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Early counselling of displaced workers:

Effects of collectively funded job search assistance

Josefine Andersson

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Early counselling of displaced workers

Effects of collectively funded job search assistance[1]

by

Josefine Andersson[2]

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Abstract

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

Keywords: Employment security agreements, collective agreement, job loss, mass-layoffs, notification, job search assistance, regression discontinuity design
JEL-codes: J59, J63, J68

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[2] IFAU and the Department of Economics, Uppsala University, *josefine.andersson@ifau.uu.se*

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1 Introduction

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

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

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

Job search assistance arranged in this form, as an insurance provided through collective agreements, is to my knowledge unique to the Swedish labor market. The content, however, resembles the assistance provided to the unemployed by Public Employment Services (henceforth PES) in many countries around the world. In the U.S. there are federally funded training programs for dislocated workers through the Workforce Innovation and Opportunity Act[3]. The most important difference is that the assistance provided through the Employment Security Agreements typically starts much sooner after the dismissal than in any of these cases. Outplacement services offer similar assistance, but there is not much evidence on their effects.

Approximately 60 percent of the Swedish labor force is covered by Employment Security Agreements.[4] Surprisingly, evidence is lacking on how these agreements affect the Swedish labor market and those enjoying the benefits. They could potentially have large effects on the functioning of the labor market, through the assistance provided in itself and through its interaction with public labor market policies. They may affect the body of unemployed, as redundant workers most often receive assistance even before they leave their current employer, and the effectiveness of public unemployment assistance may also be affected by the complementing measures provided through the agreements.

[3] And previously the Workforce Investment Act.

[4] 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.

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

This study is not only interesting by providing the first pieces of knowledge on how the assistance provided through Employment Security Agreements affect the Swedish labor market. The feature of Employment Security Agreements in the Swedish labor market, while an interesting phenomenon in itself, can also provide further answers to how the optimal public labor market policy should be designed. This study analyzes effects of very early and intensive assistance to job seekers as the assistance offered within the agreements typically start as soon as the worker has been given notice and continues throughout and after his or her period of notice. More than 85 percent of the sample starts the counselling program before their last day of employment with their current employer. The main objection to early intervention is the risk of deadweight losses; that providing assistance to all unemployed early in the unemployment spell might not be cost effective because of the unnecessary costs of assisting workers who would have found a job on their own regardless. There is little empirical evidence to support this objection (Weber & Hofer, 2004a). Thanks to the unique setting of the counselling program studied, this study contributes to the knowledge about the effects of very early intervention. To my knowledge, this is the first study that investigates the sole effects of job search counselling provided this early in the process of job loss.

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

I analyze the effects of the Employment Security Agreement for Swedish blue-collar workers displaced through mass-layoffs. I use data from the PES on individuals dismissed through layoffs of five workers or more, and data from the TSL Employment Security Fund (henceforth TSL) that administers the agreement on which of these workers have received assistance through the agreement, from 2006 to 2012. I combine this data with Swedish register data, providing a rich set of background variables as well as data on labor market outcomes. To identify the causal effects of receiving assistance through the agreement, I use a regression discontinuity approach based on the eligibility criteria for the assistance. I use the fact that workers must have been employed with one or several employers affiliated with the agreement for at least twelve consecutive months to be eligible for the assistance to estimate causal effects using a fuzzy regression discontinuity design. The empirical strategy allows me to study a LATE-effect around the cutoff, meaning that the results are estimated for a group of workers with short tenure. The sample consists of workers displaced through mass-layoffs, and a large proportion was displaced during the financial crisis 2008-2009.

The assistance provided by this agreement includes individual counseling and job search assistance from a personal coach, who can help map the workers competences, compose a CV and write job applications, train for job interviews etc. Workers may also receive some training as part of the program. I study the effects of this assistance on job finding rates and the quality of jobs found for individuals treated[5]. Since assistance is provided during the period of notice, I study how the agreement affects the probability of becoming unemployed, as well as the unemployment duration and the effects on subsequent income. The indicators of job quality studied are job duration and average monthly income in the first job after the displacement.

My results do not suggest that the counselling program has any effect on the probability of becoming unemployed or the unemployment duration for the group studied here. It also does not seem to have any significant effect on subsequent income within

[5] The empirical strategy does not allow the study of potential crowding-out effects for other job seekers, which are therefore ignored in this study.

two years following termination, or the average monthly income within the first job found for this group. My results do, however, indicate that the program has a strong, positive effect on the duration of the next job. The results do not suggest that the effect depends on how soon the counselling program starts.

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

2 Background

2.1 Employment Security Agreements

Employment Security Agreements emerged as a complement to public labor market policy and has a long history in the Swedish labor market. The first agreement was signed during the 1970s, initiated by white-collar workers who considered regular labor market policies inadequate to meet their needs for assistance when transitioning between jobs. The union and employers agreed to incorporate assistance that was better adapted to meet these needs, into the collective agreement. Nowadays, such an agreement is no longer exclusive to white-collar workers. Similar agreements have been incorporated into collective agreements for a large proportion of the labor force. Today, around 60 percent of the Swedish labor force is covered by Employment Security Agreements[6].

Assistance through the agreements is provided to workers who are dismissed due to redundancy, and who meet a set of eligibility criteria that differs between agreements. As a rule, only permanently employed workers are covered, but in recent years, temporary workers have been made eligible for at least some of the benefits within some agreements. The scope of Employment Security Agreements is expanding and remains an important matter in collective bargaining in Sweden.

The agreement that is studied in this paper, reached between the Confederation of Swedish Enterprise (*Svenskt Näringsliv, SN*) and the Swedish Trade Union Confederation (*Landsorganisationen, LO*), covers around 900,000 privately employed blue-collar

[6] There are four large Employment Security Agreements in Sweden, basically divided by sector. The municipal sector agreement is the largest one in terms of workers covered, covering 1.1 million workers. The other two large agreements, aside the on being studied in this paper, cover 950,000 privately employed white-collar workers and 250,000 state employees respectively. There is also a number of smaller Employment Security Agreements that cover on average a few ten-thousand workers each.

workers in Sweden, or over 30 percent of all employed workers[7]. Almost 100,000 companies are affiliated with the agreement. The agreement covers all blue-collar workers employed with employers who have signed the collective agreement between these two parties[8], regardless of whether the worker is a union member or not. This is the largest Employment Security Agreement in terms of the number of workers enrolled (Walter, 2015). Out of all blue-collar workers being notified of displacement during the period of study, according to the PES register on notices, 78 percent are notified from firms affiliated with this agreement. Out of these 64 percent are treated through the counselling program, or 50 percent of all notified blue-collar workers. Out of all workers in Sweden who are notified of displacement according to the PES register, 35 percent enter the employment security counselling program studied here.

Employment Security Agreements are administered by specific organizations called Employment Security Funds or –Councils. The benefits stipulated in the agreements are financed through a fee paid by employers, amounting to a small percentage of their total wage costs[9]. The SN-LO Employment Security Agreement has been in place since 2004. The agreement is administered by the TSL Employment Security Fund. The fee paid by SN member companies is 0.3 percent of total wage costs throughout the affiliation period. Workers do not apply for the program themselves. The union and the firm together file the application for workers involved in a layoff. In the case of bankruptcy, the union alone files the application on the workers behalf. The counselling itself is not provided in-house by TSL, but is instead purchased from local suppliers. The employer and union choose which supplier will provide the counselling for all workers involved in the specific layoff, from a list of suppliers preapproved by TSL. It is voluntary for the worker to take part in the program.

The assistance provided is different from traditional labor market policies in the sense that it is earlier, more intensive, more focused on individual counselling and more responsive to the needs of the individual worker. In Sweden, the PES provides more intense measures only to those who have spent a long time in unemployment or to targeted groups of unemployed, e.g. young unemployed or individuals who are deemed at risk of becoming long-term unemployed. Through the Employment Security Agreements, all

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

workers who lose their job due to redundancy enjoy early and individually oriented counseling and assistance, as long as they meet the basic eligibility criteria. The purpose of the agreement is to facilitate the transition process for workers and firms and aid the workers in swiftly finding a new job. Each eligible worker is provided with a personal coach who councils the worker in the search for a new job. The aim is for each displaced worker to find a new job, or other solution, within twelve months, but also that the new job is stable. The suppliers are assessed according to the share who has received a job within twelve months from the notified last day of employment, but also according to the share of satisfied program participants, unions and firms. Examples of services that the coach provides is to help the worker to map his or her competences, define the range of possible job opportunities, compose a CV, write job applications, and train for job interviews. Once the worker is provided with a coach they compose the job search program together according to the workers' individual needs. There are no guidelines stating how often the coach and worker should meet or how, or in which activities to engage in. This is entirely up the worker and coach to decide. Workers can also receive a shorter training effort, such as training to get a forklift operating license or the like. During the period of study, however, these training efforts were used very restrictively. They were only possible if they could be shown to yield a concrete employment opportunity, and should not intervene too much with the counselling. In special cases, more intensive training efforts could substitute the counselling. Since 2017, a new working model has been introduced which has increased the possibilities of obtaining training measures for workers in need of this.

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

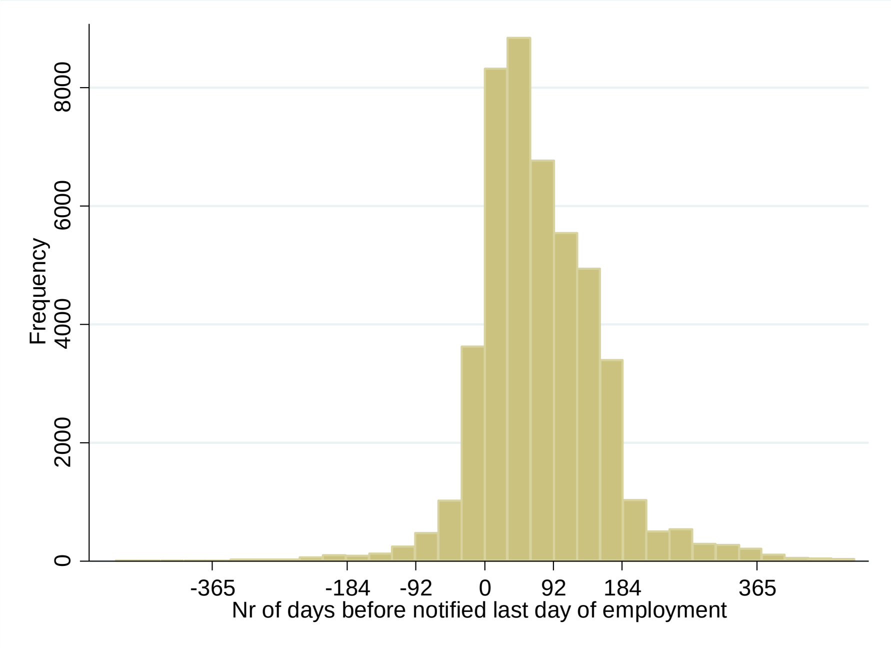


Figure 1. Timing of program start

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

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

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

2.2 Previous literature

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

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

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

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

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

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

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

3 Empirical strategy and data

3.1 The regression discontinuity design

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

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

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

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

Using the RD-design, I compare individuals who are as equal as possible except for treatment status. However, the design in itself is based on the fact that individuals have different values of the forcing variable, which drives treatment eligibility. If the forcing variable affects the outcome, the results will be biased. It is to circumvent this issue that the sample is restricted to those with values of the forcing variable that lie within a small range just around the cutoff. Since these workers are similar also with respect to the forcing variable, the hope is that the bias is negligible. How wide this range should be is a trade-off between precision, which increases with the range, and comparability of the individuals, which increases the narrower the range is. There are some data-driven methods to find optimal bandwidth sizes. The optimal bandwidth size according to, for example, Imbens & Kalyanaraman (2012), varies greatly across the outcome variables used in this study and also does not take into account the monthly character of the employment records that the forcing variable is based partly on (see section 3.1.1). I have chosen a three month bandwidth for the baseline model (however, as I will show, the conclusions are not changed using a somewhat smaller or larger bandwidth). This means that observations are reasonably close to the cutoff, while the sample size is not too small. The fuzzy nature of the cutoff in this study also means there is room for some overlap with values of the forcing variables above the cutoff in both the treatment and control groups. The same bandwidth is used for the estimation of the first and second stage results, and instead of the simple RD model in (1), I use a triangular kernel local linear regression model.[10]

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

3.1.1 Measurement error issues

I calculate the forcing variable, qualifying employment time, using employment records collected by the Swedish Tax Agency and provided by Statistics Sweden. Employment records contain monthly data on employment periods.[11] I know the exact date of each worker's notified termination date, the date which is relevant for the determination of eligibility, but since I use the employment records to find the start of the employment, I do not know the exact start date. Assuming that employment always starts the first day of the first month, this induces a one-sided measurement error in the forcing variable. My

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

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

measure of the forcing variable, x, is an overestimate of the true value, x^*, by at most one month:

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

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

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

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

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

3.2 Data

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

These data are matched to Swedish register data that provide a rich set of background variables as well as information on labor market outcomes. The register data stretches back to 1985 in many cases, and data on outcomes are available up until 2014. The register data are also used, together with the list of affiliated employers, to identify the total qualifying time of employment, for the implementation of the RD-design. The notification date is not included in the data from the PES, and is therefore estimated for the control group. I use the most common notification date according to the TSL register among those within the same notification. As a robustness check, I have also used the date when the PES received the list of notified individuals, which must be done at least one month before the first person leaves the employer and must include copy of the written notification letter handed to the employee. The conclusions remain unchanged.

I investigate the effect of the counselling services on the probability of becoming unemployed and unemployment duration. For the main results, I define unemployment as receiving UI benefits between the notification date and three months after the notified termination date.[12] The unemployment duration is defined as the number of days between the first week with UI benefits payment and the last, allowing for gaps of a maximum of four weeks between payment periods. If no UI benefit is received, unemployment duration is zero. As treatment in this case can affect the probability of becoming unemployed, this outcome may be considered endogenous. Ideally, from a program evaluation perspective, the effect on job finding rates would be measured from the

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

notification date, since this is when treatment can first start. However, due to the data limitations concerning notification dates described above, unemployment probability and duration are the preferred outcomes in this study.[13] From a public policy perspective, this is a relevant outcome even though the timing of job finding within the period of notice is indistinguishable.

I also investigate the effects on the quality of jobs found, measured as job duration and average monthly income. These outcomes are measured using the employment records described above, which include earnings for each employment period reported. I also look at total earnings during the first and second year after notice. Duration of the first job found is measured as the number of months consecutively employed with the first employer after the notification date[14]. If the consecutive employment period is right censored, this outcome value is missing. Since the employer can only report one starting and ending month per year in the register data on employment periods, a gap between periods will not be reported if they occur during the same calendar year. This poses a problem when trying to identify time until reemployment with the same firm. Rehires without gaps in employment periods according to employment records are counted as occurring within the period of notice in the main result estimations[15].

The data from TSL include information about all workers receiving treatment through the agreement. This means that the data includes workers given notice within smaller notices than those reported to the PES. As these treated differ systematically in terms of the size of the notice from the notified individuals who are found in the data from the

[13] Notification dates for the control group are estimated since they are not available in the data. An analysis based on this date is therefore not convincing. The unemployment outcome used allows unemployment start to be measured over a more flexible time frame which reduces the concerns from using estimated notification dates. It cannot, however, be used to evaluate the effects on the job finding rate over the notification period. As a robustness check, I have used employment period data to distinguish the timing of job finding before the notified last day of employment. Effects using this specification are, however, not stable to different specifications and conclusions are therefore uncertain.

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

[15] As previously mentioned, register data on employment records contain monthly data on employment periods. This means that there is measurement error in employment periods if a worker has multiple employment periods with the same employer during the same calendar year. When no gap is observed in employment periods, and the worker continues working at the dismissal firm the following calendar year after the notified last day of employment, I interpret this as a rehire. The timing of the rehire decision is however unknown, which is a problem for the estimation of job finding rates and job duration. It might be during the period of notice, or thereafter but within the same calendar year. Using data from the PES on unemployment periods from enrollment periods and unemployment insurance payment periods, I have calculated alternative rehire dates based on ending dates from these records. An enrollment period ends when the worker is not registered as unemployed without employment according to unemployment categories, and when UI payment periods end for a period longer than four week. If the worker is not enrolled or receives UI payments between the notice and the next job according to employment records, or between the notice and the next calendar year after the last day of employment for rehires, they are assumed to not have become unemployed and reemployment happened during the period of notice. It turns out that the vast majority of rehires happens within the period of notice according to these calculations.

PES, I have restricted my sample to the sample of notified workers reported to the PES[16] so that the treatment and control groups are comparable in this respect. This means that I ignore 61 percent of the available sample of treated[17]. This also affects the interpretation of the results. I estimate the effect from treatment on individuals displaced through layoffs of five people or more, rather than the average treatment effect of all treated. It is more likely that larger companies, who are more likely to be the source of these mass-layoffs, are better equipped to provide those given notice with additional assistance from the company side which may affect the effectiveness of treatment negatively, assuming decreasing marginal utility of assisting measures. In very large layoffs it is also possible that other stakeholders, such as the government, steps in. A large proportion of the sample was displaced during the financial crisis in 2008-2009. The inflow into the counselling program was extreme during this period compared to both before and after. The public employment service were in many cases also involved at an earlier stage within large dismissals during this period. Therefore, it is possible that the estimated results for treated from these mass-layoffs underestimate the true treatment effect of *all* treated.

3.3 Descriptive statistics

Descriptive statistics for the full sample, the observations within the three month bandwidth around the cutoff, and an extended sample including all treated within the TSL registers, are presented in Table 1. Differences are larger comparing both the full and the extended sample to the sample close to the cutoff. The qualifying time of employment is of course shorter, and this is accompanied by differences in some other characteristics as well. The sample close to the cutoff are on average seven years younger than the full sample and thus have shorter prior labor market experience (5.5 years on average), have earned almost half as much income on average the past five years and are less often married. They are also involved in smaller layoffs on average (among the layoffs of at least five people) than the full sample, and are displaced from smaller firms accordingly. This is not surprising given the priority principle provided by Swedish law for dismissals due to redundancy, where the default is that the last hired is first displaced. Qualifying time of employment is highly correlated with tenure with the company. Individuals with

[16] I have only included individuals who appear once in the matched sample of notified workers from the PES and TSL, or more than once but from the same data source, to ensure individuals are not double counted once as treated and once as controls, due to misreporting of dismissal firm or –date, so that they are not matched but is in fact the same dismissal.
[17] The number of TSL application projects is reduced from 26,838 to 4,514.

a longer qualifying time of employment are thus less often notified when the layoff is small. Individuals close to the cutoff are also somewhat less often women, have somewhat longer education, have spent more time in unemployment and are somewhat less often born in Sweden than the full sample.

Table 1. Descriptive statistics

	Close to cutoff	Full sample	Extended sample
Days of qualifying employment	372.55	2,394.66	2,443.11
	(67.21)	(1,926.33)	(1,944.87)
Age	32.07	39.10	39.97
	(11.59)	(12.88)	(12.76)
No. of years with income	9.00	14.45	15.14
	(7.68)	(7.96)	(7.95)
Gender (1=Woman)	0.24	0.26	0.27
	(0.43)	(0.44)	(0.44)
Years of education	11.24	11.08	11.03
	(1.60)	(1.59)	(1.57)
Married	0.23	0.32	0.33
	(0.42)	(0.47)	(0.47)
Mean annual earnings five years before notice (SEK 100)	1,130.24	2,216.16	2,222.01
	(951.80)	(973.28)	(943.07)
No. of children in household below 18	0.62	0.63	0.63
	(0.95)	(0.98)	(0.97)
Days of unemployment	903.36	819.26	874.84
	(1,162.48)	(1,035.52)	(1,083.46)
Local unemployment rate (county level)	7.55	7.70	7.76
	(1.50)	(1.47)	(1.46)
Born in Sweden	0.77	0.80	0.81
	(0.42)	(0.40)	(0.39)
Size of notice	39.91	115.45	–
	(105.90)	(308.63)	
Firm size	1,116.46	1,483.43	1,250.47
	(2,172.23)	(2,972.27)	(2,714.03)
No. of observations	2,570	68,661	143,980

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

3.4 Validity of the empirical strategy

To be able to use the fuzzy RD design there must be a strong first stage relationship. The discontinuity plot in Figure 2 shows the share of treated by days of qualifying employment. The plot shows that there is a jump in treatment at the cutoff when I exclude the observations with values of the forcing variable just above the cutoff (my donut). The underlying scatterplot reveals that the probability of treatment does not have an equally clear jump at the cutoff without the donut, but instead, as expected, increases gradually within the “donut-range”. This suggests that the number of workers who truly cross the threshold of twelve months of qualifying employment increases as my overestimated measure of the forcing variable increases within the discarded range, in the expected manner. The first stage results in Table 2 confirm that having qualifying employment time above the cutoff of twelve months increases the probability of being treated, by 35 percentage points.

Table 2. First stage relationship

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

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

Figure 2 also reveals that there is a significant share of treated also below the cutoff. One possible explanation for this is that there is a lack of stringency in the implementation of the eligibility rules. However, according to TSL, they are quite strict in enforcing the rules for eligibility. Another possible explanation is that the list of firms affiliated with the agreement contains errors which yields further measurement error in the forcing variable.[18] Since I use the fuzzy RD approach, as long as this source of error is smooth at the cutoff, it does not bias the results.

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

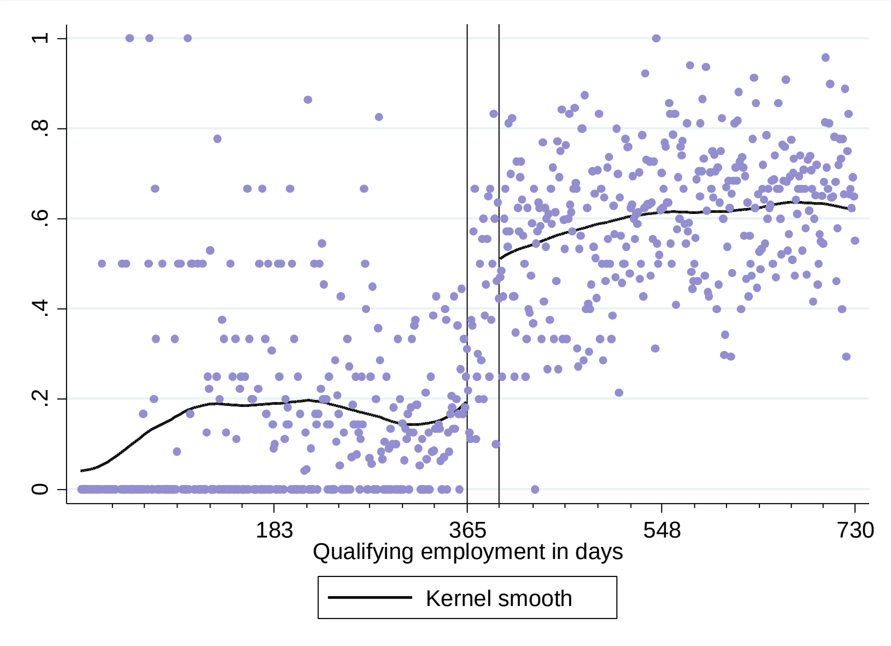


Figure 2. Share of treated by days of qualifying employment

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

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

where Y_1 denotes the potential outcome when treated and Y_0 the potential outcome when not, T_i denotes the treatment status and $\mathbf{X}_i$ a set of predetermined characteristics (in the regression discontinuity case the forcing variable should be sufficient). In other words, we need that individuals are not able to exactly control the value of the forcing variable around the cutoff, so that they in effect choose their own treatment status according to potential outcomes. Workers are dismissed by the firm due to redundancy, and it is not likely that they can plan their notified last day of employment to receive or not receive treatment. However, we might worry that firms manipulate the notification date or the length of the notice period to ensure that workers are treated. Since the assistance is paid collectively through the continuous fee and is not a direct cost to the dismissing firm, incentives to withhold assistance from workers is small. The payment procedure does however require firms to pay for the assistance before they are reimbursed by TSL. This may provide incentives to withhold assistance, but on the other hand the union is also involved in the application process and is likely to counteract such incentives from affecting treatment status. There is no way to know for sure if this is the case or not. However, it can be tested by investigating how the density of notified workers in the sample evolves at the cutoff. Figure 3 is a histogram of the distribution of workers above and below the cutoff in the forcing variable, normalized so that the cutoff value is at zero.

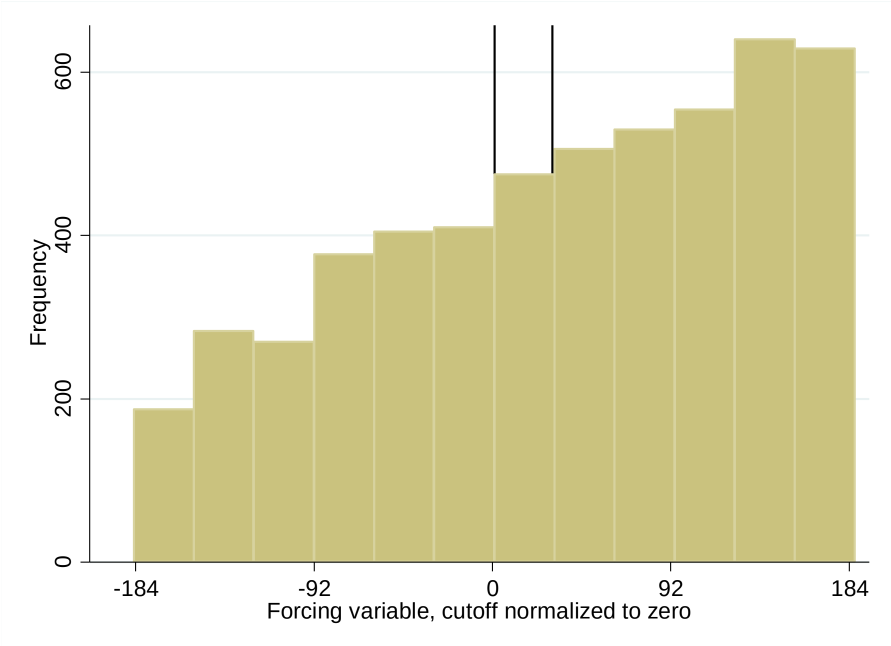


Figure 3. Distribution of displaced workers along the forcing variable

To account for the structure of the data, a bin size of one month is used. The number of notified workers does not exhibit a significant jump at the cutoff. This is confirmed by the result of the McCrary density test, which delivers an insignificant estimate of the discontinuity at the cutoff[19]. There is thus no evidence of manipulation of the forcing variable.

The regression discontinuity approach also relies on the assumption that observations on either side of the cutoff value of the forcing variable are similar so that the treatment assignment can be considered as if random just around the cutoff. This means that we assume that the expected value of potential outcomes given the value of the forcing variable, are developing smooth at the cutoff, i.e.:

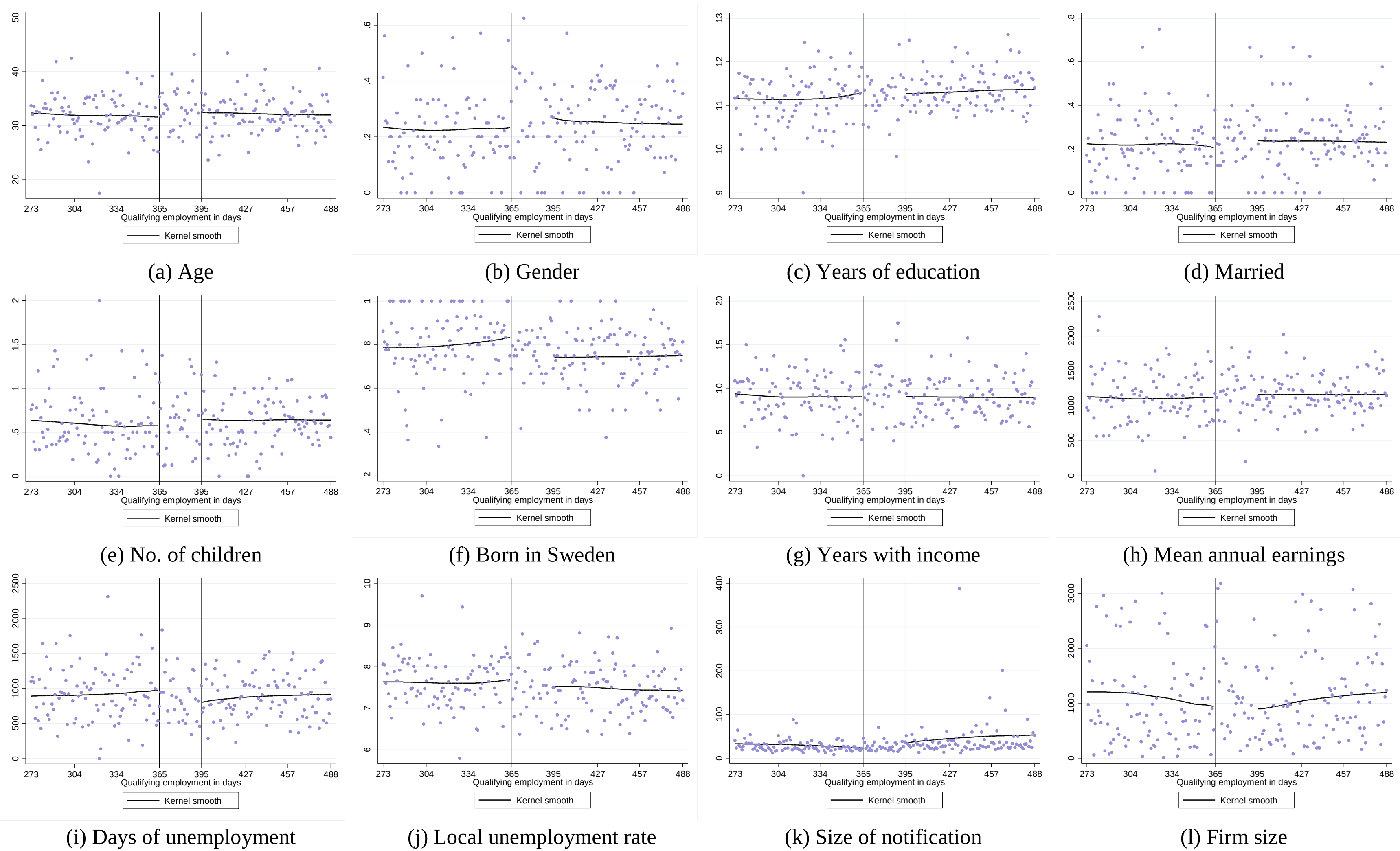
[19] The McCrary density test is a test commonly used in with the RD approach to test whether there is a discontinuity in the density of the forcing variable at the cutoff. The bin size used to perform the test is one month, again to account for the structure of the data, which places the start of each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

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

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

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

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



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

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

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

Figure 4. Basic characteristics by days of qualifying employment

4 Results

The main results for the effect of the employment security counselling program are found in Table 3. The reduced form (RF) estimates in column 1 show regression discontinuity estimates from a sharp RD model around the cutoff. This would be considered an intention to treat effect if the forcing variable was the only eligibility criteria. Since the forcing variable does not alone determine treatment, the fuzzy RD results (FRD) in column 2 uses the forcing variable as an instrument for treatment to estimate the causal effects from treatment. The point estimates for the probability of unemployment is positive, but insignificant, in both columns. The same is true for the unemployment duration.

Table 3. Main results

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

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

The effects on the quality of jobs found can only be estimated for those who find a new job during the period I study and is therefore an endogenous outcome. 93 percent of the workers in the sample, however, do find new employment within the follow up period. The effect on the completed duration of jobs found is positive, but not significant. The average completed job duration increases with around seven months according to the point estimate. For the estimation of the effect on the duration of the new job, we also need that the employment has ended during the period of study. The sample size is therefore naturally lower for the average duration outcome. Within the bandwidth, 73 percent in total, or 77 percent of those who find a new job, have ended it during the follow up period. The completed job duration is right censored, but the data is informative about the effect on job duration even if the job duration is not completed at the end of the follow up period. I have therefore also estimated the effect of jobs lasting at least a certain number of months, for which the censoring problem is smaller. In the main results the effect on the next job lasting at least 6, 12, 18 and 24 months are presented (in Figure 6 this effect is estimated for all months from 2 to 24). The results show that there is a significantly positive effect on the duration of the next job, in terms of the job lasting at least 12, 18 and 24 months, but not for lasting at least 6 months. The highest point estimate, for the effect of the job lasting 18 months, suggests that there is a 42.5 percentage points higher probability that the job lasts at least 18 months if you are treated. There is no statistically significant effect on average monthly income at the first job found after notice, or for the total income the first or second year after notice, although the point estimates are positive.[20] Estimations of all results in Table 3 include covariates, as stated in section 3.1.1. If these covariates are not included, the conclusions remain unchanged.[21]

Since the follow up period ends 2014, both the subsequent job duration and the unemployment duration are potentially right censored. To lessen the censoring problem, I have also estimated the effect on job finding, as the opposite of (still) being unemployed, each week within the first two years after the notified termination date, as well as the effect on the next job lasting at least 2-24 months, and plotted the results in Figure 5 and Figure 6, respectively.

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

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

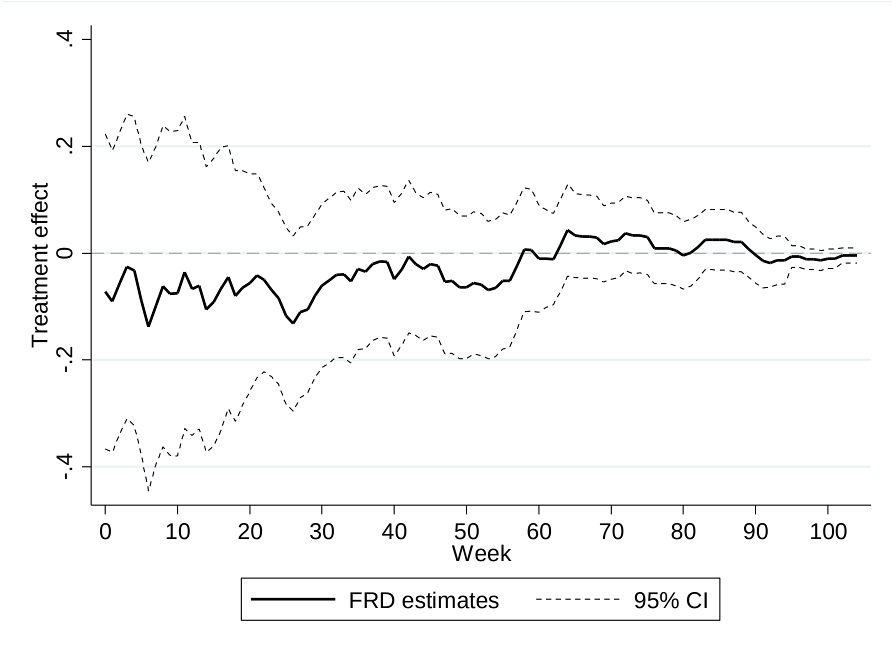


Figure 5. Treatment effect on job finding within x weeks

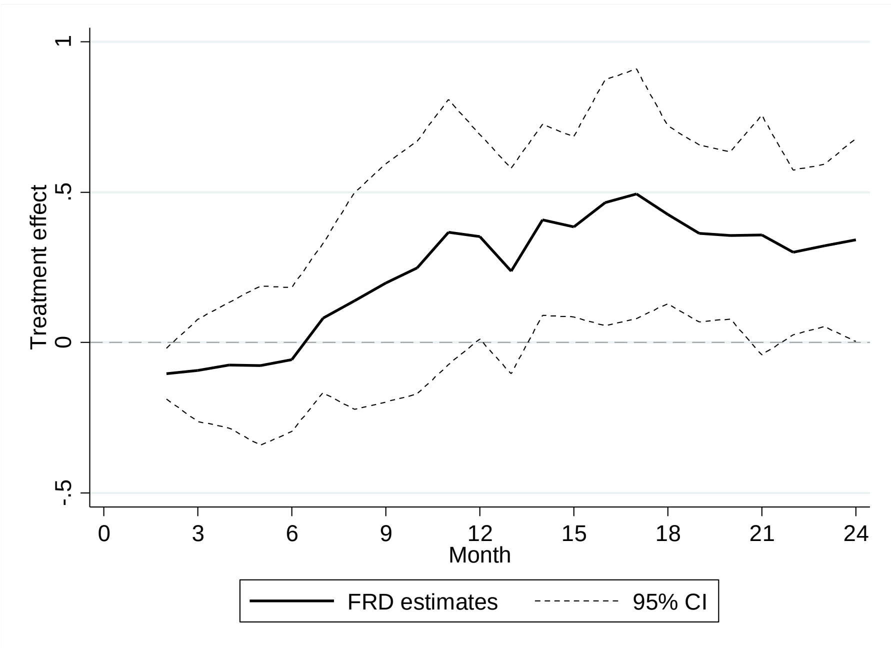


Figure 6. Treatment effect on first job lasting at least x months

Figure 5 shows the fuzzy RD results of an attempt to illustrate job finding within the first 24 months after the notified termination date. The value at zero weeks after termination shows the inverse effect on the probability of becoming unemployed from Table 3, in other words the effect on *not* starting an unemployment spell at all, and for each week between 1 and 104 the figure shows the effect on no longer being or never have become unemployed within that time. The results show that there is no significant effect on job finding during the first two years. All estimates are insignificant. Since the time limit for the job search program is at most one year, any positive effect should be detectable during this follow up period. Figure 6 shows the treatment effect on jobs lasting at least 2-24 months, respectively. The effect is negative but insignificant months 2-6, but is thereafter positive, and significant estimates are found for jobs lasting at least 12 months and most estimates thereafter.

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

4.1 Robustness analysis

I have performed a number of robustness checks to examine the robustness of the main results presented above. The first involves estimating effects using alternative measures of the outcomes above. These results are shown in Table 4. Using enrollment at the PES[22] instead of UI receipt to measure the probability and duration of unemployment yields the same conclusions as above. The point estimates are somewhat smaller for the unemployment probability while the unemployment duration effect is larger, perhaps reflecting an effect on registering at the PES before unemployment and eligibility for unemployment benefits start, but the point estimate is insignificant using this measure as well. Using employment records to instead measure the probability of non-employment and the non-employment duration[23], a somewhat different picture emerges. The point estimate for the probability of non-employment is large and negative, although not significant, but the estimate for the effect on non-employment duration shows a very large and significant negative effect. Note that the non-employment duration is negative if a job is found during the notice period. These estimates, since based on employment records, are highly

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

[23] Non employment is measured as having a gap in employment periods according to employment records. The length of this gap is measured in days since I have the precise notified termination date from the TSL and PES data, although the employment records contain monthly data. If the new employment is found during the notice period, the value of the non-employment duration is negative. Self-employment is not counted as becoming employed.

sensitive to the inclusion of the fixed effects for the month of notified termination. Concerns of measurement error in employment records is the reason for including these fixed effects in the first place. Without including covariates, the effect is instead insignificant and very close to zero.

Table 4. Results, alternative outcomes

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

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

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

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

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

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

My sample is defined by one data source, the PES register on notices, while treatment status is identified using another, the TSL register. To estimate causal effects, I need to compare treated with comparable control units *at the start of treatment.* Since controls are collected using a different data source than treated units, there is a risk that the timing of inflow into the different data sources differs in such a manner that the results are affected. If sample inclusion is determined at a point in time prior to the determination of the treatment status, there is a risk that the probability of entering the treatment program is affected by outcome variables. If a notice is first reported to the PES, and it then takes a while before TSL is involved through an application, some affected individuals may have been rehired and are therefore not included in the treatment group even though they were eligible. I have examined this possibility by comparing the date that notified individuals are reported to the PES and TSL receives the application for transition support for all eligible workers. It does not seem to be a systematic timing difference – the dates are often close in time[24] and the PES date is before TSLs' date about half the time and vice versa. This however does not ensure that individuals may not find a job before they have had their second meeting with the coach and thereby started the counselling program.

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

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

significant. The estimates for the income related outcomes are positive but not significant as when using the main sample.

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

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

Table 5. Reduced form results, notice periods

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

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

The results from the placebo cutoff are shown in Table 6. At this cutoff, there is no discontinuity in treatment assignment and no effect should therefore be found using this specification. Any measurement error- or spurious effects due to having a full year value of the forcing variable should be picked up using this cutoff value. The first stage relationship is close to zero and not statistically significant. The reduced form estimates are close to zero and insignificant (except for the income related outcomes, which are in any case also insignificant). This indicates that the results are not driven by the discontinuity in the length of the notice period.[25]

Table 6. Results using the placebo cutoff

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

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

4.2 Heterogeneous effects

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

[25] Ideally the exclusion restriction could be tested by estimating the same model for a group of workers who are not affected by the Employment Security Agreement. However, since other agreements cover large parts of the Swedish labor force, and use the same eligibility cutoff with respect to qualifying time of employment, such an analysis would not be informative of counterfactual outcomes around the cutoff. Furthermore, the current dataset does not include information about notifications from other sectors.

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

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

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

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

It is also interesting to consider how the effect of the counselling provided by the agreement differs between workers according to their own characteristics. I investigate how the effect differs with respect to workers age. The results are shown in Table A.9. Since the sample close to the cutoff is on average younger than the overall sample of notified workers, the division of workers across ages must keep age groups relatively young compared to the age distribution in general among notified workers. I estimate treatment effects separately for workers below 25 years, between 25 and 39 years and 40 years or older. The first stage relationship is not significant for the oldest group. The results indicate that the effects are better for those youngest. The unemployment duration estimate is negative, although not significant, and the job duration effect is completely driven by this group.[26]

4.3 Extension

Aside the labor market outcomes studied so far, there are a few other outcomes that may be affected by the counselling program provided by the SN-LO Employment Security Agreement. An objective of the counselling is to shorten the time spent in unemployment,

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

and to do so the individual needs of each worker is in focus. For some workers, this may not mean finding a new job. I therefore study the effect of the treatment on a number of other outcomes, as an extension. In particular, I investigate whether the counselling program had any effect on the probability to start an own firm, going into higher education, and receiving disability insurance or getting social assistance. Starting an own firm is a binary variable that does not take into account the success of the company. Going into higher education is proxied by the receipt of student aid. In Sweden, higher education is free and student aid is granted to all students accepted to a course or program[27]. The receipt of student aid is therefore almost universal among students within higher education. The results show that the program has no significant effect on starting a company or receiving disability insurance or social assistance[28]. However, there is a negative effect on going into higher education which is statistically significant the second year and onwards. Estimates of the effect for this outcome are presented in in Table 7.

Table 7. Results, higher education

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

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

The results suggest that the counselling program may convince some workers to stay in the labor market instead of going into education after being notified. This is also in line with the difference between the results for non-employment and unemployment probabilities and durations previously presented (although the main results for the non-employment outcomes are not robust to the notification date estimation strategy).

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

[28] Estimated results are available on request.

5 Conclusions

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

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

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

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

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

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

In order to paint a clear picture of the effects of the Employment Security Agreement studied here for the participant group as a whole, or of Employment Security Agreements in general, more research is needed. The results from this study, however, do suggest that these agreements may have important impacts on the matching of the Swedish labor market.

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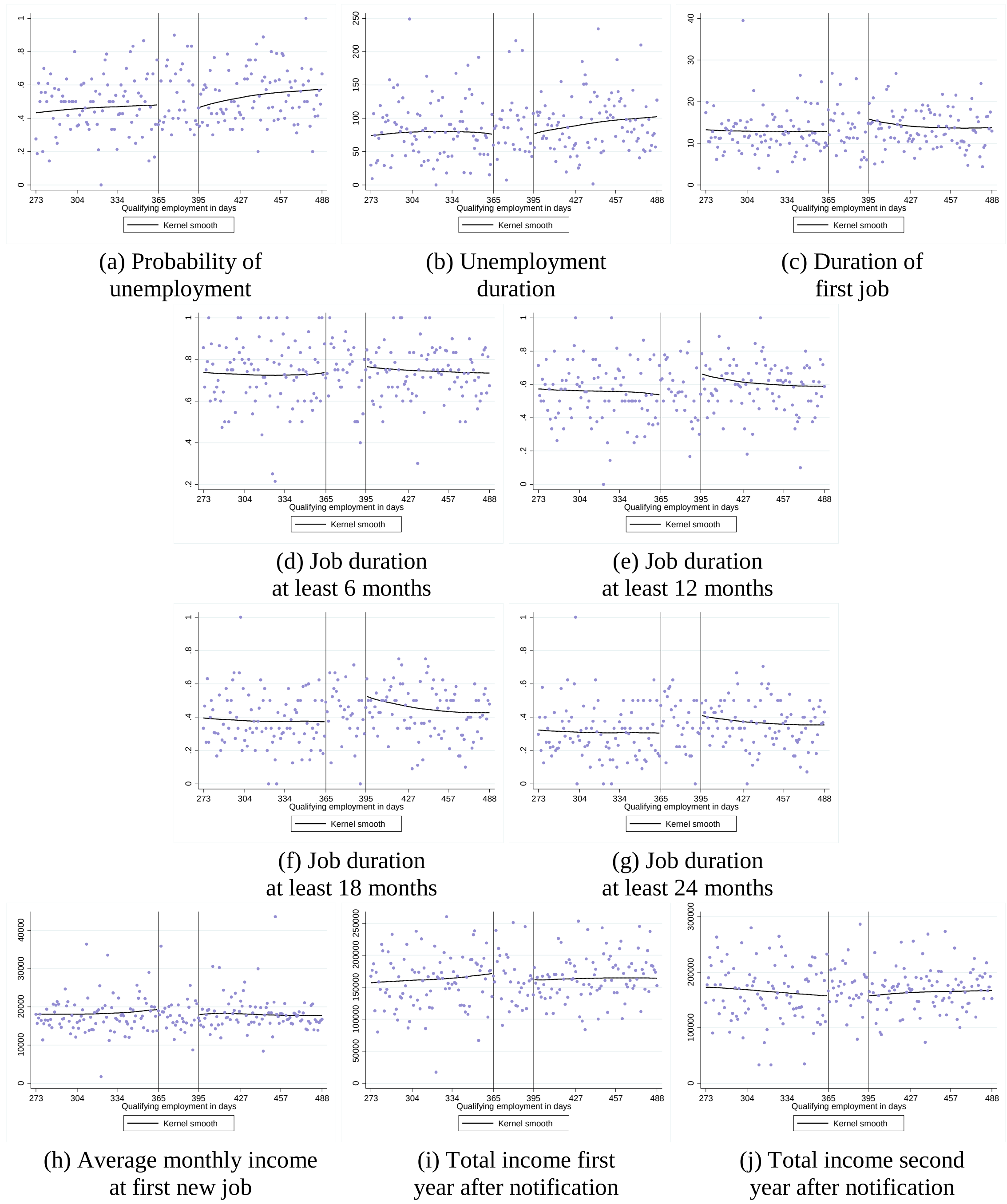
Appendix



(a) Probability of unemployment

(b) Unemployment duration

(c) Duration of first job

(d) Job duration at least 6 months

(e) Job duration at least 12 months

(f) Job duration at least 18 months

(g) Job duration at least 24 months

(h) Average monthly income at first new job

(i) Total income first year after notification

(j) Total income second year after notification

Figure A.1 Outcomes by days of qualifying employment

Table A.1 Reduced form estimates of basic characteristics

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

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

Table A.2 Results using PES date as notification date

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

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

Table A.3 Main results using different bandwidths

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

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

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

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

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

Table A.5 Results by timing of program start

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

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

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

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

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

Table A.7 Results by notice size

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

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

Table A.8 Results by supplier size

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

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

Table A.9 Results by age

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

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