)	(586.817)	(230.094)
Increase in education level	0.026	-0.011	-0.011	0.006
	(0.024)	(0.010)	(0.032)	(0.012)
Observations	1,814	1,266	1,380	1,088
Panel C. Age 45-64				
Working	-0.005	0.023	0.056	-0.028*
	(0.032)	(0.016)	(0.036)	(0.015)
Earnings above the earnings disregard	-0.019	0.003	0.057	-0.004
	(0.020)	(0.010)	(0.041)	(0.018)
Total earnings	-49.989	-1.135	770.578*	-123.568
	(78.524)	(35.530)	(400.710)	(159.236)
Increase in education level	0.012	0.001	0.000	-0.003
	(0.010)	(0.005)	(0.010)	(0.006)
Observations	3,528	2,360	2,843	2,020

Note: Each cell represents the result from a separate regression, with each row showing the regression discontinuity (RD) and propensity score nearest neighbor matching (NNM) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Table A.8. *Results by gender*

Outcome	Fulltime recipients (1) RD	(2) NNM	Part-time recipients (3) RD	(4) NNM
Panel A. Women				
Working	-0.020	-0.002	0.034	-0.017
	(0.029)	(0.015)	(0.029)	(0.012)
Earnings above the earnings disregard	-0.011	0.003	0.013	0.005
	(0.019)	(0.010)	(0.033)	(0.016)
Total earnings	-64.252	12.232	654.146*	66.782
	(93.736)	(44.072)	(336.882)	(139.171)
Increase in education level	0.002	-0.009	-0.014	-0.006
	(0.016)	(0.007)	(0.018)	(0.007)
Observations	3,951	2,700	3,641	2,746
Panel B. Men				
Working	-0.005	0.011	0.050	-0.020
	(0.037)	(0.015)	(0.049)	(0.019)
Earnings above the earnings disregard	-0.022	-0.007	0.084	-0.018
	(0.020)	(0.010)	(0.060)	(0.024)
Total earnings	-106.617	-17.898	694.012	-184.362
	(76.922)	(45.067)	(549.180)	(220.497)
Increase in education level	0.008	0.003	0.015	0.006
	(0.010)	(0.005)	(0.017)	(0.008)
Observations	3,299	2,240	1,814	1,698

Note: Each cell represents the result from a separate regression, with each row showing the regression discontinuity (RD) and propensity score nearest neighbor matching (NNM) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Table A.9. *Results by education level*

	Fulltime recipients		Part-time recipients	
Outcome	(1) RD	(2) NNM	(3) RD	(4) NNM
Panel A. Compulsory				
Working	0.001	0.026	0.120*	-0.011
	(0.036)	(0.018)	(0.065)	(0.025)
Earnings above the earnings disregard	-0.005	0.001	0.134***	-0.014
	(0.023)	(0.011)	(0.055)	(0.024)
Total earnings	70.176	30.085	1292.696**	-512.523**
	(93.429)	(39.994)	(590.037)	(226.014)
Increase in education level	-0.007	-0.001	0.002	-0.003
	(0.005)	(0.003)	(0.025)	(0.009)
Observations	2,213	1,370	1,125	708
Panel B. High school				
Working	0.002	-0.003	0.026	-0.005
	(0.038)	(0.015)	(0.034)	(0.014)
Earnings above the earnings disregard	-0.023	-0.011	-0.017	-0.007
	(0.021)	(0.010)	(0.042)	(0.016)
Total earnings	-63.736	-34.076	479.395	233.830
	(94.113)	(40.166)	(386.546)	(150.961)
Increase in education level	0.012	-0.009	-0.006	0.002
	(0.017)	(0.008)	(0.023)	(0.010)
Observations	3,634	2,580	2,772	2,040
Panel C. Tertiary				
Working	-0.089*	0.012	0.021	-0.025
	(0.051)	(0.027)	(0.042)	(0.016)
Earnings above the earnings disregard	-0.029	0.014	0.117*	0.022
	(0.030)	(0.017)	(0.069)	(0.027)
Total earnings	-395.204***	21.238	851.301	-3.888
	(137.976)	(103.947)	(613.745)	(243.567)
Increase in education level	0.000	0.004	-0.018	0.000
	(0.013)	(0.004)	(0.011)	(0.005)
Observations	1,331	980	1,546	1,256

Note: Each cell represents the result from a separate regression, with each row showing the regression discontinuity (RD) and propensity score nearest neighbor matching (NNM) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Table A.10. *Results by local unemployment*

Outcome	Fulltime recipients (1) RD	(2) NNM	Part-time recipients (3) RD	(4) NNM
Panel A. Low unemployment				
Working	0.004	0.011	0.043	-0.009
	(0.035)	(0.014)	(0.032)	(0.014)
Earnings above the earnings disregard	-0.006	0.011	0.086**	0.016
	(0.019)	(0.009)	(0.043)	(0.018)
Total earnings	-129.012*	1.918	851.051**	244.845
	(71.440)	(38.403)	(408.814)	(166.585)
Increase in education level	0.018	-0.006	-0.039**	-0.011
	(0.013)	(0.006)	(0.018)	(0.008)
Observations	3,867	2,816	2,757	2,114
Panel B. High unemployment				
Working	-0.030	-0.007	0.034	-0.014
	(0.033)	(0.016)	(0.037)	(0.015)
Earnings above the earnings disregard	-0.029	-0.017*	-0.013	-0.022
	(0.020)	(0.010)	(0.046)	(0.019)
Total earnings	-26.214	-18.389	430.780	-190.051
	(90.281)	(49.691)	(439.830)	(168.158)
Increase in education level	-0.009	-0.003	0.030	0.009
	(0.013)	(0.007)	(0.020)	(0.008)
Observations	3,383	2,108	2,698	1,916

Note: Each cell represents the result from a separate regression, with each row showing the regression discontinuity (RD) and propensity score nearest neighbor matching (NNM) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.

Table A.11. *Results by labor market attachment*

	Fulltime recipients		Part-time recipients	
Outcome	(1) RD	(2) NNM	(3) RD	(4) NNM
Panel A. Working				
Working	0.001	0.081**	0.015	0.002
	(0.086)	(0.036)	(0.019)	(0.008)
Earnings above the earnings disregard	-0.113	-0.006	0.024	-0.001
	(0.074)	(0.035)	(0.035)	(0.014)
Total earnings	-361.882	-13.960	523.845	63.635
	(425.019)	(190.630)	(341.557)	(125.121)
Increase in education level	0.009	0.003	-0.004	-0.001
	(0.032)	(0.013)	(0.014)	(0.006)
Observations	851	780	4,748	3,580
Panel B. Not working				
Working	-0.012	-0.004	-0.012	-0.104***
	(0.021)	(0.010)	(0.096)	(0.044)
Earnings above the earnings disregard	-0.001	0.002	0.058	0.022
	(0.011)	(0.005)	(0.042)	(0.018)
Total earnings	-34.925	7.752	-57.808	-290.898
	(37.894)	(15.689)	(336.148)	(179.933)
Increase in education level	0.005	-0.005	-0.011	-0.026
	(0.010)	(0.005)	(0.041)	(0.020)
Observations	6,399	4,262	707	460

Note: Each cell represents the result from a separate regression, with each row showing the regression discontinuity (RD) and propensity score nearest neighbor matching (NNM) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively.