WORKING PAPER 2018:23

Lump-sum severance grants and the duration of unemployment

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ISSN 1651-1166

Lump-sum severance grants and the duration of unemployment[a]

by

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December 7, 2018

Abstract

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.

Keywords: Employment Security Agreements, collective agreement, lump-sum severance grant, unemployment insurance, moral hazard, liquidity effect, regression discontinuity design
JEL-codes: J59, J63, J65

[a] I am grateful for comments and suggestions from Anders Forslund, Stefan Eriksson, Michael Rosholm, Erik Mellander, David Seim, Johan Vikström and seminar participants at the Institute for Evaluation of Labour Market and Education Policy (IFAU). I am also thankful for financial support and valuable comments from the TSL Employment Security Fund and for data access from the TSL Employment Security Fund and AFA Insurance.
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1 Introduction

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. 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). The response could thus also, as Chetty (2008) argues, 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.

Chetty (2008) proposes that 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. 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 is based on 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 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 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 employees in Sweden to their employers, 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

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

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 displaced due to redundancy 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, which administers the agreement, between 2006 and 2012. I match this data to data on what workers have received the severance grant from the insurance company that administers the grant, AFA Insurance, 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. There is an initial positive and significant effect on unemployment that diminishes over time. Point estimates for the effect on job finding according to a measure more closely related to employment spells, although insignificant for the most part, shows a similar pattern. I find no effect on subsequent job quality in terms of job duration or income of the first new job. My analysis 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 no higher that their individual disposable income. The results also suggest that the effect of this type of grant is 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 follows 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:

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

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

The value function of not finding a job is:

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

where

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

$V_t(A_t)$ is unambiguously concave[4], but it must be assumed 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 maximizes utility with respect to s_t to choose the optimal 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 of that maximization problem is:

$$\mu'(s_t) = V_t(A_t) - U_t(A_t) \quad (4)$$

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:

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

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

[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.

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:

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

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

The value of the relation in (6) is positive since it is 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:

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

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

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 previously 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. They also use an age requirement for eligibility, but the age requirement is 50 years in their case. 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 the loss of the grant has a negative effect on re-employment rates, contrary to other 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[7]. 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 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[8]. Out of all blue-collar workers being notified of displacement through notifications involving five employees or more during the period of study, included in the 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[9], 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 34,865 (corresponding to around USD 4,100). Above age 49 the amount increases by SEK 1,440 per year of age. The maximum amount of SEK 50,705 is reached at the age of 60 with this scheme, and this is thus the amount given to workers between 60 and 64 years old.[10] The grant is

[7] The share is based on a comparison between the total number of workers covered by the different Employment Security Agreements according to Walter, 2015 and the size of the Swedish labor force according to the Labor Force Survey conducted by Statistics Sweden, 2018.
[8] The total number of employed workers is specified in Kjellberg, 2017.
[9] 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.
[10] The exact monetary amount changes over time and these are the amounts valid during 2018. 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.

equivalent to between one and two months of the previous monthly income.[11] Workers themselves apply for the severance grant directly to the insurance company that administers the grant. 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 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.[12] 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.[13] Maximum benefit duration is 60 weeks, but for parents with children under 18 it is prolonged to 90 weeks. Before March 2007[14], 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 al. 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,

[11] 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 10^{th} and 90^{th} percentile of the grant replacement rate is 1 and 2, respectively.
[12] 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.
[13] 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.
[14] A previously higher cap for the first 20 weeks of unemployment was also abolished at the same time.

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.

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 to 2012 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 identify the causal effects of the grant on unemployment durations and the subsequent job quality of those who do find a job.[15] 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.[16] 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:

$$y_i = \alpha + \tau D_i + \beta_1(1-D_i)(X_i-X_0) + \beta_2 D_i(X_i-X_0) + \varepsilon_i \quad (9)$$

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,

[15] 90 percent found a new job during the follow up period.

[16] 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). 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.

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 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[17]. The same bandwidth is used for the estimation of the first and second stage results, using a triangular kernel local linear regression model[18]. 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, being rehired within three months, 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

[17] 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.

[18] 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.

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 Employment 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 Employment Security Fund to construct a control group for the estimation. These data 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 data from the Employment Security Fund also include individual information about notified workers, including workers notified within smaller notifications than those reported to the PES.[19] 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 the Employment Security Fund. 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

[19] 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.

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 Employment Security Funds' registers from the baseline sample. This reduces the sample of treated by close to 32 percent.[20] 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[21] and three months after the notified termination date.[22] 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.

[20] 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.

[21] 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 Employment Security Funds' register among those treated within the same notification.

[22] 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.

Employment periods and earnings must be reported by all employers for tax collecting purposes, and I use this data to study the effect on job finding. Self-employment is not counted as becoming employed. 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.[23] If the new employment is found during the 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[24], 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[25]. 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

[23] 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 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.

[24] 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.

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

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 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 (twelve 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.

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

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[26]. There is thus no evidence of manipulation of the forcing variable.

[26] 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.

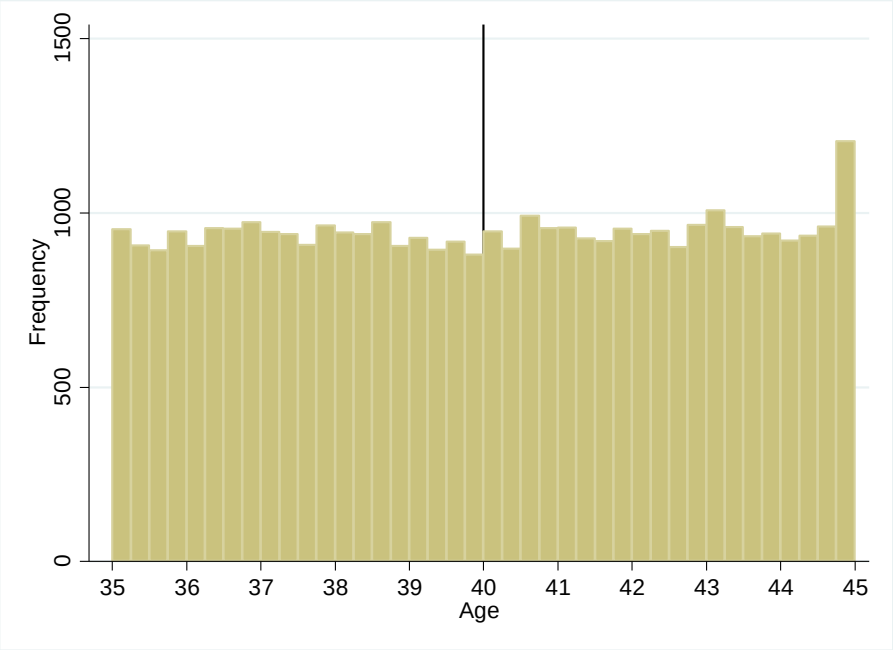
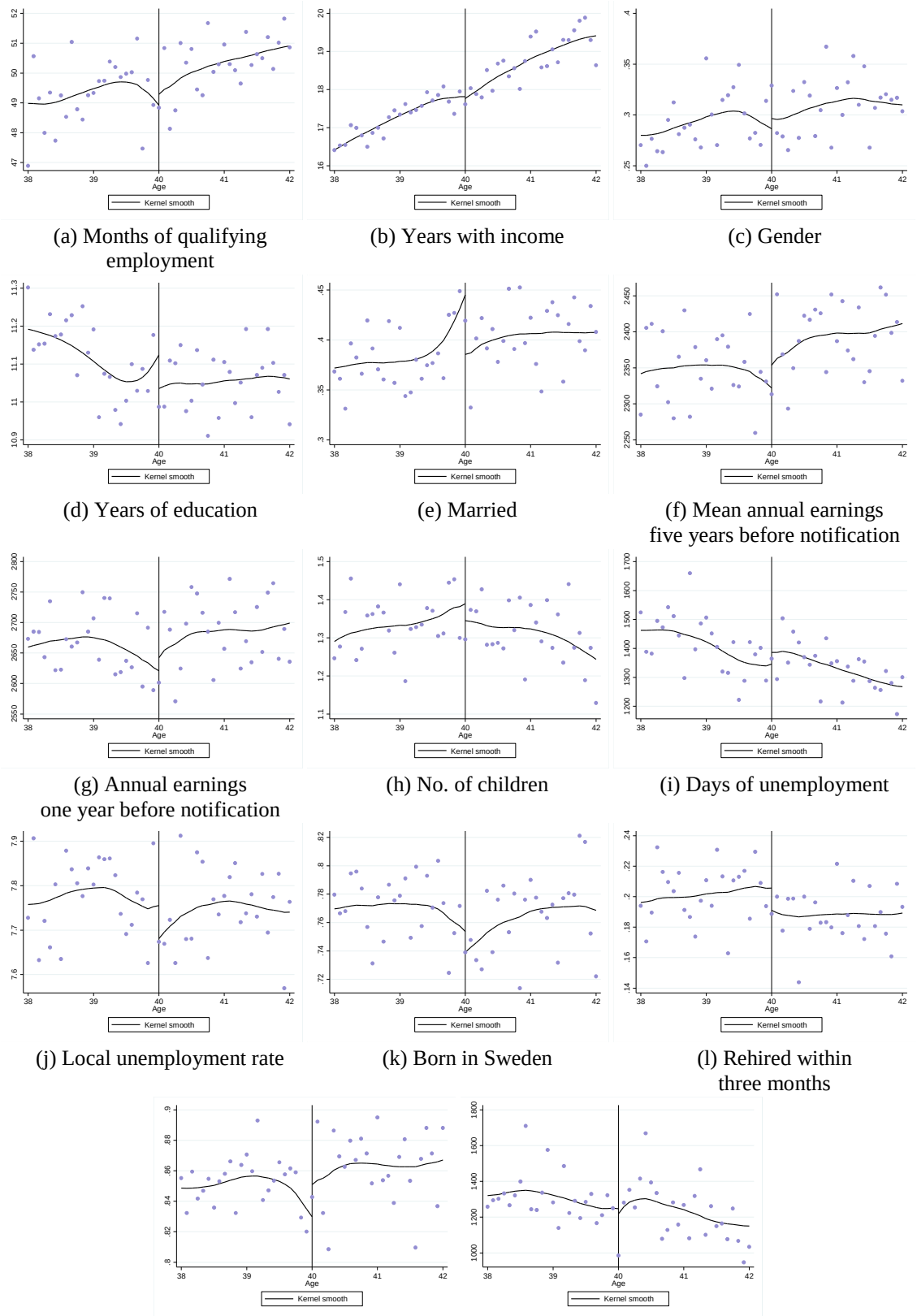


Figure 1. Distribution of displaced workers along the forcing variable

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:

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

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 potential 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.



(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

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.[27] 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 percentage points 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 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.

[27] 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.

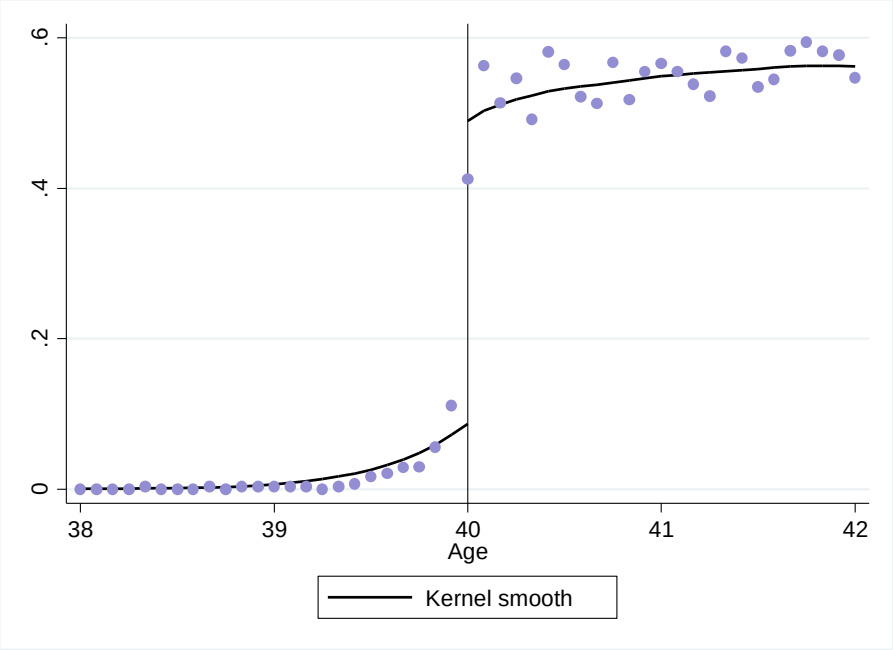


Figure 3. Probability of treatment by age

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 on unemployment and non-employment.

The results from the reduced form model in column 1 and the fuzzy RD model in column 2 both show that 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.[28] This result is

[28] 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

similar to the findings from Norway, where the lump-sum grant was found to prolong non-employment duration by between 37 and 41 days (Basten, Fagereng & Telle, 2014). 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.[29] 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, in principle 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)

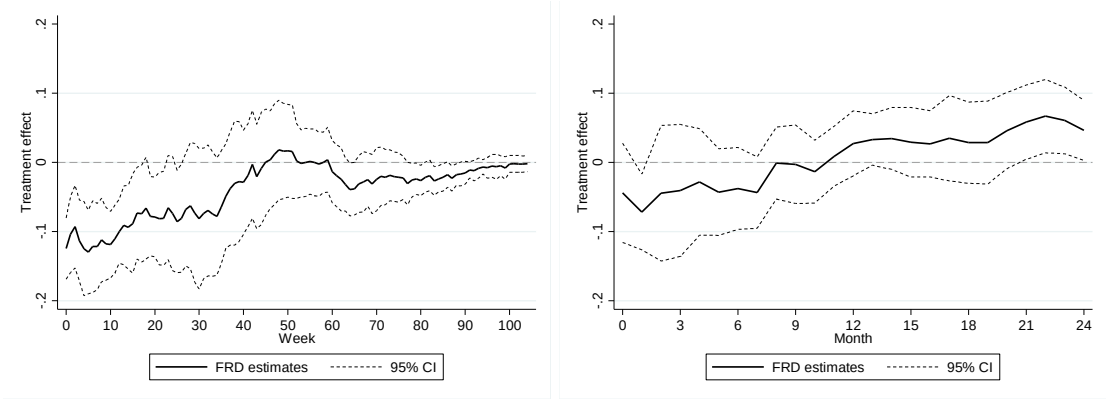
Note: Each cell represents the result from a separate regression, with each row showing the reduced form (RF) and fuzzy RD (FRD) results for a separate outcome. Clustered standard errors in parentheses, */**/*** indicates significantly different from zero at the 10/5/1 percent level respectively. The 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 the completed non- and unemployment durations (and even more so for the completed job duration). Censored observations are left out in the estimation of the effects on the average completed durations, but the data is informative about the effect on durations even if they are not completed at the end of

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.

[29] 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 follow up period. I have therefore also estimated the effect on non-employment and unemployment duration, or rather the effect on job finding, by considering the effect on finding employment within 24 months, or 104 weeks, in separate regressions using UI benefit periods and the employment records to define job finding in (a) and (b), respectively. The results are shown graphically in Figure 4. The values at zero weeks in (a) and zero months in (b) show the inverse effect on the probability of becoming unemployed and non-employed from Table 3, in other words the effect on *not* starting an unemployment and non-employment spell at all, respectively. For each week between 1 and 104 in (a) and month between 1 and 24 in (b), the figure shows the effect on no longer being or never have become unemployed or non-employed within that time, respectively.



(a) UI data (b) Employment records

Figure 4. Results on job finding

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 job finding according to the non-employment measure is smaller and only significant the first month after termination. The estimated effect on job finding 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 job finding. 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. Main results on job finding. 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.

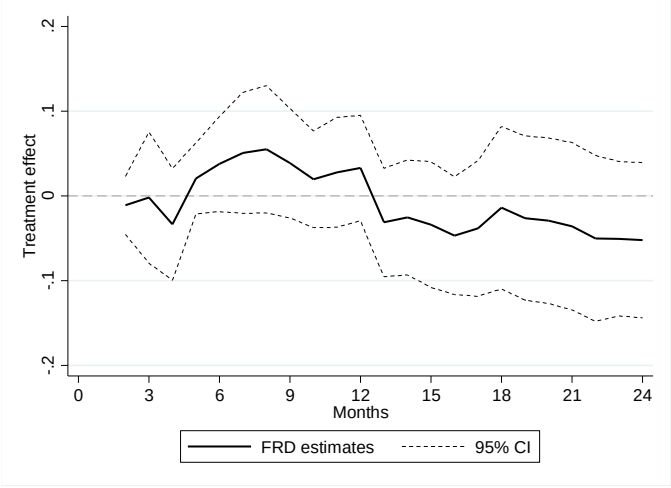


Figure 5. Results on first job lasting at least x months

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.

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 the Employment Security Funds' 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 the estimates for the effect on match quality 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.[30]

Within some collective agreements, which 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 Employment Security Funds' register.[31] 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

[30] 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.

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

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 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. Capital income may however be a poor measure of liquid assets since it includes negative capital income from e.g. mortgages and other loans.

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 previous 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 UI replacement rate of unemployment benefits. In Table A 9 the results are shown separately for workers with different replacement rates.[32] 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 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 11and Table 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.

[32] 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.

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 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. 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 in the estimated effects between the two job finding measures could reflect the fact that there is measurement error in the latter measure of job search duration, causing noisy estimates, although dynamics through a positive 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 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. The UI replacement rate does not seem to be directly decisive to the effects of the grant, nor the grant replacement rate (or the availability liquid assets, as far as they can be captured by measuring capital income one year before the notification).

The results could be interpreted as an indication that the level of unemployment benefits is below 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. 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 for different groups is needed. This is left to future research.

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Appendix

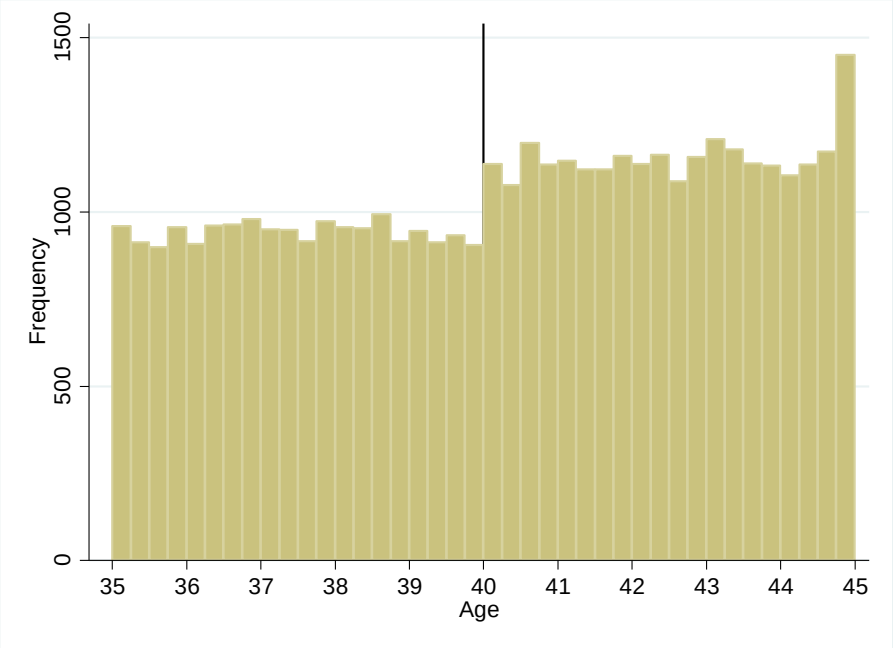
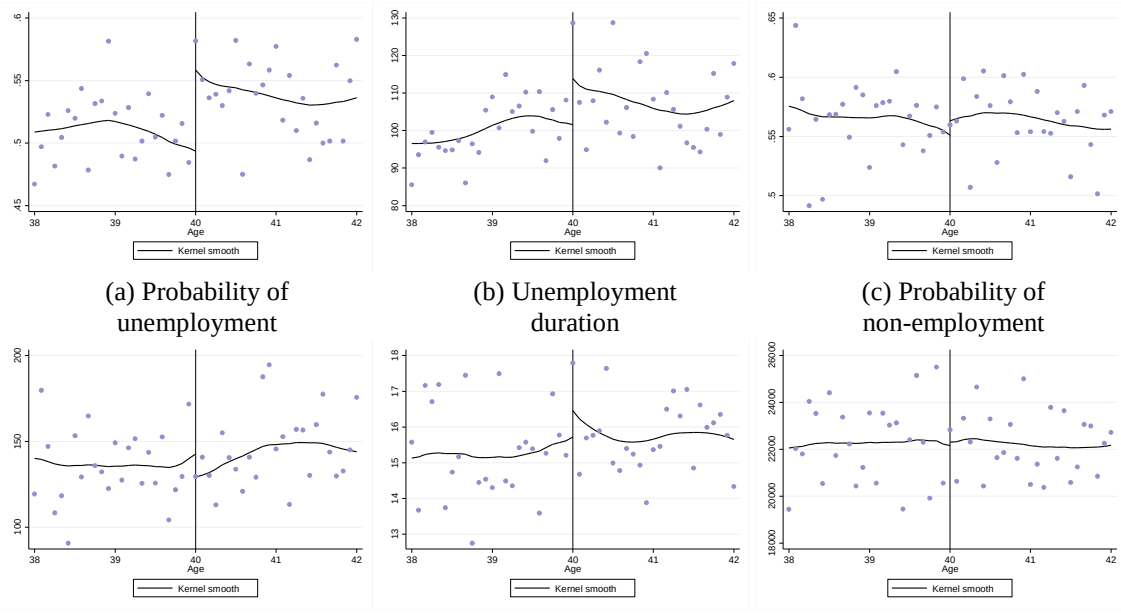


Figure A.1 Distribution of displaced workers along the forcing variable, full sample



(a) Probability of unemployment

(b) Unemployment duration

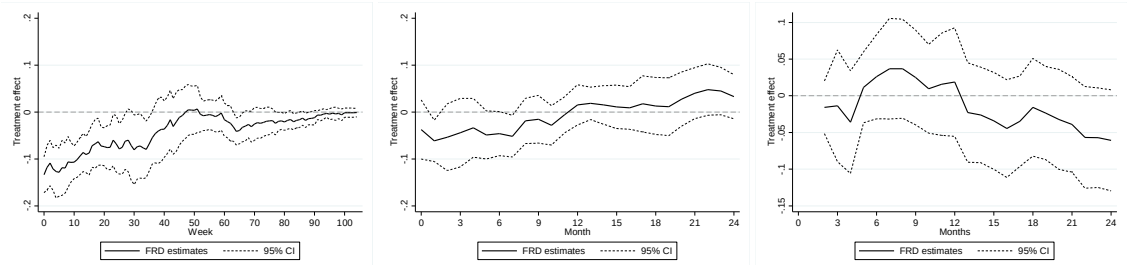
(c) Probability of non-employment

(d) Non-employment duration

(e) Duration of first job

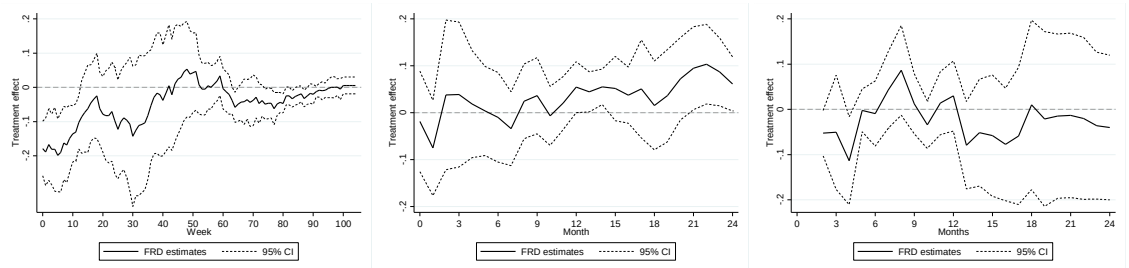
(f) Average monthly income at first new job

Figure A.2 Outcomes by age



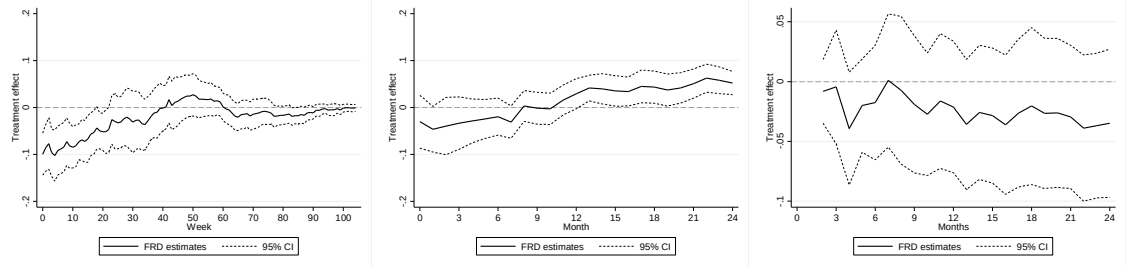
(a) Job finding, UI data | (b) Job finding, employment records | (c) Job duration

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



(a) Job finding, UI data | (b) Job finding, employment records | (c) Job duration

Bandwidth a-c: 6 months



(d) Job finding, UI data | (e) Job finding, employment records | (f) Job duration

Bandwidth e-f: 2 years

Figure A.4 Job finding 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)
	31.605	38.107
Mean annual earnings five years before notice (SEK 100)	(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) RF	(2) FRD	(3) RF	(4) FRD	(5) RF	(6) 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 Results 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 Results by subgroups, relative family disposable income

Outcome	High		Medium		Low	
	(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 Results 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 Results by subgroups, AGB replacement rate

Outcome	High		Medium		Low	
	(1) RF	(2) FRD	(3) RF	(4) FRD	(5) RF	(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 Results by subgroups, educational attainment

Outcome	Compulsory		High school		Tertiary	
	(1) RF	(2) FRD	(3) RF	(4) FRD	(5) RF	(6) 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 Results by subgroups, gender

Outcome	Men		Women	
	(1) RF	(2) FRD	(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 Results by subgroups, year of termination

Outcome	2006-2009		2010-2012	
	(1) RF	(2) FRD	(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.