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Keeping the youth out of social assistance: The effect of a mandatory search period on receiving welfare benefits

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Abstract

I exploit the natural experiment caused by the introduction of a mandatory search period for young adults who want to apply for welfare benefits in the Netherlands in 2012. I estimate the causal effect of a mandatory search period on receiving welfare benefits and other socio-economic outcomes using the discontinuity at the 27 years threshold, to compare young adults before the age cutoff to young adults after, in that sense comparing young adults who were eligible for the mandatory search period to young adults who were not. The results show a positive and robust effect of the policy on the employment of men in the short run. Unfortunately, I do not find significant or robust results for the other socioeconomic outcome variables. This may be since I perceive the receipt and not the application of welfare benefit and therefore observes the effect of being eligible for a mandatory search period and not the actual treatment. Furthermore, my results indicate that young adults who are low educated are the ones that benefit from the policy which goes against most of the literature.

Content

1 Introduction

Social assistance benefits are widely used in different countries to support individuals of working age who are not able to provide for themselves. In the Netherlands, social assistance benefit is also a part of the social security system. Under certain conditions, people between 18 years old and the pension age are eligible to receive welfare benefits. In 2018, before the COVID-19 pandemic started, social expenditures amounted to 47.8 percent of government spending whereof 4 percent was taken by social assistance receipts[1]. This makes it the fourth largest social benefit source, after health insurance, retirement benefits, and long-term care. About 4.7 percent of the labor force receives these welfare benefits whereof 9% are younger than 27 and whereof about 55 percent receive it for 2 years or longer[2].

Long-term social assistance and the costs that come with it are problematic not only from a government finance perspective but also from the perspective of the individual. Moreover, every additional year of receiving social benefits causes a decrease in a person's human capital and impacts his or her probability of leaving the social safety net, additionally, the lack of structure and social contacts may lead to (mental) health problems. Furthermore, the implementation of a social safety net is often accompanied by moral hazard, i.e. people who could have been working are switching to receiving welfare benefits. Hence, social benefits are not efficiently distributed to the ones who need them. Activation policies are then focused on introducing efficiency gains in the social welfare system, trying to get people back to work sooner and help to reduce moral hazard. These policies can take different forms going from mandatory financial incentives, monitoring, and training to search periods, trying to tackle the negative side effects of the social safety net provided by welfare benefits. Financial incentives often have a short-term impact which disappears when the measure ends. Monitoring and training are such as a mandatory search period a stick approach to activation policy, whereof the former involves (more) direct public costs. A mandatory search period has no direct public costs and has at least a comparable impact on the probability of entering social assistance, this makes it an effective instrument. It is focused on the entry side of the social safety net rather than on the exit side like most of the other activation policies. In that way, it helps to avoid the welfare trap instead of getting out of it.

[1] CBS, Uitgaven voor sociale uitkeringen nemen verder toe, 2019
[2] CBS, Aantal mensen in de bijstand daalt verder, 2018, Ministry of social affairs and employment, Participatiewet factsheet, 2019

The population receiving welfare benefits can be subdivided into three categories: young adults between 18 years and 27 years, adults between 27 years and 45 years, and older adults between 45 years and the pension age. The first group consists of individuals that just entered the labor market and are at the beginning of their working careers. This is a special group because they are at the moment of transition to adulthood which is crucial for their further development and future. Therefore, activation policies are often focused on targeting this group. From 1 January 2012 on, young adults, younger than 27 years, in the Netherlands were required to search for a job or look for education opportunities for four weeks long before they could proceed with their application for social assistance benefits[3]. This policy had the aim to overcome moral hazard and reduce the number of young people receiving social assistance, especially those who have a small distance from the labor market.

In this paper, I will study the effects of being eligible for the mandatory search period on the probability of receiving social assistance benefits, based on the introduction in 2012 in the Netherlands. Using Dutch microdata containing the young adult population receiving or not receiving social assistance benefits, I take advantage of the natural experiment that the introduction of the intervention created. I will use the discontinuity at the 27 years threshold, to compare young adults before to young adults after the cutoff, in that sense comparing young adults who were eligible for the mandatory search period to young adults who were not. To reduce my sample by focusing on young adults who have a high propensity of receiving social welfare benefits, I estimate an individual's probability of receiving benefits based on the set of descriptive variables and implement a propensity threshold to select the individuals with a probability of receiving the treatment i.e. forced to participate in the mandatory search period.

First, I perform a regression discontinuity where I compare young adults who are just older than 27 years to young adults who are just younger than 27 years. To deal with issues of seasonality, and the optimality, and Hawthorne effect, I combine the regression discontinuity design (RD) with a difference-in-difference approach over the years 2010, 2011, and 2012 into a difference-in-discontinuities design (DiD). It is important to notice that 'phasing out of the treatment' cannot happen because people must also complete the 4-week search period if they turn 27 during that period.

I will use non-parametrical and parametrical estimations of two models, using local linear regression with a triangular kernel and a data-driven bandwidth selection procedure. Additionally, I will perform similar analyses with different outcome variables i.e., employment probability, the probability of starting a study,

[3] Even though you turn 27 during the search period, you have to finish the 4 weeks.

shift to other social benefits, and I will explore variation in the results depending on gender. Last, I will include several robustness tests to further support my results.

My RD results show the effectiveness of the policy. They indicate a decrease of about 2% in the probability of receiving welfare benefits for young people in the short term. However, this result is only marginally significant and does not remain in the difference-in-discontinuities setting. Moreover, both the regression discontinuity and difference-in-discontinuities results show an increase of about 6% in the probability of being employed for young men in the short run, this effect also remains in almost all robustness checks. Both my results also confirm a shift towards other social benefits for women in the long run and away from sickness or disability benefits, the policy may increase the probability of receiving other social benefits by about 1% and decrease the probability of receiving sickness or disability benefits with about 1% in general and 2% for women especially. Notwithstanding, these results do not remain in all robustness checks. Additionally, the DiD regression is re-estimated according to the highest obtained education level, these results imply an about 4% increase in the probability of being employed in the short and long run for individuals who are lower educated (primary education). My RD results approximately satisfy the main identification assumption of continuity of potential outcomes, only a few household covariates are unbalanced around the threshold, therefore they are controlled for in the regressions. Nonetheless, the difference-in-discontinuities results survive most of the assumption tests with the effect on short term male employment as the most robust.

This paper contributes to the literature on the effect of the mandatory search period on labor activation outcomes. It is novel in evaluating the policy introduction in 2012 in the Netherlands and in first estimating its effect for young adults on the probability of receiving social assistance benefits and other relevant socio-economic outcomes.

The remainder of the paper is structured as follows: section 2 provides an overview of the literature, section 3 describes the institutional background of social assistance benefits in the Netherlands and the introduction of the mandatory search period, section 4 sets out the identification strategy as well as the empirical specifications, section 5 presents the data and their main characteristics, section 6 reports the results and finally, I will conclude and discuss the paper in section 7 and 8.

2 Literature review

In developed countries, there is an increasing tendency for labor activation policies and programs enforcing mandatory activation so that unemployed people who refuse or fail to participate in those programs can lose their right to unemployment or social assistance benefits. Based on the argument of “activation” these programs are often focused on the young, trying to encourage them to maintain a link with the labor market and avoid them becoming unemployed due to periods of being out of work (Wildemeersch 2009). This link can be maintained through education or work experience. Moreover, prolonged unemployment can have damaging results on a person’s working life, due to the lack of training and employment experience (Apergis and Apergis 2020). As a result, the employment and earnings potentials of people can be affected for the rest of their lives. Especially for young people, these consequences of unemployment can be damaging. Even for society as a whole, these consequences can be severe as social exclusion can lead to drug abuse, crime, and social unrest (O’Higgings 2001).

For the group of young people between 18 and 27 years, the transition phase from youth to adulthood has a major impact on their future development. Young adults have a long future ahead of them and that is why it is even more important for them to stay active. Notwithstanding social assistance benefits are often short-lived for this group, social assistance is a solution of last resort and should be avoided as much as possible for these young adults. This temporal need for social benefits is often associated with the life phase of the individual and especially with the transition from parental home to independent living or from school to work (Carpentier et al., 2017b, Leisering & Leibfried, 1999). Starting in 1995, Lorentzen and Dahl (2021) followed the life course of young social assistance recipients, between the age of 18-24 years, in Norway. By following these young adults over 20 years, they discovered that education and work with medium wage are both determinants of the successfulness of a young person’s labor market trajectory. Therefore, it is expected that if the mandatory search period motivates young people to (re-)enter education, work, or achieve more experience, hence their labor market outcomes will improve. Second, early unemployment is negatively associated with their future earnings which support the need for early labor activation intervention targeted at the youth.

High and persistent levels of youth unemployment and economic dependence raise a concern about the negative consequences of extended spells of unemployment early in the career. Unemployment may directly be associated with psychological distress and financial hardship for the affected youth (Goldsmith et al. 1997). Mroz and Savage (2006) found that however young people try to catch up by seeking training after a period of unemployment, they often end up with long-term adverse impacts on earnings from unemployment experienced early in the employment life cycle, a so-called scarring effect. Early unemployment spells may have more negative effects on later-life outcomes than on wages, it can also induce lower labor market attachment, lower well-being, social exclusion, and a higher probability to engage in criminal activities. Furthermore, young employment and the lack of opportunities for young people to make their transition into adulthood may lead to long-term consequences for successive generations making them unable to achieve their potential. The rise in the number of young adults not in employment, education, or training has led to concerns about the impact on social cohesion and fears of a "lost generation" (Maguire 2013). Moreover, prolonged duration in social assistance may negatively affect the preferences and behavior of the recipients, creating a so-called "welfare trap" (Contini and Negri 2007). In other words, individuals develop social benefit dependence, which reduces their chances of being self-sufficient. For the public budget, youth unemployment means direct costs for social assistance, as well as indirect costs of foregone tax payments and social security contributions. Furthermore, rising employment levels are seen as the most effective strategy for preparing for population aging (European Commission 2008).

Therefore, countries are implementing activation policies to overcome these negative consequences for young adults and society as a whole. In this way, social assistance benefits are provided to the ones that really need them. In a meta-analytical review, Liu et al. (2014) summarized the theoretical perspectives and experimental evidence of the effectiveness of job search interventions. They found that the probability of obtaining a job was 2.67 times higher for unemployed participants in job search intervention compared to the ones that did not (control group). Programs that contained teaching job search skills, improving self-presentation, boosting self-efficacy, encouraging proactivity, promoting goal setting, and enlisting social support were the most effective in improving the labor market outcomes of unemployed individuals. According to their research, both skill development and motivation enhancements are needed to effectively promote employment. Especially for young adults, these job search interventions were successful. In Portugal, the introduction of the mandatory job search period had the aim to act early on youth unemployment and therefore prevent periods of long-term unemployment at the beginning of their career. Centeno et al. (2004) performed a difference-in-difference approach to investigate the effect of job search support programs in Portugal on the labor market outcomes for youth unemployment. They found a small negative effect of the policy on the unemployment duration of the treated group i.e., a reduction of less than 1 month. Moreover, the new deal in the UK - a policy program that included extensive job assistance, training, education, and wage subsidies to employers focused on young people claiming unemployment benefits for at least six months- increased the probability of being employed for young men with 5 until 7 percent (Blundell et al. 2004 and De Giorgi 2005). The results were estimated in Blundell et al (2004) according to a difference-in-difference approach exploiting area-based piloting and age-relating eligibility and in De Giorgi (2005) according to a regression discontinuity design. However, the long-term effect of the policy is still questionable. Borland (2014) mentioned that especially for the unemployed who are less disadvantaged, labor market programs consisting of job search and wage subsidy programs are doing a good job.

Different forms of activation policies are broadly discussed in the literature encompassing non-entry into or exit out of unemployment insurance or welfare benefits. It is important to notice that literature focused on the activation of the unemployed who receive UI benefits are focused on a different target group. It is not ex-ante evident that such effects are also found for welfare recipients, who are usually a long-term inactive group. Nevertheless, this is less true for young adults since the majority stay on welfare for only a short time (Factsheet 2011). Activation requirements possibly combined with monitoring and sanctioning have proven to reduce unemployment duration and increase job entry (Duncan 2020, Markussen and Roed 2016, Fredriksson 2003). Reemployment services have proven to reduce mean weeks of unemployment benefit receipt by about 2.2 weeks, the average amount by 143 dollars, and increased earnings by 1,050 dollars (Black et al. 2003). The program made recipients who were job ready and had little or no distance to the job market exit the social support. Therefore, the program was successful in reducing the moral hazard associated with unemployment benefits. However, reemployment services and training programs are expensive programs that have adverse effects because participation in these programs reduces search efforts to find regular jobs in the short run due to locking-in effects (Lalive et al. 2005). Therefore, it should also be taken into account that when interventions lead to increased hours spent in study and training the effects on the labor market will be lagged (Breunig et al. 2003). Additionally, the warning and the enforcement of benefit sanctions -temporary reductions in UI benefits due to noncompliance with eligibility requirements- have a positive effect on the exit rate of unemployment and unemployment duration (Lalive et al. 2005). Requiring social benefit recipients to actively search for work in exchange for benefits reduces the attractiveness of social benefits and may therefore also reduce dependency (Rector 1993). Although, warnings seem to reduce post-unemployment job quality and earnings since they force individuals to lower their reservations wages (Arni et al. 2013)

In addition to labor activation programs, a reduction of social benefit or the benefit duration can be introduced to improve the labor market outcomes of recipients of social benefits such as welfare or unemployment insurance e.g. reduction of the UI benefit replacement rate or duration. So is found that a reduction in maximum aid payment of 15 percent resulted in a difference in the employment rate of 1.9-4.6% between the treatment and control group (Hotz et al. 2002). Similarly, Carling et al. (2000) estimated a 10 percent increase in the transition rate due to a cut in the UI replacement rate from 80 percent to 75 percent. Furthermore, based on a benefit increment in Canada by 36% compared to Quebec, a labor force non-participation elasticity to the generosity of the benefits of 0.28-0.36% can be estimated (Gruber 2000). However, not all research confirms the positive effect of benefit cuts (Krueger 1992). When focussing on benefit duration reductions, positive effects on employment and unemployment duration can be found in literature, so is estimated for the US that a 1-month reduction of UI duration led to a reduction of UI receipt on average of 1.8 weeks and a reduction of unemployment duration of approximately 1.1 weeks (Johnston and Mas 2018). In the opposite direction, one week increase in UI benefit duration increases the average duration of unemployment spells of UI recipients by 0.16 to 0.20 weeks (Katz and Meyer 1990). For Switzerland, a reduction of UI benefit duration from 24 to 18 months resulted in an increase in employment of 5.9 percent point and an increase in earnings by 3.9% (Cottier et al. 2019).

It is important to notice that benefit cuts, benefit duration reductions, or some labor activation programs may induce a worsening in posterior job-match quality, e. g. in terms of reemployment wages (Domenech and Vannutelli 2019). These reforms may force individuals to lower their reservation wages to be able to get out of unemployment on time (Cottier et al. 2019). On the other hand, earlier unemployment exit motivates individuals to leave when their reservation wage is still high, i.e. their human capital is not yet deteriorated that much. In that way, rapid depreciation of employment opportunities can be avoided or reduced. The second issue that can arise is a congestion or displacement effect, the new entrants in the labor market can squeeze out the already existing participants and lower their job opportunities (Katz and Meyer 1990).

In 2012, the introduction of the compulsory search period in the Netherlands caused a decrease in the number of people continuing with their social assistance benefit applications. This resulted in 20% to 56% fewer people who continued their benefit application (Divosa 2014). The effect of this program was regressive, mainly keeping young adults who experienced the smallest distance from the labor market out of social assistance. According to Bolhaar et al. (2019), job search periods in the Netherlands substantially reduced social benefits payments, even in a permanent way. They performed a randomized experiment with a setup similar to an encouragement design. Their design exploited the random assignment of treatments to caseworkers and of applicants to caseworkers within each local social office. According to their results, the likelihood of receiving social benefits was significantly reduced by 20 percentage points up to six months after registration. This lowered the social benefit payments by about 25 percent. Moreover, the decline in benefit receipt for individuals was fully compensated by the increase in their labor income due to higher reemployment rates. For these reasons, the authors advocate for job search periods as an effective instrument for targeting benefits to social applicants. Furthermore, the mandatory job search period does not involve direct financial investments. However, the impact it achieves is at least comparable to costly activation programs. (Card and Hyslop 2005, Van der Klaauw and Ours 2013, Markussen and Roed 2016 and Black et al. 2003).

On the contrary, evidence shows that voluntary programs are more effective than compulsory ones. Young people in a compulsory program are often less motivated than those in a voluntary program (O'Higgings 2001). However, this goes against Dahlberg et al. (2009) who showed that mandatory activation of welfare recipients reduces overall welfare participation and increases employment with the largest effects on young people and people born in non-western countries. They find an effect of 0.4 percentage point reduction in the probability of receiving social welfare benefits.

In compulsory programs, participation may provide the incentive for some individuals to search for work or look for an education opportunity and to avoid other programs such as training or to be able to receive social benefits later on. In this case, policy acts more as a stick than a carrot and the effectiveness of the stick approach can be questionable (O'higgings 2001, Andrigetto and Villatoro 2011). Tuomala (2011) found that for Finland that the activation reform - a mandatory activation period including training and implemented in 2006 – did not affect the probability of finding a job or leaving social support. Furthermore, leaving social assistance does not immediately mean leaving welfare support, it can also result in a shift from one benefit scheme to another, a so-called spillover effect. However, Bolhaar et al. (2019) did not find evidence for this spillover effect, it is important to keep this in mind while setting up similar research.

Moreover, social assistance recipients differ from other unemployed youth in their social backgrounds e.g., health issues, drug use, labor productivity and capacity, dropout of school rates, etc. (Hammer 2007). This makes it a special group of young adults needing additional attention and public support. According to Backman and Berman (2011) the following characteristics; male sex, being single, ethnic minority status, low educational achievement, substance abuse, low employability, and poor physical and mental health are found as predictors of low social assistance exit rates. This means that social assistance benefits are progressive which supports the social welfare system in a country and reduces the moral hazard. By introducing a mandatory search period the effectiveness of the social assistance benefit can be further improved. The intervention will keep young people with a small distance from the labor market out of the social assistance, while keep on supporting people with a larger distance to the labor market, so distributing to the ones that need the support. Moreover, if municipalities impose a search term on people over 27, they generally do that for a select group (Divosa 2014). Only applicants on welfare with a short distance to the labor market are given a search period. There are exceptions for 'dire cases', people who are not self-, and people who are in a financial emergency. Bolhaar et al. (2014) show that it can have added value to differentiate according to age and education level. A search period appears to be more effective for people under 40 and higher educated (all ages). For this group, social benefits decreased by 86 percent (Bolhaar et al. 2019). Furthermore, the search period had hardly any effect for lower-educated people over 40. Unfortunately, imposing a search term harmed the income of people applying for social assistance.

In recent literature, the evidence of youth labor activation programs, especially of the mandatory search period, seems to be limited and suggests that the programs play different roles in different countries. Therefore, a conclusion cannot easily be transferred from one country to another (Dietrich 2012). By investigating the literature, it became clear that it still lacks information on the effectiveness of the introduction of the mandatory search period specifically in lowering the number of young people with social assistance benefits in the Netherlands. I will extend the literature by investigating its effectiveness.

3 Institutional background

3.1 Social assistance in the Netherlands

When someone does not have the sufficient means to provide for subsistence, the government comes in and provides a social assistance benefit or other welfare benefits (Van Koperen 2017). In the Netherlands, the level of benefits and entitlements are regulated at the federal level, however, since 2015, the responsibilities of the implementation are at the municipality level due to the change in the Participation

Act[4]. Social assistance benefit is not financed by contributions such as the unemployment law in social insurance, but it is financed from general resources (the Netherlands) and is, therefore, a social provision[5].

In the Netherlands you are entitled to social assistance if (i) you live in the Netherlands, (ii) you are older than 18 years, (iii) you do not have enough income or assets to support yourself, (iv) you cannot rely on any other provision or benefit with which you can provide for your livelihood and (v) you are not in jail or in a remand[6]. At this moment, couples aged 21 years up to retirement age together receive a net amount of 1,481.60 euros per month (about 100 percent of the minimum wage), including holiday allowance. Singles and single parents receive 1,037.12 euros (about 70 percent of the minimum wage), with single parents also receiving an extra child budget[7]. There is no maximum time period that an individual or household can receive social assistance benefits.

Welfare recipients are mainly represented by young people, singles, and people with a migration background.[8] Women also account for slightly more than half of the share. There are five primary groups from which people can get into social assistance[9]. The first group are people who lost their job and who are not or not anymore entitled to unemployment benefits. The second group are people whose life situation changes, for example, because someone divorces or the breadwinner dies. The third group are people who finished their education. After graduation, student grants will disappear, and not every student can easily find a job after graduation. The fourth group are people who exit detention, for these people it can be very difficult to find a job so they often end up applying for welfare benefits. The last group are people with a migration background who do not speak the languages well or have unrecognized degrees from their home countries.

End of March 2021, the Netherlands counted 433 thousand people under the pension age with a general social assistance benefit. This is about 3.87 percent of the working age population and almost 12 thousand more than a year earlier. For the fourth consecutive quarter, the number of social assistance recipients has grown compared to last year. Moreover, the increase was relatively strongest among young people in the first quarter of 2021. In general, there were more social assistance recipients in each age group at the end of March 2021 than a year earlier, but the increase was the largest among young people up to the age

[4] Rijksoverheid, Waar kan ik mijn algemene bijstand aanvragen? Consulted on 29-04-2022
[5] CBS, Sociale zekerheid, Consulted on 29-04-2022
[6] Rijksoverheid, Wanneer heb ik recht op algemene bijstand? Consulted on 29-04-2022
[7] Sociaal verhaal, Hoogte bijstandsuitkering 2022, consulted on 02-05-2022
[8] CBS, Personen met bijstand; persoonskenmerken, consulted on 28-06-2022
[9] It's public projectervaring, Bijstand in Nederland, consulted on 28-06-2022

of 27. The number of people entitled to social assistance in this age group was more than 3 thousand higher than last year, this corresponds to 9 percent growth. The relative difference compared to the previous year was lower among the 27- to 45-year-olds and the over-45s, compared to the young people. Statistics Netherlands (CBS) reports these findings based on new figures (CBS 2021). The larger effect on the young can be explained by the sector they are mainly active in and the COVID-19 pandemic. Young people often work in sectors such as the event sector, hospitality services, bars, restaurants, etc. and these were the sectors that were most affected by the COVID-19 pandemic.

People receiving social assistance benefits are also subject to re-integration obligations focused on participating in finding a job as fast as possible or acquiring the necessary skills[10]. Therefore, social assistance recipients are obliged to accept and try to retain the jobs that were offered to them by their municipality. This is different than the mandatory search period because during those four weeks they are free to search for and accept whatever suits them best.

3.2 The mandatory search period under 27

For the group of young people between 18 and 27 years, the transition phase from youth to adulthood has a major impact on their future development. Young adults have a long future ahead of them and that is why it is even more important for them to stay active. Social assistance is a solution of last resort and should be avoided as much as possible for this age group. Early unemployment is negatively associated with their future earnings which further supports the need for early labor activation intervention targeted at the youth. Therefore, a mandatory search period is used to motivate young people to (re-)enter education, to work, or achieve more experience, hence their labor market outcomes will improve.

On 1 January 2012, the law Investing in Young People and the Work and Social Assistance Act joined and made it mandatory for young people under the 27- who wanted to apply for social assistance benefits- to search for a job or look for an education opportunity for 4 weeks before they could enter social assistance. This applies to singles, single parents, and married couples who are both younger than 27 years old. If they were not able to find a suitable job or an education opportunity after these four weeks have passed, they will still receive benefits with retrospective effect from the moment of initial application. In other words,

[10] Rijksoverheid, Wat zijn mijn rechten en plichten in de bijstand? Consulted on 03-05-2022

the first benefit payments are delayed, so that the amount of benefit wherefore an individual is entitled to is not reduced. it is the responsibility of the municipalities to impose the conditions during the search period, going from a number of compulsory applications, registrations at employment agencies, and jobsite uploads of their CV. After those 4 weeks, applicants must submit the supporting documents to their municipality if they want to proceed with their application for welfare benefits. Then, the municipality decides whether it is sufficient evidence of having actively been searching.

Moreover, this measure is effective in limiting entry into social assistance since people are more likely to find work during this period (Bolhaar et al. 2019). Job search periods delay the first benefit payments and motivate applicants to start actively searching for jobs or demotivates people to apply for the benefits, i.e. the 'threat effect'. As mentioned above, this intervention was introduced to help as many people as possible to work and to provide assistance only to the people who are not able to provide for themselves. In this way, mandatory job search periods reduce moral hazard problems in the social benefits system.

According to Bolhaar et al. (2019), mandatory search periods can impact labor market outcomes in various ways. First, a mandatory search period makes the application process for social assistance benefits more complex and time consuming and therefore increases the costs of applying. This can retain people from starting the application process for social assistance benefits. Second, the mandatory search period can increase the probability of finding a job and thus reduce the social assistance benefits receipt. The increase in job finding can reduce social assistance payments toward people with relatively good labor market prospects. On the other hand, applicants with a lower job potential who cannot deal with the increased complexity of the application process can be discouraged to apply for what they need and are entitled to.

Moreover, the job search requirement and the waiting period are easily transferable to other situations, which suggests that job search periods can be useful policy instruments for unemployment insurance and disability insurance. The administrative costs of imposing a job search period are small, it acts as an early intervention that prevents more costly interventions later during the period of benefits dependency.

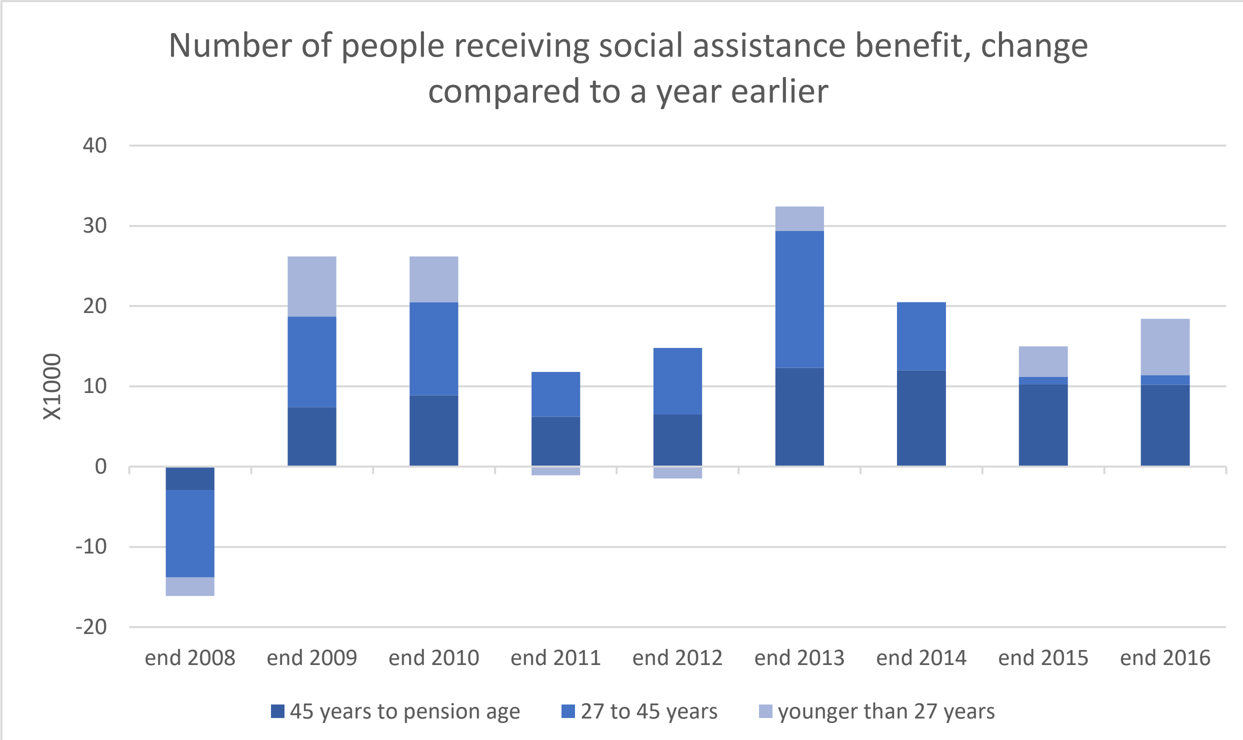

Figure 1: Individuals receiving social assistance benefits, change compared to the previous year (CBS 2017)
Note: for each age cohort the change in the number of people receiving social assistance benefit is calculated compared to the year before.

Figure 1 depicts the change in people receiving social assistance benefits compared to the previous year divided into three different age categories. It seems that the change is more cyclical for young people than for older people. In the year of the policy introduction i.e., 2012, we see a decline of 1.5 thousand young adults (younger than 27) receiving social assistance benefits compared to 2011, whereas participation among the older age groups increased. However, a decline was already present in 2011 (-1.1 thousand young people compared to 2010). This decrease in the number of young social assistance recipients was not in line with the developments in the labor market, where unemployment rose and the number of vacancies fell which may indicate that the policy worked. The proposed tightening of the work and assistance law before 1 January 2012 could made it possible that municipalities were already anticipating this amendment to the law by already introducing it before the official implementation date. However, it was until the official introduction on 1 January 2012 that it became compulsory for all young adults who wanted to apply for social assistance benefits. The years after the introduction of the policy, we see again an increase in the number of young adults receiving social assistance benefits. The increase in 2013 and its maintained level in 2014, can be explained by the increased flexibility in the labor market introduced by new forms of contract types between employer and employee, this resulted in more people being hired for a limited period or for a fixed number of hours (CBS 2015, Chkalova et al. 2015).

In 2015, the number of young people up to the age of 27 on social assistance increased by 4 thousand. This increase is partly due to the inflow of young people who are partially incapacitated for work, an estimated 2 to 3 thousand young adults (CBS 2016). Until 2015, in many cases, they could appeal to the Work and Employment Support for Young Persons with Disabilities Act. With the introduction of the Participation Act in 2015, admission to the Wajong has been limited. Only young people who are permanently and completely incapacitated for work are still eligible for benefits. Young people who are partially incapacitated for work and who have little or no income or no assets can apply for social assistance from the municipality. Due to these institutional and policy changes, data is observed from 2010 until 2013. Including more recent years would have captured confounding factors induced by new policies that were introduced.

In 2015, Zuurbier and Ruitenberg mentioned that research showed that approximately 4 out of 10 young people do not return after the search period, and therefore do not proceed with the application for social assistance. The question then is if the introduction of the new law can explain the further decline or not.

4 Identification strategy

This section discusses the two empirical strategies applied to investigate the causal effect of the mandatory search period on the probability of receiving social assistance benefits for young adults. These strategies have the aim to disentangle the causal effect of the intervention from other confounding factors. A simple OLS regression could regress receiving social assistance benefit on a dummy indicating the obligation of a search period, taking into account if someone is younger than 27 from 1 January 2012. Although, this approach would lead to biased results due to comparing young adults (younger than 27) with older adults up to the pension age. Here, selection bias may arise due to differences in the characteristics of individuals far away from the cut-off. These individuals can be different in education level, work experience, income, wealth, etc. In this case, the control group i.e., the older adults, would not be similar or comparable to the treatment group i.e., the younger adults. Henceforth, causal effects cannot be revealed. Therefore, I propose to use a sharp regression discontinuity design and a difference-in-discontinuities design as suitable econometric tools in the empirical strategy.

Instead of estimating the effect for the large group of young people, I would like to focus more on the young adults who have a probability of treatment, namely applying for welfare benefits. Therefore, to define my sample of young adults, I select individuals based on their probability of receiving welfare benefits. This probability is calculated based on a set of descriptive variables comprising gender, highest obtained education, household information, previous socioeconomic status, and annual wage. A probit regression is used to estimate the different coefficients of the explanatory variables from a sample of young adults between 20 and 30 years old in 2010 and 2011. Table A1.1 reports the marginal coefficients of the probit regression[11]. Then, the coefficients are used to predict the propensity of receiving social welfare for every individual in the main sample. Table A1.2 represents the average probability as well as the probabilities for other percentiles. Due to the low probabilities in the sample in general, the threshold is set on 12.5% to result in a more balanced and sufficient sample and to be able to make gender comparisons. Later on in this paper, different thresholds are used to check for the robustness of the results.

4.1 Regression Discontinuity design

In my research, I will utilize the feature of the implementation of the new law as a natural experiment in which individuals' age in days around the threshold is as good as random and therefore also the obligation of a search period.

I will introduce a sharp RD design and focus on all young people who are around 27 years old, estimating the local average treatment effect (LATE). For them, I will investigate the effect of the introduction of the mandatory search period in 2012. To capture the effect, I will use a regression discontinuity design wherein young people who are slightly younger than 27 are eligible for the treatment of the mandatory search period[12]. The control group exists out of people who are 27 or older. The RD design allows for causal

[11] A probit model used for a binary dependent variable, assumes that the probability of a positive outcome is determined by the standard normal cumulative distribution function and forces the outcome variable to be between 0 and1. (Greene 2008)

[12] Note that I will estimate the intention-to-treat (ITT) since I do not observe which young adults were imposed with the new law, so that the probability below the cutoff is lower than 1 and the probability above the cutoff higher

inference since the control group and treatment group assume to be alike. People who just turned 27 are not significantly different than people who are just a little younger.

I will estimate the local average treatment effect of the search period on young peoples' socio-economic status nonparametrically, using local linear regression with a triangular kernel including bias-corrected confidence intervals[13]. The major benefit of using this RD method is that it provides estimates based on data closer to the cut-off. This reduces the bias that could result from using data farther away from the cutoff to estimate the discontinuity at the cutoff. As using high-order polynomials can lead to misleading results (Gelman and Imbens 2014), I will not use them in my design.

The **basic RD model[14]** is defined as:

$$Y_i = \beta_0 + \beta_1 f(AGE)_i + D_i(\beta_2 + \beta_3 f(AGE_i)) + \upsilon_i \tag{1}$$

The model is based on a similar setting, of an age-related running variable in DI, as described in Dahl and Gielen (2021). As the dependent variable, Y_i, I will use the dummy variable indicating if the individual receives a social assistance benefit, is employed, follows an education, receives sickness , or receives other social benefits. Taking into account the 4-week search period and a maximum of two months waiting before receiving welfare benefits[15], I measure the outcomes in April of the same year and in January a year later. As explanatory variables, I will use a running variable that measures the age difference in months from being 27 and is allowed to have different functional forms, a dummy D_i indicating whether the individual falls under the obligation of the search period and the interaction term between age and the obligation dummy. It is not necessary to include covariates in an RD design, as it is a fully randomized experiment. However, it is advisable to include them to reduce variability in the estimates (Lee and Lemieux 2010). It will be useful to incorporate pre-assignment covariates that might be correlated with the post-assignment outcome e.g., education level, wealth, gender, socioeconomic status, etc.

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than 0 because some municipalities decided to voluntarily incorporate the intervention also for people who are older than 27. However, the probability below the cutoff is sufficiently higher than the probability above the cutoff. I will restore the reduced form (RF) instead of the IV since the law only provides one instrument, therefore it is not possible to obtain the average treatment effect.

[13] Calonico et al. (2014)

[14] Imbens and Lemieux (2008)

[15] Rijksoverheid, Waar kan ik mijn algemene bijstand aanvragen?

β_2 is the coefficient of interest and will capture the effect of the newly introduced law. The regression function can differ on each side of the cutoff, allowed by the coefficient of the interaction between the obligation dummy and age i.e. β_3 (Lee and Lemieux 2010). To obtain the optimal bandwidth, I will use the cross-validation procedure and consider the trade-off between bias and variance. This will provide me with the data-driven optimal bandwidth, see section 6.

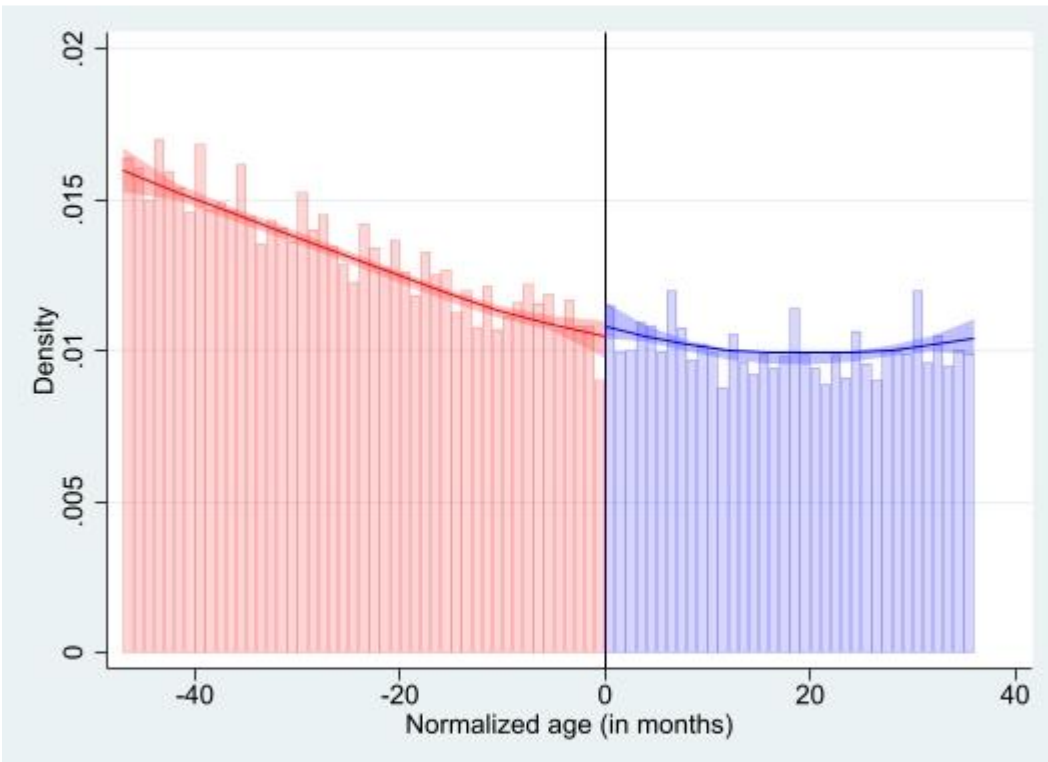

Figure 2: Density test, 2012-2013 cohort
Note: Density test at the 27 years old threshold following Calonico et al. (2014). The null hypothesis of continuity around the threshold cannot be rejected, with a p-value of 0.54.

The main assumption for this design is that individuals are unable to manipulate the assignment variable wherefore potential outcomes are continuous around the cutoff. In my setting, the assignment variable is the individual's age at the time the law was introduced and this cannot be controlled by the individual which means that the assignment to treatment at the threshold is as good as random. Individuals were not able to modify their age at the moment the law was introduced. Figure 1 shows no evidence of manipulation, as the null hypothesis of continuity around the 27 years old cutoff cannot be rejected, with a p-value of 0.54. Nonetheless, the jump between 26 and 11 months and the 27 seems suspicious. This means that in my selected sample there is a dip in young adults turning 27 in one month and a peak in young adults being 27 and this could be further enhanced by possible bunching behavior. Therefore, I will include a donut regression in my robustness checks to investigate the influence of this potential discontinuity, this involves the sample excluding the young adults turning 27 in one month and the ones being 27 for almost 1 month.

For potential outcomes to be continuous around the cutoff, all factors determining a person's socio-economic status other than being eligible to a mandatory search period must evolve smoothly around the cutoff. So that age the only discontinuity is determining eligibility for treatment. Visual evidence of continuity of predetermined covariates around the cutoff is shown in Figure A3.

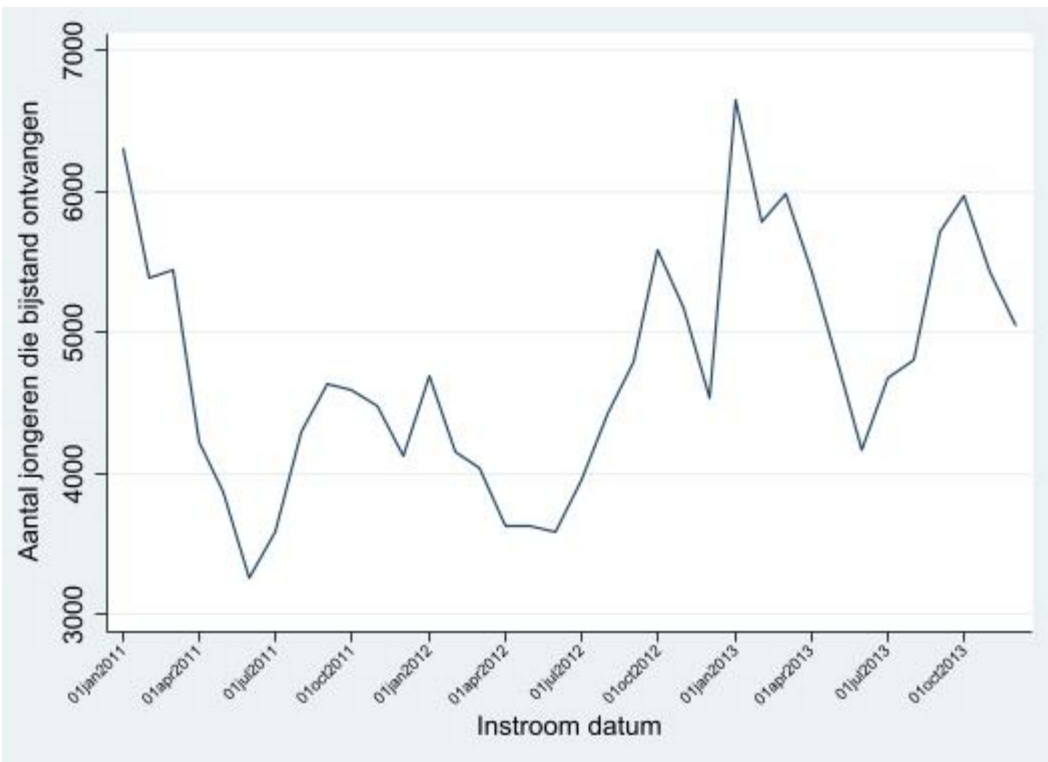

Figure 3: Number of young adults entering social assistance per intake date
Note: the dataset is focused on young adults between 20 and 30 from January 2011 until October 2013.

All covariates are continuous around the cutoff only having primary and secondary education as the highest obtained education are marginally discontinuous and will be further taken into account. Intuitively most of the civil status variables are related to age and decrease or increase when becoming older, hence these dummies will also be taken into account. Strikingly, I also perceive an age trend in the gender variable. It can be that with age women's probability to receive welfare benefits increases as their probability to stop working due to getting children increases. Accordingly, I will also analyze the results separately by gender. In its last two columns, table A3.1 shows the p-values of simple OLS regressions and RD regressions, regressing the treatment dummy on the different covariates. It is important to notice that household type is continuous around the cutoff, however on average people under or above the cutoff may have different household types. Therefore, the household dummies together with the primary and secondary education dummies are used as covariates in the estimation regressions. Additionally, table A3.2 reports the coefficient of linear regressing the propensity of receiving welfare benefits on the treatment dummy, to check if it is balanced over the treatment and control group. The coefficient is insignificant, meaning that the probability of receiving welfare benefits is balanced over the two samples.

Additionally, it is assumed that no other policy interventions change sharply at the same cutoff, so I can estimate the separate effect. Testing the second assumption is much harder than testing the first one since it is left to me to investigate if such confounding policies existed (Garcia 2020). I did not find any other change that year for young adults (younger than 27 years) entering social assistance.

Some limitations can rise concerning the RD design. First, the law was announced in December 2011 and was firstly mentioned in September 2011. This could mean that, from September 2011 on, young adults had the incentive to anticipate and quickly apply before the new policy was introduced. If this reaction was present, there would be a spike in the number of young adults applying for social assistance benefits just before 1 January 2012. Figure 3 shows the number of young adults entering social assistance per intake date. We can observe a certain seasonality in the number of entrants, high peaks in the winter months, and lower numbers during summer. If anticipation was the case we would expect a peak during the months of October 2011 until February 2012. We see a small increase between December 2011 and January 2012, which may reflect an anticipation action however, based on the seasonality present this conclusion cannot easily be made. Therefore, results should be interpreted with caution and as lower bound estimations. On the other hand, from 1 January 2012 young adults almost turning 27 at the moment could have the incentive to wait a couple of months with their application for social assistance benefits until they turn 27 years old and are not eligible anymore. Since, I do not have any information on the application itself, checking this implication will be hard and add to the limitation of this paper. However, a first indication can be given by investigating the proportion of entries per age for the different years. Figures A2.1-4 show that from 2012 on there is a dip in entries just before the 27 age cutoff, for 2012 this is at the age of 25 and for 2013 this is at the age of 26. Due to the use of bandwidth close around the cutoff in both designs (RD and DiD), this is not an issue for the 2012 estimates. Only for the 2013 cohort, this can cast doubts.

Furthermore, for this design, the issue of aging out of treatment could be a concern. Young adults who are between 26 years and 11 months, and 27 years old at the moment of application may be able to leave the treatment early because they turn 27 before the search period ends. However, the ministry of social affairs and employment confirmed that young people are obliged to complete their search period even if they turn 27 years during this period. Additionally, the effect can be downward-biased because some municipalities decided to earlier introduce this intervention in 2011. However, this should not have that much of an impact since the municipalities would have introduced it at different moments in time.

Moreover, in a standard RD design, researchers can only observe one state of the individual i.e., above or below the threshold, and not how the individual would have acted in the opposite state (Garcia 2020). However, it can be the case that both the control group and treatment group shift due to the policy intervention. For example, the policy could affect some of the untreated in a similar way as the treated individuals i.e., motivating them to search for a job or an opportunity to start an education such as their younger environment does. The latter can also happen due to some municipalities that decided voluntarily to implement the mandatory search period also for adults older than 27 years. These movements can be caused by the optimality or the Hawthorne effect. An optimality effect takes place when individuals from both groups react to the policy intervention according to their optimality condition. Whereas a Hawthorne effect occurs when individuals from the control group try to mimic the expected behavior of the treated group. Both effects can be present and can lead to an underestimation of the true effect.

4.2 Difference-in-discontinuities

After regressing the basic RD model, I will extend the model in time to a **Difference-in Discontinuities model**[16], to tackle the issue of the seasonality in social assistance receipts. A difference-in-discontinuities design combines the difference-in-difference and the RD methods. To do so, I will compare the discontinuities from 2012 and 2013 (treatment years) with the discontinuities in the previous two years

[16] Grembi et al. (2012) and (2016)

without the mandatory search period in place (2010 and 2011). In that way, the differences between discontinuities before and after the introduction of the mandatory search period are analyzed.

The model can be written as follows:

$$SA_i = \beta_0 + \beta_1 f(AGE)_i + D_i\big(\beta_2 + \beta_3 f(AGE_i)\big)$$
$$+T_i\big[\gamma_0 + \gamma_1 f(AGE_i) + D_i\big(\gamma_2 + \gamma_3 f(AGE_{i)})\big)\big] + v_i \qquad (2)$$

Here, an additional time dummy will be used to indicate the treatment years 2012 and 2013. The coefficient of interest is γ_2, capturing the effect of the introduction of the mandatory search period in 2012 or 2013. The model will be estimated parametrically since it was not feasible to estimate the full dataset nonparametrically. I will use a local linear regression with robust standard errors and a bandwidth of a year.

The same assumptions that apply for the difference-in-difference and RD designs also apply for the difference-in-discontinuities approach plus a couple of specific difference-in-discontinuities assumptions. According to Grembi et al. (2016), two identifying assumptions need to hold: (i) All potential outcomes in all periods are continuous around the age of 27. (ii) The effect of confounding variables on individuals at the age of 27 in the case of no treatment is constant over time. (iii) The effect of the search period at the age of 27 does not depend on the confounding variables, they do not interact with each other. I will test these assumptions empirically in the robustness section.

With the designs mentioned above, I will be able to estimate the causal effect of the treatment - being eligible for a mandatory search period of 4 weeks - on the probability of receiving social assistance benefits and other socio-economic outcomes for young people at the age of 27.

5 Data and descriptive statistics

5.1 Data sources

For my analysis, I use 4 CBS microdata datasets, one on individual's personal characteristics, one on an individual's socio-economic category, one on an individual's household type and one on an individuals' highest obtained education. In the merged dataset, records with all information were available from 2010 until 2020. For my research, I focused on the years 2010, 2011, 2012, and 2013, with 2010 and 2011 as control years. Of all the adults in the Netherlands, I only keep the ones that were young adults in 2010, 2011, 2012, or 2013. So that, my final dataset contains the birth date in months of each young adult and if they are receiving social assistance benefits or not, if they are employed, studying, receiving sickness or disability benefit or other social benefits as well as their gender, household type, education level and wage if applicable. I have no information on a person's wealth however, I assume that it is not yet developed because I focus only on young adults. As the policy was introduced in January, the covariates covering education level and household type, and the running variable age are observed in January in the year that applies. The running variable is normalized as the difference from being 27 years old in months. Five outcome variables are observed: receiving welfare benefits, being employed, being a student, receiving sickness or disability benefits, and receiving other social benefits. The latter encompasses benefits for the Act for Young Disabled Persons, for the Act Income Provision for the Partially Disabled Unemployed, for the Decree on assistance for the self-employed, or other social benefits. The outcome variables are measured at two moments, in April after four weeks of mandatory search period and a maximum of two months waiting before applicants would receive welfare benefits, and in January the year after to capture a more sustained effect of the policy. The full sample contains 1,592,197 observations, of young people from the years 2010 until 2013.

The sample chosen for the analysis is selected based on not yet receiving welfare benefits in January the year that applies and further refined based on their estimated probability of receiving welfare benefits. Based on an individual's gender, household information, highest obtained education, previous socioeconomic status and wage a personal probability is calculated[17]. Furthermore, individuals lacking one of these predictive variables are deleted. After setting up a (low) probability threshold of 12.5%, I obtain a main selected sample of 224,283 young adults.

5.2 Descriptive statistics

Table 1 shows the descriptive statistics of the overall subsample and the control and treatment group separately for 2012, the other years can be found in the appendix. The table includes young adults

[17] Using a probit regression design

between 25 and 29 years old in January 2012, using a bandwidth of 24 months. The second column expresses the sample means of all the selected young adults, column 5 displays the subsample means for all the young adults above 27, and Column 7 does this for the ones below 27, i.e. the treated individuals.

Table 1: Descriptive statistics, data from 2012

	Overall		Control Age >27		Treatment Age <27	
2012	N	Mean	N	Mean	N	Mean
Gender (male = 1)	24,733	0.383	11,794	0.352	12,939	0.412
Age	24,733	26.45	11,794	27.44	12,939	25.55
Household information:						
Reference person	24,733	0.395	11,794	0.394	12,939	0.397
Child at home	24,733	0.185	11,794	0.144	12,939	0.223
Single	24,733	0.332	11,794	0.326	12,939	0.338
Relationship	24,733	0.417	11,794	0.472	12,939	0.366
Having children	24,733	0.488	11,794	0.546	12,939	0.436
Highest obtained education:						
Primary education	24,733	0.390	11,794	0.391	12,939	0.390
Secondary education	24,733	0.608	11,794	0.608	12,939	0.609
Higher education	24,733	0.0012	11,794	0.0014	12,939	0.0011
Socioeconomic status:						
Employed	24,733	0.198	11,794	0.183	12,939	0.211
Receiving unemployment benefit	24,733	0.0232	11,794	0.0252	12,939	0.0215
Receiving other social benefit	24,733	0.0171	11,794	0.0159	12,939	0.0182
Receiving sickness or disability benefit	24,733	0.0647	11,794	0.0794	12,939	0.0513
Student	24,733	0.0211	11,794	0.0182	12,939	0.0237
Self-employed	24,733	0.0048	11,794	0.0061	12,939	0.0036
Average wage 2010	24,733	1,197	11,794	1,172	12,939	1,221

Note: this table shows the descriptive statistics for the selected sample in 2012. Except from age and average wage 2010, all the variables are dummy variables.

On average, we see that the selected sample exists out of more than half of women due to their higher propensity of ending up receiving welfare benefits compared to men. Furthermore, most of the young adults are in a relationship and are living together with their partners, a smaller group lives alone and the smallest group still lives at home with their parents. About half of the young adults have children. In addition, more than half of the sample have secondary education as their highest obtained education. Secondary education includes vocational education, general secondary education, and university preparatory education.

Only a small fraction of the selected sample has obtained a bachelor's or master's from higher professional or university education. In this table, the income sources and the student dummy are observed together with the covariates in January 2012, so not in April 2012 or January 2013 such as for the outcome variables. In the selected sample, only about 20% is employed and 2% follow an education. Hence, there is still room for improvement and for the policy to affect the outcome variables in a positive way. To observe potential side effects, I will also observe the influence of the policy on receiving sickness or disability benefits, and receiving other social benefits. Receiving unemployment benefits and being self-employed are not further taken into account in the analysis as these outcome variables are less likely to occur for young people. The average monthly wage is calculated only for the individuals that are working and receiving income from it.

6 Results

This section presents the main results of my research, starting with the RD regressions using data from 2012 until 2013, followed by the difference-in-discontinuities estimates, using 2010 and 2011 as control years. Last, I will check the identification assumptions and test the robustness of my results.

6.1 Regression discontinuity design

Visually, Figures 2 and A6 show the regression discontinuity plots according to Calanico (2014) for the different outcome variables, using a linear fit. Significant jumps are observed in the main outcome variable receiving welfare benefits and in the outcome variable receiving other social benefits, implying that, overall, young adults who are eligible for the mandatory search period have a lower probability of ending up in social assistance however, this may result in a higher probability of ending up receiving other social benefits such as benefits for the Disability Benefits Act for Young Disabled Persons, benefits Act Income Provision for the Partially Disabled Unemployed, benefits Decree on assistance for the self-employed or other social benefits.

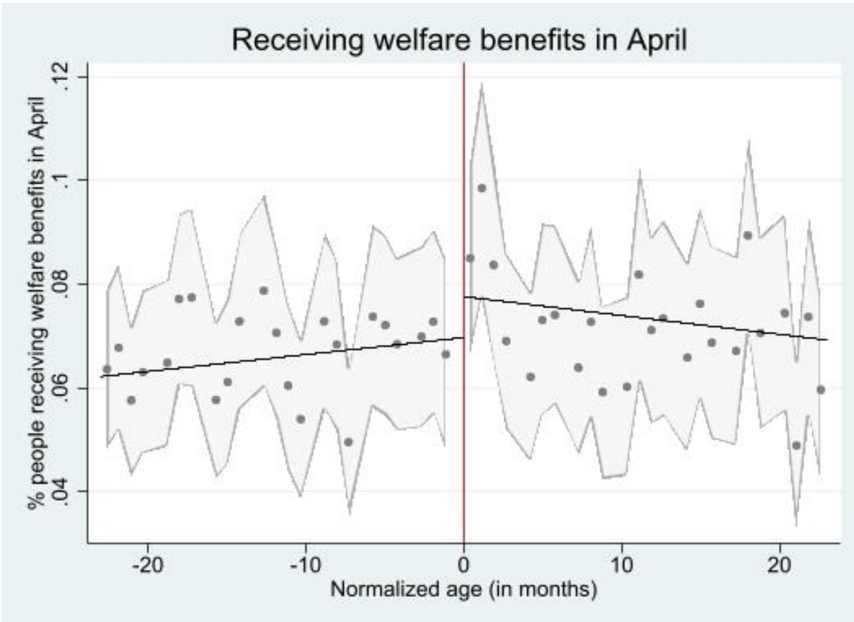

Figure 4: Regression discontinuity plot of outcome variable Receiving welfare benefits in April
Note: this figure shows the graphic results of the regression discontinuity design estimation following Equation (1) and Calonico (2014). The vertical line at the x-axis stands for the 27 years old cutoff. 95 % confidence intervals are depicted in light grey, while dark gray dots are within-bin sample percentages

Table 2 reports the RD bias-corrected coefficients for each outcome variable overall and per gender from separate regressions, as well as the standard errors, optimal bandwidth in months, and the number of effective observations used in each regression. It covers the treatment years 2012 and 2013, for the subsample selected based on their probability of receiving welfare benefits and the threshold of 12.5%. Table A7.1 only covers 2012. The regression table is organized as follows. Columns 1, 2, and 3 consider the outcome variables measured in April 2012 or 2013, in that way the mandatory search period of four weeks and a maximum of two months in between applying for and receiving welfare benefits is taken into account. Column 1 evaluates the whole selected sample of young adults, whereas Columns 2 and 3 are focusing separately on men and women. Columns 4, 5, and 6 examine the outcome variables measured in January a year later so that longer term effects can be investigated. This gives a person time to start a study and analyses the sustainability of started employment. Column 4 focuses on the selected sample in general, while columns 5 and 6 restrict the sample to men, respectively women.

Table 2: Regression Discontinuity design regression results

	(1)	(2)	(3)	(4)	(5)	(6)
	April			Year later		
2012-2013	Overall	Male	Female	Overall	Male	Female
Receiving welfare benefit	-0.0196*	-0.0119	0.0199	-0.0159	0.0208	-0.0268
Standard error	(0.0101)	(0.0162)	(0.0121)	(0.0142)	(0.0185)	(0.0177)
Bandwidth	8.360	9.686	8.395	8.923	14.554	8.506
Observations	71,330	29,899	41,431	71,330	29,899	41,431
Being employed	0.0072	0.0596***	-0.0203	0.0032	0.0295	-0.0143
Standard error	(0.0148)	(0.0202)	(0.0196)	(0.0138)	(0.0202)	(0.0172)
Bandwidth	10.793	14.572	10.018	10.912	12.639	11.431
Observations	71,330	29,899	41,431	71,330	29,899	41,431
Being a student	0.0041	-0.0057	0.0117	0.0026	-0.0046*	0.0082
Standard error	(0.0115)	(0.0148)	(0.0162)	(0.0059)	(0.0026)	(0.0096)
Bandwidth	9.409	14.162	7.930	8.977	11.551	8.775
Observations	71,330	29,899	41,431	71,330	29,899	41,431
Receiving sickness or disability benefit	-0.0076	-0.0061	-0.0082	-0.0141*	-0.0108	-0.0141
Standard error	(0.0080)	(0.0107)	(0.0108)	(0.0074)	(0.0101)	(0.0092)
Bandwidth	13.878	13.984	14.647	11.997	11.659	15.023
Observations	71,330	29,899	41,431	71,330	29,899	41,431
Receiving other social benefit	0.0114***	0.0036	0.0158**	0.0100***	-0.0010	0.0151***
Standard error	(0.0039)	(0.0032)	(0.0066)	(0.0035)	(0.0030)	(0.0053)
Bandwidth	11.835	13.220	10.210	13.260	9.479	14.573
Observations	71,330	29,899	41,431	71,330	29,899	41,431

Note: This table shows the regression discontinuity results of the 2012 and 2013 cohort as in Equation (1), using triangular kernels to give more weights to observations close to the threshold. Each coefficient comes from a different regression, only the coefficient of interest is reported. The following covariates are included in the regressions, living together, being a child at home, having children, having primary education as highest obtained education and having secondary education as highest obtained education. Optimal bandwidth chosen according to Calonico et al. (2014). Standard errors in parentheses (bias-corrected). *** p<0.01, ** p<0.05, * p<0.1

It is important to notice that when a coefficient has a positive sign, the probability of a certain outcome variable increases when someone is eligible for treatment, i.e. being younger than 27, the opposite applies to a negative sign. In other words, a positive coefficient means a positive effect of the treatment. First, the results in Table 2 indicate a negative effect of the mandatory search period on the probability of receiving welfare benefits, decreasing the probability by 2% of the individuals having a probability of receiving welfare benefits higher than 12.5%. The effect is significant at the 90 percent level. However, the longer term effect and most of the gender specific effects have the expected sign, they do not reach statistical significance. This can be due to the loss of power which is presented in the inflated standard error after splitting the sample according to sex. Second, regarding employment, we can observe a positive effect of the mandatory search period for men, increasing the probability of becoming employed by 6% of the selected individuals. However, the short term effect is highly significant at the 99 percent level, on the longer term statistical significance is no longer reached. In general, almost all the coefficients have the expected sign nevertheless, they do not all reach statistical significance. For women, we observe some coefficients with unexpected signs although they do not reach statistical significance. Third, almost all coefficients for becoming a student are not statistically significant, only for men in the longer term there is a negative effect which is significant at the 90 percent level. In the longer run, the probability of men becoming a student may decrease by 0.5% due to the mandatory search period. This sounds counterintuitive however, it might be explained by the increase in employment, meaning that the policy can involve a shift from study to employment. The latter can be started immediately while the former follows a certain academic calendar. Fourth, we can observe the negative effects of the mandatory search period on the probability of receiving sickness or disability benefits. This effect is significant at the 90 percent level in the long run, decreasing the probability by 1%. Here again, the split according to sex can have caused the loss of power for the male and female effect. Last, the most significant effect is observed in the probability of receiving other social benefits. The overall effects in the short and long term especially for women are significant at least at the 95 percent level. The probability of receiving other social benefits may increase in the short and long term with 1% in general and 2% for women.

6.2 Difference-in-discontinuities

To eliminate potential biases such as seasonality effect, Table 3 reports difference-in-discontinuities estimates according to Equation (2), using 2010 and 2011 as control years. Here, a local linear regression with a bandwidth of 12 months is used to estimate the effect of treatment. Table 3 is organized as mentioned above. Compared to Table 2, in general, the coefficients have lost in magnitude and significance. Moreover, we no longer find any significant effect of the search period on the probability of receiving welfare benefits. Only the effect on the probability of becoming employed in the short run remains, the mandatory search period may increase the probability of employment by 6%, at a 95 percent significance level. Furthermore, there is no longer an effect of the search period on the probability of becoming a student. Next to the effect on the probability of employment in the short run, the effect on the probability of receiving sickness or disability benefits, in the long run, persists even for women a significant effect can be found, decreasing the probability by 1%, respectively 2%. Both effects are significant at the 90 percent level. Finally, the effect of the mandatory search period on the probability of receiving other social benefits only remains significant in the long run for women. The probability may increase by 1%, at a significance level of 90 percent.

Table 3: Difference-in-discontinuities regression results

	(1)	(2)	(3)	(4)	(5)	(6)
2010-2013	April Overall	Male	Female	Year later Overall	Male	Female
Receiving welfare benefits	-0.0071	-0.0070	-0.0068	0.0077	0.0263	-0.00410
Standard error	(0.0078)	(0.0135)	(0.0095)	(0.0114)	(0.0190)	(0.0140)
Being employed	0.0062	0.0560**	-0.0180	-0.0036	0.0203	-0.0115
Standard error	(0.0146)	(0.0228)	(0.0190)	(0.0140)	(0.0215)	(0.0183)
Being a student	-0.0117	-0.0142	-0.0115	-0.0055	-0.00628	-0.00641
Standard error	(0.0098)	(0.0150)	(0.0129)	(0.0058)	(0.00576)	(0.00898)
Receiving sickness or disability benefits	-0.0085	-0.0013	-0.0111	-0.0129*	-0.00495	-0.0192*
Standard error	(0.0082)	(0.0155)	(0.0116)	(0.0072)	(0.00955)	(0.0103)
Receiving other social benefits	0.0053	-0.0099	0.0118	0.0088	-0.00439	0.0138*
Standard error	(0.0059)	(0.0079)	(0.0081)	(0.0057)	(0.00761)	(0.00799)
Observations	83,383	37,027	46,356	83,383	37,027	46,356

Note: This table shows the difference-in-discontinuities regression results according to Equation (2), using a local linear regression with a bandwidth of 12 months, and 2010 and 2011 as control years. Each coefficient comes from a different regression, only the coefficient of interest is reported. The following covariates are included in the regressions, living together, being a child at home, having children, having primary education as highest obtained education and having secondary education as highest obtained education. Standard errors (robust) in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Different bandwidths are used in tables A8 in the appendix. When using a bandwidth of a half year, the coefficient of becoming employed in April for men keeps the same significance level and becomes slightly higher in magnitude. Moreover, a bandwidth of two years results in more significant effects however, the causality of these effects due to the large bandwidth is questionable. Results for different probability thresholds are reported in tables A9 in the appendix. Using the full sample without setting a propensity threshold lowers the magnitude of the coefficients by about a factor of 10 and changes the significance levels of certain coefficients. Only the coefficient of men becoming employed in April remains statistically significant at the 95 percent level. For the whole sample, the long term effect on receiving sickness or disability benefits and other social benefits especially, for women do not hold anymore, while the long term effects of becoming employed, especially for men become statistically significant.

When the threshold is further increased, we observe small changes in the magnitude of the coefficient of becoming employed in April for men however, the coefficients remain significant at the 90 percent level until the 0.5 probability threshold. The effect on receiving sickness or disability benefits does not hold while increasing the probability threshold. Furthermore, the coefficients of other outcome variables pick up some significant effects while changing the probability threshold however, these effects are not robust. Tables A10 represent the results of using second and third order polynomials. While its use is discouraged (Gelman and Imbens, 2019), adding second and third order polynomials to the regression result in almost no significant effects, only for the third order polynomial does the coefficient of becoming employed in April for men reaches the 95 percent significance level. Tables A13.1&2 investigates the heterogeneity of the results according to an individual's highest obtained education. From both tables, we can say that the policy has a positive effect of about 4%, in the short and long run, on the employment of young adults who only finished primary education. This is counterintuitive to what we would expect. We would expect that the policy is the most beneficial for the individuals with the smallest distance to the labor market i.e. young adults with higher education or at least secondary education.

6.3 Robustness checks

In this subsection, I want to challenge the main results to 1) the validity of the difference-in-discontinuities assumptions, 2) the possibility that the results are due to random chance, 3) the choice of bandwidth and 4) the choice of probability threshold.

Table 4 represents evidence for the difference-in-discontinuities assumptions, using a difference-in-discontinuities specification to test the discontinuity in a set of covariates. All coefficients are insignificant for the full set of covariates. Therefore, the results prove that the covariates do not vary at the threshold over time.

Table 4: Difference-in-discontinuities continuity test

2010-2013	(1) Sex	(2) Single	(3) Relationship	(4) Having kids	(5) Living at the parents
Estimate	0.0191	0.0237	-0.0197	-0.0233	-0.0006
Standard error	(0.0169)	(0.0167)	(0.0166)	(0.0171)	(0.0126)
Observations	76,418	76,418	76,418	76,418	76,418

Note: This table shows the difference-in-discontinuities regression results according to Equation (2), using a local linear regression with a bandwidth of 12 months, and 2010 and 2011 as control years. Each coefficient comes from a different regression, only the coefficient of interest is reported. Standard errors (robust) in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A11.1 shows the estimates without taking into account the covariates. Leaving out the covariates only led to small changes in the significance and magnitude of the coefficients. The same applies to the results of the donut regression in table A12.1. Here, all adults who are 26 and 11 months or 27 are left out of the sample.

Table 5 reports the placebo difference-in-discontinuities using 2011 as the treatment year and 2010 as the control year. In that way, I check if there are no confounding factors influencing the discontinuity of the outcome variables from year to year. No results should be significantly different from zero. Unfortunately, the coefficient of receiving other social benefits a year later is marginally significant and almost double the estimate in the previous table. This lack of robustness can cast doubts over the findings, as it could be the result of natural variation in applicants for other social benefits from one year to another at both sides of the cutoff. However, this variation may also be caused by the earlier implementation of the law by some municipalities or the announcement of the policy which makes 2011 a noisy period and therefore maybe not be ideal as fake treatment year. As a second placebo test, table 6 reports the difference-in-discontinuities results of a regression using a fake age cutoff at 30 for the years 2010 until 2013. I do not observe any significant coefficients. All in all, based on the above mentioned additional regressions it can be said that the continuity assumptions hold for my difference-in-discontinuities design.

Table 5: Placebo difference-in-discontinuities regression results with fake treatment year

	(1)	(2)	(3)	(4)	(5)	(6)
	April			Year later		
2010-2011	Overall	Male	Female	Overall	Male	Female
Receiving welfare benefits	0.0193	0.0258	0.0162	0.0069	0.0179	0.0014
Standard error	(0.0177)	(0.0296)	(0.0219)	(0.0231)	(0.0379)	(0.0292)
Being employed	0.0301	0.0763	0.0039	-0.0314	0.0141	-0.0582
Standard error	(0.0309)	(0.0492)	(0.0394)	(0.0293)	(0.0455)	(0.0383)
Being a student	0.0154	0.0123	0.0200	0.0030	-0.00758	0.0097
Standard error	(0.0232)	(0.0360)	(0.0303)	(0.0112)	(0.0083)	(0.0178)
Receiving sickness benefits	0.0091	-0.0019	0.0170	-0.0071	-0.0030	-0.0099
Standard error	(0.0134)	(0.0179)	(0.0189)	(0.0106)	(0.0135)	(0.0153)
Receiving other social benefits	0.0104	0.0195	-0.0033	0.0160*	0.0212	0.0058
Standard error	(0.0095)	(0.0151)	(0.0119)	(0.0090)	(0.0145)	(0.0113)
Observations	62,758	28,739	34,019	62,758	28,739	34,019

Note: This table shows the placebo difference-in-discontinuities regression results according to Equation (2), using a local linear regression with a bandwidth of 12 months, 2010 and 2011 as fake treatment year. Each coefficient comes from a different regression, only the coefficient of interest is reported. The following covariates are included in the regressions, living together, being a child at home, having children, having primary education as highest obtained education and having secondary education as highest obtained education. Standard errors (robust) in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Furthermore, appendix tables A8-10 report the different outcome results when using different bandwidths, probability thresholds ,and polynomial orders. The regression results are re-estimated for bandwidths of a half and two years. Compared to the main results, the impact on being employed for men in April is robust to the change in bandwidth. The increase in the number of significant coefficients with the two years bandwidth may indicate the influence of confounding factors due to the larger bandwidth. When varying the probability threshold or the polynomial order, it is again the effect on being employed for men in April that remains. All in all, it can be said that the mandatory search period may improve the employment of young men in the short run and that this effect is robust.

Table 6: Placebo difference-in-discontinuities regression results with fake age cutoff

	(1)	(2)	(3)	(4)	(5)	(6)
2010-2013	April Overall	Male	Female	Year later Overall	Male	Female
Receiving welfare benefits	-0.00469	-0.00204	-0.00599	-0.00349	-0.00304	-0.00367
Standard error	(0.00646)	(0.0134)	(0.00687)	(0.0103)	(0.0195)	(0.0117)
Being employed	-0.00772	-0.0204	-0.00152	0.00394	-0.0166	0.0161
Standard error	(0.0144)	(0.0238)	(0.0182)	(0.0157)	(0.0259)	(0.0197)
Being a student	0.00811	0.0120	0.00453	0.00497	0.00483	0.00331
Standard error	(0.00640)	(0.0102)	(0.00818)	(0.00580)	(0.00879)	(0.00756)
Receiving sickness benefits	-0.0001	-0.00570	0.00192	0.00222	0.00792	-0.00273
Standard error	(0.00644)	(0.00960)	(0.00845)	(0.00794)	(0.0121)	(0.0103)
Receiving other social benefits	0.00579	-0.0116	0.0145	-0.00784	-0.0251	0.00152
Standard error	(0.00868)	(0.0182)	(0.00907)	(0.00970)	(0.0203)	(0.0101)
Observations	59,566	21,309	38,257	59,566	21,309	38,257

Note: This table shows the placebo difference-in-discontinuities regression results according to Equation (2), using a local linear regression with a bandwidth of 12 months, 2010 and 2011 as control years, and 30as the age threshold. Each coefficient comes from a different regression, only the coefficient of interest is reported. The following covariates are included in the regressions, living together, being a child at home, having children, having primary education as highest obtained education and having secondary education as highest obtained education. Standard errors (robust) in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

7 Discussion

The previous section made clear that the mandatory search period for young adults may improve the employment of young men in the short run, however, overall, it not necessarily improves the socio-economic status of all young adults. In this section, I will compare my findings with those in the empirical literature, point out the main limitations of my research, and discuss possibilities for future research.

Compared to Bolhaar (2019) and Dahlberg et al. (2009), I do not find a significant impact on receiving welfare benefits. For my research, I did not have application data at my disposal. Therefore, it was not possible to estimate the direct effect of the mandatory search period, but only the effect of being eligible for the search period. This may explain the difference in significance and magnitude between previous research and this study. Additionally, some municipalities have introduced the mandatory search period earlier or implemented it also for individuals above 27 years old. This could possibly lead to a downward bias of the results.

My results are in line with the impact of the new deal in the UK, where a labor activation program for UI benefit recipients increased the employment of men in the short run (Blundell et al. 2004 and De Giorgi 2005). However, this is in contrast with other literature that often finds a positive effect of labor activation policies on employment in general (Hotz et al. 2002, Carling et al. 2000, Cottier et al. 2019). Although the magnitude of the effects is in line with my study, I only find significant results for men in the short run. Furthermore, my results indicate that it are the lower educated young adults that benefit from the policy which goes against Bolhaar et al. (2019) who found that a search period appeared to be more effective for highly educated people.

Non-robust results in this study, suggest that young women are more likely to receive other social benefits in the long run after the introduction of the search period, which implies the presence of a spillover effect for young women. Nonetheless, this effect is not robust and casts doubt about the validity. Moreover, it can be the case that the congestion or displacement effect are more present for women so that the new entrants in the labor market can squeeze out the already existing participants and lower their job opportunities (Katz and Meyer 1990).

Due to the lack of application data, anticipation and bunching cannot be investigated. I cannot check if the announcement and the introduction of the policy involved any behavioral changes and reactions of young adults. It could be that young adults being 27 or turning 27 at the time of the announcement decided to anticipate their need for welfare benefit and quickly applied before the policy was introduced or it could be that due to the policy young adults decided to postpone their application for welfare benefits until they turn 27. If anticipation and bunching are present, results can be downward biased.

For future research, it would be interesting to add a new dataset with information on migration, crime, and detention data. To be able to make more heterogenous estimates and set up more targeted policies. Such as mentioned in the literature, applying the mandatory search period in a targeted way can help to improve the progressiveness and the efficiency of the welfare system. Additionally, it would be good to analyze if a mandatory search period would involve changes in the individual's reservation wage, resulting in lower wages and job quality.

8 Conclusion

This paper aimed to estimate the causal effect of the mandatory search period for all young adults under 27 who want to apply for welfare benefits introduced in the Netherlands in 2012 on receiving welfare benefits and individuals' socioeconomic status.

The way the policy was implemented with a random cutoff at the age of 27, provided a framework for causal inference. I estimated a regression discontinuity design model that compared young adults slightly younger than 27 with young adults slightly older than 27 in January of the years in question. To reduce my sample by focusing on young adults who have a high propensity of receiving social welfare benefits, I estimate an individual's probability of receiving based on the set of descriptive variables and implement a propensity threshold to select the individuals with a probability of receiving the treatment i.e. forced to participate in the mandatory search period. For the RD estimates, I use a local linear regression with a data-driven bandwidth selection procedure and triangular kernels. Since individuals before and after the cutoff can both react to the policy. To tackle the issue of seasonality, optimality, or Hawthorne effect , I combine the 2012 and 2013 treatment years with two control years, 2010 and 2011, into a difference-in-discontinuities model.

The results show a positive effect of the policy on the employment of men in the short run. Significant in the regression discontinuity design and difference-in-discontinuities specification, moreover, this result holds during the robustness tests and I do not find serious violations in the continuity assumptions for the difference-in-discontinuities framework. Unfortunately, I do not find significant or robust results for the other socioeconomic outcome variables which is not in line with previous literature. This may be due to the fact that I perceive the receipt and not the application of welfare benefit and therefore observes the effect of being eligible for a search period and not the actual treatment. Furthermore, my results indicate that young adults who are low educated are the ones that benefit from the policy which is surprising and goes against most of the literature.

Even though, the positive effect of the mandatory search period on the short term employment of young men stays clear. The availability of application data would improve the causal inference in future studies. Still, whether the search period had any effect on individual's wages, job quality, or criminal activities can be investigated in future research.

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Appendix

A.1 Probit regression: propensity of receiving welfare benefit

Table A1.1: Probit regression results, margins

	(1) 2011	(2) 2010
Empoyed	-0.0397***	-0.0490***
	(0.000681)	(0.000755)
Receiving UI benefits	-0.0278***	-0.0460***
	(0.00133)	(0.00127)
Receiving other social benefits	-0.0835***	0.0712***
	(0.00200)	(0.00103)
Receiving sickness or disability benefits	-0.0487***	-0.0422***
	(0.00170)	(0.00189)
Student	-0.0938***	-0.0669***
	(0.000823)	(0.000940)
Self-employed	-0.0877***	-0.0720***
	(0.00167)	(0.00189)
Gender	0.00162**	0.00310***
	(0.000735)	(0.000781)
Living at the parents	-0.0208***	-0.0263***
	(0.00143)	(0.00153)
Living alone	0.00519***	-0.00462***
	(0.00139)	(0.00149)
Living together with partner	-0.0574***	-0.0674***
	(0.00157)	(0.00168)
Having kids	0.0459***	0.0642***
	(0.00103)	(0.00112)
Bachelors' degree	-0.0751***	-0.0650***
	(0.00117)	(0.00127)
Masters' degree	-0.111***	-0.0830***
	(0.00178)	(0.00201)
Annual wage previous year	-6.28e-06***	
	(6.02e-08)	
Observations	382,941	319,475

Note: this table shows the marginal coefficients of a probit regression, regressing the probability of receiving welfare benefits on the set of explanatory variables for the years 2010 and 2011. Standard errors in parentheses (robust). *** p<0.01, ** p<0.05, * p<0.1.

Table A1.2: probabilities per percentile

Percentile	25	50	75	90	95	99	N
Probability	.0001495	.0327629	.0233897	.0953599	.1673808	.4273112	3,597,181

Note: this table shows the predicted probabilities per percentile of the whole sample of young adults between 20 and 30 years old in 2010 until 2013.

A.2 Proportion of welfare entries per age, for the years 2010 until 2013

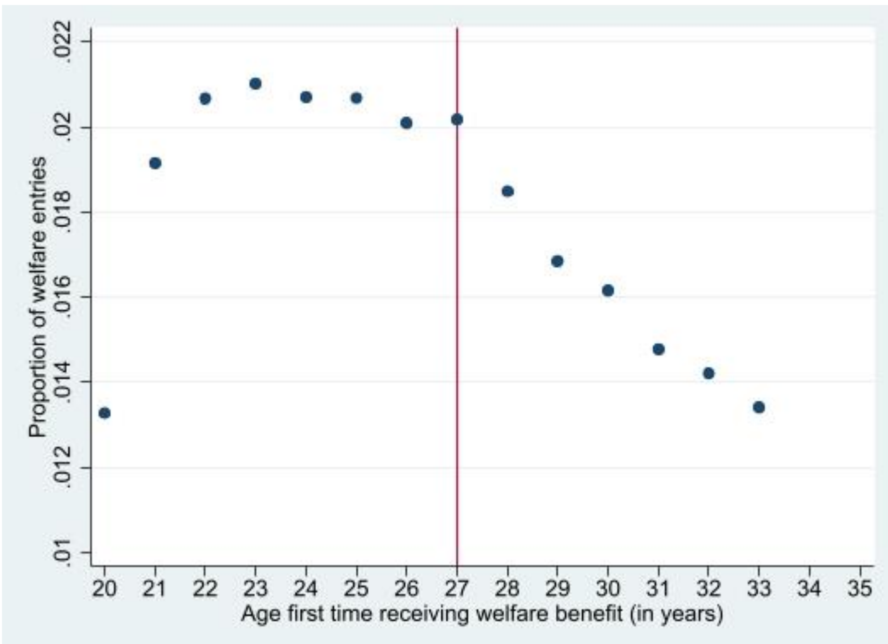

Figure A2.1: proportion of welfare entries per age in 2010

Note: this figure shows the proportion of young adults per age, age ranging from 20 to 33. Every blue dot represents the proportion of young adults for that age (in years) receiving welfare benefit.

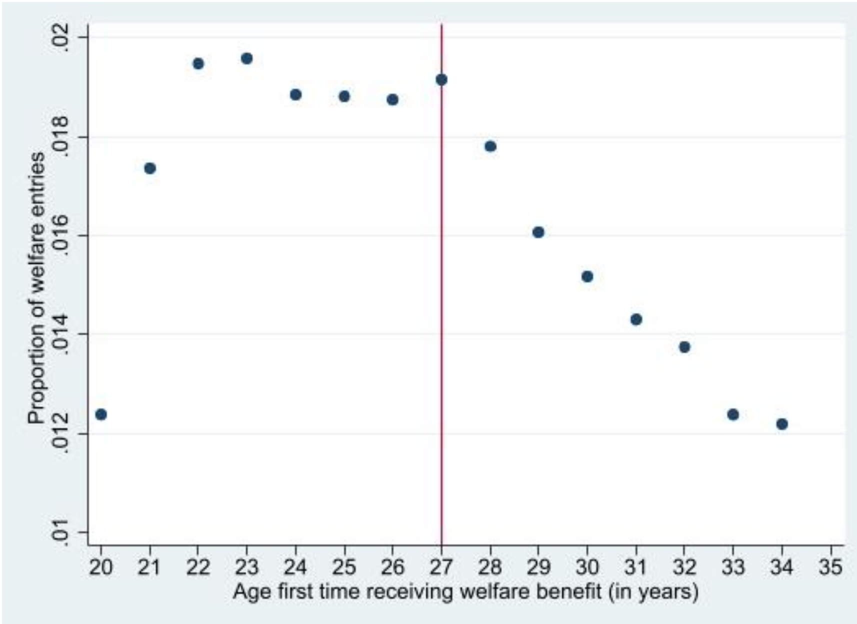

Figure A2.2: proportion of welfare entries per age in 2011
Note: this figure shows the proportion of young adults per age, age ranging from 20 to 33 .Every blue dot represents the proportion of young adults for that age (in years) receiving welfare benefit.

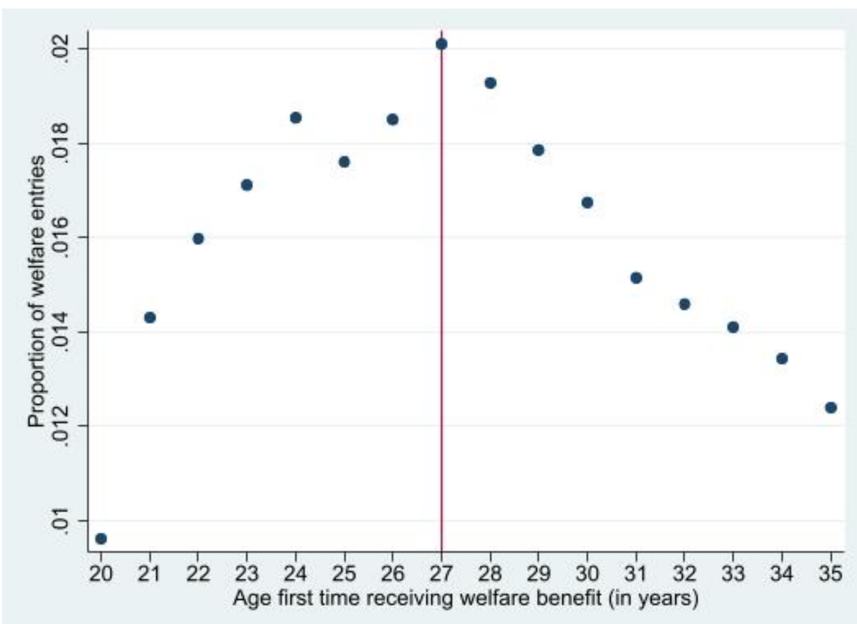

Figure A2.3: proportion of welfare entries per age in 2012
Note: this figure shows the proportion of young adults per age, age ranging from 20 to 33 .Every blue dot represents the proportion of young adults for that age (in years) receiving welfare benefit.

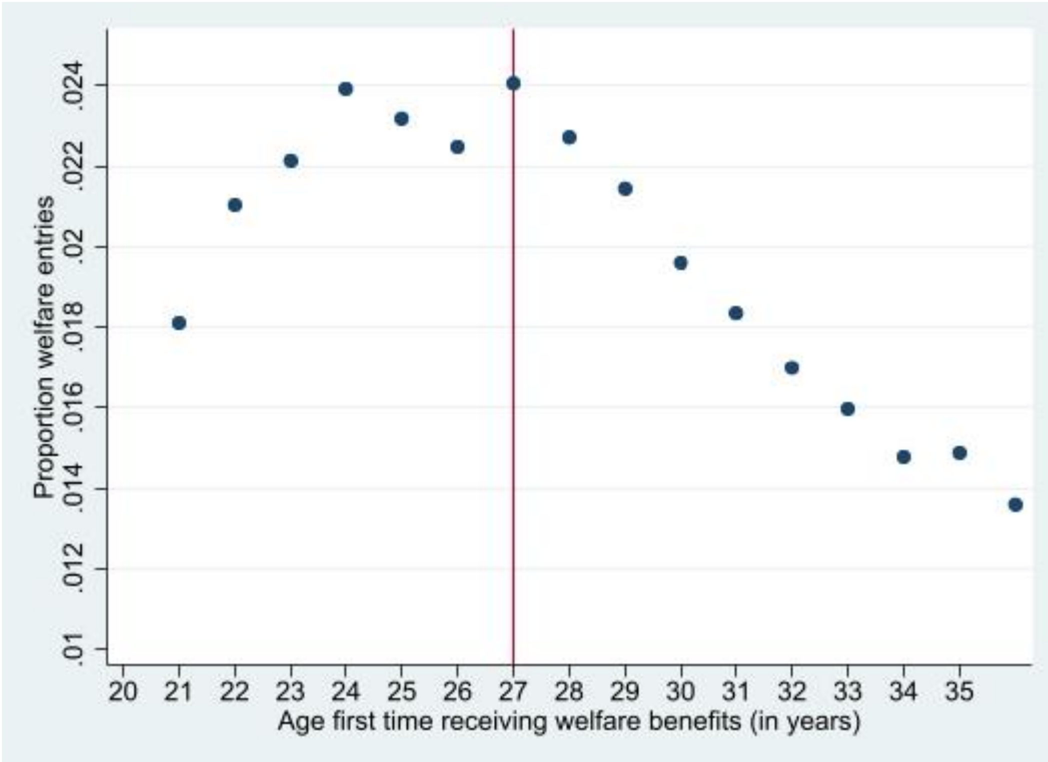

Figure A2.4: proportion of welfare entries per age in 2013

Note: this figure shows the proportion of young adults per age, age ranging from 20 to 33 .Every blue dot represents the proportion of young adults for that age (in years) receiving welfare benefit.

A.3 Descriptive statistics in the years 2010, 2011 and 2013

Table A3.1: Descriptive statistics in 2010

	Overall		Control Age >27		Treatment Age <27	
2010	N	Mean	N	Mean	N	Mean
Gender (male = 1)	76,348	0.464	28,718	0.460	47,630	0.466
Age	76,348	26.11	28,718	27	47,630	25.57
Household information:						
Reference person	76,348	0.627	28,718	0.624	47,630	0.628
Child at home	76,348	0.0655	28,718	0.0496	47,630	0.0751
Single	76,348	0.490	28,718	0.470	47,630	0.503
Relationship	76,348	0.378	28,718	0.422	47,630	0.351
Children	76,348	0.423	28,718	0.471	47,630	0.394
Highest obtained education:						
Primary education	76,348	0.274	28,718	0.284	47,630	0.268
Secondary education	76,348	0.718	28,718	0.706	47,630	0.725
Bachelor’s degree	76,348	0.0075	28,718	0.0084	47,630	0.0070
Master’s degree	76,348	0.0005	28,718	0.0007	47,630	0.0004
Socioeconomic status:						
Employed	76,348	0.722	28,718	0.733	47,630	0.715
Receiving welfare benefit	76,348	0.019	28,718	0.019	47,630	0.019
Receiving unemployment benefit	76,348	0.0275	28,718	0.0286	47,630	0.0268
Receiving other social benefit	76,348	0.0948	28,718	0.0833	47,630	0.102
Receiving sickness or disability benefit	76,348	0.0291	28,718	0.0309	47,630	0.0280
Student	76,348	0.0281	28,718	0.0228	47,630	0.0313
Self-employed	76,348	0.0040	28,718	0.0045	47,630	0.0038

Note: this table shows the descriptive statistics for the selected sample in 2010. Except from age, all the variables are dummy variables.

Table A3.2: Descriptive statistics in 2011

	Overall		Control Age >27		Treatment Age <27	
2011	N	Mean	N	Mean	N	Mean
Gender (male = 1)	8,022	0.414	3,662	0.386	4,360	0.437
Age	8,022	26.41	3,662	27.45	4,360	25.54
Household information:						
Reference person	8,022	0.533	3,662	0.529	4,360	0.536
Child at home	8,022	0.155	3,662	0.125	4,360	0.181
Single	8,022	0.474	3,662	0.473	4,360	0.475
Relationship	8,022	0.306	3,662	0.344	4,360	0.274
Children	8,022	0.458	3,662	0.518	4,360	0.408
Highest obtained education:						
Primary education	8,022	0.356	3,662	0.368	4,360	0.346
Secondary education	8,022	0.642	3,662	0.631	4,360	0.652
Socioeconomic status:						
Employed	8,022	0.318	3,662	0.321	4,360	0.315
Receiving welfare benefit	8,022	0.098	3,662	0.098	4,360	0.098
Receiving unemployment benefit	8,022	0.0355	3,662	0.0453	4,360	0.0273
Receiving sickness benefit	8,022	0.0261	3,662	0.0322	4,360	0.0209
Student	8,022	0.0236	3,662	0.0210	4,360	0.0257
Self-employed	8,022	0.0025	3,662	0.0030	4,360	0.0020

Note: this table shows the descriptive statistics for the selected sample in 2011. Except from age, all the variables are dummy variables.

Table A3.3: Descriptive statistics in 2013

	Overall		Control Age >27		Treatment Age <27	
2013	N	Mean	N	Mean	N	Mean
Gender (male = 1)	10,627	0.441	5,099	0.433	5,528	0.449
Age	10,627	26.46	5,099	27.43	5,528	25.56
Household information:						
Reference person	10,627	0.524	5,099	0.535	5,528	0.514
Child at home	10,627	0.197	5,099	0.160	5,528	0.231
Single	10,627	0.454	5,099	0.455	5,528	0.452
Relationship	10,627	0.285	5,099	0.325	5,528	0.249
Children	10,627	0.410	5,099	0.468	5,528	0.356
Highest obtained education:						
Primary education	10,627	0.339	5,099	0.357	5,528	0.322
Secondary education	10,627	0.660	5,099	0.641	5,528	0.677
Socioeconomic status:						
Employed	10,627	0.247	5,099	0.261	5,528	0.234
Receiving welfare benefit	10,627	0.098	5,099	0.103	5,528	0.093
Receiving unemployment benefit	10,627	0.0967	5,099	0.117	5,528	0.0783
Receiving other social benefit	10,627	0.0024	5,099	0.0026	5,528	0.0022
Receiving sickness or disability benefit	10,627	0.0305	5,099	0.0337	5,528	0.0275
Student	10,627	0.0222	5,099	0.0212	5,528	0.0232

Note: this table shows the descriptive statistics for the selected sample in 201. Except from age and , all the variables are dummy variables.

A.4 Balancing test covariates

Table A4.1: Balancing test results, covariates

2012-2013	Overall N	Overall Mean	Control Age >27 N	Control Age >27 Mean	Treatment Age <27 N	Treatment Age <27 Mean	Difference (p-values)	RD (p-values)
Gender (male = 1)	14,143	0.403	7,387	0.398	6,756	0.409	0.208	0.322
Household information:								
Reference person	14,143	0.437	7,387	0.441	6,756	0.433	0.317	0.084
Child at home	14,143	0.185	7,387	0.170	6,756	0.202	0.002	0.159
Single	14,143	0.371	7,387	0.376	6,756	0.366	0.238	0.050
Relationship	14,143	0.377	7,387	0.391	6,756	0.362	0.002	0.428
Children	14,143	0.466	7,387	0.483	6,756	0.448	0.004	0.334
Highest obtained education:								
Primary education	14,143	0.372	7,387	0.379	6,756	0.364	0.080	0.071
Secondary education	14,143	0.627	7,387	0.620	6,756	0.634	0.080	0.081
Bachelor's degree	14,143	-	7,387	-	6,756	-	0.867	0.509
Master's degree	14,143	-	7,387	-	6,756	-	0.512	0.820
Annual wage 2011	9,829	1,188	5,103	1,214	4,726	1,161	0.276	0.676
Annual wage 2012	4,314	2,831	2,284	2,834	2,030	2,828	0.944	0.601

Note: This table represents the descriptive statistics of the whole sample, and the control and treatment group. The differences between the both groups are estimated in two ways. First, each descriptive variable is regressed on the treatment dummy indicating the age cutoff. Second, a regression discontinuity is used to estimate the effect of the age cutoff on the descriptive variables. An bandwidth of 8 months is used. Standard errors in parentheses (robust). *** p<0.01, ** p<0.05, * p<0.1

Table A4.1: Balancing test results, probability

	Probability of applying for welfare benefit
Treatment (<27)	-0.0001
	(0.0004)
Constant	0.0346***
	(0.0003)
Observations	12,481
R-squared	0.000

Note: This table shows the regression results of an OLS regression, regressing the propensity of receiving welfare benefit on the treatment dummy with robust standard errors. *** p<0.01, ** p<0.05, * p<0.1

A.5 Regression discontinuity plots of the covariates

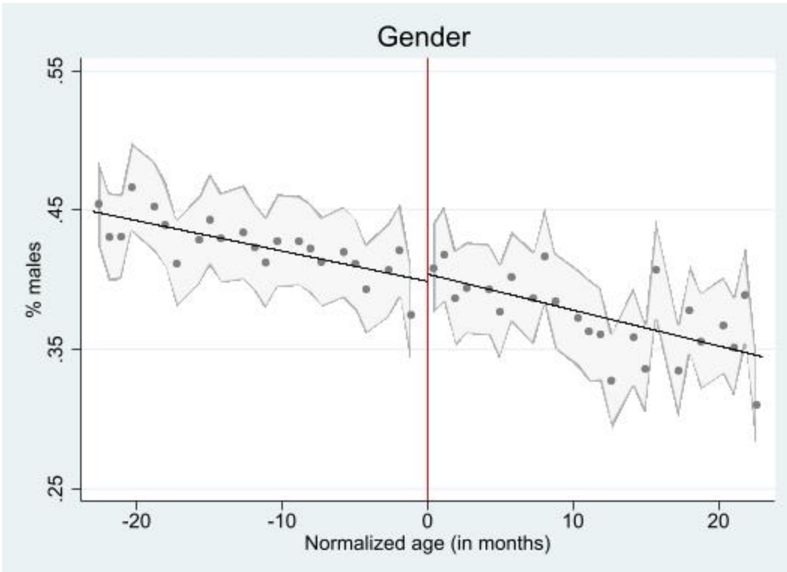

Figure A5.1: Regression discontinuity plot of covariate gender
Note: this figure shows the graphic results of the regression discontinuity design estimation following Calonico (2014). The running variable measures the distance in months from being 27 on 1 January in 2012. The vertical line at the x-axis stands for the 27 years old cutoff. 95 % confidence intervals are depicted in light grey, while dark gray dots are within-bin sample percentages.

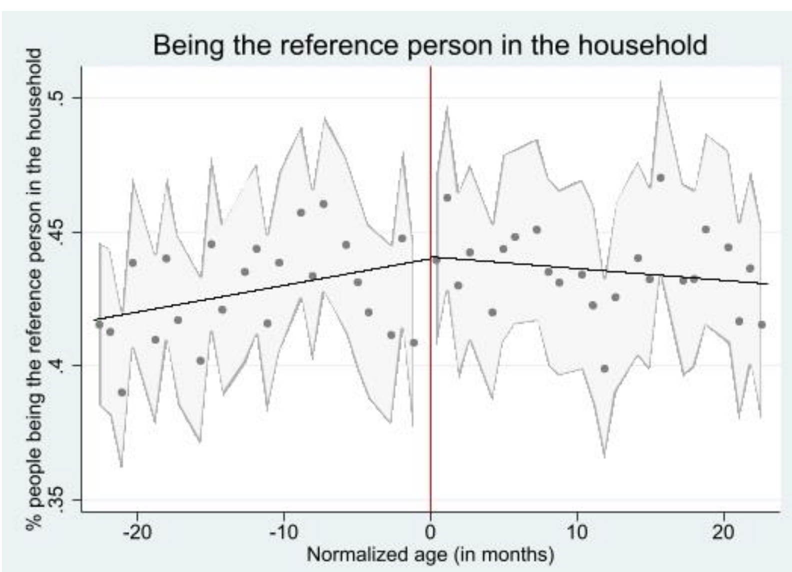

Figure A5.2: Regression discontinuity plot of covariate being the reference person in the household
Note: this figure shows the graphic results of the regression discontinuity design estimation following Calonico (2014). The running variable measures the distance in months from being 27 on 1 January in 2012. The vertical line at the x-axis stands for the 27 years old cutoff. 95 % confidence intervals are depicted in light grey, while dark gray dots are within-bin sample percentages.

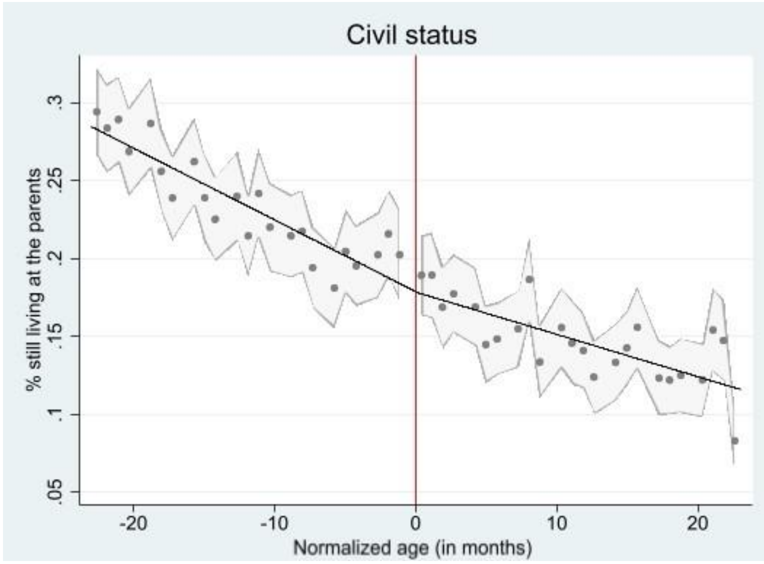

Figure A5.3: Regression discontinuity plot of covariate still living at the parents
Note: this figure shows the graphic results of the regression discontinuity design estimation following Calonico (2014). The running variable measures the distance in months from being 27 on 1 January in 2012. The vertical line at the x-axis stands for the 27 years old cutoff. 95 % confidence intervals are depicted in light grey, while dark gray dots are within-bin sample percentages.

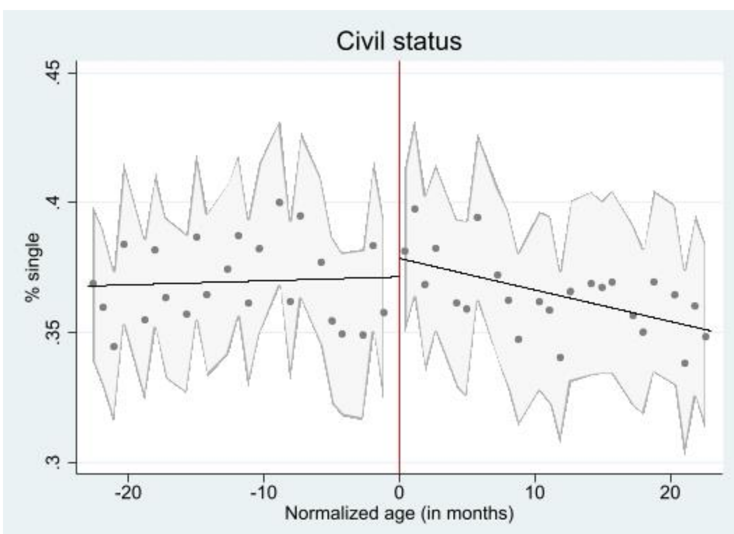

Figure A5.4: Regression discontinuity plot of covariate being single
Note: this figure shows the graphic results of the regression discontinuity design estimation following Calonico (2014). The running variable measures the distance in months from being 27 on 1 January in 2012. The vertical line at the x-axis stands for the 27 years old cutoff. 95 % confidence intervals are depicted in light grey, while dark gray dots are within-bin sample percentages.

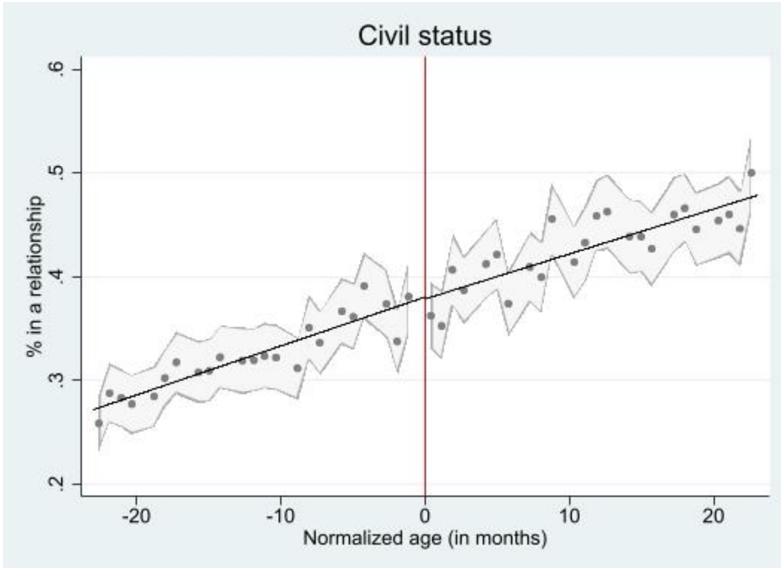

Figure A5.5: Regression discontinuity plot of covariate being in a relationship
Note: this figure shows the graphic results of the regression discontinuity design estimation following Calonico (2014). The running variable measures the distance in months from being 27 on 1 January in 2012. The vertical line at the x-axis stands for the 27 years old cutoff. 95 % confidence intervals are depicted in light grey, while dark gray dots are within-bin sample percentages.

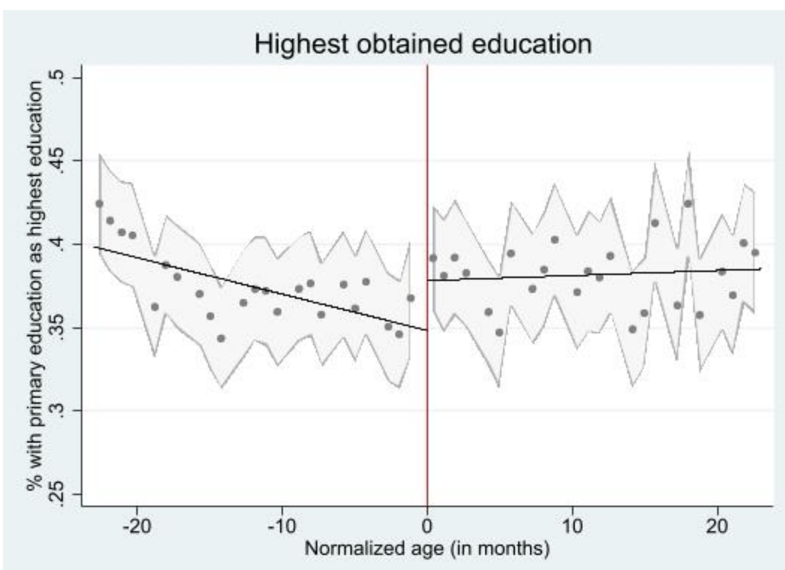

Figure A5.6: Regression discontinuity plot of covariate primary education as highest obtained education
Note: this figure shows the graphic results of the regression discontinuity design estimation following Calonico (2014). The running variable measures the distance in months from being 27 on 1 January in 2012. The vertical line at the x-axis stands for the 27 years old cutoff. 95 % confidence intervals are depicted in light grey, while dark gray dots are within-bin sample percentages.

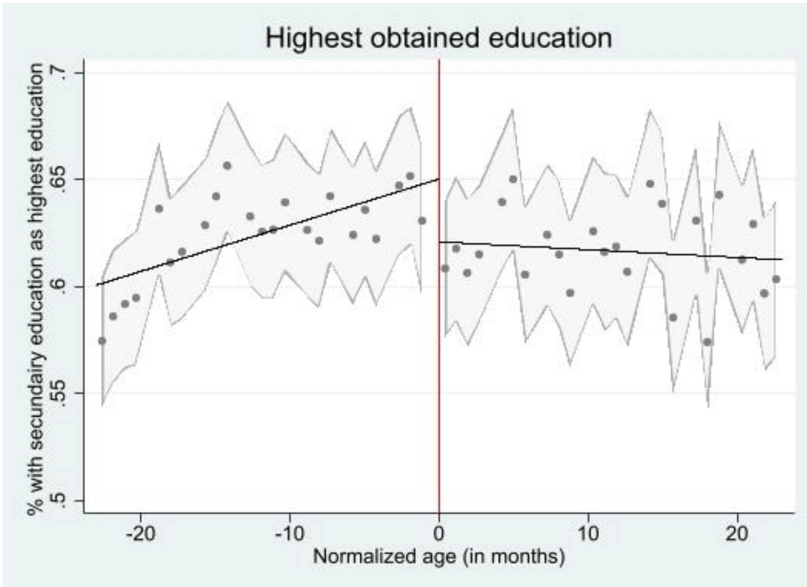

Figure A5.7: Regression discontinuity plot of covariate secondary education as highest obtained education

Note: this figure shows the graphic results of the regression discontinuity design estimation following Calonico (2014). The running variable measures the distance in months from being 27 on 1 January in 2012. The vertical line at the x-axis stands for the 27 years old cutoff. 95 % confidence intervals are depicted in light grey, while dark gray dots are within-bin sample percentages.

A.6 Regression discontinuity plots of the outcome variables

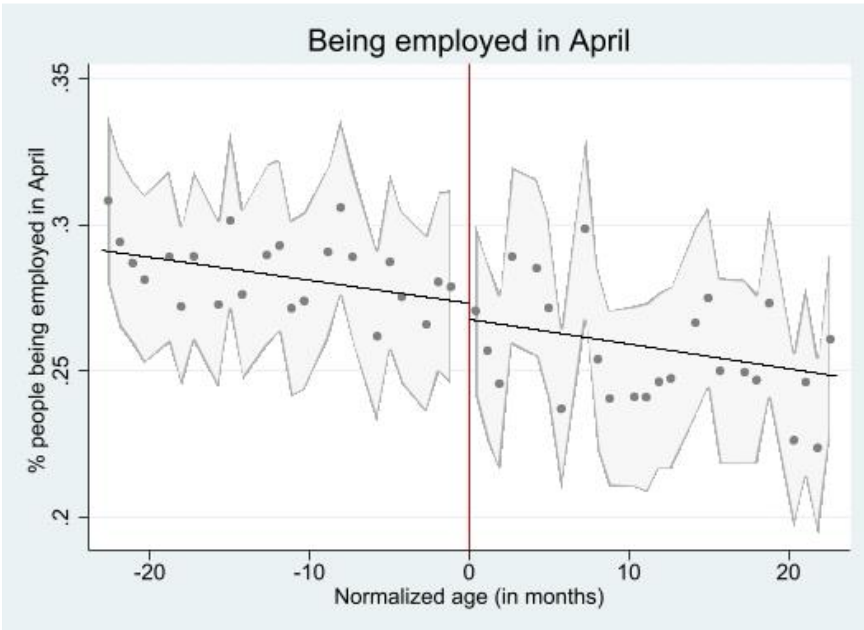

Figure A6.1: Regression discontinuity plot of outcome variable being employed in April
Note: this figure shows the graphic results of the regression discontinuity design estimation following Equation (1) and Calonico (2014). The vertical line at the x-axis stands for the 27 years old cutoff. 95 % confidence intervals are depicted in light grey, while dark gray dots are within-bin sample percentages.

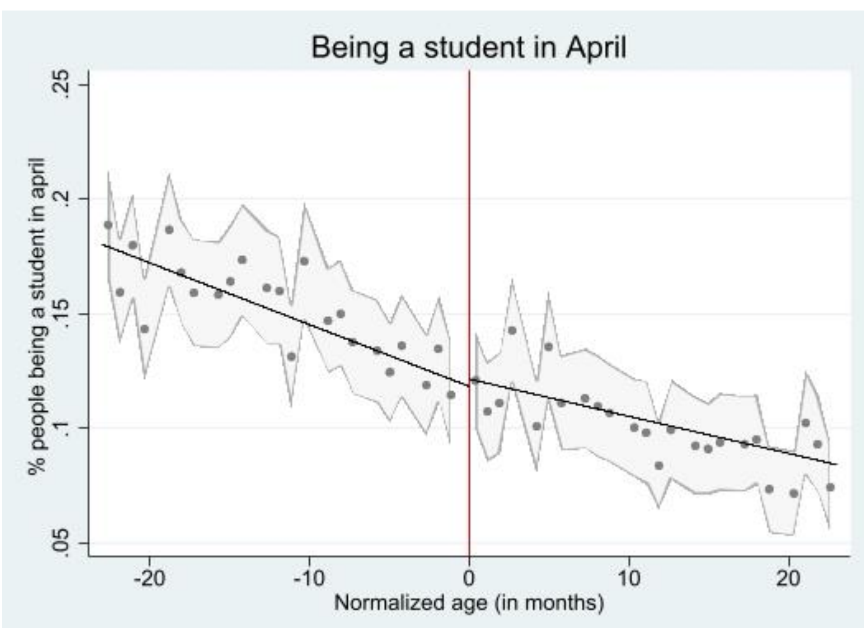

Figure A6.2: Regression discontinuity plot of outcome variable being a student in April
Note: this figure shows the graphic results of the regression discontinuity design estimation following Equation (1) and Calonico (2014). The vertical line at the x-axis stands for the 27 years old cutoff. 95 % confidence intervals are depicted in light grey, while dark gray dots are within-bin sample percentages.

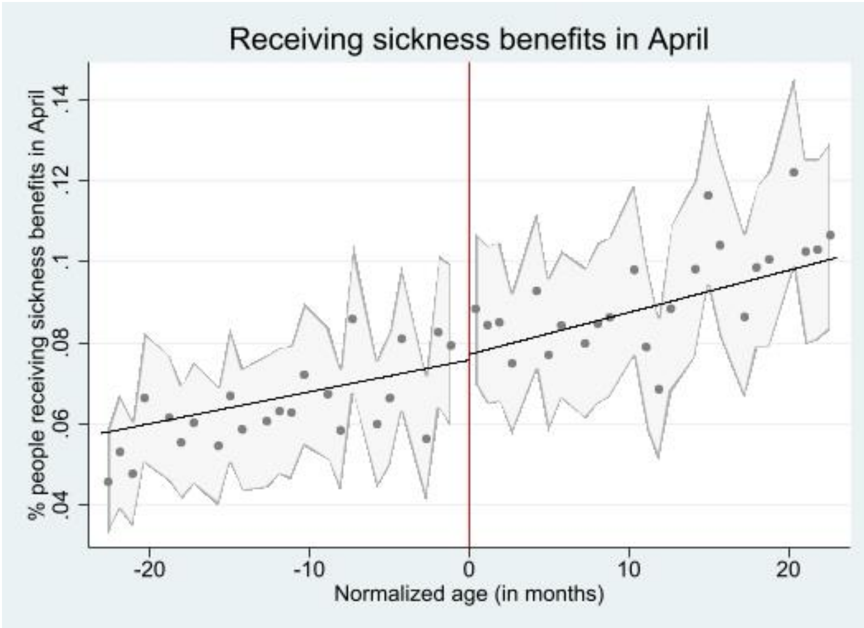

Figure A6.3: Regression discontinuity plot of outcome variable Receiving sickness benefits in April
Note: this figure shows the graphic results of the regression discontinuity design estimation following Equation (1) and Calonico (2014). The vertical line at the x-axis stands for the 27 years old cutoff. 95 % confidence intervals are depicted in light grey, while dark gray dots are within-bin sample percentages.

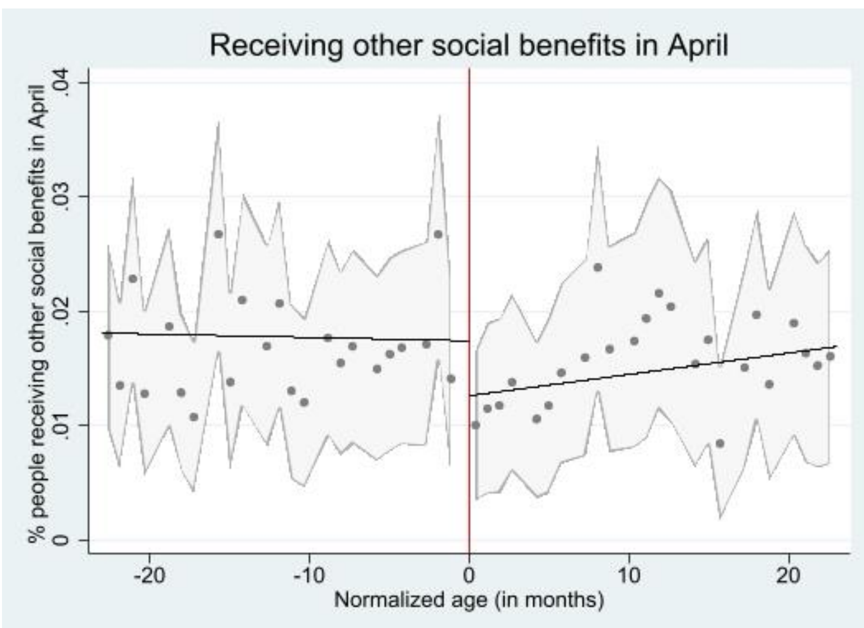

Figure A6.4: Regression discontinuity plot of outcome variable Receiving other social benefits in April
Note: this figure shows the graphic results of the regression discontinuity design estimation following Equation (1) and Calonico (2014). The vertical line at the x-axis stands for the 27 years old cutoff. 95 % confidence intervals are depicted in light grey, while dark gray dots are within-bin sample percentages.

A.7 Regression discontinuity only 2012

Table A7.1: Regression discontinuity design results, data from 2012 only

	(1)	(2)	(3)	(4)	(5)	(6)
	April			Year later		
2012	Overall	Male	Female	Overall	Male	Female
Receiving welfare benefits	-0.0284**	-0.0222	-0.0255**	-0.0037	0.0206	-0.0207
Standard error	(0.0112)	(0.0178)	(0.0123)	(0.0140)	(0.0201)	(0.0179)
Bandwidth	7.870	9.122	9.344	10.737	14.753	9.831
Observations	53,259	21,912	31,347	53,259	21,912	31,347
Being employed	0.0123	0.0875***	-0.0283	0.0125	0.0445*	-0.0046
Standard error	(0.0130)	(0.0282)	(0.0183)	(0.0138)	(0.0231)	(0.0165)
Bandwidth	16.566	10.443	12.970	14.551	14.460	15.647
Observations	53,259	21,912	31,347	53,259	21,912	31,347
Being a student	-0.0002	0.0073	-0.0006	0.0011	-0.0013	0.0021
Standard error	(0.0120)	(0.0198)	(0.0159)	(0.0058)	(0.0031)	(0.0091)
Bandwidth	12.590	13.366	10.751	13.421	15.843	13.413
Observations	53,259	21,912	31,347	53,259	21,912	31,347
Receiving sickness benefits	-0.0132	-0.0185	-0.0090	-0.0164*	-0.0152	-0.0122
Standard error	(0.0102)	(0.0144)	(0.0142)	(0.0097)	(0.0134)	(0.0114)
Bandwidth	13.067	12.530	12.819	11.432	11.598	15.286
Observations	53,259	21,912	31,347	53,259	21,912	31,347
Receiving other social benefits	0.0142***	4.23e-05	0.0218**	0.0109**	0.0014	0.0159**
Standard error	(0.0053)	(0.0039)	(0.0087)	(0.0046)	(0.0033)	(0.0073)
Bandwidth	11.835	13.854	10.745	13.654	13.167	13.356
Observations	53,259	21,912	31,347	53,259	21,912	31,347

Note: This table shows the regression discontinuity results of the 2012 and 2013 cohort as in Equation (1), using triangular kernels to give more weights to observations close to the threshold. Each coefficient comes from a different regression, only the coefficient of interest is reported. The following covariates are included in the regressions, living together, being a child at home, having children, having primary education as highest obtained education and having secondary education as highest obtained education. Optimal bandwidth chosen according to Calonico et al. (2014). Standard errors in parentheses (bias-corrected). *** p<0.01, ** p<0.05, * p<0.1

A.8 Difference-in-discontinuities with different bandwidths, parametric results

Table A8.1: Difference-in-discontinuities design, parametric results with bandwidth equal to two years

	(1)	(2)	(3)	(4)	(5)	(6)
	April			Year later		
2010-2013	Overall	Male	Female	Overall	Male	Female
Receiving welfare benefits	-0.0159***	-0.0269***	-0.0088	-0.0103	-0.0157	-0.0066
Standard error	(0.0056)	(0.0095)	(0.0067)	(0.0080)	(0.0135)	(0.0100)
Being employed	0.0396***	0.0564***	0.0274**	0.0536***	0.0707***	0.0412***
Standard error	(0.0105)	(0.0163)	(0.0138)	(0.0101)	(0.0155)	(0.0133)
Being a student	-0.0108	-0.0278**	0.0010	0.0026	0.0035	0.0011
Standard error	(0.0071)	(0.0108)	(0.0093)	(0.0042)	(0.0044)	(0.0064)
Receiving sickness benefits	-0.0056	-0.0087	-0.0030	-0.0022	0.0039	-0.0058
Standard error	(0.0058)	(0.0077)	(0.0083)	(0.0052)	(0.0068)	(0.0074)
Receiving other social benefits	0.0091**	0.0097*	0.0073	0.0146***	0.0150***	0.0124**
Standard error	(0.0042)	(0.0058)	(0.0058)	(0.0042)	(0.0057)	(0.0057)
Observations	135,416	59,820	75,596	135,416	59,820	75,596

Note: This table shows the difference-in-discontinuities regression results according to Equation (2), using a local linear regression with a bandwidth of 24 months, and 2010 and 2011 as control years. Each coefficient comes from a different regression, only the coefficient of interest is reported. The following covariates are included in the regressions, living together, being a child at home, having children, having primary education as highest obtained education and having secondary education as highest obtained education. Standard errors (robust) in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A8.2: Difference-in-discontinuities design, parametric results with bandwidth equal to a half year

	(1)	(2)	(3)	(4)	(5)	(6)
2010-2013	April Overall	Male	Female	Year later Overall	Male	Female
Receiving welfare benefits	-0.0153	-0.0272	-0.0061	-0.0123	-0.0106	-0.0109
Standard error	(0.0114)	(0.0195)	(0.0138)	(0.0164)	(0.0276)	(0.0203)
Being employed	0.0106	0.0748**	-0.0234	-0.0052	0.0455	-0.0334
Standard error	(0.0211)	(0.0333)	(0.0273)	(0.0201)	(0.0311)	(0.0263)
Being a student	5.80e-05	0.0006	0.0005	-0.0015	-0.0029	-0.0014
Standard error	(0.0140)	(0.0217)	(0.0184)	(0.0084)	(0.0080)	(0.0130)
Receiving sickness benefits	0.0024	-0.0013	0.0046	-0.0161	-0.0051	-0.0241
Standard error	(0.0118)	(0.0155)	(0.0169)	(0.0104)	(0.0135)	(0.0150)
Receiving other social benefits	0.0083	-0.0057	0.0164	0.0064	-0.0094	0.0150
Standard error	(0.0084)	(0.0111)	(0.0117)	(0.0082)	(0.0108)	(0.0115)
Observations	43,270	19,268	24,002	43,270	19,268	24,002

Note: This table shows the difference-in-discontinuities regression results according to Equation (2), using a local linear regression with a bandwidth of 6 months, and 2010 and 2011 as control years. Each coefficient comes from a different regression, only the coefficient of interest is reported. The following covariates are included in the regressions, living together, being a child at home, having children, having primary education as highest obtained education and having secondary education as highest obtained education. Standard errors (robust) in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

A.9 Difference-in-discontinuities for different probability threshold, parametric results

Table A9.1: Difference-in-discontinuities design with probability threshold equal to 0.25, parametric results

	(1)	(2)	(3)	(4)	(5)	(6)
2010-2013	April Overall	Male	Female	Year later Overall	Male	Female
Receiving welfare benefits	-0.00160	0.00504	-0.00488	0.00953	0.0329	-0.00527
Standard error	(0.0124)	(0.0203)	(0.0156)	(0.0173)	(0.0276)	(0.0221)
Being employed	-0.00158	0.0616*	-0.0433	-0.00236	0.0465	-0.0335
Standard error	(0.0225)	(0.0347)	(0.0294)	(0.0218)	(0.0333)	(0.0287)
Being a student	-0.0122	-0.0351	0.00287	-0.0119	-0.0346**	0.00289
Standard error	(0.0157)	(0.0245)	(0.0204)	(0.0105)	(0.0142)	(0.0147)
Receiving sickness benefits	-0.00352	-0.00413	-0.00104	-0.00995	-0.00604	-0.0112
Standard error	(0.0107)	(0.0141)	(0.0152)	(0.00892)	(0.0120)	(0.0126)
Receiving other social benefits	0.0146	0.0272	0.00935	0.0229	0.0246	0.0251
Standard error	(0.0186)	(0.0312)	(0.0223)	(0.0184)	(0.0310)	(0.0220)
Observations	29,116	12,050	17,066	29,116	12,050	17,066

Note: This table shows the difference-in-discontinuities regression results according to Equation (2), using a local linear regression with a bandwidth of 12 months, and 2010 and 2011 as control years. Each coefficient comes from a different regression, only the coefficient of interest is reported. The following covariates are included in the regressions, living together, being a child at home, having children, having primary education as highest obtained education and having secondary education as highest obtained education. Standard errors (robust) in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A9.2: Difference-in-discontinuities design with probability threshold equal to 0.5, parametric results

	(1)	(2)	(3)	(4)	(5)	(6)
2010-2013	April Overall	Male	Female	Year later Overall	Male	Female
Receiving welfare benefits	-0.0388	-0.0167	-0.0687	-0.0315	0.0486	-0.111*
Standard error	(0.0330)	(0.0453)	(0.0469)	(0.0422)	(0.0576)	(0.0603)
Being employed	-0.00434	0.0829*	-0.0832	0.0482	0.0861**	0.0224
Standard error	(0.0368)	(0.0475)	(0.0549)	(0.0335)	(0.0430)	(0.0504)
Being a student	-0.0426	-0.0564	-0.0314	-0.0367**	-0.0295	-0.0395
Standard error	(0.0355)	(0.0455)	(0.0527)	(0.0170)	(0.0213)	(0.0249)
Receiving sickness benefits	-0.00973	0.00190	-0.0208	-0.00465	-0.000745	-0.00717
Standard error	(0.0185)	(0.0140)	(0.0326)	(0.0135)	(0.00177)	(0.0251)
Receiving other social benefits	0.0158	0.0996*	-0.0347	0.0271	0.0990*	-0.0158
Standard error	(0.0360)	(0.0545)	(0.0467)	(0.0364)	(0.0558)	(0.0469)
Observations	5,502	2,544	2,958	5,502	2,544	2,958

Note: This table shows the difference-in-discontinuities regression results according to Equation (2), using a local linear regression with a bandwidth of 12 months, and 2010 and 2011 as control years. Each coefficient comes from a different regression, only the coefficient of interest is reported. The following covariates are included in the regressions, living together, being a child at home, having children, having primary education as highest obtained education and having secondary education as highest obtained education. Standard errors (robust) in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A9.3: Difference-in-discontinuities design with probability threshold equal to 0.682, parametric results

	(1)	(2)	(3)	(4)
	April		Year later	
2010-2013	Overall	Female	Overall	Female
Receiving welfare benefits	-0.0891	-0.0933	-0.227	-0.230
Standard error	(0.0972)	(0.0993)	(0.143)	(0.147)
Being employed	-0.124	-0.147	0.0212	0.0137
Standard error	(0.114)	(0.117)	(0.0520)	(0.0530)
Being a student	0.0431	0.0608	0.0393	0.0427
Standard error	(0.136)	(0.140)	(0.0504)	(0.0527)
Receiving sickness benefits	-0.0884	-0.0905	0.00586	0.00663
Standard error	(0.0585)	(0.0598)	(0.0289)	(0.0300)
Receiving other social benefits	0.104	0.0860	0.122	0.106
Standard error	(0.137)	(0.141)	(0.136)	(0.140)
Observations	733	721	733	721

Note: This table shows the difference-in-discontinuities regression results according to Equation (2), using a local linear regression with a bandwidth of 12 months, and 2010 and 2011 as control years. Each coefficient comes from a different regression, only the coefficient of interest is reported. The following covariates are included in the regressions, living together, being a child at home, having children, having primary education as highest obtained education and having secondary education as highest obtained education. Standard errors (robust) in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A9.4: Difference-in-discontinuities design with probability threshold equal to 0.75, parametric results

	(1)	(2)	(3)	(4)
	April		Year later	
2010-2013	Overall	Female	Overall	Female
Receiving welfare benefits	0.105	0.0951	0.190	0.176
Standard error	(0.101)	(0.103)	(0.154)	(0.159)
Being employed	-0.0449	-0.0623	0.0497*	0.0516*
Standard error	(0.0827)	(0.0842)	(0.0295)	(0.0302)
Being a student	0.141	0.145	0.0870	0.0949
Standard error	(0.156)	(0.162)	(0.0622)	(0.0673)
Receiving sickness benefits	-0.0890	-0.0933	-0.0367	-0.0384
Standard error	(0.0586)	(0.0603)	(0.0369)	(0.0386)
Receiving other social benefits	0.114	0.112	0.102	0.0987
Standard error	(0.120)	(0.126)	(0.118)	(0.125)
Observations	361	352	361	352

Note: This table shows the difference-in-discontinuities regression results according to Equation (2), using a local linear regression with a bandwidth of 12 months, and 2010 and 2011 as control years. Each coefficient comes from a different regression, only the coefficient of interest is reported. The following covariates are included in the regressions, living together, being a child at home, having children, having primary education as highest obtained education and having secondary education as highest obtained education. Standard errors (robust) in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A9.5: Difference-in-discontinuities design with no probability threshold, parametric results

	(1)	(2)	(3)	(4)	(5)	(6)
2010-2013	April Overall	Male	Female	Year later Overall	Male	Female
Receiving welfare benefits	0.000885	0.000669	0.00111	0.00143*	0.00109	0.00178
Standard error	(0.0006)	(0.000817)	(0.0008)	(0.000798)	(0.00112)	(0.00113)
Being employed	0.00356	0.00779**	-0.000507	0.00516*	0.0122***	-0.00183
Standard error	(0.00263)	(0.00372)	(0.00369)	(0.00268)	(0.00379)	(0.00376)
Being a student	-0.00261	-0.00233	-0.00266	-0.00202	-0.000490	-0.00337
Standard error	(0.00214)	(0.00314)	(0.00291)	(0.00191)	(0.00279)	(0.00260)
Receiving sickness benefits	0.000148	0.000674	-0.000472	-0.000267	0.00134	-0.00199
Standard error	(0.00087)	(0.00101)	(0.00143)	(0.000917)	(0.00106)	(0.00151)
Receiving other social benefits	-0.000260	-0.00119	0.00069	-0.00137	-0.00200	-0.000735
Standard error	(0.00121)	(0.00173)	(0.00169)	(0.00122)	(0.00174)	(0.00170)
Observations	1,592,197	805,707	786,490	1,592,197	805,707	786,490

Note: This table shows the difference-in-discontinuities regression results according to Equation (2), using a local linear regression with a bandwidth of 12 months, and 2010 and 2011 as control years. Each coefficient comes from a different regression, only the coefficient of interest is reported. The following covariates are included in the regressions, living together, being a child at home, having children, having primary education as highest obtained education and having secondary education as highest obtained education. Standard errors (robust) in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

A.10 Difference-in-discontinuities with polynomials, parametric results

Table A10.1: Difference-in-discontinuities design with polynomials of second order, parametric results

	(1)	(2)	(3)	(4)	(5)	(6)
	April			Year later		
2010-2013	Overall	Male	Female	Overall	Male	Female
Receiving welfare benefits	-0.0119	-0.0111	-0.0117	-0.0115	0.00856	-0.0235
Standard error	(0.0123)	(0.0210)	(0.0149)	(0.0178)	(0.0299)	(0.0220)
Being employed	0.00945	0.0560	-0.0136	0.00850	0.0541	-0.0168
Standard error	(0.0232)	(0.0366)	(0.0299)	(0.0220)	(0.0341)	(0.0288)
Being a student	0.00246	-0.00356	0.00640	0.000245	0.00289	-0.00349
Standard error	(0.0154)	(0.0238)	(0.0202)	(0.00922)	(0.00881)	(0.0143)
Receiving sickness benefits	-0.00291	-0.00676	-0.00124	-0.0187*	-0.0127	-0.0233
Standard error	(0.0129)	(0.0171)	(0.0184)	(0.0113)	(0.0150)	(0.0162)
Receiving other social benefits	0.0149	0.00462	0.0195	0.0107	-0.00170	0.0164
Standard error	(0.00923)	(0.0123)	(0.0129)	(0.00905)	(0.0119)	(0.0127)
Observations	83,383	37,027	46,356	83,383	37,027	46,356

Note: This table shows the difference-in-discontinuities regression results according to Equation (2), using a local linear regression with a bandwidth of 12 months, and 2010 and 2011 as control years. Each coefficient comes from a different regression, only the coefficient of interest is reported. The following covariates are included in the regressions, living together, being a child at home, having children, having primary education as highest obtained education and having secondary education as highest obtained education. Standard errors (robust) in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table A10.1: Difference-in-discontinuities design with polynomials third order, parametric results

	(1)	(2)	(3)	(4)	(5)	(6)
2010-2013	April Overall	Male	Female	Year later Overall	Male	Female
Receiving welfare benefits	-0.0238	-0.0272	-0.0193	-0.0432	-0.0521	-0.0330
Standard error	(0.0180)	(0.0316)	(0.0214)	(0.0265)	(0.0452)	(0.0324)
Being employed	0.0324	0.117**	-0.0100	0.0190	0.0783	-0.0125
Standard error	(0.0345)	(0.0553)	(0.0442)	(0.0327)	(0.0510)	(0.0425)
Being a student	-0.00384	0.0145	-0.0154	0.00470	-0.000648	0.00678
Standard error	(0.0228)	(0.0355)	(0.0298)	(0.0136)	(0.0125)	(0.0211)
Receiving sickness benefits	0.00622	-0.00796	0.0154	-0.0180	-0.0130	-0.0223
Standard error	(0.0191)	(0.0250)	(0.0272)	(0.0166)	(0.0212)	(0.0239)
Receiving other social benefits	0.00489	-0.0132	0.0143	0.00109	-0.0156	0.00944
Standard error	(0.0135)	(0.0182)	(0.0187)	(0.0133)	(0.0178)	(0.0184)
Observations	83,383	37,027	46,356	83,383	37,027	46,356

Note: This table shows the difference-in-discontinuities regression results according to Equation (2), using a local linear regression with a bandwidth of 12 months, and 2010 and 2011 as control years. Each coefficient comes from a different regression, only the coefficient of interest is reported. The following covariates are included in the regressions, living together, being a child at home, having children, having primary education as highest obtained education and having secondary education as highest obtained education. Standard errors (robust) in parentheses. *** p<0.01, ** p<0.05, * p<0.1

A.11 Difference-in-discontinuities without covariates, parametric results

Table A11.1: Difference-in-discontinuities design without covariates, parametric results

	(1)	(2)	(3)	(4)	(5)	(6)
	April			Year later		
2010-2013	Overall	Male	Female	Overall	Male	Female
Receiving welfare benefits	-0.00755	-0.00724	-0.00681	0.00697	0.0262	-0.00429
Standard error	(0.00786)	(0.0135)	(0.00952)	(0.0114)	(0.0190)	(0.0142)
Being employed	0.00273	0.0464**	-0.0254	-0.00654	0.0113	-0.0183
Standard error	(0.0146)	(0.0227)	(0.0192)	(0.0140)	(0.0216)	(0.0184)
Being a student	-0.0130	-0.0142	-0.0126	-0.00615	-0.00657	-0.00703
Standard error	(0.00997)	(0.0151)	(0.0134)	(0.00604)	(0.00575)	(0.00941)
Receiving sickness benefits	-0.00840	-0.00617	-0.0112	-0.0130*	-0.00577	-0.0196*
Standard error	(0.00818)	(0.0108)	(0.0116)	(0.00726)	(0.00963)	(0.0103)
Receiving other social benefits	0.00460	-0.00332	0.00977	0.00816	0.00185	0.0118
Standard error	(0.00566)	(0.00749)	(0.00798)	(0.00554)	(0.00723)	(0.00784)
Observations	83,383	37,027	46,356	83,383	37,027	46,356

Note: This table shows the difference-in-discontinuities regression results according to Equation (2), using a local linear regression with a bandwidth of 12 months, and 2010 and 2011 as control years. Each coefficient comes from a different regression, only the coefficient of interest is reported. Standard errors (robust) in parentheses. *** p<0.01, ** p<0.05, * p<0.1

A.12 Difference-in-discontinuities donut regression, parametric results

Table A12.1: Difference-in-discontinuities donut regression, parametric results

	(1)	(2)	(3)	(4)	(5)	(6)
	April			Year later		
2010-2013	Overall	Male	Female	Overall	Male	Female
Receiving welfare benefits	-0.000143	-0.00583	0.00349	0.0133	0.0234	0.00537
Standard error	(0.00891)	(0.0152)	(0.0108)	(0.0130)	(0.0217)	(0.0161)
Being employed	-0.00409	0.0438*	-0.0353	-0.0171	0.00349	-0.0308
Standard error	(0.0168)	(0.0258)	(0.0222)	(0.0160)	(0.0246)	(0.0212)
Being a student	-0.0143	-0.0188	-0.0130	-0.0110	-0.0128*	-0.0109
Standard error	(0.0113)	(0.0173)	(0.0150)	(0.00670)	(0.00687)	(0.0103)
Receiving sickness benefits	-0.0105	-0.000720	-0.0168	-0.0132	-7.44e-05	-0.0226*
Standard error	(0.00927)	(0.0121)	(0.0133)	(0.00824)	(0.0109)	(0.0118)
Receiving other social benefits	0.00418	-0.0101	0.0110	0.00910	-0.00540	0.0160*
Standard error	(0.00680)	(0.00902)	(0.00956)	(0.00666)	(0.00870)	(0.00942)
Observations	77,254	34,329	42,925	77,254	34,329	42,925

Note: This table shows the difference-in-discontinuities regression results according to Equation (2), using a local linear regression with a bandwidth of 12 months, and 2010 and 2011 as control years. Adults who are 26 and 11 months or 27 are left out of the sample. Each coefficient comes from a different regression, only the coefficient of interest is reported. The following covariates are included in the regressions, living together, being a child at home, having children, having primary education as highest obtained education and having secondary education as highest obtained education. Standard errors (robust) in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

A.13 Difference-in-discontinuities design according to highest obtained education, parametric results

Table A13.1: Difference-in-discontinuities estimates according to highest obtained education for short term outcomes, parametric results

	(1)	(2)	(3)	(4)
2010-2013	Primary education	Secondary education	Bachelor's degree	Masters' degree
Receiving welfare benefits	0.00628	-0.0138	-0.00893	-
Standard error	(0.0151)	(0.00899)	(0.248)	-
Being employed	0.0432*	-0.00696	-0.513	-0.193
Standard error	(0.0251)	(0.0183)	(0.373)	(0.465)
Being a student	-0.0115	-0.0166	0.335	-
Standard error	(0.0124)	(0.0135)	(0.390)	-
Receiving sickness benefits	-0.0109	-0.00882	0.0117	-
Standard error	(0.0143)	(0.0101)	(0.0163)	-
Receiving other social benefits	0.0188	-0.00603	0.00489	-0.0172
Standard error	(0.0138)	(0.00553)	(0.0542)	(0.390)
Observations	25,304	57,523	492	37

Note: This table shows the difference-in-discontinuities regression results according to Equation (2), using a local linear regression with a bandwidth of 12 months, and 2010 and 2011 as control years. The outcome variables used are observed in April of the year that applies. Each coefficient comes from a different regression, only the coefficient of interest is reported. The following covariates are included in the regressions, living together, being a child at home, having children, having primary education as highest obtained education and having secondary education as highest obtained education. Standard errors (robust) in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A13.1: Difference-in-discontinuities estimates according to highest obtained education for long term outcomes, parametric results

	(1)	(2)	(3)	(4)
2010-2013	Primary education	Secondary education	Bachelor's degree	Masters' degree
Receiving welfare benefits	0.0150	0.00397	-	-
Standard error	(0.0208)	(0.0135)		-
Being employed	0.0388*	-0.0187	-0.0992	-
Standard error	(0.0235)	(0.0175)	(0.0809)	
Being a student	-0.0158	-0.000662	0.00313	-
Standard error	(0.00989)	(0.00734)	(0.0364)	-
Receiving sickness benefits	-0.0167	-0.0116	0.109	-
Standard error	(0.0128)	(0.00889)	(0.114)	-
Receiving other social benefits	0.0264*	-0.00339	-0.0378	-0.162
Standard error	(0.0137)	(0.00526)	(0.0644)	(0.410)
Observations	25,304	57,523	492	37

Note: This table shows the difference-in-discontinuities regression results according to Equation (2), using a local linear regression with a bandwidth of 12 months, and 2010 and 2011 as control years. The outcome variables used are observed in January of a year later. Each coefficient comes from a different regression, only the coefficient of interest is reported. The following covariates are included in the regressions, living together, being a child at home, having children, having primary education as highest obtained education and having secondary education as highest obtained education. Standard errors (robust) in parentheses. *** p<0.01, ** p<0.05, * p<0.1.